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ABSTRACT

The American Association for the Advancement of Science (AAAS) has necessarily played a major role in the debates over choices and responsibilities arising from the advances of science and its applications in a rapidly changing world. The editor of this volume has selected articles, editorials, and letters from the AAAS journal "Science," from 1949-1988, that deal with: (1) scientific freedom and responsibility; (2) the difficult ethical questions that have arisen in the practice of science and its applications; (3) the risks and benefits of new technologies and the making of decisions concerning them; (4) the conflict between the traditional openness of science and the pressures for secrecy arising from concerns over national security by government officials; and (5) other related matters. (MVL)

Science, Technology, and Society

Emerging Relationships

Edited by Rosemary Chalk

Papers from SCIENCE, 1949–1988

The American Association for the Advancement of Science

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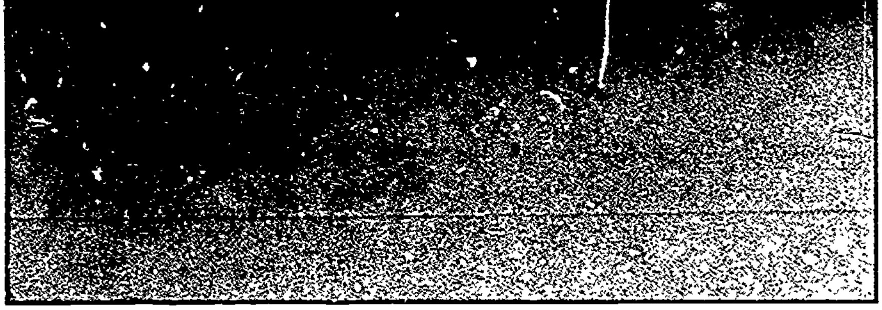


1. Science and Responsibility

Moral Imperatives in the Nuclear Age

2. Science and Freedom

Sputnik *and Segregation*

3. Science and Ethics

An Ethical Code for Scientists?

4. The Human Side of Science

Beyond the Laboratory Walls

5. Scientists and Citizens

Entering the Political Arena

6. Science and the Modern World

Autonomy and Accountability

7. Fraud and Misconduct in Science

Putting Our House in Order

8. Professional Rights and Duties in the Health Sciences

New Developments, New Debates

9. Science and Risk

How Safe Is Safe Enough?

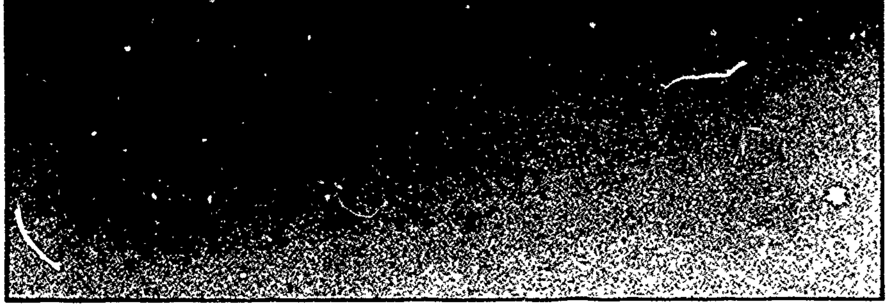
7

10. Science and National Security

Silos of Secrecy

Appendix: AAAS Committee Reports

8



The great scientific advances of the twentieth century have enabled us to see far more deeply than ever before into the nature of matter and of life. They have also endowed mankind with power to alter the environment, and the living organisms in it, in ways almost unimaginable to our ancestors. With increased power comes increased responsibility for the wise use of that power. The release of nuclear energy, one of the great turning points in human history, has thrust upon us the necessity of preventing the destruction of civilization by nuclear weapons. The emerging understanding of life processes, and their application in modern biotechnology, will permit us to modify living organisms in ways that may result in great benefit or great harm.

The wise use of these great and rapidly increasing powers in a democratic society is everybody's business. A special responsibility, however, falls upon the scientific community. Scientists have placed these new powers in the hands of mankind. Some scientists, at least, must assume the responsibility of pointing out, to the world at large, new opportunities and dangers that may not at first be obvious to the public. The use of this new power to change the world raises new ethical problems and dilemmas. The solutions are not simple, and scientists are not always wise when they attempt to deal with new social problems arising from their discoveries. Passionate disagreements can arise among conscientious and thoughtful people when they debate these issues.

The American Association for the Advancement of Science has necessarily played a major role in the debates over responsibilities and choices arising from the advances of science and its applications in a rapidly changing world. One manifestation of this concern was the establishment in 1976 of a Committee on Scientific Freedom and Responsibility, which had a specific mandate to consider these issues. Rosemary Chalk served with great ability and devotion to the staff officer of this Committee, as I can testify from having worked with her as a member and (for two years) chairman of the Committee. As the editor of this volume, she has selected articles, editorials, and letters from the AAAS journal *Science* that deal with. scientific freedom and responsibility, the difficult ethical questions that have arisen in the practice of science and its applications, the risks and benefits of new technologies and the making of decisions concerning them, the conflict between the traditional openness of science and the pressures for secrecy arising from concerns over national security by government officials; and other related matters.

The reader who is concerned with any or all of the important and difficult problems discussed here can gain valuable illumination and new perspectives that will well repay a careful reading. I warmly recommend this book to those concerned with these vital problems.

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and Molecular Biology
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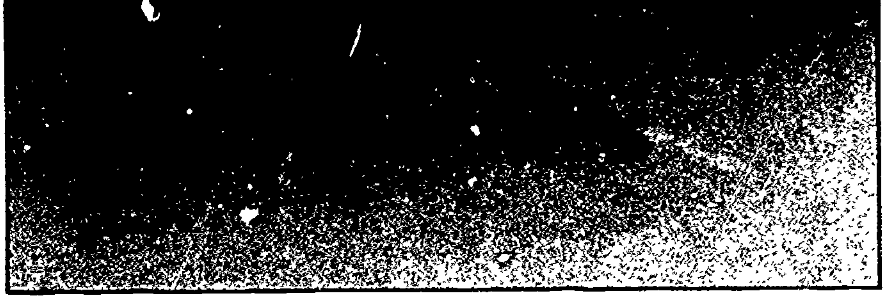
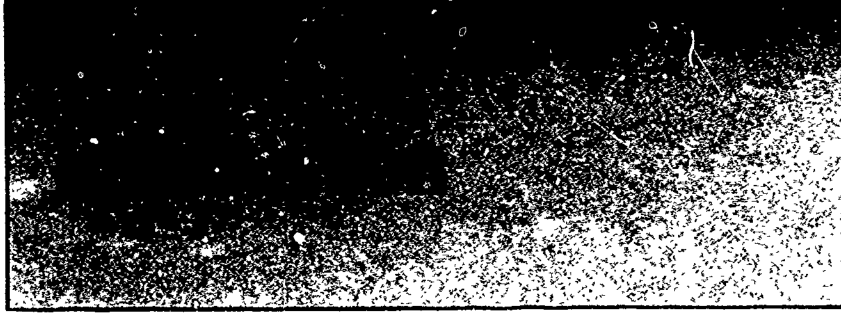


As noted in the introduction, this book builds upon an earlier collection of *Science* reprints prepared while I was the staff officer for the AAAS Committee on Scientific Freedom and Responsibility. Amy Silverman, Grayce A. Finger, and Corrine Harris each contributed to the development of the reprint series, and their efforts provided a valuable core of material now incorporated into this text.

I am indebted to Mark S. Frankel, program head for the AAAS Committee on Scientific Freedom and Responsibility, who sponsored the development of the book and assisted in suggesting and locating selected items. The staff of the AAAS Publications Office, especially Arthur Herschman, Kathryn Wolff, and Lonna Koblick offered special advice and assistance at critical points in the development of the book. My husband, Michael A. Stoto, contributed many hours solving the technical glitches of our home computer system. Finally, I express sincere gratitude to David Savold, who provided extensive time, talent, and patience throughout the editing and production process.



The relationship between science and technology and society — a field of inquiry termed STS studies — has fascinated many students and scholars throughout recent decades. Part of the fascination lies in the dynamic quality of the relationship between science and society. Science transforms society through its discoveries, but science is itself altered by social forces beyond the laboratory walls.

In the aftermath of World War II, as scientists entered the corridors of political, military, and economic power, they began to integrate their former ivory tower world of science with new social, commercial, and governmental concerns. With a vision of science as an "endless frontier," in the words of Vannevar Bush, the scientific leaders of the post-Hiroshima world sought to open up new vistas of intellectual power. They designed a complex set of public and private arrangements to channel public resources toward the development of creative scientific talent and the organization of new institutions for scientific research. Building on these reforms, they hoped to generate new knowledge and technology to benefit society as a whole.

Calls for more independence or freedom in science, in order to protect the self-guided direction of scientific inquiry and develop the most promising areas of research, were met over time by equally strong calls for social responsibility and accountability in science, the result of uneasy public concern about the directions in which laissez-faire science might be headed. The pioneering spirit that guided the early relationship between science and technology and society gradually gave way to a new sense of accommodation and acceptance of regulation, as scientists and society as a whole sought new ways to integrate into the emerging research agenda the public's concerns with individual rights, public safety, and economic costs.

Another part of the fascination with the study of science, technology, and society is found in the contrast between objective and subjective knowledge. While the formulas, mathematics, and laboratory tests that characterize advanced scientific and technical information are often beyond the understanding of most citizens, the social issues and problems associated with the development and use of science and technology are more easily comprehended, and they are experienced more directly. Conflicts over the siting of nuclear power plants, for example, or decisions governing the applications of genetic engineering, raise fundamental questions about the ethical and social principles that should guide such activities.

Is technological determinism — what can be done should be done — the best principle to invoke in shaping these technologies? How should such issues as informed consent, individual rights, or utilitarian principles emphasizing the common good, affect these decisions? If we wish to develop science in a social and human context, by what means can different sectors of society express their preferences and concerns?

Science, Technology, and Society: Emerging Relationships provides a thorough introduction to these issues. This anthology offers 85 articles, editorials, and letters published during the last 40 years in *Science*, the weekly journal of the American Association for the Advancement of Science. The material covers the period from 1949 until early 1987, providing a broad historical overview of the emerging relationship of science, technology, and society in the postwar period by authors well known in the scientific literature. Many of the articles were prepared originally as AAAS presidential addresses, Phi Beta Kappa talks, or keynote lectures at international conferences.

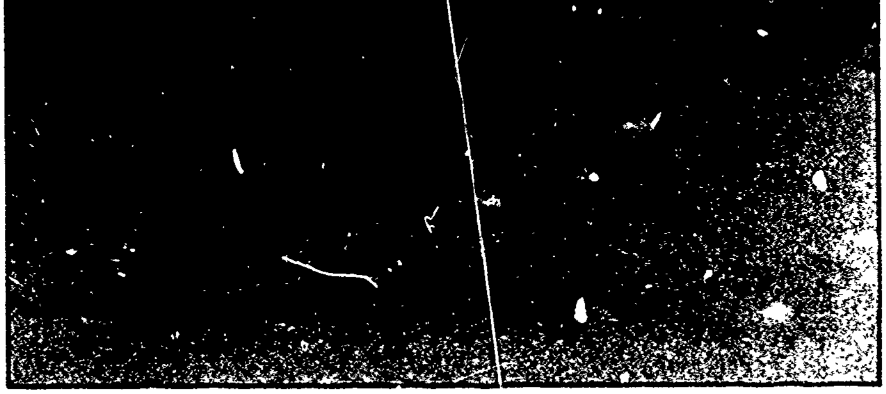
The collection is organized in reverse chronological order within chapter themes. Each chapter provides a focal point for particular issues that have characterized different stages of the evolving STS relationship. For example, the chapter on science and responsibility examines questions about the obligations that scientists bear for the social consequences of their work. How should scientists respond to social concerns about the uses of their discoveries? How can they foster accuracy and integrity in research? To whom are scientists accountable?

In addition to articles, the collection includes editorials reflecting different perspectives on science, technology, and society concerns over the past four decades. These provocative essays capture the thoughts and reflections of leaders of the scientific community who sought to articulate and clarify the pressing problems of each period in the pages of *Science*.

In the late 1950s, for example, biologist Bentley Glass suggested that the freedom of science might be threatened more by forces of racism and apartheid, as illustrated by the segregationist battles of Little Rock, Arkansas, and Pretoria, than by the technical achievements of the Soviet Union as symbolized by the successful launching of the *Sputnik* satellite.

Letters to the editor and other published correspondence highlight other concerns of the scientific community. The exchange of letters in 1982 between former AAAS Executive Officer William D. Carey and then Deputy Secretary of Defense Frank Carlucci initiated a new wave of discussions within government and science policy circles about the extent to which the openness of American scientific communication threatens the national security interests of the U.S. government. More recently, the claim by *Science* editor Daniel E. Koshland, Jr., that "99.9999 percent of [scientific] reports are accurate and truthful" generated a flurry of letters debating the extent to which fraud causes serious damage in the scientific literature.

It is my hope that this volume will be a valuable resource document for those with professional interest in the field of science, technology, and society studies. This book is also designed as a supplemental text for university courses examining the social context of science and technology, both replacing and extending the earlier *Science* Reprint Series on Scientific Freedom and Responsibility that was published by AAAS in the early 1980s. I believe the book may be of interest also to lay readers who want to know more about what we may call the conscience of science. For all readers, the book provides an in-depth examination of the purposes and values that shape the directions of science today. *—RC*



In the first half of the twentieth century, scientists on both sides of the Atlantic confidently believed scientific progress would inevitably lead to positive social change. From their work on the frontiers of intellectual discovery, they felt, society would eventually reap a harvest of progressive reform.

But the aftermath of World War II tempered this optimistic confidence with the sober realization that the political and economic forces governing social change would inevitably shape the manner and methods by which science would be applied to important social problems. The realization that the intrinsic beauty of atomic physics could lead to the death and destruction witnessed in Hiroshima prompted Robert Oppenheimer's famous comment that physicists had now "known sin." Now that scientists had successfully negotiated a new social contract that committed large amounts of public funds to the support of research, they found they had to develop a new framework of social accountability for the uses and consequences of this research.

The gradual understanding that social progress depended as much on political and economic reform as on scientific achievement prompted many leading figures in science, engineering, and the health sciences to explore ways in which scientists could help improve social conditions. Others outside the world of science also sought to define this role for scientists.

Writing in the 1950s amid Cold War tensions and public concern about nuclear tests, Bertrand Russell establishes a powerful theme by appealing to the role of scientists as citizens in clarifying the nature of their responsibility to society. "Facts which ought to guide the decisions of statesmen," he proclaims, "do not acquire their due importance if they remain buried in scientific journals." He denounces scientists who, in tandem with the various industries connected with armaments, had become "merchants of death," and he seeks to rally the mainstream members of the scientific community to participate in public debates about the consequences of nuclear war. "If disinterested scientists do not speak out," Russell warns, "the others will succeed in conveying a distorted impression, not only to the public but also to the politicians."

Calls for greater social responsibility in science increased sharply in response to the growing stockpile of nuclear weapons, the recognition of environmental damage from pesticides, and the American public's desire to end the Vietnam war. These themes found their way into the pages of *Science* as various authors sought to develop a framework that would provide guidance for the general scientific community on the appropriate balance between the traditional professional duties of the scientists and their emerging social obligations.

Echoing themes introduced by Russell, C P Snow's address to the 1960 AAAS Annual Meeting discards the doctrine of ethical neutrality in science, in which scientists disown responsibility for the applications of their research. He sets forth a rationale to replace this doctrine, building upon the intrinsic beauty and truth of basic science and the excitement of fundamental research. Suggesting that scientists must extend their duty to question and seek the truth into questions of use and applications of their work, Snow concludes that scientists who have particular knowledge about the effects of their work in public affairs have a moral imperative to say what they know.

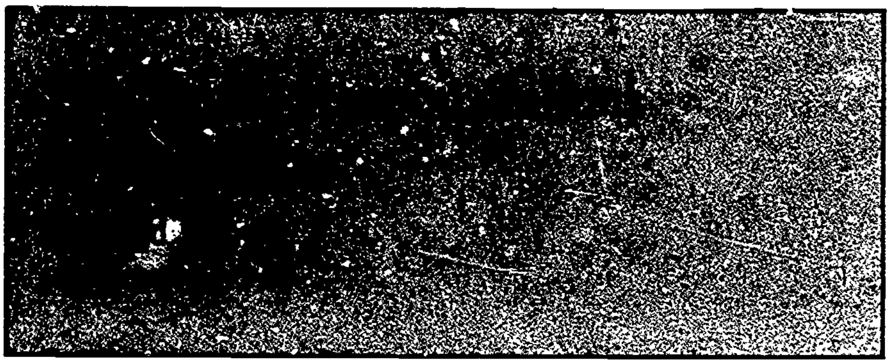
The issues raised by Russell and Snow continued to echo in the editorials in *Science* for more than twenty years as others sought to clarify ethical principles for professional conduct in public affairs. The editorials in this chapter suggest alternative ways to determine the appropriate balance between the roles of scientist as a disinterested seeker of knowledge and as a concerned citizen in contemporary society.

John T. Edsall in 1981 integrates the thinking of the previous decades into a new statement on the professional and social responsibilities of scientists. Although the two areas have much in common, he writes, the problems of social responsibility in science must be considered in light of the general code of scientific behavior that evolved over the past few centuries. Thus an emphasis on the integrity and truthfulness of scientific claims in the political arena must necessarily be an important measure of the public role of the scientist.

Anna J Harrison continues this theme in two of her articles, suggesting that scientists, individually and collectively, are obligated to provide their technical expertise to those who need information to make decisions about the uses and directions of science and technology. But they are responsible for the integrity of that information and should identify clearly their uncertainties, or biases, in attempting to fit preliminary research findings to important public policy problems.

This chapter presents issues that recur throughout the field of science, technology, and society studies. Concerns about the importance of protecting the objectivity of scientific knowledge, the uses and social applications of research, and the need to reconcile professional values with political and social interests continue to dominate discussions about the meaning of responsibility in science.

—*RC*



Science, ever since it first existed, has had important effects in matters that lie outside the purview of pure science. Men of science have differed as to their responsibility for such effects. Some have said that the function of the scientist in society is to supply knowledge, and that he need not concern himself with the use to which this knowledge is put. I do not think that this view is tenable, especially in our age. The scientist is also a citizen; and citizens who have any special skill have a public duty to see, as far as they can, that their skill is utilized in accordance with the public interest. Historically, the functions of the scientist in public life have generally been recognized. The Royal Society was founded by Charles II as an antidote to "fanaticism" which had plunged England into a long period of civil strife. The scientists of that time did not hesitate to speak out on public issues, such as religious toleration and the folly of prosecutions for witchcraft. But although science has, in various ways at various times, favored what may be called a humanitarian outlook, it has from the first had an intimate and sinister connection with war. Archimedes sold his skill to the Tyrant of Syracuse for use against the Romans; Leonardo secured a salary from the Duke of Milan for his skill in the art of fortification; and Galileo got employment under the Grand Duke of Tuscany because he could calculate the trajectories of projectiles. In the French Revolution the scientists who were not guillotined were set to making new explosives, but Lavoisier was not spared, because he was only discovering hydrogen which, in those days, was not a weapon of war. There have been some honorable exceptions to the subservience of scientists to warmongers. During the Crimean War the British Government consulted Faraday as to the feasibility of attack by poisonous gases. Faraday replied that it was entirely feasible, but that it was inhuman and he would have nothing to do with it.

Affecting Public Opinion

Modern democracy and modern methods of publicity have made the problem of affecting public opinion quite different from what it used to be. The knowledge that the public possesses on any important issue is derived from vast and powerful organizations: the press, radio, and, above all, television. The knowledge that governments possess is more limited. They are too busy to search out the facts for themselves, and consequently they know only what their underlings think good for them unless there is such a powerful movement in a different sense that politicians cannot ignore it. Facts which ought to guide the decisions of statesmen—for instance, as to the possible lethal qualities of fallout—do not acquire their due importance if they remain buried in scientific journals. They acquire their due importance only when they become known to so many voters that they affect the course of the elections. In general, there is an opposition to widespread publicity for such facts. This opposition springs from various sources, some sinister, some comparatively respectable. At the bottom of the moral scale there is the financial interest of the various industries connected with armaments. Then there are various effects of a somewhat thoughtless patriotism which believes in secrecy and in what is called "toughness." But perhaps more important than either of these is the unpleasantness of the facts, which makes the general public turn aside to pleasanter topics such as divorces and murders. The consequence is that what ought to be known widely throughout the general public will not be known unless great efforts are made by disinterested persons to see that the information reaches the minds and hearts of vast numbers of people. I do not think this work can be successfully accomplished except by the help of men of science. They, alone, can speak with the authority that is necessary to combat the misleading statements of those scientists who have permitted themselves to become merchants of death. If disinterested scientists do not speak out, the others will succeed in conveying a distorted impression, not only to the public but also to the politicians.

Obstacles to Individual Action

It must be admitted that there are obstacles to individual action in our age which did not exist at earlier times. Galileo could make his own telescope. But once when I was talking with a very famous astronomer he explained that the telescope upon which his work depended owed its existence to the benefactions of enormously rich men, and, if he had not stood well with them, his astronomical discoveries would have been impossible. More frequently, a scientist only acquires access to enormously expensive equipment if he stands well with the government of his country. He knows that if he adopts a rebellious attitude he and his family are likely to perish along with the rest of civilized mankind. It is a tragic dilemma, and I do not think that one should censure a man whatever his decision; but I do think—and I think men of science should realize—that unless something rather drastic is done under the leadership or through the inspiration of some part of the scientific world, the human race, like the Gadarene swine, will rush down a steep place to destruction in blind ignorance of the fate that scientific skill has prepared for it.

It is impossible in the modern world for a man of science to say with any honesty, "My business is to provide knowledge, and what use is made of the knowledge is not my responsibility." The knowledge that a man of science provides may fall into the hands of men or institutions devoted to utterly unworthy objects. I do not suggest that a man of science, or even a large body

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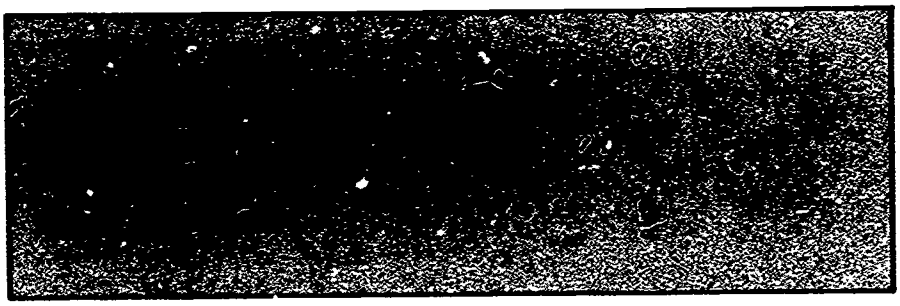
The author is a fellow of Trinity College, Cambridge, and of the Royal Society. This article is the text of an address delivered 24 September 1959 in London at a meeting of British scientists convened by the Campaign for Nuclear Disarmament.

of men of science, can altogether prevent this, but they can diminish the magnitude of the evil.

There is another direction in which men of science can attempt to provide leadership. They can suggest and urge in many ways the value of those branches of science of which the important practical uses are beneficial and not harmful. Consider what might be done if the money at present spent on armaments were spent on increasing and distributing the food supply of the world and diminishing the population pressure. In a few decades, poverty and malnutrition, which now afflict more than half the population of the globe, could be ended. But at present almost all the governments of great states consider that it is better to spend money on killing foreigners than on keeping their own subjects alive. Possibilities of a hopeful sort in whatever field can best be worked out and stated authoritatively by men of science; and, since they can do this work better than others, it is part of their duty to do it.

As the world becomes more technically unified, life in an ivory tower becomes increasingly impossible. Not only so; the man who stands out against the powerful organizations which control most of human activity is apt to find himself no longer in the ivory tower, with a wide outlook over a sunny landscape, but in the dark and subterranean dungeon upon which the ivory tower was erected. To risk such a habitation demands courage. It will not be necessary to inhabit the dungeon if there are many who are willing to risk it, for everybody knows that the modern world depends upon scientists, and, if they are insistent, they must be listened to. We have it in our power to make a good world; and, therefore, with whatever labor and risk, we must make it.



With science supporting an ever expanding military technology, many people in this country are wondering to what extent American scientists should assume responsibility for the uses to which the government puts their discoveries and talents. It has always been possible, of course, to speak of pure research, just as it has always been possible, we suppose, to speak of the pure act of sitting down to a meal and consuming it with impeccable table manners. But any piece of behavior can acquire moral properties, given the appropriate circumstances—even sitting down to eat a hamburger, as recent developments in the South have shown.

One view of the scientist's responsibility for the social consequences of scientific truths is that this responsibility ends with the scientist's willingness to do work directly or indirectly for the government, including work on weapons. According to this view, being a good scientist no more gives one special privileges in determining national policy than being a good information clerk at an airport entitles one to select destinations for travelers. The area of special competence of scientists lies in the discovery of technical facts; decisions of public policy rest with elected or appointed public officials.

An opposite opinion concerning the obligations of scientists holds that scientists should consider the possible consequences of any piece of research before it is begun, and if the research is judged more a threat to the country, or humanity at large, than a benefit, they should refuse their services. A man cannot delegate to a superior the responsibility for the moral consequences of his acts, the second view claims. To be sure, to predict future applications of new discoveries calls more for the talents of a prophet than for those of a scientist. No one now knows to what uses, or abuses, the fall of parity in physics may some day prove amenable. But somewhere along the line, basic research becomes applied research, and forecasts about the uses of discoveries become something more than anybody's guess.

Between the two opposing positions lies a third position which holds that at least some scientists, although they fear the dangers posed by a further increase in military power, have the duty to work on projects that the government deems necessary, but that scientists also have the duty to state their opinions on matters lying outside science. If this is the age of specialization, so this argument runs, it is also the age of specialists working together on teams. Public officials should have the final word, but any attempt to understand the full range of consequences—military, political, economic, and moral—of new advances in research, requires the views of the men who understand those advances best.

It is this third position that expresses our own convictions, and that seems to express the convictions of most of the persons in this country who are presently concerned with these problems—although, admittedly, agreement on general principles does not necessarily imply agreement on particular cases. The first position errs because, pushed to its conclusion, it turns the citizen's obligations to the state into despotism; while the second position errs because, if pushed, it turns the moral integrity of the individual into anarchy. The third position seeks the mean between the scientist's assuming too little responsibility for the consequences of his research and his assuming too much responsibility.

Scientists are the most important occupational group in the world today. At this moment, what they do is of passionate concern to the whole of human society. At this moment, the scientists have little influence on the world effect of what they do. Yet, potentially, they can have great influence. The rest of the world is frightened both of what they do—that is, of the intellectual discoveries of science—and of its effect. The rest of the world, transferring its fears, is frightened of the scientists themselves and tends to think of them as radically different from other men.

As an ex-scientist, if I may call myself so, I know that is nonsense. I have even tried to express in fiction some kinds of scientific temperament and scientific experience. I know well enough that scientists are very much like other men. After all, we are all human, even if some of us don't give that appearance. I think I would be prepared to risk a generalization. The scientists I have known (and because of my official life I have known as many as anyone in the world) have been in certain respects just perceptibly more morally admirable than most other groups of intelligent men.

That is a sweeping statement, and I mean it only in a statistical sense. But I think there is just a little in it. The moral qualities I admire in scientists are quite simple ones, but I am very suspicious of attempts to oversubtilize moral qualities. It is nearly always a sign, not of true sophistication, but of a specific kind of triviality. So I admire in scientists very simple virtues—like courage, truth-telling, kindness—in which, judged by the low standards which the rest of us manage to achieve, the scientists are not deficient. I think on the whole the scientists make slightly better husbands and fathers than most of us, and I admire them for it. I don't know the figures, and I should be curious to have them sorted out, but I am prepared to bet that the proportion of divorces among scientists is slightly but significantly less than that among other groups of similar education and income. I do not apologize for considering that a good thing.

A close friend of mine is a very distinguished scientist. He is also one of the few scientists I know who has lived what we used to call a Bohemian life When we were both younger, he thought he would undertake historical research to see how many great scientists had been as fond of women as he was. I think he would have felt mildly supported if he could have found a precedent. I remember his reporting to me that his researches hadn't had any luck. The really great scientists seemed to vary from a few neutral characters to a large number who were depressingly "normal." The only gleam of comfort was to be found in the life of Jerome Cardan; and Cardan wasn't anything like enough to outweigh all the others.

So scientists are not much different from other men. They are certainly no worse than other men. But they do differ from other men in one thing. That is the point I started with. Whether they like it or not, what they do is of critical importance for the human race. Intellectually, it has transformed the climate of our time. Socially, it will decide whether we live or die, and how we live or die. It holds decisive powers for good and evil. *That* is the situation in which the scientists find themselves. They may not have asked for it, or may only have asked for it in part, but they cannot escape it. They think, many of the more sensitive of them, that they don't deserve to have this weight of responsibility heaved upon them. All they want to do is to get on with their work. I sympathize. But the scientists can't escape the responsibility—any more than they, or the rest of us, can escape the gravity of the moment in which we stand.

Doctrine of Ethical Neutrality

There is of course one way to contract out. It has been a favorite way for intellectual persons caught in the midst of water too rough for them.

It consists of the invention of categories—or, if you like, of the division of moral labor. That is, the scientists who want to contract out say, *we* produce the tools. *We* stop there. It is for *you*—the rest of the world, the politicians—to say how the tools are used. The tools may be used for purposes which most of us would regard as bad. If so, we are sorry. But as scientists, that is no concern of ours.

This is the doctrine of the ethical neutrality of science. I can't accept it for an instant. I don't believe any scientist of serious feeling can accept it. It is hard, some think, to find the precise statements which will prove it wrong. Yet we nearly all feel intuitively that the invention of comfortable categories is a moral trap. It is one of the easier methods of letting the conscience rust. It is exactly what the early 19th century economists, such as Ricardo, did in the face of the facts of the first industrial revolution. We wonder now how men, intelligent men, can have been so morally blind. We realize how the exposure of that moral blindness gave Marxism its apocalyptic force. We are now, in the middle of the scientific or second industrial revolution, in something like the same position as Ricardo. Are we going to let our consciences rust? Can we ignore that intimation we nearly all have, that scientists have a unique responsibility? Can we believe it, that science is morally neutral?

To me—it would be dishonest to pretend otherwise—there is only one answer to those questions. Yet I have been brought up in the presence of the same intellectual categories as most western scientists. It would also be dishonest to pretend that I find it easy to construct a rationale which expresses what I now believe. The best I can hope for is to fire a few sighting shots. Perhaps someone who sees more clearly than I can will come along and make a real job of it.

The Beauty of Science

Let me begin with a remark which seems some way off the point. Anyone who has ever worked in any science knows how much esthetic joy he has obtained. That is, in the actual *activity* of science, in the process of making a discovery, however humble it is, one can't help feeling an awareness of

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beauty. The subjective experience, the esthetic satisfaction, seems exactly the same as the satisfaction one gets from writing a poem or a novel, or composing a piece of music. I don't think anyone has succeeded in distinguishing between them. The literature of scientific discovery is full of this esthetic joy. The very best communication of it that I know comes in G. H. Hardy's book, *A Mathematician's Apology*. Graham Greene once said he thought that, along with Henry James's prefaces, this was the best account of the artistic experience ever written. But one meets the same thing throughout the history of science. Bolyai's great yell of triumph when he saw he could construct a self-consistent, non-Euclidean geometry; Rutherford's revelation to his colleagues that he knew what the atom was like; Darwin's slow, patient, timorous certainty that at last he had got there—all these are voices, different voices, of esthetic ecstasy.

That is not the end of it. The *result* of the activity of science, the actual finished piece of scientific work, has an esthetic value in itself. The judgments passed on it by other scientists will more often than not be expressed in esthetic terms: "That's beautiful!" or "That really is very pretty!" (as the understating English tend to say). The esthetics of scientific constructs, like the esthetics of works of art, are variegated. We think some of the great syntheses, like Newton's, beautiful because of their classical simplicity, but we see a different kind of beauty in the relativistic extension of the wave equation or the interpretation of the structure of deoxyribonucleic acid, perhaps because of the touch of unexpectedness. Scientists know their kinds of beauty when they see them. They are suspicious, and scientific history shows they have always been right to have been so, when a subject is in an "ugly" state. For example, most physicists feel in their bones that the present bizarre assembly of nuclear particles, as grotesque as a stamp collection, can't possibly be, in the long run, the last word.

We should not restrict the esthetic values to what we call "pure" science. Applied science has its beauties, which are, in my view, identical in nature. The magnetron has been a marvelously useful device, but it was a beautiful device, not exactly apart from its utility but because it did, with such supreme economy, precisely what it was designed to do. Right down in the field of development, the esthetic experience is as real to engineers. When they forget it, when they begin to design heavy-power equipment about twice as heavy as it needs to be, engineers are the first to know that they are lacking virtue.

There is no doubt, then, about the esthetic content of science, both in the activity and the result. But esthetics has no connection with morals, say the categorizers. I don't want to waste time on peripheral issues—but are you quite sure of that? Or is it possible that these categories are inventions to make us evade the human and social conditions in which we now exist? But let us move straight on to something else, which is right in the grain of the activity of science and which is at the same time quintessentially moral. I mean, the desire to find the truth.

The Search for Truth

By *truth*, I don't intend anything complicated, once again. I am using the wo[illegible] a scientist uses it. We all know that the philosophical examination of the concept of empirical truth gets us into some curious complexities, but most scientists really don't care. They know that the truth, as they use the word and as the rest of us use it in the language of common speech, is what makes science work. That is good enough for them. On it rests the whole great edifice of modern science. They have a sneaking sympathy for Rutherford, who, when asked to examine the philosophical bases of science, was inclined to reply, as he did to the metaphysician Samuel Alexander: "Well, what have you been talking all your life, Alexander? Just hot air! Nothing but hot air!"

Anyway, truth in their own straightforward sense is what the scientists are trying to find. They want to find what is *there*. Without that desire, there is no science. It is the driving force of the whole activity. It compels the scientist to have an overriding respect for truth, every stretch of the way. That is, if you're going to find what is *there*, you mustn't deceive yourself or anyone else. You mustn't lie to yourself. At the crudest level, you mustn't fake your experiments.

Curiously enough, scientists do try to behave like that. A short time ago, I wrote a novel in which the story hinged on a case of scientific fraud. But I made one of my characters, who was himself a very good scientist, say that, considering the opportunities and temptations, it is astonishing how few such cases there are. We have all heard of perhaps half a dozen open and notorious ones, which are on the record for anyone to read—ranging from the "discovery" of the L radiation to the singular episode of the Piltdown man.

We have all, if we have lived any time in the scientific world, heard private talk of something like another dozen cases which for various reasons are not yet public property. In some cases, we know the motives for the cheating—sometimes, but not always, sheer personal advantage, such as getting money or a job. But not always. A special kind of vanity has led more than one man into scientific faking. At a lower level of research, there are presumably some more cases. There must have been occasional Ph.D. students who scraped by with the help of a bit of fraud.

But the total number of all these men is vanishingly small by the side of the total number of scientists. Incidentally, the effect on science of such frauds is also vanishingly small. Science is a self-correcting system. That is, no fraud (or honest mistake) is going to stay undetected for long. There is no need for an extrinsic scientific criticism, because criticism is inherent in the process itself. So that all that a fraud can do is waste the time of the scientists who have to clear it up.

The remarkable thing is not the handful of scientists who deviate from the search for truth but the overwhelming numbers who keep to it. That is a demonstration, absolutely clear for anyone to see, of moral behavior on a very large scale.

We take it for granted. Yet it is very important. It differentiates science in its widest sense (which includes scholarship) from all other intellectual activities. There is a built-in moral component right in the core of the scientific activity itself. The desire to find the truth is itself a moral impulse, or at least contains a moral impulse. The way in which a scientist tries to find the truth imposes on him a constant moral discipline. We say a scientific conclusion—such as the contradiction of parity by Lee and Yang—is "true" in the limited sense of scientific truth, just as we say that it is "beautiful" according to the criteria of scientific esthetics. We also know that to reach this conclusion took a set of actions which would have been useless without the moral nature. That is, all through the marvelous experiments of Wu and her colleagues, there was the constant moral exercise of seeking and telling the truth. To scientists, who are brought up in this climate, this seems as natural as breathing. Yet it is a wonderful thing. Even if the scientific activity contained only this one moral component, that alone would be enough to let us say that it was morally un-neutral.

But is this the only moral component? All scientists would agree about the beauty and the truth. In the western world, they wouldn't agree on much more. Some will feel with me in what I am going to say. Some will not. That doesn't affect me much, except that I am worried by the growth of an attitude I think very dangerous, a kind of technological conformity disguised as cynicism. I shall say a little more about that later. As for disagreement, G. H. Hardy used to comment that a serious man ought not to waste his time stating a majority opinion—there are plenty of others to do that. That was the voice of classical scientific nonconformity. I wish that we heard it more often.

Science in the Twenties

Let me cite some grounds for hope. Any of us who were working in science before 1933 can remember what the atmosphere was like. It is a terrible bore when aging men in their fifties speak about the charms of their youth. Yet I am going to irritate you—just as Talleyrand irritated his juniors—by saying that unless one was on the scene before 1933, one hasn't known the sweetness of the scientific life. The scientific world of the twenties was as near to being a full-fledged international community as we are likely to get. Don't think I'm saying that the men involved were superhuman or free from the ordinary frailties. That wouldn't come well from me, who have spent a fraction of my writing life pointing out that scientists are, first and foremost, men. But the atmosphere of the twenties in science was filled with an air of benevolence and magnanimity which transcended the people who lived in it.

Anyone who ever spent a week in Cambridge or Göttingen or Copenhagen felt it all round him. Rutherford had very human faults, but he was a great man with abounding human generosity. For him the world of science was a world that lived on a plane above the nation-state, and lived there with joy. That was at least as true of those two other great men, Niels Bohr and Franck, and some of that spirit rubbed off on to the pupils round them. The same was true of the Roman school of physics.

The personal links within this international world were very close. It is worth remembering that Peter Kapitza, who was a loyal Soviet citizen, honored my country by working in Rutherford's laboratory for many years. He became a fellow of the Royal Society, a fellow of Trinity College, Cambridge, and the founder and kingpin of the best physics club Cambridge has known. He never gave up his Soviet citizenship and is now director of the Institute of Physical Problems in Moscow. Through him a generation of English scientists came to have personal knowledge of their Russian colleagues. These exchanges were then, and have remained, more valuable than all the diplomatic exchanges ever invented.

The Kapitza phenomenon [illegible]n't take place now. I hope to li[illegible] see the day when a young Kapitza can once more work for 16 years in Berkeley or Cambridge and then go back to an eminent place in his own country. When that can happen, we are all right. But after the idyllic years of world science, we passed into a tempest of history, and, by an unfortunate coincidence, we passed into a technological tempest too.

The discovery of atomic fission broke up the world of international physics. "This has killed a beautiful subject," said Mark Oliphant, the father figure of Australian physics, in 1945, after the bombs had dropped. In intellectual terms, he has not turned out to be right. In spiritual and moral terms, I sometimes think he has.

A good deal of the international community of science remains in other fields—in great areas of biology, for example. Many biologists are feeling the identical liberation, the identical joy at taking part in a magnanimous enterprise, that physicists felt in the twenties. It is more than likely that the moral and intellectual leadership of science will pass to biologists, and it is among them that we shall find the Rutherfords, Bohrs, and Francks of the next generation.

The Physicist, a Military Resource

Physicists have had a bitterer task. With the discovery of fission, and with some technical breakthroughs in electronics, physicists became, almost overnight, the most important military resource a nation-state could call on. A large number of physicists became soldiers not in uniform. So they have remained, in the advanced societies, ever since.

It is very difficult to see what else they could have done. All this began in the Hitler war. Most scientists thought then that Nazism was as near absolute evil as a human society can manage. I myself thought so. I still think so, without qualification. That being so, Nazism had to be fought, and since the Nazis might make fission bombs—which we thought possible until 1944, and which was a continual nightmare if one was remotely in the know—well, then, we had to make them too. Unless one was an unlimited pacifist, there was nothing else to do. And unlimited pacificism is a position which most of us cannot sustain.

Therefore I respect, and to a large extent share, the moral attitudes of those scientists who devoted themselves to making the bomb. But the trouble is, when you get onto any kind of moral escalator, to know whether you're ever going to be able to get off. When scientists became soldiers they gave up something, so imperceptibly that they didn't realize it, of the full scientific life. Not intellectually. I see no evidence that scientific work on weapons of maximum destruction has been in any intellectual respect different from other scientific work. But there is a moral difference.

It may be—scientists who are better men than I am often take this attitude, and I have tried to represent it faithfully in one of my books—that this is a moral price which, in certain circumstances, has to be paid. Nevertheless, it is no good pretending that there is not a moral price. Soldiers have to obey. That is the foundation of their morality. It is not the foundation of the scientific morality. Scientists have to question and if necessary to rebel. I don't want to be misunderstood. I am no anarchist. I am not suggesting that loyalty is not a prime virtue. I am not saying that all rebellion is good. But I am saying that loyalty can easily turn into conformity, and that conformity can often be a cloak for the timid and self-seeking. So can obedience, carried to the limit. When you think of the long and gloomy history of man, you will find that far more, and far more hideous, crimes have been committed in the name of obedience than have ever been committed in the name of rebellion. If you doubt that, read William Shirer's *Rise and Fall of the Third Reich*. The German officer corps were brought up in the most rigorous code of obedience. To them, no more honorable and God-fearing body of men could conceivably exist. Yet in the name of obedience, they were party to, and assisted in, the most wicked large-scale actions in the history of the world.

Scientists must not go that way. Yet the duty to question is not much of a support when you are living in the middle of an organized society. I speak with feeling here. I was an official for 20 years. I went into official life at the beginning of the war, for the reasons that prompted my scientific friends to begin to make weapons. I stayed in that life until a year ago, for the same reason that made my scientific friends turn into civilian soldiers. The official's life in England is not quite so disciplined as a soldier's, but it is very nearly so. I think I know the virtues, which are very great, of the men who live that disciplined life. I also know what for me was the moral trap. I, too, had got onto an escalator. I can put the result in a sentence: I was coming to hide behind the institution: I was losing the power to say no.

A Spur to Moral Action

Only a very bold man, when he is a member of an organized society, can keep the power to say no. I tell you that, not being a very bold man, or one who finds it congenial to stand alone, away from his colleagues. We can't expect many scientists to do it. Is there any tougher ground for them to stand on? I suggest to you that there is. I believe that there is a spring of moral action in the scientific activity which is at least as strong as the search for truth. The name of this spring is *knowledge*. Scientists *know* certain things in a fashion more immediate and more certain than those who don't comprehend what science is. Unless we are abnormally weak or abnormally wicked men, this knowledge is bound to shape our actions. Most of us are timid, but to an extent, knowledge gives us guts. Perhaps it can give us guts strong enough for the jobs in hand.

I had better take the most obvious example. All physical scientists *know* that it is relatively easy to make plutonium. We know this, not as a journalistic fact at second hand, but as a fact in our own experience. We can work out the number of scientific and engineering personnel needed for a nation-state to equip itself with fission and fusion bombs. We *know* that, for a dozen or more states, it will only take perhaps six years. perhaps less. Even the best informed of us always exaggerate these periods.

This we know, with the certainty of —what shall I call it?—engineering truth. We also—most of us—are familiar with statistics and the nature of odds. We know, with the certainty of statistical truth, that if enough of these weapons are made, by enough different states, some of them are going to blow up. through accident. or folly. or madness—the motives don't matter. What does matter is the nature of the statistical fact.

All this we *know*. We know it in a more direct sense than any politician because it comes from our direct experience. It is part of our minds. Are we going to let it happen?

All this we *know*. It throws upon scientists a direct and personal responsibility. It is not enough to say that scientists have a responsibility as citizens. They have a much greater one than that, and one different in kind. For scientists have a moral imperative to say what they know. It is going to make them unpopular in their own nation-states. It may do worse than make them unpopular. That doesn't matter. Or at least, it does matter to you and me, but it must not count in the face of the risks.

Alternatives

For we genuinely know the risks. We are faced with an either-or. and we haven't much time. The *either* is acceptance of a restriction of nuclear armaments. This is going to begin. just as a token, with an agreement on the stopping of nuclear tests. The United States is not going to get the 99.9-percent "security" that it has been asking for. This is unobtainable. though there are other bargains that the United States could probably secure. I am not going to conceal from you that this course involves certain risks. They are quite obvious, and no honest man is going to blink them. That is the *either*. The *or* is not a risk but a certainty. It is this. There is no agreement on tests. The nuclear arms race between the United States and the U.S.S.R. not only continues but accelerates. Other countries join in. Within, at the most. six years. China and several other states have a stock of nuclear bombs. Within. at the most, ten years, some of those bombs are going off. I am saying this as responsibly as I can. *That* is the certainty. On the one side, therefore, we have a finite risk. On the other side we have a certainty of disaster. Between a risk and a certainty, a sane man does not hesitate.

It is the plain duty of scientists to explain this either-or. It is a duty which seems to me to come from the moral nature of the scientific activity itself.

The same duty, though in a much more pleasant form, arises with respect to the benevolent powers of science. For scientists know. and again with the certainty of scientific knowledge, that we possess every scientific fact we need to transform the physical life of half the world. And transform it within the span of people now living. I mean, we have all the resources to help half the world live as long as we do and eat enough. All that is missing is the will. We *know* that. Just as we know that you in the United States, and to a slightly lesser extent we in the United Kingdom. have been almost unimaginably lucky. We are sitting like people in a smart and cozy restaurant and we are eating comfortably, looking out of the window into the streets. Down on the pavement are people who are looking up at us. people who by chance have different colored skins from ours. and are rather hungry. Do you wonder that they don't like us all that much? Do you wonder that we sometimes feel ashamed of ourselves. as we look out through that plate glass?

Well. it is within our power to get started on that problem. We are morally impelled to. We all know that. if the human species does solve that one. there will be consequences which are themselves problems For instance, the population of the world will become embarrassingly large. But that is another challenge There are going to be challenges to our intelligence and to our moral nature as long as man remains man. After all. a challenge is not. as the word is coming to be used. an excuse for slinking off and doing nothing A challenge is something to be picked up.

For all these reasons. I believe the world community of scientists has a final responsibility upon it—a greater responsibility than is pressing on any other body of men. I do not pretend to know how they will bear this responsibility. These may be famous last words. but I have an inextinguishable hope. For. as I have said. there is no doubt that the scientific activity is both beautiful and truthful. I cannot prove it, but I believe that. simply because scientists cannot escape their own knowledge. they also won't be able to avoid showing themselves disposed to good.

J.R. Wiggins, editor of the *Washington Post and Times Herald*, has been asking some of his scientist friends: If you could put the jinni back into the bottle, would you do it? Would you, if you had the choice, undo the work that led to the release of atomic energy? The question is not historical, for obviously the past cannot be undone. Neither is it a strictly scientific question, for if Otto Hahn, Lise Meitner, Enrico Fermi, and their collaborators had not released the atomic jinni, others would have. The point of the question is its social significance, not only for atomic energy itself, but also as a forewarning of problems that may lie ahead. Consider the moral, social, and political dilemmas that would follc ᵛ upon ability to control the weather on a world-wide scale, to control genetic material, or to control human behavior.

Warren Weaver posed essentially the same question, in a somewhat more manageable form. in asking C.P. Snow, after his address at the 1960 AAAS annual meeting: If a scientist can see with reasonable clarity that continuing a particular line of research is likely to produce information that might be turned to evil ends, should he continue, or should he stop? When phrased in this way, the question poses a personal choice, but only a personal one. A particular scientist can avoid personal responsibility for findings that may be used for evil purposes. But he cannot prevent those findings from being made. If he stops, someone else will continue.

Among the several answers made to these questions is the statement that the scientist plays two roles, one as scientist and the other as citizen, and that he can and should keep the two roles separate. The distinction goes beyond saying that scientists should be concerned with the social implications of their work to say that the scientist, acting as a scientist, can press on wherever and as far as his curiosity and ability lead and permit, and that the same person, now acting as a citizen, can forget his scientific interests in helping to make decisions concerning science and its applications and its control. This is a comforting doctrine, but is it any more realistic than to expect the scientist to open all the bottles to see what they contain while the same person, as citizen, leaves firmly stoppered any that contains an ugly jinni?

Quite aside from the impossiblity of undoing the past, and quite aside from the impossibility of preventing others from doing what a particular person refrains from doing, can we expect the scientist—not an idealized abstraction but the human being in the next office—to differentiate his role as a scientist from his role as a citizen? We do not expect the clergyman to forget his cloth when he goes to vote. Nor do we ask the member of another profession to stop and ask himself: Am I acting as a member of my profession or as a citizen of my country? What can we fairly ask of a scientist?

Would you put the jinni back into the bottle if you could? The question can start a lively discussion. It can also lead to a perplexing consideration of whether or not the scientist can separate his roles.

The Moral Sense of the Scientists

The recent meeting of the American Association for the Advancement of Science provided an impressive body of evidence that many scientists now are indeed worried about their social responsibility. The announced theme of the week's sessions was "How Man Has Changed His Planet," and the phrase provided far more than a take-off point for bragging. It was a symptom of the unease that permeated the meeting.

Thus Thomas F. Malone warned one session that the possible consequences of weather modification must be weighed "before we are called upon to deal with them." Malone, vice president of the Travelers Insurance Company, told his audience: "The point is that there is still time for reflective thought, for setting objectives, for weighing alternative courses of action—in short, to act responsibly."

In the kind of exhortation that had telling effect on its audience but could earn little space in newspapers, Malone went on: "If the exploration of weather modification adds one more small brick to the edifice that contains world conflict and supports world order, science will have served a noble purpose by enriching human life. The burden of responsibility for seeing that this happens is, I believe, on scientists."

It was not only the prospects of man's modifying weather, however, that aroused concern Other aspects of man's effects on his environment—notably air and water pollution—also stirred it up.

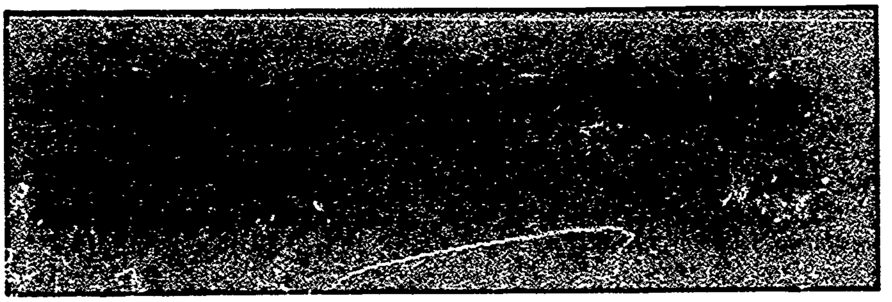
Questions from the audience at a session on pest control, for instance, indicated widespread worry about the use of chemical pesticides whose residues last a long time, such as DDT. The questioners were looking for the kind of assurance they got from George L. Mehren, Assistant Secretary of Agriculture, that most Government research money in pesticides—the 1966 figure was 79 percent—is now going into non-chemical means.

The impact of science on man's social environment drew concern, too, as the sessions on the races of humankind showed. The most heated area of dispute was on the question of how scientific inquiry would do least to feed the fires of racial animosity. One school held that the best thing to do was stay away entirely from investigations of the differences between the races, which one scholar labeled "pseudoscientific"; the other held that inquiry should go forward but that researchers have the obligation to denounce erroneous interpretations drawn from it. Geneticist Theodosius Dobzhansky, an exponent of the latter argument, added: "And in our world a scientist has no right to be irresponsible." The audience applauded his sentiment.

But exactly what is the scientist's responsibility in the matter of racial differences? The day of arguments produced no consensus.

Nor were those attending the meeting allowed to forget the historical examples of how science had hurt, rather than helped, mankind. Loren C. Eiseley, a historian of science, taxed the 19th century's evolutionists with characterizing races other than those of Western Europe as inferior, rather than simply different. The tags have persisted, he noted.

And Lynn T. White, Jr., another historian, argued that "both our present science and our present technology are so tinctured with Christian arrogance toward nature"—the attitude that it exists for the service of man—that "the remedy must also be essentially religious " Science and technology, he said, cannot answer all the questions they raise.



A few decades ago, most scientists held the view that their principal duty was to advance the frontiers of knowledge. Correspondingly, the scientific societies limited their activities to publications and meetings centered on their chosen fields. During the past few years, the activities of scientists have expanded. Many of the principal symposiums at the recent Boston meeting of AAAS dealt with public policy aspects of science and technology.

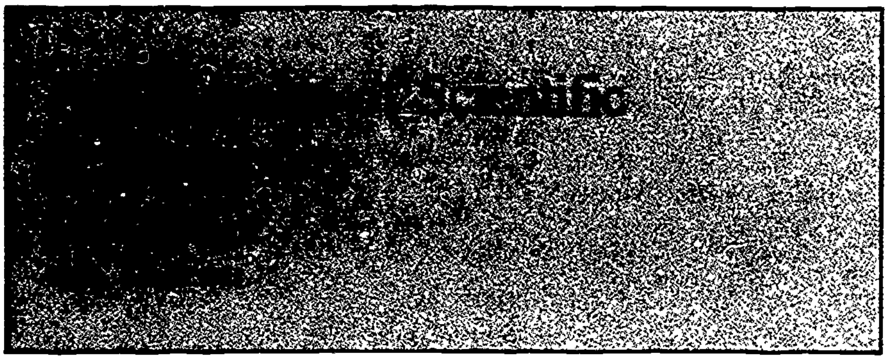
Scientists have not unanimously approved participation in policy matters by their colleagues. Some have objected that spokesmen certainly did not speak for them personally. Others have pointed out that once facts have become generally known, the scientist can no longer determine how his discoveries may be applied. To some degree, this argument is valid. Nevertheless, scientists will have continuing and important roles in determining how science is applied. One important function is that of watchdog.

In exploiting scientific discoveries, humanity will squander resources and unwittingly conduct profoundly important experiments on itself and on the environment. Who will evaluate such experiments and be alert to emerging problems? The man in the street can scarcely fill such a role. Government might, but its leadership is in the hands of politicians who rarely act until an issue is crystallized by others. Scientists or engineers in government service might act as watchdogs, but in general, politicians prefer that the bureaucrats speak only when spoken to. Employees of industry are in much the same circumstance. Thus academic scientists and the scientific societies have responsibilities that they cannot escape.

In attempting to convert opinion into action, scientists should avoid internal conflict. They form only a tiny fraction of the electorate, and at best their prestige is not such as to give much weight to partisan exhortations. In matters that are more political than scientific, members of societies are likely to be divided in their preferences. When a society attempts to achieve a monolithic position on such issues, it does so over strong objections. The outcome convinces no one, serves little purpose, and leaves debilitating wounds. The societies are more effective when they employ leverage furnished by other opinion makers. During the past decade, AAAS has met this challenge by providing forums in which technological problems that affect all of us were discussed. These presentations have been well covered by the mass media.

After the mass media begin to devote attention to a problem, public awareness increases, and politicians become interested. However, in helping to create judicious public opinion, the scientific societies can have an important role. Especially useful are fact-finding commissions and committees. Thus the Air Conservation Commission of AAAS served a valuable function in early delineation of facts concerning air pollution. Reports from committees organized by the National Academy of Sciences have been helpful in crystallizing public opinion on such issues as birth control. In general, the reports have had an effect roughly proportional to the level of scholarship and objectivity which characterized them

The goal of opinion-making should be constructive action. A prerequisite for this is thorough planning based on an adequate fund of knowledge. Scientists can make imaginative contributions to planning, and they can help ensure that the factual bases for decisions are as sound as possible.



Of all the traits which qualify a scientist for citizenship in the republic of science, I would put a sense of responsibility as a scientist at the very top. A scientist can be brilliant, imaginative, clever with his hands, profound, broad, narrow—but he is not much as a scientist unless he is responsible. The essence of scientific responsibility is the inner drive, the inner necessity to get to the bottom of things: to be discontented until one has done so; to express one's reservations fully and honestly: and to be prepared to admit error.

—ALVIN WEINBERG (*1*)

I agree with this assessment of the central role of scientific responsibility but not in all respects concerning what constitutes responsible behavior in some difficult situations. There are two major kinds of scientific responsibility. There is the pattern of responsible behavior that is associated with basic research and the communication of the results. And there are the problems that arise when scientists deal with issues involving social responsibility—such matters as the control of nuclear and other weapons, the uses and hazards of toxic chemicals and radioactive materials, the choice among various modes of producing or conserving energy, or the criteria for deciding whether to dam a river or let it flow freely. These are very different problems from those involved in basic research; the decisions reached involve value judgments. They are, and indeed should and must be, political decisions. Nevertheless, applied scientific knowledge is an important element in the making of such decisions. Scientists who enter these disputed areas encounter problems of responsible behavior that are considerably more complex than those of the scientist who is working out basic problems in the laboratory, or in thought and calculation. However the two areas also have much in common, and the problems of social responsibility cannot be considered properly without keeping in mind the general code of scientific behavior that has evolved over the last few centuries.

The pattern of conduct that has developed in basic research serves to maintain what Robert Merton called the ethos of science (*2*). It involves the acceptance or rejection of reported findings of other workers on the basis of what Merton terms "preestablished impersonal criteria," and the public presentation of scientific findings (usually, and preferably, after critical review by editors and referees) so that they are available to the whole community. It also involves the social system of "organized skepticism" that subjects reported findings to constant critical review, with no assurance of finality. Scientists are expected to point out the limits of uncertainty in their findings and the inferences they draw, and they are expected to acknowledge their debts to others whose work, both published and unpublished, has contributed to what they have achieved. Science is a communal enterprise: every contribution builds upon the work of others.

This is an idealized picture. Acknowledging the debt to other workers is indeed central in the ethos of science, yet it would be intolerable to cite a massive set of references for an ordinary paper. Aggressive scientists are sometimes skillful in getting credit for ideas that others may have published before, but they may also be genuinely ignorant of the earlier work. Even those who are quite scrupulous may pick up ideas from papers for which they serve as referees, or from serving on a panel that reviews grant applications, and they may remain quite unconscious of the source of their ideas. Since recognition of significant originality in discovery is the main road to scientific prestige and honor, most scientists are understandably sensitive to the failure of others to acknowledge their work. A few unusual people are dramatically different: they cast forth their ideas freely, and are happy to see others pick them up. This is what Jacques Monod (*3*) wrote about Leo Szilard:

> Most scientists of course do not formulate any significant new ideas of their own. The few that do are inordinately jealous of, and unduly faithful to, their own precious little ideas. Not so with Szilard: he was as generous with his ideas as a Maori chief with his wives Indeed he loved ideas, especially his own. But he felt that these lovely objects only revealed all their virtues and charms by being tossed around, circulated, shared, and played with.

I am not an anthropologist and cannot claim knowledge of how Maori chieftains share their wives, but Monod's description certainly characterizes Szilard and other unusual individuals.

The pursuit of knowledge in basic science is inevitably full of rivalry and competition, especially in the fields that are most active, but it usually proceeds in an atmosphere in which there is a great deal of free communication of ideas and active discussion. When obvious major practical results begin to appear, a trend toward secretiveness usually sets in. The most dramatic example is the effect on physicists of the discovery of nuclear fission and the secrecy that followed. More than one distinguished physicist has recalled nostalgically the intellectual freedom of exchange in physics in the years before 1939. A somewhat similar change appears to be taking place among the molecular biologists today, as the techniques of gene cloning hold forth the promise of manufacturing substances of great biological importance, cheaply and on a large scale. Some of my younger colleagues have told me that they find scientific meetings less interesting than they were, even 5 or 6 years ago: too many people, they say, are clearly holding back information, presumably with an eye to applying for patents on new processes. There have even been charges that some authors of reports are deliberately failing to cite relevant work of others in hopes of claiming a patent on some new biological process or product.

This competitive atmosphere has sometimes led to publicity of a sort previously not practiced among scientists. In a recent article entitled "Gene cloning by press conference," Spyros Andreopoulos (*4*) of the Stanford Medical Cen-

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ter News Bureau quotes a letter from Joshua Lederberg to Senator Gaylord Nelson of Wisconsin. "The possibility of profit—especially when other funding is so tight—will be a distorting influence on open communication and on the pursuit of basic scholarship," Lederberg wrote, although he noted that many, perhaps most, university scientists disagreed with his views. Andreopoulos showed that some new developments announced at press conferences receive wide publicity, before they appear in the scientific literature, while other work of at least equal significance passes through the regular channels of critical reviewing before it appears. Reports at press conferences can be misleading; for example, one new account of the production of human insulin by recombinant DNA techniques created the impression that the product was biologically active; the later publication of the data in a journal showed that this was not so (*4*).

The traditional patterns of scientific reporting and communication—the scientific ethos, in Merton's phrase—may be in danger of undergoing significant erosion. As a believer in the classical tradition of operation in basic science, I hope that the erosion may be halted.

Independent Scientists and Issues of Public Policy

A more difficult subject is the role of scientists in matters of public policy. Let me begin with a classic example from nearly 20 years ago: the publication of Rachel Carson's *Silent Spring* (*5*) with its vigorous attack on what she considered the gross misuse of pesticides. She was both a trained scientist and a gifted writer. The biological community had been concerned about the ecological damage from widespread use of pesticides such as DDT, but no authoritative body had made a critical study of the problem and publicized its conclusions. The book had an immense impact. It was also attacked by many agriculturists and nutritionists, who called it misinformed, fanatical, or even a hoax. The President's Science Advisory Committee, however, took Carson's charges seriously and set up a special panel of experts to investigate the problem. After 8 months of hearings they produced a report (*6*) that in large measure vindicated Carson's claims and also concluded that massive attempts to eradicate certain insects by pesticides were unrealistic and ecologically dangerous and that "elimination of the use of persistent toxic pesticides should be the goal." President Kennedy released the report in May 1963 and requested the responsible agencies to implement its recommendations.

The pattern of subsequent events is complex; but it would not please either the strong supporters or the fervent opponents of chemical pesticides. Some strong controls have indeed been imposed; DDT, which was the principal focus of Carson's attack, has been banned; but the general use of chemical pesticides in agriculture is probably as widespread as ever, if not more so. Many of the current pesticides are more toxic to humans than DDT. Other poisons, such as the polychlorinated biphenyls (PCB's), used in industry rather than in agriculture, have been recognized as serious environmental hazards. Highly specific pesticides for particular species of insects, such as the juvenile hormones, have been developed but as yet have found little practical use.

Mention of Carson's book can still rouse both enthusiasm and denunciation. Undoubtably in some respects she exaggerated the damage done by pesticides. My own view is that, on balance, she performed a great public service and deserves to be remembered with honor. Certainly the sense of responsibility for the environment that she inculcated is now implanted in a vast number of people.

This episode exemplifies many of the problems that scientists encounter when they become involved in issues of social responsibility. Carson was a trained scientist, but not in the field of agricultural ecology. She had much to learn, and she did learn, in the process of preparing to write the book. The agriculturists still did not regard her as a real professional in their field. However, many, if not most, of the agriculturists had financial and career ties to the use of pesticides and to the industries that produced them. The committees of the National Academy of Sciences that dealt with such matters in those days tended to be dominated by people who had similar biases. The Academy has changed and now examines systematically the industrial and other connections of the members of its committees. The aim is not to eliminate all people with possible bias—that would eliminate most of the experts, in some fields at least—but to obtain a balanced spectrum of people with different kinds of bias, together with some who might be genuinely dispassionate in considering the issues.

Since nearly all controversial issues of this sort involve technology, as well as basic science, the disputes cannot be resolved in terms of "preestablished impersonal criteria." Scientific facts and value judgments are so closely interwoven that it is exceedingly difficult to disentangle them, and the inferences to be drawn are inconclusive. Scientists can honestly disagree as to what inferences can legitimately be drawn from the facts.

Thus we are operating in a quite different domain from that of basic science. The Federation of American Scientists (FAS), which addressed this problem (*7*), accepted as inevitable ". . . that scientists involved in public debate will have to go beyond discussing what is scientifically known for certain," since public policy matters involve the making of decisions in the face of enormous uncertainties. At the same time, the FAS report said that scientists who take an active part in public debates should avoid dogmatic claims, be willing to admit and correct errors in their statements, and reason with those with whom they disagree. However, the report concluded that professional scientific societies are generally unqualified to monitor and pass judgment on the conduct of scientists involved in such debates. The societies are accustomed to dealing with more traditional patterns of conduct within the scientific community and are unequipped to deal with the far more unruly debates that arise when social and political questions are involved. It is the community of scientists who do take an active part in public debate on these controversial issues who must work out appropriate guidelines for responsible conduct. As the debate proceeds, it will become clear who the scientists are who are speaking responsibly and with due respect for the facts.

Weinberg (*1*) holds that the essential sense of responsibility is being eroded in the current debates on such matters as energy policy and environmental protection, with scientists making sweeping pronouncements on issues far outside their own fields of competence. He believes, for instance, that a scientist who thinks he has evidence that current standards of environmental protection are too lax should submit his findings to a refereed scientific journal before publicizing them. If the journal rejects the report, the author may honestly believe that the reviewers are biased. In that case he may be justified in bringing the matter before the public, while admitting that others disagree with him.

There are many cases in which such a procedure will help bring more rationality into the debate; but scientists discussing public issues are often involved in public discussions, or interviews on television, where the limited time makes it impossible to state all the reservations that a careful scientist might add to qualify his remarks. In the heat of debate there is also the tendency to overstate the case. Politicians and others would like simple answers to complex questions. Certainly scientists should be prepared to state publicly that they have made erroneous statements, and correct them; on this vital point there is no disagreement between Weinberg's position and that of the FAS.

Among the value judgments involved in these controversial issues, a fundamental difference of view is often present. If, for instance, the evidence is inconclusive about the toxicity of some industrial product, should it be banned until it is proved safe or used until it is proved dangerous? Until the last two or three decades, the latter policy was most commonly accepted. Recently the more cautious policy has prevailed; the increasingly severe standards for the licensing of drugs by the Food and Drug Administration represent perhaps the most striking example. Such caution has its penalties as well as its merits; for example, Carl Djerassi (*8*) pointed out the difficulties in the development of new and better contraceptives that the strict rules of testing have imposed. Sometimes more is lost than gained by excessive zeal in testing before release. This is likely to be true for the selective pesticides that act by inhibiting the development of certain species of insects.

Decisions on such matters as building an airport or a power plant, or damming a river, inevitably involve value judgments as well as technical facts. They require estimates of future needs, which are often highly unreliable. For example, the estimates made a decade ago about future needs for electric power in the United States have been drastically scaled down in the light of experience. Expert testimony in such matters is likely to be colored, consciously or unconsciously, by the expert's system of values.

Cost-benefit analysis in such situations of conflict is a treacherous game; the costs and benefits are usually quite incommensurable; ultimately decisions are likely to be made by the political process in which the public perception of what is desirable counts for more than the cost-benefit calculations of experts. Lord Ashby (*9*) concluded that it is probably better so:

> All attempts to rely on quantification in such decisions as these, to create them out of computer scenarios, to deduce them from cost-benefit balance sheets, are likely to make the decision worse, not better; for in the process of getting hard data, the fragile values, the unquantified information, the emotive elements which nourish the public conscience, all run through the filter and are lost, and so the quantified information assumes an importance out of proportion to its real value.

Whistle-Blowing and Professional Responsibility

Scientific and technical professional employees, in industry or government, on occasion have reason to sound warnings of dangers about processes or products, or sometimes to call attention to opportunities for improvement that they believe are being neglected. Obviously employees should approach their superiors, point out the source of trouble, and urge correction. If the superiors fail to respond and the issue is really serious, the employee can bring it before the public. People who do this are commonly called whistle-blowers (*10*).

Whistle-blowing is obviously a high-risk occupation, and those who practice it must be prepared for trouble. A classic example arose during the building of the Bay Area Rapid Transit (BART) system in San Francisco (*11*). A major feature of the system was the automated train control, developed under a contract with the Westinghouse Corporation. Three engineers, Max Blankenzee, Holger Hjortsvang, and Robert Bruder, concluded that the system design had grave defects, but their concerns were disregarded by the management. Finally, early in 1972, they went to BART's board of directors which, after a hearing, voted 10 to 2 with management. The three engineers were fired. Subsequent dangerous failures of the automated train control, which occurred after the system started to operate, fully vindicated the engineers. The California Society of Professional Engineers investigated the case and decided that the dissenting engineers "had acted in the best interest of the public welfare." The California legislature conducted an investigation that confirmed the validity of the engineers' warnings. The three then sued Westinghouse for $885,000 but eventually settled out of court for a relatively modest sum, which was probably quite inadequate compensation.

A more recent case involved Clifford Richter, a health physicist at a state hospital in Columbia, Missouri (*12*). He reported certain violations of safety regulations at the hospital to the Nuclear Regulatory Commission, as he was in duty bound to do by law. The hospital management retaliated by abolishing his job. A federal court eventually ordered his reinstatement, under the employee protection section of the Energy Reorganization Act, and the payment of back salary. The reinstatement has been challenged, however, and appealed to the U.S. Supreme Court.

In another case, Morris Baslow, a marine biologist, was fired after he presented evidence, in a court hearing on a U.S. Environmental Protection Agency inquiry, concerning the effects of cooling water from power plants on fish in the Hudson River (*13*). He urged his employers to present the evidence, but when they ignored his recommendations, he finally presented the data to the court directly. Eventually he reached an agreement with his former employers, but only after many months of delay, while he was out of work.

In these cases the whistle-blowers put their jobs and reputations in jeopardy. It is obviously in the interest of public health and safety that such people should be heard and fairly judged; and if their views are upheld after a hearing by a suitable body, they deserve commendation, and perhaps promotion, not discharge. Congress has passed several laws in recent years to protect the rights of employees who report to their employers matters that call for correction. The Nuclear Regulatory Commission is now formulating rules that should encourage employees to report matters of concern to higher officials, with guarantees against reprisals, whether or not the employee's recommendations are accepted. This represents an encouraging trend in the Executive Branch of the government. Rules to protect employees are still nonexistent in most private businesses, though a few firms have begun pioneer moves in this direction. David Ewing of the Harvard Business School has outlined detailed proposals for further reform (*14*).

Of course whistle-blowers are not always right. They might be motivated by personal malice, they may be cranks, or they may be honest, but mistaken. Both common sense, and a sense of loyalty to the employer, dictate an earnest effort to settle differences of opinion by working within the organization. However, if higher authorities fail to respond, and if the matter appears to involve serious issues of human safety and health, it may be necessary to bring the matter to public attention. The individual who takes such a risk obviously needs good legal advice and other kinds of help (*15*). Our complex society needs increasing input from those who perceive otherwise unnoted risks or opportunities and bring messages that may be unwelcome to established authorities. To use criticism and dissent constructively in dealing with both risks and opportunities, clear policies are needed, with definitions of procedures for due process in controversial cases and, if necessary, formal hearings and a possibility of appeal.

The polarization of opinions on some issues today is disturbing. The conflict between the advocates and enemies of nuclear power is one example; the dispute over the origins of cancer is becoming another. Richard Peto (*16*) described the distortions and untruths promoted by tobacco companies in their efforts to discredit the overwhelming evidence for the relation between smoking and lung cancer. At the same time he severely criticized some of the alleged evidence that would ascribe nearly all cancers to toxic substances introduced by man. S. S. Epstein, whom Peto sharply criticized, has responded vigorously (*17*). The gravity of the hazard from industrial carcinogens, to workers and others, is clear; but their relative role in the totality of human cancers is still hotly debated. In the bitterness of such controversies, either side may distort data. As Peto remarked, "Scientists on both sides of the environmentalist debate now have career interests at stake." But it is important above all that the passion for getting at the truth should be the dominant passion for scientific workers when they are trying to act as responsible scientists. That may appear sometimes to be an unattainable goal in the atmosphere of current debate, but it is worth striving for, both to maintain the confidence of the public and to keep confidence in ourselves.

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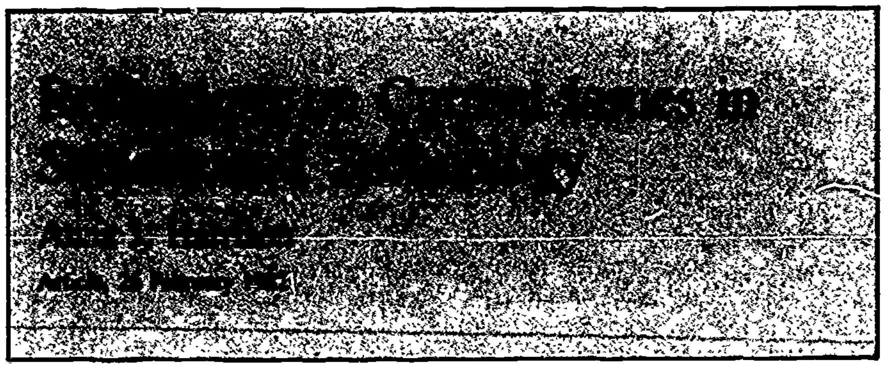
18. I am much indebted to my fellow members of the AAAS Committee on Scientific Freedom and Responsibility for our continued joint work relating to many of the problems discussed in this article I am particularly indebted to R. Baum, F. von Hippel, J. Primack, and R. Chalk. This article is, however, an expression of my personal views. This work was supported by grant SOC7912543 from the National Science Foundation.



It has been the custom for *Science* to publish at this season of the year a biography of the incoming president-elect of AAAS. I have bargained instead with the Editor to use this space to explore with you some of my interests and prejudices, hopes and fears, related to science, technology, scientists, society and, of course, AAAS. The only biographical information that is of significance to you is that I am a physical chemist by training with a modest track record in research and long experience in a liberal arts college that takes science, research, and the development of its students seriously and that I have served the larger community of scientists and society as a member of the National Science Board, 1972–1978, and president of the American Chemical Society, 1978.

Through cooperation with its approximately 300 affiliated societies, AAAS has unique potential to serve an integrative function in issues of general interest and concern to the scientific community. The focus here shall be primarily on some of those general issues: the integrity of science, the impact of science and technology on society, the impact of society on science and technology, the culture of a realistic environment for science and technology, the participation of scientists and scientific societies in public decisions, and the culture of scientific manpower. It is not important whether you agree or disagree with my perceptions. It is important that we seek to understand our profession in its current context and to take actions with due regard to the future.

Surely all of those who read *Science* understand the integrity of science and are diligent in their efforts to ensure it is not violated. However, observations of what we do and what we say suggest that we are not acutely aware that science is two things, process and the body of knowledge generated by that process, and we may not appreciate that to understand the integrity of process is to understand the integrity of the body of knowledge generated.

By process, I mean all of those things that are involved in the design of experiments or programs of observations, the execution of these experiments or programs, the processing of information and assessment of the validity of results including models that may have been generated. What are the assumptions on which the work is based? What are the assumptions and approximations introduced in the generation of models? Scientific studies have been extended to

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more and more complex systems using more and more complex methodologies, higher and higher levels of sophisticated instrumentation, and more and more prepackaged computer programs. We have continuously expanded our capabilities and productivity and, in so doing, we have compounded the problems associated with the assessment of the validity of the product. By process, I also include the scrutiny, testing, and revision to which scientific knowledge is continuously subject.

It is generally accepted that the free exchange of scientific know-how and scientific knowledge is a basic tenet of the scientific community. At the same time, it is well known that an individual may create barriers to retard the flow of information within an institution, that an institution may create barriers to inhibit the flow of information to other institutions, and that a nation may regulate the exchange of information with another nation. Herein lie very thorny issues, the resolution of which has a great deal to do with the environment in which the scientific community proceeds. Actions to restrict flow of information reflect the perceived short-term advantage of proprietary scientific know-how and knowledge but discount our mutual dependency and the long-term benefits of the free exchange of scientific knowledge. The long-term costs of mistrust and stagnation are significant indeed. Three types of situations that justify careful consideration are the relation of national security considerations to the free exchange of scientific information with Soviet scientists, the relation of the proprietary interests of industry to the free flow of information within and from academic laboratories, and the impact of the practices of individual scientists upon their colleagues and students.

If we are to understand the environment in which we work, we must understand the impact of science and technology on our intellectual perception of life and the world about us, on the quality of life, and on the quality of the environment in which we live. Undoubtedly, the greatest impact of science and technology is on the first. I suspect that those magnificent photographs of Earth from space brought about an irreversible change in our perception of our planet and its resources and this changed perception broadened and intensified the environmental protection movement.

I suggest three premises:

1) every technological innovation, regardless of how great its positive impact on society, also has a negative impact on society,

2) the benefits and the negative impacts may be experienced by different subsets of society, and

3) the benefits and the negative impacts may be experienced in different time frames.

The term "benefit/risk analysis" is consistent with the first premise, and historians tell me that this sweet/bitter consequence of technological innovation is a characteristic of all social, economic, and political change. Some of the long-term consequences of technological innovations can be surprising. Spectacular advances in medical technology have enabled us and our descendants to live longer: two consequences are the escalation in the rate of consumption of resources and the escalation in pollution. We must be alert to the total consequences, both positive and negative, both short-term and long-term, and be willing to seek courses of actions to minimize and more equably distribute the negative consequences of technological innovations.

In a democratic society, it is the public, through its surrogates, that has the right and responsibility to make decisions in matters concerning the quality of life and the quality of the environment. Such decisions involve value judgments. Many other value judgments, internal to industry, determine the course of innovations. There has been some tendency to believe that if we all understood the science involved, we would all make the same decisions concerning the use of science and technology. This is, of course, not true as has been amply demonstrated in regard to the use of nuclear power technology. Equally informed individuals may make entirely different value judgments and take quite different positions in regard to a particular technology. Science education clarifies technical matters but it is not a route to unanimity in decisions involving value judgments.

If we are to understand the environment in which we work, we must also understand the impact of society on science and technology. It is evident that

1) the direction and the rate of extension of scientific knowledge are to a large degree determined by social, economic, and political factors, and

2) the direction and the rate of development of technology are to a large degree determined by social, economic, and political factors.

The allocation of public monies to the support of basic research is an act of faith that the research results will, on balance, enhance our national image, lead to the development of goods and services essential to the public welfare or at least to the benefit of some influential subset of society, or expand our intellectual perception of ourselves and all that surrounds us.

The allocation of funds among the various disciplines and within the disciplines is very significant in the determination of both the direction and the rate of extension of knowledge. Herein lie thorny issues. If the scientific community cannot or will not provide the leadership in establishing relative priorities, congressional committees and private foundations will, by default, set priorities among the competing interests with probable long-term disadvantage to all. We must plan for new thrusts and the development of promising innovations. Inequities are highly probable. Some disciplines are more skilled than others in presenting a united front, and funding for some disciplines is much more strongly challenged than for others by individuals who may minimize the potential of those disciplines to contribute to the public good and may, to some degree, fear the extension of knowledge in some areas.

The characteristics of the marketplace, patent laws, tax structures, tariff barriers, and a host of regulations are manifestations of the social, economic, and political forces that shape the development and productivity of technology and the research supported by industry.

I am convinced that a realistic environment for science and technology is essential for the welfare of the nation and that the entire scientific community should devote its best efforts to the culture of such an environment. I propose that the basic elements for a realistic environment are:

1) the public understanding of the powers and limitations of science and scientists,

2) the public understanding of the powers and limitations of technology and technologists, and

3) the public understanding of the processes involved in making public decisions concerning science and technology.

The word public is used here to encompass all of those who have the right to influence public opinion and participate in political processes. This, of course, encompasses the scientific community.

It is my impression that some time in the past, either the scientific community oversold or the public overbought science and technology. There are questions that science cannot address and things that science and technology cannot accomplish. We cannot guarantee a zero-level concentration, produce a risk-less technological option, or solve societal problems. We can alleviate a societal problem such as waste management by recycling, conversion to less toxic or more manageable materials, and designing containment facilities and special-purpose incinerators, but it is the public, through its surrogates, that makes the social and political decisions.

I believe that to understand the integrity of the process of science and the integritv of the body of knowledge that the process generates is very close to understanding the powers and limitations of science and scientists. I am delighted to see that an increasing number of TV science specials focus on process and suggest that we should rethink the goals of science education, particularly science education at the precollege level. It is my experience that individuals who have some concept of process can absorb new knowledge without great difficulty but that individuals who have no concept of process have great difficulty absorbing new knowledge and are incapable of being rationally critical of positions presented as having scientific validity. It is quite possible that those who have had experience with science and those who have not perceive our world quite differently and that this difference in perception is greater than we realize.

The American public knows remarkably little about technology and the steps involved in technological innovation. There are a few instances in which the mass media have indicated a potential to rectify this; there are also groups endeavoring to define goals and develop mechanisms to introduce technology into the precollege educational system and the liberal arts components of college and university curricula. All of these will require time to make a significant contribution. To me, the first step in the culture of a realistic environment for technology is to recognize that each technological innovation has the potential to have both positive and negative impacts on society and then get on with the public discussion of whether the total benefits justify the total risks. Total impacts encompass a wide range of social, economic, and political changes the consequences of which are extremely difficult or impossible to evaluate quantitatively with confidence.

If value judgments concerning the impact of science and technology on the quality of life and the quality of the environment and the consequent decisions are the prerogative of the public, what is the role of scientists and professional societies? This is another thorny question to be addressed by each scientist and each professional society. In most areas, I am quite clear on my own position. First and foremost, a scientist is an individual and has the same responsibilities and privileges as any other individual. This includes the right, as an individual, to espouse values and join others in political action.

Scientists, either individually or collectively, as scientists have the responsibility to provide technical expertise based upon training and experience and to endeavor to provide that expertise in a manner comprehensible to those who need the information. In the role of experts, scientists do not have the right to make a value judgment and then selectively present scientific information to support that value position. To do so is to negate the integrity of science.

Two types of situations are particularly troublesome. In one, rational decisions require technical information that is not available and cannot be generated through validated studies in the time available: in the other, scientists are asked to propose courses of action to solve societal problems. Both go beyond the technical expertise and, in some cases, way beyond the level of maturity of the science.

There are two choices. One is to refuse to go beyond technical competence and the other is to provide what I call informed judgment, an opinion based upon related knowledge and experience. The first is to deny the public the benefit of informed judgment, the second may jeopardize personal creditability and possibly the creditability of the discipline. In both, the scientist has responded to a request to go beyond his or her level of expertise, and the difference between opinion and knowledge must be established in the response.

The culture of scientific manpower is fundamental to everything we do. The term is being used here in a very comprehensive sense to include the entire professional life-span of scientists as well as the recruitment and early development of scientists, but space is limited and I shall simply propose that

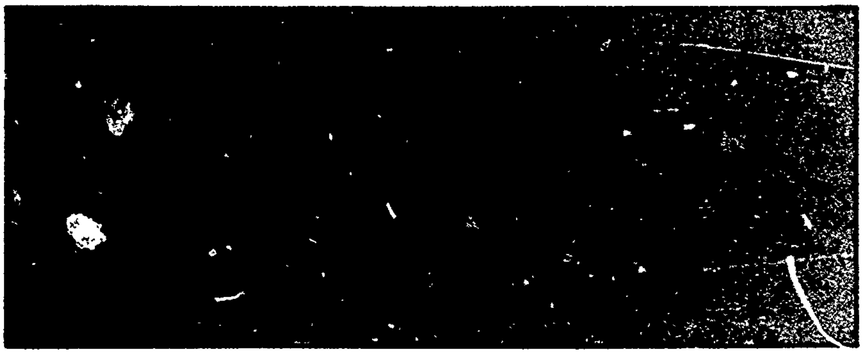
1) current science education distorts the recruitment and development of scientists,

2) the current reward system within the profession distorts the distribution of scientific talent within the profession,

3) and both distortions are to the detriment of the scientific community and the public's well-being.

If academic education provides the environment that enables a student to accomplish something, then we must rethink what it is students should be enabled to do. I nominate for the top of the priority list "discover the integrity of science."

In conclusion, attention is called to some of the more visible AAAS activities related to issues touched on above: the annual review, in collaboration with a number of affiliated societies, of the federal budget for research and development, the consortium of affiliated societies involved in international programs, the activities of the committee on scientific freedom and responsibility, the publication of *Science 82* for the reading public, and the recent AAAS long-term commitment to the improvement of science education.



The American Association for the Advancement of Science is a unique organization. AAAS encompasses more than 135 thousand individuals involved in the investigation of physical, chemical, biological, behavioral, social, economic, and political phenomena and the use of physical, chemical, biological, behavioral, social, economic, and political knowledge to achieve specific ends. An inspection of AAAS activities indicates a strong commitment to expansion of scientific and engineering knowledge and utilization of the capabilities of science, engineering, and technology in resolving societal issues and thus in enhancing the quality of life of this and succeeding generations—commitments not limited by national boundaries. AAAS forums effectively address multidisciplinary matters of concern to scientists and engineers and also societal issues of concern to scientists, engineers, and the general public.

In recent years, I have become increasingly aware of the confusion within the public, and also within the scientific community, as to the nature of science, engineering, and technology and the relation of science, engineering, and technology to society. To me, there are common elements and interconnections within science, engineering, and technology that are attractive and compelling. In this article, I explore these relations and also the relation of science, engineering, and technology to society. In avoiding the constraints of a language that evolved in less scientific and technical times and of terms that have acquired prejudicial and devisive connotations, the wording has to be, at times, both detailed and repetitive.

My approach is both pragmatic and simplistic. It is not important whether you agree or disagree. It is important that we make the effort to explore relations among science, engineering, and technology and their relations to society. In this simplistic approach, science, engineering, and technological innovations are approached as processes of investigation, each generating a body of knowledge that consists of a data base, an array of methodologies, and an array of concepts.

Science, Engineering, and Technological Innovation as Processes of Investigation

Science is the process of investigation of physical, chemical, biological, behavioral, social, economic, and political phenomena. Process is used in the collective sense to include everything the investigator does from the selection of the phenomena to be investigated to the assessment of the validity of the results. Process includes the selection of the methodology, the choice of instrumentation, the delineation of protocol, the execution of protocol, the reduction of data, the development of constructs, and the assessment of the certainty or uncertainty of the results. The details of process are dependent on the phenomena and the relative significance of observation, experimentation, and theoretical modeling in the investigation. The legacy of the investigation of phenomena is scientific knowledge consisting of a data base, an array of methodologies, and an array of concepts.

Engineering is the process of investigation of how to solve problems and includes everything the investigator does from the acceptance of the problem to the proof of the validity of the solution. Engineering has been primarily concerned with the use of physical phenomena and to a lesser degree with the use of chemical phenomena. This almost exclusive involvement with physical and chemical phenomena is still evident in the structure of many schools of engineering and in the structure of the National Science Foundation programs in engineering. Engineering is now also concerned with biological phenomena, and subdisciplines such as medical engineering, bioengineering, and genetic engineering have emerged. The legacy of investigations of problem-solving is the body of engineering knowledge consisting of a dáta base, an array of methodologies, and an array of concepts.

Technology is the process of production and delivery of goods and services Technological innovation, the activity more parallel to science and engineering, is the process of investigation leading to more effective production and delivery of a good or service, production and delivery of a significantly modified good or service, or production and delivery of a new good or service. Process encompasses everything investigators do from identification of concept to successful production and delivery of the good or service. Here again, technology and technological innovation have been involved primarily with the use of physical and chemical phenomena in the production and delivery of goods and services. Today, there is a rapidly expanding involvement with biological phenomena. A legacy of technological innovation is a body of technological knowledge consisting of a data base, an array of methodologies, and an array of concepts. Technological knowledge may also encompass a body of empirical know-how derived over time through an arts-and-crafts approach to the production of goods and services.

It is true that science drives engineering and technological innovation, but it is equally true that both engineering and technology drive science. The three processes, science, engineering, and technological innovation are synergistic. Each is dependent upon the other two; each supports the other two. It is this synergism that so enhances the total capabilities of science, engineering, and technology. The productivity of this synergism is abundantly evident in the events that have and are propelling us into an information society. In rapidly developing areas of new technology at the forefront of scientific knowledge, the distinction between science and engineering diminishes as scientists investigate how to solve problems as well as investigate phenomena and engineers investigate phenomena as well as how to solve problems. Technology, of course, involves not only scientists and engineers but many others working together within an institutional structure essential to the production of goods and services.

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Scientific, Engineering, and Technological Knowledge

It is the combined body of knowledge derived from the processes of investigation that are science, engineering, and technological innovation that has become a resource of unprecedented value in local, regional, and world affairs. There is no term in the English language to encompass this conglomerate of knowledge, and it may be incorrectly referred to as scientific knowledge. To do so has escalated the erection of barriers to free exchange of true scientific and engineering knowledge among scientists and engineers throughout the world.

The integrated body of scientific, engineering, and technological knowledge is (i) the basis for the investigations of phenomena, problem-solving, and innovation in the production and delivery of goods and services; (ii) in part the basis of our perception of the universe; of physical, chemical, biological, social, economic, and political environments throughout the world; of ourselves; and of relations with others including relations among nations; and (iii) the basis of technological innovation, the production and delivery of goods and services, and the effective use of the products of technology.

The first role, the basis of the expansion of knowledge, ensures the enhancement of the future capabilities of science, engineering, and technology. The second role, the basis of perceptions, provides a background for assessments, negotiations, and decision-making and is a significant component of what is becoming known as the new liberal arts. The third role, the support of technology, promises to gratify, at least in part, the desire for the benefits of the products of technology and the contribution of technology to the economy. This is a promise viewed with cautious optimism by those who fear the negative impacts of technology on the environment and on the quality of life.

Benefits and Burdens of Technology

Society is not home free with the benefits of goods and services and the benefits of economic development. Every technological change, be it by transfer or by innovation and regardless of how great the positive impact on society, also has a negative impact. This is a statement with no proof. I have for some years challenged audiences to cite examples of technological change for which it is not true. The most apt reply so far was proposed by a West Point cadet who suggested the flyswatter.

Some consequences of technological change may be surprising. For example, the great success of medical technology in saving lives and enhancing the quality of life is intensifying many social issues. There is simply so many more of us to consume and to pollute. To recognize this is in no way to imply that efforts in medical innovation should be diminished. It does imply that issues related to high density populations must be addressed simultaneously.

The subset of society that derives the benefits may not be the subset that bears the burdens of technological change. The time frame of the benefits may be quite different from the time frame of the negative impacts. And the magnitude of the benefits and the magnitude of the burdens of technology may be quite different. The sweet-bitter characteristic of technological change is not a unique characteristic of technological change; it is a characteristic of change—of all social, economic, and political change. The goal of technological transfer and technological innovation is to bring about change. The great challenge is to use technological change selectively to enhance the quality of life and to disperse more equably the benefits and burdens of technological change throughout society. This challenge has the potential to unite the endeavors of those in science, engineering, and technology with the goals and endeavors of all society, including, of course, scientists and engineers.

Medicine, Agriculture, and Education

Many endeavors such as medicine, agriculture, and education are in part science, in part engineering, and in part technology. I find it very illuminating to think of them in that way. For example, investigating the chemistry of the brain and its relation to how we learn and remember is science. The endeavor to solving problems of communication and of the development of curricula and curriculum materials is engineering. The schools themselves are institutions of technology delivering services that enable students to expand their knowledge and understanding of the universe and of the past and present aspirations, achievements, and failures of the peoples on the earth. The schools have the institutional structure and problems characteristic of institutions of technology—physical plant, management, work force, product design, and quality control. Viewed in this way, we could compare the productivity of our schools with the productivity of other institutions of technology.

Is it possible that the forces that determine the competitive position of automobiles and other products of technology in world markets are related to the forces that determine the position of children in worldwide testing? If so, should we seek fundamental causes of both with the expectation that the strategies that enhance the position of our technological products in the marketplace may also enhance the achievements of children in the classroom? Do we expect a higher level of commitment and diligence on the part of children in their efforts to extend their knowledge and understanding than we expect of their elders in continuing to extend their knowledge and understanding?

The Resolution of Societal Issues

Some societal issues, such as the perceived potential of products of our technological society such as the chlorofluoroalkanes to diminish the ozone content of the stratosphere and permit more ultraviolet energy to reach the surface of the earth and the perceived potential of the combustion of fossil fuels to increase the carbon dioxide content of the atmosphere and elevate the temperature of the earth, are truly world issues. Other issues, such as malnutrition, disease, acid rain, waste disposal, natural catastrophies, poverty, unemployment, discrimination, and child abuse manifest themselves locally and regionally and can sometimes be resolved locally and regionally. In the sense that the problems are ubiquitous, they also are world issues.

The manner in which societal issues are perceived and the steps undertaken to resolve or ameliorate them are significantly dependent on the available body of scientific, engineering, and technological knowledge. Even so, the scientific, engineering, and technological community, a subset of the public, cannot resolve societal issues. Such issues relate to the quality of life, value judgments are called for and value judgments are the prerogative of the public and surrogates of the public, elected officials and those appointed, either directly or indirectly, by elected officials. Only society can resolve societal issues. Judgments appropriate to one society are not necessarily appropriate to another society nor are they necessarily appropriate to the same society at a later time. The values of society are a continuously evolving characteristic of the culture. The priorities of a society must reflect these values and, at the same time, be responsive to social, economic, and political pressures as well as the availability of renewable and nonrenewable resources. In response to these pressures, priorities may undergo rapid change.

It is essential that decision-makers understand the probable consequences of each available option (including the option to do nothing) sufficiently to make decisions that are consistent with the values of the society. This is as true for positions taken in regard to social, economic, and political negotiations and actions as for positions taken in regard to technological changes involving physical, chemical, and biological phenomena. With increasing reliance on referendums in decision-making at the state and local level and the increasing use of initiatives to bring issues directly to the voter, more individuals are involved in making decisions about such sophisticated topics as land use, resource conservation, waste disposal, the use of nuclear energy, and disarmament.

The roles of scientists and engineers are to identify issues, assess the nature and the magnitude of the issues, identify areas requiring further investigation, propose technological options, assess the probable positive impacts and the probable burdens of each option, and communicate these assessments to the public or the surrogates of the public in such a manner that the assessments can be understood. These can be challenging tasks. There are no generally accepted quality of life indicators, and the practice of using economic indicators is at best a very inadequate substitute—particularly if the data base cannot be disaggregated to monitor identifiable subsets of society in successive time intervals. Economic indicators are valid components of the assessment of the quality of life but in themselves are not sufficient to assess the quality of life. Once the decision is made to implement a particular option, scientists and engineers may have large roles in its implementation and the monitoring of the consequences of the actions taken.

There are great variations in the utilization of physical, chemical, biological, behavioral, social, economic, and political knowledge in the resolution of societal issues. The approach to resolution through negotiation is highly dependent on knowledge and understanding of social, economic, and political structures and priorities of the local communities, states, regions, and nations involved as well as understanding of the relation of proposed solutions to the structures and priorities. The resolution of issues such as child abuse or discrimination in access to education and employment are sought in behavioral, social, economic, and political phenomena. Even though the resolution of issues such as toxic waste and generation of adequate electrical power are sought through the utilization of physical, chemical, and biological phenomena, there are also a host of behavioral, social, economic, and political issues that must be resolved. One of the effective measures in the resolution of the electrical power issue has been self-imposed conservation of electrical energy by the public.

If the social, economic, and political issues are not resolved, it may become increasingly difficult to implement technological options or to use effectively those that have been implemented. Traditionally, the focus has been upon the utilization of physical and chemical phenomena to resolve societal issues; it is not at all clear how adequate has been the incorporation of a more comprehensive body of knowledge in resolving these issues. The current federal R&D budget strongly supports the investigation and use of physical phenomena.

Responsibilities of Scientists and Engineers

Scientists and engineers are united by responsibilities that are uniquely the responsibilities of scientists and engineers. Five such responsibilities are identified below, all of which are addressed with varying degrees of focus and diligence by AAAS: (i) to ensure the integrity of scientific and engineering knowledge, (ii) to facilitate the identification and resolution of barriers to communication among scientists and engineers and to assess to scientific and engineering knowledge, (iii) to maintain the distinction between the roles of a scientist or an engineer as an expert witness and as an advocate, (iv) to endeavor to enable all individuals to extend their knowledge and understanding of physical, chemical, biological, behavioral, social, economic, and political phenomena, of engineering, and of technology throughout their lifetimes, and (v) to ensure freedom from discrimination in education and in employment opportunities related to science, engineering, and technology.

The integrity of knowledge. Any misadventure in the processes of investigation, for whatever reason, compromises the integrity of the knowledge generated and initiates the diversion of resources into nonproductive endeavors. Fraud, the deliberate corruption of process, is antiethical to the standards and practices of the scientific and engineering professions; its detection attracts wide public attention and seriously diminishes public confidence in scientific and engineering knowledge and also diminishes public confidence in scientists and engineers.

As serious as fraud is, its occurrence can, in time, be detected; I am much more concerned about inadvertent misadventures, which I believe are much more prevalent and pose a more insidious threat to the integrity of process and consequently to the integrity of knowledge. The sophistication of modern methodologies, of instrumentation, and of computer capabilities enhances our productivity and also allows the opportunities for misadventure to proliferate. The probability of misadventure is also increased by the movement of scientists and engineers into rapidly developing interdisciplinary fields and into the investigation of increasingly complex systems utilizing a wide variety of methodologies and concepts, some of which may be new to a number of investigators. It is probably more difficult to ensure the integrity of scientific and engineering knowledge today than it has ever been. To test, reevaluate, and revise constitute the ultimate safeguard, but the necessity to identify a high incidence of essentially random misadventures is to be avoided through high professional standards in teaching and in research supervision

Barriers to communication. Barriers to the transfer of scientific, engineering, and technological knowledge diminish the utilization of this knowledge as a base for further investigations, for new perceptions, and for the support of technology, including the effective use of the products of technology. Such barriers are worldwide societal issues. Current electronic capabilities have the potential to enable all the peoples of the world to transfer information at the speed of light. The barriers to access are the cost of information services and the sequestering of new knowledge to protect perceived short-term personal, institutional, and national advantages Scientists and engineers have the responsibility to ensure that serious issues raised by these barriers are addressed as long-term societal issues, with a full assessment of total costs and total benefits associated with the various options for resolving them.

Experts and advocates. The roles of scientists and engineers as experts and as advocates are both honorable, but they are different. Confusion about that difference on the part of scientists and engineers as well as by lawyers and the general public has diminished the credibility of scientists and engineers as participants in the resolution of societal issues.

To be an expert, the individual must have attained and demonstrated competence in the area of expertise, and the individual is obligated to delineate, without prejudice, what is known and to what degree of certainty it is known, what is not known, and what is probably knowable utilizing current methodologies.

The role of the advocate is to advance or defend a particular position or option through the selective presentation of information to support a position or option. Scientists or engineers choose the role of advocate when they make a value judgment in favor of a particular option and support that option over others. This is their right as citizens. On this particular issue they have chosen the role of advocate and waive the role of expert.

Circumstances can cast a scientist or an engineer in the role of advocate though it is not his or her intent to be such. For example, in the adversarial structure of our courts, a scientist or engineer called as an expert witness by one of the contending parties is constrained to present information that is consistent with the arguments of that side of the case, even though the witness knows that there is equally valid information that would be supportive of the other side of the case. This, in my opinion, is demeaning and destructive to the expert witness and, in the long run, destructive to the credibility of our courts. A scientist or engineer called by the court as an expert witness for the court is not constrained to support any argument and can, in fact, serve as an expert witness.

Lifelong education. It is highly probable that most of what an individual knows and understands about science, engineering, and technology 10 or 15 years after terminating the formal academic experience has been acquired subsequent to the formal academic experience. This follows from the rapid expansion of scientific, engineering, and technological knowledge. It is also highly probable that how much an individual knows and understands 10 to 15 years later is highly dependent on the nature of the formal academic experience.

The education of an individual is the consequence of how that individual responds to a great multiplicity of enabling experiences—some provided for the individual and some created by the individual. The great challenge is to enable all individuals to continue to extend their knowledge and understanding of science, engineering, and technology throughout their lives. It is frequently the new developments in science, engineering, and technology that are most relevant to the resolution of societal issues.

It has been my experience that in endeavoring to communicate with legislators, lawyers, business personnel, and journalists, who may have little background in science, it is comparatively easy to bring them up to speed in recent scientific advances if the individual understands the nature of scientific knowledge and the nature of the process of investigation that generates knowledge. In particular, it is essential that the individual understands the uncertainty associated with scientific knowledge and has some concept of probability. Without the understanding, it is very difficult, if not impossible, to use scientific knowledge as a basis for decision-making.

The schools, the museums, and the mass media are in the business of providing enabling experiences. If the public is to keep pace with science, engineering, and technology, scientists and engineers must use their knowledge and understanding of the nature of the changes taking place to assist others to ensure that appropriate enabling experiences are made available to the public.

Discrimination. For society to derive the benefits of the creativity and productivity of the physically handicapped, minorities, and women in the scientific, engineering, and technological professions, it is essential that scientists and engineers be vigilant in ensuring freedom from discrimination in acces to education and in employment opportunities related to science, engineering, and technology.

Conclusion

The coherence of the scientific disciplines, the synergism of science, engineering, and technology, the congruity of responsibilities of scientists, engineers, and the public in resolving societal issues constitutes a tremendous potential to expand knowledge, to protect and improve the quality of the environment, and to enhance the quality of life of all the peoples of the earth. I suggest that the great deterrents to the utilization of that potential are limited commitment to enabling all students, those who do not become scientists and engineers as well as those who do, to have access to meaningful experiences with mathematics and science in the schools and limited commitment to enabling all individuals to extend their knowledge and understanding of science, engineering, and technology throughout their lives.

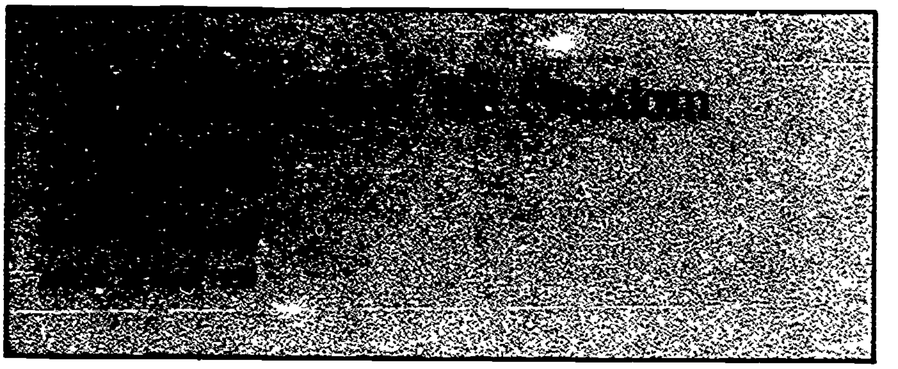
The authors in this chapter explore the relationship between science and freedom in two different ways. The first approach stresses the fundamental importance of freedom of inquiry and the need to protect scientific independence from intrusions by religious or political dogma. The second approach, an almost uniquely American perspective, equates scientific progress with the development of democracy and constitutional traditions. Building on this latter argument, some writers have suggested that science and freedom go hand in hand, believing as they do that only within a democratic form of government can the freedom of communication essential to scientific progress properly thrive.

Various incidents have prompted reflection on the need to protect and strengthen scientific freedom in order to preserve the traditional autonomy and independence of the individual scientist. John T. Edsall, for example, responded sharply to the effects of McCarthyism on the policies of the Public Health Service in the 1950s. In a departure from customary practice, the PHS announced that it would withhold research grants on the basis of supposedly subversive activities by the individual scientific investigator. In a powerful testimonial on behalf of the importance of scientific freedom, Edsall argues that government must support the independent, unfettered investigator who provides leadership in science and protect the conditions under which such work can flourish. Until governmental policies change, Edsall states, he will not accept funds from any agency that denies support to others for unclassified research for reasons unconnected with scientific competence or personal integrity.

No discussion about the principles associated with scientific freedom would be complete without some mention of Galileo. In an address to the Pontifical Academy of Sciences in 1980, Pope John Paul II reviews the details of the Church's actions against Galileo, and invited further scholarly investigation of this incident to dispel the mistrust that it has traditionally introduced into exchanges between the worlds of science and religion. The collaboration between religion and modern science is to the advantage of both, the Pope states, and just as religion requires religious freedom, science legitimately claims freedom to carry on research.

The editorials concerned with freedom and science in this chapter also pursued themes shaped by contemporary events. Writing in the midst of early protests against racial segregation and apartheid, Bentley Glass challenges the scientific community to dedicate itself to the defense of freedom of thought and the independence of scientific inquiry in resisting repressive racist policies. Can there be any question, he asks, that the freedom of mankind — and the freedom of science along with it — are threatened more by the defeats of Little Rock and Pretoria than by the Soviet success in launching *Sputnik?* Joseph Turner, in another provocative essay, resists the trend toward endorsing democratic government as the only form of society in which science can truly advance by suggesting that the overall effects of totalitarian governments on the progress of science have not yet been fully determined. DeWitt Stetten, Jr., examines scientific freedom in the context of public debates over the uses and applications of recombinant DNA research and warns against the dangers of artificial constraints on the progress of science.

How free should scientists be in pursuing their intellectual interests? Is it enough to trust the character of the individual scientist in setting boundaries on the methods and means used in developing scientific knowledge? In presenting the case for freedom in science, the authors in this chapter assumed that the unwritten norms of the scientific community would guide professional behavior. As will be seen in the following chapter, others looked to a formal code of ethics to regulate this freedom. —*RC*



The vast growth of the support of scientific research by Government has given the Government great powers over the careers of scientific investigators. On the whole, these powers have been used thoughtfully and with restraint by enlightened administrators who have worked in close collaboration with the scientists themselves. However, a serious threat to the freedom of the individual and to certain basic rights has arisen lately. Research grants for unclassified research by men of high competence and generally unchallenged integrity have been withheld, or abruptly revoked, because of unspecified allegations of supposedly subversive activities. Therefore all scientists must welcome both the recent request from Sherman Adams, Assistant to the President, to Detlev W. Bronk, that the National Academy of Sciences take the problem under consideration, and Bronk's prompt acceptance of this responsibility on behalf of the Academy *(1)*.

The issues involved are grave; they have aroused widespread concern among the members of the scientific community of the United States Yet there has been little public discussion of the issues and apparently no attempt at explicit formulation of principles We may have confidence that the National Academy Committee *(2)* that is to deal with the problem will face the issues wisely and forthrightly. Yet committees like this one can operate effectively only on the basis of informed and thoughtful opinion diffused generally among American citizens, especially those of the scientific world. As one of these citizens, I have set down the following considerations, formulated gradually during many months of study and discussion with colleagues *(3)*. Certainly I claim no special wisdom or insight. Whatever may be of value in this discussion is only a reaffirmation of principles long formulated and long honored but apparently often forgotten in the stress of the atmosphere of crisis in which we live today.

We must first be explicit with regard to where most of the trouble has occurred More than one Government agency has been involved in actions of the type I am discussing; but the most numerous and most [illegible]ous incidents have been related to research supported by the U.S. Public Health Service. During the last decade, the Public Health Service has established a splendid record of achievement in its program of research grants and fellowships, which have been administered with wisdom, and with respect and understanding for the conditions required by scientific men to achieve the best that is in them. In the early spring of 1954, however, reports began to circulate that grants for open, unclassified research were being revoked or denied, on grounds apparently political and unconnected with the competence or integrity of the investigators involved. A statement of the policy involved was made by Oveta Culp Hobby, Secretary of the Department of Health, Education, and Welfare, on 28 April 1954. This was published at the time in several newspapers and has since been circulated by the Federation of American Scientists. The most relevant paragraphs follow.

> We do not require security or loyalty investigations in connection with the award of research grants When, however, information of a substantial nature reflecting on loyalty of an individual is brought to our attention, it becomes our duty to give it more serious consideration In those instances where it is established to the satisfaction of this Department that the individual has engaged or is engaging in subversive activities or that there is serious question of his loyalty to the United States, it is the practice of the Department to deny support.
>
> If the subject is an applicant, the grant is not awarded. If the subject is an investigator responsible for a grant-supported project or is the recipient of salary from the grant, the grant is terminated unless the sponsoring institution desires to appoint an acceptable substitute
>
> The Public Health Service supports a large segment of medical and related research through more than 2,000 grants which involve some 14,000 persons each year Although this practice has been followed since June, 1952, fewer than 30 persons have been denied support.

We may note that the policy, for which Secretary Hobby has taken responsibility in this statement, was not initiated by her, for its beginning in June 1952 antedates the present Administration in Washington. This point is mentioned to emphasize that the issues I am discussing may be considered apart from party politics.

The actions described in Secretary Hobby's statement had already aroused deep concern among scientists. For instance, on 15 April 1954, the American Society of Biological Chemists adopted a resolution strongly protesting such actions and requesting the National Academy of Sciences to investigate the situation. Similar action was later taken by the American Physiological Society *(4)* and by other scientific societies and groups. These protests and the vigorous action of the authorities of the National Academy in pursuing the problem have certainly played a major part in bringing about the inquiry that the National Academy Committee has been asked to undertake.

Secretary Hobby's announcement is not a statement of what might happen; it is a statement of what has happened and what is continuing to happen. It was acknowledged in the statement itself that nearly 30 investigators had been affected by this ruling. Others—I do not know how many—have been affected since. In some cases the action taken involves the refusal to award or to renew a grant, on grounds unconnected with the scientific qualifications of the investigator or with his personal integrity and character, as such terms are commonly understood by ordinary men. In other cases the action has been more drastic, it has involved the sudden revocation of a grant already awarded and approved, sometimes in the midst of a 3- or 5-year term of support. Action has been taken suddenly, perhaps with a month's notice, after which all funds were cut off. Explanation for the action has been refused, but a double blow has been dealt the investigator involved. First, he has been deprived of funds vital to his research, often on extraordinarily short notice. Second, the action could be taken by some as implying something dubious, possibly something sinister, concerning the investigator's past. These implications are there, they are intangible, nothing is revealed, no opportunity is given to the investigator to know the nature of the implied charges or to offer any reply. The revocation of funds, under such circumstances, can threaten his future career and make other agencies reluctant to support his work If he is in a position that lacks tenure, it may even threaten the loss of his job. It may be said that the careful avoidance of publicity that has been maintained in these matters is a protection for the individual involved However, when the investigator turns to another agency to seek support, the fact that a grant has been revoked or denied must almost inevitably appear, and the potential threat to his future career will arise in acute form.

It should be reiterated here that the research for which these men have been

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granted funds is not secret in any way No threat to national security is involved in their pursuing their work openly. They are charged with no offense against the law. The action taken against them has often involved the breaking of a moral agreement, if not a legal contract, by the supporting agency. The action is taken outside the security system and outside the law, in a no man's land of undefined accusations and vague suspicions.

A fundamental distinction between open and classified research must be emphasized. Virtually all of us acknowledge the necessity of a security system whenever secret work is involved. In the present troubled state of the world—a condition which is neither peace nor war and which has no earlier parallel in the experience of most of us—some such system is indeed a somber necessity The rigorous requirements of security inevitably involve at times the barring of highly qualified individuals from access to secret information, if there is any reason for substantial doubt concerning their loyalty or discretion. On occasion the decision must be taken to deny the individual such access, even though in fact he may be completely loyal and trustworthy. If the system is wisely administered, such cases should be rare.

The application of the rules is not simple; the value of employing an unusually gifted individual must be balanced against the risks involved in his employment; for every person is to some extent a security risk. The general principles, however, are clear, and they differ in at least one fundamental respect from the principles of the law The assumption that an individual is innocent until proved guilty cannot be taken over directly into the security system. To work under that system is a privilege, not a right, and individuals may on occasion be rejected on suspicion, even if those suspicions are unfounded. We may grant these general principles underlying the operation of the security system, but at the same time we may raise grave questions regarding the wisdom with which they have been applied in specific cases. Overzealous application of the rules, resulting in the exclusion of highly qualified individuals from service to their country for inadequate reasons, may in itself be one of the greatest of security risks

Withholding unclassified research grants on the basis of undisclosed information, however, is a policy that raises totally different issues. It is, in effect, an intrusion of the security system into a realm that has nothing to do with security. Security considerations are a painful necessity, we accept them as having compelling force within the area where secrecy must prevail. They are irrelevant and dangerous when invoked outside that area.

Whether he operates under the security system or not, every person is subject to the law. If he has engaged in criminal subversive activities he is subject to trial and to punishment. Such activities, when they exist, are indeed frequently so cloaked in secrecy that it may be exceedingly difficult to obtain the evidence justifying a legal conviction, even when one may be convinced that the individual is actually guilty. Such criminal activities, however, must be sharply distinguished from expressions of opinion, no matter how repulsive the opinions may appear to most of us.

For the most part, the identity of the persons who have been denied support for unclassified research is unknown to me. I have learned the names of three or four of them, however, and they are men whom I know well. They are outstanding in their fields of research. They have made major contributions to our understanding of such subjects as the structures of biological macromolecules, immunological reactions, and metabolic processes. They are admired, respected, and trusted by their scientific associates. Some of them in the past may have upheld political views that seem to me foolish or ill-judged, but these are matters that they are free to decide for themselves. I do not know one among my scientific colleagues who would question the integrity or character of these men or who would doubt in any way their suitability to receive support for open and unclassified research.

The damage done directly to these men by the policies of the U.S. Public Health Service is a serious matter. The actions taken are regarded as frankly outrageous by many, including myself. However, I submit that the gravest damage done by these policies is not to the men whose grants are withheld. The few whose names are known to me stand high in the esteem of their colleages, both they and we deeply resent the imputations cast upon them. As yet I know of none of them who has not been able to obtain support for his research elsewhere. This may not be true of all, some indeed, I am sure, fear that their ability to get support elsewhere is threatened. All this is bad, but the worst effects are upon other persons who continue to be approved and to receive support. Each one can picture himself also among those that are in trouble, even though he, himself, is in no danger, he may become more guarded in his speech, some thoughts that come to him he may not speak to his colleagues as freely as before, hesitating now and then lest he may say something that might conceivably be used against him.

Two of my colleagues—men of great capacity, courage, and force of character—have told me they have found this attitude beginning to affect them. They granted that on rational grounds they had nothing to fear; they were clear in their consciences and in the eyes of the law; but they knew too well the obscure nature of the grounds on which support had been denied to others; they feared for the younger people working in their departments, whose future could be imperiled by the denial of support, and they became more cautious because of this anxiety. Such fears are destructive. The struggle to guarantee to all men the right to speak their minds on controversial issues without fear of reprisals has gone on for centures. That right is always in jeopardy and it must be constantly and actively maintained. It is certainly vital for scientific workers, to whom independent thinking is a basic necessity in their work.

These fears are supported by indications that information from anonymous accusers is being used as a ground for disqualifying individuals from holding grants from the Government. The actions taken have been so carefully cloaked in secrecy that it is nearly impossible for a private individual to know what has been done. However, evidence from anonymous accusers, not speaking under oath, was employed against John P. Peters in the hearing that led to his removal as a special consultant to the Public Health Service. In the words of an editorial in the *Washington Post and Times Herald* on 29 Nov. 1954 *(5)*, the work of Peters "involved no access to confidential or strategic information." Several eminent men—Charles W. Seymour, former president of Yale, Charles E. Clark, judge of the Second Circuit Court of Appeals, and C.N.H. Long, former dean of the Yale Medical School—testified under oath on Peters' behalf. Yet the verdict given upheld the anonymous hostile informants, the identity of all but one of whom was unknown even to the board that passed on the case.

The case of Peters is still under consideration by the Supreme Court, and it would be impertinent to express an opinion in advance concerning what the verdict should be. Let us tentatively make the assumption that the Supreme Court will hold that Government has been acting within its rights in removing Peters from his position. I would still hold that Government, even if it has these powers, should refrain from exercising them except for grave reasons of national security. If a man has access to secret and vital information or if he is in a position in which he might endanger national security by sabotage during a crisis, then accusations against him from any source must be carefully weighed. They must be weighed with caution—a skillfully worked anonymous accusation, framed by a clever Communist agent, could be a powerful weapon in disqualifying a loyal and gifted scientist from serving his country in a sensitive position. Nevertheless, when national security is involved, such warnings cannot be disregarded. If a man working under the security system is removed from his post because of anonymous charges made against him, this need not imply guilt of any sort; it merely means that there is considered to be a risk in his employment in a sensitive post, a risk that the responsible authorities do not feel justified in taking.

For a man who operates outside the security system, however, the usual standards of our law and our society should prevail. If such a man is trusted and respected by his colleagues and neighbors, if they testify to his integrity, anonymous accusations should be ruled out of consideration in relation to his fitness to receive a grant for open research. It is a dirty business to make such accusations or to lend an ear to them when they are made; they poison the straightforward trust in dealings between men, which is the normal basis on which scientists, like most other people, carry on their work together. To strike at this basis of trust is to sow suspicion and hostility, to weaken the coherence of our society, and thereby to damage the national security itself.

The policies attacked here violate a long tradition—a tradition deeply rooted in English and American law—extending far beyond the confines of the law as such. This tradition insists upon the right of the individual, if an accusation is lodged against him, to know the nature of the accusation and the identity of the accuser. One might attempt to evade the issue here by saying that there is no accusation—that the Government is free to grant or withhold funds as it pleases; that the receiving of funds for scientific research is a privilege, not a right, and that this privilege may be withdrawn at any time by the granting authority at its own discretion. We may admit that technically there is much truth in this. The Government may set the terms upon which it bestows these funds; if the proposed recipient disapproves the terms, he is free to refuse the proffered funds until the terms are altered. But our Government exists to serve the people, and it is my conviction that the people are not best served by offering money for basic scientific research on such terms as this. It is not enough, of course, for the scientists to be convinced of this fact; the ultimate decision is in the hands of the American people and many will not accept the point of view expounded here unless it is fully and carefully explained to them.

It is a matter of profound regret to me that the policies that are here attacked have been formulated and applied by the U.S. Public Health Service, which has performed magnificent service during the years since the war in the support of fundamental research in the United States. Its policies have, in general, been admirable. Its administrators have shown an enlightened outlook in promoting fundamental research; and, apart from the lamentable issues here discussed, they have shown an admirable solicitude for the freedom of the investigator. The laboratory with which I have been associated for many years has received generous and understanding support from the Public Health Service, which has made possible a long series of researches with which I am proud to have been associated. All this I am happy to acknowledge. However, the recently developed policy of the U.S. Public Health Service with which I am concerned here, while ostensibly designed to oppose subversion, appears to me to be itself subversive—subversive of the traditional liberties of the individual and of his right to be judged by due process of law or by something analogous to due process in matters that do not lie strictly within the domain of the law.

Many will say that the issues involved are not as grave as I depict them; that very few people are being hurt; that these disturbances will pass; and that we may endanger the whole structure of Government support of science by challenging the procedures now being adopted. I reject such arguments. Certainly I do not share the fears of the alarmists who believe that our society is rapidly becoming totalitarian; the fact that articles like this one can be published and freely discussed is good disproof of such ideas. Yet the trend toward totalitarian procedures is present in the arbitrary actions that I have discussed; and the time for resistance is now, not later. The men who are enforcing the decisions I oppose are certainly not ruthless autocrats—they are probably conscientious administrators, worried about maintaining the flow of Government funds for science and fearful lest congressional investigators should charge some recipient of a Government grant with being a subversive character. All this is human and understandable, but I believe that it shows a dangerous timidity on the part of certain administrators and that it has done great harm. Even if only a few of our colleagues are hurt—whether it is one person or many—I believe that we should stand up and protest on their behalf. In any case the threat is not to a few persons only; it is to all of us; for no one knows whether or not he will be the next victim, and whether or not he will find his own support cut away and his own future in jeopardy.

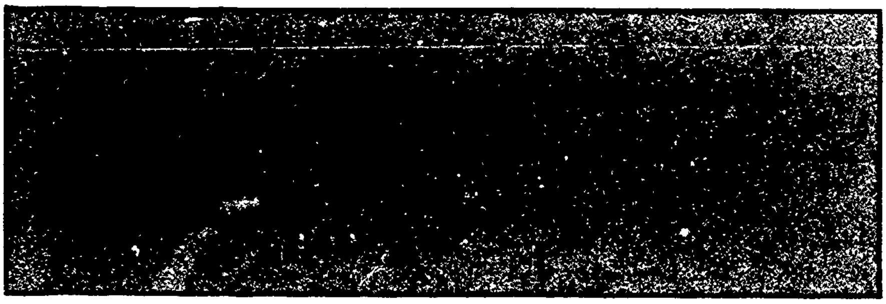
Inevitably the decision concerning proper action in this grave situation is not easy for most scientists. Because I am not a department head, and because I derive my research support from other agencies that have maintained the tradition of freedom, I feel that I can speak more openly than many of my colleagues. I can say only that the withholding of research grants for unclassified research on grounds unconnected with the scientific competence and integrity of the investigator is abhorrent to me. Under the circumstances I shall neither ask for nor accept funds from any Government agency that denies support to others for unclassified research for reasons unconnected with scientific competence or personal integrity. If I do receive funds for research and I learn subsequently that the granting agency has adopted such a policy toward other individuals, I shall stop using such funds and shall return the unexpended balance to the agency that awarded them to me.

I state this as a personal policy without urging that my colleagues join me in it. I know many whose personal convictions are essentially identical with mine but who feel precluded from taking similar action because of their responsibility for obtaining funds for their departments and especailly for the younger workers whose careers would be imperiled if funds were cut off. For myself, however, I can say only that I see great danger in the present situation. Having freedom to speak and holding the convictions that I do, I feel that I cannot keep silent in the face of a policy which I believe to be a threat both to the freedom of science and to the basis of the social order in which most of us believe.

Immensely powerful forces are transforming our society and the status of science and scientists within our society. For a vigorous modern nation, a flourishing science is a condition for national strength and even for survival. Scientists are urgently needed, and the pressure grows increasingly strong to consider the scientist as the servant of the state. Insofar as he operates under the security system, or in any case in which he serves as an adviser to Government, the scientist is indeed a servant of the state. It is imperative, however, to preserve, in spite of the portentous growth of the power of the state, the tradition of free scientific inquiry by persons who in their work owe no allegiance except to the spirit of inquiry, the desire to understand, and the sense of beauty in discerning patterns of order amid the chaotic multiplicity of phenomena. It is the independent, unfettered investigators who have made the great germinal discoveries, and if we do not provide the conditions in which such men can flourish, we shall lose leadership in science. Yet in our world of today such scientists, like others who work on problems of a more applied type, must receive much of the support for their work from Government. It is one of the great problems of our time to maintain a Federal Government that has at its disposal immense material resources and immense power, and still to insure that the power is used with due respect for the integrity of the individual and for his personal freedom. It is, I think, one of the great American achievements of the past generation that we have largely succeeded, during a period of profound social change, in combining these almost incompatible objectives. I believe that we can and must succeed in doing this in the field of Government support of science. To succeed requires incessant vigilance to prevent undue encroachments on personal freedom and a patient determination that scientists and Goverment administrators cooperate and understand one another to make the system work.

REFERENCES AND NOTES

1. *Science* **121**, 7A (11 Feb 1955).
2. *Science* **121**, 490 (8 Apr. 1955).
3. A draft of this paper was sent to Oveta Culp Hobby, Secretary of the Department of Health, Education, and Welfare, on 23 Dec. 1954 The letter of acknowledgment stated that the problems discussed in the paper were being considered On 27 Jan. 1955 I again wrote to Secretary Hobby asking whether the paper contained any misstatements about the U.S. Public Health Service. The reply mentioned none but stated that the problem was still under study. I then decided to submit the paper for publication, after revising the opening statement to take account of recent events and adding two paragraphs related to the use of anonymous informers — J T E
4. *Science* **120**, 1010 (17 Dec 1954).
5. Reprinted in *Science* **120**, 1009 (17 Dec. 1954).



In the 325 years since the ordeal of Galileo the physical sciences have been emancipated from political and ecclesiastical domination and, as the globe-encircling sputniks testify, can thrive even in the shadow of the State's adoption of dialectical materialism as its official philosophy. The life sciences, a century since Darwin, still stand embroiled, still struggle for th necessary freedom to grow untrammelled by common prejudice and official disfavor. Yet in the end all science must stand free or become the slave of the State, the prostitute of material desires. For scientists there can, therefore, be no greater dedication than to the defense of their freedom of thought and of choice of investigation.

The Congress for Cultural Freedom in a conference in Hamburg in 1953 undertook to arouse men to the defense of scientific freedom. That effort has been continued since 1953 through the untiring work of the International Committee on Science and Freedom, of which Michael Polanyi is chairman. This committee has published to date nine bulletins, of which the last three deal respectively with "Self-government in Modern British Universities," "Hungary, October, 1956," and "Apartheid, the Threat to South Africa's Universities." Each of these is engrossing reading.

The academic freedom of the universities of the Western World and the freedom of science are inextricably interwoven. In fact, the latter may be considered a very branch of the former. No scientist can really afford to be unconcerned with threats to the academic freedom of any university, whether in his own land or abroad.

The Nazi doctrines of racial superiority are not dead. In one semblance or another they rear themselves wherever men of one dominant social group fear themselves threatened by the numbers and growing enlightenment of a racially different element. Apartheid preaches very plausibly the theory of the equal but separate development of white and nonwhite people in residence, in labor, in education. Yet it is plain to see that actually it is a barefaced effort of the whites to keep the black and colored people of South Africa in servitude and educational inequality. Education will not be entrusted to the natives themselves to develop in independence, but will be gently governed for them by a (white) Minister of Native Affairs, whose viewpoint is sufficiently expressed in his own words: "What is the use of teaching the Bantu child mathematics when it cannot use it in practice? That is quite absurd. . . .Education must train and teach people in accordance with their opportunities in life, according to the sphere in which they live. . . .Good racial relations cannot exist when the education is given under the control of people who create wrong expectations on the part of the Native himself, expectations which clash with the possibilities in this country."

The immediate threat is the exclusion, from the five "open universities" which now admit them, of all nonwhite students, under a Separate University Education Bill which will probably be passed early in 1958. Can there be any question that in the long run the freedom of mankind—and the freedom of science along with it—is more imperiled by the defeats of Little Rock and Pretoria than by the success of *Sputnik*?

Because science depends so immediately on freedom of inquiry, partisans of political freedom are tempted to believe that democracy, whatever its shortcomings in producing space spectaculars, must in the end be the form of government best designed to produce pure science. The argument is that the individual must be free to propose hypotheses and test them. Heinrich Schliemann in the 19th century, for example, had to be free both to believe that Hissarlik was the site of Homer's Troy and to go to the spot and dig. If some government had held, as a consequence of its metaphysics, that Troy never existed, or if some government had feared that the practice of digging for Troy might lead to a penchant for digging into other matters as well, then there would have been no archeological confirmation of the Achaean heroes.

This argument is sound, but the implications for the superiority of democracy as an environment for science are limited. For one thing, freedom by itself is not enough. Although Schliemann may have been free to dig as deeply as he liked, he had first to amass the fortune necessary to finance his digging. But what is more interesting is that science has other characteristics besides the need for freedom, and these other characteristics suggest that this need if deep may not be broad.

Besides the posing and testing of hypotheses, science is also characterized by its procedure of not attempting to answer all questions at once. Scientific knowledge is possible because it is compartmentalized, because [it is] possible to discover truths about one question and at the same time ignore other questions. Hydrodynamics, for example, can be studied independently of thermodynamics, and the two disciplines together have nothing to say about the sensory qualities of a cold drink of water on a hot day. It may well be that freedom of the most complete sort in one part of knowledge is entirely compatible with total bondage in another part.

Research in its later stages, it is true, has a way of breaking down the very barriers that made earlier progress possible. If science is compartmentalized, it also strives toward unity. The turn of the century saw how increased knowledge about the structure of the atom broke down the barriers between chemistry and physics, and we are now watching our growing knowledge of the structure of the gene break down the barriers between biology and the physical sciences. But unity, in turn, produces new compartments. If where once stood a fence there is now a house, the house itself is a kind of enclosure and within it lives a new group of specialists.

One point about a totalitarian government is clear. To the extent that it chooses to meddle with the methods of research, or to dictate the results of research, science will be the loser. But without claiming that the pull in science toward specialization is somehow stronger than the pull toward unity, it is still possible to say that freedom to study a particular problem is not immediately dependent on freedom to study every problem. What has yet to be fully determined is what happens when a totalitarian government chooses to support some parts of science vigorously and intelligently, because it sees the achievements of science as contributing to its own greater glory.

Freedom of Inquiry

DeWitt Stetten, Jr.

Editorial, 19 September 1975

The First Amendment to the Constitution explicitly forbids the Congress from abridging the freedoms of speech and of the press It imposes no comparable constraint abridging freedom to learn, to teach, or to inquire, yet these may be construed to be implicit freedoms and indeed seem to be of a comparable quality. All of these freedoms are, in fact, abridged from time to time, subject to the test of a real and present danger. In the absence of such demonstrable danger, the accepted position is and should be jealously to guard the constitutionally guaranteed freedoms, both those expressed and those implied.

Abridgments of freedom of speech and of the press have frequently occurred and have been contested. News media report movements in many communities to limit freedom to teach, especially in areas such as sex education. Analogously we encounter today serious questions, arising in part from scientists themselves, about the appropriateness of pursuing certain lines of scientific inquiry.

Some of the consequences of constraining freedom of inquiry are well known Jacob Bronowski recently reminded us that the loss of Italy's lead position in the Renaissance of science followed immediately upon and doubtless was caused by the adverse judgment of the Inquisition against Galileo, which forbade certain lines of inquiry In an otherwise impressive forward march of science in the Soviet Union, a generation of genetics research was lost by the constraints resulting from Lysenkoism Such losses must enter into the cost-benefit analysis in determining whether to encourage, permit, discourage, or forbid a particular line of investigation.

Among the lines of research against which voices have recently been raised are the following:

1) What are the genetic contributions to intelligence?

2) What kinds of experiments may properly be performed on informed consenting adults? Minors? Fetuses? Prisoners?

3) May one screen infants for a variety of genetic defects, some with known, others with currently unknown clinical consequences?

4) Under what circumstances may one tamper with the genetic process, as by the introduction of foreign genetic material into the genome?

5) When may one meddle with human

conception and pregnancy as by artificial insemination, abortion, cloning, in vitro fertilization, or the use of surrogate mothers?

In arrivir_ at considered judgments on these and a number of other problems, it is suggested that we treat freedom of inquiry as we have learned to treat freedom of speech—that is, agree to abstain when there is a real and present danger. By this test, the fact that the problem may be difficult, or that its solution may prove politically embarrassing or unpopular, is insufficient ground for invoking constraint. Indeed, a science that shies away from a line of inquiry merely because the result may be difficult to manage is in a sorry state.

Each man or woman will assess whether a real and present danger exists in each particular line of inquiry The judgment will be difficult but not entirely unfamiliar. It is the same judgment we make in assessing every instance of censorship that comes to our attention. Is the danger in pursuing a particular line of research of such a magnitude that, in another context, we would willingly abdicate freedom of speech?

Judgments will surely be individual. For example, I acknowledge the danger inherent in some of the scenarios composed for the fabrication of certain types of DNA recombinant molecules. On the other hand, it strikes me that screening infants for abnormal karyotypes presents only such difficulties and problems as practicing physicians cope with on a daily basis. The wise physician must try to minimize the adverse consequences of unfavorable diagnosis.

It is fashionable to criticize the ethics and humanity of scientists, as in other times we have criticized the writers or painters. If history is any guide, this too shall pass. Then we may arrive at the balanced state where all questions may be asked save those which pose a real danger to the community, the environment, or the individual.

Einstein Session of the Pontifical Academy of Sciences

Pope John Paul II

Article, 14 March 1980

Along with Your Excellency [Dr. Chagas] and with Drs. Dirac and Weisskopf, both illustrious members of the Pontifical Academy of Sciences, I rejoice in this solemn commemoration of the centenary of the birth of Albert Einstein. The Apostolic See also wishes to render to Albert Einstein the honor that is due him for the eminent contribution he has made to the progress of science—that is, to the knowledge of the truth present in the mystery of the universe.

I feel myself in full agreement with my predecessor Pius XI, and with those who succeeded him to the Chair of Saint Peter, in inviting members of the Pontifical Academy of Sciences, and all other scientists, to bring about "the progress of the sciences ever more nobly and more intensely, without asking anything more of them; this excellent aim and this noble effort represent a mission of serving the truth with which we entrust them" (*Motu proprio In multis solaciis*, 28 October 1936, on the Pontifical Academy of Sciences: *Acta Apostolicae Sedis* 28, 1936, p. 424).

Science and Religion

The search for truth is the task of basic science. The researcher who moves into this primary area of science feels all the fascination of the words of Saint Augustine: "*Intellectum valde ama*" (*Epist.* 120, 3, 13: *Patrologia Latina* 33, 459)—that is, to "love intelligence greatly" and its function, to know the truth. Basic science is a good, worthy of being very much loved, for it is knowledge and therefore the perfection of man's intelligence. Even more than its technical applications, it must be honored for itself, as an integral part of our culture. Fundamental science is a universal good that all people must be able to cultivate in complete freedom from every form of international servitude or intellectual colonialism.

Basic research must be free with regard to political and economic powers, which must cooperate in its development without impeding its creativity or subjugating it to their own ends. Like any other truth, scientific truth must render account only to itself and to the supreme truth that is God, creator of man and of all things.

In its second aspect, science turns to practical applications, which find their full development in the diverse technologies. In the area of its concrete applications, science is necessary to humanity in order to satisfy the just requirements of life and to overcome the various evils that threaten it. There is no doubt that applied science has rendered and will render immense services to man if it is inspired by love, ruled by wisdom, and accompanied by the courage that defends it against undue interference by all tyrannical powers. Applied science must be allied with conscience so that through the triad science-technology-conscience, the true good of humanity will be served.

Unfortunately, as I had occasion to say in my encyclical *Redemptor hominis*, "Man today seems always menaced by what he produces. . . . This seems to constitute the principal act of the drama of human existence today" (No. 15). Man must emerge victorious from this drama, which threatens to degenerate into tragedy, and he must rediscover his authentic kingship over the world and his full dominion over the things he produces. Today, as I wrote in the same encyclical, "the fundamental meaning of this 'kingship' and of this 'dominion' of man over the visible world, which is given him as a task by the Creator, consists in the priority of ethics over technology, the preeminence of people over things, and the superiority of spirit over matter" (No. 16).

This triple superiority is maintained to the extent that the sense of the transcendence of man over the world, and of God over man, is preserved. The Church, by carrying out her mission of guardian and advocate of both transcendences, believes that she is assisting science to keep its purity in the area of basic research and accomplish its service to man in the area of practical applications.

This article is based on the address by Pope John Paul II at the Einstein session of the Pontifical Academy of Sciences, Vatican City, 10 November 1979. The Pope's speech, which was given in French, was translated for *Science* by Professor Robert Nicolich of the Catholic University of America, Washington, D.C 20064.

On the other hand, the Church willingly recognizes that she has benefited from science. It is to science, among other things, that we must attribute what the Council has said concerning certain aspects of modern culture: "New conditions have their impact finally on religious life itself The rise of a critical spirit purifies it of a magical view of the world and of superstitions that still circulate, and exacts a more personal and explicit adherence to faith; as a result, many persons are achieving a more vivid sense of God" (*Gaudium et spes*, No. 7).

The collaboration between religion and modern science is to the advantage of both, without in any way violating their respective autonomy. Just as religion requires religious freedom, science legitimately claims freedom to carry on research. The second Vatican Council, after reaffirming with the first Vatican Council the just freedom of the arts and human disciplines in the area of their own principles and their own methods, solemnly recognizes "the legitimate autonomy of human culture and especially of the sciences" (*Gaudium et spes*, No. 59). On the occasion of this solemn commemoration of Einstein, I would like to confirm again the Council's declaration on the autonomy of science in its function of searching for the truth inscribed during the creation by the finger of God. Filled with admiration for the genius of the great scientist, in whom is revealed the imprint of the creative spirit, without intervening in any way with a judgment on the doctrines concerning the great systems of the universe, which is not in her power to make, the Church nevertheless recommends these doctrines for consideration by theologians in order to discover the harmony that exists between scientific truth and revealed truth.

The Case of Galileo

Mr. President, you said very rightly that Galileo and Einstein each characterized an era. The greatness of Galileo is recognized by all, as is that of Einstein; but while today we honor the latter before the College of Cardinals in the apostolic palace, the former had to suffer much—we cannot deny it—from men and organizations within the Church. The Vatican Council has recognized and deplored unwarranted interferences: "We cannot but deplore—it is written in number 36 of the Council's constitution *Gaudium et spes*—certain attitudes found, too, among Christians insufficiently informed of the legitimate autonomy of science. Sources of tensions and conflicts, they have led many minds to think that science and faith were opposed." The reference to Galileo is expressed clearly in the note joined to this text, which cites the volume *Vita e opere di Galileo Gailei* by Monsignor Pio Paschini, published by the Pontifical Academy of Sciences.

To go beyond this stand taken by the Council, I hope that theologians, scientists, and historians, imbued with a spirit of sincere collaboration, will more deeply examine Galileo's case, and by recognizing the wrongs, from whatever side they may have come, will dispel the mistrust that this affair still raises in many minds, against a fruitful harmony between science and faith, between the Church and the world. I give all my support to this task, which will honor the truth of faith and of science and open the door to future collaboration.

Permit me to submit to your attention and consideration some points that seem to me important for viewing Galileo's case in its true light. In this matter, the points of agreement between religion and science are more numerous and above all more important than the lack of understanding that has led to a bitter and painful conflict drawn out over the following centuries.

He who is rightly called the founder of modern physics declared explicitly that the two truths, of faith and of science, can never contradict each other. "Holy Scripture and nature proceed equally from the divine Word, the former as it were dictated by the Holy Spirit, the latter as a very faithful executor of God's orders." as he wrote in his letter to Father Benedetto Castelli on 21 December 1613 (national edition of the works of Galileo, vol. V, pp. 282-285). The second Vatican Council does not express itself otherwise; it even uses similar expressions when it teaches: "Methodical investigation in every branch of learning, if carried out in a genuinely scientific manner and in accord with moral standards, never truly conflicts with faith: for earthly matters and the concerns of faith derive from the same God" (*Gaudium et spes*, No. 36).

Galileo feels in his scientific research the presence of the Creator who inspires him and aids his intuition, acting in the inmost recesses of his spirit. With regard to the invention of the telescope, he writes at the beginning of *Sidereus Nuncius*, recalling several of his astronomical discoveries: "*Quae omnia ope Perspicilli a me excogitati divina prius illuminante gratia, paucis abhinc diebus reperta, atque observata fuerunt*" (*Sidereus Nuncius*, Venetiis, apud Thomam Baglionum, MDCX, fol. 4). "All of this has been discovered and observed these last days thanks to the 'telescope' that I have invented, after having been enlightened by divine grace."

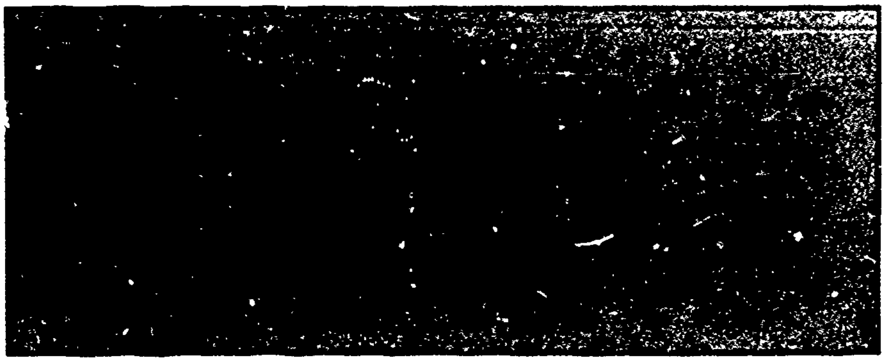
The Galilean confession of divine illumination of the mind of the scientist finds an echo in the text of the Council's constitution, on the Church in the modern world: "Whoever labors to penetrate the secrets of reality with a humble and steady mind is being led by the hand of God, even if he remains unaware of it" (*loc. cit.*). The humility stressed by the Council's text is a virtue necessary both for scientific research and for commitment to the faith. Humility creates a climate favorable for a dialogue between the believer and the scientist; it calls for enlightenment by God, recognized as such or not, but valued in both cases by one who humbly seeks the truth.

Galileo formulated important norms of an epistemological character that are indispensable for reconciling Holy Scripture and science. In his letter to the Dowager Grand Duchess of Tuscany, Christine of Lorraine, he reaffirms the truth of Scripture: "Holy Scripture can never lie, provided its true meaning is understood, which—I do not think it can be denied—is often hidden and very different from what a simple interpretation of the words seems to indicate" (national edition of the works of Galileo, vol. V, p. 315). Galileo introduces a principle of interpretation of the sacred books that goes beyond the literal meaning but is in accord with the intention and type of exposition proper to each of them. It is necessary, as he affirms, that "the wise men who explain it should bring out their true meaning."

Ecclesiastical authorities admit that there is more than one way to interpret the Holy Scriptures. In fact. it was explicitly stated in the encyclical *Divino afflante Spiritu* of Pius XII that there are different literary styles in the sacred books and therefore interpretations must conform to the character of each.

The various points of agreement that I have brought to mind do not only resolve all the problems of Galileo's case, but they contribute to creating a favorable starting point for their honorable solution, a state of mind propitious for an honest and straightforward resolution of old conflicts.

The existence of the Pontifical Academy of Sciences, with which Galileo was, in a sense, associated through the old institutions that preceded the one to which eminent scientists belong today, is a visible sign that shows to the people of the world, without any form of racial or religious discrimination, the profound harmony that can exist between the truths of science and the truths of faith.



A series of events in recent months signals the need for attention and action by the scientific community. On the surface, the events appear unrelated, but viewed collectively, the ramifications of each have substantial impact on the future of scientific inquiry.

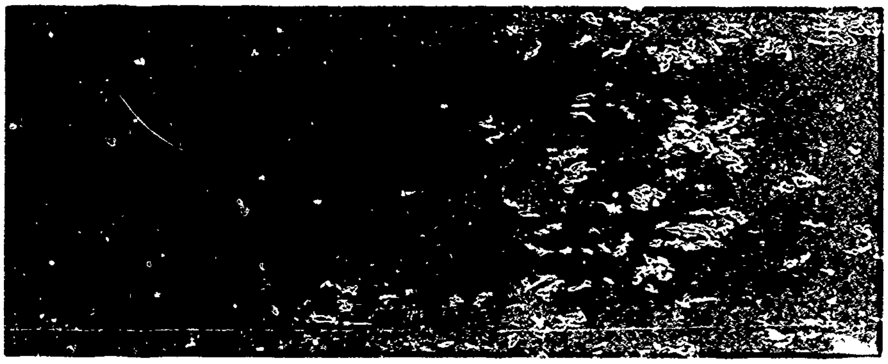
First, an active campaign has been launched by various individuals and groups to reduce, or ban altogether, the use of animals in scientific research. Initially, these factions rally against the sale and distribution of pound animals. This is an emotionally charged issue—the public readily identifies with the homeless animals because of attachments to their own pets. Legislation introduced to curtail the use of animals in research passed into law in Massachusetts in February. Although a similar measure was defeated in the California legislature, the proponents may seek a public referendum. They may also seek to establish provisions for external review boards to make judgments concerning which research proposals involving animal experimentation are justified and which are not. Evidence thus far indicates that once the objective of banning the use of pound animals has been met, the advocates push on toward banning the use of animals from any source and for any scientific purpose.

The second concern involves the suit filed by Jeremy Rifkin to block the release of genetically engineered organisms into the environment. The case in question centers on a bacterium which, in its native state, serves as a nucleus for ice crystal formation. The removal of a single gene eliminates this characteristic. The next step was to have been the introduction of the modified bacteria into the environment of crop plants, replacing the native strain and reducing the potential for frost injury to plants. The restraining order against this, obtained by Rifkin *et al.*, is based on allegations that a National Institutes of Health review committee failed to conduct an adequate study and submit a satisfactory environmental impact statement. However, the basic issue is that many supporters of the litigation are fundamentally opposed to genetic engineering and seek to block application of the new technology.

Third is a suit brought by California Rural Legal Assistance, representing the California Agrarian Action Project, to block mechanization research in agriculture. One objective is to require the University of California to submit social impact statements on proposed research projects before they can be approved. This suit illustrates again the ways in which special interest groups attempt to regulate scientific research which they perceive is not beneficial to them. They do not accept the evidence that overall social and economic benefits far outweigh the costs.

Each case is individually controversial and each decision sets a precedent. Considered collectively, the impact can be overwhelming. It is essential, therefore, that members of the scientific community become active participants in the debates. Highly committed and articulate individuals and groups are presenting their cases to the public and the lawmakers without equally articulate rebuttal from scientists. Since litigation has become the method by which policy to constrain scientific research is decided, scientific societies may well need to invest—individually and collectively—in legal representation to present their views in opposition to such constraints.

As the National Science Board Commission on Pre-College Education recently concluded, it is critical that all students return not only to the fundamentals of reading, writing, and arithmetic, but also to scientific and technological literacy. In the interest of free inquiry and the advances that science has brought—and must continue to bring—to civilization, we must invest our energies on all fronts. To allow these and other anti-science activities to go uncontested would be unconscionable.



Universities are being exhorted by a wide variety of interest groups to take official positions on issues such as military research, the U.S. corporate presence in South Africa, and restrictions on information flow. Often the groups making such demands are perplexed by the resistance they meet, since they believe their particular perspective to be in the long-term interest of the human community and, therefore, of the university community as well.

It is essential to understand, however, that over the past century American colleges and universities have been transformed from quiet centers of traditional moral, political, and social values into educational and research centers at which inquiry is more important than dogma. It is only in recent times that the faculties and students of colleges and universities have acquired both the freedom and the obligation to consider subjects and pursue lines of investigation that may contradict prevailing beliefs in science or threaten the vested interests of powerful social and political groups. It is only in this century that the notion of academic freedom as a defining characteristic of universities has become pervasive.

This is a fundamental change. But the distinction between universities as institutions and faculty and students as individuals is often not recognized by the various publics who support universities and who look to the university as an institution for an affirmation or reaffirmation of particular points of view.

The work of the academic community is undeniably related to and supported by a particular set of values. These include the value of knowledge, the benefit of fair and open inquiry, respect for other points of view, and the possibility of human progress. In addition, most universities are now on record as taking a stand on some moral issues such as affirmative action and research on human subjects. We must, however, be very cautious about adding to this list. Without developing a means of distinguishing ideas from ideologies we risk the possibility of undermining the environment that supports our principal commitments and responsibilities. Returning to an earlier model of moral, political, and scientific orthodoxy would, however, undercut academic freedom and open discourse, transforming the character of contemporary higher education and undermining the university's capacity to make positive contributions to society.

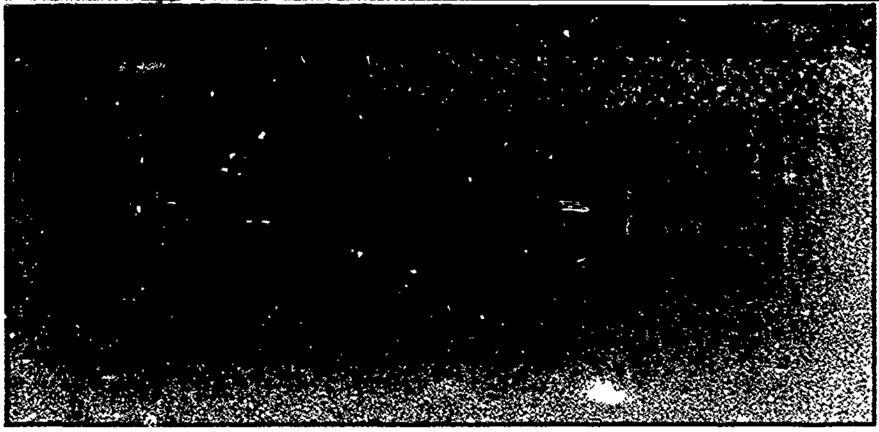
Although academic freedom is not the only value that should inform our actions, we should consider no erosion of academic freedom without carefully scrutinizing the reasons for it. Perhaps we could ask ourselves questions such as the following as we prepare for the discussions.

1) What is the source of the university's right to free inquiry and what is its relation to the society that grants that right? In particular, what obligations accrue from this right?

2) If the university as an institution takes a moral or political stand, what implication does this have for members of the community with other points of view?

3) How do we identify those moral and political issues on which a university should adopt a particular point of view? For example, is the range of admissible inquiry a matter for administrative decision? If so, under what circumstances do we allow restrictions on teaching and research programs that offend an individual's moral or political values?

Experience indicates that transforming moral sentiments into policy statements requires carefully articulated ideas of the mission of a university and the impact of teaching and research on that mission. In this context, I believe that a university remains a creative part of society only as long as it remains an intellectually open community and not the ally of a particular point of view.



Should there be a universal code of ethics for scientists? The pros and cons of this issue became the subject of debate as the discussion of freedom and responsibility in science began to raise ethical concerns among scientists. Calls were heard for development of a formal set of principles to guide the behavior of members of the scientific community.

Not everyone agreed with this approach. Many scientists strongly resisted the notion that their profession should adopt a formal code of ethics as had the professions of law, medicine, and engineering. It was suggested that such a code would do little to prevent willful misconduct and might stifle creative thought by merely stressing formal etiquette.

In an early analysis of obstacles to developing a uniform code of ethics for scientists, Ward Pigman and Emmett B. Carmichael review rights and obligations to be considered prior to the adoption of a formal code and suggest steps to reconcile the traditions of science with the emerging relationship between science and government.

Bentley Glass, noting that science and ethics are both inescapably subjective activities that are affected by evolutionary forces, takes the position that there needs to be an ethical basis for scientific activity to incorporate it into human affairs. His four basic principles for an ethic of science are: to cherish truthfulness, to avoid self-aggrandizement, to defend the freedom of scientific inquiry and opinion, and to communicate one's findings through publication and instruction.

An exchange of letters in 1963 provides a window on the thoughts and passions attached to the proposed development of a code of ethics for scientists. To Lawrence Cranberg's suggestion that scientists may wish to emulate their engineer colleagues by developing a formal code, F.R. Fosberg and Henry Lanz respond that such codes serve little purpose in guiding truly virtuous conduct. Indeed, writes Lanz, the "mere thought of setting up a code of ethics for scientists is insulting." A final letter from W.E. Graham describes briefly the efforts of the Society for Social Responsibility in Science to implement principles of personal moral responsibility for the consequences of professional work. Echoing sentiments expressed sporadically in earlier times, the SSRS pledge is one of the few efforts by a scientific organization to require its members to adhere to principles of social responsibility as a prerequisite of membership. —*RC*

A new phenomenon of our present-day society is the obviously important role played by science. Only a short time ago science was considered by many "practical" men as a plaything of inconsequential importance in contributing to the welfare of society. Although the significance of science was becoming more generally evident before World War II, this war demonstrated to the public in general and to legislators and businessmen in particular that science, especially basic science, is much more than a scholarly pursuit—that it is a vital force for the advancement or destruction of society. Science is now "big business." As a result, the scientist cannot and must not remain a scholarly recluse divorced from the remainder of society. His behavior and that of society toward him will greatly influence the progress of science and, to an increasing extent, that of society itself.

During its long period of development, science has evolved a code of professional tradition and ethics, largely in an unwritten form. This code, really the foundation of the scientific method in many of its aspects, has to a considerable extent been responsible for the achievements of science. Polanyi's *(1)* description of the effect of disregard for scientific traditions is applicable to many of our modern industrial and research organizations:

> Those who have visited the parts of the world where scientific life is just beginning, know of the backbreaking struggle that the lack of scientific tradition imposes on the pioneers. Here research work stagnates for lack of stimulus, there it runs wild in the absence of any proper directive influence. Unsound reputations grow like mushrooms: based on nothing but commonplace achievements, or even on mere empty boasts. Politics and business play havoc with appointments and the granting of subsidies for research. However rich the fund of local genius may be, such environment will fail to bring it to fruition.

The important achievements of science and its contributions to our civilization seem adequate proof of the basic validity of these traditions. On the other hand, conditions of scientific work have changed greatly, and obviously the traditions must be interpreted in terms of prevailing conditions. Science has emerged from a period in which the predominant effort was made by individuals, sometimes of almost an amateur status, to a period marked by the development of large research groups, many in the pursuit of research for profit. As a result, it is timely for the scientist to consider his professional traditions and to relate them in terms of the structure of modern scientific work.

These traditions are essentially an unwritten code of professional ethics. As pointed out by Leake *(2)*, the term "professional ethics" as used generally includes the attitude of the individual scientist to society and to other scientists. It will be so used here This concept of professional ethics inextricably involves social obligations, questions of etiquette, and adherence to accepted traditions. Claude Bernard *(3)* has contributed one of the better discussions of the ethical qualities needed in scientists, and the relationship of these qualities in the scientific method, although his remarks apply in the main to medicine and physiology.

Some of our professional organizations have established formal written codes of professional ethics *(4)*. In the medical field numerous papers and books have been written on the subject. One of the first extensive codifications was that of Percival *(2)* (1803), but the general precepts of Hippocrates (circa 500 B.C.) have modern acceptance. A major consideration at the first meeting of the American Medical Association in 1847 was the formulation of a code of professional ethics *(2)*. The present code provides means for enforcement by its members. There has been some discussion of the professional responsibilities of industrial chemists *(5)* and a code has been proposed for this group *(6)*. Scientific groups generally, however, have not formalized their traditions but have passed them on by example and by word of mouth as an informal part of the graduate student's training.

This failure of scientists as a group to consider ethics is revealed in the fact that *Chemical Abstracts*, since it was founded in 1907, listed only four references under ethics in its indices. It is true that professional codes at best can only express an ideal; their acceptance and application will depend upon the individual scientists. We believe, however, that the scientist's position in the world today makes it extremely important that his time-proved traditions be reconsidered in terms of modern circumstances and possibly written into a formal code. We believe that such an action would maintain the advance of science, increase its public support, and improve the professional relations of scientists. Improved professional relations would better morale and increase productivity among research men. Mills *(7)* has pointed out the social implications of ethical behavior in the distribution of research grants.

The planning of an ethical code for scientists should take into account first the scientist's general obligations as a member of society, and beyond that his special obligation as a scientist to protect society—here, there are many problems related to warfare, to the health and general well-being of mankind, and to nationalism versus internationalism. Such a code should preserve the scientist's ethical traditions and incorporate the scientific method. It should state the scientist's obligation to explain the nature and purposes of science, and the policies in dealing directly with the public. It should clarify the scientist's attitudes toward patents and secrecy restrictions. It should affirm the scientist's obligations to individuals—to his employer, his associates, other scientists, and his assistants and graduates—and scientists' obligation as a group to other professions.

We have merely indicated the scope of the problem. To deal with it fully in all its phases would require the efforts of scientists in many different fields of study and kinds of employment. Some of these phases have already received considerable attention. Because the results of atomic research have such unmistakable implications for society, attention has been paid to the scientist's attitude on the use of his discoveries, particularly for military purposes, and to the necessity of his being socially conscious *(8, 9, 10, 11, 7, 12)*. Other phases of the problem have received little or no public consideration.

Many of the scientist's obligations are reciprocal in the sense that the scientist has grown to expect certain conditions for his work, and to a considerable extent these conditions affect the quality of his work. Sometimes his obligations are conflicting. He may at times be faced with the dilemma of obligations to his employer that conflict with obligations to the public as a whole. What should his attitude be when his employer's immediate interest causes harm to the general public? Suppose that his employer is a company that is pouring waste products into a stream and he knows that at a reasonable cost this pollution could be greatly minimized. Should he assume, as a lawyer does, that his primary obligation is to his client, and become an automatic defender of the company's position? Or should he consider

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that he has duties to society greater than those to the company?

A group of very pressing problems is presented in the application of traditions related to the authorship and publication of scientific researches. In the early days of science most articles carried the name of only one worker, whereas multiple authorship is now most common and sometimes ten or more persons may be involved. As a detailed example of the need for a code of professional ethics, we will discuss some of the problems involved in authorship.

General Obligations of Authors

Quality of Papers. Everyone will agree that scientific articles should be of good quality, should be original in content, and should describe all work in a reproducible fashion. These are fundamental requirements of the scientific method and yet most scientists would admit that many research articles are published that are deficient in some or all of these respects.

Claude Bernard *(13)* has described the importance of adequate details·

> In scientific investigation, minutiae of method are of the highest importance. The happy choice of an animal, an instrument constructed in some special way, one reagent used instead of another, may often suffice to solve the most abstract and lofty questions. . . .the greatest scientific truths are rooted in details of experimental investigation which form. as it were, the soil in which these truths develop.

Even casual inspection will show that many articles are not written so that the work can be repeated. Traditional procedure is often ignored in reporting new compounds; occasional articles will not give analyses of new compounds or the compounds will be poorly described so that their identity is questionable. Scientific journals lack space to print all the good material they receive today, and understandably urge authors to shorten their articles, but great care is needed to avoid eliminating important details.

Direct responsibility to prior work. The traditions of science demand that any report of scientific work must consider prior work, integrate it in the general subject, and cite proper references to it. Frequent violations of this principle must be familiar to all scientists. One of us has previously called attention to an instance of this type, particularly in relation to the naming of methods *(14)*.

The basic concept behind this principle, even more fundamental than professional courtesy, is that frequently the solution to a problem may already be in the literature and needless repetition is economic waste. Thorough literature searching can be defended from an economic standpoint alone. In order to speed the incorporation of new work into the general body of basic knowledge, each author has the responsibility of assisting in the integration of his work with that of previous workers.

To many scientists, establishment of priority for new discoveries is important, and organizations that seek patent protection for their work may set up involved and expensive procedures to establish the date of discovery. Is this a tradition that should be continued? Some scientific journals do not carry the date a manuscript was submitted, and few indicate whether essential changes of content have been made after that date. "Letters to the Editor" may require careful controls to prevent abuses.

Criticism and disagreement. The scientific method requires that all research work be open to critical examination and testing by researchers in the field. It also requires that dissenting theories and results be treated with tolerance, and not suppressed merely because they disagree with currently accepted ideas. Many scientists would add that mistakes and errors should be publicly acknowledged.

The widespread violation of these principles today is affecting not only the progress of science but our economy as well. Many commercial research organizations keep closed files of their researches as a matter of policy, in the belief that they will have an advantage over their competitors. Most of them do not realize that lack of criticism of the worker by qualified colleagues in his own field fosters the carrying out and perpetuation of poor or erroneous work, the continued employment and promotion of unqualified workers, and the perpetuation of poor research policies. Criticism by members of the worker's organization and by consultants is usually inadequate because of the influence of personal motives and lack of knowledge in the specific field. Objections to excessive secrecy in military research should take into account this principle as a primary consideration.

Classical examples of the value of scientific controversy are well known. When properly conducted, such debates lead to clarification and advancement of knowledge. But improperly conducted, they lead to enduring feuds, and because of this possibility, there is a tendency among editors of journals to suppress scientific polemics. A continuation or extension of this trend will be a severe blow to the scientific method. However, as stated by Wise *(12)*

> The research worker should not permit himself to become embittered or involved in useless polemics. . .It simply means that his criticisms must be objective and that they must not descend to the plane of personalities He must show that he is dealing with a set of data, not with an enemy.

Property rights of the scientist in his work. A currently controversial problem of the application of scientific tradition involves the rights of a researcher to his work. The decision to try to publish or reveal his research once was the sole right of the scientist. Now, with the investigator receiving financial support from others in most cases, the final decision is tending to fall on the provider of the funds. In the extreme case, what is there to prevent someone in authority from taking over the work of an associate and passing it off as his own work? What should an editor of a scientific journal do if he receives for publication a suitable manuscript from an established worker and simultaneously a letter from the supporting group saying that it should not be published? Should the supporting group be required to provide satisfactory and convincing grounds? By tradition and perhaps even by legal mandate, the rights of an artist to certain phases of the disposition of his work have been affirmed. Should these not apply equally to the scientist, whose application of his science is often an art?

At least one established graduate institution has the policy that all doctoral theses are published solely in the names of the individual graduate students. In certain instances, the idea was suggested by a member of the faculty, who carried out some preliminary work, supervised the principal research, drew or helped draw the conclusions, rewrote the thesis and wrote the final published version. Is this an example of acceptable ethics?

Publicity. Some scientists violently oppose general publicity and popularization of their work. Others seek publicity, and some even condone or support erroneous and misleading publicity. What should be the attitude of the scientist? Does he owe the public a duty to attempt to explain the purpose and significance of his work? Should chicanery and excessive or misleading publicity on the part of scientists and nonscientists be exposed as a function of scientific societies? It is of interest that the *Principles of Medical Ethics* includes a considerable discussion of the impropriety of advertising and publicity-seeking and that AMA members are required to advise the public against misrepresentation. A firm stand on this issue by scientists generally might be of considerable help in establishing the professional status of the scientist in the public mind.

Multiple authorship

With the change in status of research, owing to its being produced not by independent individuals but by several dependent workers or even large groups, the scientific tradition in respect to the etiquette of authorship needs reinterpretation or extension. The responsibilities involved in multiple authorship or group research must be analyzed.

"Senior" authorship and order of names. To many scientists, the order of the authors' names on a publication has a significance. Is this a tradition that should be preserved, clarified, and enforced, or is it an outmoded, unessential form of etiquette? In current publications, the application seems uncertain and haphazard. Should the concept of the "senior author" (the first one listed) be preserved? If so, should the senior author be the person highest in the administrative rank, the one who has done most of the laboratory work, the one who has written the paper, the one who furnished the original idea, or the one whose technical skill and thoughts have carried along the research?

Administrators and financial supporters. In publications, what consideration should be given to administrators and financial supporters? Some scientists might say that they should be indicated as authors only when their contribution to the actual solution of the problem has been substantial, continuous, and of a high level. Probably most scientists would agree that mere general administrative supervision of a project or even the suggestion of the original idea for the project is insufficient for an authorship. Certainly no one should be granted authorship of any type merely because he has seniority or is in charge of a laboratory We cite as an example a man serving as technical liaison between a company and a research organization who insisted that his name be included as an author, before he would ask for supporting funds for the research, although his total contribution was limited to this action.

Graduate students and technical assistants Criteria are necessary for assessing the role of graduate students and technical assistants in relation to authorship. Should not senior authorship for a graduate student be limited to those instances in which a real contribution. beyond adequate laboratory work, has been made? On the other hand, is it not the duty of the directing professor to encourage the student to his maximum performance rather than use him as a laboratory assistant? If a technical assistant is to be given authorship of any type, more than an adequate performance of routine methods should be required of him.

Group projects. An example of the large group projects that characterize modern science is the penicillin research during World War II. Industrial organizations provide many more examples. Frequently there is no attempt on the part of administrators to set up the program so that the work of individual investigators is kept discrete. The improved quality of work resulting from the establishment of definite responsibility might be the basis for making a definite statement in regard to this problem. The interpretation of the scientific tradition in terms of modern group research is an extremely important and as yet unexplored field.

Preparation of manuscripts. The published paper is the final record of the finished research work, and the medium through which the information is made generally available and useful. With the present shortage of publication space, the preparation of the manuscript becomes more important than ever. Rigid adherence to established scientific traditions on the part of authors and editors becomes increasingly essential.

To many persons, the preparation of a research paper may seem to be a routine matter, but actually it requires a high order of skill and technical knowledge and an acquaintance with scientific traditions. In many researches the actual preparation of the manuscript, the integration of the findings with the prior related work, consideration of the significance of the data, and arrangements for publication may require a considerable portion of the time and skill required for the entire project. Possibly the actual preparation of the manuscript should be a factor in defining the responsibilities of the senior author. Laboratory workers without a good background of knowledge, and research administrators without close daily contact with the laboratory work and a thorough knowledge of the field probably should be discouraged from actual preparation of the manuscript. On the other hand, simple manuscript revision, in spite of the poor writing ability of many scientists, should not generally be made the basis for authorship of a research paper Still another problem is determining the duties and responsibilities of the referees of scientific articles.

The interpretation of scientific traditions, and their formal codification, if that is to be accomplished, are essentially a task for scientists. As this discussion demonstrates, the problems of interpretation are manifold and if they are not solved they may severely hinder the progress of science. The harm done may not only be general but may apply particularly to industrial research of the group type. Incidental effects of the code, but of considerable importance, would be the great improvement in morale among scientific workers, the improvement in the quality of scientific work, the assistance it would give to editors of scientific journals and research administrators, and the basis it would provide for exposing poor work and even instances of chicanery It would be of great assistance in the training of graduate students in the scientific method. The preparation of a formal code of professional ethics should be of considerable value in establishing the professional status of the scientist in the public mind. It seems more than a coincidence that the groups that already have formal statements of their social and professional responsibilities and have definite rules of professional behavior are those definitely accepted by the public as having professional status.

As we have pointed out, violations of professional ethics on the part of scientists are frequent and familiar to all scientists. Sometimes they are deliberate violations for personal power or gain Frequently, they are the results of carelessness or unfamiliarity of research administrators or research workers with the established traditions. They may even result from excessive pressure of work, a condition that appears common in industrial research. Some violations are the result of misguided attempts by editors and reviewers of scientific journals to shorten articles.

Is not the time opportune for our scientific organizations, or some agency of UNESCO, to consider the manner of the application of scientific traditions to the newly developed conditions of scientific research? We suggest that the establishment of a definite code of professional ethics and conduct by our major scientific groups would have profound and favorable effects for science, society, and the scientist.

A mere statement of principles would be of help. An extensive codification and attempt to discipline or expose gross violations might be desirable. Our societies have various ways and means of enforcing regulation. Exclusion from membership and control of publications and means of publicity are powers that could be used to control unscrupulous and continuous violations. There may appear to be an anomaly in scientists' establishing a formal code of ethics to preserve traditions that include independence in their work, but this merely reflects an anomaly in present conditions of scientific work. It seems far better for scientists to affirm such a code positively than to be regimented to an increasing extent without any control over the conditions under which they must work. A.V. Hill *(11)* puts the problem as follows:

> The important thing is not a creed "which except a man believe faithfully he cannot be saved " What matters is that scientific men should argue and discuss the matter of scientific ethics as one of infinite importance to themselves and the rest of mankind with the same honesty, humility and resolute regard for the facts they show in their scientific work.
>
> If they do, then something will surely crystallize out from their discussion, and I have faith enough in the goodness and wisdom of most scientific men to believe that the result on the whole will be good and wise It may in the end be embodied in a new Hippocratic Oath, or it may be absorbed in trade union rules for the scientific profession, or ethical behavior in science may just come to be accepted as an honorable obligation as unbreakable as that of accuracy and integrity

We add that all problems will not be solved, but science is expanding and moving. The rate of progress will be profoundly affected by the consideration that is given to the maintenance and proper application of time-proved scientific traditions as the conditions of scientific work change.

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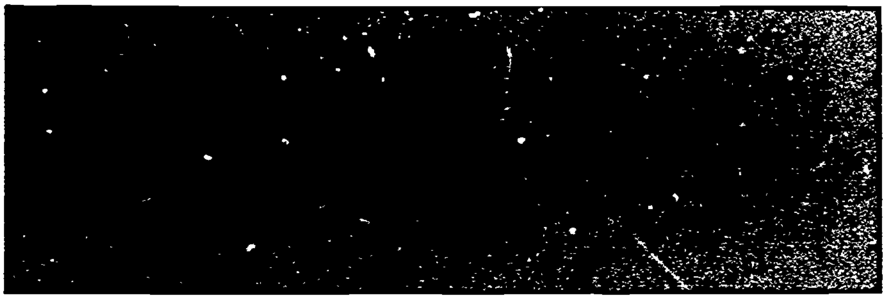

It has been said that science has no ethical basis, that it is no more than a cold, impersonal way of arriving at the objective truth about natural phenomena. This view I wish to challenge, since it is my belief that by examining critically the nature, origins, and methods of science we may logically arrive at a conclusion that science is ineluctably involved in questions of values, is inescapably committed to standards of right and wrong, and unavoidably moves in the large toward social aims.

Human values have themselves evolved. Man arose after some two billions of years of organic evolution, during which species after species originated, flourished, and fell, or occasionally became the progenitors of species that [illegible] new and better adap[illegible] of the evolution [illegible] of [illegible]ness, like [illegible] un[illegible] simply [illegible] genc [illegible] gene of a [illegible] cies, [illegible] perpetu[illegible] grew out of his [illegible] orig[illegible] the environment [illegible] of [illegible] necessary [illegible] as bodily [illegible] clothing was [illegible] vented; the homeostatic regulation of his body temperature and blood pressure, breathing, and predominant direction of blood flow; his embryonic and fetal growth inside the mother and his prolonged dependence upon maternal lactation; the slow maturation that enabled his brain to enlarge so greatly; the keen vision so necessary to the hunter using his weapons—all of these and many other important human characteristics that contributed to the social nature of man and cemented the bonds of family and tribe arose adventitiously, were improved step by step, and endured because they promoted human survival. Our highest ethical values—the love of the mother for her child and of the man for his mate, the willingness to sacrifice one's own life for the safety of the family or tribe, and the impulse to care for the weak, the suffering, the helpless—all of these too had the same primitive beginnings.

But these ethical values are always, in the evolutionary scheme of things, relative, and never absolute. Whenever the environment becomes changed, the [illegible] existing traits becomes [illegible] of natural selection [illegible] alignment of the [illegible] an alteration of the [illegible]rnal [illegible] behavior, a modification [illegible] was [illegible] long [illegible] this [illegible] reproductive [illegible] of [illegible] things, [illegible]

porting accurately to other persons the movements of the stars, the planets, and the sun and moon, the behavior and migrations of the food animals, the usefulness of certain seeds for food and of certain stems for fibers, the poisonous properties of others. For generations all such practical lore was transmitted only by word of mouth, but the day came when useful knowledge could be written down and preserved inviolate from the forgetfulness and the twists of memory. These were the first simple steps in the development of science: observation, reporting, written records, communication. To such must be added the processes of human reasoning, at first mostly by analogy, so often wrong; then by improved analysis, by deduction from an established truth, or by induction of an established truth from a multitude of observations.

Seen aright, science is more than the instrument of man's increasing power and progress. It is also an instrument, the finest yet developed in the evolution of any species, for the malleable adaptation of man to his environment and the adjustment of his environment to man. If the human species is to remain successful, this instrument must be used more and more to control the nature and the rate of social and technological change, as well as to promote it. In this sense, at least, science is far more than a new sense organ for comprehending the real relations of natural phenomena and the regularities we call "laws of nature." It is also man's means of adjustment to nature, man's instrument for the creation of an ideal environment. Since it is preeminently an achievement of so-

[illegible] author is Academic Vice President, State [illegible] of New York, Stony Brook. This article [illegible] condensation of a chapter from the [illegible] *and Ethical Values*, just published [illegible] Carolina Press, Chapel [illegible] reprinted with the permission of the cial man, its primary function is not simply that of appeasing the individual scientist's curiosity about his environment—on the contrary it is that of adjusting man to man, and of adjusting social groups in their entirety to nature, to both the restrictions and the resources of the human environment.

Ethics is a philosophy of morals, a moral system that defines duty and labels conduct as right or wrong, better or worse. The evolutionist is quite prepared to admit the existence of right and wrong in terms of the simple functions of biological structures and processes. The eye is for seeing, an evolutionary adaptation that enables an animal to perceive objects at a distance by means of reflected light rays. Sight conveys information about food, water, danger, companionship, mating, the whereabouts and doings of the young ones, and other vitally important matters. Should one not then say, "To see is right; not to see is wrong"? Similarly, the mind reasons as it does because in the countless ages of evolutionary development its characteristic mental processes led to successful coping with the exigencies of life. Humans whose mental processes, because of different genes, too often led them to wildly erroneous conclusions did not so often leave children to reason in similar ways. It is thus right to be guided by reason, wrong to distrust it. Does it not follow, finally, from consideration of the social role and function of science, that it is *right* to utilize science to develop and regulate human social life, adjustment to change, and rate of social transformation? Conversely, it is *wrong*—morally and ethically wrong—not to do so. We must use whatever light and whatever reason we have to chart our course into the unknown.

Those who distrust science as a guide to conduct, whether individual or social, seem to overlook its pragmatic nature, or perhaps they scorn it for that very reason. Rightly understood, science can point out to us only probabilities of varying degrees of certainty. So, of course, do our eyes and ears, and so does our reason. What science can do for us that otherwise we may be too blind or self-willed to recognize is to help us to see that what is right enough for the individual may be wrong for him as a member of a social group, such as a family; that what is right for the family may be wrong for the nation; and that what is right for the nation may be wrong for the great brotherhood of man. Nor should one stop at that point. Man as a species is a member—only one of many members—of a terrestrial community and an even greater totality of life upon earth. Ultimately, what is right for man is what is right for the entire community of life on earth. If he wrecks that community, he destroys his own livelihood. In this sense, coexistence is not only necessary but also right, and science can reveal to us the best ways to harbor our resources and to exploit our opportunities wisely.

The Subjectivity of Science

From the foregoing description of science as itself an evolutionary product and a human organ produced by natural selection, it may already be guessed that I do not adhere to the view that either the processes or the concepts of science are strictly objective. They are as objective as man knows how to make them, that is true; but man is a creature of evolution, and science is only his way of looking at nature. As long as science is a *human* activity, carried on by individual men and by groups of men, it must at bottom remain inescapably subjective.

Our sensory apparatus and the structure of the human nervous system, within which arise our sensations, grow and develop as they do from the first beginnings in the human embryo because of the particular genetic constitutions we inherit from our parents. First and foremost, we are *human* scientists, not insect scientists, nor even monkey scientists. The long past of our evolutionary history, with its countless selections and rejections of various kinds of genes and combinations of genes, has made us what we are. Try as we will, we cannot break the bonds of our subjective interpretations of the physical events of nature. We are born blind to many realities, and at best can apprehend them only by translating them by means of our instruments into something we can sense with our eyes or ears, into something we can then begin to reason about by developing abstract mental concepts about them, by making predictions on the basis of our hypotheses, and by testing our theories to see whether reality conforms to our notions.

This line of reasoning leads us to the conclusion that the objectivity of science depends wholly upon the ability of different observers to agree about their data and their processes of thought. About quantitative measurements and deductive reasoning there is usually little dispute. Qualitative experiences like color, or inductive and theoretical types of reasoning, leave great room for disagreement. Usually they can be reduced to scientific treatment only if the subjective color can by agreement be translated into some quantitative measurement such as a wavelength, only if the reasoning can be rendered quantitative by use of a calculus of probability. It nevertheless remains a basic fact of human existence that the subjectivity of the individual personality cannot be escaped. We differ in our genes, each of us possessing a genotype unique throughout all past and future human history (unless we happen to possess an identical twin). To the extent that our genes endow us with similar, though not identical, sensory capacities and nervous systems, we may make similar scientific observations, and we may agree to ignore the existence of the variables in our natures that prevent us from ever making exactly the same measurements as someone else or arriving at exactly the same conclusions. But it is perilous to forget our genetic individuality and our own uniqueness of experience. These form the basis of the ineradicable subjectivity of science. In the last analysis science is the common fund of agreement between individual interpretations of nature. What science has done is to refine and extend the methods of attaining agreement. It has not banished the place of the individual observer, experimenter, or theoretician, whose work is perhaps subjective quite as much as objective.

These considerations may seem so obvious as not to require the emphasis just given them. Yet I believe not. Somehow there has crept into our writings about the nature and methods of science a dictum that science is objective while the humanistic studies are subjective, that science stands outside the nature of man. What a profound mistake! Science is ultimately as subjective as all other human knowledge, since it resides in the mind and the senses of the unique individual person. It is constrained by the present evolutionary state of man, by the limitations of his senses and the even more significant limitations of his powers of reason. All that can be claimed for science is that it focuses upon those primary observations about which human observers (most of them) can agree, and that it emphasizes those methods of reasoning which, from empirical results or the successful fulfillment of predictions, most often lead to mental constructs and conceptual schemes that satisfy all the requirements of the known phenomena.

Science, Integrity, and Intellectual Freedom

From a consideration that science is a human activity, inescapably subjective, and a product of biological evolution, it is possible to derive a genuine ethical basis of science. J. Bronowski, in an essay entitled "The Sense of Human Dignity" (*1*, p. 63), has sketched a treatment that serves well for a beginning. The values and duties which are the concern of ethics are social, he affirms. The duties of men hold a society together, he says; and "the problem of values arises only when men try to fit together their need to be social animals with their need to be free men." Philosophy must deal with both the social and individual aspects of value. Most philosophical systems have found this very difficult to do. Thus dialectical materialism swings far to the side of social values and leaves little scope for individual freedom. Positivism and analytic philosophy, as typified by Bertrand Russell and Wittgenstein, on the other hand, emphasize the values of the individual.

Hence, continues Bronowski, because the unit of the positivist or the analyst is one individual man, "positivists and analysts alike believe that the words *is* and *ought* belong to different worlds, so that sentences constructed with *is* usually have a verifiable meaning, but sentences constructed with *ought* never have" (*1*, p. 72).

The issue, then, is simply whether verification can indeed be assumed to be carried out by one man. Bronowski concludes, and I find it impossible to deny, that in the practice of science this supposition is sheer nonsense. Verification depends completely on the existence of records that may be consulted, of instruments that may be used, of concepts that must be understood and be properly utilized. In all these ways, knowledge is a social construct, science a collective human enterprise, and verification is no procedure of the naked, unlettered, resourceless man but an application of the collective tools of the trade and the practiced logic of science to the matter at hand. It is a fallacy to assume that one can test what is true and what is false unaided. But then it must follow that all verification, all science, depends upon communication with others and reliance upon others. Thus we come straight to the *ought* of science, for we must be able to trust the word of others. A full and true report is the hallmark of the scientist, a report as accurate and faithful as he can make it in every detail. The process of verification depends upon the ability of another scientist, of any other scientist who wishes to, to repeat a procedure and to confirm an observation.

Neither the philosophy of dialectical materialism nor that of the individualist accords with the basic nature of man and of scientific truth. The extreme social position leaves no room for the conscience of man and the exercise of intellectual freedom because the community dictates what is right and what a man *ought* to do. Yet the positivist's position is also faulty because "how a man *ought* to behave is a social question, which always involves several people; and if he accepts no evidence and no judgment except his own, he has no tools with which to frame an answer" (*1*, p. 72). Again, "All this knowledge, all our knowledge, has been built up communally; there would be no astrophysics, there would be no history, there would not even be language, if man were a solitary animal" (*1*, p. 73).

"What follows?" asks Bronowski, and answers (*1*, p. 73): "It follows that we must be able to rely on other people; we must be able to trust their word. That is, it follows that there is a principle which binds society together, because without it the individual would be helpless to tell the true from the false. This principle is truthfulness. If we accept truth as a criterion, then we have also to make it the cement to hold society together." Whence he derives the social axiom:

"We OUGHT to act in such a way that what IS true can be verified to be so."

So Bronowski. If his reasoning be accepted, and to me it seems unarguable, we must conclude that the cement of society is nothing less than the basic ethical tenet of science itself. The very possibility of verification, the assurance that one's own conclusions are not dreams, hallucinations, or delusions rests upon confirmation by others, by "competent" observers whom we trust to tell the truth.

The scientist's integrity. Ethics rests upon moral integrity. Science rests upon the scientist's integrity. This is so implicit in all of our science that it is rarely expressed and may be overlooked by novice or layman. Bronowski mentions examples of what happens when this basic moral commandment is violated by a scientist. Lysenko is held up to scorn throughout the world and eventually is deposed (*2*). Kammerer commits suicide (*3*). It is very interesting that both of these notorious examples, and others less well known, such as that of Tower, a quondam professor of biology at the University of Chicago, have related to attempts to "prove" or bolster the theory of the inheritance of acquired characteristics. The singular attractiveness of this theory for violators of scientific integrity is no doubt owing to its social significance, since if true it would offer a quick and easy way for man to control the direction of human evolution and would lessen the obdurate qualities of genes modifiable only by mutation in uncontrollable directions.

It is not so generally recognized by these superficial evolutionary philosophers that, if true, the inheritance of characters produced through modifications of the environment would call in question the value of all evolutionary gains, since the modified characters would themselves have no real genetic permanence and would shift and vary with every change of environment. They also do not recognize one of the most essential aspects of heredity, the protection of the genetic nature against vicissitudes. The reason why death is so necessary a part of life is that the ground must be cleared for fresh life. The reason why the genotype must remain unmodifiable by ordinary environmental causes is because the course of life for every individual involves the cumulative effects of injury, disease, and senescence. The new generation must indeed start *fresh*, that is, free from all the disabilities incurred during life by its parents and remoter ancestors. Evolution through the action of natural selection upon mutations, most of which are harmful and nonadaptive, while only a rare exemplar among them is possibly advantageous, is a process slow in the extreme. But it preserves the gains of the past, and it permits every generation to be born anew, unburdened by decrepitude, to try out its varieties of genotypes in each niche of the environment.

The loss of scientific integrity through deliberate charlatanry or deception is less common than the violation of scholarly honesty through plagiarism. The theft of another man's ideas and the claim that another's discovery is one's own may do no injury to the body of scientific knowledge, if the substance of what is stolen be true. It may even do no harm to the original discoverer, who may be dead or in no need of further credit to advance his own career. It is nevertheless a canker in the spirit of the thief and does damage to the fabric of science by rendering less trustworthy the witness of the scientist.

Plagiarism shades into unacknowledged borrowing. Which of us in fact can render exactly the sources of all his ideas? Psychologists have now amply demonstrated the ease with which self-deception enters into the forgetfulness of borrowed benefits. The wintry wind of man's ingratitude blows only on the donor of benefits forgot. Around the self-deluded recipient blow only the mildest, gentlest zephyrs of spring. The newer patterns of scientific publication and support of research have multiplied a thousandfold the opportunities for the scientist's self-deception. Editors of scientific journals today customarily rely upon referees for opinions regarding the merit of manuscripts submitted for publication. The enormous expansion of scientific activity and the development of hundreds of new specialties have made this referee system necessary. The best referee is of course some other scientist who is working closely on the same scientific problems but is not associated with the author in the actual work —in other words, a competitor, since we must not forget that scientists are people who must earn a living, and since compensation and repute follow productivity and publication. Natural selection is at work among scientists, too! What is most alarming about the workings of the referee system is not the occasional overt lapse of honesty on the part of some referee who suppresses prompt publication of a rival's work while he harvests the fruit by quickly repeating it—perhaps even extending it—and rushing into publication with his own account. What is far more dangerous, I believe, because it is far more insidious and widespread, is the inevitable subconscious germination in the mind of any referee of the ideas he has obtained from the unpublished work of another person. If we are frank with ourselves, none of us can really state where most of the seminal ideas that lead us to a particular theory or line of investigation have been derived. Darwin frankly acknowledged the ideas of Malthus which led him to the Theory of Natural Selection; but although he was one of the most honest of men, and one who was deeply troubled when Alfred Russel Wallace sent him in 1858 the brief paper setting forth his own parallel derivation of Darwin's theory, Darwin nevertheless never made the slightest acknowledgment of the idea of natural selection which he had surely read in the work of Edward Blyth in 1835 and 1837 (*4*). We may guess that Darwin's reasoning at the time went rather as follows:

> Blyth's conception is that natural selection leads to a restriction of hereditary variation in populations. Through elimination of the more variable specimens in a species, nature keeps the species true to type and prevents it from becoming maladapted to its environment. Blyth's Natural Selection is not an evolutionary force at all, but instead is a force for maintenance of the status quo.

Yet it is very hard to understand why, when the full significance of the action of natural selection dawned upon Darwin, he did not reexamine the ideas of Edward Blyth. It should have been perfectly evident to him that the very same force that would eliminate variation and maintain the status quo of the species in a stationary environment would operate quite differently in a changing environment. Will we then ever know the extent to which Darwin was really indebted to Blyth, or how the ideas he probably rejected as invalid actually prepared the way for his reception of Malthus's thoughts in 1838?

The conscientious referee of unpublished scientific manuscripts is similarly a gleaner in the harvest fields of others. The only possible way to avoid taking an unfair advantage would be to refuse to referee any manuscripts that might conceivably have a relationship to one's own research work. The consequences [illegible] editors left with piles of unevaluated manuscripts might become desperate, were there not, as I believe, a reasonable solution in the possibility that the role of referee could be limited to scientists who have ceased to do active experimental work themselves. What with the increasing life span and the large number of retired but mentally vigorous older scientists, the supply of competent referees would perhaps be sufficient. To be sure, the criticism may be raised that the older scientific men cannot properly evaluate the significance and merit of really revolutionary new ideas and lines of work. Neither, for the most part, can the young! A combination of older referees in the field and younger ones knowledgeable but not working in the same specialty might solve this difficulty.

What has been said about referees applies with even greater force to the scientists who sit on panels that judge the merit of research proposals made to government agencies or to foundations. The amount of confidential information directly applicable to a man's own line of work acquired in this way in the course of several years staggers the imagination. The most conscientious man in the world cannot forget all this, although he too easily forgets when and where a particular idea came to him. This information consists not only of reports of what has been done in the recent past but of what is still unpublished. It includes also the plans and protocols of work still to be performed, the truly germinal ideas that may occupy a scientist for years to come. After serving for some years on such panels I have reached the conclusion that this form of exposure is most unwise. One simply cannot any longer distinguish between what one properly knows, on the basis of published scientific information, and what one has gleaned from privileged documents. The end of this road is self-deception on the one hand, or conscious deception on the other, since in time scientists who must make research proposals learn that it is better not to reveal what they really intend to do, or to set down in plain language their choicest formulations of experimental planning, but instead write up as the program of their future work what they have in fact already performed. Again, the integrity of science is seriously compromised.

Science and intellectual freedom. The first commandment in the ethical basis of science is complete truthfulness, and the second is like unto it:

Thou shalt neither covet thy neighbor's ideas nor steal his experiments.

The third is somewhat different. It requires fearlessness in the defense of intellectual freedom, for science cannot prosper where there constraint upon daring thinking. here society dictates what experiments may be conducted, or where the statement of one's conclusions may lead to loss of livelihood, imprisonment, or even death.

This is a hard ethic to live by. It brought G rdano Bruno to the stake in 1600. The recantation of Galileo was an easier way; the timidity of Descartes and Leibniz, who left unpublished their more daring scientific thoughts, was understandably human but even less in the interest of science or, ultimately, of the society that felt itself threatened. Whether in the conflict of science with religion, or with political doctrine (as in Nazi Germany), or with social dogma (as in the Marxist countries), scientists must be willing to withstand attack and vilification, ostracism and punishment, or science will wither away and society itself, in the end, be the loser.

From the beginning the inveterate foe of scientific inquiry has been authority—the authority of tradition, of religion, or of the state—since science can accept no dogma within the sphere of its investigations. No doors must be barred to its inquiries, except by reason of its own limitations. It is the essence of the scientific mind not only to be curious but likewise to be skeptical and critical—to maintain suspended judgment until the facts are in, to be willing always, in the light of fresh knowledge, to change one's conclusions. Not even the "laws" of science are irrevocable decrees. They are mere summaries of observed phenomena, ever subject to revision. These laws and concepts remain testable and challengeable. Science is thus wholly dependent upon freedom—freedom of inquiry and freedom of opinion.

But what is the value of science to man, that it should merit freedom? There are those, indeed, who say that science has value only in serving our material wants. To quote one of them: "Science is a social phenomenon, and like every other social phenomenon is limited by the injury or benefit it confers on the community. . . . The idea of free and unfettered science . . . is absurd." Those were the words of Adolf Hitler, as reported by Hermann Rauschning (*5*). In Soviet states a similar view is held officially; and in the Western democracies, likewise, not a few scientists as well as laymen have upheld a similar opinion. The British biologist John R. Baker has pointed out that this view shades through others, such as the admission that scientists work best if they enjoy their work, and the supposition that science has value in broadening the outlook and purging the mind of pettiness, to the view that a positive and primary value of science lies in its creative aspect "as an end in itself, like music, art, and literature" (*6*). "Science aims at knowledge, not utility," says Albert Szent-Györgyi (*7*); and Alexander von Humboldt wrote in his masterpiece, *Cosmos*, that "other interests, besides the material wants of life, occupy the minds of men" (*8*).

It is readily demonstrated that the social usefulness of the conclusions of science can rarely be predicted when the work is planned or even after the basic discoveries have been made. John R. Baker, in his book *Science and the Planned State*, has cited numerous examples that show the impracticability of too narrowly planned a program of scientific research. The sphere of investigation must be determined by the investigator's choice rather than by compulsion—by perception of a problem to be solved rather than by a dogma to be accepted blindly. Science must be free to question and investigate any matter within the scope of its methods and to hold and state whatever conclusions are reached on the basis of the evidence—or it will perish. But science is represented only by the individual scientists. These persons must acknowledge the moral imperative to defend the freedom of science at any cost to themselves. Every Darwin needs a Thomas Henry Huxley. Every Lysenko demands his martyred Vavilov, his hundreds of displaced geneticists before he is finally deposed. Modern science, from its very beginnings near the end of the 16th century, became immediately concerned with a major political issue, the freedom of the scientist to pursue the truth wherever it might lead him, even though that conclusion might be highly disturbing to settled religious beliefs or social conventions and practice. The pyre of Bruno and the ordeal of Galileo led directly in spirit to the attacks on Charles Darwin 250 years later and to latter-day instances of the social suppression of scientific findings. The distortion of genetics by racists in Nazi Germany finds a counterpart in the United States. Mendelian genetics in the U.S.S.R. and the nutritive qualities of oleomargarine in Wisconsin share a similar fate. The third commandment then reads:

Thou shalt defend the freedom of scientific investigation and the freedom of publication of scientific opinion with thy life, if need be.

Science and communication. Inasmuch as science is intrinsically a social activity and not a solitary pleasure, another primary aspect of the ethics of science is the communication to the world at large, and to other scientists in particular, of what one observes and what one concludes. Both the international scope of scientific activity and the cumulative nature of scientific knowledge lay upon the individual scientist an overwhelming debt to his colleagues and his forerunners. The least he can do in return, unless he is an ingrate, is freely to make his own contributions a part of the swelling flood of scientific information available to all the world.

There are at least five distinct obligations his indebtedness places upon each scientist. The first of these is the obligation to publish his methods and his results so clearly and in such detail that another may confirm and extend his work. The pettiness and jealousy that lead some scientists, in their effort to stay ahead of the ruck, to withhold some significant step of procedure or some result essential to full understanding of the stated conclusions have no place in the realm of science. In other instances it is sheer laziness or procrastination that is at fault. Whatever the only-too-human reason, science suffers.

A second obligation that is far more frequently neglected is the obligation to see that one's contributions are properly abstracted and indexed, and thus made readily available to workers everywhere. Many scientists ignore this obligation completely. Yet, as the sheer volume of scientific publication passes a half-million and soon a million articles a year, it is obviously insufficient to add one's own leaflet to the mountains of paper cramming the scientific libraries of the world. The need to have scientific findings abstracted and indexed has been fully recognized by such international bodies as the International Council of Scientific Unions: its Abstracting Board has urged every author to prepare an abstract in concise, informative style, to be printed at the head of each scientific paper; and the editors of most scientific journals have now made this a requirement for acceptance and publication of a paper. Nevertheless, few authors prepare their abstracts without a reminder, and few heed the requirements for a concise, informative summary that will permit proper indexing of the major items treated in the paper.

A third obligation is that of writing critical reviews, which will be true syntheses of the knowledge accumulating in some field. I firmly believe that there is no scientific activity today more necessary and at the same time less frequently well done than this one. I have said elsewhere (9):

> To be sure, the scientist seeks for facts —or better, he starts with observations. . . . But I would say that the real scientist, if not the scholar in general, is no quarryman, but is precisely and exactly a builder—a builder of facts and observations into conceptual schemes and intellectual models that attempt to present the realities of nature. It is the defect and very imperfection of the scientist that so often he fails to build a coherent and beautiful structure of his work . . .
>
> The creativity of scientific writing lies precisely here. The task of the writer of a critical review and synthesis . . . is not only indispensable to scientific advance —it surely constitutes the essence of the scientific endeavor to be no mere quarryman but in some measure a creator of truth and understanding. The aesthetic element that makes scientist akin to poet and artist is expressed primarily in this broader activity.

The critical nature of the critical review grows from our constant forgetfulness of all this. The young scientist is taught carefully and methodically to be a quarryman or a bricklayer. He learns to use his tools well but not to enlarge his perspective, develop his critical powers, or enhance his skill in communication. The older scientist is too often overwhelmed by detail, or forced by the competition of the professional game to stick to the processes of "original research" and "training." The vastness of the scientific literature makes the search for general comprehension and perception of new relationships and possibilities every day more arduous. The editor of the critical review journal finds every year a growing reluctance on the part of the best qualified scientists to devote the necessary time and energy to this task. Often it falls by default to the journeyman of modest talent, a compiler rather than critic and creator, who enriches the scientific literature with a fresh molehill in which later compilers may burrow.

All this need not be so, but it will remain so without a deeper sense of the obligation of the scientist to synthesize and present his broadest understanding of his own field of knowledge. Tomorrow's science stands on the shoulders of those who have done so, no less than on the shoulders of the great discoverers.

A fourth obligation is communication to the general public of the great new revelations of science, the important advances, the noble syntheses of scientific knowledge. There have always been a few eminent scientists who did not scorn to do this: Thomas Henry Huxley, John Tyndall, and Louis Pasteur set the pattern in the 19th century, and in our own time there have indeed been many who followed their precedent. Yet there seems to be a growing tendency to turn this obligation over to professional science writers who, however good, should not replace the direct, personal, and authoritative appeal of the scientist to the general public. As our culture and civilization become day by day more completely based on scientific discovery and technological application, as human exploration becomes ever more restricted to the endless frontiers of science, every citizen must know whereby he lives and whereupon he leans. A democracy rests secure only upon a basis of enlightened citizens who have imbibed the spirit of science and who comprehend its nature as well as its fruits. In fulfilling the requirement of our age for the public understanding of science the scientist must shirk no duty.

A final obligation in the total purview of scientific communication is the obligation to transmit the best and fullest of our scientific knowledge to each succeeding generation. It is well said that genetic transmission of human characteristics and powers is now far overshadowed by cultural inheritance. The transmission of knowledge is the role of the teacher, and the obligation of the scientist to teach is his last and highest obligation to the society that gives him opportunity to achieve his goals.

To every scientist—to some sooner, to some only late—there comes the realization that one lifetime is too short and that other hands and other minds must carry on and complete the work. Only a few scientists are therefore content to limit their entire energies to exploration and discovery. Research is one end, but the other must be the training of the new generation of scientists, the transmission of knowledge and skill, of insight and wisdom. The latter task is no less necessary, no less worthy. From the beginnings of human history, the exponentially accelerating growth of human power . . . has required each generation to instruct and inform the next.

This is the challenge that faces every teacher of a science as he steps into the classroom or guides the early efforts of an individual student. Here, in this sea of fresh faces—here, amidst the stumblings and fumblings—may be the Newton or Einstein, the Mendel or Darwin of tomorrow. For few—so very few—men are self-taught. The teacher cannot supply the potentialities of his students, but he is needed to see that the potentialities will unfold, and unfold fully. His is not only the task of passing on the great tradition of the past, with its skills and accumulated knowledge; he must also provide breadth and perspective, self-criticism and judgment, in order that a well-balanced scientist may grow to full stature and continue the search.

Of all the resources of a nation, its greatest are its boys and girls, its young men and women. Like other material resources, these can be squandered or dissipated. They are potential greatness, but they are only potentialities. Science creates knowledge and knowledge generates power, but knowledge resides only in the minds of men who first must learn and be taught, and power is tyranny unless it be guided by insight and wisdom, justice and mercy. The greatest of men have been teachers, and the teacher is greatest among men (*10*).

The Social and Ethical Responsibilities of Scientists

The scientist escapes lightly—instead of ten commandments only four: to cherish complete truthfulness; to avoid self-aggrandizement at the expense of one's fellow-scientist; fearlessly to defend the freedom of scientific inquiry and opinion; and fully to communicate one's findings through primary publication, synthesis, and instruction. Out of these grow the social and ethical responsibilities of scientists that in the past 20 years have begun to loom ever larger in our ken.

These may be considered under the three heads of proclamation of benefits, warning of risks, and discussion of quandaries. The first of these, the advertisement of the benefits of science, seems to be sufficiently promoted in these days when science is so well supported by government and private agencies and when grants are justified on the basis of social benefits. Every bit of pure research is heralded as a step in the conquest of nuclear or thermonuclear power, space exploration, elimination of cancer and heart disease, or similar dramatic accomplishments. The ethical problem here is merely that of keeping a check-rein on the imagination and of maintaining truthfulness. But the truth itself is so staggering that it is quite enough to bemuse the public.

Since 1945 more and more scientists have become engaged in warning of the great risks to the very future of man of certain scientific developments. First the atomic bomb and then the hydrogen bomb brought swift realization of the possibility of the destruction of all civilization and even the extinction of all human life were a nuclear war to break out. The atomic scientists, conscience-stricken, united to secure civilian control of nuclear energy. Albert Einstein and Bertrand Russell issued an appeal to scientists to warn the world of the tragic consequences of overoptimism and of an unbridled arms race. Joined by a dozen notable scientists, they initiated the "Pugwash" Conferences on Science and World Affairs in 1957. In these conferences scientists of East and West sat down together to talk, in objective scientific terms, of the military and political problems of the world and their resolution. It was not that the scientists at all felt themselves to be more highly qualified than diplomats and statesmen, economists or lawyers, to find solutions of the most difficult and delicate problems of international relations. They acted on two grounds only: that they understood the desperate nature of the situation about which the world must be warned in time; and that they hoped discussions by persons accustomed to argue in objective, scientific terms might pave the way for better understanding and more fruitful negotiation on the part of officials. In the ensuing discussions of the effects of fallout from nuclear weapons tests on persons now living and on the generations yet unborn, scientists played a very important role. In no small measure, I believe, historians of the future will recognize how great a part was played by the scientists in bringing about the partial nuclear weapons test ban. Scientists are now deeply involved in politics, and naturally enough often on both sides of the argument, for although they may agree upon the basic scientific facts which are relevant to the issue, there are rarely enough established facts to clinch the argument and there is always room for differences of opinion in interpreting the facts. In these matters the ethic of the matter requires the scientist to state his opinion on matters of social concern, but at the same time to distinguish clearly between what he states to be fact and what opinion he holds. Moreover, his opinion about matters within his technical sphere of competence is an "informed" opinion; his opinion about other matters, even other scientific matters, is that of a layman. He must in all honesty make clear to the public in what capacity he speaks.

Nuclear war is only one of the dire misfortunes that are poised above the head of modern man. The unrestricted and appalling rate of population increase in most countries of the world, if projected just a few decades into the future, staggers the imagination with its consequences. Effective control of the birth rate is the only conceivable answer to effective reduction by modern health measures of the death rate. This is the world problem second in importance at the present time, and must engage the conscience of the scientist.

The problem of the future is the ethical problem of the control of man over his own biological evolution. The powers of evolution now rest in his hands. The geneticist can define the means and prognosticate the future with some accuracy. Yet here we enter the third great arena of ethical discussion, passing beyond the benefits of science and the certain risks to the nebulous realm of quandaries. Man must choose goals, and a choice of goals involves us in weighing values—even whole systems of values. The scientist cannot make the choice of goals for his people, and neither can he measure and weigh values with accuracy and objectivity. There is nonetheless an important duty he must perform, because he and he alone may see clearly enough the nature of the alternative choices, including laissez faire, which is no less a choice than any other. It is the social duty and function of the scientist in this arena of discussion to inform and to demand of the people, and of their leaders too, a discussion and consideration of all those impending problems that grow out of scientific discovery and the amplification of human power. Science is no longer—can never be again—the ivory tower of the recluse, the refuge of the asocial man. Science has found its social basis and has eagerly grasped for social support, and it has thereby acquired social responsibilities and a realization of its own fundamental ethical principles. The scientist is a man, through his science doing good and evil to other men, and receiving from them blame and praise, recrimination and money. Science is not only to know, it is to do, and in the doing it has found its soul.

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27 September 1963

Ethical Code for Scientists?

In recent months a bibliography (*1*) has been assembled entitled "Some ethical problems of science and technology." It covers the period from January 1955 to July 1963 and includes about 300 references in English in a compilation characterized as "not exhaustive."

This bibliography supplies ample evidence of the interest of scientists, engineers, and the public in the ethical aspects of the relationships of scientists and engineers to society and to one another. There emerges, however, one item of substantial difference between the approaches of the engineers and the scientists to their ethical problems, which deserves attention. Engineers have shown a definite interest in organized action (*2*) to improve ethical practice—for example, by emphasis on ethical considerations in the training of engineers, and by the adoption of formal codes of ethics by the various professional societies of engineering. Except among the psychologists, who have adopted a set of "Ethical Standards for Psychologists" (*3*), there is no evidence of similar action by scientists, who seem to have confined their efforts in the area of ethics to discussion.

It is true that it has been proposed at least twice (*4*) that scientists as a group should adopt a code of ethical practice. In their proposal, Pigman and Carmichael, in 1950, discussed the scope of such a code in some detail, but thus far there has been no indication that these, or any similar proposals, are being acted upon.

In taking formal action in the area of ethics, engineers are in accord with traditions long established in other professions (*5*) and with a strong trend in many other occupational groups toward ethical self-regulation. It is tempting, therefore, to speculate on the reasons for the divergence of scientists from what has become substantially [illegible] norm of social conduct.

One relevant factor, clearly, is the traditional remoteness of scientists from the temptations of the market place and from stresses generated by competition for professional advantage, for power, and for influence. But even in 1950 Pigman and Carmichael were observing that this remoteness was a thing of the past. Today, such a worldly problem as conflict of interest (*6*) is far from a trivial concern for many scientists, and one can readily argue that, in his role as government adviser, government contractor, government official dispensing large sums of public money, grant recipient, entrepreneur, consultant, supervisor, or employee, the scientist is at least as much enmeshed in ethical problems as the engineer.

Perhaps scientists have merely been somewhat slow to adapt to the great changes which have taken place so rapidly in the scientific professions in recent years. Perhaps the reasons for the difference in approach lie deeper. In any case, the record of need and of precedent suggest that a course of positive action in the area of ethics is something which merits the thoughtful attention of the scientist and the scientist-educator, and of their professional organizations.

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15 November 1963

Code of Ethics

Lawrence Cranberg states [*Science* **141**, 1242 (1963)] that, in contrast to engineers, psychologists, and members of other professions, scientists have no code of ethics, probably because of their remoteness from the marketplace or their slowness to adapt to the great changes which have taken place in recent years. He suggests that we devote our thoughtful attention to this matter.

Very strong in my own scientific upbringing was the principle of "scientific honesty" and the complete realization that this is the very essence of science. I have also seen one or two instances of how rapidly and completely even outstanding scientists disappeared from the scientific community when caught in an overt violation of this ethic. It certainly never occurred to me that this was a matter on which we should vote! I wonder if adopting it formally would make it more effective.

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Cranberg, in his discussion of an ethical code for scientists, seems surprised that no action has been taken in this area. Could it be that the scientific community as a whole feels that such a code is unnecessary? My guess would be "yes"!

From certain points of view a code of ethics is implicit in the word *scientist*. The game of science is played under certain rules—uncodified, yes, but nevertheless present and adhered to by most scientists. Cranberg's examples of codes in certain professions are not applicable to scientists. By and large, the medical profession, lawyers, engineers, psychologists, and so on have codes set up primarily for legal purposes, not for moral purposes.

The mere thought of setting up a code of ethics for scientists is insulting!

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6 December 1963

Ethical Code for Scientists?

Lawrence Cranberg [*Science* **141**, 1242 (1963)] makes the point that scientists have been dilatory, as compared to engineers, in doing more than merely talk about formal codes of ethics for their professions. I am not in a position to equate the efforts of the various professions in making clear the relationship of their work to society. However, I wish to point out that one group of scientists, the Society for Social Responsibility in Science, has taken its social responsibilities seriously.

Each scientist, in becoming a member of this society, agrees: "(1) to foresee, insofar as possible, the results of his professional work, (2) to assume personal moral responsibility for the consequences of this work, not delegating this responsibility to his employer, (3) to put his own efforts only into that work which he feels will be of lasting benefit to mankind, and (4) to share his scientific knowledge, and such ethical judgments as are based upon it, with government and laymen in order that they may intelligently use the tools which science provides."

This is, in effect, a code of ethics, which, as Cranberg says, is much needed today.

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However worthy the SSRS may be, its concern with a limited, special range of ethical problems and its existence apart from the main body of professional scientific organizations only emphasize the disparities which exist between scientists and other occupational groups with respect to ethical education and regulation. These disparities remain to be justified or eliminated.

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While the authors in the preceding chapters were expressing their concerns about the increasing connections between science, government, and the rest of society, other members of the scientific community were turning attention to the general lack of public understanding of science. In a culture dominated by the applications of scientific work, public awareness of the human side of science, especially understanding of the details of daily life in the laboratory, was not keeping up with the astounding pace of scientific development.

This isolation of the affairs of scientists from the affairs of ordinary life is dangerous, several writers noted, because it leads to false images and stereotypes that perpetuate myths based on ignorance and false hopes. If the promise of science did not live up to the public's belief in its power to produce certainty and progress, the backlash would be enormous.

Gerald Holton's introduction to these themes appeals to intellectuals within and outside the world of science to diminish the separation between scientists and humanists. To foster greater public understanding of the meaning of progress in scientific research, Holton urges colleagues to shatter false images of science in literature and the arts and to replace these with a contemporary picture in keeping with the role of science in the modern world.

Robert S. Morison, observing a growing skepticism about the value of rationality throughout society, fears that skeptics might eventually erode public support for science. He concludes that science can no longer afford to be comfortably isolated from ordinary human affairs, and urges the scientific community to redouble its efforts to present science as a fully understandable process.

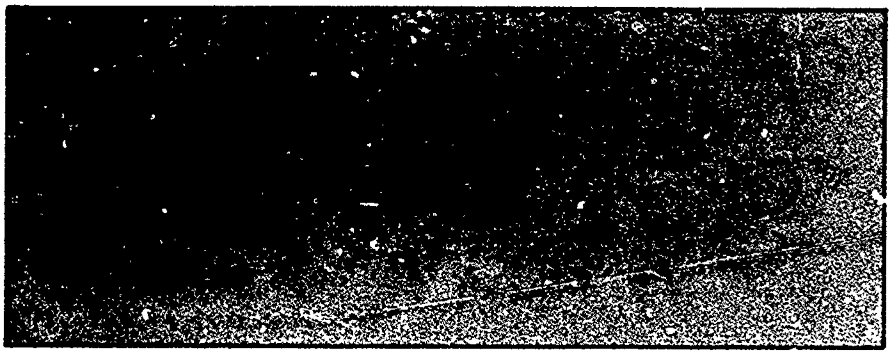
One of the more striking consequences of increased concerns about public understanding of science has been the search for new ways to observe and record the actual behavior of scientists and engineers as they carry out their professional work. June Goodfield, in her plea for more humanity in science, calls for a study of science "as it is done" as a prerequisite of reconstructing the history of science. In particular, she calls for stronger efforts to explicate the real nature of scientific activity in order to assist its incorporation into human culture.

In describing the life and times of Albert Einstein, Carlos Chagas captures the essence of Einstein's professional and political development, giving particular emphasis to Einstein's battles against anti-Semitism, his efforts to promote world peace, and his postwar activities dedicated to educating others about the horrors of nuclear war. Chagas observes that Einstein, perhaps one of the few scientists easily recognized and understood by the public at large, was a visible conscience for many in a world of anguish.

The increased emphasis on the humanity of scientists led to more awareness within the scientific community of the human dilemmas faced by its members. The first chair of the National Academy of Sciences' Committee on Human Rights, Robert W. Kates, describes the committee's effort to respond to human rights violations affecting scientists throughout the world. He identifies the troubling issues related to the nature of human rights, the choice of cases for action by the Academy, and the method of protest.

In an editorial in 1978, William D. Carey notes that the boundaries that once clearly separated science from ordinary life no longer remain intact. He encourages scientists to overcome their indifference to human rights violations, to respond to government controls on science textbooks, and to deepen their involvement with the rights of the physically handicapped, women, and minorities.

Questions of freedom, responsibility, and ethics in science ultimately lead to cracks in the strict barriers that once existed between the abstract principles derived from observations of natural phenomena and the social values that govern everyday life. These cracks appear most noticeably in situations where individual scientists become advisers and advocates in public debates designed to influence government decisions or regulatory actions. The authors in this chapter, in calling more attention to the human side of science, set the stage for blurring distinctions between the role of scientist as scientist and the role of scientist as citizen —*RC*



When future generations look back to our day, they will envy us for having lived at a time of brilliant achievement in many fields, and not least in science and technology. We are at the threshold of basic knowledge concerning the origins of life, the chemical elements, and the galaxies. We are near an understanding of the fundamental constituents of matter, of the process by which the brain works, and of the factors governing behavior. We have launched the physical exploration of space and have begun to see how to conquer hunger and disease on a large scale. Scientific thought appears to be applicable to an ever wider range of studies. With current technical ingenuity one can at last hope to implement most of the utopian dreams of the past.

Hand in hand with the quality of excitement in scientific work today goes an astonishing quantity. The world-wide output is vast. There are now over 50,000 scientific and technical journals, publishing annually about 1,200,000 articles of significance for some branch of research and engineering in the physical and life sciences. Every year there are about 60,000 new science books and 100,000 research reports (*1*). And the amount of scientific work being done is increasing at a rapid rate, doubling approximately every 20 years. Every phase of daily and national life is being penetrated by some aspect of this exponentially growing activity.

It is appropriate, therefore, that searching questions are now being asked about the function and place of this lusty giant. Just as a man's vigorously pink complexion may alert the trained eye to a grave disease of the circulatory system, so too may the spectacular success and growth of science and technology turn out, on more thorough study, to mask a deep affliction of our culture. And indeed, anyone committed to the view that science should be a basic part of our intellectual tradition will soon find grounds for concern.

Some of the major symptoms of the relatively narrow place science, as properly understood, really occupies in the total picture are quantitative. For example, while the total annual expenditure for scientific research and development in this country is now at the high level of over $10 billion, basic research—the main roots of the tree that furnishes scientific knowledge and the fruits of technology—has a share of about 7 percent at best (*2*). Correspondingly, a recent manpower study showed that of the 750,000 trained scientists and engineers, only 15,000 are responsible for the major part of the creative work being done in basic research (*3*). Another nationwide survey found that in 1958 nearly 40 percent of the men and women who had attended college in the United States confessed that they had taken not a single course in the physical and biological sciences (*4*, p. 150). Similarly, in contrast to the overwhelming amount of, and concern with science and technology today, the mass media pay only negligible attention to their substance: the newspapers have been found to give less than 5 percent of their (nonadvertising) space to factual presentations of science, technology, and medicine, and television stations, only about 0.3 percent of their time (*4*, pp. 1–3; *5*). In short, all our voracious consumption of technological devices, all our talk about the threats or beauties of science, and all our money spent on engineering development should not draw attention from the fact that the pursuit of scientific knowledge itself is not a strong component of the operative system of general values.

The Atomization of Loyalties

In the qualitative sense, and particularly among intellectuals, the symptoms are no better. One hears talk of the hope that the forces of science may be tamed and harnessed to the general advance of ideas, that the much deplored gap between scientists and humanists may be bridged. But the truth is that both the hopes and the bridges are illusory. The separation—which I shall examine further—between the work of the scientist on the one hand and that of the intellectual outside science on the other is steadily increasing, and the genuine acceptance of science as a valid part of culture is becoming less rather than more likely.

Moreover, there appears at present to be no force in our cultural dynamics strong enough to change this trend. This is due mainly to the atrophy of two mechanisms by which the schism was averted in the past. First, the common core of their early education and the wide range of their interests was apt to bring scholars and scientists together at some level where there could be mutual communication on the subjects of their individual competence; and second, the concepts and attitudes of contemporary science were made a part of the general humanistic concerns of the time. In this way a reasonable equilibrium of compatible interpretations was felt to exist, during the last century, between the concepts and problems of science on the one hand and of intelligent common sense on the other; this was also true with respect to the scientific and the nonscientific aspects of the training of intellectuals. Specialists, of course, have always complained of being inadequately appreciated; what is more, they are usually right. But although there were some large blind spots and some bitter quarrels, the two sides were not, as they are now in danger of coming to be, separated by a gulf of ignorance and indifference (*6*).

It is of course not my purpose here to urge better science education at the expense of humanistic and social studies. On the contrary: the latter do not fare much better than science does, and the shabby effort devoted to science is merely the symptom of a more extensive sickness of our educational systems. Nor do I want to place all blame on educators and publicists. Too many scientists have forgotten that especially

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at a time of rapid expansion of knowledge they have an extra obligation and opportunity with respect to the wider public, that some of the foremost research men, including Newton and Einstein, took great pains to write expositions of the essence of their discoveries in a form intended to be accessible to the nonscientist. And in the humanities, too many contributors and interpreters seem to scoff at Shelley's contention in his *Defence of Poetry* that one of the artist's tasks is to "absorb the new knowledge of the sciences and assimilate it to human needs, color it with human passions, transform it into the blood and bone of human nature."

It is through the accumulation of such neglects just as much as through deterioration in the quantity and quality of instruction given our future intellectual leaders that the acceptance of science as a meaningful component of our culture has come to be questioned. Again, this process is to a large extent merely one aspect of the increasing atomization of loyalties within the intelligentsia. The writer, the scholar, the scientist, the engineer, the teacher, the lawyer, the politician, the physician—each now regards himself first of all as a member of a separate, special group of fellow professionals to which he gives almost all his allegiance and energy; only very rarely does the professional feel a sense of responsibility toward, or of belonging to, a larger intellectual community. This loss of cohesion is perhaps the most relevant symptom of the disease of our culture, for it points directly to one of its specific causes. As in other cases of this sort, this is a failure of image.

Pure Thought and Practical Power

Each person's image of the role of science may differ in detail from that of the next, but all public images are in the main based on one or more of seven positions. The first of these goes back to Plato and portrays science as an activity with double benefits: Science as pure thought helps the mind find truth, and science as power provides tools for effective action. In book 7 of the *Republic*, Socrates tells Glaucon why the young rulers in the Ideal State should study mathematics: "This, then, is knowledge of the kind we are seeking, having a double use, military and philosophical; for the man of war must learn the art of number, or he will not know how to array his troops; and the philosopher also, because he has to rise out of the sea of change and lay hold of true being. . . . This will be the easiest way for the soul to pass from becoming to truth and being."

The main flaw in this image is that it omits a third vital aspect. Science has always had also a mythopoeic function—that is, it generates an important part of our symbolic vocabulary and provides some of the metaphysical bases and philosophical orientations of our ideology. As a consequence the methods of argument of science, its conceptions and its models, have permeated first the intellectual life of the time, then the tenets and usages of everyday life. All philosophies share with science the need to work with concepts such as space, time, quantity, matter, order, law, causality, verification, reality. Our language of ideas, for example, owes a great debt to statics, hydraulics, and the model of the solar system. These have furnished powerful analogies in many fields of study. Guiding ideas—such as conditions of equilibrium, centrifugal and centripetal forces, conservation laws, feedback, invariance, complementarity—enrich the general arsenal of imaginative tools of thought.

A sound image of science must embrace each of the three functions. However, usually only one of the three is recognized. For example, folklore often depicts the life of the scientist either as isolated from life and from beneficent action (7) or, at the other extreme, as dedicated to technological improvements.

Iconoclasm

A second image of long standing is that of the scientist as iconoclast. Indeed, almost every major scientific advance has been interpreted—either triumphantly or with apprehension—as a blow against religion. To some extent science was pushed into this position by the ancient tendency to prove the existence of God by pointing to problems which science could not solve at the time. Newton thought that the regularities and stability of the solar system proved it "could only proceed from the counsel and dominion of an intelligent and powerful Being," and the same attitude governed thought concerning the earth's formation before the theory of geological evolution, concerning the descent of man before the theory of biological evolution, and concerning the origin of our galaxy before modern cosmology. The advance of knowledge therefore made inevitable an apparent conflict between science and religion. It is now clear how large a price had to be paid for a misunderstanding of both science and religion: to base religious beliefs on an estimate of what science cannot do is as foolhardy as it is blasphemous.

The iconoclastic image of science has, however, other components not ascribable to a misconception of its functions. For example, Arnold Toynbee charges science and technology with usurping the place of Christianity as the main source of our new symbols. Neo-orthodox theologians call science the "self-estrangement" of man because it carries him with idolatrous zeal along a dimension where no ultimate—that is, religious—concerns prevail. It is evident that these views fail to recognize the multitude of divergent influences that shape a culture, or a person. And on the other hand there is, of course, a group of scientists, though not a large one, which really does regard science as largely an iconoclastic activity. Ideologically they are, of course, descendants of Lucretius, who wrote on the first pages of *De rerum natura*, "The terror and darkness of mind must be dispelled not by the rays of the sun and glittering shafts of day, but by the aspect and the law of nature; whose first principle we shall begin by thus stating, nothing is ever gotten out of nothing by divine power." In our day this ancient trend has assumed political significance owing to the fact that in Soviet literature scientific teaching and atheistic propaganda are sometimes equated.

Ethical Perversion

The third image of science is that of a force which can invade, possess, pervert, and destroy man. The current stereotype of the soulless, evil scientist is the psychopathic investigator of science fiction or the nuclear destroyer—immoral if he develops the weapons he is asked to produce, traitorous if he refuses. According to this view, scientific morality is inherently negative. It causes the arts to languish, it blights culture, and when applied to human affairs, it leads to regimentation and to the impoverishment of life. Science is the serpent seducing us into eating the fruits of the tree ot knowledge—thereby dooming us.

The fear behind this attitude is genuine but not confined to science: it is directed against all thinkers and innovators. Society has always found it hard to deal with creativity, innovation, and new knowledge. And since science assures a particularly rapid, and therefore particularly disturbing, turnover of ideas, it remains a prime target of suspicion.

Factors peculiar to our time intensify this suspicion. The discoveries of "pure" science often lend themselves readily to widespread exploitation through technology. The products of technology—whether they are better vaccines or better weapons—have the characteristics of frequently being very effective, easily made in large quantities, easily distributed, and very appealing. Thus we are in an inescapable dilemma—irresistibly tempted to reach for the fruits of science, yet, deep inside, aware that our metabolism may not be able to cope with this ever-increasing appetite.

Probably the dilemma can no longer be resolved, and this increases the anxiety and confusion concerning science. A current symptom is the popular identification of science with the technology of superweapons. The bomb is taking the place of the microscope, Wernher von Braun, the place of Einstein. as symbols for modern science and scientists. The efforts to convince people that science itself can give man only knowledge about himself and his environment. and occasionally a choice of action, have been largely unavailing. The scientist *as scientist* can take little credit or responsibility either for facts he discovers—for he did not create them—or for the uses others make of his discoveries, for he generally is neither permitted nor specially fitted to make these decisions. They are controlled by considerations of ethics. economics, or politics and therefore are shaped by the values and historical circumstances of the whole society (*8*).

There are other evidences of the widespread notion that science itself cannot contribute positively to culture. Toynbee, for example, gives a list of "creative individuals," from Xenophon to Hindenburg and from Dante to Lenin, but does not include a single scientist. I cannot forego the remark that there is a significant equivalent on the level of casual conversation. For when the man in the street—or many an intellectual—hears that you are a physicist or mathematician, he will usually remark with a frank smile, "Oh, I never could understand that subject"; while intending this as a curious compliment, he betrays his intellectual dissociation from scientific fields. It is not fashionable to confess to a lack of acquaintance with the latest ephemera in literature or the arts, but one may even exhibit a touch of pride in professing ignorance of the structure of the universe or one's own body, of the behavior of matter or one's own mind.

The Sorcerer's Apprentice

The last two views held that man is inherently good and science evil. The next image is based on the opposite assumption—that man cannot be trusted with scientific and technical knowledge. He has survived only because he lacked sufficiently destructive weapons; now he can immolate his world. Science, indirectly responsible for this new power, is here considered ethically neutral. But man, like the sorcerer's apprentice, can neither understand this tool nor control it. Unavoidably he will bring upon himself catastrophe, partly through his natural sinfulness, and partly through his lust for power, of which the pursuit of knowledge is a manifestation. It was in this mood that Pliny deplored the development of projectiles of iron for purposes of war: "This last I regard as the most criminal artifice that has been devised by the human mind; for, as if to bring death upon man with still greater rapidity, we have given wings to iron and taught it to fly. Let us, therefore, acquit Nature of a charge that belongs to man himself."

When science is viewed in this plane —as a temptation for the mischievous savage—it becomes easy to suggest a moratorium on science, a period of abstinence during which humanity somehow will develop adequate spiritual or social resources for coping with the possibilities of inhuman uses of modern technical results. Here I need point out only the two main misunderstandings implied in this recurrent call for a moratorium.

First, science of course is not an occupation, such as working in a store or on an assembly line, that one may pursue or abandon at will. For a creative scientist, it is not a matter of free choice what he shall do. Indeed it is erroneous to think of him as advancing toward knowledge; it is, rather, knowledge which advances towards him, grasps him, and overwhelms him. Even the most superficial glance at the life and work of a Kepler, a Dalton, or a Pasteur would clarify this point. It would be well if in his education each person were shown by example that the driving power of creativity is as strong and as sacred for the scientist as for the artist.

The second point can be put equally briefly. In order to survive and to progress, mankind surely cannot ever know too much. Salvation can hardly be thought of as the reward for ignorance. Man has been given his mind in order that he may find out where he is, what he is, who he is, and how he may assume the responsibility for himself which is the only obligation incurred in gaining knowledge.

Indeed, it may well turn out that the technological advances in warfare have brought us to the point where society is at last compelled to curb the aggressions that in the past were condoned and even glorified. Organized warfare and genocide have been practiced throughout recorded history, but never until now have even the war lords openly expressed fear of war. In the search for the causes and prevention of aggression among nations, we shall, I am convinced, find scientific investigations to be a main source of understanding.

Ecological Disaster

A change in the average temperature of a pond or in the salinity of an ocean may shift the ecological balance and cause the death of a large number of plants and animals. The fifth prevalent image of science similarly holds that while neither science nor man may be inherently evil, the rise of science happened, as if by accident, to initiate an ecological change that now corrodes the only conceivable basis for a stable society. In the words of Jacques Maritain, the "deadly disease" science set off in society is "the denial of eternal truth and absolute values."

The main events leading to this state are usually presented as follows. The abandonment of geocentric astronomy implied the abandonment of the conception of the earth as the center of creation and of man as its ultimate purpose. Then purposive creation gave way to blind evolution. Space, time, and certainty were shown to have no absolute meaning. All a priori axioms were discovered to be merely arbitrary conveniences. Modern psychology and anthropology led to cultural relativism. Truth itself has been dissolved into probabilistic and indeterministic statements. Drawing upon analogy with the sciences, liberal philosophers have become increasingly relativistic, denying either the necessity or the possibility of postulating immutable verities, and so have undermined the old foundations of moral and social authority on which a stable society must be built.

It should be noted in passing that many applications of recent scientific concepts outside science merely reveal ignorance about science. For example, relativism in nonscientific fields is generally based on farfetched analogies. Relativity theory, of course, does not find that truth depends on the point of view of the observer but, on the contrary, reformulates the laws of physics so that they hold good for every observer, no matter how he moves or where he stands. Its central meaning is that the most valued truths in science are wholly independent of the point of view. Ignorance of science is also the only excuse for adopting rapid changes within science as models for antitraditional attitudes outside science. In reality, no field of thought is more conservative than science. Each change necessarily encompasses previous knowledge. Science grows like a tree, ring by ring. Einstein did not prove the work of Newton wrong; he provided a larger setting within which some contradictions and asymmetries in the earlier physics disappeared.

But the image of science as an ecological disaster can be subjected to a more severe critique (9). Regardless of science's part in the corrosion of absolute values, have those values really given us always a safe anchor? A priori absolutes abound all over the globe in completely contradictory varieties. Most of the horrors of history have been carried out under the banner of some absolutistic philosophy, from the Aztec mass sacrifices to the auto-da-fé of the Spanish Inquisition, from the massacre of the Huguenots to the Nazi gas chambers. It is far from clear that any society of the past did provide a meaningful and dignified life for more than a small fraction of its members. If, therefore, some of the new philosophies, inspired rightly or wrongly by science, point out that absolutes have a habit of changing in time and of contradicting one another, if they invite a re-examination of the bases of social authority and reject them when those bases prove false (as did the Colonists in this country), then one must not blame a relativistic philosophy for bringing out these faults. They were there all the time.

In the search for a new and sounder basis on which to build a stable world, science will be indispensable. We can hope to match the resources and structure of society to the needs and potentialities of people only if we know more about man. Already science has much to say that is valuable and important about human relationships and problems. From psychiatry to dietetics, from immunology to meteorology, from city planning to agricultural research, by far the largest part of our total scientific and technical effort today is concerned, indirectly or directly, with man—his needs, relationships, health, and comforts. Insofar as absolutes are to help guide mankind safely on the long and dangerous journey ahead, they surely should be at least strong enough to stand scrutiny against the background of developing factual knowledge.

Scientism

While the last four images implied a revulsion from science, scientism may be described as an addiction to science. Among the signs of scientism are the habit of dividing all thought into two categories, up-to-date scientific knowledge and nonsense; the view that the mathematical sciences and the large nuclear laboratory offer the only permissible models for successfully employing the mind or organizing effort; and the identification of science with technology, to which reference was made above.

One main source for this attitude is evidently the persuasive success of recent technical work. Another resides in the fact that we are passing through a period of revolutionary change in the nature of scientific activity—a change triggered by the perfecting and disseminating of the methods of basic research by teams of specialists with widely different training and interests. Twenty years ago the typical scientist worked alone or with a few students and colleagues. Today he usually belongs to a sizable group working under a contract with a substantial annual budget. In the research institute of one university more than 1500 scientists and technicians are grouped around a set of multimillion-dollar machines; the funds come from government agencies whose ultimate aim is national defense.

Everywhere the overlapping interests of basic research, industry, and the military establishment have been merged in a way that satisfies all three. Science has thereby become a large-scale operation with a potential for immediate and world-wide effects. The results are a splendid increase in knowledge, and also side effects that are analogous to those of sudden and rapid urbanization—a strain on communication facilities, the rise of an administrative bureaucracy, the depersonalization of some human relationships.

To a large degree, all this is unavoidable. The new scientific revolution will justify itself by the flow of new knowledge and of material benefits that will no doubt follow. The danger—and this is the point where scientism enters—is that the fascination with the *mechanism* of this successful enterprise may change the scientist himself and society around him. For example, the unorthodox, often withdrawn individual, on whom most great scientific advances have depended in the past, does not fit well into the new system. And society will be increasingly faced with the seductive urging of scientism to adopt generally what is regarded—often erroneously—as the pattern of organization of the new science. The crash program, the breakthrough pursuit, the megaton effect are becoming ruling ideas in complex fields such as education, where they may not be applicable.

Magic

Few nonscientists would suspect a hoax if it were suddenly announced that a stable chemical element lighter than hydrogen had been synthesized, or that a manned observation platform had been established at the surface of the sun. To most people it appears that science knows no inherent limitations. Thus, the seventh image depicts science as magic, and the scientist as wizard, *deus ex machina*, or oracle. The attitude toward the scientist on this plane ranges from terror to sentimental subservience, depending on what motives one ascribes to him.

Impotence of the Modern Intellectual

The prevalence of these false images is a main source of the alienation between the scientific and nonscientific elements in our culture, and therefore the failure of image is important business for all of us. Now to pin much of the blame on the insufficient instruction in science which the general student receives at all levels is quite justifiable. I have implied the need, and most people nowadays seem to come to this conclusion anyway. But this is not enough. We must consider the full implications of the discovery that not only the man in the street but almost all of our intellectual leaders today know at most very little about science. And here we come to the central point underlying the analysis made above: the chilling realization that our intellectuals, for the first time in history, are losing their hold of understanding upon the world.

The wrong images would be impossible were they not anchored in two kinds of ignorance. One kind is ignorance on the basic level, that of *facts*—what biology says about life, what chemistry and physics say about matter, what astronomy says about the development and structure of our galaxy, and so forth. The nonscientist realizes that the old common-sense foundations of thought about the world of nature have become obsolete during the last two generations. The ground is trembling under his feet: the simple interpretations of solidity, permanence, and reality have been washed away, and he is plunged into the nightmarish ocean of four-dimensional continua, probability amplitudes, indeterminacies, and so forth. He knows only two things about the basic conceptions of modern science: that he does not understand them, and that he is now so far separated from them that he will never find out what they mean.

On the second level of ignorance, the contemporary intellectual knows just as little of the way in which the main facts from the different sciences fit together in a picture of the world taken as a whole. He has had to leave behind him, one by one, those great syntheses which used to represent our intellectual and moral home—the world view of the book of Genesis, of Homer, of Dante, of Milton, of Goethe. In the mid-20th century he finds himself abandoned in a universe which is to him an unsolvable puzzle on either the factual or the philosophic le[illegible]. [illegible] the bad effects of the separation of culture and scientific knowledge, this feeling of bewilderment and [illegible] homelessness is the most terrifying. Here is the reason, it seems to me, for the ineffectiveness and self-denigration of our contemporary intelle[illegible]'s. Nor are the scientists themselves protected from this fate, for it has always been, and must al[illegible]ys be, the job of the humanist to construct and disseminate the meaningful total picture of the world.

To illustrate this point concretely we may examine a widely and properly respected work by a scholar who warmly understands both the science and the philosophy of the 16th and 17th centuries. The reader is carried along by his authority and enthusiasm. And then, suddenly, one encounters a passage unlike any other in the book, an anguished cry from the heart (*10*): "It was of the greatest consequence for succeeding thought that now the great Newton's authority was squarely behind that view of the cosmos which saw in man a puny, irrelevant spectator (so far as a being, wholly imprisoned in a dark room, can be called such) of the vast mathematical system whose regular motions according to mechanical principles constituted the world of nature. The gloriously romantic universe of Dante and Milton, that set no bounds to the imagination of man as it played over space and time, had now been swept away. Space was identified with the realm of geometry, time with the continuity of number. The world that people had thought themselves living in—a world rich with colour and sound, redolent with fragrance, filled with gladness, love and beauty, speaking everywhere of purposive harmony and creative ideals—was crowded now into minute corners in the brains of scattered organic beings. The really important world outside was a world hard, cold, colorless, silent, and dead; a world of quantity, a world of mathematically computable motions in mechanical regularity. The world of qualities as immediately perceived by man became just a curious and quite minor effect of that infinite machine beyond. In Newton, the Cartesian metaphysics, ambiguously interpreted and stripped of its distinctive claim for serious philosophical consideration, finally overthrew Aristotelianism and became the predominant world-view of modern times."

[illegible] cover-ing the [illegible] modern [illegible] engendered [illegible] law. The [illegible] view of modern [illegible] for lost [illegible] tical system —how far th[illegible] man that K[illegible] [illegible] century: "N[illegible] last measure the pow[illegible] mind on a true [illegible], and [illegible] God, who founded everything in [illegible] world on the norm of quantity, also has endowed man [illegible] a mind which can comprehend these norms!" Was not the universe of Dante and Milton so powerful and "gloriously romantic" precisely because it incorporated, and thereby rendered meaningful, the contemporary scientific cosmology alongside the current moral and esthetic conceptions? Leaving aside the question of whether Dante's and Milton's contemporaries, by and large, were really living in a rich and fragrant world of gladness, love, and beauty, it is fair to speculate that if our new cosmos is felt to be cold, inglorious, and unromantic, it is not the new cosmology which is at fault but the absence of new Dantes and Miltons.

And yet, Burtt correctly reflects the present dilemma. What his outburst tells us, in starkest and simplest form, is this: By having let the intellectual remain in terrified ignorance of modern science, we have forced him into a position of tragic impotence; he is blindfold in a maze which he cannot traverse.

Once this is understood, the consequence also becomes plain. I find it remarkable that the intellectual today does not have even more distorted images and hostile responses with regard to science, that he has so far not turned much more fiercely against the source of apparent threats to his personal position and sanity (*11*)—in short, that the dissociation has not resulted in an even more severe cultural psychosis.

But this, I am convinced, is likely to be the result, for there is at present no countercyclical mechanism at work. Some other emergencies of a similar or related nature have been recognized and are being dealt with: We need more good scientists, and they are now being produced in greater numbers; we need more support for studies in humanities and social science, and the base of support is growing gratifyingly. We sorely need to give our young scientists more broad humanistic studies —and if I have not dwelled on this it is because, in principle, this can be done with existing programs and facilities; for the existing tools of study in the humanities, unlike the tools in science, are still in touch with our ordinary sensibilities. But hardly anything being done or planned now is adequate to deal with the far more serious problem, the cultural psychosis engendered by the separation of science and culture.

One may of course speculate as to how one could make science again a part of every intelligent man's educational equipment—not because science is more important than other fields, but because it is an important part of the whole jigsaw puzzle of knowledge. A plausible program would include sound and thorough work at every level of education—imaginative new programs and curricula; strengthened standards of achievement; extension of college work in science to comprise perhaps one-third of the total number of courses taken by the nonscience student, as used to be the rule in good colleges some 50 years ago; greater recognition of excellence; expansion of opportunity for adult education, including the presentation of factual and cultural aspects of science through the mass media. But while some efforts are being made here and there, few people have faced the real magnitude of the problem, aware of the large range and amount of scientific knowledge that is needed before one can "know science" in any sense at all. Moreover, while some time lag between new discoveries and their wider dissemination has always existed, the increase in degree of abstraction, and in tempo, of present-day science, coming precisely at a time of inadequate educational effort even by old standards, has begun to change the lag into a discontinuity.

This lapse, it must be repeated, is not the fault of the ordinary citizen; necessarily, he can only take his cue from the intellectuals—the scholars, writers, and teachers who deal professionally in ideas. It is among the latter that the crucial need lies. Every great age has been shaped by intellectuals of the stamp of Hobbes, Locke, Berkeley, Leibnitz, Voltaire, Montesquieu, Rousseau, Kant, Jefferson, and Franklin—all of whom would have been horrified by the proposition that cultivated men and women could dispense with a good grasp of the scientific aspect of the contemporary world picture. This tradition is broken; very few intellectuals are now able to act as informed mediators. Meanwhile, as science moves every day faster and further from the bases of ordinary understanding, the gulf grows, and any remedial action becomes more difficult and more unlikely.

To restore science to reciprocal contact with the concerns of most men—to bring science into an orbit about us instead of letting it escape from our intellectual tradition—that is the great challenge that intellectuals face today.

References and Notes

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3. *Naval Research Advisory Committee Report on Basic Research in the Navy* (1 June 1959), vol. 1, p. 29; *ibid.* vol. 2, p. 34.
4. *The Public Impact of Science in the Mass Media* (Univ. of Michigan, Ann Arbor, 1958).
5. There is evidence that these figures have been increased by a factor of perhaps 1.5 since the survey was conducted. See H. Krieghbaum, *Science* **129**, 1095 (1959).
6. Perhaps the most eloquent and influential voice among those who have recently addressed themselves to this problem is that of C. P. Snow in the Rede Lecture, *The Two Cultures and the Scientific Revolution* (Cambridge Univ. Press, Cambridge and New York, 1959). I recommend his book, although with certain reservations. See also E. Ashby, *Technology and the Academics* (Macmillan, London, 1958) and F. Burkhardt, *Science and the Humanities* (Antioch Press, Yellow Springs, Ohio, 1959).
7. See, for example, the disturbing findings of M. Mead and R. Metraux, "Image of the scientist among high-school students," *Science* **126**, 384 (1957). I have presented the approach in this middle section in the "Adventures of the Mind" series, *Saturday Evening Post*, 9 January 1960.
8. It is, however, also appropriate to say here that there has been only a moderate success in persuading the average scientist of the proposition that the privilege of freely pursuing a field of knowledge having large-scale secondary effects imposes on him, in his capacity as citizen, a proportionately larger burden of civic responsibility.
9. See, for example, C. Frankel, *The Case for Modern Man* (Beacon, Boston, 1959).
10. E. A. Burtt, *The Metaphysical Foundations of Modern Science* (Doubleday, New York, ed. 2, 1932), pp. 238–239.
11. For a striking recent example see the virulent attack on modern science in the final chapter of Arthur Koestler's *The Sleepwalkers* (Macmillan, New York, 1959).

Science and Social Attitudes

Robert S. Morison

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Like all people with some scientific training, I suffer from feelings of unease when attempting to deal with the actions, and especially the attitudes, of people. For one thing, I do not have at my command the sampling and interview techniques wielded with so much aplomb by my colleagues in the social sciences. Fortunately for my own piece of mind, my scientific training was accompanied by enough exposure to the art of medicine so that I retain considerable respect for clinical intuition and judgment. This discussion relies much more on these elusive instruments than it does on quantitative scientific analysis.

As a matter of fact, it puts no great strain on one's clinical intuition to observe that large numbers of people in various parts of the world—including, perhaps most significantly, the advanced parts—are less happy about science and technology than they once were. The evidence is of various kinds. Perhaps the most quantitative is provided in the United States by the relative decline in students entering the sciences and the scientifically based professions. In some instances, such as engineering, the numbers have fallen absolutely in the face of a steady increase in the total number of potential students in each age class. Even more quantitative, and certainly more compelling to the individual scientist, is the evidence provided by the slowdown in appropriations for science. Third, one may cite the intuitions and reflections of thoughtful social clinicians like René

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Dubos (*1*), who has so courageously summarized the shortcomings of scientific approaches to human problems. True enough, he finally draws the conclusion that what we need is not less science but more. Nevertheless, the argument depends on a careful demonstration that science raises new problems of increasing complexity as it continues to solve the older and simpler ones.

Earlier Attitudes toward Science

Before going on to a discussion of the possible reasons for a decline in public regard, we should pause to remind ourselves that the change may not be so large or so profound as we might suppose. It is not very clear that there ever was a time when a substantial part of the population really understood science, cared much about the kind of knowledge it produces, or thought much about its ultimate effects. Improvements in technology were welcomed because of the increased production of what were generally regarded as good things at less cost in human effort. On the other hand, the reduction in human labor was soon recognized to have a negative side. In the first place, as the Luddites saw very early, it tended to throw men out of work, at least temporarily. What was even worse from the psychological point of view, the machine tended to change the status of skills which had been acquired with much effort over long periods of years. Nevertheless, on balance, the industrialization of production both on the farm and in the factory has been regarded by most people as a net good; for, it must be remembered, even at the height of the Medieval and Renaissance periods, skilled craftsmen constituted only a very small portion of society. The great bulk of mankind labored in the most unimaginative and unrewarding way as farmhands with a status little better than that of serfs. Somewhat later than the general recognition of technological improvements in production came an even greater appreciation for the contributions of science to medicine and public health.

Most men probably never did take much interest in what might be called the philosophical aspects of science. Few really read Condorcet or the other Encyclopedists, and it is doubtful that any but a small handful of intellectuals ever thought that science would provide a way of life free of undue aggressions, anxiety, loneliness, and guilt. Perhaps the Communist Party is the only large social organization that has ever seriously believed that man himself may be improved through improving his material circumstances. Among Christians, as among adherents of many other religious faiths, there has always been a substantial body of opinion which holds that the reverse is true and that material prosperity has, in fact, an adverse effect on the human soul.

The progress of science undoubtedly has had some effect in reducing the grosser forms of superstition. One supposes, for example, that most men are in some sense grateful for being less afraid of thunder and lightning than man used to be. But, here again, it is doubtful that the scientific way of looking at the world has ever completely displaced older, more magical approaches to the deep questions. It does not appear that President Nixon, when making up his mind whether or not to deploy the ABM system, consulted an astrologer, but it is not unknown for heads of states in other parts of the world to do so, and most of our metropolitan daily newspapers maintain an astrology column as well as the more sophisticated services of Ann Landers. Indeed, it is estimated that there are 10,000 professional astrologers but only 2000 astronomers in the United States (*2*).

Putting aside the grosser forms of superstition and turning to better-developed and better-thought-out ways of looking at the world, I would hazard a guess that the metaphysical outlook of most people, even in the United States, is more influenced by Plato and Aristotle than by Galileo and Hume. Indeed, it might be interesting for a graduate student in intellectual history to survey this very question. For example, do you suppose the majority of Americans would consider the following statements to be true or false? "Other things being equal, heavy bodies fall faster than light ones." "Metals feel cold to the touch because that is their nature." "Justice and honesty are real things and part of the divine plan of the universe; men try to establish justice through the machinery of the law and the courts, but their efforts will always fall short of the higher ideal of justice as it exists in the divine plan."

Coming down out of the clouds, we might ask ourselves how many people ever really got much fun out of studying mathematics and physics in high school? How many felt pleased to discover that a suction pump doesn't really suck water, but merely creates a potential space into which the water is pushed by atmospheric pressure? If one looks back 40 or 50 years, one seems to remember that rather less than the majority of one's classmates really enjoyed physics and chemistry and the kind of picture they give of the world. Perhaps a somewhat larger number found satisfaction in biology, with its greater emphasis on immediate experience and the pleasure one gets from contemplating nature's wide variety rather than its unifying mechanics.

World War II called a great deal of attention to science and made many people grateful for its role in enabling England and its allies to maintain the integrity of the free world. Along with the extraordinary buildup of military technology came a very great increase in biological knowledge of a kind which could be applied to medicine and public health, and to agriculture.

The press showed increasing interest in reporting scientific events, and the quality of scientific reporting has greatly improved in the quarter century since the war. Most significantly, a grateful and more understanding public provided vastly increased financial support for what the scientist wanted to do.

On the scientist's side there was a burgeoning of interest in making science more accessible to the general public. Most noteworthy in this movement, at least in the United States, was the effort of outstanding university scientists to improve the presentation of science to students in elementary and secondary schools. There is little doubt that this effort has greatly improved preparation for college in all branches of science. The generous men who initiated the program hoped for something more, for they felt that, if the story could only be presented properly, anyone of average intelligence would share the pleasure of the most able scientist in discovering the orderly arrangement of the natural world. Nothing could be more admirable than the dedication and self-sacrifice of men like Zacharias and the late Francis Friedman, and nothing more charming in its humility than their apparent belief that almost everyone is potentially just as bright as they themselves. Unhappily, it has not turned out as they hoped. Elegant though the Physical Science Study Committee Physics Course undoubtedly is, it has not proved much more successful than any other method in making physics attractive to secondary school students.

Nevertheless, on balance, public in-

terest in science became greater after the war than it had been before, and it was further stimulated by the orbiting of Sputnik. It is very difficult to say how much of this interest was due to competition for ever more sophisticated weapons, how much to a pure cultural rivalry which puts the moon race into the same category as an Olympic track meet, and how much to the age-old wish to cast off the shackles which bind us to a single planet. However one apportions the credit among these three factors, it seems reasonably clear that an appreciation for basic science, as the scientists understand science, played a relatively small role.

Reasons for the Change of Mood

The decade of the 1960's has certainly seen a slackening in public approval of science. Is this change simply a return to the earlier, more or less normal state of ignorance and indifference, or are we witnessing an actively hostile movement? In either case it may do us all good to try to identify some of the more important reasons for the change of mood.

1) Science is identified in the public mind largely with the manipulation of the material world. It is becoming clearer and clearer that the mere capacity to manipulate the world does not insure that it will be manipulated for the net benefit of mankind. Nowhere is this more obvious, perhaps, than in the matter of national defense. As pointed out above, the generation that knew at least one of the great world wars is grateful to the scientist for having fashioned the means of victory over a grave threat to a free world. The oncoming generation views the situation in quite a different way. To them the obvious alliance between the scientific community and the military is an evil thing. far from making the world more secure, it has produced an uneasy balance of terror, with the weight so great on both sides that any slight shift may lead to unimaginable catastrophe.

It seems undeniable that those of us who have grown up with this situation have also grown somewhat callous to the fact that such a high percentage of support for university science comes from military sources. We tend to remember, for example, the marvelously enlightened policy of the Office of Naval Research, which did so much to foster pure science while the Congress continued to debate the desirability of a National Science Foundation. Those who come upon the situation for the first time, however, see almost nothing but a conspiracy between some of the best brains of the country and the unenlightened military. In any case, it must be admitted that science and technology appear to contribute disproportionately to the more fiendish aspects of an evil business—the defoliation of rice fields, the burning of children with napalm, and the invention of new and more devastating plagues.

2) Until fairly recently, the contributions of science and technology to increased production both in industry and in agriculture have been generally regarded as on the plus side. Even here, however, doubts are beginning to arise. Much of the increased production comes at the cost of a rapid exhaustion of natural resources and the increasing contamination of what is left of our natural environment. Nor is it clear that all of the goods and services produced really do a great deal to increase the sum total of human happiness. Indeed, it can be shown that the modern affluent consumer is, in a sense, a victim of synthetic desires which are created rather than satisfied by increased production (*3*). On the other hand, a substantial percentage of the population remains without even the bare essentials of life. Rapid increases in agricultural production have pretty well abolished famine in the advanced countries of the world, but the revolution in rural life has benefited only a few of the most successful farmers. The rest are clearly worse off than they were before; and, indeed, the large majority of them are hastening into the cities, where they create problems which have so far proved insoluble. Furthermore, the advanced technologies which make the increased production possible are now found to be doing as much harm to the environment as the more long-standing and better recognized industrial pollutions.

3) Surely everyone can agree that science has done wonderful things for the improvement of health. But, even here, uncomfortable questions are being asked. Have our best doctors become so preoccupied with the wonders of their technology that they have become indifferent to the plight of large numbers of people who suffer from conditions just as fatal but much less interesting? Even the most earnest advocates of increased research in heart disease, cancer, and stroke must be a little bit embarrassed by the fact that the United States, which used to be a world leader in reducing infant mortality rates, has now fallen to 15th place.

4) It is not only the maldistribution of resources that concerns the general public; they are becoming increasingly uneasy about the moral and ethical implications of advances in biological science. In many respects these advances seem to threaten the individual's command over his own life.

Actually, of course, the individual never did have as much control over his own life as he felt he had. Science may have simply made his own impotence clear to him by showing how human behavior is molded by genetic and environmental influences. Like everything else, it seems, human behavior is determined quite precisely by a long train of preceding events, and the concept of free will has become more difficult to defend than ever.

Perhaps more immediately threatening is the fact that science puts power to control one's behavior in the hands of other people. Intelligence and personality tests place a label on one's capacity which is used from then on by those who make decisions affecting one's educational and employment opportunities. New methods of conditioning and teaching threaten to shape one's behavior in ways which *someone else* decides are good. Drugs of many kinds are available for changing one's mood or outlook on life, for reducing or increasing aggressive behavior, and so on. So far, these drugs are usually given with the cooperation of the individual himself, except in cases where severely deviant behavior is involved, but the potential for mass control is there. Indeed, there is already serious discussion about the ineffectiveness of family planning as a means of controlling the world's population, and suggestions are made for introduction, into food or water supplies, of drugs that will reduce fertility on a mass basis.

As if these assaults on individuality were not enough, some biologists are proposing to reproduce standard human beings, not by the usual complicated and uncertain methods involving genetic recombination, but by vegetative cloning from stocks of somatic cells. In the face of all this, can we blame the great majority of ordinary men for feeling that science is not greatly interested in human individuality and freedom?

5) Science is not as much fun as it used to be, even for its most devoted practitioners. The point here is that science encounters more and more difficul-

ty in providing a satisfyingly coherent and unified picture of the world. The flow of pure scientific data is now so prodigious that no one can keep up with more than a small fraction of it. Although most of us still retain some sort of faith that the universe, with all its infinite variety of detail, can in some way be reduced to a relatively simple set of differential equations, most of us recognize that this goal is, in practice, receding from us with something like the speed of light. That simple set of physical and chemical principles on which the older generation grew up is now turning out to be not very simple at all, and the relation between these simple principles and the complex events of biology are not nearly so clear as they were when Starling enunciated his "law of the heart."

Although it is probably too easy to exaggerate the degree to which the progress of science results in the fragmentation of knowledge, the beginning student in the sciences finds a great deal of difficulty in relating his courses in chemistry, physics, and biology to one another. Even within a single discipline, he feels overwhelmed and frustrated by the number of apparently isolated facts that he has to learn.

6) Closely related to the foregoing thoughts on the growing complexity of science and the decline in the intellectual satisfaction generally derived from it is the question of student attitudes, for most of us make our first serious acquaintance with science as students.

My overall impression, in returning to a university after a lapse of 20 years, is one of disappointment that so few students seem to have very much fun either in their science courses specifically or in university life in general. This lack of pleasure is certainly more striking in the first 2 years, when the student is adjusting to a totally new social environment and devoting his attention to building the groundwork for later, more exciting studies. But I keep asking myself why these first 2 years of foundation-laying have to be so unsatisfying.

In the first place, I have come to believe that we discourage many students by expecting too much of them. We want them all to learn at a rate determined by the best. This can only mean that all *but* the best feel themselves to be dying of a surfeit rather than enjoying a marvelous meal. I am also coming sadly to the conclusion that, no matter how the subject is presented, a substantial number of college-level students have relatively little interest in the facts of science and lack the capacity to find pleasure in its generalizations. Whether the failure is primarily intellectual, in the sense that students simply have difficulty in understanding the nature of the generalization, or whether it is emotional and esthetic, in that they derive little pleasure from the generalization once it is understood, is not easy to determine. In either case, the prospect of unifying the community around a common understanding of science seems relatively remote.

An article by Richard N. Goodwin in the *New Yorker*, entitled "Reflections—sources of the public unhappiness" (*4*) puts some of the difficulties of science into a larger perspective. It provides a brilliant analysis of the unhappiness not only of our obviously dissident left-wing youth but of the many members of the forgotten middle class who, during the last election, swung rather wildly between George Wallace and Eugene McCarthy. Goodwin discusses this phenomenon in terms of the traditional Jefferson-Hamilton model and comes to the conclusion that a great many Americans feel that they have lost control of certain crucial factors in their life styles. Although I am far from being as convinced as Goodwin is that it will be possible to return a large portion of our decision-making to states and local communities, I agree with much of his analysis of the underlying problem. He is particularly convincing, for example, when he shows how Secretary McNamara, in his apparent efforts to rationalize the Department of Defense and bring the military more closely under civilian control, actually succeeded in constructing a Frankenstein monster which began to control him, as "when he was compelled against his own judgment to go ahead with an anti-ballistic missile system."

For our purposes, the key word here is "rationalize." Our rationalized systems do, indeed, seem to have developed the capacity to live lives of their own, so that mere men are compelled, against their will, to follow where the logical process leads. As we saw above, the medical profession is following in the footsteps of its dynamic research program and undertakes to perform heart transplants, at great expense, largely because it has found out how to do them. In the same way, we devote several billions of dollars each year in going to the moon, because it is *there* (and, again, because we know how to do it). Everyone who has done much science on his own knows that the next step he takes is determined in large part by the steps that have gone before. It follows that the progress of pure science, at least, is determined by the internal dynamics of the process and by the opening up of new leads rather than by public demand to meet new needs. The practical applications to human welfare, when looked at in this philosophical framework, become accidental bits of fallout, as the nuclear bomb itself "fell out" from the innocent effort of J. J. Thompson, Rutherford, Bohr, Fermi, and others to understand the nature of matter. No doubt all these men felt completely in command of their own research programs, but the public does not look at it this way, and, in a curious sense, the public may be more right than the scientists. This line of thought brings us to point 7 in our bill of indictment.

7) The continuing momentum of science toward goals of its own choosing appears to be coupled ever less closely to solving problems of clear and pressing consequence to human welfare. As we now see, enlightened congressmen and senators, well aware of the power of the scientific method but skeptical of its capacity to guide itself automatically to the points of greatest human concern, are making explicit legislative attempts to mobilize science to solve the problems of the pollution of our environment and the crime in our cities, if not, indeed, the unsatisfactory nature of our life in general. Realizing that nuclear physics is not very closely coupled to these matters, they are turning to social science in the hope that there is a group of scientists who can do for society what the physicists have done for the natural world.

Skepticism about Rational Systems

Skepticism about rational systems is, of course, not confined to science. Indeed, it well may be that the antipathy to science is merely a bit of fallout from the growing antipathy to rational systems in general (*5*). The movement has been a long time in the making. Lionel Trilling (*6*), for example, traces much of the despair, the irrationality, and the increasing devotion to the absurd of much modern literature to Dostoevski's *Letters from the Underworld*, in which, you will remember, the protagonist, in his violent diatribes against the existing order, concentrates his hatred on those "gentlemen" who believe that 2 and 2 make 4. What is even more frightening for our own time is the way the same anti-hero reassures himself of his own individual freedom by affirming his ability to choose the more evil of two options (*7*).

We, who have grown up rejoicing in science, were confident in our acceptance of Sir Francis Bacon's aphorism that we cannot command nature except by obeying her (*8*). We really did not mind obeying as long as we knew that we would ultimately command. But now the empirical evidence may be turning to support those who feel that science is in some sense in the grip of natural forces which it does not command. Too often we conjure up genies who produce short-term benefits at the risk of much larger long-term losses. We develop marvelous individual transport systems which poison the air we breathe; learn how to make paper very cheaply at the cost of ruining our rivers; and fabricate weapons that determine our defense strategy and foreign policy rather than being determined by them. Above all, the applications of science have produced an unrestricted increase in the human population which we recognize as fatal to our welfare but have only the vaguest idea how to control. In a short time we will be able to design the genetic structure of a good man. There is some uncertainty about the exact date, but no doubt that it will come before we have defined what a good man is.

In the foregoing analysis, in an effort to obtain intellectual respectability I have painstakingly tried to break our problem down into a series of numbered subheadings. Actually, they all add up to the same thing: Although the general public is grateful to science for some of its more tangible benefits, it is increasingly skeptical and even frightened about its long-term results. The anxiety centers on the concept of science as the prototype—the most magnificent and most frightening example of the rational systems which men make to control their environment and which finally end by controlling *them*. It may be well to recall that the medieval structure of natural law was even more rational than science, in the sense that it depended on the mind alone without submitting its conclusions to empirical checks. It managed for a time to obtain even greater control than science has over both the bodies and (especially) the spirits of the people of the Western world. It, too, developed an interesting life of its own as it followed the paths of reason into ever more subtle areas. It failed, for a number of reasons, but primarily, perhaps, because neither the logic-chopping of the medieval philosophers nor the temporal power of the papacy which it was designed to support appeared to be sufficiently related to the longings of individual human beings. The Reformation, for all the complexity of its theology and, often, the brutality of its methods, was primarily an effort to assert the rights of the individual conscience over the medieval power structure.

A Watershed?

I am not really sure that we stand on the kind of watershed Luther stood on when he nailed his theses to the door of the cathedral, but we may make a serious mistake if we do not at least entertain that possibility. If we fail to recognize the average man's need to believe that he has some reasonable command over his own life, he is simply going to give up supporting those systematic elements in society which he sees as depriving him of this ability.

As I noted above, so perceptive a critic as Lionel Trilling traces much of modern literature and art to a long-standing revolt of sensitive and creative men against the systematic constraints of society. The New Left can be regarded as a politization of the same trend. Actually, of course, anarchy had a political as well as a purely intellectual existence when Dostoevski was writing, but the 19th-century political anarchists were effectively liquidated by the Marxists, who felt that they had a better idea. Now that Marxist communism has developed most of the ills of bourgeois industrial society plus its own especially repressive form of bureaucracy, anarchism is again put forward as an attractive alternative to organized, corrupt societies.

There is a difference, however, in the way 19- and 20th-century anarchists regard science. On the whole, the 19th-century ones were atheists and saw religion as the co-conspirator with government and business. Science tended to be favored, partly because of its contributions to man's material welfare, but perhaps even more because of its aid in debunking religion.

Two paragraphs from Mikhail Bakunin are worth quoting, partly because of the flavor of the rhetoric (*9*).

> [The churches] have never neglected to organize themselves into great corporations . . . the action of the good God . . . has ended at last always and everywhere in founding the prosperous materialism of the few over the fanatical and constantly famishing idealism of the masses.
>
> The liberty of man consists only in this: that he obeys natural laws because he has himself recognized them as such, and not because they have been externally imposed upon him by any extrinsic will whatever, human or divine, collective or individual.

The New Left certainly agrees with Bakunin about the need to destroy the existing order, but it tends to see God in a different light. In the United States, religion has been conscientiously separated from the State for so long that it is no longer regarded as part of the apparatus of repression. Indeed, many draft-card burners and other protestors against the immorality of the existing order are primarily religiously motivated. On the other hand, science as the interpreter of the laws of nature, which Bakunin set against the laws of the State, has lost its revolutionary character and is viewed as a dangerous collaborator of the industrial-military complex. One of the difficulties may be that science has become so complicated that the ordinary man no longer believes that "he himself has recognized them [natural laws] as such" but feels that "they have been externally imposed upon him."

Educating the Public

What, then, can we do to improve the image of science as something of human scale, understandable and controllable by ordinary men? In the first place, we will have to continue our efforts toward educating the public, both in school and outside it, through reporting in our newspapers and magazines. Although I have given some reasons for believing that there are limitations to the capacity of much of our population to understand and take pleasure in the way science understands the natural world, I still believe that much more can be done to improve matters than has been done so far. As for the formal part of education, I propose that we rather deliberately reduce the rate at which students must handle the material set before them, so that they can master it without feeling frustrated and overwhelmed. If we begin the process, as is now fashionable, in the early elementary years, continue it through college, and carefully design things so as to avoid redundancy, students might end up with a much more complete understanding than they do now. This effort is worth even more money and time than have been put into it so far.

As for less formal methods for presenting science to adults, we should devise some analogy that would do for the general public what agricultural extension courses have done for the farmer and his wife. The average successful farmer, although he is far from being a pure scientist, has an appreciation for the way science works. Certainly he understands it well enough to use it in his own business and to support agricultural colleges and the great state universities that grew out of them.

As one who has spent a considerable period of his life worrying about medicine and public health, I am much less happy about our efforts to instruct the average man in a rational or scientific attitude toward the conduct of his own life. It has proved ever so much easier to persuade the average farmer to plant hybrid corn than to persuade the average man to give up smoking cigarettes. We have been almost too successful in persuading farmers to put nitrogen on their fields, while we continue to fail in trying to persuade the average man to put minute amounts of fluorine in his water supply. Few individual doctors seize the opportunity to explain to their patients, in even quasi-scientific terms, what their illnesses are, and I am appalled by the bizarre notions of human physiology which are entertained by some of my best friends.

Granted that doctors do not have enough time to talk to their patients and that many doctors really are not very scientifically oriented themselves, we might think seriously of setting up in every city a kind of paramedical service designed to teach people about their own illnesses. A doctor with a patient who is developing coronary insufficiency, for example, could refer his patient not only for an electrocardiogram, a blood-cholesterol, and clotting-time determinations but for instruction, in a class of cardiacs, on just how the heart and circulation work. Such an enterprise might help individual patients adjust to their illness more suitably, but this is not the real point. The aim would be to take advantage of an unhappy accident in order to increase the individual's motivation to learn something about science. Therefore, such clinics should be paid for not only by the Public Health Service but by the Office of Education.

Second, we must make a major effort to bring the course of science, and especially its technological results, under better and more obvious control by individual human beings and their representatives. We are, it is true, slowly gearing ourselves to do something about pollution of the environment, but the overall guidance and control of this effort is largely in the hands of part-time experts who fly in and out of Washington to attend meetings which issue prophesies of doom or unsupported reassurances, as the composition of the particular panel may dictate. Somehow, thinking about the long-term results of technology, formulating the options in such a way that the public can understand them, and guiding the course of events along the chosen path must become as exciting and rewarding for the best minds as is the present pursuit of basic scientific knowledge. Above all, the options must be made clearly understandable to the people, and the people must feel that they are doing the choosing. The present method of announcing that such-and-such a corporation is about to erect a large atomic power plant on a certain body of water and then engaging in a debate, based on inadequate information, about the effects of the heat on the lake or river, the degree of radioactive contamination, and so on, is totally unsatisfactory.

The process of educating the public should begin much earlier, with discussion of the need for additional power plants and of the probable cost of putting them here or there, in terms of increased power rates on the one hand and increased contamination of the environment on the other. The public must slowly be brought to see that every such occasion involves a real choice between real alternatives, and that the alternatives must be balanced against one another. Similar considerations apply to the use of insecticides. Nobody, as far as I know, has seen fit to make any even approximate estimates of what our food might cost if we were to abandon the use of these agents. Similarly, nobody has told what it would cost to produce high-octane gasoline by means of some method other than the addition of tetraethyl lead.

We have been very negligent in devising ways and means of ensuring that the cost of introducing new technologies is borne by the people who immediately benefit from their use. If anything, the trend may be away from emphasis on this relationship. For example, the introduction of the cotton picker and of modern methods of weed and insect control, not to mention the enormous subsidies provided by the American taxpayer, have made the culture of cotton in a few counties in the South and Southwest extremely profitable, so that large landholders have become extremely wealthy. Presumably, the public at large has benefited by a slight reduction in the cost of cotton cloth. On the other hand, the social costs of this industrial revolution in agriculture have been incalculable; they have been borne primarily by the large number of Negro laborers who have been uprooted and transported into the cities, where they found themselves ill-prepared to benefit from the urban amenities enjoyed by their more prosperous fellow citizens. The economic costs of supporting them in an alien environment have been borne, not by the wealthy southern landowners and certainly not by the individuals who paid a bit less for the cotton cloth, but almost entirely by the displaced people themselves and by the people who pay real estate taxes in a handful of our larger cities.

All these problems are, however, subject to some kind of scientific analysis, and the options can be placed scientifically before the public. In preparation for this kind of decision making, we should probably overhaul our teaching of science, and especially of mathematics, so as to give the average man greater ability to evaluate evidence presented in modern scientific form. High school courses in statistics, probability, and systems analysis are clearly more relevant to modern living than Euclidean geometry, and might well replace this and other time-honored introductory courses in mathematics.

Role of Science in Military Affairs

Third, an effort should be made to clarify the role of science in military affairs. Although most of us who are acquainted with the facts know that much of the research supported by funds from the military services actually contributes as much to civilian life as to military matters, this fact is not known to the general public or to the student body. Cornell students, for example, are disturbed to learn that the largest single donor to research at their university is the Department of Defense, even though one of the university's two largest research enterprises is the observatory at

Arecibo, whose contributions to pure science are of far more consequence than anything it has ever contributed to the Air Force. If the military uses of science occurred as fallout from scientific investigations undertaken for peaceful purposes, this would be far better for morale than continuation of our present course, in which pure science appears as the crumbs that fall from the rich Pentagon's table. The obvious and actually very easy way to accomplish this would be to reduce military appropriations by what, to the military, would be a tiny amount and substantially increase appropriations for the National Science Foundation and the National Institutes of Health. Certain civilian agencies, such as the Department of Commerce and the Department of the Interior, should also be supporting far more basic and applied research than they are now.

Whether the civilian establishment for science should engage in any research of military consequence is a matter for debate, but such debate should be encouraged. Many universities of good will long ago decided that secret research has no place on a university campus, but this does not prevent them from doing unclassified work which has a clear military bearing, nor does the university ordinarily discourage its faculty from serving as consultants on classified projects carried out elsewhere.

There are obvious theoretical and practical difficulties confronting any other policy. Until now, for example, most scientists have felt that the importance of advancing knowledge overshadowed questions regarding the source of support. The control we now have of malaria is a net gain, regardless of the fact that from the discovery of the malarial parasite in North Africa to the development of control methods by the American Army during World War II, research on malaria was often carried on by military personnel.

Furthermore, it is clearly important that we have, as consultants to the military, civilian scientists who learn the details of proposed weapons systems so that they can make an appropriate case against, as well as for, deployment of these systems.

Finally, as long as we feel ourselves threatened by the scientific and military establishments of other nations, it is with some difficulty that most of us who have special skills, gained largely through contributions from the American public, can refuse to use those skills for the defense of that same public. This last issue is becoming a rather knotty one, however, since we may have reached a point at which war is so disastrous for both sides that there is simply no point in undertaking the exercise at all.

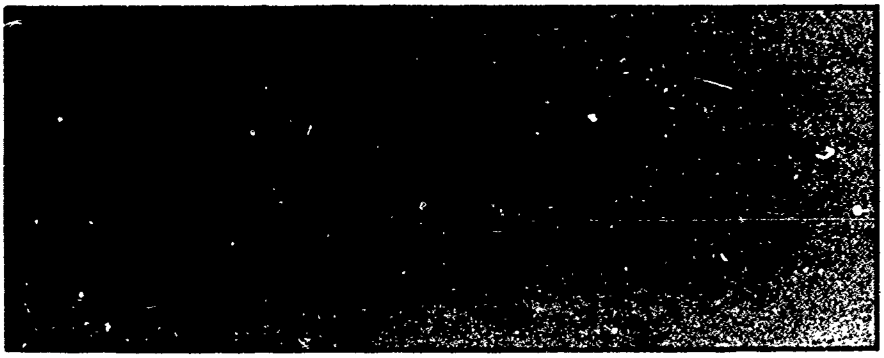
Conclusion

The most important lesson for the scientific community would appear to be one that can be stated as follows. Science can no longer be content to present itself as an activity independent of the rest of society, governed by its own rules and directed by the inner dynamics of its own processes. Too many of these processes have effects which, though beneficial in many respects, often strike the average man as a threat to his individual autonomy. Too often science seems to be thrusting society as a whole in directions which it does not fully understand and which it has certainly not chosen.

The scientific community must redouble its efforts to present science—in the classroom, in the public press, and through education-extension activities of various kinds—as a fully understandable process, "justifiable to man," and controllable by him. Scientists should also take more responsibility for foreseeing and explaining the long-term effects of new applications of scientific knowledge. A promising procedure for planning the control of such effects is presentation of the probable outcomes of various available options so that choices can be made by the public and their representatives. Costs and benefits must be estimated not only in quantitative, dollar terms but, increasingly, in terms of qualitative and esthetic judgments. Thus ends the comfortable isolation of science from the ordinary concerns of men as a "value-free" activity.

References and Notes

1. R. J. Dubos, *The Dreams of Reason* (Columbia Univ. Press, New York, 1961), p. 167.
2. C. E. Sagen, personal communications.
3. J. K. Galbraith, *The Affluent Society* (Houghton Mifflin, Boston, 1958), chap. 3.
4. R. N. Goodwin, *New Yorker* **1969**, 38 (4 Jan. 1969).
5. C. E. Schorske, "Professional ethos and public crisis: a historian's reflections," *Mod. Language Ass. Amer. Publ. 83*, 979 (1968).
6. L. Trilling, *Beyond Culture* (Viking, New York, 1965).
7. F. Dostoevsky, *Letters from the Underworld*, C. J. Hogarth, Trans. (Dutton, New York, 1913). All of part 1 is relevant to this discussion, especially page 37: "Moreover, even if man were the keyboard of a piano, and could be convinced that the laws of nature and of mathematics had made him so, he would still decline to change. On the contrary, he would once more, out of sheer ingratitude, attempt the perpetration of something which would enable him to insist upon himself; and if he could not effect this, he would then proceed to introduce chaos and disruption into everything, and to devise enormities of all kinds, for the sole purpose, as before, of asserting his personality. . . . But if you were to tell me that all this could be set down in tables—I mean the chaos, and the confusion, and the curses, and all the rest of it—so that the possibility of computing everything might remain, and reason continue to rule the roost—well, in that case, I believe, man would purposely become a lunatic, in order to become devoid of reason, and therefore able to insist upon himself."
8. F. Bacon, *Novum Organum* (1620), aphorism 129.
9. M. Bakunin (Bakounine), *God and the State* (1893).



It is an honor and a pleasure to be asked to give the Phi Beta Kappa lecture to the American Association for the Advancement of Science, and after nearly a century and a half of a remarkable tradition in both America and England, it is a good opportunity to take stock. Here in Denver, representatives of the scientific profession, the media, and the enlightened citizenry of the town meet in an atmosphere if not of complacency, at least one that shows a tendency toward mutual admiration. We are the heirs of a very worthy tradition, which in England goes back some 146 years to 1831, when the British Association for the Advancement of Science held its first meeting in York, and thus started these annual celebrations of worthy endeavor and high purpose.

Now if we look around, both at this present gathering and retrospectively at history, we are tempted to deduce that all is indeed well with the relationship of science and society, and that 146 years earlier all, indeed, was well: that from the beginning the public and the scientific profession together have enjoyed a persistently happy partnership. Both deductions are quite wrong. All is not well with the present relationship between science and society, and in the early years too, when the scientific profession was born in Europe, all was not well. The golden days for the mutual involvement of science and society came much later. At the beginning there were, in fact, great tensions as the profession emerged, and equally there are some now. And I want to argue that at least one problem which lies at the root of some of our present troubles was a specter at these feasts from the very start and has haunted the profession ever since. If I may change my metaphor; it has been like the Cheshire cat, taking on firm outlines at one period, fading at another, and then returning to sneer at us once more. This problem is the theme of my article and it is the problem of humanity in science.

Now this phrase can have several meanings. Marie Curie once said, "Science deals with things not people." The problem arises if, and when, scientists and technologists are tempted to deal with people as things. One of the problems in and around science arises through the inevitable stance of detached objectivity whereby a scientist must approach the natural world. It can be no problem—but as recent work in the biomedical sciences or genetic engineering demonstrates, it can be a serious problem. And at a time when the social contract between this profession and society is in the process of being renegotiated, as it is now, humanity in science considered in these terms becomes deeply significant.

The second interpretation of this phrase means a consideration of the human beings who do science: those remarkable people who come to us in an assemblage infinitely varied. It is difficult to reach out and touch the humanity, or the humaneness, in the people who do science, because science is essentially a communal activity whose results must be expressed in the passive voice, to be understood by anyone throughout geographical space and historical time. The expressions of science come in forms from which all the human content has necessarily been drained. So the questions, Who are the people who do science as individual human beings? What is the relationship between them and the scientific ideas they create? How and in what form are individuality and creativity brought to bear and expressed in science?—these are pressing questions which have not received the attention they deserve. I want to ask one other question as well: Why, with very few exceptions, have these themes or these people never stimulated great works of literature or art?

Lastly, the problem of humanity in science can mean the relationship between scientific and humanistic modes of thought and their impact on one another. This touches on the central core of creativity which lies at the heart of both humanistic and scientific work. I shall raise all these questions and answer some of them. I shall also suggest that our attempts to mirror the human life in and around science have been somewhat defective, to put it mildly. Our failure to do this has both deprived our artists of the possibility of portraying this great area of human activity, and contributed to the myths about the scientist and scientific activities which are highly dangerous, especially in the present time. I shall also argue that the schisms and the problems we now see on the contemporary scene arose in an historical setting and had significant historical ramifications. The failure to come to grips with the problem of humanity in science in the 19th century has had important consequences for the relationship between science and society in the 20th century. This problem originated deep in the social matrix of this profession, and its solution lies equally deep in our social matrix.

The British Association for the Advancement of Science

By now the origin of the British Association for the Advancement of Science is well known. Its founding was stimulated by a book by Babbage, *Reflections on the Decline of Science in England* (*1*), written in 1830, at a time when the word scientist was not even coined. After traveling extensively on the Continent, Babbage came to the conclusion that measured by almost any criteria you could mention—status, honorary distinction, or government post—science did not enjoy a status comparable to that of any other profession in England. Not only was it in a very inferior position, it was not a profession at all. As a result, the British Association for the Advancement of Science was formed. Their motives were "To give a stronger impulse and a more systematic direction to scientific enquiry and to obtain a greater degree of national interest in the objects and prosecution of science" (*2*). Their aims were not entirely altruistic, of course. The members of the Association did, indeed, want to remedy the situation that Babbage had portrayed, but they also felt that by showing society the practical justification for the existence of science, by demonstrating science's capacity to re-

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spond to social problems, they would strengthen the ties between the practitioners of science and the public. In other words, they had Rousseau's original social contract in mind, defined as a situation of mutual support, where each party relinquishes a measure of freedom for the wider social good.

They described themselves as the "cultivators of science." This was true, for that is exactly what they were. But they were also realistic and practical and, with their new pragmatic attitude, appealed not to the disinterested search for truth but to the benefits that could come by a close association between the profession and society. What happened? While all their initial efforts were not quite disastrous, it is absolutely true to say, as with Wellington after Waterloo, that "it was a damned close run thing" (*3*). For 10 years that profession and the British Association were subject to a degree of indifference or derision and parody, and at times were treated with such humiliation by the newspapers that I sometimes wonder that the profession got off the ground at all. The fact that after 20 years they were inclined to hole up and become more and more introverted surprises me not a little bit when one remembers what was written. For example, *The John Bull Examiner*, reporting on the Association's meeting of 1835, described it as "a whole lot of glaring humbug." The distinguished divine J. Keble, who founded Keble College at Oxford University, described the British Association as "a heap of quack philosophers." Even after 10 years, the London *Times* was being mightily rude in the glorious way that the London *Times* can be from time to time. A convention of Nonconformist ministers set themselves up as a clerical organization modeled on the British Association, and the paper reporting this described the latest British Association meeting in Devonport, 1841, as a "sort of philosophical race-week" and commented that the "new synod of tabernacle Savans" might "even surpass the freaks and fooleries of their model" (*4*)

Now why English society took this attitude is something that I have dealt with at length elsewhere (*5*). But it has to be admitted that, to a certain extent, the members of the British Association were sitting ducks for parody, for as membership increased so did the publicity both before and after the meetings. As the publicity got more flowery and full of puff, the hospitality became more lavish and the dinner menus became more lengthy. These were published along with the scientific papers. After the Newcastle meeting in 1838, *The London Literary Gazette* printed a report from *The Newcastle Journal* which listed the amount of game donated to the feast by the aristocratic lords of Newcastle, "to prove that gastronomy beats astronomy." The *Times*, reporting on the same meeting, spoke of the grand promenade in the ballroom of Newcastle's finest hotel, where some 4000 people were entertained, at which the amusements and refreshments were of the most recherché description. One longs for more details.

Yet, sadly, the British Association did have a record of offending British Victorian susceptibilities. They held one meeting at Castle Howard, the home of Lady Mary Howard, who was a kind of Victorian Carrie Nation. She was so offended by the junketings of the scientists and the hangers-on that she caused the family's wine cellar to be drowned in the lake, and supervised the massacre herself by knocking off the top of every bottle before it went under. Now, as Chaudhry shows in his interesting article on Charles Dickens (*6*), in spite of the Association's very genuine attempts to improve public understanding of science, what in fact came out was an indiscriminate mixture of science, technology, pomposity, and vanity. "Far from popularising science," he wrote, "the British Association had only succeeded in vulgarising it." It appears that its very reasonable aims had been given up in favor of activities which were far from scientific (*7*).

Charles Dickens

My purpose here is to concentrate on one gadfly, Charles Dickens. He became editor of *Bentley's Miscellany*, and in the autumn of 1837 had intended to publish *Oliver Twist* in the magazine. But he was diverted from this, for on 9 September the British Association held its annual meeting in Liverpool. In October of that year, Dickens wrote in the magazine "The full report of the first meeting of The Mudfrog Association for the Advancement of Everything" (*8*). He had already taken one laugh at the expense of the British Association in *Pickwick Papers*, and he now proceeded to take several more basing his parodies on the annual reports. To build up an exaggerated mock excitement of the tension and drama of science, Dickens employs the simple satire of the bustle of the newspapers, which are putting out communiques at hourly intervals, then half-hourly intervals, and then quarter-hourly intervals. The climax comes when Augustus, somebody's pet pug-dog, is stolen on the eve of the meeting and is dissected by two professors in their disinterested search for truth. The scientists who perpetrate this dastardly crime are thereby correspondingly assaulted by the owner of the aforesaid Augustus, an unmarried lady of otherwise impeccable virtue.

Dickens selected his items for parody from the agendas of all the sections, and few of them escaped his scalpel. For instance, he describes one section whose members were to take a cauliflower and redesign it as a parachute, to be guaranteed to come down from a height of no less than 1½ miles. In this he merges two episodes that had recently occurred: somebody had described a giant water lily at Kew, and also a parachutist had fallen to his death the week before. Having constructed a parachute on what he described as impeccable scientific principles—that is, in the shape of an inverted umbrella—the aviator had gone up to one of London's highest points and had come down to an untimely death. The *Times* reported this as "an unfortunate aeronautical catastrophe."

Among other things, Dickens also describes Mr. Tickle's spectacles, which are designed to enable the wearer to discern, in very bright colors, objects at a very great distance, but which render him wholly blind to those immediately before him. He parodies recent government reports; he talks about whether we could, in fact, utilize the industrial fleas in the zoos. He thinks we ought to license the fleas to do work for us, for they could labor under the direction and control of the state, and their widows and orphans could be put in insect almshouses. From the study of these insects at work we would derive valuable hints for "the improvement of our metropolitan universities, our national galleries and other public edifices." The core of Dicken's parody is not difficult to fathom. The first thing he hated was humbug, the second thing he hated was the denial of humanity, and all his parody has a striking social intent. Humbug is easily disposed of—all you have to do is prick it with a pin. But denial of humanity is not, and it is this humanity which was encapsulated in Marie Curie's aphorism which I mentioned before.

I said that the problem really begins when we are tempted to regard people as things, this especially was the root of Dickens' hearty dislike of statistics. For he saw a situation when science would cease to regard its objects of study as hu man beings and regard them solely as numbers in a statistical equation, and he would have none of it.

In passing, we may note that Dickens himself was greatly influenced by early Carlisle, who attacked the whole of the utilitarian movement and numerical quantification. Like Carlisle, too, he had a great distrust of institutions designed apparently only for talk. He did not like politics; he did not like Parliament, and he certainly did not like people who, like Mrs. Jellibee, organized charity for the Africans with a general whip-round in order to provide underwear for the people of Senegal. He was very suspicious of such attempts, for he believed that organized charity was merely an institutional device for channeling our sympathies, making them impersonal and unavailable in human terms. It is sad that Dickens, with his comic irony and his vivid sympathy, has no real solution to all this except that of a good heart. In *Oliver Twist*, along comes that nice man and gets Oliver out of the problem. But by the time we get to the later novels, *Our Mutual Friend* and *Great Expectations*, Dickens is sadly pessimistic. He implies that human relationships are so riddled with material ambition, selfishness, and snobbery that the possibilities of humanizing our society recede more and more.

Allegiance of the Scientist

Now as I have indicated, the detached, objective stance which has served, and will continue to serve, science so admirably as a methodology finds no cause for concern when we are investigating the passage of gases through the pores of a leaf. But it really begins to be a source of concern when we look at the implications of recent biomedical research or many of the implications of recombinant DNA research, as they relate to human genetic engineering. Whenever science begins to impinge on the autonomy of human beings such problems always arise and these force us to reexamine, in our new setting, a very old question—namely, that of the allegiance of the scientist. This was first raised in the middle of the 19th century by a number of people, including Lyon Playfair, who was to be president of the British Association in 1885. But the mid-century was a time when science and society were moving to delineate the forms of their social contract. The question is this: Where is the allegiance of a scientist properly due? Is it to an abstract ethic? Is it to methodology? Is it to themselves as a profession, or is it to society?

In the 19th century it came to be taken for granted—and almost by default—that the profession's allegiance was solely to the first (5). But in the last quarter of this century it is surely patently clear that it must become very much wider. So my first plea is for some practical recognition of the new ethical imperatives operating both on science and on society. At this stage let me give only one example. The United States has the most sophisticated and remarkable biomedical research establishment in the world. It also has a remarkable scientific profession, which is highly privileged. I agree with the distinguished immunologist, Barry Bloom; it is both morally right and scientifically possible to concentrate some of our intellectual and technological effort on the pressing medical problems of the Third World—leprosy, malaria, and schistosomiasis for a start.

There is something distasteful at the sight of a highly developed society being forced to divert great resources, both financial and intellectual, to the cure of its own self-inflicted diseases. We can characterize these as the diseases of choice—those which arise from excesses in its life-style, or the pollution of its environment. In 1975 the United States spent $22 billion on alcohol and $12 billion on tobacco, and in the preceding year it spent $400 million on cancer research. Yet the World Health Organization estimates that to do an effective interdisciplinary remedial job on the problem of the Third World diseases, those that arise not by choice but by causes external to the people themselves, would cost only $15 million per annum. I do not wish this point to be misunderstood. This is not a plea for less basic research. I continue to be greatly impressed with the importance of such work and how, for example, work supported for research in cancer can have an impact in other areas of medicine—how, for example, basic immunological studies bear immediately on autoimmune disease. This plea is both for a change in life-style and a research program directed to the problems I mention. To support such work would, I believe, be one of the most farsighted acts this new Administration could undertake. There is an untapped source of idealism, energy, and intellectual skills in the young scientists of this nation, and I would like to see the development of a scientific Peace Corps devoted to tackling some of these problems, to gaining much more knowledge and helping to implement the solutions in terms of the cultures, life-styles, and aspirations of these other countries. Nothing would do more political good to this nation and few single acts, I think, would bring more decency into the world.

Now I must emphasize that there is nothing God-given or immutable in the scientific profession's apolitical disinterested search for truth. Even in the 17th century, more than one-third of the papers of the Royal Society were about social problems and the relationship of science to them. In addition, in the early years of the 19th century the initial aims of the British Association were to bring the problems of society and the skills of science together. That the profession was driven into itself was not, I would argue, its fault. Moreover we have seen a similar emphasis in the late 20th century. When Sir John Kendrew was president of the British Association in 1972, he pleaded for a change in attitude and asked for the profession to look at some of the pressing social problems and direct their knowledge toward their solution. More recently, in the press conference that attended the publication of his book on O. T. Avery, Dubos (9) reminded us that Avery's discovery arose not from his interest in the gene, but from his interest in pneumococcal pneumonia. Dubos consistently argues that many scientific problems which have their origins in a deep social context may turn out to be more fruitful, in all kinds of ways.

So that was one critique: science is cold and inhuman and also does not concern itself with the needs of society. The second critique, which is my next theme, also has historical origins: somehow science manages to extract the warmth and beauty from the world, and this is also drained from the personalities of the prosecutors of science. Moreover, insofar as they are scientific solutions, our solutions to humanity's problems inevitably become humanely cold, too. Dickens took up this theme, so did Blake and Keats, and in our time so did F. R. Leavis.

Humanizing Society

So, it is argued, the job of improving and humanizing our society can find neither tools nor methods in science, for then the remedy becomes worse than the disease. We must take note of one counterclaim that was offered in the past and is often offered now. In the effort to humanize ourselves, to enhance our ethical and moral sensibilities, people have often appealed to the humanities to do it for us, almost as to an ideology. The redemptive power of the humanities to produce an enlarged consciousness, to make us aware of the reality of our human predicament, to enlarge our sympathies has been an important theme, whether in Wordsworth, in Shelley, or in many 20th-century writers. I am slightly skeptical about this. I am not at all convinced that somehow, from a study of the great thinkers of the past alone, we automatically get access to moral virtue. It is disturbing but nevertheless true that people can be extraordinarily sensitive to music and poetry and not necessarily apply this to their daily lives. Steiner (*10*) has reminded us how people returned from a day's work in the concentration camps and then put Mozart on their gramophones. I remember, too, a delightful occasion at a conference on science and the humanities when the philosopher Max Black reminded us of the exquisite capacity of philosophers to argue questions of ethics and morality in the most rigorous and convincing style, but he went on to say, "If I wanted to know whether an action I proposed to take was right or wrong, I wouldn't ask my professional colleagues, I'd ask my wife." Stoppard (*11*) equally reminds us how Lenin, when he felt himself being moved by the Appassionata Sonata of Beethoven, rigidly turned away, saying, "We've just got to hit people."

Did the Shakespearean plays, with their almost God-like insight into the way that people behave, make people understand more, make people act better, make people feel more humane? It was with considerable surprise that I learned from David Daiches that the same people who went to the Globe Theatre or to any Elizabethan or Jacobean play, and saw these marvelous dramas with their rich poetry and their human understanding, would at the same place in the same afternoon watch a monkey tied to the back of a horse, chased by dogs who slowly bit it to death. This was their favorite occupation between the acts. For there is a large gap between appreciating the wonders of artistic imagination and going out and doing likewise, as there is between knowing ethical norms and going out and doing likewise, which no amount of discussion of "is" and "ought" will alter. This is my main quarrel with F. R. Leavis—the myth of the redemptive power of great works of art, the belief that by teaching a small group of elite to appreciate Lawrence and George Eliot you will change civilization. You won't at all—not by this alone.

Why have I gone into this at such length? I have three reasons. First, I think it unfair and unwise to regard the humanities in this therapeutic light. They are good in themselves and should not be regarded as remedies for our own failings. Second, we must not delude ourselves about how easy it is. We must not pretend that words and university courses are a substitute for human hearts and human action. Third, we must be very careful of hypocrisy. For if we insist that the scientific profession and the medical profession have a care and a human concern which we ourselves as members of society are not prepared to have or to act on, we shall be raging hypocrites.

Absence of Science in Literature

Now let me go to my second task and raise the question. Why is there the myth that humanity and warmth are drained from the world by science, and how is it that scientists have not been the objects of great works of art or literature? There are, of course, famous exceptions—George Eliot's *Middlemarch*, Sinclair Lewis's *Arrowsmith*, and to a certain extent the novels of C. P. Snow, although he has mostly described scientists acting as politicians or administrators, as anything other than workers in a laboratory. If there is a shortfall here, we must see this in terms of the social streams in the early 19th century, around the time when the British Association was beginning. Here, I believe, we find another and perhaps more serious division between science and the arts. In the early years of the French Revolution, the romantics heralded the new age of freedom and reason—a time when poetry and science would lie down like the lion and the lamb, and inspire and celebrate together. Indeed, from the time of Copernicus to that of Newton, scientists could write and assume that what they wrote all intelligent men would read.

But Wordsworth, Coleridge, Shelley, and Tennyson notwithstanding, by the middle of the 19th century this assumption began to fade. It was partly the reality of the French Revolution followed by Bonapartism which smashed that vision, partly the effect of the Industrial Revolution and the shape of the new world to come, when scientists were equated with the engineering devils who had blackened the face of England. The new world that was now fashioned seemed totally alien to the world of poetry and art. So the romantic movement then revived again in a different form, to protest against the deadening effect of the rational style.

There was, on the one hand, the vision of the detached onlooker, who stood aside from the world. But this seemed to be at variance with the desire of the poet to enter with his feelings into the world and to respond and resonate with it. As Keats wrote, natural philosophy apparently undermines this vision; it certainly makes the world much less accessible. Then William Blake came, to speak of a science that darkens the imagination and murders the soul. So the rift began. Among the poets and the artists there was the cultivation of that inner realm of feeling which poetry, but not science, would reflect. Moreover, it was a place where, as Charles Davy reminds us, the truths of poetry did not have to meet the challenges of the truths asserted by science. This difference was not an issue about the nature of the world and who made it, for that topic was left to the classical conflict between the theologians and the scientists, it was the question of the place "where three dreams cross"—but it was a place which a scientist never inhabited and probably could not understand.

Now one consequence of this rift came to be reflected in education, a second, in the ignoring of science and its ideas as themes for artists. Such issues as the relationship between a sensitive artist and bourgeois society, which has been a tremendous theme of literature, never found their counterparts in literature about science. This was partly because people could not decide on what side of the issue scientists lay, but equally because, hating the Industrial Revolution and the philistinism of the whole bourgeois mentality, and equating the scientists with the technologists, people somehow pushed them onto the side of the enemy. Thus scientists became, and have remained, suspect in the eyes of artists.

So we have had a long tradition of writing and investigating how artists and writers work (Joyce Cary's marvelous book *The Horse's Mouth* comes immediately to mind), but where is its scientific equivalent? The psychology of artistic creation has been with us for 200 years, and even though Poincaré raised similar questions about scientists, no one has ever gone really deeply into the psychology of scientific discovery. There is so much that remains unexplored—the human themes around science are vivid and fascinating. But writers do not know how scientists work. They see science as power, or as politics, but there are precious few accounts of the way the scientific imagination is expressed. It may be that novelists are afraid of revealing their ignorance, but I think the reason goes much deeper. I think it is because, somehow, we have not made science accessible. Yet, I contend, if you want as your sympathetic hero a man of imagination, of intellectual interests, of deep moral dilemmas, a scientist will fit your picture—especially, perhaps, a recombinant DNA scientist—not only as well as but, in many ways, perhaps even better than an artist.

Yet one can say that, after all, we have had a long tradition of looking at science. We have indeed had the professional scrutineers of the enterprise, the philosophers and the historians of science. But if, as Neff (12) argued, the measure of our success here is the measure of the means by which we have chosen to woo humanity to recognize its own likeness and understand itself, how have we, in professional philosophy and history of science, measured up? Have we succeeded in getting science to recognize its own likeness? No. Judged by scientists and others, much philosophy of science has been just irrelevant—at best a series of brilliant axiomatic games, but often pretentious nonsense, like the pretentious nonsense you can easily find in all academic disciplines. But worse, I think, is what it has omitted to do for us. H. Reichenbach said, philosophers of science are "not interested in the context of discovery so much as in the context of justification" (13). But this is only a very small part of the science enterprise.

History of Science

What of history of science? It is a profession I have practiced for a great part of my academic life, and I look around and ask, Where in history of science is our Macaulay, our Namier? Come to that, where is Tolstoy? Who has dealt adequately with the relationship of the individual in science to the march of scientific progress? History as we know it is a tapestry, the parts of which are made up of the mosaic of the small, everyday, individual events. And we derive the pieces of the mosaic from a whole variety of sources over and above formal academic articles: from newspapers, journals, diaries, cabinet documents, and so on. The real historian is the one who can piece the mosaic imaginatively to form the tapestry and so present the past to us in its full, vivid color. Not apart from the waspish memoirs of Watson (14), we have seen little of this kind attempted in the history of science, and in any case we would be much better advised to turn not to Watson but to Sayre (15), who genuinely attempted to match a person and a personality with the progress of thought. There are a few notable exceptions: Holton (on Einstein) (16) and Rosenberg (17), both of whom are convinced that the scientists' subjective state of mind has a marked influence on the progress of science; Koestler (18) whose vivid account of Kepler in *The Sleepwalkers* is another, superb, example; and Frank Manuel on Newton.

To be fair to the members of my profession, whom I by no means disavow, science makes it very difficult for us to comprehend its history. There are two reasons for this. First, the pieces of the mosaic are often just not there. Second, as Sir Peter Medawar reminded us (19), a scientific paper not only conceals but in fact actually misrepresents the reasoning and the imagination and the creation that has gone into it, for the stern eyes of John Stuart Mill are staring out at the editor of every journal. As Medawar also emphasized, the past of science does not have a dignified independent existence of its own, for a scientist's present work is of necessity shaped by what others have done and thought before him. So science is a wave front of a continuous secular process which carries its own history with it. But even admitting the difficulties that are placed in our way as we try to relate the individual work to the march of scientific history, I still believe that we could, and should, be very much more imaginative and comprehensive in mirroring this activity.

Now why am I so confident? It is because during the last 2½ years, I have been following Sir Peter Medawar's recommendations, and Gerald Holton's too, and have been listening in at the keyhole of daily science. I have, in fact, been living with one group of scientists, day after day, as they do science, not as they afterwards say they do science. I have been seeing the smudges, the thumbprints and bloodstains, of a personal struggle with one's ideas. Now after 20 years in orthodox history of science, I am appalled that I could have so ignored the very human core of its history. Where are the people that, as an historian of ideas, I wrote about? Did I paint them so that I could recognize them and their unique personalities, and how they bore on their science? No, I did not. So my last plea is that it is time to apply to the history of science the lessons of Vico and Herder which have been so beautifully expounded by Sir Isaiah Berlin (20). We must become more sophisticated and come to grips with this problem, and reformulate our discipline.

History is not a totally unknown country. It is a study of the human past as a form of collective self-understanding of human beings and their world. It always has been that and it should be always like that. It is a story of human activities, what men did, what they thought, what they suffered, what they strove for, what they aimed at, what they accepted, what they rejected or conceived, or imagined. It tells us about their motives, their purposes, their ambitions, their ways of acting and their ways of creating. These, Vico insisted, are the activities we know, and we know because we are all involved in them as actors, not as spectators. Historians and philosophers of science have been too much spectators and have not been sufficiently involved with science. The kind of knowledge we seek is not just the knowledge of facts or the knowledge of logical truths or the logic of method. The kind of knowledge we seek is more like the knowledge of a friend, his character, his ways of thought or action, an intuitive sense of the nuances of his creative personality. We must use imaginative power of a high degree to enter into the other's mind and world, and this means appreciating them as people as well as scientists.

Without entering into history in this sense, the past remains dead, a collection of objects. Similarly, without entering into science in this sense, scientific history will remain a dead collection of objects or ideas which apparently has been created by stuffed figures in a museum. The only way of achieving any self-understanding is systematically to retrace our steps, historically, psychologically, and above all anthropologically into science. We can begin now to study science as it is done and try to understand those private moments of creativity—to enter with the scientist into "the place where the three dreams cross." We have to enter with empathy into other people's minds and into their modes of being. Then and only then can we go back and redo the history of science.

I have argued that at this time the scientific life and the scientific imagination are not really accessible, for all that more is probably being written about science than at any time in its history. If it were accessible, we could demythologize it. When we have done this, we could incorporate it into public understanding. When it is incorporated into public understanding, then, and only then, will science be truly integrated into our culture.

Science and Society

Finally I want to say something not very original, but I do not think that matters. Science and society can no longer afford to entertain myths and misunderstandings about each other, as the recent public debates about recombinant DNA reveal. Public understanding of science is as vital as it was in the early days. But there is the other side to this coin, and that is the scientist's understanding of the public. Science and society must be closer to one another. When the one is truly incorporated into the other, we will appreciate the humanity that has in fact always been present in science, and in essential respects will always be found there. But if I argue that scientists should now consider new ways of expressing this humanity in response to the new ethical imperatives in our society I also argue that society should think of new ways both of helping them to do this and of understanding them.

The profession's allegiance can no longer be to a methodological ethic alone. But this does not mean giving up the truth, and we will still look to scientists for significant contributions to objective truth as well as to the practical expressions of science. I am not arguing for a return to stages of irrationality or wishful thinking, but for the application of knowledge of facts in new compassionate ways I think we are reasonably entitled to ask the scientific profession to assess the problems of contemporary society, and where scientific solutions are called for, to give them first priority. It would be magnificent if, instead of being on the defensive vis-à-vis society as we have seen in recent years, scientists actively extended their notion of accountability in this way. With their example before us, we might then go on and tackle the problem of accountability in other groups—in industry and in the media, for example—and thus help create a climate where all such professional groups recognize their debt and responsibility to society at large. Now is very much the right time—a delightful time in our lives—when it is splendid, is it not, to be able to use old-fashioned words such as "morality" and "honor" without a fear of being sneered at.

I also wonder what Charles Dickens would say if he came back now. He lived his life in a deep pessimism. But looking back I think he would have possibly more grounds for optimism.

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A Very Human Business

William D. Carey

Editorial, 17 March 1978

In the kinetic behavior of AAAS over the past decade, not excluding the present, social controversy has troubled the waters and provoked questions about what is science's proper business and what is n While the prevailing wisdom is that the fires of American discontent are burnt out, the evidence is that it does not take much to rekindle them. Science may yearn to be seen as impartial, a sanctuary of reconciliation whose members hear no evil, see none, and above all speak none. If scientists were saints, it might be so.

At the recent AAAS Annual Meeting the principal eruption was one of information and communication, with nearly 140 symposia and almost 1000 speakers shoehorned into five days and nights. But there were other eruptions too, more impassioned and divisive, whose echoes still resound. The questions will not be quieted. What pushes scientists, whatever their fields and credentials, to plunge into social and political controversies which are not central to the search for and application of knowledge? What is politics doing in the house of science?

There is no way, in a dynamic society, that a strict line can be drawn between science and the tides of social change. They overlap, and if it is argued that science is value-free the same cannot be said of scientists. If scientific freedom is half the operative equation, responsibility is the other half. Where trouble comes is in divining the issues on which scientists should take a stand, and in attesting the legitimacy of their interventions. There are no glib rules, no magisterial fiats, to go by Conscience and conviction are among the valuables rescued from the debris of the recent American experience, and they will have to do.

The scientific community has seldom complained when politics has used science for its various purposes, and it is curious that so much anguish is expressed when it is the scientists' turn to make use of politics The glorious Apollo program, for all its benefits, was more the child of politics than of the craving for exploration The war on cancer has similar origins. The sweepstake staged for siting the solar energy laboratories was more of the same. Like it or not, political motives have been largely responsible for driving American science and technology for three decades, and under the banner of energy they are likely to do so for the next three. It is outright myth to imagine that science and politics exist in separate spheres.

It is not a matter of indifference that in police states the human rights of fellow scientists are violated. Nor is it beneath scientists to borrow textbook political methods in the defense of basic research against statutory regulation. When state governments dictate what is to be taught in the schools about the origins of life, we have little hesitation in taking up the fight. If these responses are legitimate, it is not clear why scientists should not also go to the mat for the physically handicapped in science, for the rights of other minority groups, or on behalf of an equal chance for women in science. The advancement of science will not be measured entirely by the growth of research budgets or the per capita share of Nobel Prizes.

If there is a hitch in the argument, it concerns the public's grasp of what the scientists are up to. Will a public which listens when scientists speak on saccharin or laetrile care to hear their views on social morality? The reaction is likely to be that the scientists have left their domain. Where the public will finally lose patience is at the appearance of arrogance, no matter what may be the moral strength of the scientists' position. It is here that a line surely can be drawn.

Science and social controversy will touch each other more often than not, and usually with good reason. AAAS will get its share. But when the sparks fly again, as they are bound to, there is a lot to be said for the view of our colleagues in London, that science is "a very human business"*(1)*.

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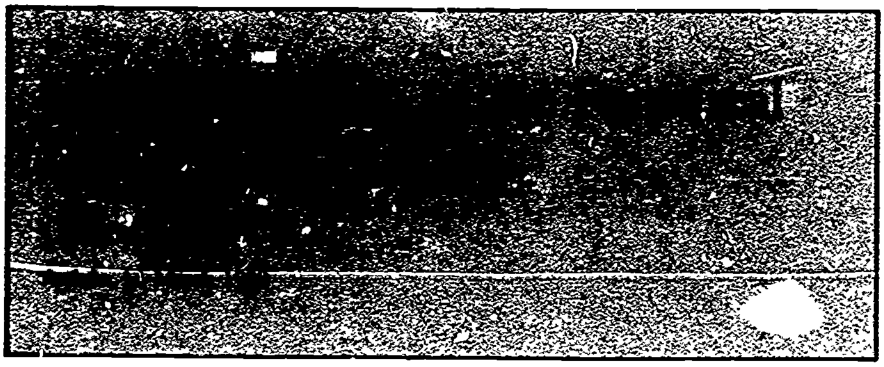

On 10 November 1979, a meeting of the Pontifical Academy of Sciences was held in Rome in honor of the centenary of the birth of Albert Einstein. The Einstein Session was historical in three ways. It was the first time the Pope presided over a session of the Pontifical Academy. It was the first such session to be attended by the cardinals, many of whom were in Rome at the time. Finally, the content of the Pope's talk was remarkable, particularly his emphasis on the autonomy of scientific truth and its independence of religious truth.

In the course of this century science, working in two directions, has completely changed human life. One of these directions is basic research; the other is technological applications. It matters little that basic research is often regarded with mistrust in many circles, or that technological applications have not always been pursued with prudence and wisdom. Man's irrationality cannot take away from science and technology their role as powerful levers to serve our society, guided by spiritual, moral, and ethical values, to help overcome the obstacles to man's progress produced by a materialistic and opportunistic civilization, where existence destroys essence and possessions take the place of love.

Albert Einstein's work is in the realm of pure research. It contemplates the laws of nature at the level where the supreme harmony of the Divine Creation reigns. For the importance of his works, Einstein is compared to the greatest minds in the field of universal thought. I might mention among his predecessors only Galileo Galilei, who applied the keen edge of his genius to the development of science and, like Einstein, became the symbol of an era.

In studying the existing information regarding Einstein, we find, together with the great scientist, an exceptional human being, whose chief concern was with justice. And yet no one is more outstanding for his constant modesty and unceasing faithfulness to his moral principles. If he is sometimes accused of occasional self-contradiction, this is only superficial and rather the result of a firm attachment to his convictions. One thing certain is that he radiated an extraordinary intellectual force and a charisma that age only increased.

If Einstein the scientist was completely absorbed in seeking a unifying theory of the forces at work in the universe, Einstein the citizen served the cause of justice with the same zeal and courage. From 1914 until his death he fought against militarism, the abuse of power, and racial discrimination, and he staunchly defended peace.

Einstein's Early Years

Born in Ulm on 14 March 1879, Einstein lived in Munich until he was fifteen. In Munich he began his study of mathematics, the geometry of Euclid became his bedside breviary. He also immersed himself in Kant. During this period his mother made him study music and the violin—a genuine Ingres violin—which would serve as his refuge in moments of relaxation, distress, or trial. The family environment helped to form his values; at home he learned modesty and simplicity, so well exemplified in his manner of dress and his indifference to material wealth. "Men's lowly objectives—possessions, apparent success, luxury—have always seemed to me despicable."

After his elementary studies, Einstein matriculated at the Luitpold Gymnasium. His stay at the Gymnasium was for him decisive. He could not tolerate the Prussian discipline there and what he called the "methods of fear, force, and artificial authority." At the Gymnasium were born his lifelong rebellion against authoritarianism and classical school dis-

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cipline and the roots of his anti-militarism. Also, he began to feel the need to reconsider the importance of evidence that had been considered irrefutable.

Einstein did not complete his course in Munich. Interrupting his studies there, he joined his parents in Milan for a year before going to Switzerland, where he completed his secondary schooling in Aarau. He became enthusiastic over the democratic climate which had been established for centuries in the Swiss Confederation, as well as the absence of a professional army. Making a decision that was surprising in a young man of sixteen, he gave up his German citizenship to become a Swiss citizen. He entered the Ecole Polytechnique Fédérale, where in 1900 he received his diploma. The years he lived in Zurich were of great importance because they laid the foundations for the scientist.

His faithful companions—Michelangelo Besso, Konrad Habicht, and Maurice Solovine among others—tell of the astonishment and admiration of everyone in the presence of his great intelligence and his mastery of physics. Those were the years when he studied the works of Maxwell, and learned from a lecture by Henri Poincaré that it may not be possible to maintain the concepts of absolute space and absolute time and even the geometry of Euclid to be valid in mechanics. Besso brought him Mach's book *The Science of Mechanics*, which even more deeply described the difficulties of the interpretation of the two concepts of Newton. He thus began to prepare himself for relativity.

The pupil was not popular with his teachers. A very serious student, he was nonetheless irregular in class attendance. Also he was critical sometimes arrogant, always impulsive in his comments, often hurting feelings. Consequently, he was unable to realize his dream of joining the teaching staff of the Ecole Polytechnique, or even that of other faculties. A stroke of good fortune led to his entry into the Patent Office in Bern, where he found calm and leisure and could so develop his unusual scientific theories. In 1905 he published four basic papers, in which he treated the theory of special relativity, explained Brownian motion, introduced the idea of quanta, and established the relationship between mass and energy.

After 1911 Einstein was sought by numerous universities. Following a brief period at the German University of Prague, he returned to Zurich as a professor at the very school that had scorned him, but finally in 1914 he could not resist the intellectual attraction of Berlin, where Planck and Nernst, among others, lived. He abandoned Zurich and went to become part of the heart of German physics, on terms that bear witness to the prestige he had achieved. He was named director of the Kaiser Wilhelm Institute of Physics, professor without specific teaching obligation at the University of Berlin, and member of the Prussian Academy of Science on exceptional terms. The period when he lived in Berlin permitted him to complete his theory of general relativity and establish his theory regarding the structure of the universe.

Influence of Political Developments

In Berlin, Einstein was no longer the scientist limited to science, but became involved in the political life of his period. Soon his sense of justice was sharpened and he played an active role as pacifist and began his battle against anti-Semitism.

During his youth in Zurich Einstein had been interested briefly in Judaism, but it was in Prague, when by chance he came upon a Jewish cemetery of the 5th century, that he came face to face with the centuries-old history of his people and found himself integrated with them. In Berlin he was surprised by the anti-Semitic positions taken by the university and the government, which foreshadowed a movement that was to reach its full force with the Nazi regime. He saw that the war of 1914 to 1918 was in the making and that it was awaited, if not hoped for, in certain military, political, and economic circles. He was astounded.

When war was declared, he was surprised to see his scientific friends offer their services as experts and take an active part in the war effort. He was thrown into the struggle, however, by the "Manifesto to the Civilized World," which was published in October 1914 and signed by 83 German scientists, among them several of the most outstanding. The Manifesto relieved Germany of all blame, justified the invasion of Belgium and, in terms that were to fail later, spoke of the annihilation of the white race by the Slavic hordes. Einstein readily signed an "Anti-Manifesto," which he helped to write. One of its sentences was prophetic: "No one will win the battle that rages today; all the nations that are engaged in it will pay a very high price." This antiwar declaration had only four signers, but Einstein did not lay down his arms. He immediately joined a movement started by Ernst Reuter, who was to become mayor of Berlin. The aim of the movement was to obtain an early peace and create an international organization for the preservation of peace, an ideal to which Einstein was devoted for the rest of his life.

The years that followed World War I were not easy ones for Einstein. Scientifically, he achieved great fame. The theory of general relativity was established by the direct observation of one of its principles. In 1919, during a total eclipse of the sun, the deflection of the light of the stars by the gravitational field of the sun, as Einstein had foreseen, was verified. However, Einstein was unhappy with the turn of international events. He refused to attend the 4th Solvay Congress in 1924 because the German scientists were not invited. Reaffirming his internationalism, he wrote to Marie Curie: "I understand that the Belgians and the French are not psychologically prepared to meet the Germans. But when I learned that the German scientists were excluded as a matter of principle, because of their nationality, I realized that by going to Brussels I would be lending my support to such a decision." At the time of the capture of the Ruhr Valley by the French army, Einstein, notwithstanding the anti-German feeling that he so often expressed, vigorously condemned the Allies.

The development of geopolitics and the recourse to armaments at that time so increased Einstein's preoccupation that he often joined pacifist movements, which used his name. His desire to contribute to world peace reached its height in the 1930's. In New York in 1930 he made a speech in which he said that "if 2 percent of the citizens refused to be drafted" governments would lose their ability to wage war. The speech—for which he was insultingly nicknamed "the 2 percent man"—was used against him by the followers of McCarthy even after he had obtained his American citizenship. His disillusionment with the efforts of the League of Nations and the slowness with which the Disarmament Conference held in Geneva in 1932 carried on its work led him to a new tirade. In a press interview before 60 foreign correspondents he appealed to the workers of the world to abandon their work in the armament factories and cease all activity related to the transport of armaments. Again he urged them to oppose the draft.

However, with the events in Germany after Hitler's rise to power, which led to an increase in militarism and armaments and a reappearance of anti-Semitism, Einstein was obliged to reexamine his pacifist position and change his ideas. Only force could win over the power of evil. He wrote to King Albert: "In the heart of Europe there is a power, Germany, which is preparing for war by all possible means. This has created such danger for the Latin countries, Belgium, and especially France, that they are necessarily forced to use their armies."

During World War II, he served the American war effort and played a role, which is sometimes exaggerated and sometimes belittled, in the organization of the Manhattan Project and the development of the atom bomb. Since the Hitler regime had made it impossible for Einstein to return to Germany in 1938, he established himself at the Institute for Advanced Study in Princeton, where he worked until his death on 25 April 1955. In Princeton he continued his work on unified field theory and tried in vain to remove the uncertainties which the theory of quantum mechanics (for which he had paved the way) had introduced into atomic and subatomic physics. He dedicated great efforts to the cause of Zionism—to which he had been driven by the Prague incident and German anti-Semitism--and after the war resumed his efforts on behalf of peace and international understanding, which were now threatened by the horror of a nuclear war, to which he had in a way unwillingly contributed.

Philosopher of Nature

Little by little, Einstein became a wise man, a sage, whose advice was sought and whose life was cited as an example. In a country where communication was rapid, the newspapers, radio, and television brought to the public the innumer- events of his daily life, although he ...d to protect his privacy. Each week he received hundreds of letters, and he tried to answer them, especially when he sensed a correspondent in distress. People lined the streets to see him pass and buses full of tourists stopped, as they still do today, in front of 110 Mercer Street, where the man lived who completely revised science in our lifetime.

Einstein became, through his international positions, a sort of conscience of a world in anguish. For many he was one of the greatest philosophers of nature, if not the most important of our time. His views of nature embodied a new Pythagorean approach. The harmony of the universe sealed by beauty was at the center of his thinking. To achieve its highest realization, Einstein had to postulate the existence of a superior being or system as the creator of a unified field of force and organizer of the mathematical harmony of the world. This concept has a pantheistic tinge and is very close to the thinking of Spinoza. Einstein himself, in reply to some questions, stated: "I believe in Spinoza's God, who reveals himself in the harmony of all things, and not in a God who is interested in the actions and the destiny of each individual."

Although he was certainly a rationalist, Einstein was not an atheist. Respect for the thought and the history of his people produced in him an underlying religious nature. He stated: "Science without religion is lame, and religion without science is blind." The idea of a God was always with him and he affirmed: "I would like to know how God created the world. I am not interested in this or that phenomenon, nor in the spectrum of a chemical element. I want to know His thoughts, the rest is a detail."

But in his search for universal harmony and the esthetics of natural laws, Einstein never lost sight of the human condition and the importance of realities which are above science. He said: "Our times are characterized by extraordinary discoveries in science and its technical applications. Who of us is not impressed by it? However, let us not forget that knowledge and technical aptitudes do not lead humanity to a happy and dignified life. . . ." To Queen Elizabeth of Belgium, stricken by a dual grief, he sent a message of consolation, from which I quote: "After all, there is something eternal which remains, beyond tomorrow, beyond destiny and human disappointments."

Einstein was a sower and, as Saint Paul said, because he sowed generously the fruits of his activity are found abundantly in our thoughts and our activities.

Human Issues in Human Rights

Robert W. Kates

Article, 11 August 1978

To be active in defense of human rights is to confront human issues both disturbing and difficult. For almost 2 years a committee of the National Academy of Sciences (NAS) has sought to develop a sustained program of human rights activity appropriate to its membership and its tradition of concern for the rights of scientific workers.

The Academy Committee

The Committee on Human Rights was chartered by the council of the academy and is composed of seven members of the academy and three liaison members from the National Academy of Engineering and the Institute of Medicine (*1*). Exceptional support is given to it by the staff of the Commission on International Relations (*2*). The committee received financial support from the academy's endowment funds.

Our activity focuses on the plight of individual scientists, engineers, and medical personnel suffering severe repression. Such cases may originate in the files of Amnesty International, with the initiative of a member of the academy, in a letter from a victim's friend or family, or in the informal reviews that we have undertaken of the fate of scientific workers in different areas.

When a case of severe repression has been identified, we begin a further pro-

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cess of inquiry that always involves consultations with a responsible spokesperson of the country where our colleague resides or is imprisoned and, where possible, with members of human rights organizations and scientific institutions, with a representative of the U.S. embassy in the country involved, and with the victim's family and friends. A great deal of care goes into these inquiries as we try to maintain the same standards of evidence, balance, and open-mindedness that characterize academy assessments on scientific matters.

When a clear-cut case has been developed—clear-cut in the sense that there is a strong basis to assume that an individual is, indeed, undergoing severe repression—we formulate a series of requests appropriate to the case and prepare a public statement. The statement and the requests are reviewed by the academy's 12 elected councillors; when approved, sometimes after revision, they are made public. Once this is done, our committee tries to undertake a sustained effort in behalf of the aggrieved individual, combining public remonstrance with appeals to the concerned government, their national academy if such exists, or other scientific institutions, and providing moral support to the individual and his or her family.

A major feature of the committee's effort is our correspondent network of some 350 members of the academy who have volunteered to receive communications from the committee and to act upon them. Thus, the efforts of the committee are amplified as correspondents write or cable governmental representatives, offer support to families, and call attention to the plight of individuals within their professional societies, at their workplace, or through their international contacts.

Currently, inquiries and other efforts are being directed in behalf of individuals in 11 countries, and public statements have been issued in behalf of 18 individuals: Federico Alvarez-Rojas, Claudio Santiago Bermann, Gabriela Carabelli, Juan Carlos Gallardo, Antonio Misetich, Eduardo Pasquini, and Elena Sevilla of Argentina; Vladimir Lastuvka and Ales Machacek of Czechoslovakia; José Luis Massera of Uruguay; Sergei A. Kovalev, Yuriy F. Orlov, and Antoliy B. Shcharanskiy of the U.S.S.R.; T. W. Kamil, I. Made Sutayasa, Bursono Wiwoho, and Kamaluddin Singgih of Indonesia; and Ibrahima Ly of Mali.

The last five cases, which we have just formally adopted, can demonstrate some of the committee's process. The Indonesian scientists have each been held incommunicado from immediate families and relatives for long periods of time. They are representative of a much larger number of Indonesians (3) who also are being held, often without trial, without access to legal counsel, and without other opportunity to have their arrest and detention clarified or adjudicated.

T. W. Kamil, about 50 years old, is one of Indonesia's leading linguistic scholars from the University of Jakarta; he studied at the University of Michigan. He was arrested on 30 October 1965 and has been detained since that time. He is reported to be held at Nusakembangan prison.

I. Made Sutayasa, believed to be about 36 years old, was an archeologist employed at the National Research Center of Archeology until 1975 when he was arrested in Jakarta upon his return from a conference of archeologists in Australia. He has not been formally charged or tried; it is believed he is being held on the island of Bali. He is married and has four children and may have been a member of the Indonesian Communist Party before it was banned.

Bursono Wiwoho, about 55 years old, was professor of educational psychology at Gadjah Mada University in Jogjakarta, Java. Professor Wiwoho had been a member of the National Planning Board under the Sukarno government and at the time of his arrest in late 1965 was chairman of the Indonesian Association of Scientists. He is reported to be either in Jogjakarta prison or detained on Buru Island, the location of a permanent resettlement camp for untried political prisoners. He participated in the nationalist movement to gain independence from the Dutch, after which he studied in Prague from 1951 to 1954 where he earned a degree in psychology. He later headed the psychology department of Gadjah Mada University in Jogjakarta and was a founder of the Indonesian Scholars Association. He is reported to be in poor health.

Kamaluddin Singgih, age 47 years, was teaching in the department of mechanical engineering at the Technological Institute in Bandung when detained January 1966. Later that same year he was released to assist in flood control projects—only to be taken into custody again in January 1967. Originally imprisoned in Salmba prison in Jakarta, he is now on Buru Island. Singgih studied physics and engineering at Stuttgart between 1960 and 1962 and was reputed to be sympathetic to the Communist Party.

Our information concerning these Indonesian colleagues is thinner than for most of our cases. Three of them have been in detention for upwards of 12 years and their very isolation makes it difficult to obtain current information about them. Thus we have quietly and privately sought—so far without success—to obtain clarification as to their status from the government of Indonesia.

We know somewhat more about Ibrahima Ly, a Ph.D. in mathematics from Moscow State University and, until his arrest and imprisonment, a mathematics teacher at the École Normale Supérieure in Bamako, Mali. He was arrested in June 1976 along with 14 other persons who were accused of writing a political tract that urged Malians to vote "no" in a referendum called by the Malian government to approve a new constitution. He was subsequently brought to trial in April 1975 under charges of subversion against the government, found guilty, and sentenced to 4 years of imprisonment.

Our list of concerns does not always get longer. The scientific community can share in the satisfaction that Juan Carlos Gallardo of Argentina has been released and is again working in physics in the United States and that Elena Sevilla should follow shortly. And among those cases that were resolved during the course of our inquiries, we note that:

- Ismail Mohammed, who spoke out against segregationist policies in South Africa and was detained there in September 1976 and fired from his university post, is now practicing mathematics again at another university.
- Grigoriy Chudnovskiy, a gifted young Soviet mathematician, was permitted to leave the U.S.S.R. with his brother, David, who is also a mathematician, and his elderly parents; he is currently in the United States receiving treatment for myasthenia gravis.
- Taysir al Arouri, a Palestinian physicist, who was detained for 2 years without charge or trial by the Israeli government, has been released; it is hoped that he will return to his position at Bir Zeit University, teaching physics and mathematics.
- Professor "X," an African mathematician, fearful of his life if he returns to his country, has had his visa extended to remain in the United States.

In each of these cases, our inquiries were some of many such efforts and remonstrances from scientific societies and individual scientists. We can all share in the satisfaction of their resolution.

But these are still the exceptions. Four

Argentinian physicists (Alvarez-Rojas, Carabelli, Misetich, and Pasquini) are simply reported as "not registered" by the Argentinian government. Massera enters his third year in prison in Uruguay despite his willingness to leave the country and take up an offer of a post in Italy. Kovalev enters his fourth year in prison despite offers from Stanford and Cornell for visiting posts. . . Czechoslovakia, the appeal by Machacek has been rejected and Lastuvka's sentence has been reduced by 1 year by the Czechoslovakian Supreme Court. They still remain as the first imprisoned victims of the Charter 77 movement, a Czechoslovakian effort to monitor human rights. Orlov and Shcharanskiy have been summarily tried and found guilty in the U.S.S.R.

As we sought to develop our program in our initial year of activity, we faced three major issues:

Whose human rights?

Which human rights?

How do we act for human rights?

Whose Human Rights?

The issue of whose human rights we were concerned with touches on the essence of a basic question that we had to consider: "Are scientists special?" After some months of discussion and reading, the consensus of our committee was negative.

Scientists are not special, neither as victims nor as torturers do we deserve to be singled out. It is true, as the Ziman report (*4*) states, that science as an activity has certain characteristics—a reverence for truth that leads its practitioners to query and dissent, a process of verification that requires open dissemination and communication, a universality of discourse and goals whose common language and pursuits go beyond national borders—that may be readily seen as threatening to authoritarian regimes or even in conflict with nation-building goals. But the Sierra Chica prison in Argentina is filled not with scientists but with young workers and students. As Philip Handler put it in an interview in *BioScience* (*5*):

> . . . tortured shoemakers hurt quite as hard as tortured scientists. Protesting only for scientists doesn't quite fit with my own beliefs about all of this. Scientists happen to be a little bit more visible. The world knows about them. The shoemakers are taken off behind the barn and shot.

If we scientists are not special, though, why limit our human rights activities to our own—to scientists, engineers, and health personnel? Our answer is simply: they are our own. We hope that other groups—trade unions, bar associations, women's groups, and shoemakers everywhere are equally or more active. But we will surely be more effective in identifying victims, in documenting their cases, and in supporting them, if we can appeal to that special quality of collegiality that we share and if we can use for such appeals the established avenues of scientific communication. In so doing, however, we know that in many, if not most cases, the nameless victims of repression have few if any scientists among them, and in some cases, scientists serve with the repressors.

Having chosen to limit our efforts to "our own" we were still left with some issues of definition: how far do we extend the concept of scientist and at what point in a career does an individual become a scientific colleague? Our answer, in keeping with the universality of science, was to seek a similar universality of outreach: not merely to the well-known and well-connected, not only to the ideologically similar, but to all victims in science, engineering, and medicine whoever they may be.

It is understandable that our academy officers have taken the strongest possible position in defense of Sakharov, a foreign associate of the academy; that as academicians who are well known and well connected we know and thus respond to the victims from our own ranks; or even that some of us more readily respond to victims of our own personal ideological persuasion. But it cannot be, and it is not, the position of our committee to limit our efforts to such cases.

Thus we have spent a considerable portion of our collective energy on trying to extend our knowledge of the plight of individuals in the less-known areas of the world and to examine the situation in our own and other countries of similar ideological persuasion. We have done so even at the cost of limiting our efforts in behalf of better-known cases, often to the pique and annoyance of our own members and colleagues.

We are, of course, limited in just how well we extend ourselves. In many parts of the world, science is poorly developed and so are our contacts with those areas. And we surely miss or rationalize away some of the travesties of human rights in our own midst. But it is not for want of trying.

Which Human Rights?

In considering the second issue—Which human rights?—I urge a reading of the Ziman report (*4*). It makes one basic point and makes it well. Beginning in 1945 and up to the present, a major international shift took place in human rights activity. Human rights today are not merely moral rights, they are international legal rights. An internationally accepted code of human rights—economics, social, political, and religious—has been developed. Most nations have not ratified that code, fewer nations implement those rights, and in only one case, the Council of Europe, does an effective international appeals court exist (*6*). But almost all nations give lip service to it, and many nations have signed treaties that accept at least a portion of the evolved code. There *is* an international standard for human rights.

Using this standard is extremely helpful to us in our selection process. We do not have to sit in judgment of our colleagues now more than 12 years in Indonesian prison camps or 13 months in Lefertovo prison. When the Israeli government says that Taysir al Arouri "incited terrorist activities" or Uruguayan officials say that Massera was found guilty of "subversive association" (*7*) we do not need to make judgments that are clearly beyond our knowledge. It is sufficient to know that Al Arouri has been detained without being tried or charged under so-called Jordanian Administrative Law (which is really the relic of the British Mandate Law and which ironically was used to imprison many pre-independence Israeli patriots). And to know that Massera probably has been tortured and has certainly suffered physical impairment, even though Uruguay has an established tradition of allowing opponents exile and there is a post in Italy for him and his wife (who is also imprisoned).

Thus we draft our requests—an end to torture, rights to trial, to representation, to visitation, to courtroom observers, to receive and send scientific literature, to exile (as opposed to continued detention), to humanitarian release (in case of hardship), and to extension of existing amnesty. All of these are rights implicit in the international code.

In requesting compliance with the international code, we need not underestimate the threats that nations perceive from dissidents: we live in a world where terrorism is a reality and not limited to national liberation or the overthrow of tyranny, where subversion and dissent may threaten all but the strongest and oldest of societies. Nonetheless, the prisons of the world would empty of prisoners of conscience if countries would adhere to the minimal rights of the *Universal Declaration of Human Rights* and its successor documents.

It we take international rights seriously and try to use them effectively to refute the charge of intervention in the internal affairs of other countries, we cannot choose them selectively. We Americans have an ideological bent that selectively equates human rights with certain civil and political rights, ignoring many other rights embodied in those covenants. Those who would dispute our right to speak out raise this issue repeatedly with us. Here are three examples. A member of an American scientific society who was born in Argentina, writes an open letter to the society's president condemning his action to protest the repression of Argentinian colleagues:

> You [the President] have called attention to nefarious practices in Argentina. Well. I must tell you that the average citizen there has more freedom in some aspects than we have here. Have you heard . . . that hundreds of thousands of elderly people, and some not so elderly, in certain areas of the United States are locking themselves up and starving because of the fear of street violence. Our American cities are filled with men and women like the elderly couple in New York who committed double suicide recently for fear of going out and being mugged again. Have you . . . written letters to President Carter about this shameful problem . . . [a] problem unknown in Argentina? (*8*).

The Iranian representative to the World Bank writes:

> In spite of some 30 years of debate over this complex issue in the United Nations. American and Western libertarian philosophy still regards "human rights" in a very narrow context: as essentially political, universal and timeless.
>
> But as far as the third world is concerned, they are largely one-sided, passive and abstract. They reflect political rights for the redress of grievances, personal immunity for unlawful or unnecessary search and seizure, habeas corpus privileges, due process of law for incarceration or imposition of fines, the absence of cruel and inhuman punishment, and a host of other individual freedoms of action.
>
> But they are silent about the society's obligation toward the individual, they say little about the right to employment, the right to obtain a meaningful education, the right to enjoy a minimum of life's amenities. These "active" and "positive" sides (that is, society's obligations) are either ignored or considered as secondary in the roster of Western "human rights" (*9*).

A Soviet legal expert echoes these sentiments:

> When they discuss human rights, many Western ideologists emphasize not socioeconomic problems but the freedom of the individual Without question [these are] an essential element of democracy.
>
> All these are not enough, however, in our view. We believe that society must also guarantee the individual the right to education, to work, and to material security. . . . There are now 17 million unemployed in the industralized capitalist countries . . . we have no unemployment [in the USSR] since the beginning of the thirties (*10*).

I have the impression that many human rights activists respond to these concerns as if they were red herrings to be ignored or a rationalization for repression and not to be credited with serious attention. In my opinion, this would be in error. To justify a sentence for Kovalev or Orlov by the fact that we tolerate a high unemployment rate in the United States or to excuse the thousands of kidnappings in Argentina on the basis of the muggings that occur in the United States would clearly be nonsense. But to ignore the substance of these challenges would be a serious mistake.

The international codification of human rights provides for economic and social rights as well as political rights. To use one of the examples cited: it is indeed shameful that in the richest nation on earth 6 to 7 percent of the work force is almost always out of work, and 30 to 40 percent of black youth seem permanently unemployed. Our most rational goals for unemployment range from 4 to 6 percent; in contrast, not only in socialist countries, but in many Western European countries, a 1 percent rate is considered a national calamity.

Let us welcome a genuine dialogue. As we point out to our Soviet colleagues that their remarkable employment record is not enhanced by denying dissidents the right to work, let us also consider ways in which we in science and technology may work to enhance the right of all to work, to have security in our cities, and to meet basic human needs.

There is another view as to which human rights can be asserted. It is the view of Sir Andrew Huxley and other British scientists within the Royal Society. He distinguishes between defense of the political rights of dissident scientists and the defense of scientific rights in the face of intrusion of the state He notes:

> The persecutions of the present day are not directed against scientific doctrines or against scientific enquiry as such, they are directed against individual citizens who have had the courage to speak up against oppressive features of the regimes under which they live Among these brave individuals there are, for example, writers and medical men as well as scientists The appropriate reaction therefore comes from us not as scientists but as citizens: if we wish to join in some corporate protest, it should be through one whose prime concern is with science If a scientific body publicly takes a step whose justification is political and not scientific, it will lose the right to claim that it is acting purely in the defense of science on some future occasion when it wishes to speak out against, say, a repetition of the Lysenko affair (*11*).

In choosing the clear-cut case of Lysenko and the role of some political dissidents, Sir Andrew has the advantage of posing the extreme cases to support his opinion. Unfortunately, we find it more difficult to grapple with the continuum of repression, to determine just what is scientific and what is political. To cite just two examples:

- Some Argentine scientists appear to have been denounced, killed, or imprisoned because they differed on scientific policy with the military or were actively reformist in the governance of their institute or university.
- Many Soviet scientists who are clearly political dissidents are punished by manipulating the institutions and even the literature of science: they are denied work, advancement, and degrees, and their works are even excised from the literature. Pressure is brought on their colleagues to acquiesce to or carry out these acts.

Drawing lines in science and human rights is difficult. We presume little righteousness in the way we have drawn ours, but we fail to see the incongruity of a scientific society concerning itself with the human condition of fellow scientists as well as with their scientific doctrine

How Do We Act for Human Rights?

Having chosen to speak to the internationally agreed-upon human rights of individual scientists, the issue of method still remains. How do we act in behalf of those rights?

Our major effort has been to develop an approach that matches patience with persistence. Our committee began work a year prior to President Carter's initiatives, and the academy's human rights activities date back to 1956. But we are sufficiently wise, or cynical, to note how cyclic and fluctuating public and government interest in current issues seem to be. Ours is a nation given to hyperbole, we fear that the crusade for human rights may go the way of the "great society" or the "war against poverty " It is irresponsible for members of human rights groups to raise the hopes of victims for redress and then turn to some other seemingly more current or pressing issue. Thus we have tried to develop a capability for patient, sustained, and persistent inquiry and support.

At the same time we have had to face (and continue to face) various tactical issues. From time to time we have been pressed by academy members and friends toward quite differing views. One member stated his dilemma as follows:

I applaud Carter's statements on human rights as an aspect of national policy. Will a letter from a lowly academic and civil servant, even though a member of NAS, really add anything to Carter's forceful statements and the NAS Committee declaration? Whatever it does, will it also be at the cost of the cordial working relations, indeed friendships, that I have managed to open up over the last decade with a number of Soviet scientists not to mention the matter of field access? Or worse, will it get my Soviet friends in trouble? It is easy to be for human rights when you're not personally threatened by their loss (*8*).

After much thought, this member did write one of the most moving letters I have encountered, expressing to Soviet authorities that it was the very respect and affection he had for their country and for his scientific colleagues that impelled him to appeal in behalf of an imprisoned colleague.

A very different view is expressed by a participant in the exchange program organized by the academy:

Our experience at a meeting in the USSR shows that generalized protest is almost useless, but specific threats and promises can accomplish a lot

My protest is against the Academy's decision to exert no actual pressure on the Soviets. Therefore since the Academy is useless on this issue, individuals and groups of American scientists have to protest against the Academy as well as against Soviet behavior (*8*).

In response to this and similar concerns, our position continues to be to hold to the universality principle, to not restrict the channels of scientific communication in the name of trying to maintain and extend them. This is our institutional position and it is my own. But it is not necessarily the position of all individual scientists. Scientific communication is in the last analysis a voluntary act, like so much we do. Increasingly, more and more scientists may withhold their participation in exchanges with individuals or groups they feel are unresponsive or even complicit in repressing colleagues. This has clearly been the response in the wake of the Orlov conviction in the U.S.S.R. His heavy sentence, closed trial, and the apparent treatment of his family and friends dismayed much of the scientific community. Individuals and groups, many with deep personal commitment to scientific exchange with the Soviet Union, canceled or delayed their planned trips, seminars, and meetings.

The Distortions of Humanity, Nations, and Science

How we allow the pain of the victim and the cacophony of protest to enter the quiet and generally well-mannered house of international science is a question that will persist and recur. But lurking in the nether darkness of questions asked but not answered are the most troubling of all issues, those raised by the profound distortions of humanness, nationhood, and science that follow in the wake of repression.

Torture is widespread. According to Amnesty International it occurs in some 60 nations. It is not limited to the Idi Amins or to the secret police of the world.

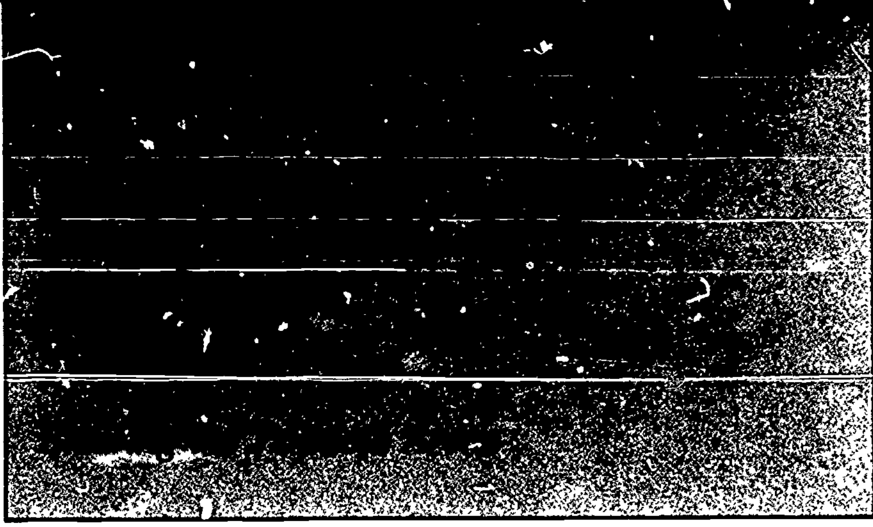
Our own country was accused by responsible observers of countenancing the use of torture and serious violations of human rights in South Vietnam and of permitting the teaching of highly questionable interrogation techniques in countries receiving assistance in the training of domestic police. Because of the European Covenant on Human Rights, Britain has compensated victims of inhuman and degrading techniques used in Northern Ireland.

It is not clear how much technical skill is needed to employ the electric shock apparatus used in basement cells. And one cannot imagine the professional standards of the doctors who patch up the victims in silence or of the psychiatrists who prescribe "treatment" for the dissidents they label insane. But surely they demonstrate that our scientific ethic is not universal and that it fails us now, as it did when horrors were perpetrated in scientifically run concentration camps.

For scientists to be active in defense of human rights is to learn and relearn what Rabelais knew in 1532: "Science without conscience spells but destruction of the spirit" (*12*)

References and Notes

1. Christian Anfinsen, Lipman Bers, Clifford Geertz, Franklin Long, John Ross, and Berta V Scharrer serve with me from the academy. Daniel Drucker and William Slichter are the liaison members of the National Academy of Engineering, and Adam Yarmolinsky is the liaison member for the Institute of Medicine.
2. Murray T[illegible]d serves as Executive Secretary, assisted by Jay Davenport, Sandra Erb, and Gerson Sher
3. Estimated by the Indonesian government to number 29,000, of whom 10,000 were to be re[illegible]sed in December 1977 and the remainder by the end of 1978 These estimates are contested by Amnesty International and some Indonesian human rights releases and purported leaders who place the number in excess of 50,000
4. Our most valuable reading was the report of a comm[illegible]ttee chaired by J Ziman The Council for Science and Society in collaboration with the British Institute of Human Rights, *Scholarly Freedom and Human Rights* (Rose, London, 1977)
5. E M Leeper, interview with Philip Handler, "Academy shifts emphases to keep up with the times," *BioScience* 27, 244 (April 1977)
6. For a less optimistic view and excellent review of the status of implementing these rights, see William Korey, "U N human rights. Illusion and reality," *Freedom at Issue* 42, 27 (September-October 1977)
7. Communications to the Committee on Human Rights (1977)
8. Communication to the Committee on Human Rights, paraphrased slightly to preserve anonymity (1977)
9. Jahangir Amuzegar, "Rights and wrongs," *New York Times* (29 January 1978), section IV, p 17
10. Interview with Vladimir Kudryavtsev, "Human rights How they are understood in the U S S R ," *Soviet Life* (July 1977)
11. Excerpt from an address by Andrew Huxley to the British Association for the Advancement of Science, August 1977 Reprinted in *Chemical and Engineering News* (26 September 1977), p 5
12. François Rabelais, *Gargantua and Pantagruel*, book II, chapter 8 (1532) (Heritage Press, New York, 1942)
13. I gratefully acknowledge the assistance of M Berberian, C[illegible] Jiman F Grohman, and the staff and members of the Committee on Human Rights in the preparation of this article



Responding to calls for greater political involvement on the part of scientists in addressing contemporary social issues, several writers explored how scientists might become more active in public affairs. Several fundamental questions were identified. How can scientists ensure that politicians have access to the latest research findings and interpretations in controversial areas at the moment when important public policy decisions must be made? What are the professional risks to scientists who seek to combine their expert skills of measurement and observation with the political world of influence and coalition building? And, at what point does a scientist cease to be a "disinterested observer" and become an advocate in public debate? Attempts to answer these questions stimulated debate and discussion throughout the scientific community.

J.R. Schenken developed a set of basic principles of responsible individualism that he offers as a guide for scientists entering the world of politics and public affairs. In the wake of the Oppenheimer security hearing, Schenken concludes that scientists could no longer afford the luxury of political celibacy. He believes that their privileged status created a social obligation to speak out clearly on basic concepts of freedom.

Responding to the themes of technological alienation widely publicized in the late 1960s, James D. Carroll suggests a way to make technology more responsive to individual and social needs. His "participatory technology" is one means of reintegrating individual and group needs in the development, use, and regulation of technology.

Frank von Hippel and Joel Primack explore how individual scientists can restore the constitutional balance of power between the public, the Congress, and the executive branch of government in regulating the effects of science and technology. Asserting that the executive branch often exploited its access to reservoirs of scientific expertise, they describe a series of incidents in which scientific advisers were used as a source of political advantage. They urge their colleagues to apply their talents in the public interest by acting on behalf of disadvantaged groups seeking to clarify important issues of public health or safety involving the uses of science and technology.

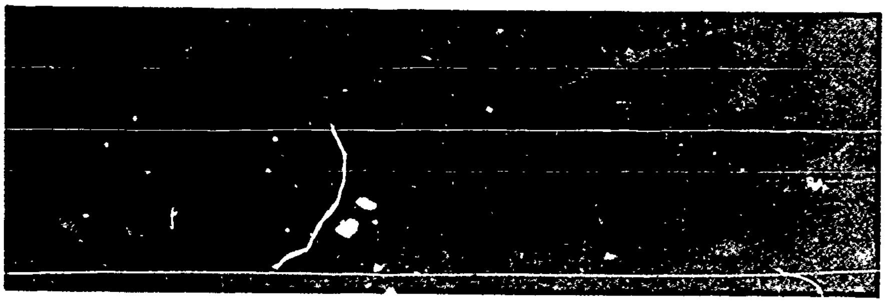
Reporting on public ambivalence about the benefits and consequences of technological development, Todd R. La Porte and Daniel Metlay conclude that "technological dissent" is not the product of a fundamental antitechnology or anti-intellectual bias Rather, it reveals a sophisticated awareness on the part of the general public that the social consequences of technology can produce conditions that threaten important social values These authors urge science policymakers to monitor public attitudes and beliefs to ensure that the public's confidence in science and technology is not eroded by a growing uneasiness about the consequences of scientific research and its technical applications.

Edward Wenk, Jr., suggests in a 1979 editorial that while individual scientists have important contributions to make in educating the public about the relationship between science and politics, professional organizations should provide direct assistance to public-interest activities and information to citizens as part of their regular information dissemination functions.

The premature release of research findings to the public can be counterproductive, however, as C.B. Raleigh's example of earthquake prediction science illustrates. Acknowledging that seismologists have important contributions to make in fostering public awareness of potential earthquake patterns, Raleigh maintains that the predictive uncertainties associated with this research require that the information be viewed as tentative. He suggests that scientists who publicize erroneous or uncertain information may be vulnerable to litigation, even though they acknowledge the likelihood of error in their work. The public's right to know, he concludes, is tempered by the need to protect scientists from restraints that would inevitably result from improper public warnings.

In a classic defense of the separation of the roles of scientist and citizen, Philip Handler describes distortions produced by the image of scientists engaged in adversarial positions in public policy debates. Scientists who advocate selected environmental or consumer causes, he argues, are doing so more as citizens than as scientists, and their professional judgments may thus become clouded by ideological beliefs. He concludes that scientists best serve public policy and public causes by living within the ethic of science, not that of politics

Institutional actions to encourage greater political involvement on the part of scientists are blunted by contradictory positions on the appropriate role of scientists in public affairs All agree that scientists must provide factual information through traditional research and publication activities, but there is disagreement about the lengths to which individual scientists and their scientific organizations should go in supporting social reform efforts Ultimately, these become matters of individual choice, guided both by general principles of social and professional responsibility in science and by various political ideologies. —*RC*



Scientific meetings in general, and particularly medical meetings, are rightfully dominated by the philosophy of René Descartes, who provided a guiding spirit in the 17th century when he wrote:

> I am sure that there is no one, even among those who make its [medicine's] study a profession who does not confess that all that men know is almost nothing in comparison with what remains to be known. I judge there was no better provision against a short life or lack of experience than faithfully to communicate to the public the little which I should myself have discovered, and to beg all well inclined persons to proceed further and then to communicate to the public all the things which they might discover in order that the last should commence where the preceding had left off, and thus by joining together the lives and labors of many, we should collectively proceed much further than anyone in particular could succeed in doing.

But the present complicated problems of our world—a world in which conflicting ideologies threaten the very existence of what we call Western Civilization, and a world in which scientific specialists are fortunate to be able to meet in an atmosphere free from government restrictions, military censorship, or the security surveillance of secret police—call for more than a dedication of our intellects and our labors to mankind.

In order that we may continue as a free society, every person must know and follow the basic principles of responsible individualism that have made our present achievements possible. The present technologic era began sometime in the 18th century, when wealth and science were united amid the clamor of men and women who since the Renaissance had regained their will to be free from political and spiritual enslavement.

A new man was created and mass production became the order of the day. The artisan became a specialist in labor and virtually became part of a machine. In the process of this metamorphosis he lost much of his imagination and creativeness. Such a man is restless because he has no impelling objectives in life. He fears the future because his talents are limited; he looks to society for his rights, and in so doing, he is in danger of losing his obligations. He avoids "travel along the road and is attracted to the Inn," as Cervantes puts it, where he can sit with self-satisfaction because he has no goal—no place to go.

Ortega was proud but somewhat alarmed at his Spanish countryman who, instead of merely pointing the way, readily escorted the inquiring foreign visitor to the point of inquiry, and thereby left a favorable impression of extreme courtesy. Ortega wonders if this act was a sign of national pride, or was it in reality a sign that his countryman had no mission in life, and therefore welcomed the opportunity to have one, to travel on the road, and not sit in the Inn. He suspects the latter has gripped the minds of many of his fellow men in all countries.

That is the brief story of the artisan who became a specialist in labor and some of his relationships with society. How did the scientist fare as the result of the 18th century marriage with wealth? A new type of scientist was eventually created. He too was engulfed in the surge of technicism because his effectiveness in this union with wealth depended upon the degree of his specialization, and hence his interests became "gradually restricted and confined into narrower fields of occupation." By the late 19th century he found that he could not survive on the broad philosophic concepts as Goethe had—biologist, physicist, chemist, botanist, playwright, poet, producer, and for 60 years the chief political adviser to Karl August, Duke of Weimar.

The new scientist is a specialist in science. Physics and chemistry have become a multitude of subspecialties. The scientist's learning continues; his technical skill becomes high; but his liberal education diminishes. He becomes entrenched so deeply in the intricacies of his specialty that the social problems of daily life seem unimportant; but the people hold him in high regard, and therefore he feels compelled to express opinions on subjects beyond his experience. The force of his words upon society is in direct proportion to the name he has made in his specialized field—a power so great that it is frightening. In some countries he has already paid a heavy price for his high position of specialized learning. Of the many examples, I shall recall only a few.

An Associated Press dispatch from Leipzig, Germany, dated 11 Nov. 1933, said

> In picturesque Leipzig, University German Professors, in an Armistice Day meeting, appealed to the intelligentsia of the world today for a better understanding of Germany. It was their way of urging popular support for the Nazi government in tomorrow's elections.

It is impossible to believe that these professors fully understood the objectives of the National Socialist Party of Germany, or if they did, they were already the political victims of demagoguery.

Also, in 1933, a brillant scientist, a young man 25 years of age, who had received his doctorate at the University of Leipzig, "left Germany on the rise of Hitler." He probably made no public statement at that time, and if he had, no one would have been impressed, but any statement he makes today is considered impressive news throughout the world. His name is Edward U. Teller, "the man who, by many, is considered the chief architect of the hydrogen bomb."

These incidents constituted the backdrop for a modern tragedy in which the leading character was a world-renowned scientist, J. Robert Oppenheimer, an important member of an international team whose contribution to the world was the technologic interpretation of Einstein's equation. No human being can accurately evaluate the impact of this technologic interpretation upon the future of mankind, but the best available opinion is probably that of Einstein himself. When he was asked what weapons would be used in World War III, his answer was that he did not know, but that he did know what would be used in World War IV—sticks and stones.

Oppenheimer was judged by a jury of his peers—all men of note and distinction—a chemist, a man of business, and an educator. The majority opinion judged him a loyal citizen of the United States, but unable to measure up to the requirements of the security system. With no intention to judge, but with only the hope to explain, it is my belief that their judgment was correct but their premise was wrong. He should have been found guilty only of a defection, one which he acquired inadvertently as a result of his scientific provincialism. Oppenheimer paid the supreme penalty of dishonor, an awful price to pay, for his subconscious vow of political celibacy.

Who knows how many brilliant and potentially productive minds will be stunted for fear of similar reprisal because they do not understand the true meaning of this indictment? What must be done to prevent another such catastrophe? Is the advice offered by Teller an adequate solution?

Referring to the Oppenheimer judgment, Teller told William L. Laurence of the *New York Times* that "scientists as a group should stay out of politics except in areas touching

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on science." Today his statement makes impressive news. Has he, too, become a political hermit? Should he not have warned his fellow scientists that such a course is fatal to the free life? Our founding fathers who wrote the Constitution of the United States and the Bill of Rights certainly did not sanction political hermitage.

Scientists seem to have forgotten that as Ortega said, "politics is much more of a reality than science, because it is made up of unique situations in which man suddenly finds himself submerged, whether he will or no."

In a republic the politician reflects the will of the people—the majority, but the majority may accept such things as penicillin and television as commonplace, and yet become indifferent to the cause of their existence. It is the minority, and not the majority, who challenge truth and create new concepts. Freedom in a republic exists only as long as the majority recognizes that the minority must be free to pursue thought, wherever it may lead.

Is it not incumbent upon every one of the 750,000 scientists of this country, and those in every other nation, to be able and willing to speak out clearly on the basic concepts of freedom? By virtue of the natural process of the years of rigorous academic selection, those who finally emerge as true scientists admittedly must have superior minds. If they will not speak out, who will?

Medicine was also an outgrowth of this technologic era—a coalition of the physical and biological sciences. Again, we look with gratitude to Europe—to France for the stethoscope and internal medicine, immunology and radium, to Germany for the x-ray, medical microbiology and pathologic anatomy, to Austria for obstetrics, to England for general surgery and the antibiotics, just to mention a few nations. All of Europe surging forward—free to study, to learn, to think, to imagine, to have ideas, and to be rewarded, as, in the words of Bacon, "The ancients assigned divine honors to the authors of inventions."

We in America developed slowly, making most of our contributions after the beginning of the 20th century.

We take particular pride in having harnessed, through clinical pathology, the many uncoordinated outgrowths of laboratory medicine into a cohesive unit that has become invaluable to the sick During the last 30 years the number of clinical pathologists in this country has increased tenfold, from 200 to 2000. Members of the International Congress of Clinical Pathology represent a total of 14 clinical pathology societies in 12 countries—mute testimony to the growth of this important specialty.

In the United States clinical pathology occupies a position of such magnitude that, although it has not yet attained independent status in all schools of medicine, no hospital in the United States can be accredited by the Joint Commission on Accreditation and no hospital approved for intern or resident training without an adequate clinical pathologic service.

Medicine and its broadest subspecialty, clincial pathology, are truly the products of the free minds of many lands, who "by joining together the lives and labors of many," have made this meeting possible and the world a much better place in which to live.

We are indeed fortunate that whatever may be revealed in the field of medicine will be free for the use of all mankind, regardless of political affiliations, religious convictions, or national boundaries. Medicine does not hoard its achievements. No true physician has ever patented a medical discovery. No one has ever denied its use by friend or enemy, only armed conflict between nations with differing political beliefs has ever imposed restraints upon the results of medical investigation. Even then, the individuals who waged war, who were enemies only by accident, ministered to the sick and wounded, whether they were friend or foe.

But what assurance do we have that medicine will continue to remain outside the domain of security regulations, secret police, political restraints, and the like?

In countries outside the iron curtain, even where varying degrees of government control hover over medical practice, there are few restraints upon the individual physician who seeks truth wherever he may find it, and gives freely of his labors to mankind. The only assurance we can have that this privilege to serve will continue is an enlightened citizenry who understands the principles of individual liberty and creativeness.

Because of the intimate contact with the ills of men, physicians have gained an enviable position of respect and confidence among men. This trust obligates them to become disciples of the philosophy of individual freedom in order that patients do not become apathetic to the cause of the miracles of modern medicine

Are physicians capable of accepting this responsibility? Or will they become political eunuchs? To what degree has specialization forced the physician to abandon a continuing pursuit of a liberal education and led him into the false security of political and scientific provincialism?

In order to answer these questions it may be well to review the educational background of the average medical man in the United States. A tabulation of the premedical educational requirements for admission to 78 schools of medicine in this country shows an alarming degree of concentrated training in the sciences. Approximately four times as many semester hours are required in science subjects as are required in the humanities. It is of more than passing significance, and I believe planned foresight, that only one school in the United States requires more hours of study in the humanities than in the sciences. That school is Meharry University, whose student body is composed entirely of Negroes.

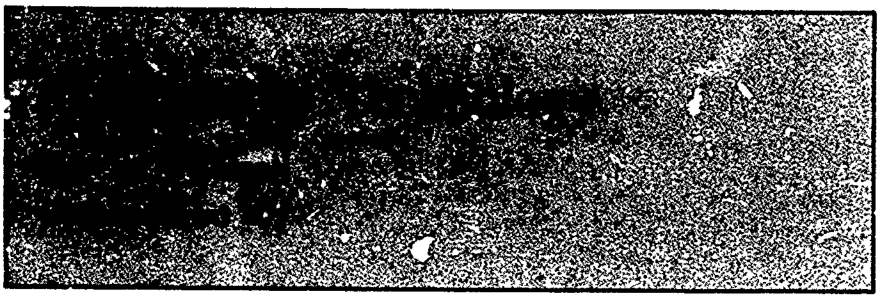
Add to the premedical schooling 4 more years of medical education, 1 year of internship, and 3 to 4 years of resident training in a medical specialty, and 2 years in the military, also in medicine, and you have a supersaturated, learned young man, 31 to 33 years of age, with almost pure scientific instruction throughout 14 of the most formative years of his intellectual growth. One saving factor is that he is intimately exposed to people and their problems—more closely than any other man, with the possible exception of the clergyman

Pathologists have a solemn role in medical education, whether it is for nurses, medical technologists, medical students, interns, residents, or the medical staff In this role lies the opportunity to broaden the educational base of medicine, in order to preserve the right of every physician to continue his scientific pursuits unhampered lest we become so engrossed in our own special field that we forget to look after the common good.

We must reject the philosophy of self-content, expressed by Hegel in 1831, but which is still heard in our day

> Let us content ourselves with what we have been allowed to achieve under the pressure of the circumstances and with the doubt whether amid the loud clamour of the day, there is left any room for sympathy, with the passionless stillness of the science of pure thought

We can and must meet the challenge of the janiceps of modern living—the science of the daily life as well as the science of pure thought



In recent decades the idea of the alienation and estrangement of man from society has emerged as one of the dominant ideas of contemporary social thought. While interpretations of the concept of social alienation vary, Etzioni (*1*) has expressed the core of the idea as "the unresponsiveness of the world to the actor, which subjects him to forces he neither comprehends nor guides. . . . Alienation . . . is not only a feeling of resentment and disaffection but also an expression of the objective conditions which subject a person to forces beyond his understanding and control."

There is considerable speculative and observational testimony and some empirical evidence (*2*) that the scope and complexities of science and technology are contributing to the development of social alienation in contemporary society. Keniston (*3*) for example, suggests that technology and its effects have been a factor in the alienation of many young people. At the same time he notes that the attitude of many young people toward technology is ambivalent because revolt against the effects of technology must inevitably exploit the technology it opposes. In a different vein, De Jouvenel (*4*) has testified to the adverse psychological impact of scientific and technological complexities on sustaining general confidence in one's judgment. "Because science saps such individual confidence, we have a problem, which I feel we can meet but which it would be imprudent to deny." In a more general observation Mesthene (*5*) recently has referred to "the antitechnology spirit that is abroad in the land."

Participatory Technology

In this article I analyze the incipient emergence of participatory technology as a countervailing force to technological alienation in contemporary society. I interpret participatory technology as one limited aspect of a more general search for ways of making technology more responsive to the felt needs of the individual and of society. The term *participatory technology* refers to the inclusion of people in the social and technical processes of developing, implementing, and regulating a technology, directly and through agents under their control, when the people included assert that their interests will be substantially affected by the technology and when they advance a claim to a legitimate and substantial participatory role in its development or redevelopment and implementation. The basic notion underlying the concept is that participation in the public development, use, and regulation of technology is one way in which individuals and groups can increase their understanding of technological processes and develop opportunities to influence such processes in appropriate cases. Participatory technology is not an entirely new social phenomenon, but the evidence reviewed below suggests that its scope and impact may be increasing in contemporary society.

I first analyze several facts of which people are becoming increasingly aware that suggest why participatory technology is emerging as a trend, and I then analyze different forms of this trend. Finally, I evaluate some of its implications.

Underlying Realizations

One primary reason for the emergence of participatory technology is the realization that technology often embodies and expresses political value choices that, in their operations and effects, are binding on individuals and groups, whether such choices have been made in political forums or elsewhere. In the language of contemporary political science, by "political value choices" I mean choices that result in the authoritative allocation of values and benefits in society. In its most significant forms politics culminates in the determination and expression of social norms and values in the form of public law, public order, and governmental action. To an indeterminate extent, technological processes in contemporary society have become the equivalent of a form of law—that is, an authoritative or binding expression of social norms and values from which the individual or a group may have no immediate recourse. What is at issue in the case of the computer and privacy, the supersonic transport and noise levels, highway development and the city, the antiballistic missile and national security, and the car and pollution is the authoritative allocation of social values and benefits in technological form.

The second realization is a correlative of the first. Technological processes frequently are the de facto locus of political choice. They are often political processes in which issues are posed and resolved in technical terms. In the absence of appropriately structured political processes for identifying and debating the value choices implicit in what appear to be technical alternatives, technical processes become, by default, the locus of political value decisions. In the context of a concern for the environment, technical questions of waste disposal systems involve value choices. In the context of a concern for urban development, technical questions of highway location and development involve value choices. In the context of a concern for privacy, technical questions of data collection and retrieval involve value choices. Technological processes often embody significant value questions that are difficult to identify and resolve in public forums because the processes are technically complex and occur in administrative organizations to which citizens do not have easy access.

Third, there is the realization that the public order of industrial society is not particularly well structured for identifying, publicizing, and resolving in public forums political questions implicit in technological processes. The public order of industrial society is founded on, and perpetuates, values, compromises, and perceptions that are being rendered obsolete by transformation of the social and political condi-

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tions from which they were derived. The public order of industrial society preeminently expresses perceptions of material need and the values of economic growth—perceptions and values rooted in the experience of material want and economic insecurity of past generations. Because of the development of powerful technologies of production, and because of other factors, these perceptions and values, as embedded and expressed in public institutions and processes, do not encompass the total area of concern, which is expanding to include the quality of the environment, race, urban development, population growth, educational opportunity, the direction of technology, and other matters. Established means of structuring and expressing political concern themselves often border on obsolescence, because they are often based on geographical and functional jurisdictions that are unrelated to the issues on which the public must take action. If these jurisdictions were otherwise defined—for example, were defined to include an entire metropolitan area—they might provide the structure for more effective representation of diverse views and might facilitate public action through bargaining and trade-offs.

Today, in the face of population growth and technological complexity, legislative bodies, except in unusual cases such as that of the antiballistic missile, delegate to administrative agencies the responsibility for regulating, developing, and controlling technology. The general objectives of these administrative agencies involve mixed questions of value and technique, and the agencies resolve such questions in terms of their bearing on realization of the general objectives. Often the general objectives further the interests of individuals and groups allied with a particular agency. To the Department of Defense the question of the desirability of developing, maintaining, and transporting chemical and biological agents is primarily a matter of national defense policy. It is not primarily a question of the humaneness of such agents, or of their ultimate effects on the environment, or of their value or threat to man in contexts other than that of national defense.

By default, the responsibility for scrutinizing mixed questions of technology and value from the perspective of societal well-being often passes to special-interest groups and to individuals who may or may not be in a position, or be well equipped, to learn of and to influence such decisions. This is one aspect of the more general phenomenon of the devolution of authority from public representatives and administrators to "private" groups and individuals in contemporary society.

Fourth, there is the realization that, in contemporary society, political action directed toward the achievement of political value objectives, such as the production of 2.6 million housing units a year, often depends on the ability to translate the desired objective into technical tasks. Marcuse (*6*) observes that "the historical achievement of science and technology has rendered possible the *translation of values* into technical tasks—the materialization of values. Consequently, what is at stake is the redefinition of values in *technical terms*, as elements in the technological process. The new ends, as technical ends, would then operate in the project and in the construction of the machinery, and not only in its utilization [emphasis in the original]."

To a considerable extent, the achievement of more effective processes of education, housing, delivery of health care, postal service, public safety, and urban development depends on the political and technological capacity of contemporary society to agree on, and to translate, value objectives into technological acts. Traditional legislative declarations of intent are not sufficient. The establishment of a right to a decent home in a suitable environment requires more than a legislative act declaring that such a right exists. It also depends on the development of technical capability to translate the right into reality.

This does not mean that in the formulation of political objectives, a technological, problem-oriented mode of thought must replace humanistic, intuitive, moral, and other modes of thought. It means that other modes of thought often depend for realization in public life and action on their expression in technical form, and that the development and control of that form is itself a political value-oriented act.

Fifth, there is the realization that the status enjoyed by technology as an agent for both bringing about and legitimatizing social change contributes to the growth of participatory technology. There is a tendency, stressed by Ellul (*7*), Rickover (*8*), and others, for contemporary man to accept change in technological form as inevitable and irresistible. In some cases, new technologies probably are accepted because of the specific results they produce for the individual, such as the mobility that, under some conditions, is made possible by the automobile. But there seems to be an additional social, psychological, and economic element at work—what Ellul calls "technological anaesthesia"—that generates acceptance of technological innovation irrespective of the particular effects that may result. Many people seem willing to use cars in urban areas even though such use may contribute little to mobility and may adversely affect the environment and health. It seems paradoxical but true that, while some changes in institutions and behavior are strongly resisted, other changes often are readily accepted when a technological element in the situation is the agent of change.

Participatory technology is one limited way of raising questions about the specific technological forms in terms of which social change is brought about. It is directed toward the development of processes and forums that are consistent with the expectations and values of the participatory individuals, who may resort to them in the absence of other means of making their views known. In participatory technology, however, as in other participatory processes, the opportunity to be heard is not synonomous with the right to be obeyed.

I here analyze three kinds of activities to illustrate some of the empirical referents of the concept of participatory technology.

Litigation

The first is the citizen lawsuit, directed toward the control and guidance of technology. As Sax (*9*) indicates, "The citizen-initiated lawsuit is . . . principally an effort to open the decision-making process to a wider constituency and to force decision-making into a more open and responsive forum. . . . [The] courts are sought out as an instrumentality whereby complaining citizens can obtain access to a more appropriate forum for decision-making."

The courts, of course, rely heavily on adversary proceedings, various forms of which have been suggested (*10*) as appropriate for handling scientific and technological issues involving the public interest. Not only can litigation restrict the use of technology, it can also lead to the modification and redevelopment of existing technology and stimulate the development of new technology to satisfy social values expressed in the form of legal norms, such as a right to privacy

The legal response to cases involving technology has taken two forms. The first is an extension of those aspects of the legal doctrine of standing which determine who has a right to be heard in court on particular issues involving activities undertaken or regulated by public agencies. The second is a search by legal scholars, practicing lawyers, and judges for systems of conceptual correspondence in the terms of which scientific and technological developments and activities can be conceptualized and evaluated as changes in social values and norms that may warrant a legal response. The appropriate role of law in the regulation of genetic experimentation is an example.

An extension of the doctrine of standing has occurred in several recent cases involving technology, although the extension is not limited to such cases. In the words of the United States Supreme Court (*11*), "The question of standing is related only to whether the dispute sought to be adjudicated will be presented in an adversary context and in a form historically viewed as capable of judicial resolution." The basic question is "whether the interest sought to be protected by the complainant is arguably within the zone of interests to be protected or regulated by the statute or constitutional guarantee in question" (*12*). The question of standing is a question not of whether a party should win or lose but of whether he should be heard.

The current extension of the doctrine is sometimes called the "private attorney general" concept. Under this concept a private citizen is allowed to present a case as an advocate of the public interest. A leading case is Scenic Hudson Preservation Conference *v.* Federal Power Commission (*13*), decided by the Second Circuit of the United States Court of Appeals on 29 December 1965. On 9 March 1965 the Federal Power Commission granted a license to Consolidated Edison Company to construct a pumped storage hydroelectric project on the west side of the Hudson River at Storm King Mountain in Cornwall, New York. A pumped storage plant generates electric energy for use during peak load periods by means of hydroelectric units driven by water from a headwater pool reservoir. The Storm King Project, as proposed by Consolidated Edison, would have required the placement of overhead transmission lines on towers 100 to 150 feet (30 to 45 meters) high. The towers would have required a path some 125 feet wide through Westchester and Putnam counties from Cornwall to the Consolidated Edison's facilities in New York City—a distance of 25 miles (40 kilometers). The petitioners were conservation and other groups and municipalities who claimed that the project, as designed by Consolidated Edison and as approved by the Federal Power Commission, would destroy the character of the land and the beauty of the area.

The Federal Power Commission argued, among other things, that the petitioners did not have standing to obtain judicial review of the legality of the license because they "make no claim of any personal economic injury resulting from the Commission's action."

The Court of Appeals held that the petitioners were entitled to raise the issue of the legality of the license and the licensing procedure even though they might not have a personal economic interest in the question. The court reasoned that a citizen has an interest in actions that affect the nature of the environment, and that this interest is arguably within the zone of interests that are or should be protected by law. On the merits of the case, the court held that the Federal Power Commission was required to give full consideration to alternative plans for the generation of peak-load electricity, including a plan proposed by one of the petitioners for the use of gas turbines.

The Scenic Hudson case is significant because it set a precedent for the enlargement of the opportunity of citizens, acting as citizens and not as private parties, to secure judicial review of the actions of public agencies, and of actions of the interests these agencies often regulate, in cases involving technology as well as other matters. The decision supports the proposition that, in certain cases, citizens will be recognized in court as advocates of a public interest, on the grounds that, as members of the public, they have been or may be injured by the actions complained of. They need not claim that they have been or will be injured economically or otherwise as private persons (*14*).

The development of the "private attorney general" concept does not mean that substantive changes will automatically occur in the constitutional, statutory, and common law doctrines that regulate rights and duties pertaining to the development and use of science and technology. The work of analysts in the areas of law, science, and technology—analysts such as Patterson (*15*), Frampton (*16*), Cowan (*17*), Miller (*18*), Cavers (*19*), Mayo and Jones (*20*), Korn (*21*), Green (*22*), Ferry (*23*), Wheeler (*24*), and others (*25*)—indicates the difficulties of developing systems of conceptual correspondence between scientific and technological developments and legal concepts and doctrines. Scientific, technological, and legal systems often further different values and serve different purposes, and the reconciliation of conflicts in these values and purposes is only in part a juridical task. The "private attorney general" concept, however, does invite more active judicial scrutiny of such conflicts and may contribute to substantive changes in legal doctrine in the future (*26*) in areas such as the computer and privacy; air and water supply and pollution, noise control; medical, genetic, and psychological experimentation; drug testing and use; nuclear energy and radiation; food purity and pesticides; and the control and handling of chemical and biological weapons.

While the legal form of citizen participation in the control and development of technology has severe limitations because it tends to be (i) reactive rather than anticipatory, (ii) controlled by restrictive rules of evidence, and (iii) subject to dilatory tactics, litigation has proven, over time, to be a significant element in the efforts of individuals and groups to influence the processes and institutions that affect them.

Technology Assessment

A second form of participatory technology comes within the scope of existing and proposed processes of "technology assessment." While the concept of technology assessment can be interpreted to include the kinds of legal action I have discussed (*27*), the term usually is used to refer to activities that are somewhat more anticipatory in nature and broader in scope.

To some extent "technology assessment" is a new label for an old activity—the attempt to comprehend, and to make informed decisions about, the implications of technological development.

The movement to formalize and improve this activity in a public context was initiated in 1967 by Senator Edmund Muskie (*28*) in the Senate and by Representative Emilio Q. Daddario (*29*) in the House of Representatives. This movement has successfully directed attention to some limitations in the way technological questions are currently considered in the American system of politics and government.

"Technology assessment" was defined in the bill introduced by Daddario in the House of Representatives on 7 March 1967 as a "method for identifying, assessing, publicizing, and dealing with the implications and effects of applied research and technology." The bill asserted that there is a need for improved methods of "identifying the potentials of applied research and technology and promoting ways and means to accomplish their transfer into practical use, and identifying the undesirable by-products and side effects of such applied research and technology in advance of their crystallization, and informing the public of their potential danger in order that appropriate steps may be taken to eliminate or minimize them."

The strengths and weaknesses of various forms of existing and proposed technology assessment are extensively analyzed in the hearings conducted by the Muskie (*30*) and Daddario (*31*) subcommittees; in the studies undertaken for the Daddario subcommittee by the National Academy of Sciences (*32*), the National Academy of Engineering (*33*), and the Science Policy Research Division of the Legislative Reference Service (*34*); and in related analyses, such as those made by the Program of Policy Studies in Science and Technology of George Washington University (*35*).

In these hearings and reports, citizen participation in technology assessment is both described and advocated. The analysis by Coates (*36*) of 15 case histories of technology assessments identifies one case that involved direct citizen participation—the examination of consumer products undertaken by the National Commission on Product Safety, which was established by Congress on 20 November 1967. In 1968 and 1969, the commission investigated the safety of such products as toys and children's furniture, architectural glass, power mowers, power tools, glass bottles, and aerosol cans. Citizens testified before the commission and directed the commission's attention to various incidents and problems. Coates observes that citizens participated in this particular assessment because the experience of members of the public with various products was itself part of the subject matter of the inquiry. There was no direct citizen participation in the other assessments examined by Coates, but the subject matter of several of the assessment processes suggests that some form of citizen contribution, either direct or through representative intermediaries, would have been appropriate. This is true, for example, of the assessments of environmental noise, and of future public transportation systems of advanced type.

In his written testimony submitted to the Daddario subcommittee, Mayo (*37*) stresses the importance, in assessment processes, of direct participation or representation of persons affected by a technology. He emphasizes the fact that technology assessment has a dimension beyond the identification and analysis of the impacts of technology. This is the dimension of evaluation of the social desirability or undesirability of such impacts. Since different segments of the public may view the impacts in various ways, as beneficial or detrimental, comprehensive evaluation is difficult without direct inputs from such segments. While special-interest groups can be relied on to express their views, they cannot safely be regarded as representative of the views of all major segments of the public that may be concerned.

Of the various hearings and reports generated by the Daddario subcommittee, the report of the technology assessment panel of the National Academy of Sciences places the greatest emphasis on citizen participation and representation. This panel asserts that legislative authorization and appropriation processes are inadequate as technology assessment processes because legislative processes frequently consider only the contending views of well-organized interest groups and often do not direct attention to long-range consequences. The panel further argues that, while technology assessment occurs in industry and in government agencies, with few exceptions the basic questions considered concern the probable economic and institutional effects of a technology on those who are deciding whether to exploit it. Existing processes fail to give adequate weight to "the full spectrum of human needs" because not enough spokesmen for diverse needs have access to the appropriate decision-making processes.

In the judgment of the panel, extensive citizen participation and representation in the assessment process is necessary both for practical reasons and for reasons of democratic theory. There are two practical reasons. First, citizen participation in the early stages of the development of a technology may help to avoid belated citizen opposition to a technological development after heavy costs have been incurred. Second, "objective evaluation" is impossible unless the diverse views of interested parties have been considered. On the level of political theory, the panel suggests that, in a democratic framework, it is necessary to consider the views of those who will be affected by a particular course of action.

The National Academy of Sciences panel explicitly acknowledges that technology assessment in some of its aspects is a political process because it involves questions of value (*32*, p. 83): "We can hope to raise the level of political discourse; we must not seek to eliminate it." The panel concludes (*32*, pp. 84 and 87) that there is a "need to accompany any new assessment mechanism with surrogate representatives or ombudsmen to speak on behalf of interests too weak or diffuse to generate effective spokesmen of their own. . . . Means must also be devised for alerting suitable representatives of interested groups to the fact that a decision potentially affecting them is about to be made. . . . *Whatever structure is chosen, it would provide well-defined channels through which citizens' groups, private associations, or surrogate representatives can make their views known. . . . It is particularly important to couple improved assessment with improved methods of representing weak and poorly organized interest groups*" [emphasis in the original].

As the National Academy of Sciences report states, and as Folk (*38*) stresses, to be effective technology assessment must function as part of the political process. What is at issue is the distribution and exercise of a form of decision-making power over technology. New technology assessment processes and structures probably would open decision-making processes to a wider constituency than now exists, and might change the distribution of power over some decisions involving technology. At the very least, new processes and structures might make it difficult for those accustomed to making technological decisions to do so without the knowledge of many other concerned people. It is doubtful that new assessment processes would be regarded as neutral either by those who now dominate technological decision-making processes or by those who might disagree with the results. Even though every effort were made to analyze questions of value as dispassionately as possible, or to exclude such questions entirely from assessment processes, dissatisfied parties almost certainly would attack the results and seek to offset them by other forms of political action.

Persuasion, bargaining and trade-offs in values are at the heart of political processes. Whether effective assessment can or should attempt to avoid these processes is questionable. Because technology assessment is to some extent a political process, the participation or representation of citizens may be not only desirable from the perspective of democratic theory but also necessary in political practice. Even such participation may not assure the effectiveness of the process in a larger political context.

Ad Hoc Activity

A third form of participatory technology encompasses a variety of ad hoc activities of individuals and groups beyond the scope of structured processes of litigation and assessment. This form includes activist intellectualism of the sort undertaken by Carson (*39*), Nader (*40*), and Commoner (*41*); quasi-official action of the kind undertaken by Congressman R. D. McCarthy concerning chemical and biological warfare (*42*); political and informational activities (*43*) of the sort undertaken by such groups as the Citizens' League Against the Sonic Boom, the Scientists' Institute for Public Information, the Sierra Club, Friends of the Earth, and Zero Population Growth; and sporadic activities of loose coalitions of individuals and groups energized by particular situations and issues.

Rather than attempt to survey such ad hoc activities, I here briefly describe and analyze an example of abortive participation that occurred in 1967 and 1968 in the initial efforts to develop a new town on the site of Fort Lincoln in Washington, D.C. (*44*). In some ways the Fort Lincoln example is typical of problems that often arise in processes of citizen participation in urban development. In other ways the case is distinctive because the primary purpose of the Fort Lincoln project was to demonstrate on a national basis the potentials of technological and administrative innovation for urban development.

On 30 August 1967, President Johnson publicly requested several members of his administration and of the government of the District of Columbia to begin at once to develop a new community on the site of Fort Lincoln, which consists of 345 acres of nearly vacant land in the northeast section of Washington, D.C. The President explained the purpose of the project as the development of a community that would demonstrate the potentials of administrative and technological innovation in urban development. The Fort Lincoln project was conceptualized as the leading project in a national program to develop "new towns intown" on federally owned land in various cities throughout the country.

On 25 January 1968, Edward J. Logue, who had achieved national recognition as an urban development administrator in New Haven and Boston, was retained as principal development consultant for Fort Lincoln. In the following 10 months, Logue and his associates developed an ambitious and innovative plan (*45*) that was based on, among other things, a thorough analysis (*46*) of the potentials for technological innovation in the development of Fort Lincoln and on a proposal (*47*) for an innovative educational system for the new community.

Fort Lincoln was a federal urban renewal project. Some form of citizen participation in urban renewal projects is required by law. Logue and the government officials involved in the Fort Lincoln project had had extensive experience with citizen participation in other urban development projects, including a model cities project in Washington, D.C. In developing the plans for Fort Lincoln, they made extensive efforts to fashion a participatory structure that would be acceptable to the citizens of the northeast section of Washington. For the most part they failed. Political activists in the area perceived the technical planning process as the locus of political opportunity and choice concerning such questions as the number of low-income families to be housed on the site. Although these activists disagreed over who could speak for the citizens, they agreed that the residents of the area should be granted funds to hire professionals to participate with and for them in the technical planning and development processes. At one point the Department of Housing and Urban Development offered to grant money for this purpose to the council that represented the citizens, but for various reasons the council rejected the offer.

The Nixon Administration suspended development of Fort Lincoln in September 1969, pending further study. One analyst (*48*) has argued that the project was suspended because neither federal nor local officials believed that the development plan was either technologically or politically feasible. Other analysts (*49*) have suggested that the project was suspended because members of the Nixon Administration regarded it as a personal undertaking of President Johnson's and as an example of the overly ambitious social engineering activities of "the Great Society."

The struggle over citizen participation diminished support for the project in the neighborhood and among its potential supporters in other areas of the city. No strong political constituency favored the project. The Nixon Administration could and did suspend it without antagonizing any strong or vocal interest group.

Fort Lincoln is one example of the extent to which technical planning and development processes can become the locus of political conflict when these processes are perceived as the de facto locus of political choice. It is also an example of some of the difficulties that can arise in the course of efforts to reconcile the dictates of administrative and technological reasoning with the dictates of the political thinking of participating individuals in particular situations.

Problems

Like many other participatory processes, participatory technology raises questions about the adequacy of the theory and practice of representative government.

According to traditional theories of American public life, citizens should express their demands for public action to their political and governmental representatives. Conflicting demands should be reconciled by persons elected or appointed to policy-making positions in which they are publicly accountable for their actions. Administrative and technical processes are not, in theory, the appropriate locus for the exercise of political influence and the reconciliation of political conflicts, because these processes are not usually structured as open political forums, and because most administrators and technical people are not directly accountable to electorates.

This theory of government is a prescriptive rather than a descriptive one It does not correspond well with the realities of the exercise of political power in and through administrative and technical activities. Among other things, increases in population, the expansion of the public sector, and the increase in technological complexity have changed the number and, to some extent, the nature of demands and possibilities for governmental action in recent decades. While legislative bodies and individual elected officials continue to respond to some of these demands, many other demands are considered and resolved in administrative processes of limited visibility. The very act of translating most legislation into specific processes usually involves an exercise of political choice. Furthermore, agencies often invite demands upon themselves as a way of expanding the scope of their support and powers.

The politicalization of administration in this century, especially in response to the activities of interest groups, is a widely recognized phenomenon (*50*).

Participatory technology is an attempt to influence public agencies directly, and, through them, the quasi-public and private interests they often influence and regulate. Like other participatory processes, participatory technology in some of its forms circumvents traditional processes of expressing demands through elected representatives and of relying on representatives to take appropriate action.

The hazards of participatory technology are many. On the one hand it can be used by administrative and technical people in a manipulative way to generate the illusion of citizen support of a particular course of action. On the other hand it can degenerate into forums for the exercise of obstructionist, veto-power techniques and paralyze public action. It can generate an overload of demands that agencies are not equipped to handle. It can be used as an instrument by an aggressive minority to capture decision-making processes and to impose minority views on a larger community. It can simply shift the locus for the exercise of "the tyranny of small decisions" (*51*) from one group to another or merely enlarge the core group that exercises control. Finally, it can lead to the dominance of technological know-nothing over the judgments of qualified individuals who are legally responsible for, are dedicated to, and understand processes of public action.

At the same time, as Spiegel and Mittenthal (*52*) observe, "Citizen participation can occur in partnerships with a governmental unit as well as against it. Its nature can be cooperative and integrative or conflicting and oppositional. . . ." Participatory technology, if appropriately structured, can contribute to decision-making processes that take into account alternative points of view, and can help an agency perform its functions in a more effective and open manner. It can provide a means by which the individual who feels powerless in the face of technological complexity can find a forum for the expression of his views.

The basic questions are these: In what cases is citizen participation in technological processes warranted, and according to what rationale? How should participation be structured and conducted? How much weight should participation be given in decision-making processes?

To provide a priori answers to these questions is impossible because of the variety of situations to which they apply For this reason it is recommended that public agencies, scientific and technical associations, and individual members of the scientific, technological, and political communities undertake analyses of these questions in the various situations for which they have responsibility or to which they have access. No single activity by a particular organization such as the National Academy of Sciences can meet the need. The analysis must be as broad-based as the activities to which these questions apply.

At the same time, the men responsible for policy making in foundations should consider the establishment of an experimental center for responsive technology. Such a center would analyze, on a continuing basis, the question of the ways in which public participation in technological decisions involving a public interest can be structured, and would support such participation in appropriate cases. The center might also support the education of proponents of technology, who would be qualified to recognize alternative conceptions of the public interest in technological matters and to present these conceptions to decision-making bodies.

Summary

The hunger to participate that exists today in various segments of the American public is in part a response to what some people perceive as an unresponsiveness of institutions and processes to the felt needs of the individual and of society. It is also, in part, an expression of a desire for a redistribution of power in American public life.

Technology is one of the major determinants of the nature of public as well as private life in contemporary society. Participatory technology is an attempt on the part of diverse individuals and groups to influence technological processes through participation in existing or new public processes by which technology is or can be developed, controlled, and implemented. Like other processes of direct citizen participation in governmental decision making, it raises many questions about the adequacy of existing theories and practices of representative government. These questions cannot be answered on an a priori basis. Members of the educational, scientific, technical, and governmental communities should analyze these questions in an effort to develop answers that are appropriate to the particular situations for which they are responsible and with which they are concerned.

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Public Interest Science

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Although scientists as technical experts make important contributions to the federal policy-making process for technology, that process remains basically political. At present, the primary recipient of technical advice on matters of public policy is the executive branch of the federal government. To the extent that this arrangement results in an informed executive branch dealing with a relatively uninformed Congress and public, a corresponding shift in power occurs. Indeed, it is not unheard of for the executive branch to abuse its near monopoly of politically relevant technical information and expertise. We cite below several case studies exemplifying the sorts of abuses that occur: politicization of advisory committees;

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suppression and misrepresentation of information, and analyses.

This leads us to the question of whether individual scientists can contribute significantly to a restoration of a balance of power between the public, Congress, and the executive branch of the government. We find, again on the basis of case studies, that a few scientists can be surprisingly effective in influencing federal policies for technology if they are sufficiently persistent and skillful and if various other circumstances are favorable. These success stories and the present high level of concern about the adverse side effects of technology among both scientists and the public suggest that the time is propitious for a much more serious commitment within the scientific community to "public interest science."

This article is divided into two main sections. The first deals with devices by which the executive branch exploits its scientific advisers for political advantage while concealing much of the information they have provided; the second discusses ways in which scientists can help bring into being counterbalancing political forces by providing the public and Congress with the information they need.

For brevity we refer below to scientists advising officials in the executive branch of the government as insiders and scientists taking issues to the public and Congress as outsiders. Of course the same scientist can and sometimes does find himself in both these roles at different times.

Abuses of the Executive Advisory System

Many thousands of scientists serve part-time on committees advising officials in the executive branch. It appears, however, that, if substantial political and bureaucratic interests are at stake, the dangers these insiders point out are often ignored. This is not surprising; it is one reason why our government was designed with checks and balances. These checks and balances are undermined, however, when executive spokesmen can use the authority of inside advisers to mislead the public and Congress about the technical facts or uncertainties that must be taken into account in the policy-making process.

Thus, for example, William Magruder, director of the supersonic transport (SST) development project, appeared before a congressional committee to allay fears about the SST sonic boom, airport noise, and stratospheric pollution. Magruder summarized the Administration's views on these issues as follows (*1*):

> According to existing data and available evidence there is no evidence of likelihood that SST operations will cause significant adverse effects on our atmosphere or our environment. That is the considered opinion of the scientific authorities who have counseled the government on these matters over the past five years.

Compare the above with the following quotations from the report of a panel of President Nixon's SST ad hoc review committee (*2, 3*) which included in its distinguished membership the President's science adviser. [The report was released 8 months after its completion, as a result of strenuous effort by Representative Henry Reuss (D-Wis.)]. Regarding the effect of the SST on the upper atmosphere, the panel noted that a fleet of SST's "will introduce large quantities of water vapor into the stratosphere," and concluded that much more research was needed before serious deleterious effects could be excluded. With regard to the impact of the SST sonic boom on the human environment, the panel concluded

> . . . all available information indicates that the effects of the sonic boom are such as to be considered intolerable by a very high percentage of people affected.

Finally, as to the impact of the SST engine noise, they stated

> . . . over large areas surrounding SST airports . . . a very high percentage of the exposed population would find the noise intolerable and the apparent cause of a wide variety of adverse effects.

In its adverse statements on the SST's environmental impact, the ad hoc committee report echoed many other reports available to the Nixon administration (*4*). Thus Magruder's statement is extremely misleading. Similar misrepresentations of scientific advice have been made by spokesmen for the federal executive branch in virtually all the other cases that we have studied (*5*).

Perhaps the most frequent means by which the public is misled in through the incomplete statement. Typically, an executive branch spokesman tells Congress that agency A, after consulting the greatest authorities, has decided to do X. The spokesman neglects to mention, however, that the experts have given mostly reasons why X might be a dangerous policy. The public cannot check what the experts actually said, because the reports are kept secret. Of course Congress can ask several well-known scientists to appear before it and offer their views on the matters at issue in congressional hearings, but this is no substitute for requiring an executive branch agency to make available for public review and criticism the detailed technical basis for its decisions.

Examples of Abuses

There is a whole spectrum of devices by which the federal executive's advisory establishment has been used to mislead Congress and the public. Perhaps a few additional examples will indicate the possibilities:

1) In the final throes of the SST debate, an advisory committee report was released which stated that, with noise suppressors, the SST airport noise could be reduced to tolerable levels (*6, 7*) No report was issued on what these changes would do to the SST performance, however. Every indication is that the noise suppressors, whose weight was of the same order of magnitude as the total payload of the aircraft, would seriously threaten the already questionable economic viability of the aircraft (*7*). Thus, government officials can selectively make public advisory committee reports that present only some of the positive terms in a cost-benefit calculation.

2) A report on sonic boom effects by an advisory panel organized by the National Academy of Sciences–National Research Council (*8*), was so written that, when it was released, it stimulated a *New York Times* headline (*9*), "Sonic Boom Damage Called 'Very Small.'" In fact, simple calculations based on extensive government tests results lead to the estimate that, with 400 SST's flying supersonically over the United States, the sonic boom damage each year would be of the order of a billion dollars (*10*). What the advisory committee had meant to say was that the probability is small that a single sonic boom would damage a particular building, and therefore that experiments on sonic boom damage should be carried out in a laboratory with a sonic boom simulator. When a

clarifying statement was eventually issued, after a petition from Academy members, it appeared only in the Academy newsletter and received no press coverage.

Thus advisory committee reports may be so written that they are seriously misleading, at least to the press. Political and institutional pressures may prevent the issuance of a proper clarification, or the press may ignore it.

3) In 1966 a report by an independent laboratory under contract to the Department of Health, Education, and Welfare indicated that 2,4,5-T (2,4,5-trichlorophenoxyacetic acid), a popular weed and brush killer, causes birth defects. This report was repeatedly sent back for "further study" for 3½ years (*11*) until it finally became public as an indirect result of a Nader investigation (*12*, p. 21). In the meantime, enormous quantities of this chemical were used in the defoliation of about one-eighth of the area of South Vietnam (*12*, p. 85; *13*).

It may give an idea of the amount of bureaucratic foot-dragging involved in this case to note that, when one of the chemical manufacturers suggested that an impurity, not 2,4,5-T itself, might have caused the birth defects, the experiments that had taken 3½ years to complete were repeated in about 6 weeks. Both 2,4,5-T and the contaminant were found to produce birth defects (*11*). When these results became public, the use of 2,4,5-T in Vietnam was banned, its domestic use was partially restricted, and further restrictions are now being debated (*11*).

The studies relating to the question of whether pesticides cause birth defects were undertaken partly in response to the public furor caused by Carson's *Silent Spring* (*14*). Nevertheless, even while the public was being assured that the government had undertaken to protect it from such possible dangers, the government was concealing relevant new information. Thus, when the government has exclusive access to certain information about a public health hazard, it can simply ignore it.

4) In October 1969, Secretary of Health, Education, and Welfare Finch was forced by law to ban foods containing cyclamates because cyclamates had been shown to cause cancer in animals. At the same time, he decided to overrule protests from the Food and Drug Administration and allow manufacturers of these products to continue to sell them as nonprescription drugs for the treatment of diabetes and obesity (*15*, *16*). After announcing his decision, he called together an advisory committee which reported back that, indeed, Secretary Finch was right in overruling the FDA medical people. The committee concluded (*15*, p. 86):

> . . . the medical benefits in these instances [treatment of diabetes and obesity] outweigh the possibility of harm.

After the publication of a Nader study report on the background of Finch's decision (*17*), its legality was examined in a rather devastating congressional investigation. The advisory committee was then called together again, and, although it had received essentially no new evidence, it issued a new report on the safety and effectiveness of cyclamates. This time the committee contradicted its earlier statement by saying (*16*, p. 13):

> The literature provided to the group does *not* contain acceptable evidence that cyclamates have been demonstrated to be efficacious in the treatment and control of diabetes or obesity. [Italics ours]

Cyclamates were thereupon totally banned. In this example it appears that an advisory committee became so political that it adapted its advice to the political needs of the official whom it was advising.

Correcting the Record

It is natural to ask whether insiders cannot do something to curb these abuses. In fact, advisers have tried to set the record straight in a number of recent cases:

Richard Garwin, a member of the President's Science Advisory Committee, was chairman of a committee of scientists reviewing the SST project for President Nixon at the beginning of his presidency. Although his committee's report was kept secret its existence was not, and Garwin was invited to testify at Congressional hearings (*4*). In his testimony he expressed his personal criticisms of the SST, documenting them from publicly available sources.

Garwin explained his actions in the following words (*18*):

> I'm not a full-time member of the administration, and I feel like a lawyer who has many clients. The fact that he deals with one doesn't prevent him from dealing with another so long as he doesn't use the information he obtains from the first in dealing with the second. Since there are so few people familiar with these programs, it is important for me to give to Congress, as well as the administration, the benefit of my experience.

Kenneth Pitzer was chairman of a President's Science Advisory Committee panel charged with looking into the safety of underground testing of large nuclear weapons in November 1968. The panel concluded that there was a significant danger of earthquakes and resulting tidal waves being triggered by bomb testing in the Aleutians. They also commented (*19*):

> . . . the panel believes that the public should not be asked to accept risks resulting from purely internal government decisions if, without endangering national security, the information can be made public and decisions can be reached after public discussions.

The report expressing the panel's concerns was kept secret. Pitzer, however, helped make these concerns public (*20*).

Sidney Drell and Marvin Goldberger served on a committee advising John Foster, Director of Defense Research and Engineering, on the effectiveness of the Safeguard ABM system. When Foster misrepresented their committee's report as supporting the Administration position, they spoke up to set the record straight (*21*). Goldberger expressed their opinion of Safeguard rather pungently. He said

> . . . I assert that the original Safeguard deployment and the proposed expanded deployment is spherically senseless. It makes no sense no matter how you look at it.

Unfortunately, these examples appear to be the exceptions. It seems that advisers usually watch in silence when they know that the public is being misled. The authors of the National Academy of Sciences sonic boom study mentioned above, and also academy officials, actually resisted the issuing of a clarifying statement.

Two main reasons are given for this silence: (i) Most advisers have very little faith in the effectiveness of speaking out, and they fear that by going public they would lose their inside influence. (ii) There is also the argument that, since the President is elected by all the people, he has the ultimate responsibility for making national policy. In its extreme form, this "elected dictatorship" theory of government leaves the adviser with only the responsibility to see that the President and the officials in his administration are well informed.

The loss of effectiveness argument emphasizes the serious dilemma in which a frustrated inside adviser may be placed as a result of the executive branch's insistence upon loyalty and confidentiality. However, insiders should beware of exaggerating their supposed effectiveness, and of confusing prestige with influence.

The elected dictatorship argument obviously denies the whole system of checks and balances by which our democracy has been safeguarded. It also ignores the fact that the ultimate responsibility in a democracy resides with the individual citizen, and that denying him the information he needs to defend his own health and welfare effectively deprives him of the rights of citizenship. The writers of our constitution understood this very well. James Madison said (*22*):

> Knowledge will forever govern ignorance. And a people who mean to be their own governors must arm themselves with the power knowledge gives. A popular government without popular information or the means of acquiring it is but the prologue to a farce or tragedy, or perhaps both.

It is obvious that the responsibilities of government science advisers should be discussed widely, both within the scientific community and in the larger political community. Lack of such discussion leaves scientists unprepared when they become advisers and find themselves confronted with difficult and unfamiliar decisions—often in an atmosphere of great pressure. Science advising, no less than scientific research, needs a code of ethics. And this code should take into account the fact that we live in a democracy in which the ultimate responsibility resides not with the President, or even with the government as a whole, but with the individual citizen.

Before going on, let us try to rectify the misunderstandings that may have resulted from the discussion so far. We do not wish by our criticisms of the abuses of the executive science advisory system to diminish or obscure the many important and legitimate functions inside advisers perform (*23*). Their roles as independent critics and connoisseurs of technical policies and people are essential throughout the executive branch. The executive advising system also provides a tremendously important path by which information and ideas can flow rapidly through the government, and between governmental and independent scientists, outside the slow bureaucratic filter. Indeed, in our opinion it has been a serious weakness of the most recent administrations that they have failed to exploit adequately these potential strengths of the advisory system.

Public Interest Science

The executive branch of our government has not been acting in an unbiased manner in making available to the citizen the technical information he needs. Scientists must therefore make their expertise directly available to the public and Congress.

The idea that the public, as well as the government and industry, should have scientific advisers is an old one—as is the idea that the interests of the public should have lawyers to defend them. It was not until the 1960's, however, that public understanding of the insensitivity of governmental and industrial bureaucracies led to a substantial commitment in the legal profession to public interest law. It appears to us that the scientific community may now have reached a similar point. The growing public awareness of the dangerous consequences of leaving the exploitation of technology under the effective control of special industrial and governmental interests has led to a readiness within the scientific community to undertake a serious commitment to what we have termed "public interest science."

There is an important difference between the practice of public interest law and public interest science, however. In a legal dispute, once both parties have obtained a lawyer, they can hope to obtain a fair and equal hearing in front of a trained judge who gives their arguments his undivided attention, whereas in a public debate over an application of technology tremendous inequalities exist. The contending sides must speak to a distracted public through news media to which executive officials have comparatively easy and routine access. Moreover, an executive official speaks with the authority of his office, while an independent scientist is usually an unknown quantity to the public.

In view of these inequalities, it is interesting to find out whether the public interest activities of independent scientists can activate political and legal restraints on irresponsible actions of the executive branch. In working on this question, we have thus far examined the effectiveness of outsiders in informing the public about the negative aspects of the SST, the decision to deploy the Sentinal and Safeguard antiballistic missile systems, the program of crop destruction and defoliation in South Vietnam, and the regulation of pesticides. We have also studied the effectiveness of a local group of scientists, the Colorado Committee for Environmental Information, in bringing to public attention in 1968 through 1970 the dangerous practices of two federal agencies in Colorado.

Examples

In all these instances, the outsiders have had a surprisingly large effect, considering their small numbers, in bringing to public attention an aspect of the issue that concerned them. Consider a few examples:

1) Serious public opposition to the SST developed only after a few scientists, notably Shurcliff, made dramatically clear in press releases and advertisements that the sonic booms created by a fleet of SST's flying supersonically overland would be intolerable (*4*).

2) The residents of the Denver area did not realize that they might have a problem until scientists of the Colorado Committee for Environmental Information (CCEI) issued a public statement describing the possible consequences of an airplane crashing into the huge stockpiles of nerve gas stored near the end of Denver's busy airport. After trying in vain to reassure the public, and then to transport the nerve gas across the country to dump it in the ocean, the Army finally agreed to destroy it (*24*).

3) The U.S. program of defoliation and crop destruction in South Vietnam came to an end when a group of scientists sponsored by the AAAS brought back photographs and a detailed report of the devastation that resulted (*25*).

4) The deployment of an ABM system to defend the major cities of the United States became a public issue only after scientists in the Chicago area and elsewhere raised what most experts considered a minor issue—the possibility of the accidental detonation of an ABM (antiballistic missile) warhead in the metropolitan area it was supposed to be defending (*26*).

Of course, we could equally easily compile a list of cases in which public protests by scientists have had little effect on federal policy. Most technical issues cannot be taken directly to the public because there is little public resonance with the ideas involved. That does not decrease the importance of the issues that can be taken to the public, however.

The effectiveness of outsiders in influencing government policy seems to depend on many factors. For one, where outsiders have been influential, the dangers they pointed out usually threatened huge numbers of people personally. Their effectiveness seems also to have depended upon how important the policy being criticized was to the government. Consider the obsolete nerve gas, for example; leaving it at such a dangerous location was simple negligence that could be rectified by spending a little money when it became clear that reassuring statements would no longer suffice. On the ABM, SST, and pesticide regulation issues, however, the critics were attacking policies that governed the allocation of billions of dollars. Over these issues the battles have been rough and prolonged and have required the active involvement of large numbers of citizens in addition to scientists.

The effectiveness of the outsiders also often depends upon the timeliness of an issue. Thus, after Shurcliff and a few others had been denouncing the SST for years, the new environmental movement came to see it as a symbol of all that is destructive to the environment. Similarly, the ABM became a popular issue in part because the public had become concerned about the insatiable appetites of the military-industrial complex. And, after a few biologists and ecologists had been protesting for years about defoliation and crop destruction in South Vietnam, they were finally heard when the public had become disgusted with the United States' entire Indochina policy.

Our case studies give substantial encouragement that some issues can be taken to the public by scientists with partial success at least. It is not easy, however. Enormous persistence and skill are required, as well as a good and timely case, to be heard above the din that accompanies everyday living in this country.

Credibility

It is also necessary for the scientist to establish credibility—that is, that he is not a "crackpot." Credibility has sometimes come from the quotation of government reports that contradict the official line. It has come from preparing a compelling and well-documented case from the open literature, as Carson did in her criticism of pesticide regulation (*14*). It has come from a study sponsored by a scientific organization: an example is the AAAS study of the effects of defoliation in Vietnam (*25*).

Yet another technique for handling the credibility problem was applied quite effectively by CCEI (*24*). In two of the debates in which it became involved the CCEI publicly challenged the responsible government agency to establish the basis for its assertions. The Colorado group accompanied the challenge with a specific list of technical questions, the answers to which would make possible an independent determination of public safety. Finally, credibility—and also publicity—can be obtained if one can persuade Ralph Nader to take up the issue. The extent to which we all depend on Nader in these matters is a testimonial to the timidity of the professional societies, universities, and national laboratories.

The scientist's public credibility must, of course, be earned. A specialist who uses his authority as a recognized scientist to lend support to a political position without presenting the technical arguments casts doubt both on his political position and on his scientific authority. The standards of accuracy to which a scientist adheres in public statements should be no lower than those he strives to attain in his scientific work. It is also necessary for the scientist to maintain a sense of perspective; it is all too easy to exaggerate the significance of a subject on which a critic happens to be an expert. The danger of crying wolf is not merely that the next time a justified alarm may be ignored; it may also happen that the false alarm will be heeded and the nation stampeded toward a foolish or unnecessarily hasty action. Obviously, the proper ethics for outsider science advising deserves discussion within the scientific community no less than the ethics of insiders.

During and after each of the major technological debates of recent years there have been charges that scientists who participated as outsiders were politically biased and scientifically irresponsible (*27*). While there have certainly been a few instances that substantiate such charges, the vast majority of independent scientists who have argued technological issues before the public have been honest and accurate. A scientist's reputation is his most precious possession, and the scientist who misrepresents the truth or makes unsound technical judgments calls down upon himself the censure of his colleagues. In any event, technical arguments presented in public can be rebutted in public, in the usual self-correcting manner of scientific discourse. Indeed, it is unfortunate that the statements of executive branch officials are not subject to similar constraints. Apparently, the standing of these officials depends more on their loyalty than on the accuracy of their public statements.

As we have mentioned, the route of taking issues to the public is very important but also quite limited; many issues cannot be so treated. Other routes are available, however. Sometimes recourse to the courts is possible. Recent developments in the law, particularly the National Environmental Policy Act of 1969, make this approach increasingly effective. Taking advantage of the protection offered by the law requires more than public interest lawyers, however. It requires public interest scientists as well. The collaboration of scientists and lawyers in the Environmental Defense Fund is one notable example; another is the current collaboration between the M.I.T.-based Union of Concerned Scientists and a number of the leading environmental organizations in a legal challenge to the Atomic Energy Commission to establish an adequate basis for evaluating the safety systems of nuclear reactors (*28*).

Organization and Funding

Thus far there has been little funding for public interest science. Almost all who are involved do it as an unremunerative sideline. Perhaps this is good. Only recently the scientific community delegated its public responsibilities mostly to the insiders. As governmental regulatory agencies have repeatedly demonstrated, responsibility cannot be successfully delegated—it can only be shared. Large numbers of part-time outsiders are required to keep the system honest.

More than part-time people are required, however. The coordination of the efforts of part-time people and the lobbying to see that the issues they raise get a fair hearing rapidly become a full-time job. This is the function, for example, of Jeremy Stone, executive director of the Federation of American

Scientists (*29*). Under Stone's leadership the FAS has been instrumental in establishing a new tradition of open adversary hearings before the House and Senate Armed Services Committees and in providing technically competent witnesses before many other congressional committees.

Examples of full-time public interest scientists are few and far between. Ralph Lapp could be identified as such a person. Like Ralph Nader, he supports his activities by writing and lecturing on the issues with which he is currently concerned. A number of academics seem also to have become nearly full-time public interest scientists. Universities have the advantage of having undergraduate and graduate students who are willing to commit great amounts of energy and idealism to a project (*30*), although, as Ralph Nader has shown, such students will go where the action is even if it is not at a university.

Foundations are beginning to show an interest in funding public interest science projects, and the federal and state governments may begin funding them in earnest if the field becomes more respectable—like public interest law. Nevertheless, it is doubtful that direct government funding will provide the kind of political insulation appropriate to some public interest science. Responsibility for some funding should be closer to the scientific community itself. Scientific societies could do some of it. Another possibility would be for universities and other research contractors to devote part of their overhead on research contracts to a fund for public interest science controlled by the scientists at the institution. This is in effect how law firms and medical doctors support their pro bono activities.

One need only look at the student-funded Public Interest Research Groups in Minnesota and Oregon (*31*) to see how varied the possible sources of support for public interest science are. The more diverse the sources of support, the more securely established public interest science will become as one of the responsibilities of the scientific community.

Summary

We have described some of the abuses that develop when policy for technology is made behind closed doors in the executive branch of the federal government. And we have tried to demonstrate that public interest science is no more quixotic than public interest law.

References and Notes

1. Testimony before the Senate Appropriations Subcommittee on Transportation (27 August 1970), p. 1336.
2. Report by the Panel on Environmental and Sociological Impact of the President's SST ad hoc review committee, March 1969. The report and other material generated by panels and members of the review committee were introduced by Representative Sidney Yates into the *Congressional Record* (31 October 1969), pp. H10432–46. Some of this material is also reproduced as an appendix to Shurcliff's book (*3*). Congressman Reuss tells how he used the Freedom of Information Act to force the release of the reports in the *Congressional Record* (18 November 1969), p. E9733.
3. W. A. Shurcliff, *SST and Sonic Boom Handbook* (Ballantine, New York, 1970).
4. We have presented a case study of the involvement of scientists as insiders and outsiders in the SST debate [*Bull. At. Sci.* 28 (4), 24 (1972)].
5. We discuss further examples in *Appl. Spectrosc.* 25, 403 (1971), and in *The Politics of Technology: Activities and Responsibilities of Scientists in the Direction of Technology* (Stanford Workshop on Political and Social Issues, 590A Old Union, Stanford University, Stanford, California, 1970). The SWOPSI publication grew out of a student-faculty workshop at Stanford in 1969–1970 led by J. Primack, F. von Hippel, M. Perl. See also M. Perl, *Science* 173, 1211 (1971); C. Schwartz, *The Nation* 210, 747 (1970).
6. Report of L. L. Beranek, chariman, SST Community Noise Advisory Committee to W. M. Magruder, director of SST Development, Department of Transportation, 5 February 1971.
7. C. Lydon, *New York Times*, 1 March 1971, p. 15.
8. Subcommittee on Physical Effects of the Committee on SST-Sonic Boom, *Report on Physical Effects of the Sonic Boom* (National Academy of Sciences–National Research Council, Washington, D.C., 1968).
9. *New York Times*, 5 March 1968.
10. Results of government tests over a number of cities with military jets, compiled by Shurcliff (*3*), give an average of about $600 damage awards per million "man-booms" even for booms considerably less intense than those that would have accompanied the SST. If we then assume that each of 400 SST's flies 10,000 miles (1 mile = 1.6 kilometers) daily at supersonic speeds, creating a boom path 50 miles wide, populated with an average density in the United States of about 60 people per square mile, we obtain a rough estimate of $2.5 billion annual damage. Although this estimate could doubtless be made more exact, it certainly indicates that sonic boom damage is not a negligible problem.
11. *Effects of 2,4,5-T on Man and the Environment*, Hearings before the Subcommittee on Energy, Natural Resources, and the Environment of the Committee on Commerce, United States Senate, 91st Congress, 2nd Session, 7 and 15 April 1970. This history is summarized in the testimony of Surgeon General Jesse Steinfeld on pp. 178–180.
12. T. Whiteside, *Defoliation* (Ballantine, New York, 1970), p. 21.
13. A. Galston, in *Patient Earth*, J. Harte and R. Socolow, Eds. (Holt, Rinehart, & Winston, New York, 1971).
14. R. Carson, *Silent Spring* (Houghton Mifflin, Boston, 1962).
15. This example is documented in *Cyclamate Sweeteners*, and *The Safety and Effectiveness of New Drugs (Market Withdrawal of Drugs Containing Cyclamates)*. Hearing before a Subcommittee of the Committee on Government Operations, House of Representatives, 10 June 1970 and 3 May 1971, respectively.
16. *Regulation of Cyclamate Sweeteners*, Thirty-Sixth Report by the House Committee on Government Operations, 8 October 1970.
17. J. S. Turner, *The Chemical Feast* (Grossman, New York, 1970), chap. 1.
18. H. Sutton, *Saturday Review* interview, 15 August 1970.
19. The summary of the panel report is reproduced in *Underground Weapons Testing*, Hearing before the Senate Committee on Foreign Relations, 91st Congress, 1st Session, 29 September 1969. The panel report was released that same morning: *Underground Nuclear Testing*, AEC Report TID 25810, September 1969, pp. 51 ff.
20. Pitzer's address, "Affecting National Priorities for Science," before the American Chemical Society, 14 April 1969 [*Chem. Eng. News*, 21 April 1969, pp. 72–74] was partly devoted to the underground testing issue. Pitzer had just received permission from the President's science adviser to make public his personal views on the issue. For his congressional testimony, see (*19*, p. 33).
21. *ABM, MIRV, SALT, and the Nuclear Arms Race*, Hearings before the Subcommittee on Arms Control, International Law, and Organization of the Senate Committee on Foreign Relations, 91st Congress, 2nd Session, March–June, 1970. Foster's citation of the committee report appears on pp. 442–444; rebuttals by Drell and Goldberger appear on pp. 525–580. Senator Fulbright was finally able to obtain a declassified version of the report and inserted it in the *Congressional Record* along with his comments on pp. S12901 ff., 6 August 1970.
22. James Madison, letter to W. T. Barry, 4 August 1822. We thank Paul Fisher, director of the Freedom of Information Center, University of Missouri, for providing us with this reference.
23. The functions of the advisory system have been widely discussed; see, for example, T. E. Cronin and S. D. Greenberg, Eds., *The Presidential Advisory System* (Harper & Row, New York, 1969). The essay by H. Brooks, reprinted in this volume, is especially useful.
24. A short case study of the effectiveness of the Colorado Committee in this and two other cases may be found in (*5*).
25. P. Boffey, *Science* 171, 43 (1971).
26. A. H. Cahn, *Eggheads and Warheads: Scientists and the ABM*, thesis, Massachusetts Institute of Technology (1971).
27. A recent, well-publicized example of such an attack is the Operations Research Society of America report criticizing congressional testimony of several scientists against the Safeguard ABM system. [ORSA Ad Hoc Committee on Professional Standards, *Operations Res.* 19 (5), 1123 ff. (1971)]. The first part of the ORSA report purports to be a statement of ethics for operations analysts, but it provides little ethical guidance beyond urging loyalty to one's employer under almost all circumstances. The report's attack on the anti-ABM scientists focuses upon a very narrow technical issue, from the analysis of which the report then draws a broad and unjustifiable condemnation of the ABM critics. (For detailed criticism of the ORSA report, see statements of numerous technical experts collected and reprinted in the *Congressional Record*, pp. S1921–51, S2612–13, S3521–23 (17 and 29 February; 7 March 1972); and P. Doty, *Minerva*, in press.
28. R. Gillette, *Science* 176, 492 (1972).
29. The Federation of American Scientists, 203 C Street, NE, Washington, D.C., is the only registered lobby of scientists FAS has been traditionally interested in issues associated with nuclear weapons, but recently it has provided testimony before Congress on many other technological issues.
30. A good example of such a university-based program is the Stanford Workshops on Political and Social Issues. More than a hundred "workshop"-courses for academic credit at Stanford University have been sponsored by SWOPSI during its 3 years of existence, and these have produced more than a dozen comprehensive and authoritative reports on subjects like "Air pollution in the San Francisco Bay area," "Balanced transportation planning for suburban and academic communities," "Logging in urban counties," and "DOD-sponsored research at Stanford." Several of these reports have had considerable political impact [for SWOPSI's address see (*5*)].
31. For a discussion of the manner in which such groups can be organized, see R. Nader and D. Ross, *Action for a Change—A Student's Manual for Public Interest Organizing* (Grossman, New York, 1971).

The relatively recent prominence given to issues concerning the environment, notably the debate on supersonic transport, and to the so-called energy crisis reflects a growing uneasiness about technological matters among a generally acquiescent public. There no longer appears to be a broad consensus on the automatic benefits of technological development; its consequences are increasingly perceived as problematical. This new situation could affect both scientists and engineers in terms of the legitimacy accorded their work, the limits within which they may do it, and the level of resources made available for it. For even though a direct relationship between public attitudes and the way decision-makers behave is difficult to establish, the public's mood does create boundaries within which officials generally act.

This article presents findings concerning the public's attitudes toward technology and science which suggest that considerable refinement of our past generalizations is necessary. Evidence suggests that (i) the public makes a distinction in their evaluations of the outcomes of scientific work and technological work; (ii) the public's reaction to the impact of technology upon society is one of wariness and some skepticism; (iii) the public applies a rather wide range of sometimes contradictory values to its evaluation of technology; (iv) the public has a distrust of the institutions associated with decision-making in technical policy areas; and (v) a clear element of political ideology is present in the evaluations of technology made by an important segment of the public.

Only recently has there been sufficient evidence concerning potential public uneasiness about science and technology to stimulate systematic attempts to gauge prevailing opinion on these matters. Most commentaries on these attitudes have been largely impressionistic. They note that the "golden age" of science and technology has passed. They agree that the widespread conviction about the inevitable benefits to come from scientific advance (a conviction pointed to as early as 1830 by de Tocqueville as imprinted on the American genius) has been severely eroded. Edward Shils sums up the case (*1*):

> Whereas it was once believed that every new technological possibility was automatically and inevitably beneficial, the great achievements in outer space [among others] have helped to dim the light once cast by technological progress. . . . Science, engineering and technology have all become amalgamated into a single entity which is conceived as a source of damage and costly waste. The research workers, engineers, military men, industrialists, and politicians are seen as homogeneous groups with each section pursuing its own advantage at the expense of the rest of society.

This slackening in public approval has been attributed to a number of factors. Robert Morrison, for example, cites the distrust of the way power holders manipulate the world; the concern over maldistribution of resources; anxiety about the ethical implications of further technological advances in some areas of medicine and the biological sciences; and growing awareness that much scientific research lacks social relevance (*2*). The picture of the public mind presented in such commentaries is painted in tones of suspicion and guarded pessimism. Cognizant of this decline in the prestige of science, still other writers appeal for circumspection lest negative public reaction lead to "harmful restrictions on all scientific research" (*3*).

But a somewhat different picture emerges from reports of recent work done by public opinion researchers (*4*–*6*). That the scientific community, and other interested publics, have fallen victim to "quick overgeneralization and grand simplifications as to the scope, source, and direction of anti-science sentiments" (*4*) is the finding of at least two studies (*4*, *5*). These reports note that (i) most people feel that science and technology have made life better; (ii) the prestige of scientists and engineers is relatively high; and (iii) there is a high degree of confidence in the ability of science and technology to solve a wide range of social problems. The conclusion invited by such findings is that the American public is generally friendly toward the scientific community and that scientists and engineers may proceed with at least cautious optimism about the public fate of their activities.

That conclusion is predicated on the assumption that the public makes no distinction between science and technology and, further, that if the public generally is friendly toward scientists, then technologists—those who implement technological systems—need fear no animated opposition (*7*). But although a single web of logic and theory undergirds both scientific knowledge and technological implementation, our appreciation of their sociopolitical contexts is not enhanced by attributing to the public at large an implicit melding of their social effects.

Public opinion data do not speak for themselves. What they say depends upon the questions put to them. In the study reported here we sought answers to questions about the "general climate . . . for the development *and use* of scientific knowledge" and about the "choice of ends" to which they are directed (*5*, p. 96). Our findings suggest that the themes of available systematic studies as well as of the more pessimistic impressionistic accounts must be somewhat modified. They also tell us that equally misleading is the charge that those who are uneasy about or hostile toward technology are antirational or anti-intellectual. To accept this claim does nothing to assist in the discovery of what may be behind such antagonisms or to determine whether they are justifiable.

The Study Context

As part of a larger study of technology and social change, we set out in 1972 to probe public opinion on a wide range of technology-related topics. Accordingly, a survey was commissioned to gather information on the

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Table 1. Should science and technology be controlled?

Statement*	Strongly agree 1	2	Agree-disagree 3	4	Strongly disagree 5
Science					
1. Allow studies; obtain future benefits	54.2	32.1	3.8	5.9	4.0
2. Science good, use of science bad	45.9	29.0	5.4	13.5	6 6
Technology					
3. Control invention and life worsens	14.7	22.5	11.0	29.8	21.9
4. No interference with right to buy justifiable	18.1	26.8	8.3	27.1	19.6
5. Insufficient knowledge for regulation	21.4	25.1	10.8	27.4	15.3

* The full wording of the statements for agreement or disagreement were as follows: 1. Unless scientists are allowed to study things that don't appear important or beneficial now, a lot of very beneficial things probably won't ever be invented. 2. Basically all scientific discoveries are good things; it is just how some people use them that causes all the trouble. 3. Any attempt to control which inventions are widely produced or made available will make our lives worse. 4. No one should attempt to regulate which inventions are produced because it interferes with the individual's right to decide what he wants to buy. 5. No one should attempt to regulate which inventions are produced because they do not know how to do it. All data are expressed as percentages (percent across; $N = 980$). Those expressing no opinion ranged from 1 5 to 2.0 percent for statemer 1 and 2, and from 4.1 to 5.6 percent for statements 3 to 5.

perceived importance of te-hnology as a feature of social change; on criteria considered important in technology assessment; on approval or disapproval of 12 specific future technological capacities; on perceptions of technology's effects on the quality of life; and on attitudes toward scientific work as distinguished from technology. Using a multistage sampling design, we interviewed 980 adult Californians.

Since most policies with respect to science and technology are national in scope, the question of the generality of our results should be raised; for strictly speaking "the public" referred to in what follows is the California population. However, that we can have confidence in the generality of the data we collected is indicated by national estimates of demographic characteristics such as age, income, sex, race, and occupational distributions obtained from the 1970 census: these estimates deviate no more, and usually somewhat less, than 4 percent from the California profiles. On only one characteristic, education, do national averages differ significantly from California's. The percentage of Californians (31.4 percent) with at least 1 year of college education is about 35 percent greater than the national average (23.3 percent). This slight skewing of educational distribution extends to our sample as well; 47 percent had at least 1 year of college. This higher education level suggests that Californians in general and our sample in particular may be, on the average, more likely than respondents in a national sample to be informed about science and technology. Over a wide range of attitudes we found no significant difference, however, among groups with different educational attainment (*8*, *9*).

Moreover, when we compare our survey to that recently sponsored (*5*) by the National Science Foundation (NSF), several items common to both surveys show a reasonably high degree of correspondence in distributions (*10*). In sh t, evidence available from indirect inc .ators concurs that California does not deviate from the rest of the nation in important ways with regard to attitudes toward science and technology. Indeed, since the population of California is nearly one-tenth that of the entire United States and since its economy includes a large proportion of the total scientific and technological work done in this nation, our findings may have greater policy relevance than would be the case for data gleaned from any other single state or region.

Research in public opinion is beset with some formidable measurement problems. The data gathered are "opinions" and as such may be transiently held, possibly changing with time and circumstance. This may be particularly true when the attitudes examined are not central to the person interviewed; such is often the case with the data gathered here. In addition, the opinions measured may not be founded on correct factual information; thus, they can be altered by additional information from educational efforts or other sources. Nev ^-eless, if we are interested in what the public at large thinks about science and techn logy, this technique with all its limitations is the only one available.

Social Perceptions in Technology

Over the past 10 years an increasing volume of work has purported to describe some of the social effects of technology on people's lives, outlook, and values (*11*, *12*). Some observers have argued that technology has become the source of disquieting changes in the human condition and that it (and science) is running rampant, beyond control. This argument is perhaps most strongly put by Jacques Ellul in his description of the "technological phenomenon," a pervasive situation where decision-making processes are so structured as to admit of only one outcome —the rather blind, never-ending implementation of new techniques (*12*). If such misgivings were widespread they could provide a milieu in which the control of science as well as technology would be sought. But such a situation hinges on a general public belief that scientific discovery and consequent technological implementation are nearly indistinguishable aspects of a continuous process.

Table 1 presents data related to several aspects of the public's evaluation of the soc . ' .. of science and of technology. For the purpose of this survey, we have chosen to define science and technology as follows (*7*): Science is, implicitly, the activity of discovering new knowledge and includes the development of prototype inventions. Technology, on the other hand, is the activity which leads to the widespread availability of products based predominantly on such scientific knowledge. The data show that there was considerable agreement that scientific activities are intrinsically beneficial and should not be controlled, but that the use to which scientific knowledge is put can make trouble. They also demonstrate that the standard defenses of technological autonomy are rejected by a substantial fraction of those interviewed. More people disagreed that regulating technology would affect the quality of life adversely than those who believed it would. Again, more people felt that the advantages of regulating technology outweighed the benefits of a laissez-faire approach. Interestingly, the sample was almost evenly split with respect to judgments about whether or not the regulation of technology was possible. Taken together, these data imply that the public at large does not find the outcomes of scientific activity a problem. Rather it is the outcome of technological implementation that is the source of concern, thereby creating a potential both for the demand and for the expectation that those outcomes

Table 2. How disenchanted are people with technology?

Statement*	Low disenchantment		Inter-mediate	High disenchantment	
	1	2	3	4	5
1. To go back to nature desirable	32.3	24.6	8.7	22.1	12.2
2. Life too complicated	24.5	33.3	8.0	24.3	10.0
3. Overdependence on machines	9.2	12.8	5.7	34.3	38.0
4. Technology can solve problems	5.5	10.3	5.2	30.9	48.3

* The full wording of the alienation-confidence statements was as follows: 1. It would be nice if we would stop building so many machines and go back to nature. 2. Technology has made life too complicated. 3. People have become too dependent on machines. 4 People shouldn't worry about harmful effects of technology because new inventions will always come along to solve the problems. All data are expressed as percentages (percent across; $N = 980$). The numbers of people expressing no opinion ranged from 1 5 to 2.6 percent.

should be regulated. A plausible corollary to these findings, somewhat at odds with other survey research, is that if the public came to see science and technology as indistinguishable on the practical level, the very large consensus favoring unregulated scientific activity might diminish rapidly.

Alienation and Confidence

Uneasiness about technology can have a more nearly Luddite character: the belief that further techno-industrial advance will result in net social loss. Expressions of longing for a return to nature or to a more simple life unencumbered by machines typify that troubled attitude as, to a lesser extent, does reduced confidence in technology's power to solve man's problems. People most disenchanted with technology tend to accept these notions. Table 2 presents the pattern of responses to four questions probing the degree to which the "alienated" attitude they convey is held by the public. It shows opinion to be divided on the desirability of returning to a more natural state and on whether life has been made too complicated by technology. While a little over half of those questioned did not agree with those notions, a third of the sample did. Thus, although the typical notions associated with technological alienation did not predominate among our sample, they were accepted by a strong minority.

More clearly evident were attitudes expressing a limited confidence in technology. Strong majorities, over 70 percent, agreed that we had become too dependent upon machines and that it is not sensible to expect technology to develop solutions to problems caused by technological development. These relatively high percentages seem to signal deep wariness about overdependence on or overconfidence in technology as a means for dealing with social problems associated with technological development. Perhaps more significant is the fact that only 5 percent expressed no "disenchanted" sentiments, 70 percent expressed at least two, and 50 percent three or four such notions.

In a sense, the data in Tables 1 and 2 provide evidence that Ellul's vision of a populace enamored with technique and unable to resist technological development for its own sake does not hold for our sample. An undercurrent of skepticism about dependence on technology does restrain wholehearted enthusiasm about its effects, and it is likely that if such skepticism grows, so will pressures for regulating technical development.

Technology, Past Benefits, and Value Criteria

Against this background, what can be said about the public s evaluation of specific existing technological developments? Our sample was asked to indicate whether each of five such developments have made life in general better or worse. The technologies in question were highly visible ones, widely implemented and quite well known to most people: household appliances, automobiles, automated factories, the space program, and atomic weapons. These things formed a measure of respondents' overall evaluation of present technology (*13*).

Figure 1 presents the distribution of this index. It reflects a distinctly positive evaluation of present technology and is consistent with the results of the NSF survey (*5*). The data, therefore, show positive public response to past and present technological development, overlaid with a set of concerns about the more general consequences of that development. This combination of attitudes appears to reflect a tension in values, visible in the priorities held by the public which determine whether a technological development is "advantageous."

Respondents were asked to rank a number of social values—ranging from highly utilitarian values to more humanistic and egalitarian concerns—and to indicate the importance they should be given in evaluating technology's impact. Not unexpectedly there was no strong consensus on what values should be given priority. Yet a relatively high degree of support was expressed for a wider range of priorities than simply the economic values of employment and taxes which are often presented as the basis for decisions on technology-related public policy. Table 3 presents the percentages of respondents indicating what values were considered "extremely" important, as well as the average rank accorded them by the whole sample.

Not surprisingly, the impact of technological development on employment was ranked as the most important consideration, though pollution effects drew the highest percentage of "extremely important" designations. Perhaps the most interesting result is that four of the seven values were believed to be extremely important by a majority of the sample. That the public considers a wide-ranging combination of values to be important criteria for evaluating the consequences of technical development complicates both the activities of technologists and the task of policymakers, for some of these values seem clearly to be in tension. (Notably, neither the importance of the U.S. image abroad nor leisure time struck a particularly responsive chord in the public.)

Thus our data show that a plurality of the public seems to approve of the regulation of technology, that many more desire a wide range of values to be taken into account in its implementation, and that in varying degrees an uneasiness about the social consequences of this implementation is present. Now we ask what level of confidence our public expressed in the technology-related decisions made by its institutions of governance. The degree to which it regards those engaged in decision-making as legitimate provides an approximate answer.

Technology and Decision-Makers

Six situations in which decisions are made about how to implement a particular technology were set before respondents (*14*). The respondents were

Table 3. What are the important values to be considered in the implementation of technology?

Goal	No. who considered goal of "extreme" importance (%)	Mean ranking	Standard deviation	N
To increase employment	60.6	3 00	1.55	933
To reduce pollution	72.3	3.16	1.74	929
To make life enjoyable	47.0	3.33	1.99	929
To reduce taxes	56.3	3.71	1.91	933
To improve the lot of poor people	59.7	3.76	1.69	929
To improve the U S. image abroad	32.6	5.05	1.71	931
To increase leisure time	17.8	5 96	1.41	929

Table 4. Attitudes and characteristics of the "potential public" for technological politics.

Index	Matrix of association (Pearson's r)							
	1	2	3	4	5	6	7	8
1. Evaluation of technology								
2. Confidence in technology	.302							
3. Alienation from technology	−.402	−.349						
4. Effect on standard of living	.273	279	−.255					
5. Public under-representation	−.311	−.229	.207	*				
6. Party/ideology	−.348	−.256	.358	*	.328			
7. Age	.211	.270	−.289	*	*	−.303		
8. Pollution rank	*	*	−.234	*	*	*	*	
9. Regulate technology	*	*	.300	*	*	*	*	*

* Correlation coefficients below ± 2 and not significant.

then asked to indicate which of eight actors or institutions would actually have the most (and the least) say in making each kind of decision (*15*). In addition, our respondents were asked to indicate who ought to have the most (and least) say in the same decisions. Estimates were then made of the degree to which the respondents felt that those actors whom they saw as actually making the decisions in these various technical areas were, in their opinion, really entitled to do so. Similarly, the degree to which respondents saw illegitimate involvement in decision processes can be estimated.

The specific results varied somewhat from one decision area to another, but several consistent patterns emerged. (i) Technical experts rated highly; they were seen as exercising legitimately a great deal of influence over decisions in each of the technical areas. (ii) Top government leaders drew considerably less support. Those interviewed perceived government leaders to be involved in all six areas, but in only two, space travel and military uses of space, was their presence seen as warranted. (iii) Business leaders received little or no confidence from our sample. While they were perceived to be influential in four of the six areas, they were not welcomed in any of them. (iv) The public saw itself as the "actor" most entitled to be involved in all decision areas in question. At the same time it saw itself as accorded least access to them—again in all six areas.

These data are consistent with a number of recent findings. Certain Harris Poll results have shown that the public places "a great deal of confidence" in scientists and engineers; the NSF-sponsored study (*5*) indicates that a substantial minority feels that "the degree of control which society has over technology should be increased." And many polls show a significant increase in the public's distrust of all public and private institutions. Apparently the institutions established to represent the values which people want used as criteria in decisions to be made about technology's use have not kept up public confidence. At the same time, technical experts, scientists, and engineers, have been able to maintain it, at least until now, even in the face of apparently substantial mistrust of the technological decision-making processes themselves.

This public confidence seems a signal accomplishment for the scientific and technological communities. It may rest on the public's perception of the technical expert's role as a man of knowledge; he is viewed as competent. Similarly, people's distrust of business and government could be a reaction to what they perceive as the inability of these groups to get things done correctly; what they consider failure on the part of businessmen and politicians to meet public commitments they may attribute simply to incompetence.

An alternative explanation can be found in the distinctions noted by Herbert Simon between factual and valuational premises as components of decision-making (*16*). The ability to render a competent decision requires factual knowledge. A person's knowledge about a decision situation legitimizes his involvement in it; hence, as we have just noted, the trusted stature of technical experts in the public's mind. But valuational elements also are an integral part of any decision process. Advocating certain social values, political and business leaders claim the right to participate in decisions on technological issues. In so doing—in setting goals and establishing priorities—they are expected to reflect the public's value interests; otherwise, they lose that right and their involvement in technological decision-making will begin to be considered invalid. That those interviewed in our survey evinced just such a mistrust of business leaders and government officials opens doubt that these decision-makers were really representing the public's value preferences. At the same time, the public clearly accorded itself legitimacy to participate

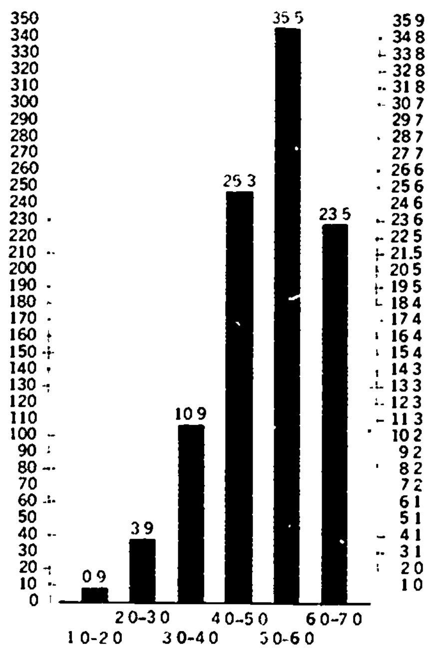

Fig. 1. Frequency distribution of Guttman index of evaluations of implemented technologies (mean, 4.846; variance, 2 100, standard deviation, 1.449).

in decisions on technological matters while feeling far removed from any access to the decision process.

These findings have direct implications for scientists and engineers: (i) As opposing value preferences continue to compete in the decision process, the scientific and technological communities will almost inevitably be drawn deeply into political controversy. Technical experts could be pressed to represent social values as well as to provide factual information for policy decisions (*17*). (ii) That members of the public are seriously disquieted about the existing decision processes related to technological development could result in strong pressure for its public control. (iii) Should that occur, and should the public begin to link scientific discovery determinantly to the negative effects of technology, the relative autonomy of science could diminish.

Technology and the Potential Public for Political Action

Whatever the public's attitudes, they are not likely to become the basis for public policy unless crystallized into articulate demands for change. Efforts to voice demands, to organize pressure for or against policies and political candidates come only from those portions of the general population motivated to action. Those people most likely to become involved in activities calculated to prompt policy action on technology-related matters we shall call here the "potential public" for technological politics (*18*).

Certain aspects of social life seem a priori to make people aware of and interested in policy for science or technology. More highly educated people, people who have voted in past elections, and people who hold jobs closely involved with some type of technology are likely to number disproportionately among the citizenry concerned with such policy. To the extent that the public enters into controversies involving technology, participants and leaders in the debate are likely to come from the segment so described. To the extent that decision-makers monitor public attitudes, they will feel the views of this potential public disproportionately. How then did our respondents feel about the social effects of technological development?

Using the factors noted above, we developed a scale by means of which respondents scoring on its upper half were designated the potential public. Thirty-one percent of the sample (303 respondents) fell into this group. This number represents a fairly substantial proportion of our total sample, probably higher than the putative national figure, because of the higher education level of Californians. Comparison of the potential public with the remainder of the sample showed that the only major differences in demographic and political characteristics were that the potential public was somewhat younger, made several thousand dollars more per year, and on the average had 2 more years of education (about 2 years of college). While the potential public was a bit more "pro-technology" with respect to the variables reported above, the differences were too small to be substantively significant (*19*). In short, the potential public for technological politics is generally similar to the rest of our sample over a wide range of opinion.

A very interesting difference between the potential public and the rest of the sample, however, is the degree to which their attitudes are interrelated. For those not included in the potential public, most attitudes appear to be haphazardly organized. That is, they display no consistent pattern of internally coordinated opinion. But the potential public does exhibit a patterned and cohesive set of attitudes toward the outcomes of scientific work and toward technological activities. While we do not wish to suggest that the attitudes of the larger group are unimportant, its relatively random responses do indicate that it is not likely to be a source of much criticism. The issue area apparently lacks salience for these people. They are therefore likely to be acquiescent to policies governing technology, unless of course they are personally confronted with visible outcomes of such policies or lack of such policies as was the case for gas station owners, truckdrivers, and others during the recent fuel distribution emergency.

Nine indices were used to analyze relationships among this potential public's attitudes: (i) a technology evaluation index, as described in Fig. 1; (ii) an index of confidence in technology, composed of the last two items in Table 2; (iii) an index of technological alienation, in which we used the first two items in Table 2; (iv) an index of an effect on standard of living, indicating the degree to which it was believed that there would be "a decline in the standard of living if there were less technological development"; (v) an underrepresentation index, summarizing the degree of perceived illegitimate exclusion of the public in the decision-making process for three forms of public technology—rapid transit, military technology, and space exploration; (vi) pollution rank, indicating the importance placed on environmental concerns compared with other criteria; (vii) an index of technology regulation, in which we used the last three items in Table 1; (viii) age; and, (ix) a six-point scale combining party and ideological identification ranging from "liberal Democrat" to "conservative Republican" (*20*).

Our primary interests here are those attitudes toward technology which fall into three areas of opinion: (i) attitudes associated with evaluations about specific benefits of present technology; (ii) attitudes associated with confidence or lack of it about depending on technology to solve social problems; and, (iii) attitudes related to a feeling of disenchantment with, or alienation from, some of the general conditions prompted by technology. Such opinions would indicate how the potential public sees specific uses of technology for the near future and what its feelings are about the longer term, broader consequences of technological development.

The data show that the potential public, like the entire sample, was generally positive about the benefits of present technological development. over 65 percent indicated that these developments had been appreciably beneficial, while only 16 percent believed that they had not been. There was much less confidence in the idea that our depending on technology as a solution to present problems is sensible: only slightly over one-third (35 percent) felt quite sure that it is sensible, while almost half (49 percent) felt that it is not. Finally, while the feelings of the potential public did not extend to widespread alienation by the more general conditions prompted by complex technologies, 45 percent reported some sense of alienation.

The first three indices display a consistent set of relations. Table 4 shows that those who regarded present technology as beneficial also tended to express confidence in technology and to hold fewer alienated attitudes. Similarly, those who expressed confidence in the efficacy of technology also expressed less disaffection. Each of these indices had other correlates. Those people who positively evaluated present technologies also tended to believe that technology is necessary for maintaining our stan-

Table 5. Regression coefficients from the "potential public," calculated for primary attitudes and other factors.

Index	Evaluation	Confidence	Alienation	Living standard	Representation	Party	Age	Pollution	Regulate	R
Evaluation of technology		.11	−.20	.15	−.16	−.17	*	*	*	.53
Confidence in technology	.11		−.24	.17	−.10	*	.15	*	.17	.49
Alienation from technology	−.18	−.21		−.11	*	.18	*	−.15	.24	.60

* Not significant at $P < .05$. The standard error in all instances ranged from .05 to .06, for regression. $N = 262$

dard of living ($r = .273$) and to be less inclined to feel that the public is underrepresented in decisions about government-supported technologies ($r = -.311$). People who gave positive evaluations were, notably, somewhat older ($r = .211$) than those who did not and, probably associated with this age factor, they were relatively conservative politically. The intervening variable of ideology correlates ($r = -.348$) with the positive evaluations. The degree to which our respondents were confident or dubious about depending on technology for solving problems displayed a similar set of associations. For this variable, however, we observe a somewhat stronger relationship with age and a bit less pronounced association with political ideology.

The more general attitudes which we have summarized as a feeling of "alienation"—attraction to the idea of a less complicated and more natural world—were associated with the greatest number of other attitudes. Those who tended to express a disaffection toward technology also tended to put a lower evaluation on the benefits of technological development and to have less confidence in technology as a problem solver. They were also more skeptical about the necessity of technological development for the sake of maintaining present standards of living ($r = -.255$) and were concerned about public representativeness in technological decision-making ($r = -.207$). In addition, their alienation was related to the conviction that the effects of pollution should be more taken into account whenever technological decisions are being made ($r = -.300$) and, perhaps more significantly, to an increasing propensity to consider seriously the need for regulating technology ($r = -.234$). Those tending toward feelings of alienation were relatively young ($r = -.289$). This age factor was probably associated with their partisan and ideological persuasions for they were also preponderantly Democratic and liberal ($r = -.358$). Thus in the potential public a number of attitudes based on judgments about the relationship of technology to economic well-being, on concerns for the environment and for democratic decision-making, and on approval of regulation of technology were consistently related to a more generalized condition of technological dissent.

To complete our analysis, regression coefficients were calculated for the primary factors to determine the proportion of variance explained by the set of attitudes discussed above (see Table 5). Some of the associations considered in Table 4 proved to be dependent upon an intervening variable. Nevertheless, age, political differences, dissatisfaction with decision-making, and value judgments remain important predictors of attitudes toward technology (*21*).

Summary

Our analysis of the interviews with a sample of the California public about a range of their attitudes toward technology shows that a modification of our understanding of the collective state of mind on this subject is in order. The current assessment of the public as largely, and somewhat vacantly, enamored with science and technology does not hold. Nor does a picture of a public generally hostile and alienated by technology. Neither panglossian optimism nor prophecies of doom can be supported by these interviews. Rather a more mixed picture emerges. Out of that picture, a potential public can be isolated, whose mood it behooves science policy-makers to watch. This group tends to associate a number of related conditions with technological development; moreover, it is likely to make assessments on those relationships so perceived.

To the degree this group has "antitechnological" feelings, these feelings are clearly linked to the group's awareness that the social consequences of technology can produce conditions which threaten important values. The particular distribution of age and political identification suggests that those who are young and who identify themselves as "liberal" form the core of potential opposition to technological development and that such opposition is at least in part a function of different value preferences. The associations between political identification and attitudes about technology, distrust of decision-making, and concern for environmental impacts all make this point. In short, "technological dissent" cannot be written off as anti-intellectual and without foundation. It is, in fact, preeminently sensible.

What the alignments visible within the potential public portend for the future is not clear, although they do not allow us to accept an inference drawn from past studies—that because the young retain confidence in scientists and engineers all is well for the general climate of science and technology. We can only speculate whether, as these younger people grow older, they will carry their uneasiness about technology with them. Were they to do so, and were this group to be joined by still younger people who also hold these wary attitudes, the context of scientific and technological work could become much more fraught with political controversy. Another point emerging from our interpretation is how very crucial to continued free scientific inquiry is the distinction between scientific work and technological activities apparently now made by a sizable portion of the public. Should this distinction become lost, perhaps through continual merging of science's role with technology's by the popular press, attitudes now mainly associated with technology could spill over to scientific research as well.

Yet our data also provide evidence of the successes of the scientific and technological communities. They have become such a critical part of life that people are seriously concerned with their future development. The opportunity is present for both communities to find ways of responding to the situation so that thoughtful action can be taken to implement technology for the benefit of the commonweal.

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3. P. Abelson, *ibid.* **173**, 285 (1971).
4. A. Etzioni and C. Nunn, *ibid.* **181**, 1123 (1973). See also *Science Indicators* (5, pp. 96–100).
5. National Science Foundation, *Science Indicators* (Government Printing Office, Washington, D.C., 1973).
6. For a review of other studies relating to pu blic attitudes toward technology, see G. R. Funkhouser, *Public Understanding of Science: The Data We Have*, Workshops on Goals and Methods of Assessing the Public's Understanding of Science (Materials Research Laboratory, Pennsylvania State Univ., 1972); and I. Taviss, *Technol. Culture* **13**, 606 (1972).
7. Our conceptions of science and technology include both the definition of their activities and the people who are mainly engaged in carrying them out. The definitions in both cases are familiar; see J. K. Fiebleman, *Technology, Culture* **2**, 305 (1961); and C. Mitcham and R. Mackey, Eds., *Philosophy and Technology* (Free Press, New York, 1972). The people who animate science are, of course, scientists Technology is carried out by engineers, architects, physicians, and technical experts of many kinds.
8. Other evidence bearing on the question of educational level and opinion response is mixed. Devine (9) reports no systematic differences between high and low educational groups on a range of policy questions, while others do find some differences of opinion [see, for example, S. Verba and N. Nie, *Participation in America* (Harper and Row, New York 1972)].
9. D. Devine, *The Attentive Public* (Rand McNally, Chicago, 1970).
10. In particular, *Science Indicators* (5) contains four questions that were designed to probe dimensions which we also examined in our California study: (i) the relative prestige and confidence adhering to a number of professions including business people, scientists, engineers, and national legislators, (ii) the need for increased social control of science and technology; (iii) the benefits of a number of technical capabilities including space exploration, military technology, health care, and mass rapid transit; and (iv) evaluation of present technologies On the first three variables, both studies uncovered similar attitudes. Small differences are evident, but these could be attributable to method variance or sample error. On the fourth variable, much the same concordance is observable, though the California sample is somewhat less favorable to present technology than is the national sample. But, again, the differences could be due to measurement method: Our study used a scale of questions to measure the respondent's evaluation, while the NSF study simply asked the subject, "Have science and technology changed life for the better or for the worse?" Interestingly, however, if we cross-tabulate response on this variable with such demographic characteristics as race, education, sex, income, and age we find the same general patterns emerging in both studies.
11. See especially Ellul (12); H. L. Neiburg, *In the Name of Science* (Quadrangle Books, Chicago, 1966); V. Ferkiss, *Technological Man: The Myth and the Reality* (Braziller, New York, 1969); L. Mumford, *The Myth of the Machine*, vol. 1, *Techniques and Human Development* (Harcourt Brace, New York, 1967); W. Sypher, *Literature and Technology: The Alien Vision* (Random House, New York, 1968). See also, J. D. Douglas, Ed., *The Technological Threat* (Prentice-Hall, Englewood Cliffs, N.J., 1971); M. Brown, Ed., *The New Technology and Human Values* (Wadsworth, Belmont, Calif., 1966); P. Goodman, *The New Reformation: Notes of a Neolithic Conservative* (Random House, New York, 1970); and L. Winner, *Public Policy* **20**, 35 (1972).
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13. While the choice of these technologies was arbitrary we feel that they reflect the variance of opinion concerning technologies and that they are suggestive of the broad notions of technology seen as capability. These presuppositions were supported by the fact that the five items form a well-defined Guttman scale and when factor-analyzed they loaded strongly on a single factor. The scale had a coefficient of reproducibility 0.93, Menzel's coefficient of scalability is 0.67.
14. These included decisions on the regulation of energy consumption, mass public transportation, genetic engineering, data banks, and civilian and military uses of outer space
15. These were congressmen, executive branch officials, the courts, consumer groups, business leaders, technical experts, the public in general, and no one.
16. H. Simon, *Administrative Behavior* (Free Press, New York, 1957), pp. 15–60. See also J. Thompson, *Organizations in Action* (McGraw-Hill, New York, 1967), pp. 134–149.
17. While this could conceivably occur we do not suggest that technical experts would be more able than any other group to do so with extraordinary effectiveness.
18. Our reasoning closely follows the arguments of those who distinguish between the generally uninvolved public and those who, by virtue of their education or personal association with issues, or both, are aware of them and hence likely to be motivated to act on them. See P. Converse, in *Ideology and Discontent*, D. Apter, Ed. (Free Press, New York, 1964); G. Almond, *The American People and Foreign Policy* (Praeger, New York, 1960); and Devine (9).
19. Even an *F*-test was just barely statistically significant at $P = .05$ for many of these variables.
20. The items for each index constructed loaded strongly on only one factor when the principal components solution was rotated to a varimax solution.
21. The most important cases were the relationship of age and party or ideology to technology evaluation and alienation on the one hand and to confidence in technology on the other. In the former case, the relationship has party or ideology intervening between age and the attitude in question. In that instance age does not have an independent effect; it disappears when party and ideology is controlled for. In the latter case, age has an independent effect which remains even after controlling for party and ideology. No attempt was made to develop a causal model because of the likelihood that the three major attitudinal variables are reciprocally related, a condition which would make any causal model underidentified.
22. This project was supported by the Ames Research Center, NASA (grant 05-003-0471). The scope of the survey was broadened by additional assistance from the International Technology Assessment Program, Institute of International Studies, University of California, Berkeley The staff of the Institute of Governmental Studies assisted ably in this and several earlier studies related to the survey. We gratefully acknowledge the comments of Kai N Lee and Mary Fenneman on earlier versions

Scientists, Engineers, and Citizens

Edward Wenk, Jr.

Editorial, 16 November 1979

Meeting needs for public understanding of science was adopted as federal policy in 1950 legislation creating the National Science Foundation. On the basis of currently low levels of citizen comprehension, that original NSF program can hardly be viewed as successful. But how should we expect any federal program to buck a powerful cultural tide?

Huizinga addressed this issue almost half a century ago (1) As he wrote, universal education and modern publicity, instead of raising the level of culture, have produced symptoms of devitalization and degeneration. Science as new knowledge has not settled into the culture, and the aggregate of discoveries cannot be equated to culture.

Among knowledge consumers, with everyone getting a taste of everything, there is depreciation of critical judgment. And that process has been accelerated by techniques of mass entertainment wherein participation slides from active to passive mode, speeding abdication of informed judgment to others. Everyone becomes a trivial, Monday morning quarterback.

As to the scientific community, it has considered it primary role as one of acquiring and extending knowledge, leaving to others the roles both of educating the public about the social implications of science and of exercising responsibility over ways and means for adapting and controlling natural forces The scientific community may also share in the blame for weakening of an intellectual conscience that underpins critical discernment.

Granted, various groups of scientists have become crusaders for specific issues. A few have tackled broader questions of survival. While not identified with causes as such, other scientists have become activists in their own community, applying their expertise to local issues.

Professional organizations also now get involved Journals have carried articles on key policy issues to enlighten members. Some organizations have confirmed their tax-exempt status, then summoned up their courage and taken public positions, submitted testimony, and stepped up attention to professional activities in which ethical dimensions of social responsibilities are at stake. The concepts of technology assessment have begun to be integrated into professional engineering practice and teaching.

Looking ahead, the scientific and engineering communities could be of more direct assistance through heavier commitment of their professional societies to public interest activities and to citizen understanding. At present, organizations of scientists and engineers devote the greatest fraction of their income from dues to dissemination of technical information. The public is never excluded, but the content and style of such communication are so highly specialized as to discourage participation by any but the expert.

Thus the scientific community and the engineering professions have failed to help the other 98 percent of the population who are nonspecialists to grasp the technical foundations of modern life and associated threats to survival. Some of the difficulty arises from cultural isolation of the scientific and engineering communities. One antidote lies in a more systematic exposure to issues that concern society generally, especially regarding those whose lives seldom intersect the technical aristocracy, and whose consequently remote concerns and dreams are alien and heard vicariously, if at all. When the technical community recognizes that it must address the stark questions of who wins, who loses, and how much, then they may also recognize that the attack on these questions of cultural and psychological as well as operational effects involves a kaleidoscopic blend of technical with social knowledge. This surely will widen the perspective and enrich the value base intrinsically present in all judgments that the technical community is called upon to make on technology-intensive public policy *(2)*.

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1 J Huizinga, *In the Shadow of Tomorrow* (Norton, New York, 1936), p 79

2 This editorial is excerpted from E Wenk, Jr., *Margins for Survival* (Pergamon, New York, 1979), pp. 144–147

Public Doubts About Science

[illegible]

[illegible] June 1980

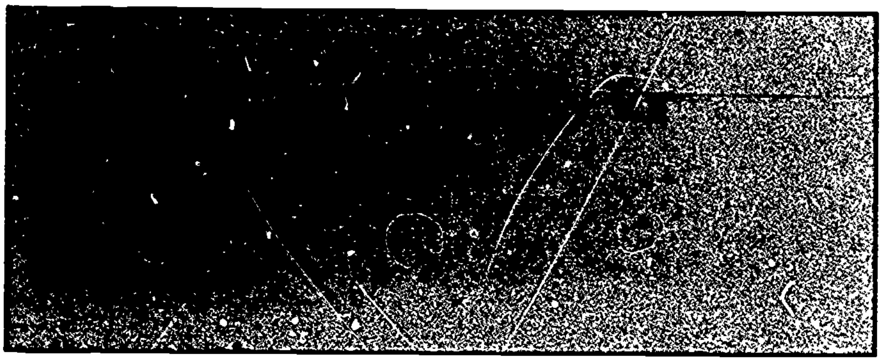
Important to the future of science and technology is the fact that the public has somewhat lost confidence in the ultimate value of the scientific endeavor. It is not that they hold pure science or scientists in any less esteem. But they are less certain that scientific research will inevitably yield public benefit.

For the first time in centuries, there are thoughtful persons who are not morally certain that even our greatest achievements do, indeed, constitute progress. To some philosophers it is no longer clear that objective knowledge is an unquestioned good. Glimpses of such doubts have emerged in public discussions of nuclear energy, or sociobiology, and, most recently, in the heated but foolish discussions of research utilizing recombinant DNA. The intellectual elite in every era has always been pessimistic. But today, concerned that "that which can be done, will be done," there has arisen an antiscientific, antirationalistic trend that should give us pause. At its ugliest—or most absurd—it finds expression in gurus, tarot cards, and astrology, faddist approaches to nutrition, and easy assertion and acceptance of unfounded allegations of environmental hazard. That antiscience attitude perniciously infiltrates the news media, affecting the intelligentsia and decision-makers alike. It must be confronted at every opportunity.

The public image of science and scientists has been distorted by the participation of scientists in public policy formation. Beneath the surface the environmental and consumer movements may be an expression of anomie, a cry of protest for the sense of powerlessness of the individual educated citizen—patently a serious sociopolitical circumstance. However, a frequent surrogate for that deep-seated complaint is an expression of concern about the safety of some product or technology, based always on an assertion of risk that was first brought forward by some member of the scientific community. The societal response has been attempts at examination of such matters by risk and cost/benefit analysis. Well, risk/benefit analysis can certainly inform the decision-maker. But his decision must necessarily still turn on a value judgment, conditioned by his social, economic, philosophic, and religious views. But that is the nature of the political process. The public acceptability of a given level of risk is a political, not a scientific, question.

Difficulty arises in the scientific community from confusion of the role of scientist qua scientist with that of scientist as citizen, confusion of the ethical code of the scientist with the obligation of the citizen, blurring the distinction between intrinsically scientific and intrinsically political questions. When scientists fail to recognize these boundaries, their own ideological beliefs, usually unspoken, easily becloud seemingly scientific debate.

A decade ago it might have been desirable to flag potential hazards for public attention and proceed as if each were a clear and present danger. It is time to return to the ethics and norms of science so that the political process may go on with greater confidence. The public may wonder why we do not already know that which appears vital to decison—but science will retain its place in public esteem only if we steadfastly admit the magnitude of our uncertainties and then assert the need for further research. And we shall lose that place if we dissemble or if we argue as if all necessary information and understanding were in hand. Scientists best serve public policy by living within the ethics of science, not those of politics. If the scientific community will not unfrock the charlatans, the public will not discern the difference—and science and the nation will suffer. There is, in short, a large burden on the scientific community to be seen as constructive in dealing with real problems, as straightforward, forthcoming, honest, and courageous—not intimidated, as all too many have been for the last decade.—*Excerpted from a speech on "Science and the American Future," given at Duke University, Durham, North Carolina, on 6 March 1980.*



One of the more vexing questions confronting scientists whose research may have a direct impact on the lives of the public has to do with the pressures to release observations and conclusions prematurely. One example is the question of early dissemination of information bearing on the likelihood of a major earthquake.

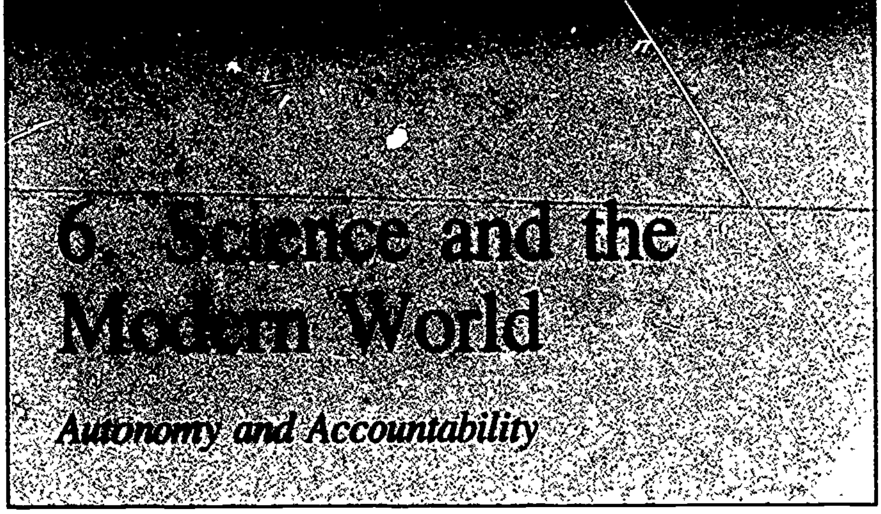
Geophysicists generally agree that results of earthquake prediction studies to date, although promising, do not indicate early arrival at the goal of providing accurate predictions of the time, place, and magnitude of large earthquakes. It is possible, from retrospective looks at phenomena immediately preceding some large earthquakes and from a few accurately predicted events such as the 1975 Haicheng earthquake in northeast China, to see certain premonitory patterns However, when confronted by one or more apparent anomalies in geophysical observations in earthquake-prone areas, what is the responsible course of action for the scientists involved?

There have been too few repetitions of the precursor-earthquake relationship to allow any realistic estimates of the probability of an earthquake's occurrence. Therefore, attempts to inform the public of the scientists' concern over potentially precursory changes must be so hedged with qualifications that public officials would have no basis for taking any but routine precautionary measures, such as leaving fire trucks outside fire stations. Estimates of the time and magnitude of the possible event would probably be too uncertain to call for such measures as evacuation of unsafe structures and shutdown of nuclear reactors. However, between these extremes there is still a range of precautions that could be taken, although at some expense and with some loss of normal public services. If a severe earthquake following a vague warning does occur with injury and loss of life, does the failure of public officials to act render them liable for civil suits? If, on the other hand, the earthquake does not take place, are the costs of having taken routine precautions likely to reflect on the competence of the officials? Indirect costs to individuals, such as loss in property value, might also make for civil liability.

In our litigious society, little allowance is made for normal human errors if they have a substantial effect on someone's pocketbook or health. A course of action might be to maintain that earthquake prediction is still a subject for research and that no results, at present, warrant short-term or public warnings. A critical, objective look at the assembly of precursory phenomena collected up to now might lead most scientists to take this stance, in view of the potential for litigation and loss of public confidence to ensue from erroneous public predictions.

A different position has been taken in the past. In 1979 the U.S. Geological Survey issued a press release noting anomalous variations in radon emission and strain accumulation in southern California. It was hoped that the release would spur prudent homeowners to prepare for a possible large earthquake. A factual account of the observations and their significance, or lack thereof, is the only way to dispel the rumors that flourish in the absence of official statements. Thus, discussion of the observations has appeared to be the soundest policy despite uncertainty about the public's reaction to the expressions of concern accompanying the discussion.

As our understanding of precursory phenomena improves, there will undoubtedly be warnings that are more specific and hence likely to occasion a serious response. Unless allowances are made for the possibility of an erroneous or ill-timed warning, few scientists will be willing to risk the opprobrium and litigation that could arise from open discussion. Thus, there is a conflict between the public's need for information and the scientists' need to be protected against threats to their livelihood. Unless some legal protection is forthcoming, the matter will most likely be resolved by the scientists retreating to an uninformative conservatism when asked to interpret their results in the public forum.



6. Science and the Modern World

Autonomy and Accountability

Debate over the social responsibilities of academics and their institutions often mirrors changing social conditions. In recent years, discussions have focused on appropriate responses to the shifting global economy and political tensions between the United States and the Soviet Union. Should faculty members direct their efforts toward research designed to provide a competitive advantage for U.S. interests? Should universities collaborate with industrial sponsors to stimulate more productive and innovative technologies? When science becomes a source of national and commercial advantage, how can the principles of autonomy and independence that have traditionally characterized the work of scientists and their institutions best be protected?

This chapter examines sources of major social tensions and opportunities for adapting science to the challenges of the modern world Scientists are cautioned, in an editorial by J.R. Pierce, to be careful in seeking government funds to support their research goals. Dorothy Nelkin's article illustrates how changes in the public status of science affect the control of research data and other forms of intellectual property by analyzing selected legal and administrative disputes involving access to scientific information.

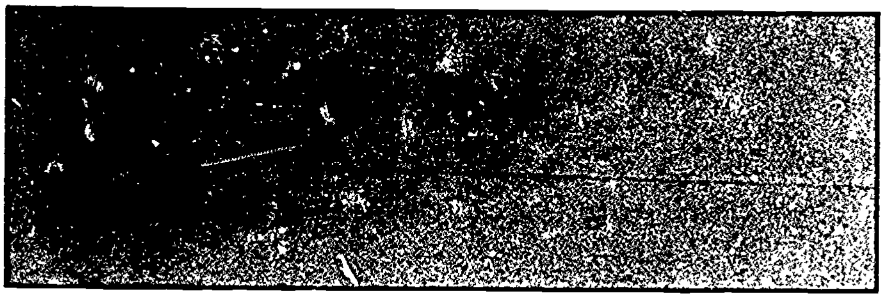
Emerging relationships between universities and industrial sponsors offer another subject for debate A Bartlett Giamatti, Robert D. Varrin and Diane S. Kukich, Philip H. Abelson, and Robert L. Sproull all highlight noteworthy initiatives, but their enthusiasm is often tempered with concern about potential conflicts between the world of academia and the world of commerce. Industry is advised to respect and support values that have long characterized basic research — openness, sharing of research data, equipment, and personnel, and the right to publish and disseminate findings without artificial constraints or barriers At the same time, universities are warned to avoid entrepreneurial actions that could jeopardize their scholarly integrity.

In addition to these concerns, leaders of the American scientific community urged their colleagues to turn attention to global problems that had been long neglected. An article by David A. Hamburg encourages scientists to apply the tremendous resources of science and technology to the social problems presented by the prevalence of totalitarian governments and the proliferation of nuclear weapons. Gerard Piel, in an editorial, again underscores the need to protect openness in science. "without publication, science is dead."

Problems less global in nature but nonetheless indicative of the social tensions permeating through science and technology in recent times are discussed in Ronald L. Numbers's article on creationism in twentieth century America and in Brian G. Zack's editorial on abortion. Each provides a glimpse of the conflicts that surface when scientific expertise enters spheres once dominated by religious authority. We see again a search for balance and new efforts to define an appropriate relationship between moral principles and scientific facts.

Another aspect of tensions in modern science is evident in recent concerns about the impact of fraud and misconduct in science. As scientists are caught up in competitive pressures and research supervision becomes lax, the incentive to cheat and cut corners becomes tempting. Can the scientific community effectively deal with misconduct by its members? Are tighter governmental controls necessary to ensure the integrity of research? This dark side of science is examined in the following chapter, as authors consider the need for institutional reforms in light of reports of deception by scientists

—*RC*



Scientists include a few inspired and dedicated individuals, some self-deluded charlatans who overestimate what they know and can do, and a very large number of intelligent, well-taught, skilled, and talented men and women who are capable of contributing substantially to man's knowledge, capabilities, and welfare.

Scientists have friends in government who want to see them well nourished and well employed. Scientists also face serious hazards and temptations. Scientists are assailed by people who believe that ignorant and intense talk, concern, love, hate, and social and political action bring human betterment rather than stultifying and unprofitable conformity. Scientists are tempted by others who want to give them huge sums of money toward ends that scientists have neither the knowledge to reach nor highly promising avenues of approach.

Mammon worship holds that money can buy anything. Organization worship holds that organization can accomplish anything. Money can buy many scientists, but it cannot always buy results. Organizations can consume scientists, but they do not necessarily produce results.

With money and organization, the atom bomb was made quickly, because scientists already knew how to go about making it. Money and organization put men on the moon, because scientists know how to get there. Money and organization have not produced fusion power or a cure for cancer because scientists do not yet know how to attain these goals. Scientists do know that the achievement of these goals will require basic understanding that they do not yet possess. They do have fruitful ideas for research. They cannot predict when research will give them the knowledge necessary to attain the goals, but they will know when research has produced adequate basic knowledge to make the goals attainable.

In the end, most scientists will do whatever there is money for doing. Scientists know, or should know, which socially and economically useful goals are within reach and which have a good chance of accomplishment through promising research. Yet, in their personal and collective actions, scientists often seem more concerned with the total number of dollars, with the public image of science, and with the cry for certain specific results than with the sensible selection and vigorous pursuit of fruitful areas of research and application. It will be a sad thing for scientists if they fail to choose wisely and act energetically toward valuable and attainable goals—for, if they do not choose what they shall do, others will choose for them.

Abortion and the Limitations of Science

[illegible] Zack

Editorial, 17 July 1981

The Congress of the United States has asked medical science to tell it when human life begins. The very asking of the question by a legislative body, the directing of the question to the field of inquiry known as science, and the answering of the question by scientists indicate a misunderstanding of the appropriate roles and relationships of science and jurisprudence.

Science is only one of a number of valid fields of inquiry, and it must not take on itself the responsibility for providing answers to questions outside its proper realm. Science deals with the prediction and explanation of events in the physical (including biological) world. Far from dealing with absolutes of truth and right, it attempts to construct a hypothetical model of reality which reflects as closely as possible the world perceived by our senses and, when our senses are insufficiently precise, by our instruments.

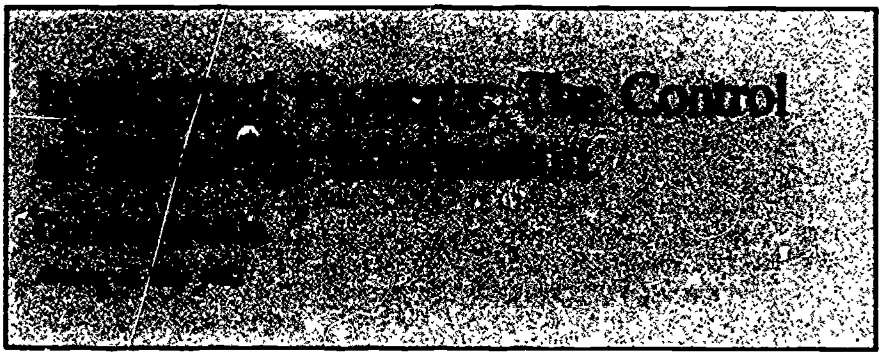
Life, in a scientific sense, is a hypothetical construct which is valid only to the extent that it aids in accurately conceptualizing the biological world. It is a powerful concept precisely because it has performed that function so well. But life, to the scientist, is not an elemental quality, as were earth, fire, air, and water to the ancients. It is a state of being, a matter of definition, and the line between life and nonlife is not always drawn easily. Is the smallest known virus particle alive?

This same discussion can be applied to any scientific conceptualization or definition, including the definition of human. The scientist, as a model builder of perceived reality, is justified in defining life, and in defining human, and in concluding that within this scientific conceptual model the fertilized egg of a human being is in itself a human life.

Jurisprudence, as a field of scholarship, and legislative action, as one of its practical applications, are concerned with very different sorts of inquiry. Broadly speaking, the law has as its purpose the establishment of a code of conduct to govern the actions of the members of a community in order (at least in our society) to enable them to best live together in harmony. One of the most basic functions of the law must then be to identify those actions which are abhorrent to the community and outlaw them. Thus murder, considered abhorrent by most members of most communities, is widely regarded by the law as the most serious of crimes. And so the law wants science, the definer of life and the definer of human, to tell it when human life begins, so that it may know when to define its ending as a crime.

It must now be clear that the human life of the scientist's perceptual modeling and the human life whose inviolability the law seeks to ensure are coincidentally the same words used in two entirely different conceptual frameworks. The law wants to know if the zygote, embryro, and fetus are human lives because it wants to know if these entities are entitled to the same rights and protections which the community has agreed to confer on human beings who have already been born.

The issue is thus not whether the zygote, embryo, and fetus are human lives in a scientific, definitional sense. The asking of that question is testimony to a profound misunderstanding of the capabilities and limitations of science. The issue is at what stage of development shall the entity destined to acquire the attributes of a human being be vested with the rights and protections accorded that status. It is to the moral codes of the people that the law must turn for guidance in this matter, not to the arbitrary definitions of science. The people are, of course, divided; the separate and combined influences of religious belief, secular morality, personal experience, blind emotion, and even caprice will be felt on all sides of the issue. It will be the difficult task of the lawmakers to create from this turmoil a reasoned and just code of action, but these are the voices which must first be heard. Science may never make moral judgments, the law must. To ask science to define human life in scientific terms for use by the law in moral terms is a travesty of both honorable traditions.



Who should control scientific information? This issue is at the center of a growing number of legal and administrative disputes. To whom do the data from federally funded research belong: the scientist who does the research or the government agency that pays for it? At what point in the research process are the data to be made available to interested citizens, competing scientists, or industrial firms? May the researchers themselves use their data and ideas in any way they choose?

Such questions have long been controversial because of the application of science to practical problems and its role in public affairs. They have become more urgent, however, as the gap between the production of knowledge and its application has narrowed. Related changes in policies concerning information disclosure, university-industry collaboration, patenting, and military security follow directly from the utility of research, turning scientific data and even ideas into "intellectual property"—something that is "owned or possessed" and therefore subject to competing claims (*1*).

Few principles exist to establish a definition of intellectual property. A congressional committee report has described the confused state of government information policy: the "profusion of inconsistent and often conflicting laws, policies and practices" (*2*). With few guiding principles, the struggle over control of scientific data and ideas has taken the form of discrete disputes in which all parties stake their claims in terms of moral rights and responsibilities: the "right to know," the "right to privacy," the "right to access," the "right to control one's own product," the "obligation to protect research subjects," and the "responsibility to protect the public interest."

Scientists who accept public funds are required by federal regulations to be accountable for the use of those funds, to give the funding agency access to their results as stipulated in a prior agreement, and to accept concomitant public disclosure. The federal agency's specific choice of funding instrument has itself a practical importance for the recipient's control (*3*). Except in the case of classified military research, scientists assume that data from projects funded by grants (as opposed to contracts or cooperative agreements) belong to the researchers. External controls on work done under grants are seen as a threat to the quality and integrity of research and an infringement on scientific freedom. In this context, applying concepts of "property" to scientific ideas is extremely controversial.

Much of the recent discussion about intellectual property in science has focused on the commercialization of biomedical research, but this is only part of a wider phenomenon. By describing diverse disputes over the control of research, I hope to suggest the broad range of situations in which questions of intellectual property have become important, the implications of recent changes in the relation of science to government and industry, and the problems involved in negotiating the principles of ownership and control (*4*).

Public Access versus Professional Control

In 1976 a group of physicians filed a request under the Freedom of Information Act (FOIA) for data gathered during a long-term clinical study of the effects of five diabetes treatment regimens, in which 1000 patients had been monitored over periods ranging from 5 to 8 years. A private consortium called the University Group Diabetes Program (UGDP) had conducted the study under the sponsorship of one of the National Institutes of Health (NIH). Under the terms of the grant NIH had the right to access to the raw data, but it never exercised that right.

The UGDP researchers found that certain regimens might increase the incidence of heart disease without any offsetting benefits. When they published these findings, the FDA recommended changes in the use and labeling of a particular diabetes treatment drug. An association of physicians, concerned lest a useful drug be removed unnecessarily from the market, requested access to the raw data and when refused brought suit (*5*). The U.S. Court of Appeals in 1978 ruled against disclosure on the grounds that the data, which were not actually in the possession of NIH, did not constitute "agency records," defined by statutory language as material that an agency has "created or obtained." In a dissenting opinion, however, Judge David Bazelon argued that federal funding of the research and reliance on the data for regulatory action were sufficient reasons to

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require disclosure and that, under FOIA, data underlying government actions must be open. In 1980 the Supreme Court upheld the ruling of the lower court, avoiding the policy issue concerning the public's right of access and basing its decision on the narrow legal question of possession.

A case with a different outcome also turned on the principle of custody. In 1978 Milo Shannon-Thornberry completed data collection on the socioeconomic factors affecting infant feeding practices and the relative effect of bottle and breast on morbidity and mortality rates. A group of nonprofit church organizations had sponsored the research. Lacking the resources to convert the data to computer records, Thornberry enlisted the facilities of the Department of Health, Education, and Welfare's Center for Disease Control (CDC) to help in the tabulation. He agreed in return to make the survey material available to the federal agency. In September 1979 two manufacturers of infant formula, Mead-Johnson and Abbott Laboratories, requested the raw data. Thornberry objected. He had collected them with private financing and claimed prior rights to analyze and publish them. He feared that the intent of the request was "industrial sabotage" of research threatening to corporate interests. In fact, a division of Abbott later circulated to physicians a letter undermining the credibility of the research.

In April 1980 a federal district court held that the CDC's possession of the data and its involvement in the project defined the data as "agency records" (6). Although CDC's role in the research was mostly clerical, the court considered disclosure to be proper; it chose to avoid the policy question about the intent of FOIA requests.

In other cases the courts have taken a more substantive view of requests that threaten professional control. James Allen, of the University of Wisconsin, had been working on a long-term, federally funded study of the effect of dioxin on rhesus monkeys. In 1979 he presented preliminary results as testimony during a hearing of the Environmental Protection Agency (EPA) on the use of dioxin in commercial pesticides. After the hearing, Dow Chemical Company tried to obtain all the data from Allen's study, arguing that if results were made public the background data should be available as well. Allen objected, on the grounds that the work was not completed or properly analyzed and that the testimony was only a preliminary progress report. An EPA administrative law judge granted Dow's request and issued a subpoena, but Allen refused to comply, on the principle of the scientist's right to autonomy. Following this reasoning, a U.S. district court overturned the ruling, agreeing that disclosure of data to a company with vested interests could jeopardize a costly study and that the public interest was better served by withholding data until after peer review (7). This decision was upheld by a federal appeals court in 1982.

The right of public access to information is sometimes invoked to obtain copies of unfunded research proposals. Requests to NIH for copies of proposals increased from 300 in 1975 to more than 1600 in 1979 (8). Although only a few of these requests (11 in 1979) have been for unfunded applications, scientists are sensitive to the threat of disclosure and contend that this would jeopardize the grant evaluation process and allow plagiarism or the pirating of ideas. Ideas, the scientist's "stock in trade," are in their view analogous to trade secrets in industrial firms.

The Right of Access versus Obligations of Confidentiality

In 1976 the National Heart and Lung Institute (NHLI) of NIH supported a longitudinal study of the health history of individuals with certain medical profiles. The researcher maintained detailed personal records and, in compliance with the Privacy Act, submitted only the final report of his findings to NHLI. During the course of study an independent investigator requested access to the records in order to conduct his own research. NHLI allowed its contractor to decide how to comply with the conditions governing the disclosure of confidential personal information (2, pp. 9–10).

In other cases, however, federal agencies try to maintain greater control. For example, HEW funded a study by a private research organization, Minnesota System Research Incorporated (MSRI), to evaluate the accuracy of HEW project ratings. When interviewing the scientists who had rated the projects, MSRI researchers promised not to reveal their identity. HEW, however, asked for the computer tapes, which contained the names of the respondents. The investigators objected on ethical grounds, but HEW insisted that a researcher could not promise confidentiality without first obtaining written agency permission. Eventually MSRI released the tapes (9).

Conflicts over confidentiality are most common in medical or social science research in which personal information is gathered (*10*). Special problems arise in the study of deviant or politically sensitive groups. Federal agencies require that in research on crime, drug addiction, political protest, and mental illness guarantees be provided to protect research subjects who could be liable to legal pressures if their identities were revealed. However, release of personal data gathered in health and epidemiological research could be equally damaging to individuals (*11*). The right of researchers to protect their subjects is especially vulnerable when it conflicts with political or policy goals. For example, there is the well-known case of the political scientist Samuel Popkin, who was imprisoned for contempt of court in 1972 because he refused to give the names of the persons he had interviewed to a federal grand jury investigating the publication of the Pentagon papers (*12*). The case is not unique. One study found that between 1966 and 1976, 50 subpoenas were issued demanding revelation of the sources and subjects of research (*13*).

The principles governing confidentiality in research remain inconsistent. The National Research Act of 1974 contains provisions to protect the privacy of human subjects, but it does not protect them from subpoena by the courts; nor does the legislation protecting the privacy of medical information used in health research (*14*). Researchers seek immunity from subpoena in order to avoid compromising their sources of data, but access to those sources is often necessary for law enforcement or policy purposes, or simply in order to maintain accountability in competitive areas of research.

Competitive Secrecy versus Open Communication

In 1977 a man with leukemia donated a sample of the cancerous cells from his bone marrow to research hematologists at the University of California School of Medicine. The scientists succeeded in creating a new cell line that could be used to study leukemia. They sent a sample to a colleague, who discovered that the cell line produced interferon, the body's natural antiviral protein. That scientist sent his sample to another colleague, who worked at the Roche Institute of Molecular Biology, funded by the pharmaceutical firm Hoffmann–La Roche. He, in turn, used the sample to develop an optimal medium for the production of interferon. The biotechnology firm Genentech, under contract to Hoffmann–La Roche, then used the cells to manufacture interferon genes, creating the potential for a profitable enterprise. There followed a dispute between the University of California and Hoffmann–La Roche over the ownership of the genes (*15*). The University of California claimed ownership, and the right to future royalties, as the institutional home of the scientists who had created the cell line. Hoffmann–La Roche also claimed ownership, and filed a patent application covering both the interferon and the gene splicing manufacturing process. Lawyers from the university protested, arguing that the firm had made unauthorized use of the material, taking commercial advantage of the open exchange of information and material among academic scientists.

This is but one of several disputes that may affect the practice of freely circulating materials and research findings among colleagues. Donald Kennedy, president of Stanford University, observes: "Scientists who once shared prepublication information freely and exchanged cell lines without hesitation are now much more reluctant to do so . . . the fragile network of informal communication that characterizes every especially active field is liable to rupture" (*16*).

Two decisions in 1980 brought these issues to public attention. First was the Supreme Court decision to allow the patenting of a genetically engineered bacterium (*17*). Second was the signing of the Patent and Trademark Amendment Act allowing universities, nonprofit institutions, and small businesses to apply for patents on federally funded research, the profits to be used to support further research (*18*).

Patents are specifically intended to avert proprietary secrecy as well as provide incentives for invention. But some fear that patenting possibilities in areas of basic science could also lead to closing of communication among researchers because of competition for patent priorities (*19, 20*). As universities seeking new forms of income and scientists attracted by possibilities of commercial development become directly involved in the industrial exploitation of research, tensions also develop between university adminstrators and faculty, between professors with commercial interests and graduate students whose careers depend on open discourse, and between federal and industrial research sponsors when there is a mingling of research support.

Tensions between commercial and academic interests are not new. Research cooperation between universities and industry flourished in the early part of the century, drastically reshaping the university system (*21*). Just as they are today, academics were ambivalent, welcoming such collaboration as a source of vitality but fearing its intrusion on academic freedom. Industry-university relations stabilized, however, through a system of contracts and cooperative agreements in engineering and the applied sciences. Scientists doing basic research participated mainly through individual consulting arrangements (*22*).

Today these traditional modes of interaction are changing. Ad hoc consulting arrangements have turned into equity participation in new venture-capital firms. In line with federal policy encouraging industry-university cooperation, universities are accepting collaborative arrangements in areas of basic as well as applied research. By the end of 1981, eight major agreements were either consummated or undergoing final negotiations.

National Security versus Scientific Freedom

In the summer of 1980, a computer scientist at the Massachusetts Institute of Technology wrote a proposal to work on the mathematical basis for developing computer techniques that would be impervious to code breaking. He applied for a grant from the National Science Foundation (NSF), which has routinely supported cryptography research. Since 1977 NSF has sent such proposals to the National Security Agency (NSA) for technical review because of their potential significance for foreign intelligence activities. That agency is responsible for the collection of intelligence information, does most of its own research, and has been increasingly uneasy about studies relating to its concerns but outside its control. This particular project was the first basic research to attract serious attention, and NSA wanted to assume part of the funding so that it could require review for military sensitivity prior to publication. Mathematicians working in this area were appalled at the idea that their work might be classified as unpublishable and therefore unavailable for public use (*23–25*). They negotiated a system of voluntary restraints in exchange for an advisory role.

The cryptography case must be seen in the light of two emerging trends: the trend toward extending national security controls to projects that are not sponsored by agencies concerned with military technology, and the trend toward applying such controls not only to hardware but to basic ideas and to "strategic information," that is, to information that if released could possibly harm the national interest.

Constraints on research of military significance are obviously not new. Under the Invention Secrecy Act, patentable discoveries may be placed under "secrecy orders" if disclosure is deemed detrimental to national security. This usually pertains to inventions developed by people working under defense contracts, but it has been more and more frequently applied to other inventions as well (*23, 26*).

Formidable secrecy controls also govern atomic research (*27*). Since World War II the federal government has maintained unambiguous authority to control all such research within or outside government laboratories. Information in this area is "born classified"—it is an official secret from the moment it exists. With the commercial development of nuclear energy, some provisions of the Atomic Energy Act of 1964 were relaxed. However, federal controls over access to information in this area give government agencies all proprietary rights.

The International Traffic in Arms regulations (ITAR), authorized by the Arms Export Control Act, provide that publication of unclassified information that can advance any significant military application requires prior approval by a cognizant agency. This provision is so inclusive as to allow flexibility of interpretation with respect to research (*28*).

Several incidents suggest the trend toward more rigid interpretation of such regulations. In the winter of 1980 a series of restrictions was imposed on scientific exchange (*29*). The Department of Energy issued an order requiring government clearance of any communication between its contractors and Soviet scientists. The Commerce Department forced the American Vacuum Society to withdraw its invitation to Soviet bloc scientists to attend an international conference on magnetic bubble memory devices. The State Department refused to issue visas to eight Soviet scientists who had applied to attend a conference on lasers and electro-optical systems. Also, it sent letters to university science departments asking them to restrict the movements of Chinese students and visitors.

Some proposed bills would impose further restrictions. One would require academic institutions accepting foreign students for study in certain scientific fields to submit detailed information on what they would be learning, whom they would work with, and where they planned to travel. More threatening is a legislative proposal to extend to ideas the provisions of the Arms Export Control Act which require a State Department license for exporting "critical technology" (*30*). Scientists who want to publish or lecture overseas on any subject relating to a technology listed on the U.S. Munitions List would have to obtain a prior license, and would bear the burden of proof that they would not be disseminating ideas harmful to our national security. The scope of restricted subjects could include research related to computers, lasers, and cryptography, but the list is far from clear. This and a similar revision of the Export Administration Act have remained in the House Foreign Affairs Committee. They are obstructed by questions of constitutionality and superseded by the Executive order that greatly increases the government's power to classify research not clearly related to national security. The Executive order omits the critical requirement established during the Carter Administration that decisions imposing secrecy must be balanced against the right to know, and mandates that "if there is reasonable doubt about the need to classify . . . the information shall be considered classified."

Negotiations

Scientists and their research sponsors are struggling to negotiate practices and articulate principles that would clarify questions of control in terms appropriate to the growing importance of scientific information. Open access to information serves the public interest, being consistent with the democratic values of open government and facilitating fiscal and management control over governmentally funded activities (*31*). But unlimited access may threaten the scientific process. At what point in the course of research should data be released? Are ideas to be publicly available in their tentative stages? Are data tangential to a project to be available as well? How can the integrity of long-term projects such as clinical trials be maintained while demands for public review are being satisfied?

Such questions have elicited efforts to define the terms of access under the FOIA. The director of NIH, for example, sought a specific exemption for data from ongoing clinical trials and epidemiological studies, although requests for such data have been rare. He brought the matter before the Ethics Advisory Board of HEW, arguing that premature release of data from clinical trials could impede the randomization necessary in the trials, and in the case of epidemiological studies could lead to publicity about misleading trends that would unduly alarm the public. He hoped to establish the principle that data that are preliminary, incomplete, or not yet validated should be exempted from FOIA because the public interest would thereby be better served. The Ethics Advisory Board recommended legislation to provide a limited exemption (*32*).

The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research reviewed the implications of using FOIA to obtain information about research protocols and designs. The commission concluded that HEW's policy of releasing data only after funding is awarded is necessary in order to protect peer review procedures, and recommended legislation to ensure a continuation of the existing practice (*33*).

As a result of these various pressures, it is now proposed, in a bill to revise FOIA, that research be added to the other matters, such as trade secrets, that are exempt from compulsory disclosure.

Professional societies are establishing their own special committees to respond to questions of intellectual property and are experimenting with codes of ethics that include provisions about disclosure. Individual scientists have proposed research agreements that would confer a status of "executive privilege" on research in order to protect confidentiality. However, such agreements would not be legally binding. Usually scientists protect information by practicing "defensive record keeping," but they have also sought protective legislation (*34*). For example, in 1980 a bill was introduced in Congress on privacy of research records (S. 867 and H.R. 3409) which was intended to exempt researchers from subpoenas that would violate the privacy of their subjects. The subjects of research on drug and alcohol abuse are already protected; this bill would have extended such statutory protection to all federally funded research. Legislation might have provided some clarification of the problem of confidentiality, but the bill never got as far as a hearing.

A remarkable number of studies and proposals have tried to resolve the problems of proprietary secrecy and academic autonomy associated with increasing industry-university collaboration. Universities are negotiating limited partnership arrangements with industries that would yield economic benefits yet allow them to maintain internal control over research practices and to publish in areas where proprietary secrecy could pose serious constraints. An NIH working group has recommended that all institutions receiving research funds have a written patent policy including provisions for resolving proprietary disputes (*20*). The Recombinant DNA Advisory Committee (RAC) of NIH has revised its guidelines in order to protect proprietary information while allowing the necessary review. These guidelines allow access to research information only to committee members and staff, who must pledge confidentiality and maintain locked files.

Military efforts to extend control over the disclosure of nonclassified research are also under negotiation. The mathematicians concerned about control of cryptography have established a system of voluntary restraint. Researchers have agreed to submit their papers to NSA for review prior to publication, and NSA in turn has agreed that, if there are potential security problems, it will consult an advisory group before blocking publication (*25, 35*). Vice Admiral Bobby Inman, seeing direct contradictions between scientific practices of open publication and national security needs, proposes extending this system of voluntary restraint to other fields. The threat of such restraints is forcing a sharpening of distinctions between basic and applied research, although convergence has served science well.

Most challenges to scientific discretion end in appeals for statutory protection, based on an assumption that the researcher has the "right" to assess the terms of disclosure. Such protection would require a more coherent effort to clarify the social role of scientists and the nature of research that is worthy of protection. Should criteria for protection be based on the credentials of scientists or on their sources of support, on the methodology of a project or on the social purposes to which it may be applied? Under what conditions would mandatory disclosure or, conversely, secrecy be appropriate?

At present, the scientific response to such questions is curiously ad hoc. Arising from individual incidents, the efforts to negotiate control over research are often inconsistent. Scientists appeal for statutory protection from the disclosure requirements of FOIA, but in doing so they help to weaken legislation that encourages open exchange of information. They seek greater military support of research but are outraged by national security restraints. They emphasize the useful application of basic research but then draw distinctions to avoid external control. Good reasons underlie each response, but such contradictions may make science more vulnerable to control. For example, in justifying national security constraints, Inman has called scientific claims of freedom "disingenuous" in the light of the trade secrecy restrictions that academics routinely accept as part of their industrial ties (*36*).

The response of scientists rests on the principle of sovereignty and on the belief that the public interest is better served if research is under scientific control. It rests on a notion that scientists have a "right" to control their research, that autonomy is necessary in order to maintain integrity, to avert the misinterpretation of premature data, and to protect their "stock in trade." Those who request data claim the "right to know" as an essential condition of democracy. Government agencies claim the right to information as part of their obligation to assure responsible use of federal funds, to meet policy goals, or to maintain national security or law enforcement in the public interest.

In today's political context, resolution of these conflicting claims leans toward greater constraints on freedom of information—constraints apparent in the weakening of FOIA, the extension of national security classification, and other restrictions on open communication, some of which protect scientific sovereignty and some of which threaten it.

Contradictions persist, reflecting the deep ambivalence within science about its cognitive and practical dimensions. Is science the pursuit of truth or the pursuit of useful knowledge, a carefully disciplined process or a professional and instrumental activity? The ambivalence so apparent in the disputes over the control of research suggests that there have been significant changes in the social role of science and in the importance of research. Indeed, these disputes are part of a larger struggle to renegotiate relations between science and the public that were established at a time when science was a very different social enterprise.

References and Notes

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4. I shall concentrate on basic research, excluding those areas of military and applied industrial research that are indisputably subject to proprietary secrecy or military classification However, it is important to note that the debates about disclosure of scientific information take place in the context of a broader debate over the exemption of trade secrets from the Freedom of Information Act This debate has come to focus on the proprietary status of test data on health and safety. See T. O. McGarity and S Shapiro, *Harv Law Rev* 93, 831 (1980)
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37. Material and criticism were generously supplied by Rosemary Chalk, David Goldwyn, Harold Relyea, Robert Gellman, Barbara Mishkin, and members of the AAAS Committee on Scientific Freedom and Responsibility. The hospitality of the Laboratoire d'Econometrie, Ecole Polytechnique (Paris), is appreciated

Many universities are examining searchingly their relations with industry. The basic reason is financial. The academic community is nervous about federal funding of research. Some universities report that they have retained a satisfactory level of support, but half or more have not. Apprehension about federal support has been coupled with other financial problems of the universities brought on by recession and inflation.

In this environment it has become fashionable to look to industry as a possible source of funds. Already a number of universities have entered into contracts involving substantial sums, and additional arrangements will doubtless follow. In general, industry has not been devoting a sufficient sum to basic research within its own laboratories or elsewhere. It was treated to a lesson when a large number of companies were caught flatflooted by academic developments in molecular biology. Other sectors of industry have become concerned about future supplies of personnel trained in computer-related fields.

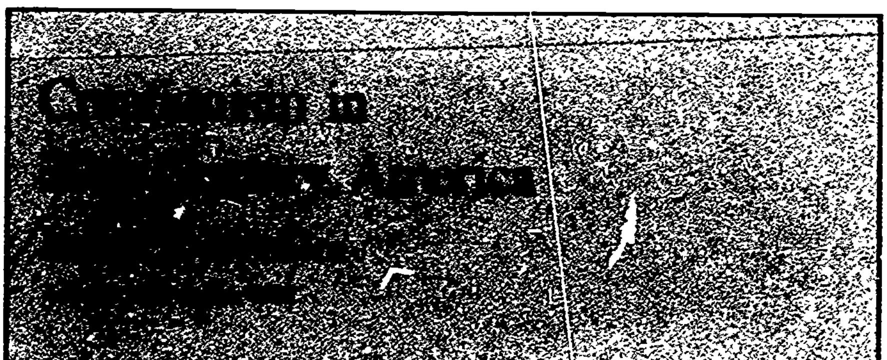
Despite an apparent basis for close cooperation between academia and industry, the likely outcome is far from a cure-all for the financial ills of the universities. The money spent by industry at universities is unlikely to top 10 percent of the federal funds they now receive. Close cooperation between universities and industry could lead to harmful tensions induced by competing value systems. Universities already have their share of such differences. The humanists look down on the engineers and vice versa; the various science departments usually have little interaction. However, the faculty share common goals in the pursuit of knowledge and in fostering the education of the young. Most of the faculty place these goals above that of attaining personal wealth.

The value system and the mode of conducting research and development in industry are quite different from those of academia. To survive, a company must make a profit. It must evolve with the changing times. And it must be well managed, with a clearly defined chain of command. The bankruptcy courts are very busy these days. Only the strong and nimble remain viable.

In industry, the pressure of the bottom line inevitably dictates policies with respect to R&D. The goal is not pursuit of knowledge; it is the attaining of proprietary advantage. Accordingly, research results obtained at industrial laboratories often go unpublished or are released slowly in the patent literature. In the university, fast publication of scientific findings is eagerly sought. Much of the activity in industry is conducted by interdisciplinary teams whose members are arbitrarily assigned to tasks. Projects may be suddenly terminated. Only a favored few in the typical industrial laboratory have the privilege of personally choosing a research area and sticking with it through discouraging phases of effort. This frenetic tempo is incompatible with the tempo of graduate training in the natural sciences. In their thesis research, it is essential that students pursue a line of inquiry patiently and in depth.

These examples of differing values and procedures make it obvious that close collaboration between academia and industry is likely to create new problems and tensions. That is not to say that cooperation is undesirable. One time-tested method of cooperation is that of consultation, preferably conducted off-campus. Professors spend at most an average of a day a week at this. They bring their expertise to industry and in turn learn of new developments and new job opportunites for their students.

However, some of the new arrangements between universities and industry come close to inserting an industrial enclave into the campus. It would be unfortunate if such examples were carelessly multiplied. Rather, emphasis should be placed on avoiding relationships that might damage the universities and their ability to carry out well their essential functions of undergraduate and graduate education.



Scarcely 20 years after the publication of Charles Darwin's *Origin of Species* in 1859 special creationists could name only two prominent naturalists in North America, John William Dawson of McGill and Arnold Guyot of Princeton, who had not embraced some theory of organic evolution (*1*). Liberal churchmen were already beginning to follow their scientific colleagues into the evolutionist camp, and by the end of the century evolution was appearing even within the ranks of evangelical Christians. In the opinion of many observers, belief in special creation seemed destined to go the way of the dinosaurs. But contrary to the hopes of liberals and the fears of conservatives, it did not become extinct. The majority of late-19th-century Americans remained true to a traditional reading of Genesis, and as late as 1979 a public opinion poll revealed that half the adults in America continued to believe that "God created Adam and Eve to start the human race" (*2*).

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This article focuses on the intellectual leaders of creationism, particularly the small number who claimed scientific expertise. Drawing on their writings, it traces the ideological development of creationism from the crusade to outlaw the teaching of evolution in the 1920's to the current battle for equal time. During this period the leading apologists for special creation shifted from an openly biblical defense of their views to one based largely on science. At the same time they grew less tolerant of notions of an old Earth and symbolic days of creation, common among creationists in the 1920's, and more doctrinaire in their insistence on a recent creation in six literal days and on a universal flood.

The Antievolution Crusade

Early in 1922 William Jennings Bryan (1860–1925), Presbyterian layman, thrice the Democratic candidate for the presidency of the United States, and secretary of state under Woodrow Wilson, heard of an effort in Kentucky to ban the teaching of evolution in public schools. "The movement will sweep the country," he predicted hopefully, "and we will drive Darwinism from our schools" (*3*, p. 277). His prophecy proved overly optimistic, but before the end of the decade more than 20 state legislatures did debate antievolution laws, and at least five—Oklahoma, Florida, Tennessee, Mississippi, and Arkansas—passed restrictive legislation. Many individuals shared responsibility for these events, but none had a greater share than Bryan. His entry into the fray produced a catalytic effect and gave antievolutionists what they needed most: "a spokesman with a national reputation, immense prestige, and a loyal following" (*3*, p. 272).

The development of Bryan's own attitudes toward evolution closely paralleled that of the fundamentalist movement. Since early in the century he had occasionally alluded to the silliness of believing in monkey ancestors and to the ethical dangers of thinking that might makes right, but until the outbreak of World War I he saw little reason to quarrel with those who disagreed. The war, however, exposed the darkest side of human nature and shattered his illusions about the future of Christian society. Obviously something had gone awry, and Bryan soon traced the source of the trouble to the paralyzing influence of Darwinism on the conscience. By substituting the law of the jungle for the teaching of Christ, it threatened the principles he valued most: democracy and Christianity. Two books in particular confirmed his suspicion. The first, Vernon Kellogg's *Headquarters Nights* (1917), recounted firsthand conversations with German officers that revealed the role of Darwin's biology in the German decision to declare war. The second, Benjamin Kidd's *Science of Power* (1918), purported to demonstrate the historical and philosophical links between Darwinism and German militarism (*3*, pp. 261–265).

About the time that Bryan discovered the relation between Darwinian ideas and the war, he also became aware, to his great distress, of unsettling effects the theory of evolution was having on America's own young people. From frequent visits to college campuses and from talks with parents, pastors, and Sunday School teachers, he learned about an epidemic of unbelief that was sweeping the country. Upon investigating the cause, reported his wife, "he became convinced that the teaching of Evolution as fact instead of a theory caused the students to lose faith in the Bible, first, in the story of creation, and later in other doctrines, which underlie the Christian religion" (*4*). Again Bryan found confirming evidence in a recently published book, *Belief in God and Immortality* (1916), by the Bryn Mawr psychologist James H. Leuba, who demonstrated statistically that college attendance endangered traditional religious beliefs (*3*, pp. 266–267).

Armed with this information about the cause of the world's and the nation's moral decay, Bryan launched a nationwide crusade against the offending doctrine. Throughout his political career Bryan had placed his faith in the common people, and he resented the attempt of a few thousand scientists "to establish an oligarchy over the forty million American Christians" and to dictate what should be taught in the schools (*5*). To a Democrat like Bryan, it seemed preposterous that this "scientific soviet" would not only demand to teach its insidious philosophy but insist that society pay its salaries (*3*, p. 289). Confident that nine-tenths of the Christian citizens agreed with him (*6*), he decided to appeal directly to them, as he had done successfully in fighting the liquor interests. "Commit your case to the people," he advised creationists. "Forget, if need be, the high-brows both in the political and college world, and carry this cause to the people. They are the final and efficiently corrective power" (*7*).

Leadership of the antievolution movement came not from the organized churches of America but from individuals like Bryan and interdenominational organizations such as the World's Christian Fundamentals Association, a predominantly premillennialist body founded in 1919 by William Bell Riley (1861–1947), pastor of the First Baptist Church in Minneapolis. Riley became active as an antievolutionist after discovering, to his apparent surprise, that evolutionists had already infiltrated the University of Minnesota (*8*). The early 20th century witnessed the unprecedented expansion of public education (enrollment in public high schools nearly doubled between 1920 and 1930), and fundamentalists like Riley and Bryan wanted to make sure that students attending these institutions would not lose their faith. Thus they resolved to drive every evolutionist from the public school payroll. One creationist went so far as to say that the German soldiers who killed Belgian and French children with poisoned candy were angels compared with the teachers and textbook writers who corrupted the souls of children with false teachings and thereby sentenced them to eternal death (*9*).

Creationist Science and Scientists

In 1922 William Bell Riley outlined the reasons why fundamentalists opposed the teaching of evolution: "The first and most important reason for its elimination," he explained, "is in the unquestioned fact that evolution is not a science; it is a hypothesis only, a speculation" (*10*). Bryan often made the same point, defining true science as "classified knowledge . . . the explanation of facts" (*11*). Although creationists had far more compelling reasons for rejecting evolution than that it was "not a science," their insistence on this point was not merely an obscurantist ploy. They based it on a once-respected principle, associated with Sir Francis Bacon, that emphasized the factual, nontheoretical nature of science (*12*). By identifying with the Baconian tradition, creationists could label evolution as false science, could claim equality with scientific authorities in comprehending facts, and could deny the charge of being antiscientific. "It is not 'science' that orthodox Christians oppose," wrote a fundamentalist editor. "No! no! a thousand times, No! They are opposed only to the theory of evolution, which has not yet been proved, and therefore is not to be called by the sacred name of *science*" (*13*).

Creationists kept assuring themselves that the world's best scientists agreed with them. They received an important boost at the beginning of their campaign from an address by the distinguished British biologist William Bateson, in 1921, in which he declared that scientists had not yet uncovered "the actual mode and process of evolution" (*14*). Although he warned creationists against misinterpreting his statement as a rejection of evolution, they paid no more attention to that caveat than they did to the numerous proevolution resolutions passed by scientific societies.

The creationists could claim few scientists of their own: a couple of self-made men of science, one or two physicians, and a handful of teachers who, as one evolutionist described them, were "trying to hold down, not a chair, but a whole settee, of 'Natural Science' in some little institution" (*15*). Of this group, the most influential were Harry Rimmer (1890–1952) and George McCready Price (1870–1963).

Rimmer, a Presbyterian minister and self-styled "research scientist," obtained his limited exposure to science during one term at a small homeopathic medical school, where he picked up a vocabulary of "double-jointed, twelve cylinder, knee-action words" that later served to impress the uninitiated (*16*). He attended Whittier College and the Bible Institute of Los Angeles for a year each before entering full-time evangelistic work. About 1919 he settled in Los Angeles, where he set up a small laboratory at the rear of his house to conduct experiments in embryology and related sciences. Within a year or two he established the Research Science Bureau "to prove through findings in biology, paleontology, and anthropology that science and the literal Bible were not contradictory" (*17*, p. 278). The bureau staff—that is, Rimmer—apparently used income from the sale of memberships to finance anthropological field trips in the western United States, but Rimmer's dream of visiting Africa to find proof of the dissimilarity between gorillas and human beings never materialized. By the late 1920's the bureau lay dormant, and Rimmer signed on with Riley's World's Christian Fundamentals Association as a field secretary (*17*, p. 279).

Besides engaging in research, Rimmer delivered thousands of lectures, primarily to student groups, maintaining the scientific accuracy of the Bible and ridiculing evolutionists. To attract attention, he repeatedly offered $100 to anyone who could discover a scientific error in the Scriptures; the offer apparently never cost him any money (*18*). He also, by his own reckoning, never lost a public debate. After one encounter with an evolutionist in Philadelphia, he wrote home gleefully that "the debate was a simple walkover, a massacre—murder pure and simple. The eminent professor was simply scared stiff to advance any of the common arguments of the evolutionists, and he fizzled like a wet fire-cracker" (*17*, pp. 329–330).

Price, a Seventh-day Adventist geologist, was less skilled at debating than Rimmer but more influential scientifically. As a young man Price attended an Adventist college in Michigan for 2 years and later completed a teacher-training course at the provincial normal school in his native New Brunswick. The turn of the century found him serving as principal of a small high school in an isolated part of eastern Canada, where one of his few companions was a local physician. During their many conversations, the doctor almost converted his fundamentalist friend to evolution. Price nearly succumbed on at least three occasions, but each time he was saved by prayer and by reading the works of the Adventist prophetess Ellen G. White, who claimed divine inspiration for her view that the Noachian flood accounted for the fossil record on which evolutionists based their theory. As a result of these experiences, Price vowed to devote his life to promoting creationism of the strictest kind (*19, 20*).

By 1906 he was working as a handyman at an Adventist sanitarium in southern California. That year he published a small volume entitled *Illogical Geology, The Weakest Point in the Evolution Theory*, in which he brashly offered $1000 "to any one who will, in the face of the facts here presented show me how to prove that one kind of fossil is older than another." (Like Rimmer, he never had to pay.) According to his argument, Darwinism rested "logically and historically on the succession of life idea as taught by geology" and "if this succession of life is not an actual scientific fact, then Darwinism . . . is a most gigantic hoax."

In a review (*21*), David Starr Jordan, president of Stanford University and an authority on fossil fishes, warned Price that he should not expect "any geologist to take [his work] seriously." The unknown author had written "a very clever book" but it was

> a sort of lawyer's plea, based on scattering mistakes, omissions and exceptions against general truths that anybody familiar with the facts in a general way cannot possibly dispute. It would be just as easy and just as plausible and just as convincing if one should take the facts of European history and attempt to show that all the various events were simultaneous.

As Jordan recognized, Price lacked any formal training or field experience in geology. He was, however, a voracious reader of geological literature, an armchair scientist who self-consciously minimized the importance of field experience.

During the next 15 years Price occupied scientific settees in several Adventist schools and authored six more books attacking evolution, particularly its geological foundation. Although not unknown outside his own church before the early 1920's, he did not begin attracting [illegible]al attention until then. Shortly af[illegible] Bryan declared war on evolution, [illegible]ce published *The New Geology* (1923), the most systematic and comprehensive of his many books. In it he presented his "*great law of conformable stratigraphic sequences* . . . by all odds the most important law ever formulated with reference to the order in which the strata occur." This law stated that "*any kind of fossiliferous beds whatever, 'young' or 'old,' may be found occurring conformably on any other fossiliferous beds, 'older' or 'younger.'*" To Price, so-called deceptive conformatives (where strata seem to be missing) and thrust faults (where the strata are apparently in the wrong order) proved that there was no natural order to the fossil-bearing rocks, all of which he attributed to the Genesis flood.

A Yale geologist reviewing the book for *Science* accused Price of "harboring a geological nightmare" (*22*), but Price's reputation among fundamentalists rose dramatically. Rimmer hailed *The New Geology* as "a masterpiece of REAL science [that] explodes in a convincing manner some of the ancient fallacies of science 'falsely so called' " (*23*). By the mid-1920's Price's by-line was appearing in a broad spectrum of conservative religious periodicals, and the editor of *Science* could accurately describe him as "the principal scientific authority of the Fundamentalists" (*24*).

The Scopes Trial and Beyond

In the spring of 1925 John Thomas Scopes, a high school teacher in the small town of Dayton, Tennessee, confessed to having violated the state's recently passed law banning the teaching of human evolution in public schools. His subsequent trial focused international attention on the antievolution crusade and brought William Jennings Bryan to Dayton to assist the prosecution. In anticipation of arguments on the scientific merits of evolution, Bryan sought out the best scientific minds in the creationist camp to serve as expert witnesses. The response to his inquiries could only have disappointed the aging crusader. Price, then teaching in England, sent his regrets—along with advice to Bryan to stay away from scientific topics (*20*, p. 24). Howard A. Kelly, a prominent Johns Hopkins physician who had contributed to the *Fundamentals*, confessed that, except for the creation of Adam and Eve, he believed in evolution (*25*). Louis T. More, a physicist who had just written a book entitled *The Dogma of Evolution* (1925), replied that he accepted evolution as a working hypothesis (*26*). Alfred W. McCann, author of *God—or Gorilla* (1922), took the opportunity to lecture Bryan for supporting prohibition in the past and for now trying "to bottle-up the tendencies of men to think for themselves" (*27*).

At the trial itself, things scarcely went better. When Bryan could name only Price and the deceased George Frederick Wright as scientists for whom he had respect, the caustic Clarence Darrow, attorney for the defense, scoffed:

> You mentioned Price because he is the only human being in the world so far as you know that signs his name as a geologist that believes like you do . . . every scientist in this country knows [he] is a mountebank and a pretender and not a geologist at all.

Eventually Darrow forced Bryan to concede that the world was indeed far more than 6000 years old and that the 6 days of creation had probably been longer than 24 hours each (*20*, p. 24).

Though one could scarcely have guessed it from his public pronouncements, Bryan was far from being a strict creationist. In fact, his personal beliefs regarding evolution diverged considerably from those of his more conservative supporters. Shortly before the trial he had confided to Kelly that he, too, had no objection to "evolution before man but for the fact that a concession as to the truth of evolution up to man furnishes our opponents with an argument which they are quick to use, namely, if evolution accounts for all the species up to man, does it not raise a presumption in behalf of evolution to include man?" Until biologists could actually demonstrate the evolution of one species into another, he thought it best to keep them on the defensive (*28*).

Bryan's concession at Dayton spotlighted a serious and long-standing problem among antievolutionists: their failure to agree on a theory of creation. Even the visible leaders could not reach a consensus. Riley, like Bryan, interpreted the days of Genesis as ages, believing that the testimony of geology necessitated this approach. Rimmer favored an exegesis that identified two separate creations in the first chapter of Genesis, the first, "in the beginning," perhaps millions of years ago, and the second, in six actual days, approximately 4000 years before the birth of Christ. He adopted this view in part because his scientific mind could not fathom how, given Riley's scheme, plants created on the third day could have survived thousands of years without sunshine until the sun appeared on the fourth (*29*). According to the testimony of acquaintances, he also believed that the Bible taught a local rather than a universal flood (*30*). Price, who cared not a whit about the opinion of geologists, insisted on nothing less than a single recent creation in six literal days and a worldwide deluge. He regarded Riley's day-age theory as "the devil's counterfeit" (*31*) and Rimmer's gap theory as only slightly more acceptable (*32*).

Although the court in Dayton found Scopes guilty as charged, creationists had little cause for rejoicing. The press had not treated them kindly, and the taxing ordeal no doubt contributed to Bryan's death a few days after the trial ended. Nevertheless, the antievolutionists continued their crusade, winning victories in Mississippi in 1926 and in Arkansas 2 years later. By the end of the decade, however, their legislative campaign had lost its steam. The presidential election of 1928, pitting a Protestant against a Catholic, offered fundamentalists a new diversion, and the onset of the depression in 1929 further diverted their energies.

But contrary to appearances, the creationists were simply changing tactics, not giving up. Instead of lobbying state legislatures, they shifted their attack to local communities, where they engaged in "the emasculation of textbooks, the 'purging' of libraries, and above all the continued hounding of teachers" (*33*). Their new approach attracted less attention but paid off handsomely, as school boards, textbook publishers, and teachers in both urban and rural areas, North and South, bowed to their pressure. Darwinism virtually disappeared from high school texts, and as late as 1941 one-third of American teachers feared being identified as evolutionists (*34*).

Creationism Underground

During the heady days of the 1920's, when their activities made front-page headlines, creationists dreamed of converting the world; a decade later, rejected and forgotten by the establishment, they turned their energies inward and began creating an institutional base of their own. Deprived of the popular press and unable to publish their views in organs controlled by orthodox scientists, they determined to organize their own societies and edit their own journals. Their early efforts, however, all encountered two problems: lack of a critical mass of scientifically trained creationists and lack of internal agreement.

About 1935 a small group of creationists, led by a Wheaton College professor, formed the Religion and Science Association to create "a unified front against the theory of evolution" (*35*, p. 159). Among those invited to participate in the association's first convention were representatives of the three major creationist parties, including Price, Rimmer, and one of Dawson's sons who, like his father, advocated the day-age theory (*35*, p. 209). But, as soon as the Price faction discovered that their associates had no intention of agreeing on a short Earth history, they bolted the organization, leaving it in shambles (*36*).

Shortly thereafter, in 1938, Price and some Adventist friends in the Los Angeles area, several of them physicians associated with the College of Medical Evangelists (now Loma Linda University), organized their own Deluge Geology Society and, between 1941 and 1945, published a *Bulletin of Deluge Geology and Related Science*. As described by Price, the group consisted of "a very eminent set of men. . . . In no other part of this round globe could anything like the number of scientifically educated believers in Creation and opponents of evolution be assembled, as here in Southern California" (*20*, p. 26). Perhaps the society's most notable achievement was its sponsorship in the early 1940's of a hush-hush project to study giant fossil footprints, believed to be human, discovered in rocks far older than the theory of evolution would allow. This find, the society announced, demolished that theory "at a single stroke" and promised to "astound the scientific world!" (*37*). But despite such success and the group's religious homogeneity, it too soon foundered—on "the same rock," complained a disappointed member, that wrecked the Religion and Science Association, that is, "pre-Genesis time for the earth" (*38*).

By this time creationists were also beginning to face a new problem: the presence within their own ranks of young university-trained scientists who wanted to bring evangelical Christianity more into line with mainstream science. The encounter between the two generations often proved traumatic, as is illustrated by the case of Harold W. Clark (born 1891). Once a student of Price's, he had gone on to earn a master's degree in biology from the University of California and taken a position at a small Adventist college in northern California. By 1940 his training and field experience had convinced him that Price's *New Geology* was "entirely out of date and inadequate" as a text, especially in its rejection of the geological column. When Price learned of this, he angrily accused his former disciple of suffering from "the modern mental disease of university-itis" and of currying the favor of "tobacco-smoking, Sabbath-breaking, God-defying" evolutionists. Despite Clark's protests that he still believed in a literal 6-day creation and universal flood, Price kept up his attack for the better part of a decade, at one point addressing a vitriolic pamphlet, *Theories of Satanic Origin*, to his erstwhile friend and fellow creationist (*20*, p. 25).

The inroads of secular scientific training also became apparent in the American Scientific Affiliation (ASA), created by evangelical scientists in 1941. Although the society adopted no statement of belief, during its early years strict creationists found its atmosphere congenial. However, in the late 1940's some of the more progressive members, led by a geochemist, J. Laurence Kulp, began criticizing Price and his followers for their attempts to compress Earth history into less than 10,000 years. Kulp, a Wheaton alumnus and a Plymouth Brother, had acquired a doctorate from Princeton University before joining the Department of Geology at Columbia University. Although initially suspicious of the conclusions of geology regarding the history and antiquity of the earth, he had come to accept them (*39*). As one of the first evangelicals professionally trained in geology, he felt a responsibility to warn his colleagues in the ASA about Price's work, which, he believed, had "infiltrated the greater portion of fundamental Christianity in America primarily due to the absence of trained Christian geologists." In what was apparently the first systematic critique of the "new geology" Kulp concluded that the "major propositions of the theory are contradicted by established physical and chemical laws" (*40*). Conservatives within the ASA not unreasonably suspected that Kulp's exposure to "the orthodox geological viewpoint" had severely undermined his faith in a literal interpretation of the Bible (*41*). As more and more ASA members drifted from strict creationism, a split appeared inevitable.

Henry M. Morris and the Revival of Creationism

In 1964 a historian predicted that "a renaissance of the [creationist] movement is most unlikely" (*42*). And so it seemed. But even as those words were penned a revival was under way, led by a Texas engineer, Henry M. Morris. Reared a nominal Southern Baptist, and a believer in creation, Morris as a youth had drifted unthinkingly into evolutionism and religious indifference. A thorough study of the Bible after his graduation from college convinced him of its absolute truth and prompted him to reevaluate his belief in evolution. After an intense period of soul-searching he concluded that creation had taken place in six literal days because the Bible clearly said so and "God doesn't lie." Corroborating evidence soon came from the book of nature. While sitting in his office at Rice Institute, where he was teaching civil engineering, he would study the butterflies and wasps that flew in through the window; being familiar with structural design, he calculated the improbability of the development of such complex creatures by chance. Nature as well as the Bible seemed to argue for creation (*43*).

For assistance in answering the claims of evolutionists, he found little creationist literature of value other than the writings of Rimmer and Price. Although he rejected Price's peculiar theology, he took an immediate liking to his flood geology and incorporated it into a little book, *That You Might Believe* (1946), the first book, so far as he knew, "published since the Scopes trial in which a scientist from a secular university advocated recent special creation and a worldwide flood" (*44*). In the late 1940's he joined the ASA—just in time to protest Kulp's attack on Price's geology. But his words fell on deaf ears. In 1953, when he presented some of his own views on the flood to the ASA, one of the few compliments came from a young theologian, John C. Whitcomb, Jr., who belonged to the Grace Brethren. Morris and Whitcomb subsequently became friends and decided to collaborate on a major defense of the Noachian flood. By the time they finished their project, Morris had earned a Ph.D. in hydraulic engineering from the University of Minnesota and was chairing the civil engineering department at Virginia Polytechnic Institute. Whitcomb was teaching Old Testament studies at Grace Theological Seminary in Indiana (*43*).

In 1961 they brought out *The Genesis Flood* (*45*), the most impressive contribution to strict creationism since the publication of Price's *New Geology* in 1923. In many respects their book appeared to be simply "a reissue of G. M. Price's views, brought up to date," as one reader described it (*46*). Beginning with a testimony to their belief in "the verbal inerrancy of Scripture" (*45*, p. xx) Morris and Whitcomb went on to argue for a recent creation of the entire universe, a fall that triggered the second law of thermodynamics, and a worldwide flood that in 1 year laid down most of the geological strata. Given this history, they argued, "the last refuge of the case for evolution immediately vanishes away, and the record of the rocks becomes a tremendous witness . . . to the holiness and justice and power of the living God of Creation!" (*45*, p. 451).

Despite the book's lack of conceptual novelty, it provoked an intense debate among evangelicals. Progressive creationists, who interpreted the days of Genesis symbolically, denounced it as a travesty on geology that threatened to set back the cause of Christian science a generation, while strict creationists praised it for making biblical catastrophism intellectually respectable. Its appeal, suggested one critic, lay primarily in the fact that, unlike previous creationist works, it "looked *legitimate* as a scientific contribution," accompanied as it was by footnotes and other scholarly appurtenances (*47*). In responding to their detractors, Morris and Whitcomb repeatedly refused to be drawn into a scientific debate, arguing that "the real issue is not the correctness of the interpretation of various details of the geological data, but simply what God has revealed in His Word concerning these matters" (*48*).

Whatever its merits, *The Genesis Flood* unquestionably "brought about a stunning renaissance of flood geology" (*49*), symbolized by the establishment in 1963 of the Creation Research Society. Shortly before its publication Morris had sent the manuscript to Walter E. Lammerts, a Missouri–Synod Lutheran with a doctorate in genetics from the University of California. As an undergraduate at Berkeley Lammerts had discovered Price's *New Geology*, and during the early 1940's. while teaching at UCLA. he had worked with Price in the Creation-Deluge Society. After the mid-1940's. however. his interest in creationism had flagged. until reawakened by the Morris and Whitcomb manuscript. Disgusted by the ASA's flirtation with evolution. he organized in the early 1960's a correspondence network with Morris and eight other strict creationists. dubbed the "team of ten." In 1963 seven of the ten met with a few other like-minded scientists at the home of a team member in Midland, Michigan. to form the Creation Research Society (CRS) (*50*).

The society began with a carefully selected, 18-man "inner-core steering committee." which included the original team of ten. The composition of this committee reflected. albeit imperfectly. the denominational. regional, and professional bases of the creationist revival. There were six Missouri–Synod Lutherans. five Baptists. two Seventh-day Adventists. and one each from the Reformed Presbyterian Church. the Reformed Christian Church, the Church of the Brethren. and an independent Bible church (information about one member is lacking). Eleven lived in the Midwest. three in the South, and two in the Far West. The committee included six biologists but only one geologist, an independent consultant with a master's degree. Seven members taught in church-related colleges. five in state institutions. the others worked for industry or were self-employed (*51*).

To avoid the creeping evolutionism that had rent the ASA and to ensure that the society remain loyal to the Price-Morris tradition. the CRS required members to sign a statement of belief accepting the inerrancy of the Bible. the special creation of "all basic types of living things." and a worldwide deluge. It restricted membership to Christians (*51*). [Although creationists liked to stress the scientific evidence for their position. one estimated that "only about five percent of evolutionists-turned-creationists did so on the basis of the overwhelming evidence for creation in the world of nature." The remaining 95 percent were creationists because they believed in the Bible (*52*).] To legitimize its claim to being a scientific society. the CRS published a quarterly journal and limited full membership to persons with a graduate degree in a scientific discipline (*51*).

At the end of its first decade the society claimed 450 regular members. plus 1600 sustaining members—those who did not meet the scientific qualifications (*50*, p. 63). Eschewing politics. the CRS devoted itself almost exclusively to education and research, funded "at very little expense. and . . . with no expenditure of public money." Among the projects it supported were expeditions to search for Noah's ark. studies of fossil human footprints and pollen grains found out of the predicted evolutionary order. experiments on radiation-produced mutations in plants. and theoretical studies in physics demonstrating a recent origin of the earth (*53*). A number of members collaborated in preparing a biology textbook based on creationist principles (*54*). In view of the previous history of creation science. it was an auspicious beginning.

While the CRS catered to the needs of scientists, a second, predominantly lay. organization carried creationism to the masses. Initiated in 1964 in the wake of interest generated by *The Genesis Flood*. the Bible-Science Association came to be identified by many with one man, Walter Lang. an ambitious Missouri-Synod pastor who assertively prized spiritual insight above scientific expertise (*55*). As editor of the widely circulated *Bible-Science Newsletter*. he vigorously promoted the Price-Morris line and occasionally provided a platform for individuals on the fringes of the creationist movement. such as those who questioned the heliocentric theory and who believed that Einstein's theory of relativity "was invented in order to circumvent the evidence that the earth is at rest" (*56*). Needless to say. the pastor's broad-mindedness greatly embarrassed creationists seeking scientific respectability. who feared such bizarre behavior would tarnish the entire movement (*57*).

Scientific Creationism

The creationist revival of the 1960's attracted little public attention until late in the decade, when fundamentalists became aroused about the federally funded Biological Sciences Curriculum Study texts. which featured evolution. and the California State Board of Education voted to require public school textbooks to include creation along with evolution. This decision resulted in large part from the efforts of two southern California housewives. Nell Segraves and Jean Sumrall. associates of both the Bible-Science Association and the CRS. In 1961 Segraves learned of the U.S. Supreme Court's ruling in the Madalyn Murray case protecting atheist students from required prayers in public schools. Murray's ability to shield her child from religious exposure suggested to Segraves that creationist parents like herself "were entitled to protect our children from the influence of beliefs that would be offensive to our religious beliefs" (*47*, p. 58). I. was this line of argument that finally persuaded the Board of Education to grant creationists equal rights.

Flushed with victory. Segraves and her son Kelly in 1970 joined an effort to organize a Creation-Science Research Center (CSRC). affiliated with Christian Heritage College in San Diego. to prepare creationist literature suitable for adoption in public schools. Associated with them in this enterprise was Henry Morris. who resigned his position at Virginia Polytechnic Institute to help establish a center for creation research. Because of differences in personalities and objectives. the Segraves in 1972 left the college. taking the CSRC with them. and Morris set up a new research division at the college. the Institute for Creation Research (ICR) (*43*). Morris announced that the new institute would be "controlled and operated by scientists" and would engage in research and education. not political action (*58*). During the 1970's Morris added five scientists to his staff and, funded mainly by small gifts and royalties from institute publications. turned the ICR into the world's leading center for the propagation of strict creationism (*43*). Meanwhile. the CSRC continued campaigning for the legal recognition of special creation, often citing a direct relation between the acceptance of evolution and the breakdown of law and order. Its own research. the CSRC announced, proved that evolution fostered "the moral decay of spiritual values which contributes to the destruction of mental health and . . . [the prevalence of] divorce. abortion. and rampant venereal disease" (*59*).

The 1970's witnessed a major shift in creationist tactics. Instead of trying to outlaw evolution. as they had done in the 1920's. antievolutionists now fought to give creation equal time. And instead of appealing to the authority of the Bible. as Morris and Whitcomb had done as recently as 1961. they consciously downplayed the Genesis story in favor of what they called "scientific creationism." Several factors no doubt contributed to this shift. One sociologist has suggested that creationists began stressing the scientific legitimacy of their enterprise because "their theological legitimation of reality was no longer sufficient for maintaining their world and passing on their world view to their children" (*47*, p. 98).

There were also practical considerations. In 1968 the U.S. Supreme Court declared the Arkansas antievolution law unconstitutional, giving creationists reason to suspect that legislation requiring the teaching of biblical creationism would meet a similar fate. They also feared that requiring the biblical account "would open the door to a wide variety of interpretations of Genesis" and produce demands for the inclusion of non-Christian versions of creation (*60*).

In view of such potential hazards. Morris recommended that creationists ask public schools to teach "*only* the *scientific* aspects of creationism" (*61*), which in practice meant leaving out all references to the 6 days of Genesis and Noah's ark and focusing instead on the evidence for a recent worldwide catastrophe and on arguments against evolution. The ICR textbook *Scientific Creationism* (1974) came in two editions: one for public schools, containing no references to the Bible, and another for use in Christian schools, which included a chapter on "Creation according to Scripture" (*61*).

In defending creation as a scientific alternative to evolution, creationists relied less on Francis Bacon and his conception of science and more on two new philosopher-heroes, Karl Popper and Thomas Kuhn. Popper required all scientific theories to be falsifiable; since evolution could not be falsified, argued the creationists, it was by definition not science. Kuhn described scientific progress in terms of competing models or paradigms rather than the accumulation of objective knowledge. Thus creationists saw no reason why their flood-geology model should not be allowed to compete on an equal scientific basis with the evolution model. In advocating this two-model approach to school boards, creationists were advised (*62*):

> Sell more SCIENCE. . . . Who can object to teaching more science? What is controversial about that? . . . do not use the word "creation." Speak only of science. Explain that withholding information contradicting evolution amounts to "censorship" and smacks of getting into the province of religious dogma. . . . Use the "censorship" label as one who is against censoring science. YOU are for science; anyone else who wants to censor scientific data is an old fogey and too doctrinaire to consider.

This tactic proved extremely effective. Two state legislatures and various school boards adopted the two-model approach, and an informal poll of school board members in 1980 showed that only 25 percent favored teaching nothing but evolution (*63*).

Except for the battle to get scientific creationism into public schools, nothing brought more attention to the creationists than their public debates with prominent evolutionists, usually held on college campuses. During the 1970's the ICR staff alone participated in more than a hundred of these contests and, according to their own reckoning, never lost one (*64*). Morris preferred delivering straight lectures and likened debates to the bloody confrontations between Christians and lions in ancient Rome, but he recognized their value in carrying the creationist message to "more non-Christians and non-creationists than almost any other method" (*65*). Fortunately for him, an associate, Duane T. Gish, holder of a doctorate in biochemistry from the University of California, relished such confrontations. If the mild-mannered, professorial Morris was the Darwin of the creationist movement, then the bumptious Gish was its Huxley. He "hits the floor running," just like a bulldog, observed an admiring colleague; and "I go for the jugular vein," added Gish himself (*66*). Such enthusiasm helped draw crowds of up to 5000.

Early in 1981 the ICR announced the fulfillment of a recurring dream among creationists: a program offering graduate degrees in various creation-oriented sciences (*67*). Besides hoping to fill an expected demand for teachers trained in scientific creationism, the ICR wished to provide an academic setting where creationist students would be free from discrimination. Over the years a number of creationists had reportedly been kicked out of secular universities because of their heterodox views, and leaders had warned graduate students to keep silent, "because if you don't, in almost 99 percent of the cases you will be asked to leave" (*68*). Several graduate students took to using pseudonyms when writing for creationist publications.

To All the World

It is still too early to assess the full impact of the creationist revival sparked by Morris and Whitcomb, but its influence, especially among evangelical Christians, seems to have been immense. Not least, it has elevated the strict creationism of Price and Morris to a position of apparent orthodoxy. It has also given creationism a claim to scientific respectability unknown since the deaths of Guyot and Dawson.

Unlike the antievolution crusade of the 1920's, which remained confined mainly to North America, the revival of the 1960's rapidly spread overseas as American creationists and their books circled the globe. Largely as a result of stimulation from America, including the publication of a British edition of *The Genesis Flood* in 1969, membership in the British Evolution Protest Movement, founded in 1932, quadrupled, and two new creationist organizations sprang into existence, the appearance of one, the Newton Scientific Association, coinciding with a visit by Morris to England in 1973 (*69*). On the Continent the Dutch assumed the lead in promoting creationism, encouraged by the translation of books on flood geology and by visits from ICR scientists in 1977 (*70*). Similar developments occurred elsewhere in Europe, as well as in Australia, Asia, and South America. By 1980 Morris's books alone had been translated into Chinese, Czech, Dutch, French, German, Japanese, Korean, Portuguese, Russian, and Spanish (*71*). Creationism had become an international phenomenon.

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The University, Industry, and Cooperative Research

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Article, 24 December 1982

In this century, the time lag between the creation of a new scientific concept and its general application is usually measured in decades. Occasionally, however, the gap is compressed as a new theoretical insight moves swiftly to the stage of application and, hence, of wide, practical dissemination. We are now in the throes of such a movement in the field of applied research in genetic engineering.

At times of swift and intellectually exciting development, with the potential for enormous benefits to society and financial profits to skillful entrepreneurs, it is natural to ask questions about the appropriate relationship of universities to commercial sponsors of university research, and, indeed, about the very nature of the university. Because Yale participates actively in many developing areas of science and technology, we have been seeking answers to these questions. For the past year, a faculty Committee on Cooperative Research, Patents, and Licensing has been considering the issues raised by our increasing relationships to private commercial firms. On the basis of the committee's recommendations, and in consultation with the Research Advisory Board, chaired by the provost, we will soon bring before the Yale Corporation the results of these deliberations. The corporation will then issue a statement of policy to govern the nature and extent of university and faculty involvement in the commercial application of our scientific and scholarly research. In this article I discuss some principles on which such a policy can rest.

The university exists to protect and foster an environment conducive to free inquiry, the advancement of knowledge, and the free exchange of ideas. Such an environment depends crucially on trust and openness, and on a clear understanding of a set of principles governing scholarly inquiry. The principles are simply stated: the university and individual members of the faculty pledge themselves to the open, unimpeded, and objective pursuit of ideas, to the exchange of ideas openly and without deceit; and to the full and wide dissemination, through teaching and written publication, of the results of scholarly inquiry. The appropriate discipline on the dissemination of ideas is the critical scrutiny of responsible experts in order to assure the general public that completeness in investigation and citation, and rigorous and logical analysis in drawing conclusions, have been applied in the work.

As the university in its corporate body pledges to protect and foster an environment conducive to free inquiry, so also must the individual members of the faculty. As that environment and those principles engage a spirit that transcends the letter of stated principles, so each faculty member must sustain the university's commitment to free inquiry by fostering a spirit of collegiality, a shared sense of respect for and trusteeship of shared values of openness and intellectual freedom that the university exists to embody in the larger society. And, as the university in its administrative body must recognize that the members of the faculty, collectively and individually, are at the core of the university; and that, on behalf of members of the faculty, it is essential to protect academic freedom as well as to foster traditions of faculty self-regulation and self-government, so also is it essential that each faculty member recognize that the primary and overriding obligation of every faculty member, in terms of his or her commitment of time, attention, and intellectual energy, is to the university, that is, to the stu-

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dents, colleagues, and general mission of the university.

These principles of free inquiry and open dissemination of ideas, as well as the values of collegiality, mutual trust, and primary commitment, exist to protect the environment for free inquiry. They also form the principles and assumptions underlying all that follows.

Both university-based research, concerned primarily with the advancement of fundamental knowledge, and industry-based research, concerned primarily with marketable application, should serve the general well-being of society albeit in differing ways. Since the knowledge typically developed in university-based research is of a fundamental nature, it will often have a multitude of potentially useful applications. Because many of these eventual applications cannot be foreseen, it is particularly appropriate that such knowledge be disseminated as widely as possible so that all may use it if they will. While private industry pursues basic research, it does so less often, in part because it is so difficult to capture an adequate financial return from such long-term, risky efforts.

Universities are marketplaces where ideas are freely available; where knowledge is pursued by way of the norms of free discussion and the free access to and exchange of information; and where the freedom to publish must obtain. In contrast to the university, the commercial enterprise is appropriately animated by the profit motive. Commercial application of new knowledge typically requires a substantial investment in applied research and development, and commonly in the equipment required by new products or methods of production. A profit-making enterprise will undertake such an investment, and all its associated risks, only when it can reasonably expect an adequate return, a return not likely to occur if competitors are first to the marketplace. The opportunity for private profit provides the encouragement for the socially beneficial application of new technology. To realize profits from technological innovation, however, a company must strive to protect its proprietary knowledge and to prevent its exploitation by commercial competitors.

The development of theoretical concepts, born in the university, and the transformation by industry of those concepts into practical application, are often complementary processes. The complementary nature of their activities, however, simply throws into relief the basic difference between universities and industries: the academic imperative to seek knowledge objectively and to share it openly and freely; and the industrial imperative to garner a profit, which creates the incentive to treat knowledge as private property.

With these underlying principles of free inquiry and free market in mind, we can now examine specific issues concerning university-industry relationships. The first is the appropriate nature of faculty involvement with profit-oriented companies, particularly such companies which seek to market new processes and products growing from university-based research. The second is the appropriate conditions of grants or contracts for basic research by existing companies to universities, especially when these conditions require some form of exclusive relationship, of license or treatment, by the university with the company as a condition to the grant. There may well be cases that are ambiguous and where reasonable people will have to wrestle with the application of whatever policy emerges. For that reason, I see the provost's Research Advisory Board playing a continuing role in administering our policy. I believe that the following considerations must be taken into account in forming that policy.

Faculty Involvement with Profit Oriented Companies

There are potential conflicts of commitment and potential conflicts of interest whenever a member of the faculty is involved with extra-university entities. Let us here consider the specific issues surrounding the involvement of a member of the faculty with a company seeking to exploit university-based research.

I doubt that a faculty member can ordinarily devote the time and energy the university requires and also pursue a substantial involvement in any such outside company. Such involvement necessarily demands great concentration and commitment, particularly at the outset or if business goes badly. When a faculty member becomes substantially involved in a company, the conflict in norms governing the dissemination of knowledge becomes very difficult to reconcile. The burden of maintaining a teaching program and two separate research programs, where the results of one research program are to be widely disseminated and the results of the other may have to be kept secret in the pursuit of commercial success, is more than even the most responsible faculty member can be expected to shoulder. Finally, such involvement risks putting one's students and research associates in ambiguous circumstances, such that the graduate or postdoctoral student would not know, when working with a professor, for whom he or she was working—the university, the professor, or the company. Of all members of the university community, the student especially ought to be working for himself or herself, and ought to be guided in research and trained in skills and techniques that are designed to produce a first-rate scholar, not profit for a company in the private sector.

I believe that if a faculty member becomes a manager of a company pursuing commercial application of his or her university-based research; or acquires, through gift or purchase, stock shares in this kind of company in such proportion to the total number of shares that he or she can have a significant effect on the decision-making of that company, then there is a presumption that the faculty member's involvement in the outside entity is substantial. In such an event, there should be a review of the relationship, the possible consequence being that the faculty member might well have to decide to leave the faculty for a limited period of time, perhaps 1 year, by taking an unpaid leave of abscence to pursue those outside interests. If, at the end of that time, the faculty member were to wish to retain the outside interests described above, then that person would relinquish tenure, if he or she had it, and assume "adjunct" status if the relevant department or school were to recommend such an appointment in the usual way. The alternative for such a person would be to sever completely all ties to the university. Were such a person to wish to become a full-time member of the faculty at a later date, such a possibility would require the availability of an open position and the use of the institution's full appointments procedure.

There are relationships of individual faculty members to commercial companies, even those using the results of university-based research, that traditionally the university has allowed and will continue to allow. In these "consulting" relationships members of the faculty provide advice to companies but do not directly manage corporate research. "Consulting" can enhance a person's professional competence, and further the mission of the university. Our rule is that a faculty member may spend not more than 1 day in a 7-day week in such a role. Thus there is a limit on the commitment of time and energy.

Serving as a consultant to a company or, within the rule of reason, accepting payment in equities from some cash-poor, idea-rich company, is less likely to create conflicts of commitment or conflicts of interest than serving in a role that has a significant effect on corporate decision-making. A faculty member who has gone beyond any reasonable definition of "consulting" has reached the point where the question arises whether he or she should remain a full-time member of the faculty.

Universities frequently require that faculty members wishing to engage in consulting obtain the permission of a chairman or dean. More recently, the Committee on Cooperative Research, Patents, and Licensing has also recommended that each faculty member provide, as part of the routine annual report to the president, a description of the commitment and the organizations involved in his or her nonuniversity professional work. This recommendation has been accepted, and it will be implemented in the coming academic year.

Such disclosure—of consulting relationships, of relationships with outside companies engaged in application of a Yale faculty member's research, or of relationships with companies that sell to the university goods or services—is, I believe, the best stay against conflicts of interest or conflicts of commitment. Disclosure of this sort recognizes that there are grey areas where reasonable people might have differing views and it provides the occasion for discussion. In such disclosure to the administration, there is no monitoring of colleague by colleague. Rather a premium is put where it ought to be, on trust and openness.

Conditions of Grants and Contracts

The second issue for university-industry relationships concerns the appropriate principles in an agreement between an established company and the university when a company wishes to support basic research in a specific area. In discussing such agreements, questions of exclusivity often arise, either with regard to proprietary information provided by a company as part of an arrangement for cooperative research or with regard to exclusive license to whatever the university is entitled to patent.

The university is the only entity that can enter into arrangements for cooperative research, and the university's position with regard to exclusive licensing agreements is the following. In general, the university would prefer to grant nonexclusive licenses, in order to make knowledge as widely available as possible. The university, however, in certain circumstances, may grant an exclusive license, thus encouraging a firm to develop an invention. It will sometimes be clear that society will be better served by the grant of an exclusive license in order to bring the knowledge to the public and that the benefits to society from such exclusivity are greater than the costs of any diminished competition.

Each individual agreement must and will be negotiated on its merits. Through such negotiations, Yale will insist on principles which seek to assure that its patentable inventions will be fully and beneficially used, and that knowledge with a potential benefit to society at large will reach the public in a timely and useful fashion.

Research grants from business firms raise other questions as well, questions that are the same as those raised by research sponsored by the federal government or by private foundations. When contemplating a prospective grant or contract with any sponsor, the university will first consider whether the potential would exist for upsetting the intellectual equilibrium and human relationships in a department were one kind of research to be funded out of proportion to other kinds of research. As an indispensable condition to arrangements for cooperative research with industry, just as with government-sponsored research, the university will not accept restriction, inhibition, or infringement upon a member of the faculty's free inquiry or capacity orally to communicate the results of his or her research. In addition, the university will not accept any restriction of written publication, save the most minor delay to enable a sponsor to apply for a patent or license. Such a del-- should not be so long as to lengt appreciably the time normally req...d to bring results into print.

Yale has, through its faculty Committee on Cooperative Research, Patents, and Licensing and its Research Advisory Board, the capacity to assess adherence to these principles and conditions. The university will only agree to arrangements for sponsored research, from any sector of society, which are compatible with its norms and mission, and will not agree to any arrangement which will impair the environment of openness and free communication of ideas.

I have by no means addressed all the issues in this area. Difficult cases and anomalous situations, requiring the patience, wisdom, and goodwill of members of the faculty and administration alike, will present themselves. I have, however, suggested here some principles and general guidelines. We have responsible forums to explore these suggestions and to assess the cases that exist or that will arise.

The opportunities for cooperative research between universities and industries are very exciting and can rebound to the benefit of society. These opportunities should not drive us toward arrangements for basic research that abridge our principles. Nor should the university ignore the potential availability of funds from commercial sponsors. We should negotiate appropriate arrangements, openly arrived at, that can further our mission. The constant challenge for the university is to know in clear and principled terms how to cherish learning, and its pursuit, for its own sake, and how to assist in bringing the results of free inquiry to the rest of the society for the good of the public.

There are many good reasons for the great current attention to university-industry relations, but there are troublesome reasons as well. One is that universities are now unusually hungry. There is nothing wrong with hunger. But a hungry man may cut corners in his rush to nourishment, and he may be taken advantage of in negotiations. Fear of this is leading to the threat of protectionism, as exemplified by recent attempts to classify or otherwise control access to university research, including that joint with industry.

In designing university-industry connections, protecting interests by high-level negotiations is wrong. The adversary process, and the proliferation of lawyers to manipulate it, was never intended to apply to joint programs, where the output is also joint, where it is by no means a zero-sum game, and where the accomplishments for all participants are far greater if speed and simplicity of negotiations take the place of exquisitely detailed legal contracts. Protectionism is dangerous and habit-forming. Circumstances exist where it is appropriate, but only for a short time. One of the few essentials of agreements is that any secrecy or interference with open publication or student interaction should be strictly temporary.

The dominant problem of supporting enough basic research in universities will remain. This must continue to be a federal responsibility; no company or industry can harvest the results soon enough to justify any investment larger than keeping a window on basic research and a conduit for the movement of bright young people into the company. Hard work in the universities will lead to important cooperative research agreements with industry, but unremitting effort will be required to maintain or enlarge the basic research on which all else rests.

But there is far more at stake than support for universities. University-industry interaction should not be looked upon as support at all, but as an absolutely necessary part of the survival both of American institutions and of the American economy. As the economy stumbles, protectionism of all kinds becomes rampant, and everyone loses. From the university's standpoint, cooperative projects with industry affect graduate (and even undergraduate) work in healthy ways. To use Harvey Brooks's phrase, giving students "respect for applied problems" is an important part of their education. Wisdom begins when students (and even professors) realize that an invention is not a product and a product is not an industry. What is perhaps most at stake is attracting some of the ablest young people to those fields that can make a difference in the survival of our society. Particle physics ought to be done, just as art galleries ought to be maintained, and the richer the country is the more particle physics and art galleries it should support. But it would be a disaster if protectionism, of either the government or the industry variety, were to discourage some of the best young people from going into applied fields.

Universities are resilient institutions. We are sufficiently strong in depth that we can afford to experiment. If we move too fast or in an inappropriate direction, we can pull back. Our resilience means that we do not have to be so protectionist that we become precious. After all, what we properly call "integrity," the rest of the world calls "selfishness." Incidentally, I prefer Eric Ashby's words "inner logic" to "integrity." We must be careful to preserve our inner logic, certainly, and incidentally our 501(c)3 status (or the similar tax-exempt status of our affiliated foundations). But the public at large is less interested in the precise boundaries between universities and industry or universities and government; after all, the public is paying for *all* of these entities. Above all we should indulge in protectionism of a higher sort: We should protect our willingness and ability to take risks, to experiment, to undertake new directions, and to help a new generation prepare themselves for lives of service.—*Adapted from an address at the Conference on University-Industry Relations, Madison, Wisconsin, 16 November 1982.*

Science and Technology in a World Transformed

David A. Hamburg

Article, 1 June 1984

My father was born in 1900 and died in 1984. In those years, he participated in one of the most drastic transformations any species has ever experienced. It is difficult to comprehend the extraordinary changes that people have witnessed in this century as a result of advances in science and technology. We are so deeply embedded in the present that it takes a difficult mental effort to comprehend the time scale of human life on Earth and the recency of the kind of world that we live in now. Human ancestors have been separate from the apes for about 5 to 10 million years. For almost that entire time, there were fewer than 1 million people on earth, subsisting by hunting and gathering in small, nomadic groups. Agriculture and large, settled populations have existed for much less than 1 percent of that epoch, and our technical world has been present for a mere moment in the time scale of human evolution. The way we live today is, in many important respects, a novelty for our species.

Our ancestors—prehuman, almost-human, and distinctly human—lived in

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small groups in which they learned the rules of adaptation for survival and reproduction. They used simple tools to cope with the problems of living and struggled to obtain more control over their own destiny. For the most part, they were vulnerable to the vicissitudes of food, water, weather, predators, other humans—whatever nature might bring. Their world began to change with the onset of agriculture about 10,000 years ago. But the most momentous changes occurred with the industrial revolution two centuries ago, and above all with its pervasive implementation in the 20th century.

Much of the technology that structures American lives today, in ways we largely take for granted, is of extremely recent origin. In 1900 there were few automobiles or household telephones, motion pictures were just getting under way; there were no household radios, no airplanes, no televisions, no computers. Today it is almost impossible to imagine a world without these technologies—and in this country, without their presence everywhere. What a difference a century makes—indeed, even a decade as events move now.

Opportunities and Complications

The opportunities arising from our profoundly enhanced capability in science and technology are visible in every sphere of human life—in medicine and public health, in agriculture and food supply, in transportation and communication, and elsewhere. Every advance has brought side effects—like a new medicine whose benefits are clear but whose complications take considerable time to appear. But complications there are, extreme population growth in much of the world, drastic urbanization with its crowding of strangers beyond any prior experience, environmental damage, resource depletion the immense risks of weapons technology, and new patterns of disease—all are largely products of changes that have occurred only in the most recent phase of human evolution. We have rapidly changed our technology, our social organization, our diet, our activity patterns, the substances of daily use and exposure, patterns of reproductive activity, tension-relief, and human relationships. These changes are laden with new benefits and new risks, and the long-term consequences are poorly understood.

Many of the technological changes are exceedingly attractive since they free our species from hardships and dangers: they provide gratifications that were beyond reach at least for most people in the past. In many respects, ordinary citizens live today as kings of an earlier time never could.

The automation of the household has drastically reduced the requirement of physical labor at home. Its social implications have been far-reaching. Similarly the revolution in telecommunications has come close to making this country a single large community in some respects and may one day have a similar effect on the world as a whole.

Technological innovation is now associated with far-reaching and extremely rapid changes in the nature and scope of work available in this and other countries. The pervasive mechanization of work appears to be tangible on the horizon, not as a distant prospect, but as a powerful current gathering momentum and affecting the entire economy in far-reaching ways. Benefits in productivity are clearly visible. Concomitant social dislocations are not as visible but are as likely to occur.

Growing Pressures on Resources

There are in many parts of the world today strong tensions between population pressures and available resources. These tensions have explosive potential within countries and also for international conflict. It has become a matter of our enlightened self-interest as well as of decent human concern for us to try hard to understand our species in its worldwide interdependence, paying just as much attention to the Southern Hemisphere as to the Northern Hemisphere. For our own sake as well as theirs, we need to strengthen our ties and work cooperatively with people in developing countries toward the reduction of poverty, ignorance, and disease.

Since the sciences provide our most powerful problem-solving tool, it is essential that we bring their strengths to bear on these problems to the maximum extent possible. The task requires an intensive effort now to learn what can be extracted from the efforts at economic and social development during the past several decades, sorting out failures from successes, looking for strengths on which to build future efforts of practical value.

The problems that face modern industrial nations are related to sociotechnical conditions that have appeared very recently in the evolution of the human species: the magnitude and rate of change make it difficult for us to devise and implement solutions to these problems. In developing countries, the shift from old to new ways has occurred even more rapidly, and change has been partly imposed from outside. These nations confront exceedingly difficult problems. Across much of Africa, Asia, and Latin America, explosive population growth and abject poverty have contributed to extremely severe health problems that stand as an enormous obstacle to sustained development and social progress. Their burden of early death and long-term disability is exceedingly heavy. Infectious diseases take the lives of a great many infants before they reach 1 year of age and handicap for life many of those that survive. Susceptibility to a wide range of diseases is heightened by the marginal character of subsistence.

Global Interdependence

This set of facts raises ethical questions for countries with strong scientific capability. In recent decades, the United States has given little attention—in research, education, and practice—to some of the most important disease problems in the world today. How can we and the other more developed nations help improve health in developing countries? One way in which we are especially able to help is through research—including capacity-building in developing countries so that they can tackle their own problems. Priority areas are, (i) epidemiological assessment of specific needs, (ii) applying molecular biology to parasitic diseases, (iii) devising a wider array of fertility-control methods with special reference to cultural acceptability and feasibility of use, and (iv) clarifying relations of health and behavior, with special reference to breast-feeding, nutrition, child care, sanitation, water use, and family planning. What is fundamentally needed is a heightened awareness within the scientific community of the opportunities that exist, since even a modest shift of attention to such problems could yield major benefits.

Difficult as these problems are, and crying out for a larger place in the work of the scientific community, another aspect of our global interdependence is even more urgently in need of attention. The overriding problem facing humanity today is the threat of nuclear holocaust. Humanity's capacity for destruction has radically outstripped its institutional capacity to control intergroup violence.

That violence is rooted in the nature of the human species. Mass expression of violence—war, terrorism, and genocide—persists throughout the world, and no people should be considered incapable of it. But the invention and deployment of nuclear weapons represents a qualitative break in the history of violence. It is now possible to destroy human life on Earth. Both the United States and the Soviet Union probably have the capacity to do that, or at least to make the human condition unbearable. In 1983, new evidence of the incredible devastation—immediate, long-term, and permanent—of nuclear war was brought to light.

Former Secretary of Defense Harold Brown points out that in the first half-hour of a nuclear war there might well be 100 million deaths each in the United States, in the Soviet Union, and in Europe. It is plausible that a billion people would die in just a few weeks. In point of fact, there is nothing in our history as a species to prepare us to comprehend the real meaning of such devastation.

Human societies have a pervasive tendency to make distinctions between good and bad people, between heroes and villains, between ingroups and outgroups. It is easy for most of us to put ourselves at the center of the universe, attaching a strong positive value to ourselves and our group, while attaching a negative value to certain other people and their groups. It is prudent to assume that we are all, to some extent, susceptible to egocentric and ethnocentric tendencies. The human species is one in which individuals and groups easily learn to blame others for whatever difficulties exist. But in the present predicament around nuclear conflict blaming is at best useless and most likely counterproductive.

A new level of commitment of the scientific community is urgently needed to reduce the risk of nuclear war. This requires a mobilization of the best possible intellectual, technical, and moral resources in a wide range of knowledge and perspectives. A science-based effort is essential to maximize analytical capability, objectivity, and respect for evidence—the outlook that is characteristic of the scientific community worldwide.

These efforts should bring together scientists, scholars, and practitioners in order to clarify the many facets of avoiding nuclear war. To generate new options for decreasing the risk, we need analytical work by people who know the weaponry and its military uses, people who know the Soviet Union, people who know international relations broadly, people who know the processes of policy formation and implementation, people who understand human behavior under stress, and people who understand negotiation and conflict resolution. Such analytical studies are likely to be more useful if they take into account policy-makers' perspectives, and policy-makers can benefit greatly from ready access to new ideas, a wider range of options, and deeper insights.

Commitment to Science Education

The rapid acceleration of technological and social change in recent decades sharply heightens the importance of our educational institutions. Indeed, during the past year we have experienced a sort of national rediscovery of education, particularly with reference to science and technology. We must address fundamental challenges in education.

1) How can we give all our children, regardless of social background, a good opportunity to participate in the modern technical world? In this time of high unemployment we must especially consider employment opportunity.

2) What constitutes a decent minimum of literacy in science and technology that should be part of everyone's educational heritage?

3) Given the rapidity of sociotechnical change, how can we make lifelong learning a reality so that people can adjust their knowledge and skills to new circumstances?

4) Since educational institutions will more than ever be trying to hit a moving target as they prepare people for unpredictable circumstances, how can we prepare for change itself?

5) How can we enlarge that talent pool so that we can find promising people for science-based careers, regardless of their socioeconomic background?

6) How can we broaden the spectrum of the sciences so that modern education will become increasingly informative with respect to the human experience?

7) How can the educational system foster a scientific attitude that is useful in problem-solving throughout the society, in relating scientific principles to the major issues on which an informed citizenry must decide?

8) How can we achieve an informed worldwide perspective in an era of profound interdependence?

The American dilemma in science education involves a remarkable paradox. We have the largest and probably the most respected scientific community in the world, yet our precollegiate science education is at a low ebb. Is there a way to resolve this paradox creatively?

We must seek mechanisms to link the science-rich sectors of our society and the science-poor sectors—that is, connect the scientific talent of universities, colleges, corporate laboratories, and national laboratories with the elementary and secondary schools, thereby strengthening national capability for broad education in the sciences: physical, biological, and behavioral.

The linking of science-rich to science-poor sectors as partners is a means to both teacher education and curriculum development. We can learn from the successes and failures of the curriculum reform movement that followed Sputnik. Improvements in education can flow from the collaboration of classroom teachers with subject matter experts (for example, in physics, chemistry, or biology) and also with scholars in the field of human cognition and learning. Such collaborations could be an important step toward incorporating teachers of science into the scientific community. While major efforts must be made to improve the salaries of teachers, and especially those of science teachers, it is equally important to find ways to bolster respect for their profession and strengthen their morale. Participation in the scientific community would be helpful in this regard.

Closer links of elementary and secondary schools with colleges, universities, corporate laboratories, and government laboratories could include summer institutes for teachers, Saturday activities for teachers throughout the school year, summer jobs in science for teachers, and the preparation of curricular materials. Leadership from different sectors of society will be necessary for the major upgrading that is required. The schools are central to the effort, but they alone do not have the resources or the clout to do the job.

The Transformed World

The rapid, pervasive, and truly unprecedented transformation resulting from science and technology is a central fact of our lives. It calls for the strengthening of institutional capability for objective, scholarly analysis of critical issues based on a broad foundation of knowledge and experience.

Colleges and universities, academies, and free-standing institutes can mobilize a wide range of talent to address the great issues of our time in a sustained, fascinating, and effective way. They can give us a better chance to get the complex facts straight and to clarify the most promising options—all this in a way that is credible and even intelligible to nonspecialists. Such efforts can be helpful to open-minded policy-makers, but also—and perhaps more importantly in the long run—to the education of a broadly informed public on the great issues of our time and the policy choices available to us.

Surely the problems associated with the social and economic concomitants of technological change are not insuperable. The opportunities provided by the advancing technology, if judiciously utilized, suggest that it is worth a lot of trouble and hard work to find ways to make a decent social adjustment. We will require broad, mul faceted analytical work to understand more deeply what is going on and to anticipate insofar as possible the likely consequences of major technological trends.

To deal effectively with real-world policy problems requires novel conjunctions of knowledge and talent. Typically, many different facets of a complex issue must be taken into account. The great problems do not come in packages that fit the traditional disciplines or professions, however excellent they may be. Organizations such as universities, scientific academies, and research institutes can make a greater contribution than they have in the past if they can organize effectively to share information, ideas, and technical abilities widely across traditional barriers and systems. Of course, most advances in knowledge require specialization. Yet for crucial social purposes these pieces must somehow be related to each other.

A particularly valuable undertaking is the intelligible and credible synthesis of research related to important policy questions. What is the factual basis drawn from many sources that can provide the underpinning for constructive options in the future? This is especially significant in view of the fact that pertinent information is almost always widely scattered. Moreover, it is very difficult for the nonexpert and sometimes even for the expert to assess the credibility of assertions on emotionally charged issues. In the current process of world transformation, such studies are needed to tackle vital and complex issues in an analytical rather than a polemical way.

Whether it be toward avoiding nuclear war, strengthening education in the sciences, or fostering human resources in developing countries, there is a precious resource in the great scientific community of the United States—and its links to the worldwide scientific community—which can be brought to bear on these crucial problems. This involves activation of a wide range of the sciences and an unusual degree of cooperation among them. Furthermore, it involves linking analytical work with education in a variety of modes, for the general public must take an informed part in the decisions that affect all our futures. The scientific and scholarly community can deepen its contribution to pressing social concerns if it is informed and stimulated by those on the firing line, whether the latter be engaged in teaching poor children, struggling with policy dilemmas, or coping with international tensions. There can be a mutually beneficial interplay between social concerns and basic inquiry.

Conclusion

There is little in our history as a species to prepare us for this hypermodern world that we have so rapidly made. The transformation of the world will press us toward transformation of our institutions—to keep up with events, to understand below the surface, to look ahead and prepare, to enjoy the fascination of deep insights, to make wise use of technology, to relieve poverty and disease, above all to resolve the deadliest of conflicts. In the historic effort to avoid unmitigated disaster and fulfill the potential of our species, science can help profoundly and in novel ways. But to do so, the sciences must transcend their traditional boundaries and achieve an unprecedented level of mutual understanding, innovation, and cooperation. These efforts will have to go across disciplines, across sectors of societies, and across nations.

Let us hope that fundamental values of freedom, curiosity, opportunity, diversity, excellence, and human decency will guide our institutions as they evolve. These enduring values can make it possible for us to cope with the great problems of our time, to work steadily towards the humane uses of science and technology, and to take advantage of unprecedented opportunities that are emerging for the benefit of people everywhere.

Guidelines for Industry-Sponsored [illegible]

[illegible] Kukich

[illegible]

Any discussion of cooperative endeavors between universities and industries inevitably focuses on the disparities between the two. Universities operate under several basic principles: that their primary functions are to preserve existing knowledge and to seek and disseminate new knowledge, that freedom is essential to inquiry, and that research and teaching are inseparable. For industries, making a profit and providing useful products and services are the primary functions, financial rewards are essential for assuming risks, and the freedom to carry out or support all types of research is critical. Focusing on these differences has engendered mistrust and fostered misunderstandings between the two sectors (*1*).

Many of society's needs could be met most effectively if universities and industries joined their broad range of capabilities and facilities (*2*). Not only would society benefit from the fruits of cooperative research, but the private and academic sectors themselves would stand to gain much if they capitalized on their differences and developed ways to reinforce their strengths. Universities need the financial support and the technical

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know-how that industry can provide, whereas industry looks to the nation's universities for new talent, new ideas, and basic research facilities (*3*). The increasing complexity of product and process development has made it difficult for industrial laboratories to have the expertise needed to keep up with technological and scientific advances (*4*).

However, cooperative research relationships require careful management (*3*). The key to their success lies in anticipating potential problems and developing guidelines for averting them or dealing with them effectively if they occur. The following are among the most critical guidelines for universities establishing research relationships with industry. Although following them will not guarantee perfect interaction between the two types of organizations, it will promote the benefits of such interaction while limiting conflicts.

Ten Management Guidelines

Retain publication rights. This policy is based on the fundamental purpose of a university—to preserve and transmit existing knowledge and to generate new knowledge. As the primary trustee of the world's knowledge, the university has an obligation to society; the ability to fulfill that obligation is dependent upon the university's freedom to publish. The freedom to publish is critical not only to faculty but also to students, for industries often use publication as a criterion of research competence when evaluating students for employment (*5*).

Industrial sponsors frequently have proprietary interests to protect, and they will ask to review proposed publications in order to ensure that proprietary information is not revealed to the public. This request creates a potential conflict with the university's need to publish research results. However, the conflict is easily resolved if the university agrees to keep confidential any proprietary information it acquires to conduct the research, and the industry agrees to publication of new research findings.

The industrial sponsor has the right to review publications as well as the right to request that any proprietary information be removed if it has been included in a publication. This right should in no way be regarded as veto power, however. The contract should stipulate that the sponsor's approval shall not be unreasonably withheld. If opinions differ as to whether or not the company's rights have been violated, the two parties must attempt to reconcile these differences, with the ultimate decision being left to the university.

Publication may pose a problem in research interactions that generate inventions. The property rights to inventions are best protected by patents, but preparing patent applications is complicated and time consuming. Problems arise because publication of an invention prior to filing a U.S. patent application bars foreign filings, and U.S. applications must be filed within a year of publication. This issue can be resolved if the university agrees to delay publication for up to 6 months in order to give the sponsor time to file a patent application (*6*).

A graduate thesis becomes a publication as soon as it is placed on the shelf of a university library. If a thesis discloses patentable material, the publication dilemma can be resolved by sequestering the document for no more than 1 year. This policy should require that the student, the faculty advisor, the department chair, the dean of the college, and the dean of the graduate school agree on the need to sequester; the company must supply satisfactory arguments to convince them of this need.

Sequestering a thesis usually benefits the student, because at most universities inventors share the income derived from inventions. As industrial sponsors usually have some rights to the research through licenses, most will be amenable to this solution. Under no circumstances, however, should this policy result in a degree being delayed, the thesis should merely be held out of circulation.

Retain ownership of all patents. Universities have an obligation not only to disseminate knowledge through publications but also to make a best effort to bring inventions to the marketplace. Although it is usually advantageous for companies to market their inventions, they may decide as part of a profit-motivated strategy that certain inventions should be withheld. The principal way for a university to prevent a company from withholding the products of a joint research program is to retain ownership of all patents issued for faculty, staff, and student inventions. Property rights may be transferred from a university to an industrial sponsor by a license agreement, but that same agreement should provide the university with march-in rights if, after some clearly defined period, the invention is not exploited by the licensee.

The details of a license, except for the above stipulation, should be left negotiable; a license can be exclusive or nonexclusive, royalty bearing or royalty-free, depending on the circumstances. Most universities prefer to grant nonexclusive licenses, but exclusive licenses are frequently granted at the request of the industrial sponsor (*7*). For a royalty-bearing license, guaranteeing minimum royalties 2 or 3 years after licensing provides an incentive for commercialization. This protects the inventor's rights as well as those of the institution, for most universities share a significant portion of the royalties with the researcher (*5*).

The university's obligation to protect the inventor's rights means that royalty-free licenses should be granted only under exceptional circumstances, but industries may feel that a royalty-free license is justified because they supported the work. How, then, does a university deal with a sponsor whose policy demands ownership of the patents from a sponsored research agreement? One possibility is for the university to request that a portion (at least 15 percent) of the research support be designated an unrestricted research grant that can be used to reward the inventor. In all cases, patent ownership and distribution of royalty income should be stipulated prior to initiation of the research.

Establish copyright policies for software. As computers have assumed an increasingly important role, academic institutions have devoted substantial funds to purchase computer hardware and software, to hire new faculty and staff, and to house this equipment and personnel. These investments have generated a large amount of computer software ranging from personal programs of little value to anyone other than their authors to general-interest programs of substantial academic and commercial value. As software technology continues to grow, copyrights will assume greater economic importance at universities (*8*).

Software is usually developed under policies based on the traditional scholarly activities of faculty, particularly book authorship. Most universities offer faculty members complete ownership of all scholarly works protected by copyright (*9*). The university's contribution to such work typically comprises release time, secretarial support, supplies, and office facilities—all at little incremental cost to the institution.

Nevertheless, because the creation of software entails significant use of university personnel, equipment, and funds, the university should claim some share in the benefits from its commercialization. Universities therefore need policies to protect their rights in this area, but because the issue is relatively new, such policies now vary widely from one university to another. Some hold that the software belongs wholly to the institution because it is a piece of technology produced with university equipment; others treat programs as literature and assign all rights to the author (9).

One way to deal with this issue is for the university to set a threshold below which it has no interest; that threshold is usually the point at which computer software becomes marketable. This eliminates most of the problem. For cases where the software is commercialized, the university should not expect to recover all developmental costs; however, marketing expenses and legal fees should be recouped before any income is shared with the author. The university's patent policy can serve as a guide to the equitable apportionment of income; this policy usually allocates the greater portion of the income to the institution—a two-to-one ratio is typical.

Minimize the use of proprietary information in research and do not require graduate students to sign confidentiality agreements. Most universities will not undertake confidential research for the federal government or for industry. The reason for this policy relates, again, to the fundamental purpose of a university. If research results cannot be published, the university has failed to fulfill part of its obligation to society.

However, a university can accept proprietary information from an industrial sponsor if it is used to generate new knowledge that, in turn, is publishable. Although receipt of this information is not a problem in itself, the institution should minimize the amount of proprietary information it accepts in order to avoid conflict when the new research results are published.

The university should stipulate that it will accept proprietary information only when it is in writing and designated as proprietary. The only individuals with authorized access to this information should be full-time faculty or staff, students should be insulated from this type of data. Industrial sponsors may require investigators to sign a confidentiality agreement prohibiting them from divulging the information for a given period, often up to 5 years (5). Such an agreement is an unrealistic burden to place on a student who may soon be seeking employment in the same industrial sector as the sponsor. The education of students is a primary function of the university, and students should be free to bring to their future employers all of the knowledge they have gained.

If the student must be exposed to proprietary information, the confidentiality agreement should be between the sponsor and the student. The university cannot be responsible for the enforcement of such an agreement, and this position should be clearly stated.

Create research units with faculty and students, and hire full-time researchers to staff such units if necessary. Many of the most successful research relationships between universities and industries have resulted from universities creating research units devoted to specific problems or areas. Often those research units are a response to an industry identifying a research need and recognizing that a given university has the expertise to do the work (*10*). These centers or institutes are staffed with faculty and students as well as a contingent of full-time, nonfaculty researchers, many with industrial backgrounds. The combination of faculty, students, and professional researchers enables these centers to respond to industrial needs in a more timely fashion than is possible with the more traditional approach to research found at most universities.

These units do not operate as private consulting firms. They work in academic-year cycles (that is, a minimum 1-year project duration), and—even more important—they have a strong educational focus. Students associated with these centers as part of their academic training gain a great deal from such work: financial stability for their research, communication skills, job opportunities, and close interaction with industry, including contact with management, exposure to technical knowledge, and use of equipment and materials (*11*).

The primary difference between a research center and a more conventional academic department is that productivity is enhanced by the full-time professional researchers. (An old adage seems particularly appropriate here. ten professors at one-tenth time do not a person-year make.) Moreover, a full-time researcher with industrial experience tends to produce more inventions than the typical faculty member. This is not to suggest a qualitative difference between the work done by the professional researcher and that of the faculty member, but the industrially trained individual can more readily recognize an invention. In fact, working with professional researchers frequently helps faculty identify inventions arising from their research results. These research units are not only productive but also likely to attract industrial support because the availability of researchers with industrial experience enhances communication between the two sectors.

Faculty should not be permitted to consult with sponsors in the sponsored research area. One of the perquisites of faculty at most research universities is the right to consult privately on a limited basis. Consulting enhances both the professional competence of the individual and the reputation of the institution (*7*), but faculty members must balance their outside consulting activities with their institutional responsibilities for teaching and research (*12*). A potential conflict may arise, for example, if faculty members are invited by the sponsors of their university research to consult in the same specific research areas. Any know-how or patents arising from this work could accrue to the sponsor through the consulting agreement, whereas the work done on the sponsored research project through the institution was probably designated university property in the institutional agreement or contract.

The key factor in implementing this rule is to define clearly the scope of the research being supported by the industry and allow the rule to apply only to that narrow area. This approach frees researchers to consult with industrial sponsors in their general fields of expertise. Unfortunately, enforcement of this rule is virtually impossible; its success depends on the integrity of the researcher.

A faculty entrepreneur's company should not be permitted to sponsor his or her research on campus. This guideline arises from another recent development. In the past, faculty members consulted with outside firms, but few started their own companies. However, much academic research is now easily and rapidly translated into products and processes with commercial value. In the area of biotechnology alone, for example, more than 200 companies have been established within the last 4 years (9).

Obviously new rules are needed to deal with this situation. Implementing them requires first that entrepreneur be defined. If a company is wholly owned by a faculty member, then that individual is clearly an entrepreneur. However, when the faculty member owns a percentage of an outside company, some gauge is needed to determine when that percentage is high enough to present a conflict of interest. While it is difficult to establish a firm rule for this situation, a general guideline is that more than 10 percent ownership in a company constitutes an equity interest.

Once faculty entrepreneurship has

been established, the faculty member's on-campus responsibilities and off-campus venture must remain completely separate (*13*). Objectivity is difficult to achieve when a researcher has responsibilities both to the institution and to a venture in which he or she has a vested interest, yet objectivity is essential to the judgments needed for education and scientific research (*14*).

The key issue is accountability: when the institution accepts a research grant, it accepts the responsibility for accomplishing the work, and the sponsor expects the project to be completed correctly and on time. A researcher who is also a sponsor, however, becomes both judge and jury. He or she is hardly in an appropriate position to decide, for example, whether or not a late completion report is acceptable. The institution must deal with the sponsor in one way and the faculty member in another; when they are one and the same, this becomes impossible. The faculty entrepreneur's company should, however, be allowed to sponsor other research on campus, even within his or her own department, because this situation allows a clear separation of roles.

The extension of this prohibition against faculty sponsoring their own work is that a researcher's graduate students should not be employed by the researcher's company either. The reason is simple. Ideally, the choice of a graduate student's research topic is a free one; in reality, however, the choice is often made after the faculty adviser has been consulted and the availability of funds considered. Even given these limitations, the student should still have some choice. However, that choice may be undermined if the faculty entrepreneur's company is supporting the student's work: the research may be directed toward the specific goals and needs of the company. Moreover, the work may be accelerated to meet the company's schedule rather than the student's needs. These abuses may be rare, but guidelines are needed to prevent them from occurring.

A corollary to this guideline is that faculty entrepreneurs should never be allowed to lease or use space in their university departments for private business. The resulting mixture of personnel and facilities can confuse colleagues, students, support staff, and outsiders in determining whether a given project is part of an institutional responsibility or a private business activity (*7*).

Beware of international agreements. A common mistake universities make in drawing up international agreements lies in assuming that the elements of a successful domestic agreement can be applied. A second error lies in assuming that experience with one foreign sponsor is applicable to another. Cultures vary so widely that experience with one country may help very little in making arrangements with another. Any guidelines or policies established by a university regarding international agreements must therefore be sufficiently flexible to allow for case-by-case modifications.

A major difference between domestic and international contracts is that the negotiation phase of the latter can often generate expenses that are substantial in proportion to the actual research support. Aside from the staff commitment to these negotiations, legal fees both at home and abroad can mount up quickly. Unfortunately, recovering these negotiating and legal expenses is difficult. To be realistic, the university should double its estimate of the expenses that will be incurred during the preproposal stage.

Another problem is that tax-exempt U.S. institutions seldom have the same status overseas. Income derived from licenses on patents can be taxed by the foreign licensee's government; as a result, the net income from the licensed product or process can be considerably below the negotiated amount.

Translation can present yet another problem with such arrangements. Most contractual documents and deliverables (completion reports, for example) are written in English, and the details as well as the overall spirit of these documents must be preserved when they are translated into the sponsor's language. An attorney well versed in the sponsor's language is almost essential.

If, after considering all the risks of such an association, a university decides to enter into an international agreement, several suggestions can guide those involved in drawing up the contract. The most important is to clarify the nature of the project immediately so that team and technical requirements, cost estimates, logistic support, and other relevant factors can be specified.

While the guidelines for an international agreement must be general enough to allow for variations in details from one case to another, the contract itself should be as specific as possible. Provision should be made for scope of the work, financing, salaries, medical benefits, language training, reports, publications, patents, subcontracts, taxes, travel, transportation and storage of materials, equipment, training, termination, and disputes. A complete and explicit contract is the university's best antidote to disaster (*15*).

Share personnel and equipment with industry. Although there is renewed interest in cooperation between universities and industries, in fact, the two parties have always interchanged ideas and people. Many industrial employees have held adjunct appointments at universities just as faculty have often had formal consulting agreements with industries. More recently, however, arrangements have been made whereby a researcher's time is shared; the sharing need not be on a 50:50 basis, but a split less proportionate than 25:75 is probably unwise.

Such arrangements have potential pitfalls. Industrial research usually produces proprietary information, whereas university work is expected to lead to publications. The question of patent or copyright ownership must be considered also, especially since many companies do not share income with authors and inventors as universities do. Another problem may arise from the industrial prohibition against consulting with another firm, contrasted with the rather liberal policies of most universities regarding faculty consulting. A researcher working for both a university and an industry would probably be restricted to the combined salaries of the two positions and not have the opportunity for extra compensation through consulting.

However, the advantages of sharing outweigh the problems in most cases. Researchers in industry have access to excellent facilities and support staff, and they usually work with a group of researchers in a given area. The advantages of a university association are the stimulation derived from working with students and interacting with a more diverse group of colleagues (*16*).

Universities and industries can share not only personnel but also equipment. Sharing is particularly beneficial with sophisticated research equipment that would be used infrequently by industry. In such a case, several companies can contribute to the purchase of the equipment, house it at a university, and schedule its use. The major advantage to the university lies in having state-of-the-art equipment for faculty, students, and staff to use when the sponsor is not using it. The university should offer industrial personnel access to the equipment and make appropriate arrangements for liability protection. Cooperative arrangements can help universities fill the needs created by declining federal support of equipment and facilities (*17*).

Prepare a model research agreement for potential industrial sponsors. As industry increases its support for research, the need for universities to develop model agreements for potential industrial sponsors becomes more compelling. A university's negotiating position is strengthened when it presents standard provisions to prospective clients early on (5). All of the above guidelines should be incorporated into the model agreement.

The university should also have a standard international agreement. Although the needs, philosophies, and expectations of foreign sponsors vary, a model agreement can still serve as a sound basis to begin negotiations for overseas work.

The university's first contact with a potential industrial sponsor is often through the faculty researcher (*18*). Faculty should therefore be familiar with these model agreements and present them early in discussions with potential sponsors. And, again, as many details as possible should be stipulated in the agreement, whether it be with a domestic or a foreign sponsor, to circumvent problems as the research association develops. Although many clauses in such an agreement will be open to negotiation, having the framework of the agreement available from the beginning can avert misunderstandings. Another advantage is that a model agreement, although subject to variation through negotiations, demonstrates to potential sponsors that the university approaches all sponsors with the same set of rules.

Conclusion

There is much to be gained from increased interaction between universities and industries, both for the institutions themselves and for society as a whole. Innovation cannot occur without basic research as input, and basic research that does not lead to technological innovation in the form of marketable products and processes does little to better our quality of life. Too much emphasis has been placed on the dichotomy between the "pure," basic research done at universities and the applied research considered the province of industry. Rapid growth in scientific advancement has blurred the line separating the two (*16*). Increased interaction between universities and industry is inevitable as the federal government contributes an ever smaller portion of the research dollar; such relationships may prove to be the best way to serve the needs of all involved, including the public (*3*).

Many have expressed concern that universities are selling out by becoming so closely allied with industry (*19*). But cooperative research can threaten academic freedom only if universities allow it to do so. If, instead, they monitor themselves closely, they can reap the rewards of interaction with industry without sacrificing the principles that constitute their very foundation. But university research administrators should not view our guidelines as an exhaustive set. Many more will be added as faculty researchers, corporate sponsors, and university administrators gain new insights from their experiences.

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The Social Process of Science

"Being well-informed about science is not the same thing as understanding science." Therewith, James Bryant Conant discounted the popular interest in science roused by the thunderclap that ended World War II. Now as then, in a world transformed by the application of scientific knowledge, people put that knowledge in the same category with what they know by revelation or other received authority. What needs to be understood is how, scientifically, we come to know what we know.

Scientists know nothing for certain. The advancement of science is a social process, a pubic process, and yet an intensely private one. Societies that would enjoy its material benefits must understand science in both its aspects.

"The truth of an idea," William James perceived at the turn of this century, "is not a stagnant property inherent in it. Truth happens to an idea. . .[by] the process of. . .its verification." In the private process, the scientist must face the singular loneliness of the sovereign He can accept no authority but his own conscience and judgment.

The work proceeds in ways very different from that suggested by its impersonality in formal publication. "The process I want to call scientific," the physicist Percy Bridgman wrote, "is a process that involves the continual apprehension of meaning. . .accompanied by the running act of checking to be sure that I am doing what I want to do, and of judging correctness or incorrectness. This checking and judging and accepting. . .are done by me and can be done for me by no one else. They are as private as my toothache, and without them science is dead."

The intensity of this private process—its toothache—is raised by the fact that it is integrally public. It is intended for publication. Without publication, science is dead.

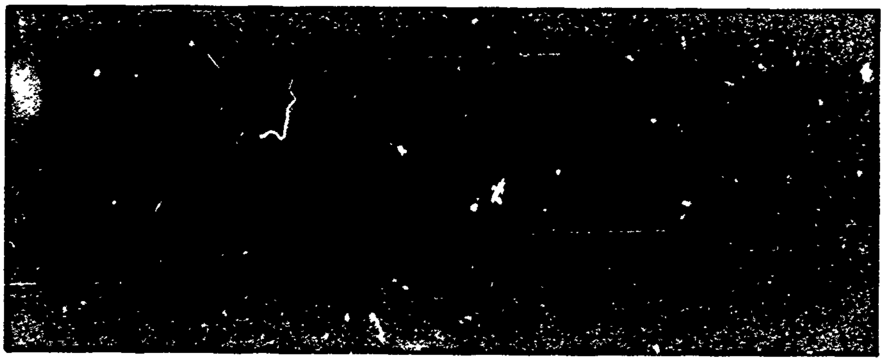
Upon publication, verification of the work proceeds. As the sociologist Robert Merton has observed, "Only after the originality and consequence of the work have been attested by significant others can the scientist feel reasonably comfortable about it." Merton's term for this public process is "'Communism'. . . .The substantive findings of science are the product of social collaboration and are assigned to the community. . . .The scientist's claim to 'his' intellectual 'property' is limited to that recognition and esteem which, if the institution functions with a modicum of efficiency, are roughly commensurate with the significance of the increment brought to the common fund of knowledge."

The remarkable fact, established by the open public record of science, is that this social process functions with high efficiency. It is not that scientists are more dedicated, honest, and selfless than other citizens; they are disciplined to behave that way by their collaboration. Error and fraud are exposed sooner rather than later by the communal process of verification. With equal reliability the consensus of the community distinguishes the significant from the trivial This is the more remarkable considering what Merton calls "the basic uncertainty of genuinely independent originality in science."

Thus Niels Bohr was once prompted to observe of a radical and baffling proposal by the aging Werner Heisenberg: "Yes, it is crazy, but it is not crazy enough!"

It has been said that science will flourish only in a society that cherishes its norms. The reason, openness, tolerance, and respect for the autonomy of the individual that distinguish the social process of science, however, are norms desirable in every human community. They describe a world in which, we can agree, all of us want to live.

Happily, the social process of science brings along the means to realize its values. For it finds convincing verification in the technologies it begets. During the past four centuries science has been liberating increasing numbers, now nearly one-third, of mankind from toil and want and even from submission to received authority. No national constitution written in this century has failed to hold out the promise, at least, of political and economic democracy. The people of the world—if nations can keep the peace—may see this revolution in the condition of man fulfill its promise in the next century.—*Excerpted from a public lecture given at Moscow State (Lomonsov) University, Moscow, U.S.S.R., on 25 November 1985, on the occasion of the conferral by the university of the degree Doctor Honoris Causa.*



The world is now, as it has been for a long time, awash in a sea of ethnocentrism, prejudice, and violent conflict. The worldwide historical record is full of hateful and destructive indulgences based on religious, racial, and other distinctions—holy wars of one sort or another. What is new is the destructive power of our weaponry: nuclear, enhanced conventional, chemical, and biological. Moreover, the worldwide spread of technical capability, the miniaturization of weapons, the widely broadcast justifications for violence, and the upsurge of fanatical behavior are occurring in ways that can readily provide the stuff of very deadly conflicts. To be blunt, we have a rapidly growing capacity to make life everywhere absolutely miserable and disastrous.

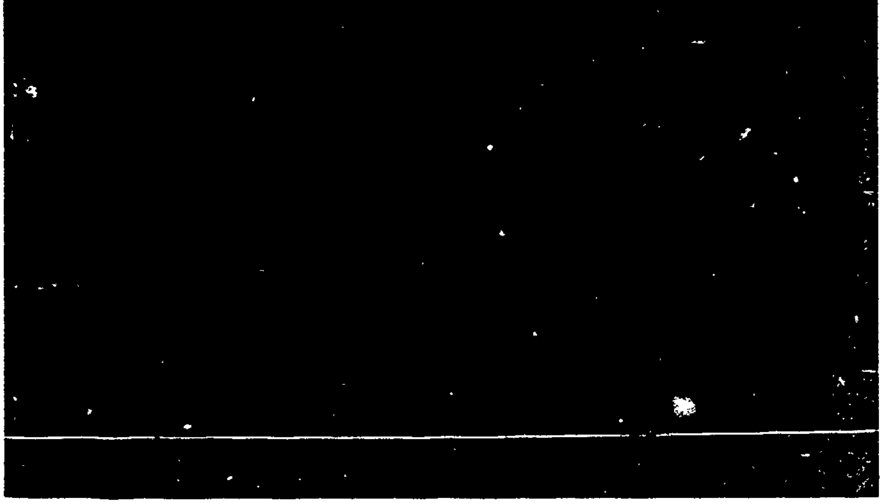
Centuries ago, it was common for military conquerors to put captives to the sword or to reduce them to slavery. By the end of the 19th century, it was widely believed that we had achieved a sufficiently civilized status to make such horrors impossible. Yet the world since then has seen near extermination of peoples, massacres, and massive deportations. We have learned and are continuing to learn how great the horrors can be when supposedly civilized nations set about destroying depreciated people.

In a world full of hatred, repression, terrorism, small wars, and preparation for immense wars, human conflict is a subject that deserves the most careful and searching inquiry. The stakes are now so high that there is an urgent need for cooperative engagement with these problems over a wide range of inquiry involving the physical, biological, behavioral, and social sciences. There is no royal road to truth, no single perspective that offers overriding promise. Just as the sources and manisfestations of human conflict are immensely varied, so too are there many useful approaches to understanding, preventing, and resolving conflict.

Conflict and its prevention or resolution have not been major subjects for scholarly inquiry until quite recently, and even now they attract only marginal interest in many of the world's great research institutions. Nevertheless, some interesting and useful work is being done, including research on biological evolution and mechanisms of aggressive behavior; deep inquiry into the origin and resolution of past conflicts and study of contemporary conflicts; formulation of fundamental concepts pertinent to a wide range of conflicts; experimental research on simulated conflicts; the study of negotiations, both in real-life circumstances and in simulated ones; the study of various intergroup and international institutions as they affect conflict; research specifically focusing on issues of war and peace; and the study of conflicts at various levels of organization, such as families, communities, and nations, in the search for common factors and principles, so that discoveries at one level may illuminate issues at another level. The strengthening of both experimental and observational research, keeping in mind actual conflict and real-world decisionmakers, could probably lead to major contributions in the next decade.

It is certainly not beyond human ingenuity to move this subject higher on the world's agenda. Strong organizations covering wide sectors of science, technology, and education can take a more active role in coping with this critical issue. The scientific community is the closest approximation we now have to a truly international community, sharing certain fundamental interests, values, and standards as well as certain basic curiosities about the nature of matter, life, behavior, and the universe. The shared quest for understanding is one that knows no national boundaries, no inherent prejudices, no necessary ethnocentrism, and no barriers to the free play of information and ideas. To some extent, the scientific community can provide a model for human relations that might transcend some of the barriers that have long plagued us and have now become so dangerous.

In a fundamental way, the modern world is the creation of science and technology in all of its aspects—those we relish and those we fear. The time is ripe for the scientific community to provide worldwide leadership in addressing the ubiquity of prejudice, the profound and pervasive impact of ethnocentrism, and the greatly enhanced risks of these ancient orientations in the rapidly changing world of the late 20th century.



The internal mechanisms by which scientists validate each other's research results have been of little public interest until recently. But when charges of fraud, falsification, and plagiarism are headlined in the morning newspaper, it is clear that these quality control mechanisms are no longer strictly internal matters.

Increased government scrutiny of the means by which scientists check their work can be expected when public funds have underwritten research that has been called into question. Congressional committees have criticized the early responses by government agencies investigating allegations of wrongdoing by publicly funded researchers. Efforts are currently under way to assure honesty and integrity in research by defining and strengthening the norms that govern professional conduct in science.

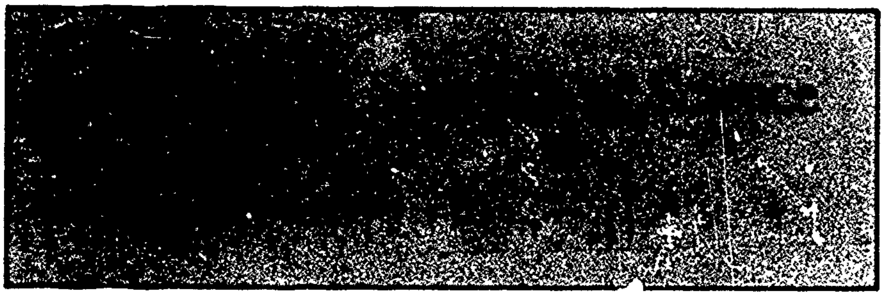
Whether recent highly publicized reports of fraud demonstrate that misconduct has significantly increased within the scientific establishment is difficult to determine. Even if hundreds of cases were documented, these would still represent only a small fraction of the total research currently under way. Moreover, many officials fear that bureaucratic overreaction to selected incidents of fraud could cause greater damage to productive research than the harm resulting from undetected deceit.

In any case, most research institutions lack formal mechanisms for identifying and correcting deceptive or sloppy science Standard peer review practices are not designed to identify fraudulent research, and it is unlikely that collaborators would report suspicions about a colleague if an institution lacks procedures to provide a fair and impartial investigation of the matter.

Following the early reports of individual cases of fraud in science, William J. Broad prepared an in-depth examination of the significance of scientific fraud. He concludes that the fact-checking mechanisms in science do not work effectively. The reputations of individual investigators, he reports, may shield their work from appropriate scrutiny by skeptical colleagues. His article prompted letters from Edith D. Neimark, Harold Hillman, Carvel Blair, and George A. Silver presenting different views about the meaning of misconduct in research practice. While some maintain that scientists have traditionally demonstrated high ethical standards in their professional work, others argue that more forceful monitoring is necessary. As an antidote to misconduct, DeWitt Stetten, Jr., suggests tighter supervision within smaller research training groups.

More recently, *Science* editor Daniel E. Koshland, Jr., comments that fraud is an unwelcome but inevitable part of the research process "as long as human beings are doing the experiments." He believes, however, that "99.9999 percent of reports are accurate and truthful" in science, and that the cumulative nature of science makes it more likely that deceit will be uncovered more quickly than in other professions. Raymond R. White, and Michael R. Rosen and Brian F. Hoffman respond with more skepticism. Their letters claim both that a greater amount of research may be in error than has been previously reported and that proposed governmental regulations unfairly shield those who accuse others of wrongdoing.

Wherever the truth lies, the recent preoccupation with fraud and misconduct in science has contributed to a "let's clean up our act" attitude and a desire to restore integrity in the house of science. The emphasis on responsibility in science has shifted from earlier social reform efforts to a more centrist position emphasizing honesty and truthfulness in science. The health sciences in particular are attempting to define scientists' rights and duties in a time of rapid change. These issues are examined again in the next chapter as authors search for new principles of autonomy and accountability to shape the development and application of medical knowledge in the latter half of the twentieth century. —*RC*



There is little doubt that a dark side of science has emerged dr ·.ng the past decade. In ever-increasing detail, the scientific and general press have reported the pirating of papers and the falsification of data. Four major cases of cheating in biomedical research came to light in 1980 alone, with some observers in the lay press calling it a "crime wave." Federal investigators say two of these cases may end in criminal charges.

In a profession that places an unusual premium on honesty, the emergence of fraud has created something of a stir. Scientific societies are holding symposia on the subject. The National Institutes of Health has taken administrative steps to cope with the putative rise in cheating. And Congress, as it does with issues significant or otherwise, is preparing to hold hearings* on the falsification of data in biomedical research.

Is the issue important? After all, reported cases of cheating are few, and NIH funnels government funds into nearly 18,000 extramural projects. *Science* recently put the question to a dozen research directors, NIH officials, bench scientists, and sociologists. One recurring observation was that fraud has always been around, but not always advertised. A Nobel laureate, for example, was said to have coauthored a paper in the early 1960's that was retracted due to the cheating of a junior associate. The unseemly details of the Nobel retraction never went out of the lab, and therein, say some observers, lies one difference between the finagling of yesterday and today. Changes in comtemporary science and its interactions with society are making fraud in the labs more visible.

- John Long, a researcher with $750,000 in federal funds at Massachusetts General Hospital, forged data and for 7 years watched over a cell line for the study of Hodgkin's disease that proved useless (*Science*, 6 March 1981).
- Vijay Soman, a researcher at Yale medical school, plagiarized a rival's paper, fabricated data, and received for 1980 alone some $100,000 in NIH support. Eleven papers were retracted. He ultimately returned to his home in India, but left his coauthor and boss, Philip Felig, ii 'dministrative and ethical tangle (*Science*, 3 October 1980).
- Elias A. K. Alsabti, a young researcher from Jordan, pirated almost word-for-word at least seven papers and published them in obscure journals. (*Science*, 27 June 1980).
- Marc Straus, a Boston University researcher who in 3 years was awarded nearly $1 million in cancer research grants, submitted reports containing repeated falsifications. He resigned under fire, insisting that he was the victim of a conspiracy by select members of his 20-person staff. More than 2 years later, after the Boston *Globe* ran a five-part series on the affair, the National Cancer Institute initiated an investigation.

In response to these and a few other incidents, Congress has invited two witnesses to the falsification drama, Long and Felig, to come and give their views on what, if anything, is happening to U.S. biomedical research. Also invited are a bevy of NIH officials, research directors, and bioethicists. Cheating is also being discussed during symposia at the upcoming annual meeting of the Council of Biology Editors. Meanwhile, at the Harvard School of Public Health, the seventh national conference on Public Responsibility in Medicine and Research just held a session on "How to detect and prevent fraudulent or unethical research."

Until recently, charges or even discussions of scientific fraud were seldom aired in public. Most scientists, conscious of their image and eager to avoid political interference, tried to stay out of the limelight. Control was an internal matter. An informal group of scientists could hold court and decide to ban an offender from the realm of research. More fundamentally, science was said to be self-correcting. If an experiment was important enough, other scientists would try to repeat it. This self-correcting mechanism would expose cheating and encourage honesty. It would detect and deter. Dubbed "organized skepticism," this view was originally set forth by Robert K. Merton, the father of the sociology of science. "Scientific inquiry," he wrote, "is in effect subject to rigorous policing, to a degree perhaps unparalleled in any other field of human activity." Initially propounded in 1942, this view has become the conventional wisdom. Donald Fredrickson, director of NIH, today puts 't this way. "We deliberately have a very small police force because we know that poor currency will automatically be discovered and cast out."

A sterling example of such self-correction comes from the case of the Nobel retraction. The incident unfolded at Yale in the late 1950's, with the arrival of a young graduate student in biochemistry. Working in the lab of Melvin Simpson, the student quickly made significant gains in the cell-free synthesis of cytochrome *c*, a key protein in cellular energy-releasing reactions. In early 1960, Simpson and the student coauthored a paper on the successful experiments that received wide attention because it was the first time such a single, highly purified protein has been synthesized outside a cell. The success carried the student, now equipped with a Ph.D. from Yale, to the lab of Fritz Lipmann at Rockefeller University, where he coauthored a paper with the Nobel laureate. The promising career, however, soon suffered a setback.

Simpson, back at Yale in late 1960 after spending several months on sabbatical in England, reassembled his lab and started trying to extend the successful experiments with cytochrome *c*. His efforts met with failure. "I had gone all around Europe giving seminars on our success," he recalls. "And now I couldn't repeat it. Imagine the agony." A call from Lipmann at Rockefeller revealed that people in his lab were also having difficulty repeating the student's work. The student was called back to Yale and told to duplicate the cytochrome *c* experiment. He worked under around-the-clock supervision, and failed. He was subsequently told to leave research in general. Two retractions, one

*The House Science and Technology subcommittee on investigations and oversight, chaired by Albert Gore (D-Tenn.), will hold hearings on 31 March and 1 April, as this issue of *Science* is going to press. Testifying will be Donald Fredrickson (NIH), Philip Handler (National Academy of Sciences), LeRoy Walters (Kennedy Institute of Ethics at Georgetown University), Philip Felig (Yale), John Long (formerly of Massachusetts General Hospital), Ronald Lamont-Havers (Mass General), Patricia Woolf (Princeton), Stuart Nightingale (Food and Drug Administration), Alexander Capron (President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research), and ¨iam Raub (NIH).

from Simpson and one from Lipmann's lab, were published in late 1961. Sometime later it was discovered that the student's undergraduate college in Massachusetts had no record of his ever receiving a degree.

Since that time, revelations of cheating—but not necessarily cheating itself—seem to have slowly but steadily increased. The cause? According to Robert H. Ebert, former dean of the Harvard medical school, part of the reason may be increasing pressure. Writing in the *New York Times* about the fabrication of data by John Long at Mass General, Ebert said "it would be a mistake to consider this an example of human frailty and nothing more. Medical schools and academic research centers have inadvertently fostered a spirit of intense, often fierce competition, which begins during the premedical experience and is encouraged thereafter. . . . There is intense pressure to publish, not only to obtain research grant renewals but in order to qualify for promotion."

The implication in this account of a rise in cheating itself is dismissed in many quarters. Pressure, say a chorus of commenters, has always been around. Moreover, many hold that the rate of finagling has remained roughly the same throughout the years, and cite the purported cooking of data by Mendel, Newton, and Ptolemy to back up their beliefs.

A radical view of the ubiquity of fraud comes from philosopher of science Paul Feyerabend (*Science*, 2 November 1979), who holds that small-scale cheating is essential to the advancement of science. He argues that no theory, no matter how good, ever agrees with all the facts in its domain. A scientist must therefore rhetorically nudge certain facts out of the picture, defuse them with an ad hoc hypothesis, or just plain ignore them. A similar but less polemical view is expressed by philosopher Thomas S. Kuhn (*Science*, 8 July 1977). Kuhn divides the history of science into periods of normal and revolutionary activity, arguing that during normal periods, anomalies observed by the scientist must be suppressed or ignored.

If finagling of one sort or another is endemic, what then causes increased exposure? Here it is necessary to make a distinction: exposure of fraud to other scientists and exposure to the public.

In the first instance, one mechanism that may bring cases of cheating out in the open is the denunciation of scientists by one another due to cutbacks in research funding, according to Ronald Lamont-Havers, a former NIH official who witnessed the Long affair from his position as director of research at Mass General. If this is indeed the case, troubled times may lie ahead. Since 1979, NIH has had a drop in purchasing power, and this year the percent of approved grants lucky enough to get funded has dropped to 30—an all-time low.

The increasingly close scrutiny of research that has direct implications for public policy or public health is also a factor in inter-scientist exposure, according to Columbia University sociologist Harriet Zuckerman. This clearly seems to be the case in the Straus affair at Boston University. Data from about 200 patients studied by Straus and his team were kept in the computer files of the Eastern Cooperative Oncology Group, a 40-hospital consortium funded by the National Cancer Institute to conduct large-scale testing of new cancer treatments. In 1978, five members of Straus's team disclosed to officials at Boston University problems with the data. Falsifications ranged from changing a patient's birthdate to reporting treatments and laboratory studies that were never done and inventing a tumor in a patient who had none. Boston University says a detailed study of medical records found no evidence of patient mistreatment or inappropriate care. Disagreeing with this view is a Food and Drug Administration official familiar with an ongoing investigation of the Straus affair: "To say the least, some of this had serious clinical implications, both in the sense that the patient in the study was endangered, and that data generated would present conclusions that were poorly founded."

Concerning exposure to the public, one factor repeatedly singled out is the growth of a vigorous scientific press. Indeed, the National Association of Science Writers, founded in 1934 by 15 reporters, now has more than 1000 members. And clearly, the NCI investigation of the Straus affair would never have materialized had it not been for the series in the *Globe*. Some observers, however, suggest that the press for the most part tends to purvey, rather than initiate, exposures.

A general rise in social consciousness among scientists may account for some of the increasing public exposure, according to E. Frederick Wheelock, a microbiologist at Jefferson Medical College in Philadelphia whose work was pirated by Jordanian researcher Alsabti. "In the past," he says, "the system was much more closed. People were afraid to call attention to cheating." In his own case, Wheelock at first hesitated to charge Alsabti with piracy. Wheelock had kicked Alsabti out of his lab after two young researchers came to him with proof that Alsabti was making up data. Later, when Wheelock saw his work being published in the scientific literature by Alsabti, he discussed the problem with his program manager at the National Cancer Institute, who suggested that he alert the wider community. After first writing to Alsabti and demanding retractions (that did not materialize), Wheelock wrote letters to *Nature*, *Science*, *Lancet*, and the *Journal of the American Medical Association* and described ways for researchers to "avoid such episodes in the future."

The list of possible reasons for increasing exposure rambles on, most everyone having their own pet speculations. Lurking in the record of events, however, is an intriguing contradiction. A review of the cases where cheating has come to light during the past decade shows that the failure to duplicate experiments plays a relatively minor role in uncovering fraud. This self-correcting mechanism "worked" in earlier episodes: in the cases of Mendel, Newton, and Ptolemy (though it took two millennia), or in the case of the Nobel retraction. During the past decade, however, other means have predominated, the mechanism often being the detective work of young lab assistants or young scientific rivals who have extra-experimental evidence of cheating, who have some independent reason for suspicion. This was the case in all four of the 1980 fraud episodes. It was also the case with the Sloan-Kettering affair and the painted mouse of William Summerlin (*Science*, 14 June 1974), although Summerlin's work was also under fire because it could not at the time be repeated.

This gap between real and ideal ways of detecting and preventing fraud (what sociologists of science euphemistically refer to as the "social control of science") has helped fuel a heated critique* of the conventional wisdom during the past decade.

On the deterrence side of the debate, critics have argued that the self-correcting mechanism does not distinguish between error and fraud. In the published literature, an experiment is only found

*For a summary, see J. Gaston, "Disputes and deviant views about the ethos of science" in *The Reward System in British and American Science*. (John Wiley and Sons, 1978), pp 158–184.

right or wrong. Given the ever-present academic pressure to succeed in a spectacular way, this chance of being found wrong may not deter a researcher from cheating. After all, guesses, fudging, and unconscious finagling that are correct go undetected.

Defenders of the conventional wisdom say that this weakness, by definition, does not make any difference. The only thing that matters is the accumulation of scientific "truth," and not whether a falsifying researcher is caught and punished.

It is here, on the detection side of the debate, that critics rail most vehemently. The acceptance or rejection of claims in science often depends not so much on "truth," according to such observers as philosopher Ian I. Mitroff at the University of Pittsburgh, but on who makes the claim and how well the claim fits prevailing beliefs. In short, the goodness of a reputation or the attractiveness of a theory often gives immunity from scrutiny.

This circumvention of the idealized mode of detection was probably a factor in why the problems with John Long's contaminated cell lines at Mass General escaped detection for so many years. He worked in a prestigious lab at one of the world's leading teaching hospitals. "With the credentials of background and training that Long presented, the study section would expect that he would be aware of this [contamination] problem," says Stephen Schiaffino of the NIH division of research grants.

Immunity from scrutiny was also clearly a factor in the case of Cyril Burt (*Science*, 26 November 1976), the English psychologist whose studies of identical twins supported his theory that intelligence is determined partly by heredity, and whose work went unchallenged during his lifetime. As a government adviser in Britain in the 1930's and 1940's, Burt was influential in setting up a school system in which children were assigned to one of three educational levels on the basis of a test given at the age of 11. According to Leon Kamin, a psychologist at Princeton, Burt's data remained unchallenged for so long because they confirmed what everyone wanted to believe. "Every professor knew that his child was brighter than the ditchdigger's child," he says, "so what was there to challenge?"

Burt's work was picked up by researchers in the United States, and figured prominently in the debates over whether heredity might underlie racial differences on IQ scores. Eventually, after a reign of nearly 40 years, his data were found to be riddled with internal implausibilities and basic methodological oversights. Some researchers concluded that Burt may have doctored or even invented his collection of IQ data.

Critics for the most part do not argue that the conventional wisdom is wrong, but rather, taken alone, it is inadequate to explain how science really works on a day-to-day basis. Perhaps the most troubling observation is that even when the self-correcting mechanism works, it addresses only experiments and observations that are "important" to pure science, to the accumulation of scientific truth. No one, after all, takes much time to repeat clinical trials of new drugs, therapies, or treatments. Replication of a multi-institutional clinical trial, such as the one at Boston University that Straus worked with, is financially and structurally impossible. In terms of the self-correcting mechanism, these are not applicable areas of research, although they may be important in terms of patient welfare.

Just as there was no scientific or institutional mechanism to detect or deal with fraud in the Straus affair, neither was there a federal mechanism. When three top officials at Boston University medical center flew to Washington to tell the NCI director about their rapidly unfolding problems, NCI told them there was nothing the government could do.

The slow response of the federal bureaucracy, the questioning of the self-correcting mechanism, and the emergence of a few graphic examples of fraud have combined to stir considerable activity concerning data abuse. At Boston University, the multi-hospital group that got stuck with the project's bad data has set up a system of random audits to ensure that the program will never again be vulnerable to such falsification. Congress is in the process of holding hearings. The President's commission on bioethics plans to hold a number of sessions at "sites of controversy involving the conduct of research."

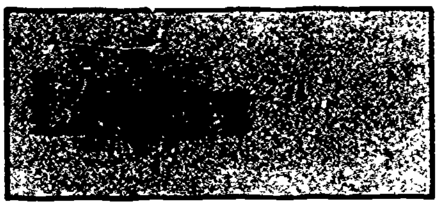
Confronted with the increasing reports of fraud-related incidents, NIH recently took steps to prevent abuse in the future. In November 1980, debarment regulations went into effect that allow the government to cut off an entire institution from NIH grants if just one researcher is caught misusing grant money or falsifying reports (*Science*, 14 November 1980, p. 746). This sweeping mechanism was needed, says NIH associate director William Raub, in order to put the onus for prevention and detection of fraud on the institution. Previously, institutions might have been tempted to look away. Over the years, the administrative costs charged by universities for nurturing the research enterprise have risen so that they now average more than 27 percent of a grant.

No person or institution has yet been debarred, and NIH officials say they have no plans to make debarment retroactive. If it were, all of Yale University, for example, could well be cut off from the federal research-dollar pipeline. In addition to the threat of debarment, NIH officials say they have now built into their vast computer network an alert system so that NIH administrators are warned if an investigator applying for a new grant is himself under investigation for cheating. Flagged so far by this system are Straus, Soman, and a third, unidentified researcher who is currently under investigation.

Is it important? Perhaps the emerging issue of fraud represents a small, seamy side of science that warrants nothing more than a cursory glance before being tossed onto the pile of passed-over issues. One might argue that the major cases are few, and the minor ones are just that, minor. Science is above it all. Nobel Prizes are awarded and greatness is measured not on the basis of "honesty," but insight. Newton and Mendel may have finagled, but their theories are today committed to memory by every high school student.

In a sense, all this is correct. It is also true that fraud in the literature wastes the time and money of researchers who pursue leads only to find them wrong. Simpson spent 1 year untangling the cytochrome *c* mess, and, because of this unanticipated chore, lost a priority battle in a different area of biochemistry. Similar amounts of time are probably wasted in other fabrication episodes. Further, in a profession where "organized skepticism" is meant to be the rule, the emergence of a type of fraud not detected by this self-correcting mechanism may prove especially corrosive to community ideas. This mechanism did not and could not deter data fabricators at Boston University, with the result that patient safety was probably jeopardized. And the fact that immunity from scrutiny often seems to supersede any kind of "organized skepticism" can only lead to the discouragement of the young, who tend to be far from immune. In the case of the imbroglio at Yale, it was a 29-year-old NIH researcher who brought charges against Soman, an assistant professor, and Felig, a professor with an endowed chair. "I just found it hard to believe that Felig had engaged in any hanky panky," said an appointed NIH auditor who, after a wait of 6 months, decided not to investigate the data of Soman and Felig. During this noninvestigation, the young researcher quit NIH and research in general.

No matter why they come forth, the recent cases illuminate much. They disclose a gap between the ideal and the real, between reliance on automatic self-policing and the fact that mechanisms such as immunity from scrutiny often prevail. They hint at support of philosophical views that say finagling of one sort or another may be endemic to the research enterprise. Perhaps further study of the dark side will disclose more about the structure of science. At the very least, the recent cases illustrate that "organized skepticism" and the self-policing nature of science need themselves be taken with a little more skepticism.



22 May 1981

Fraud, Science, and Safeguards

The article by William J. Broad on fraud and the structure of science (News and Comment, 10 Apr., p. 137) was timely but perhaps a bit too bearish on the current state of morality in research. Any deliberate fudging of the data for personal aggrandizement is to be deplored whenever it occurs; however, given the huge increases in the number of persons doing research, I do not think that the relative frequency of instances of fraud has increased. My guess would be that the safeguard mechanisms are working adequately and that the relative frequency is, if anything, lower than in earlier periods. It would, of course, be good to have accurate data.

A corrective perspective is to view fraud in science in the context of fraud in other areas of endeavor, especially commerce and the professions. When placed in the context of escalating malpractice suits and the clamor for consumer protection agencies and legislation, and the sorts of incidents that have produced these trends, one must conclude that scientists have managed to maintain high ethical standards in a society where personal integrity as a cherished virtue is rapidly disappearing. It is to be hoped that the response of the scientific community to lapses of honor among researchers can serve as a model and an inspiration for other areas of endeavor to "clean up their act." It is easy to lose perspective when one focuses exclusively on individual acts of fraud and to come up with recommendations for corrective measures which may not, in fact, be needed or useful. If something "ain't broke," don't fix it. It is not clear that the standards of scientists need fixing beyond regular maintenance.

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31 July 1981

Research Practices

The article by William J. Broad on fraud and the structure of science (News and Comment, 10 Apr., p. 137) and the subsequent letter from Edith D. Neimark (22 May, p. 873) raise moral questions. I would like to mention other, more common practices.

1) Collaborators or supervisors put their names on manuscripts (and thus assume intellectual responsibility for them) reporting research work which they themselves have not done and which they have discussed inadequately or not at all with the workers who carried it out.

2) Research workers do not submit for publication single experiments or series of experiments which do not fit in with their hypotheses.

3) Scientists fail to do relevant crucial experiments which they themselves have identified, or to which their attention has been drawn.

4) Authors deliberately fail to cite other authors whose work predates or contradicts their own.

5) Referees fail to read sufficiently carefully manuscripts of papers, book, or theses, thus missing findings or desiderata which are crucial to the validity of potential publications.

These widespread practices have a considerably greater impact on knowledge than the relatively rare acts of fraud.

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19 March 1982

Fraud Investigation

Scientists are quick to demand academic and scientific freedom, as in condemning Admiral Inman's suggestions for classification of certain cryptographic research (News and Comment, 22 Jan., p. 383). Yet one result of unbridled freedom was the academic cover-up of a recent alleged biomedical fraud. Scientists in a democracy should temper their love of individual freedom with recognition and acceptance of their unique responsibilities toward their country and their fellow citizens. As Thomas Paine observed in a different but no less pertinent context, "Those who expect to reap the blessing of freedom must, like men, undergo the fatigue of supporting it."

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19 March 1982

Recent allegations of fraud, fabrication, and plagiarism among investigators in the biomedical community raise a disturbing question about biomedical research practices: Is it in the nature of the association between biomedical research and medical education that one may seek the source of the apparent increase in unethical practices?

While the incidents have been well covered in *Science*'s News and Comment columns and the reporters deserve praise for their careful and comprehensive presentations, it is important to remember that they are subject to the constraints imposed by the protective codes observed by the scientists providing the information—codes, incidentally, observed with equal force in the official reports submitted by these same scientists. The "courtesy" displayed toward colleagues, while not deceitful, is surely self-serving. The biomedical research community may not be any less honorable than other professional groups—nor any more so. Shaw (*1*) noted that every profession was a conspiracy against the laity. The appearance of full disclosure may be there, but one can be sure there will always be a bit of *suppressio veri* and/or *suggestio falsi*.

As a consequence, I believe that a full and forthright examination of these problems in all their complexity is in order and that such an examination must be overseen by a nonscientist. I argue from analogy with situations in which an outside prosecutor is selected when an elected official is to be investigated.

The competition for place and status that now preoccupies workers in the field deserves much more questioning and justification if it is to be continued. The shameful scrambling for space and grants in the face of dwindling research funds and increasing numbers of investigators has an almost Malthusian ring! Clearly, something is amiss in the structure of biomedical research. Can it be that a relationship we have taken for granted over the past 50 years is flawed? The sociological studies of Barber *et al.* (*2*) indicate that the perversion of ethics in research is a result of competition. And Relman (*3*) acknowledges the influence of an industrial value system on the medical establishment, although he carefully avoids inculpating medical education and its research-oriented goals.

Let there be a national review, and let us not overlook the possibility of separating biomedical research from medical education, thereby restoring traditional ethical qualities to both.

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21 December 1984

Reported Laboratory Frauds in Biomedical Sciences

In recent years, we appear to have been hit by a wave of accounts in journals and newspapers of fraudulent reporting of scientific data. Many factors may have contributed to this, including an increase in the number of persons devoting substantially full time to research; an increase in the competition for available support dollars; an increase in the pressure to publish abundantly; possible changes in the nature of the training process that leads to the Ph.D. degree; and possibly, just possibly, a deterioration in the morality of our scientists. Assignment of weights to each of these and other factors is, at this time, not possible. However, review of reported cases indicates that certain generalities may be noted.

The frauds appear to occur most frequently in our most prestigious research and teaching institutions. A young man enters the laboratory of a very successful investigator, one who has an unusually large bibliography and who has, therefore, secured generous grant support and abundant laboratory space. The young man (there is a paucity of cases in which women are involved in fraud) inspects his new environment to ascertain what it takes to succeed there, and he soon concludes that, since his preceptor, who is obviously successful, has published an unusually large number of papers, this is the route to success. He therefore tries to follow this example and may publish ten or more papers in a year. Those of us experienced in the production of biomedical research know that to generate novel research results sufficient to fill that number of papers in the allotted time is not easy. Our young candidate, therefore, is forced by circumstances to consider the routes he may follow to achieve his ambition. On the one hand he may choose to plagiarize from the results of others. On the other hand he may choose to fabricate results and experiments. By these means, he hopes to diminish the time normally consumed in honest research, which entails the planning and design of experiments, the acquisition of the necessary technical skills, consideration of the relevant literature, and finally the preparation of his own research report. All of these steps consume time and energy.

One of the consequences of the selection of a very successful investigator as preceptor is that he will have attracted the attention of other pre- and postdoctoral fellows. He is likely to have a large number of trainees at some level of training who are accountable to him. Sometimes this number exceeds 20, and this, together with the fact that successful preceptors are likely to spend much time on the road giving lectures and participating in various committees and editorial board functions, reduces the time the preceptor may spend with each trainee. There are cases in which the trainee meets with his preceptor for 1/2 hour every 6 weeks. This is simply not enough if the preceptor is to take his teaching responsibility seriously. He should review with each trainee experiments done yesterday, and he should outline with the trainee the experiments planned for tomorrow. He should guide the trainee through the maze of the literature, and he should assist the trainee in the acquisition of the necessary experimental skills.

In addition, both by example and by precept, he has an obligation to make certain that each of his trainees is fully sensitized to the absolute requirement of total honesty in the reporting of scientific results. Many professions recognize unacceptable deviations from strict honesty. The banker cannot tolerate the embezzler. The military person cannot tolerate the deserter. And the intelligence services cannot tolerate the mole. Science cannot tolerate the man who takes lightly his moral obligation to report strictly what is true.

In light of the foregoing, it is suggested that research training groups under a single preceptor be kept small in size, permitting abundant contact between preceptor and trainee. The preceptor, if he is to fulfill his moral obligation, will undertake to spend significant periods of time at frequent intervals with each of his trainees. The candidate trainee will be well advised to select a preceptor who is not excessively encumbered with large numbers of trainees. He should recognize that a preceptor with an excessively lengthy bibliography may set quantity of publication above quality of research and may thus be a poor role model. In several of the recently publicized instances of laboratory fraud, the trainee has been the prime target of criticism, while the preceptor has been treated sympathetically and with commiseration. There may be instances in which this distribution of blame is not appropriate. Frequently, when one member of a group produces many more publications than do his contemporaries, this is taken as an indication of unusual ability. It may be appropriate to view such exceptions as a basis for suspicion and an indication for scrutiny.

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Fraud in scientific research is unacceptable and inevitable. It is unacceptable because the entire procedure of publishing and advancing knowledge is based on trust—that the literature reports accurate measurements of actual experiments. If each researcher had to go back and repeat the literature, the enormously productive rush of modern science would slow to a snail's pace. Even good intentions are not enough. Sloppy experimentation and poor scholarship are condemned. Outright fraud is intolerable.

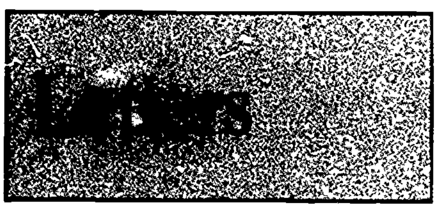
Nevertheless, some fraud will exist as long as human beings are doing the experiments. Any system in which advancement, fame, and fortune await a successful practitioner will tempt a certain number of individuals to cut corners. That number may well be smaller in science than in other fields, not because scientists are more moral than others, but because the cumulative nature of science means inevitable exposure, usually in a rather short time.

An oversimplified admonition might be, "You may escape detection by falsifying an insignificant finding, but there will be no reward. You may falsify an important finding, but then it will surely form the basis for subsequent experiments and become exposed." Therefore, there is little percentage in falsifying science, and the speed with which recent examples of this unfortunate human frailty have been revealed is an indication of the pace of modern science. Some newspaper reporters have used recent fraud cases to imply that the structure of science is crumbling or that there is a cover-up, forgetting that the extent of the scientific enterprise has grown a thousandfold since the 1800s. We would expect a greater number of cases of fraud today, but there is no evidence of an increased percentage. And there in no modern equivalent of Piltdown man, a fraud that took years to uncover. Still, it is important that scientists be ever vigilant, and the rash of recent frauds does suggest some dangers in modern science.

One danger arises from the nature of interdisciplinary research. Many papers have numerous authors: investigators in a laboratory that has cloning expertise collaborate with others in a laboratory that has expertise in physical instrumentation and another laboratory that does animal tests to publish a joint paper. The results of this kind of collaboration have had spectacular success, in the main, and no one would wish to limit such joint efforts. Yet when no one person has expertise in all aspects of the research, there can be dangers. A second problem arises when busy scientists, who have too many projects and too little time, supervise projects in which they have infrequent contact with those doing the experiments. Finally, the competitive world of modern science fosters some entrepreneurs who are so intent on the next grant or the big success that they forget that every good experimenter must be his own devil's advocate. A principal investigator must not only devise critical tests for his findings, but must also generate an atmosphere that encourages co-workers to report the bad news as well as the good news.

The procedures recently established by the National Institutes of Health and various universities to deal with fraud seem admirable and appropriate. The punishments for offenders are severe: usually, total derailment of a career. Because the repercussions associated with fraud are so serious, some investigations of such charges take long periods of time, but fairness to the accused is essential. Once guilt is ascertained, the loss of a career in science seems appropriate in many cases. Restitution in some form for the wasted time of those who based further experiments on the false report might be considered appropriate as well. The larger the group, the more interdisciplinary the research, the more competitive the area, the more is the need for watchful skepticism.

Having acknowledged that, we must recognize that 99.9999 percent of reports are accurate and truthful, often in rapidly advancing frontiers where data are hard to collect. There is no evidence that the small number of cases that have surfaced require a fundamental change in procedures that have produced so much good science. To continue the great advances that are being made, we must accept that perfect behavior is a desirable but unattainable goal. Vigilance? Yes. Timidity? No.



20 March 1987

Accuracy and Truth

Daniel E. Koshland, Jr.'s, assertion that no more than one in a million scientific papers departs significantly from accuracy and truth (Editorial, 9 Jan., p. 141) might have been made by Pollyanna. The idea that we scientists are ethically 99.9999% pure is not only ridiculous but also obviously self-serving.

Besides the actual falsifications of data, which probably pollute two orders of magnitude more reports than Koshland imagines, less direct deceits are abundant. I would estimate that over 10% and perhaps even a majority of all published works contain one or more of these dec 'ts:

1) Omission of negative results from corollary experiments.

2) Presentation of statistical analyses where mathematically required conditions are not met, or discussed.

3) Failure to mention equally simple hypotheses that were untested or are untestable.

4) Citation of work as proving a point, which it does not.

5) Citation of work as supporting a point, which it does not.

Scientists routinely try to make their work look more significant than it is. This is natural and human, but it does distort the published product. Editors are certainly aware that this is so, and act to limit the distortion, although not effectively enough.

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Response: I did not state that scientists are ethically 99.9999% pure. What I stated was that I believe that proportion is not far off with regard to the correct data placed in the literature. Scientists are human and there will be both error and fraud, but it is as irresponsible to exaggerate these deviations as to ignore them. The errors listed by White all occur, but I would disagree strongly with regard to their frequency. I looked at one journal, the *Journal of Biological Chemistry*, which published 17,000 pages in 1986. Using a rough estimate of 50 pieces of data per page, one gets close to 1 million bits of information for one journal in one year. There are hundreds of journals in biochemistry alone and hundreds more in such diverse fields as physics, geology, psychiatry, and so forth. Yet, only one or two cases of fraud are exposed per year. My guess is that very few of the data in all these articles contain significant errors, let alone deceitful ones. Reading that a value is 10.1 in a table but that in the text it is 10.3 may be annoying, but anyone who has written a manuscript knows how easy it is to create such an error by the usual rounding off of data. Omitting negative results is not necessarily either deceitful or incorrect. There are standard procedures for omitting negative results, such as when subsequent positive results reveal the source of the initial error, or where statistically appropriate replications indicate that an unexplained deviation is beyond statistical significance. Statistical analyses are also a source of error, and that is why *Science* hires a consultant statistician. Scientists usually like to state alternative hypotheses, but lack of imagination or referees who say "don't speculate" can be reasons for leaving them out as well as deliberate deceit. Poor citations are probably the most prevalent of the ills listed, but the most important error in that regard is usually lack of scholarship, that is, a tendency to use a conventional citation without rereading the work or to choose one citation to illustrate a field when more than one paper is relevant. Failure to cite competitors can be deceitful or an excess of self-delusion.

Despite these deviations, which I believe will always be with us, the biological literature does, to an incredibly high degree, reflect accurate data on incredibly rapidly advancing frontiers. There is no error-free world. Perfection is the goal, but if we wait until we achieve it, progress will be very slow.—DANIEL E. KOSHLAND, JR.

27 March 1987

NIH Fraud Guidelines

The editorial "Fraud in science" by Daniel E. Koshland, Jr. (9 Jan., p. 141), states that the National Institutes of Health procedures for dealing with possible misconduct in science (*1*) "seem admirable and appropriate." We take exception to two recommendations made by NIH for awardee institutions, first, to item (b), "protecting the privacy of those who in good faith report apparent misconduct" (*1*, p. 24) and, second, to item (i), "if the possible misconduct is not substantiated undertaking diligent efforts where appropriate to restore the reputation of those under investigation" (*1*, p. 24).

Concerning item (b), we believe this policy is morally wrong and contravenes the rights a citizen of the United States might reasonably expect. We know of no civil court in which an individual is prevented from knowing the identity of his accuser. This should not be the policy of universities, which are supposed to be centers of enlightenment and education. The policy suggests that anonymous informants should be condoned and that, by providing the descriptor "in good faith," some university official will have the wisdom and background to judge whether the individual is acting in good faith.

We believe the following. Those of us who have made a career in science value our integrity and that of our confreres above any other single trait; we rigorously apply this trait to our own research and in evaluating that of our peers; we have a moral obligation to report any suspected errors or suspected dishonesty to the individual whom we suspect, and then, if our doubts are not assuaged, to university officials. We must as a community demand the most rigorous and correct behavior of ourselves and of our peers; at no time should we hide behind a cloak of anonymity, and in no way should NIH or the university encourage or condone such an action.

Point (i) is equally troubling because it implies that regardless of efforts to protect an individual who has been accused, the conduct of an investigation will become general knowledge and will damage the individual during its course. There should be no need to "restore the reputation" of one who is unjustly accused, because in this nation we are innocent until proven guilty. If an investigation is conducted correctly and responsibly, then this recommendation is superfluous.

The federal regulations make no recommendations about how an institution is to carry out its inquiries. If we accept the federal role model, the local institutions will appoint a "Misconduct Policy Officer" or MPO. In considering this we note with amusement that the federal official designated as chief MPO is the Deputy Director for Extramural Research and Training, or DDERT. We do not believe the university should appoint a comparable official whose role will be to deal with "dedirt." Rather, we believe any alleged instance of misconduct should be considered by a panel of peers who are impressed with the solemnity of their task and who have access, as does the accused individual, to all facts and all persons involved in the process of accusation.

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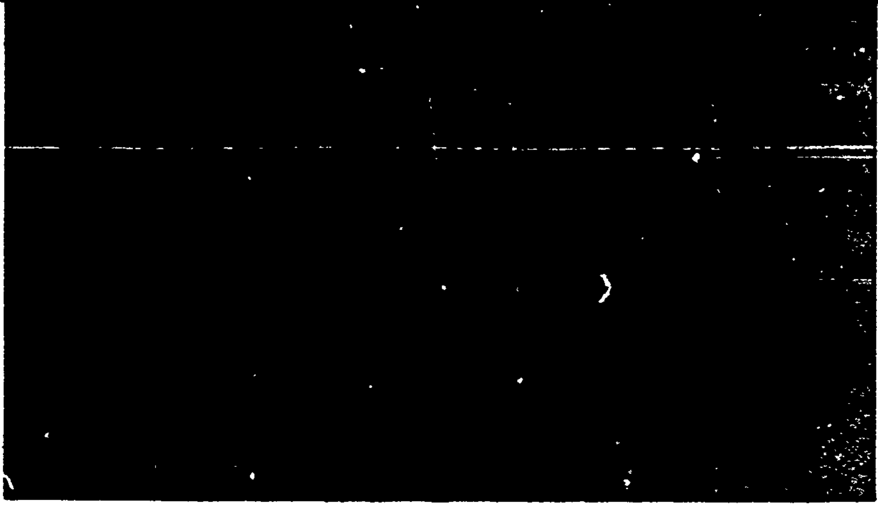


The effects of technological change on existing legal, social, and political structures and the consequences of new regulatory standards for the development and application of medical knowledge are dominant themes in current discussions of rights and duties in the health sciences. The health sciences are the source of the most visible and intense debates over questions of freedom and responsibility in science. As the miracles of modern medicine enter the mainstream of daily life, new issues arise: the cost and quality of health care; the role of government and other institutions in establishing minimum standards of prevention and protections for the rights of others; the meaning of concepts such as privacy, confidentiality, dignity, and autonomy in a society increasingly concerned with cost effectiveness and efficiency in the delivery of advanced medical care.

Leon R. Kass reflects on the ultimate goals of the new biology in his examination of the objectives of genetic engineering and biomedical research. The implications of privacy and confidentiality laws and regulations for future medical and epidemiological research in the United States are reviewed by Leon Gordis and Ellen Gold, who conclude that the rights of human subjects can be protected without hindering research advances, but that on occasion some individuals may need to yield certain rights for the benefit of society as a whole.

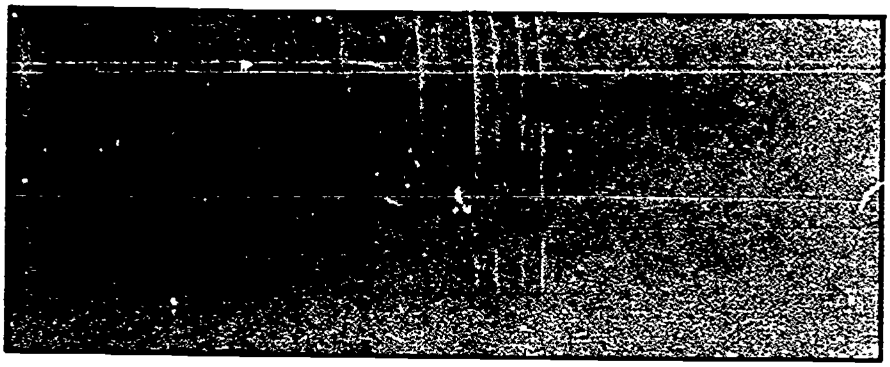
John C. Burnham, noting the social esteem and prestige that the medical profession enjoyed in the first half of the twentieth century, then traces the growing criticisms of the practice of American medicine. By the late 1950s, a campaign to modify the elevated position of physicians in American society was firmly rooted, he concludes, eventually leading to criticism of the social applications of medical science and of the competence of the individual physician.

Two articles in this chapter focus on opportunities and problems presented by genetic engineering and genetic screening. Arno G. Motulsky notes that the manipulation of recombinant DNA in human fertil zed eggs presents an unprecedented series of questions about interventions that will affect future generations, including questions of confidentiality, private versus societal goals, and self-determination. Peter T. Rowley asks whether genetic screening is a marvel that will free society from the scourge of genetic disease or a menace that will invade individual privacy and determine who may reproduce and under what conditions.

The authors in this chapter display a curious ambivalence about the promise and potential of science and technology in the health sciences. In contrast to the optimistic confidence of the authors in earlier chapters of this book, the contributors here reflect a more pragmatic outlook in applying the fruits of technical progress through existing social, political, and economic institutions. The inequities and conflicts among individual rights, social goals, and the conflicting objectives of private and public institutions may create friction that tends to escalate in the wake of each innovation or scientific change.

In the next chapter, the allocation of the risks associated with technical change is examined. Are those who will eventually bear the greatest risks from technical innovation able to participate in decisions about the manner in which these changes should be introduced into society? Once again, the need for greater involvement by scientists in educating the public at large is offered as a remedy for existing deficiencies in the political and economic mechanisms that regulate the pace and application of advances in science and technology.

—RC



Recent advances in biology and medicine suggest that we may be rapidly acquiring the power to modify and control the capacities and activities of men by direct intervention and manipulation of their bodies and minds. Certain means are already in use or at hand, others await the solution of relatively minor technical problems, while yet others, those offering perhaps the most precise kind of control, depend upon further basic research. Biologists who have considered these matters disagree on the question of how much how soon, but all agree that the power for "human engineering," to borrow from the jargon, is coming and that it will probably have profound social consequences.

These developments have been viewed both with enthusiasm and with alarm; they are only just beginning to receive serious attention. Several biologists have undertaken to inform the public about the technical possibilities, present and future. Practitioners of social science "futurology" are attempting to predict and describe the likely social consequences of and public responses to the new technologies. Lawyers and legislators are exploring institutional innovations for assessing new technologies. All of these activities are based upon the hope that we can harness the new technology of man for the betterment of mankind.

Yet this commendable aspiration points to another set of questions, which are, in my view, sorely neglected—questions that inquire into the meaning of phrases such as the "betterment of mankind." A *full* understanding of the new technology of man requires an exploration of ends, values, standards. What ends will or should the new techniques serve? What values should guide society's adjustments? By what standards should the assessment agencies assess? Behind these questions lie others: what is a good man, what is a good life for man, what is a good community? This article is an attempt to provoke discussion of these neglected and important questions.

While these questions about ends and ultimate ends are never unimportant or irrelevant, they have rarely been more important or more relevant. That this is so can be seen once we recognize that we are dealing here with a group of technologies that are in a decisive respect unique: the object upon which they operate is man himself. The technologies of energy or food production, of communication, of manufacture, and of motion greatly alter the implements available to man and the conditions in which he uses them. In contrast, the biomedical technology works to change the user himself. To be sure, the printing press, the automobile, the television, and the jet airplane have greatly altered the conditions under which and the way in which men live; but men as biological beings have remained largely unchanged. They have been, and remain, able to accept or reject, to use and abuse these technologies; they choose, whether wisely or foolishly, the ends to which these technologies are means. Biomedical technology may make it possible to change the inherent capacity for choice itself. Indeed, both those who welcome and those who fear the advent of "human engineering" ground their hopes and fears in the same prospect: *that man can for the first time recreate himself.*

Engineering the engineer seems to differ in kind from engineering his engine. Some have argued, however, that biomedical engineering does not differ qualitatively from toilet training, education, and moral teachings—all of which are forms of so-called "social engineering," which has man as its object, and is used by one generation to mold the next. In reply, it must at least be said that the techniques which have hitherto been employed are feeble and inefficient when compared to those on the horizon. This quantitative difference rests in part on a qualitative difference in the means of intervention. The traditional influences operate by speech or by symbolic deeds. They pay tribute to man as the animal who lives by speech and who understands the meanings of actions. Also, their effects are, in general, reversible, or at least subject to attempts at reversal. Each person has greater or lesser power to accept or reject or abandon them. In contrast, biomedical engineering circumvents the human context of speech and meaning, bypasses choice, and goes directly to work to modify the human material itself. Moreover, the changes wrought may be irreversible.

In addition, there is an important practical reason for considering the biomedical technology apart from other technologies. The advances we shall examine are fruits of a large, humane project dedicated to the conquest of disease and the relief of human suffering. The biologist and physician, regardless of their private motives, are seen, with justification, to be the well-wishers and benefactors of mankind. Thus, in a time in which technological advance is more carefully scrutinized and increasingly criticized, biomedical developments are still viewed by most people as benefits largely without qualification. The price we pay for these developments is thus more likely to go unrecognized. For this reason, I shall consider only the dangers and costs of biomedical advance. As the benefits are well known, there is no need to dwell upon them here. My discussion is deliberately partial.

I begin with a survey of the pertinent technologies. Next, I will consider some of the basic ethical and social problems in the use of these technologies. Then, I will briefly raise some fundamental questions to which these problems point. Finally, I shall offer some very general reflections on what is to be done.

The Biomedical Technologies

The biomedical technologies can be

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usefully organized into three groups, according to their major purpose: (i) control of death and life, (ii) control of human potentialities, and (iii) control of human achievement. The corresponding technologies are (i) medicine, especially the arts of prolonging life and of controlling reproduction, (ii) genetic engineering, and (iii) neurological and psychological manipulation. I shall briefly summarize each group of techniques.

1) *Control of death and life.* Previous medical triumphs have greatly increased average life expectancy. Yet other developments, such as organ transplantation or replacement and research into aging, hold forth the promise of increasing not just the average, but also the maximum life expectancy. Indeed, medicine seems to be sharpening its tools to do battle with death itself, as if death were just one more disease.

More immediately and concretely, available techniques of prolonging life —respirators, cardiac pacemakers, artificial kidneys—are already in the lists against death. Ironically, the success of these devices in forestalling death has introduced confusion in determining that death has, in fact, occurred. The traditional signs of life—heartbeat and respiration—can now be maintained entirely by machines. Some physicians are now busily trying to devise so-called "new definitions of death," while others maintain that the technical advances show that death is not a concrete event at all, but rather a gradual process, like twilight, incapable of precise temporal localization.

The real challenge to death will come from research into aging and senescence, a field just entering puberty. Recent studies suggest that aging is a genetically controlled process, distinct from disease, but one that can be manipulated and altered by diet or drugs. Extrapolating from animal studies, some scientists have suggested that a decrease in the rate of aging might also be achieved simply by effecting a very small decrease in human body temperature. According to some estimates, by the year 2000 it may be technically possible to add from 20 to 40 useful years to the period of middle life.

Medicine's success in extending life is already a major cause of excessive population growth: death control points to birth control. Although we are already technically competent, new techniques for lowering fertility and chemical agents for inducing abortion will greatly enhance our powers over conception and gestation. Problems of definition have been raised here as well. The need to determine when individuals acquire enforceable legal rights gives society an interest in the definition of human life and of the time when it begins. These matters are too familiar to need elaboration.

Technologies to conquer infertility proceed alongside those to promote it. The first successful laboratory fertilization of human egg by human sperm was reported in 1969 (*1*). In 1970, British scientists learned how to grow human embryos in the laboratory up to at least the blastocyst stage [that is, to the age of 1 week (*2*)]. We may soon hear about the next stage, the successful reimplantation of such an embryo into a woman previously infertile because of oviduct disease. The development of an artificial placenta, now under investigation, will make possible full laboratory control of fertilization and gestation. In addition, sophisticated biochemical and cytological techniques of monitoring the "quality" of the fetus have been and are being developed and used. These developments not only give us more power over the generation of human life, but make it possible to manipulate and to modify the quality of the human material.

2) *Control of human potentialities.* Genetic engineering, when fully developed, will wield two powers not shared by ordinary medical practice. Medicine treats existing individuals and seeks to correct deviations from a norm of health. Genetic engineering, in contrast, will be able to make changes that can be transmitted to succeeding generations and will be able to create new capacities, and hence to establish new norms of health and fitness.

Nevertheless, one of the major interests in genetic manipulation is strictly medical: to develop treatments for individuals with inherited diseases. Genetic disease is prevalent and increasing, thanks partly to medical advances that enable those affected to survive and perpetuate their mutant genes. The hope is that normal copies of the appropriate gene, obtained biologically or synthesized chemically, can be introduced into defective individuals to correct their deficiencies. This *therapeutic* use of genetic technology appears to be far in the future. Moreover, there is some doubt that it will ever be practical, since the same end could be more easily achieved by transplanting cells or organs that could compensate for the missing or defective gene product.

Far less remote are technologies that could serve *eugenic* ends. Their development has been endorsed by those concerned about a general deterioration of the human gene pool and by others who believe that even an undeteriorated human gene pool needs upgrading. Artificial insemination with selected donors, the eugenic proposal of Herman Muller (*3*), has been possible for several years because of the perfection of methods for long-term storage of human spermatozoa. The successful maturation of human oocytes in the laboratory and their subsequent fertilization now make it possible to select donors of ova as well. But a far more suitable technique for eugenic purposes will soon be upon us—namely, nuclear transplantation, or cloning. Bypassing the lottery of sexual recombination, nuclear transplantation permits the asexual reproduction or copying of an already developed individual. The nucleus of a mature but unfertilized egg is replaced by a nucleus obtained from a specialized cell of an adult organism or embryo (for example, a cell from the intestines or the skin). The egg with its transplanted nucleus develops as if it had been fertilized and, barring complications, will give rise to a normal adult organism. Since almost all the hereditary material (DNA) of a cell is contained within its nucleus, the renucleated egg and the individual into which it develops are genetically identical to the adult organism that was the source of the donor nucleus. Cloning could be used to produce sets of unlimited numbers of genetically identical individuals, each set derived from a single parent. Cloning has been successful in amphibians and is now being tried in mice; its extension to man merely requires the solution of certain technical problems.

Production of man-animal chimeras by the introduction of selected nonhuman material into developing human embryos is also expected. Fusion of human and nonhuman cells in tissue culture has already been achieved.

Other, less direct means for influencing the gene pool are already available, thanks to our increasing ability to identify and diagnose genetic diseases. Genetic counselors can now detect biochemically and cytologically a variety of severe genetic defects (for example, Mongolism, Tay-Sachs disease) while the fetus is still in utero. Since treatments are at present largely unavailable, diagnosis is often followed by abortion of the affected fetus. In the future, more sensitive tests will also permit the detection of heterozygote carriers, the unaffected individuals who carry but a single dose of a given deleterious gene. The eradication of a given genetic disease might then be attempted by aborting all such carriers. In fact, it was recently suggested that the fairly common disease cystic fibrosis could be completely eliminated over the next 40 years by screening all pregnancies and aborting the 17,000,000 unaffected fetuses that will carry a single gene for this disease. Such zealots need to be reminded of the consequences should each geneticist be allowed an equal assault on his favorite genetic disorder, given that each human being is a carrier for some four to eight such recessive, lethal genetic diseases.

3) *Control of human achievement.* Although human achievement depends at least in part upon genetic endowment, heredity determines only the material upon which experience and education impose the form. The limits of many capacities and powers of an individual are indeed genetically determined, but the nurturing and perfection of these capacities depend upon other influences. Neurological and psychological manipulation hold forth the promise of controlling the development of human capacities, particularly those long considered most distinctively human: speech, thought, choice, emotion, memory, and imagination.

These techniques are now in a rather primitive state because we understand so little about the brain and mind. Nevertheless, we have already seen the use of electrical stimulation of the human brain to produce sensations of intense pleasure and to control rage, the use of brain surgery (for example, frontal lobotomy) for the relief of severe anxiety, and the use of aversive conditioning with electric shock to treat sexual perversion. Operant-conditioning techniques are widely used, apparently with success, in schools and mental hospitals. The use of so-called consciousness-expanding and hallucinogenic drugs is widespread, to say nothing of tranquilizers and stimulants. We are promised drugs to modify memory, intelligence, libido, and aggressiveness.

The following passages from a recent book by Yale neurophysiologist José Delgado—a book instructively entitled *Physical Control of the Mind: Toward a Psychocivilized Society*—should serve to make this discussion more concrete. In the early 1950's, it was discovered that, with electrodes placed in certain discrete regions of their brains, animals would repeatedly and indefatigably press levers to stimulate their own brains, with obvious resultant enjoyment. Even starving animals preferred stimulating these so-called pleasure centers to eating. Delgado comments on the electrical stimulation of a similar center in a human subject (*4*, p. 185).

> [T]he patient reported a pleasant tingling sensation in the left side of her body 'from my face down to the bottom of my legs.' She started giggling and making funny comments, stating that she enjoyed the sensation 'very much.' Repetition of these stimulations made the patient more communicative and flirtatious, and she ended by openly expressing her desire to marry the therapist.

And one further quotation from Delgado (*4*, p. 88).

> Leaving wires inside of a thinking brain may appear unpleasant or dangerous, but actually the many patients who have undergone this experience have not been concerned about the fact of being wired, nor have they felt any discomfort due to the presence of conductors in their heads. Some women have shown their feminine adaptability to circumstances by wearing attractive hats or wigs to conceal their electrical headgear, and many people have been able to enjoy a normal life as outpatients, returning to the clinic periodically for examination and stimulation. In a few cases in which contacts were located in pleasurable areas, patients have had the opportunity to stimulate their own brains by pressing the button of a portable instrument, and this procedure is reported to have therapeutic benefits.

It bears repeating that the sciences of neurophysiology and psychopharmacology are in their infancy. The techniques that are now available are crude, imprecise, weak, and unpredictable, compared to those that may flow from a more mature neurobiology.

Basic Ethical and Social Problems in the Use of Biomedical Technology

After this cursory review of the powers now and soon to be at our disposal, I turn to the questions concerning the use of these powers. First, we must recognize that questions of use of science and technology are always moral and political questions, never simply technical ones. All private or public decisions to develop or to use biomedical technology—and decisions *not* to do so—inevitably contain judgments about value This is true even if the values guiding those decisions are not articulated or made clear, as indeed they often are not. Secondly, the value judgments cannot be derived from biomedical science. This is true even if scientists themselves make the decisions.

These important points are often overlooked for at least three reasons.

1) They are obscured by those who like to speak of "the control of nature by science." It is men who control, not that abstraction "science." Science may provide the means, but men choose the ends; the choice of ends comes from beyond science.

2) Introduction of new technologies often appears to be the result of no decision whatsoever, or of the culmination of decisions too small or unconscious to be recognized as such. What can be done is done. However, someone is deciding on the basis of some notions of desirability, no matter how self-serving or altruistic.

3) Desires to gain or keep money and power no doubt influence much of what happens, but these desires can also be formulated as reasons and then discussed and debated.

Insofar as our society has tried to deliberate about questions of use, how has it done so? Pragmatists that we are, we prefer a utilitarian calculus: we weigh "benefits" against "risks," and we weigh them for both the individual and "society." We often ignore the fact that the very definitions of "a benefit" and "a risk" are themselves based upon judgments about value. In the biomedical areas just reviewed, the benefits are considered to be self-evident: prolongation of life, control of fertility and of population size, treatment and prevention of genetic disease, the reduction of anxiety and aggressiveness, and the enhancement of memory, intelligence, and pleasure. The assessment of risk is, in general, simply pragmatic—will the technique work effectively and reliably, how much will it cost, will it do detectable bodily harm, and who will complain if we proceed with development? As these questions are familiar and congenial, there is no need to belabor them.

The very pragmatism that makes us sensitive to considerations of economic cost often blinds us to the larger social costs exacted by biomedical advances. For one thing, we seem to be unaware that we may not be able to maximize all the benefits, that several of the goals we are promoting conflict with each other. On the one hand, we seek to control population growth by lowering fertility; on the other hand, we develop techniques to enable every infertile woman to bear a child. On the one hand, we try to extend the lives of individuals with genetic disease; on the other, we wish to eliminate deleterious genes from the human population. I am not urging that we resolve these conflicts in favor of one side or the other, but simply that we recognize that such conflicts exist. Once we do, we are more likely to appreciate that most "progress" is heavily paid for in terms not generally included in the simple utilitarian calculus.

To become sensitive to the larger costs of biomedical progress, we must attend to several serious ethical and social questions. I will briefly discuss three of them: (i) questions of distributive justice, (ii) questions of the use and abuse of power, and (iii) questions of self-degradation and dehumanization.

Distributive Justice

The introduction of any biomedical technology presents a new instance of an old problem—how to distribute scarce resources justly. We should assume that demand will usually exceed supply. Which people should receive a kidney transplant or an artificial heart? Who should get the benefits of genetic therapy or of brain stimulation? Is "first-come, first-served" the fairest principle? Or are certain people "more worthy," and if so, on what grounds?

It is unlikely that we will arrive at answers to these questions in the form of deliberate decisions. More likely, the problem of distribution will continue to be decided ad hoc and locally. If so, the consequence will probably be a sharp increase in the already far too great inequality of medical care. The extreme case will be longevity, which will probably be, at first, obtainable only at great expense. Who is likely to be able to buy it? Do conscience and prudence permit us to enlarge the gap between rich and poor, especially with respect to something as fundamental as life itself?

Questions of distributive justice also arise in the earlier decisions to acquire new knowledge and to develop new techniques. Personnel and facilities for medical research and treatment are scarce resources. Is the development of a new technology the best use of the limited resources, given current circumstances? How should we balance efforts aimed at prevention against those aimed at cure, or either of these against efforts to redesign the species? How should we balance the delivery of available levels of care against further basic research? More fundamentally, how should we balance efforts in biology and medicine against efforts to eliminate poverty, pollution, urban decay, discrimination, and poor education? This last question about distribution is perhaps the most profound. We should reflect upon the social consequences of seducing many of our brightest young people to spend their lives locating the biochemical defects in rare genetic diseases, while our more serious problems go begging. The current squeeze on money for research provides us with an opportunity to rethink and reorder our priorities.

Problems of distributive justice are frequently mentioned and discussed, but they are hard to resolve in a rational manner. We find them especially difficult because of the enormous range of conflicting values and interests that characterizes our pluralistic society. We cannot agree—unfortunately, we often do not even try to agree—on standards for just distribution. Rather, decisions tend to be made largely out of a clash of competing interests. Thus, regrettably, the question of how to distribute justly often gets reduced to who shall decide how to distribute. The question about justice has led us to the question about power.

Use and Abuse of Power

We have difficulty recognizing the problems of the exercise of power in the biomedical enterprise because of our delight with the wondrous fruits it has yielded. This is ironic because the notion of power is absolutely central to the modern conception of science. The ancients conceived of science as the *understanding* of nature, pursued for its own sake. We moderns view science as power, as *control* over nature; the conquest of nature "for the relief of man's estate" was the charge issued by Francis Bacon, one of the leading architects of the modern scientific project (5).

Another source of difficulty is our fondness for speaking of the abstraction "Man." I suspect that we prefer to speak figuratively about "Man's power over Nature" because it obscures an unpleasant reality about human affairs. It is in fact particular men who wield power, not Man. What we really mean by "Man's power over Nature" is a power exercised by some men over other men, with a knowledge of nature as their instrument.

While applicable to technology in general, these reflections are especially pertinent to the technologies of human engineering, with which men deliberately exercise power over future generations. An excellent discussion of this question is found in *The Abolition of Man*, by C. S. Lewis (6).

It is, of course, a commonplace to complain that men have hitherto used badly, and against their fellows, the powers that science has given them. But that is not the point I am trying to make. I am not speaking of particular corruptions and abuses which an increase of moral virtue would cure: I am considering what the thing called "Man's power over Nature" must always and essentially be. . . .

In reality, of course, if any one age really attains, by eugenics and scientific education, the power to make its descendants what it pleases, all men who live after it are the patients of that power. They are weaker, not stronger: for though we may have put wonderful machines in their hands, we have pre-ordained how they are to use them. . . . The real picture is that of one dominant age . . . which resists all previous ages most successfully and dominates all subsequent ages most irresistibly, and thus is the real master of the human species. But even within this master generation (itself an infinitesimal minority of the species) the power will be exercised by a minority smaller still. Man's conquest of Nature, if the dreams of some scientific planners are realized, means the rule of a few hundreds of men over billions upon billions of men. There neither is nor can be any simple increase of power on Man's side. Each new power won *by* man is a power *over* man as well. Each advance leaves him weaker as well as stronger. In every victory, besides being the general who triumphs, he is also the prisoner who follows the triumphal car.

Please note that I am not yet speaking about the problem of the misuse or abuse of power. The point is rather that the power which grows is unavoidably the power of only some men, and that the number of powerful men decreases as power increases.

Specific problems of abuse and misuse of specific powers must not, however, be overlooked. Some have voiced the fear that the technologies of genetic engineering and behavior control, though developed for good purposes, will be put to evil uses. These fears are perhaps somewhat exaggerated, if only because biomedical technologies would add very little to our highly developed arsenal for mischief, destruction, and stultification. Nevertheless, any proposal for large-scale human engineering should make us wary. Consider a program of positive eugenics based upon the widespread practice of asexual reproduction. Who shall decide what constitutes a superior individual worthy of replication? Who shall decide which individuals may or must reproduce, and by which method? These are questions easily answered only for a tyrannical regime.

Concern about the use of power is equally necessary in the selection of means for desirable or agreed-upon ends. Consider the desired end of limiting population growth. An effective program of fertility control is likely to be coercive. Who should decide the choice of means? Will the program penalize "conscientious objectors"?

Serious problems arise simply from obtaining and disseminating information, as in the mass screening programs now being proposed for detection of genetic disease. For what kinds of disorders is compulsory screening justified? Who shall have access to the data obtained, and for what purposes? To whom does information about a person's genotype belong? In ordinary medical practice, the patient's privacy is protected by the doctor's adherence to the principle of confidentiality. What will protect his privacy under conditions of mass screening?

More than privacy is at stake if screening is undertaken to detect psychological or behavioral abnormalities. A recent proposal, tendered and supported high in government, called for the psychological testing of all 6-year-olds to detect future criminals and misfits. The proposal was rejected; current tests lack the requisite predictive powers. But will such a proposal be rejected if reliable tests become available? What if certain genetic disorders, diagnosable in childhood, can be shown to correlate with subsequent antisocial behavior? For what degree of correlation and for what kinds of behavior can mandatory screening be justified? What use should be made of the data? Might not the dissemination of the information itself undermine the individual's chance for a worthy life and contribute to his so-called antisocial tendencies?

Consider the seemingly harmless effort to redefine clinical death. If the need for organs for transplantation is the stimulus for redefining death, might not this concern influence the definition at the expense of the dying? One physician, in fact, refers in writing to the revised criteria for declaring a patient dead as a "new definition of heart donor eligibility" (7, p. 526).

Problems of abuse of power arise in the acquisition of basic knowledge. The securing of a voluntary and informed consent is an abiding problem in the use of human subjects in experimentation. Gross coercion and deception are now rarely a problem; the pressures are generally subtle, often related to an intrinsic power imbalance in favor of the experimentalist.

A special problem arises in experiments on or manipulations of the unborn. Here it is impossible to obtain the consent of the human subject. If the purpose of the intervention is therapeutic—to correct a known genetic abnormality, for example—consent can reasonably be implied. But can anyone ethically consent to nontherapeutic interventions in which parents or scientists work their wills or their eugenic visions on the child-to-be? Would not such manipulation represent in itself an abuse of power, independent of consequences?

There are many clinical situations which already permit, if not invite, the manipulative or arbitrary use of powers provided by biomedical technology: obtaining organs for transplantation, refusing to let a person die with dignity, giving genetic counselling to a frightened couple, recommending eugenic sterilization for a mental retardate, ordering electric shock for a homosexual. In each situation, there is an opportunity to violate the will of the patient or subject. Such opportunities have generally existed in medical practice, but the dangers are becoming increasingly serious. With the growing complexity of the technologies, the technician gains in authority, since he alone can understand what he is doing. The patient's lack of knowledge makes him deferential and often inhibits him from speaking up when he feels threatened. Physicians *are* sometimes troubled by their increasing power, yet they feel they cannot avoid its exercise. "Reluctantly," one commented to me, "we shall have to play God." With what guidance and to what ends I shall consider later. For the moment, I merely ask: "By whose authority?"

While these questions about power are pertinent and important, they are in one sense misleading. They imply an inherent conflict of purpose between physician and patient, between scientist and citizen. The discussion conjures up images of master and slave, of oppressor and oppressed. Yet it must be remembered that conflict of purpose is largely absent, especially with regard to general goals. To be sure, the purposes of medical scientists are not always the same as those of the subjects experimented on. Nevertheless, basic sponsors and partisans of biomedical technology are precisely those upon whom the technology will operate. The will of the scientist and physician is happily married to (rather, is the offspring of) the desire of all of us for better health, longer life, and peace of mind.

Most future biomedical technologies will probably be welcomed, as have those of the past. Their use will require little or no coercion. Some developments, such as pills to improve memory, control mood, or induce pleasure, are likely to need no promotion. Thus, even if we should escape from the dangers of coercive manipulation, we shall still face large problems posed by the voluntary use of biomedical technology, problems to which I now turn.

Voluntary Self-Degradation and Dehumanization

Modern opinion is sensitive to problems of restriction of freedom and abuse of power. Indeed, many hold that a man can be injured only by violating his will. But this view is much too narrow. It fails to recognize the great dangers we shall face in the use of biomedical technology, dangers that stem from an excess of freedom, from the uninhibited exercises of will. In my view, our greatest problem will increasingly be one of voluntary self-degradation, or willing dehumanization.

Certain desired and perfected medical technologies have already had some dehumanizing consequences. Improved methods of resuscitation have made possible heroic efforts to "save" the severely ill and injured. Yet these efforts are sometimes only partly successful; they may succeed in salvaging individuals with severe brain damage, capable of only a less-than-human, vegetating existence. Such patients, increasingly found in the intensive care units of university hospitals, have been denied a death with dignity. Families are forced to suffer seeing their loved ones so reduced, and are made to bear the burdens of a protracted death watch.

Even the ordinary methods of treating disease and prolonging life have impoverished the context in which men die. Fewer and fewer people die in the familiar surroundings of home or in the company of family and friends. At that time of life when there is perhaps the greatest need for human warmth and comfort, the dying patient is kept company by cardiac pacemakers and defibrillators, respirators, aspirators, oxygenators, catheters, and his intravenous drip.

But the loneliness is not confined to the dying patient in the hospital bed. Consider the increasing number of old people who are still alive, thanks to medical progress. As a group, the elderly are the most alienated members of our society. Not yet ready for the world of the dead, not deemed fit for the world of the living, they are shunted aside. More and more of them spend the extra years medicine has given them in "homes for senior citizens," in chronic hospitals in nursing homes—waiting for the end. We have learned how to increase their years, but we have not learned how to help them enjoy their days. And yet, we bravely and relentlessly push back the frontiers against death.

Paradoxically, even the young and vigorous may be suffering because of medicine's success in removing death from their personal experience. Those born since penicillin represent the first generation ever to grow up without the experience or fear of probable unexpected death at an early age. They look around and see that virtually all of their friends are alive. A thoughtful physician, Eric Cassell, has remarked on this in "Death and the physician" (*8*, p. 76):

> [W]hile the gift of time must surely be marked as a great blessing, the *perception* of time, as stretching out endlessly before us, is somewhat threatening. Many of us function best under deadlines, and tend to procrastinate when time limits are not set. . . . Thus, this unquestioned boon, the extension of life, and the removal of the threat of premature death, carries with it an unexpected anxiety: the anxiety of an unlimited future.
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> In the young, the sense of limitless time has apparently imparted not a feeling of limitless opportunity, but increased stress and anxiety, in addition to the anxiety which results from other modern freedoms: personal mobility, a wide range of occupational choice, and independence from the limitations of class and familial patterns of work. . . . A certain aimlessness (often ringed around with great social consciousness) characterizes discussions about their own aspirations. The future is endless, and their inner demands seem minimal. Although it may appear uncharitable to say so, they seem to be acting in a way best described as "childish"—particularly in their lack of a time sense. They behave as though there were no tomorrow, or as though the time limits imposed by the biological facts of life had become so vague for them as to be nonexistent.

Consider next the coming power over reproduction and genotype. We endorse the project that will enable us to control numbers and to treat individuals with genetic disease. But our desires outrun these defensible goals. Many would welcome the chance to become parents without the inconvenience of pregnancy; others would wish to know in advance the characteristics of their offspring (sex, height, eye color, intelligence); still others would wish to design these characteristics to suit their tastes. Some scientists have called for the use of the new technologies to assure the "quality" of all new babies (*9*). As one obstetrician put it: "The business of obstetrics is to produce *optimum* babies." But the price to be paid for the "optimum baby" is the transfer of procreation from the home to the laboratory and its coincident transformation into manufacture. Increasing control over the product is purchased by the increasing depersonalization of the process. The complete depersonalization of procreation (possible with the development of an artificial placenta) shall be, in itself, seriously dehumanizing, no matter how optimum the product. It should not be forgotten that human procreation not only issues new human beings, but is itself a human activity.

Procreation is not simply an activity of the rational will. It is a more complete human activity precisely because it engages us bodily and spiritually, as well as rationally. Is there perhaps some wisdom in that mystery of nature which joins the pleasure of sex, the communication of love, and the desire for children in the very activity by which we continue the chain of human existence? Is not biological parenthood a built-in "mechanism," selected because it fosters and supports in parents an adequate concern for and commitment to their children? Would not the laboratory production of human beings no longer be *human* procreation? Could it keep human parenthood human?

The dehumanizing consequences of programmed reproduction extend beyond the mere acts and processes of life-giving. Transfer of procreation to the laboratory will no doubt weaken what is presently for many people the best remaining justification and support for the existence of marriage and the family. Sex is now comfortably at home outside of marriage; child-rearing is progressively being given over to the state, the schools, the mass media, and the child-care centers. Some have argued that the family, long the nursery of humanity, has outlived its usefulness. To be sure, laboratory and governmental alternatives might be designed for procreation and child-rearing, but at what cost?

This is not the place to conduct a full evaluation of the biological family. Nevertheless, some of its important virtues are, nowadays, too often overlooked. The family is rapidly becoming the only institution in an increasingly impersonal world where each person is loved not for what he does or makes, but simply because he is. The family is also the institution where most of us, both as children and as parents, acquire a sense of continuity with the past and a sense of commitment to the future. Without the family, we would have little incentive to take an interest in anything after our own deaths. These observations suggest that the elimination of the family would weaken ties to past and future, and would throw us, even more than we are now, to the mercy of an impersonal, lonely present.

Neurobiology and psychobiology probe most directly into the distinctively human. The technological fruit of these sciences is likely to be both more tempting than Eve's apple and more "catastrophic" in its result (*10*). One need only consider contemporary drug use to see what people are willing to risk or sacrifice for novel experiences, heightened perceptions, or just "kicks." The possibility of drug-induced, instant, and effortless gratification will be welcomed. Recall the possibilities of voluntary self-stimulation of the brain to reduce anxiety, to heighten pleasure, or to create visual and auditory sensations unavailable through the peripheral sense organs. Once these techniques are perfected and safe, is there much doubt that they will be desired, demanded, and used?

What ends will these techniques serve? Most likely, only the most elemental, those most tied to the bodily pleasures. What will happen to thought, to love, to friendship, to art, to judgment, to public-spiritedness in a society with a perfected technology of pleasure? What kinds of creatures will we become if we obtain our pleasure by drug or electrical stimulation without the usual kind of human efforts and frustrations? What kind of society will we have?

We need only consult Aldous Huxley's prophetic novel *Brave New World* for a likely answer to these questions. There we encounter a society dedicated to homogeneity and stability, administered by means of instant gratifications and peopled by creatures of human shape but of stunted humanity. They consume, fornicate, take "soma," and operate the machinery that makes it all possible. They do not read, write, think, love, or govern themselves. Creativity and curiosity, reason and passion, exist only in a rudimentary and multilated form. In short, they are not men at all.

True, our techniques, like theirs, may in fact enable us to treat schizophrenia, to alleviate anxiety, to curb aggressiveness. We, like they, may indeed be able to save mankind from itself, but probably only at the cost of its humanness. In the end, the price of relieving man's estate might well be the abolition of man (*11*).

There are, of course, many other routes leading to the abolition of man. There are many other and better known causes of dehumanization. Disease, starvation, mental retardation, slavery, and brutality—to name just a few—have long prevented many, if not most, people from living a fully human life. We should work to reduce and eventually to eliminate these evils. But the existence of these evils should not prevent us from appreciating that the use of the technology of man, uninformed by wisdom concerning proper human ends, and untempered by an appropriate humility and awe, can unwittingly render us all irreversibly less than human. For, unlike the man reduced by disease or slavery, the people dehumanized à la *Brave New World* are not miserable, do not know that they are dehumanized, and, what is worse, would not care if they knew. They are, indeed, happy slaves, with a slavish happiness.

Some Fundamental Questions

The practical problems of distributing scarce resources, of curbing the abuses of power, and of preventing voluntary dehumanization point beyond themselves to some large, enduring, and most difficult questions: the nature of justice and the good community, the nature of man and the good for man. My appreciation of the profundity of these questions and my own ignorance before them makes me hesitant to say more about them. Nevertheless, previous failures to find a shortcut around them have led me to believe that these questions must be faced if we are to have any hope of understanding where biology is taking us. Therefore, I shall try to show in outline how I think some of the larger questions arise from my discussion of dehumanization and self-degradation.

My remarks on dehumanization can hardly fail to arouse argument. It might be said, correctly, that to speak about dehumanization presupposes a concept of "the distinctively human." It might also be said, correctly, that to speak about wisdom concerning proper human ends presupposes that such ends do in fact exist and that they may be more or less accessible to human understanding, or at least to rational inquiry. It is true that neither presupposition is at home in modern thought.

The notion of the "distinctively human" has been seriously challenged by modern scientists. Darwinists hold that man is, at least in origin, tied to the subhuman; his seeming distinctiveness is an illusion or, at most, not very important. Biochemists and molecular biologists extend the challenge by blurring the distinction between the living and the nonliving. The laws of physics and chemistry are found to be valid and are held to be sufficient for explaining biological systems. Man is a collection of molecules, an accident on the stage of evolution, endowed by chance with the power to change himself, but only along determined lines.

Psychoanalysts have also debunked the "distinctly human." The essence of man is seen to be located in those drives he shares with other animals—pursuit of pleasure and avoidance of pain. The so-called "higher functions" are understood to be servants of the more elementary, the more base. Any distinctiveness or "dignity" that man has consists of his superior capacity for gratifying his animal needs.

The idea of "human good" fares no better. In the social sciences, historicists and existentialists have helped drive this question underground. The former hold all notions of human good to be culturally and historically bound, and hence mutable. The latter hold that values are subjective: each man makes his own, and ethics becomes simply the cataloging of personal tastes.

Such appear to be the prevailing opinions. Yet there is nothing novel about reductionism, hedonism, and relativism; these are doctrines with which Socrates contended. What is new is that these doctrines seem to be vindicated by scientific advance. Not only do the scientific notions of nature and of man flower into verifiable predictions, but they yield marvelous fruit. The technological triumphs are held to validate their scientific foundations. Here, perhaps, is the most pernicious result of technological progress—more dehumanizing than any actual manipulation or technique, present or future. We are witnessing the erosion, perhaps the final erosion, of the idea of man as something splendid or divine, and its replacement with a view that sees man, no less than nature, as simply more raw material for manipulation and homogenization. Hence, our peculiar moral crisis. We are in turbulent seas without a landmark precisely because we adhere more and more to a view of nature and of man which both gives us enormous power and, at the same time, denies all possibility of standards to guide its use. Though well-equipped, we know not who we are nor where we are going. We are left to the accidents of our hasty, biased, and ephemeral judgments.

Let us not fail to note a painful irony: our conquest of nature has made us the slaves of blind chance. We triumph over nature's unpredictabilities only to subject ourselves to the still greater unpredictability of our capricious wills and our fickle opinions. That we have a method is no proof against our madness. Thus, engineering the engineer as well as the engine, we race our train we know not where (*12*).

While the disastrous consequences of ethical nihilism are insufficient to refute it, they invite and make urgent a reinvestigation of the ancient and enduring questions of what is a proper life for a human being, what is a good community, and how are they achieved (*13*). We must not be deterred from these questions simply because the best minds in human history have failed to settle them. Should we not rather be encouraged by the fact that they considered them to be the most important questions?

As I have hinted before, our ethical dilemma is caused by the victory of modern natural science with its nonteleological view of man. We ought therefore to reexamine with great care the modern notions of nature and of man, which undermine those earlier notions that provide a basis for ethics.

If we consult our common experience, we are likely to discover some grounds for believing that the questions about man and human good are far from closed. Our common experience suggests many difficulties for the modern "scientific view of man." For example, this view fails to account for the concern for justice and freedom that appears to be characteristic of all human societies (*14*). It also fails to account for or to explain the fact that men have speech and not merely voice, that men can choose and act and not merely move or react. It fails to explain why men engage in moral discourse, or, for that matter, why they speak at all. Finally, the "scientific view of man" cannot account for scientific inquiry itself, for why men seek to know. Might there not be something the matter with a knowledge of man that does not explain or take account of his most distinctive activities, aspirations, and concerns (*15*)?

Having gone this far, let me offer one suggestion as to where the difficulty might lie: in the modern understanding of knowledge. Since Bacon, as I have mentioned earlier, technology has increasingly come to be the basic justification for scientific inquiry. The end is power, not knowledge for its own sake. But power is not only the end. It is also an important *validation* of knowledge. One definitely knows that one knows only if one can make. Synthesis is held to be the ultimate proof of understanding (*16*). A more radical formulation holds that one knows only what one makes: knowing *equals* making.

Yet therein lies a difficulty. If truth be the power to change or to make the object studied, then of what do we have knowledge? If there are no fixed realities, but only material upon which we may work our wills, will not "science" be merely the "knowledge" of the transient and the manipulatable? We might indeed have knowledge of the laws by which things change and the rules for their manipulation, but no knowledge of the things themselves. Can such a view of "science" yield any knowledge about the nature of man, or indeed, about the nature of anything? Our questions appear to lead back to the most basic of questions: What does it mean to know? What is it that is knowable (*17*)?

We have seen that the practical problems point toward and make urgent certain enduring, fundamental questions. Yet while pursuing these questions, we cannot afford to neglect the practical problems as such. Let us not forget Delgado and the "psychocivilized society." The philosophical inquiry could be rendered moot by our blind, confident efforts to dissect and redesign ourselves. While awaiting a reconstruction of theory, we must act as best we can.

What Is To Be Done?

First, we sorely need to recover some humility in the face of our awesome powers. The arguments I have presented should make apparent the folly of arrogance, of the presumption that we are wise enough to remake ourselves. Because we lack wisdom, caution is our urgent need. Or to put it another way, in the absence of that "ultimate wisdom," we can be wise enough to know that we are not wise enough. When we lack sufficient wisdom to do, wisdom consists in not doing. Caution, restraint, delay, abstention are what this second-best (and, perhaps, only) wisdom dictates with respect to the technology for human engineering.

If we can recognize that biomedical advances carry significant social costs, we may be willing to adopt a less permissive, more critical stance toward new developments. We need to reexamine our prejudice not only that all biomedical innovation is progress, but also that it is inevitable. Precedent certainly favors the view that what can be done will be done, but is this necessarily so? Ought we not to be suspicious when technologists speak of coming developments as automatic, not subject to human control? Is there not something contradictory in the notion that we have the power to control all the untoward consequences of a technology, but lack the power to determine whether it should be developed in the first place?

What will be the likely consequences of the perpetuation of our permissive and fatalistic attitude toward human engineering? How will the large decisions be made? Technocratically and self-servingly, if our experience with previous technologies is any guide. Under conditions of laissez-faire, most technologists will pursue techniques, and most private industries will pursue profits. We are fortunate that, apart from the drug manufacturers, there are at present in the biomedical area few large industries that influence public policy. Once these appear, the voice of "the public interest" will have to shout very loudly to be heard above their whisperings in the halls of Congress. These reflections point to the need for institutional controls.

Scientists understandably balk at the notion of the regulation of science and technology. Censorship is ugly and often based upon ignorant fear; bureaucratic regulation is often stupid and inefficient. Yet there is something disingenuous about a scientist who professes concern about the social consequences of science, but who responds to every suggestion of regulation with one or both of the following: "No restrictions on scientific research," and "Technological progress should not be curtailed." Surely, to suggest that *certain* technologies ought to be regulated or forestalled is not to call for the halt of *all* technological progress (and says nothing at all about basic research). Each development should be considered on its own merits. Although the dangers of regulation cannot be dismissed, who, for example, would still object to efforts to obtain an effective, complete, global prohibition on the development, testing, and use of biological and nuclear weapons?

The proponents of laissez-faire ignore two fundamental points. They ignore the fact that not to regulate is as much a policy decision as the opposite, and that it merely postpones the time of regulation. Controls will eventually be called for—as they are now being demanded to end environmental pollution. If attempts are not made early to detect and diminish the social costs of biomedical advances by intelligent institutional regulation, the society is likely to react later with more sweeping, immoderate, and throttling controls.

The proponents of laissez-faire also ignore the fact that much of technology is already regulated. The federal government is already deep in research and development (for example, space, electronics, and weapons) and is the principal sponsor of biomedical research. One may well question the wisdom of the direction given, but one would be wrong in arguing that technology cannot survive social control. Clearly, the question is not control versus no control, but rather what kind of control, when, by whom, and for what purpose.

Means for achieving international regulation and control need to be devised. Biomedical technology can be no nation's monopoly. The need for international agreements and supervision can readily be understood if we consider the likely American response to the successful asexual reproduction of 10,000 Mao Tse-tungs.

To repeat, the basic short-term need is caution. Practically, this means that we should shift the burden of proof to the *proponents* of a new biomedical technology. Concepts of "risk" and "cost" need to be broadened to include some of the social and ethical consequences discussed earlier. The probable or possible harmful effects of the widespread use of a new technique should be anticipated and introduced as "costs" to be weighed in deciding about the *first* use. The regulatory institutions should be encouraged to exercise restraint and to formulate the grounds for saying "no." We must all get used to the idea that biomedical technology makes possible many things we should never do.

But caution is not enough. Nor are clever institutional arrangements. Institutions can be little better than the people who make them work. However worthy our intentions, we are deficient in understanding. In the *long* run, our hope can only lie in education: in a public educated about the meanings and limits of science and enlightened in its use of technology; in scientists better educated to understand the relationships between science and technology on the one hand, and ethics and politics on the other; in human beings who are as wise in the latter as they are clever in the former.

References and Notes

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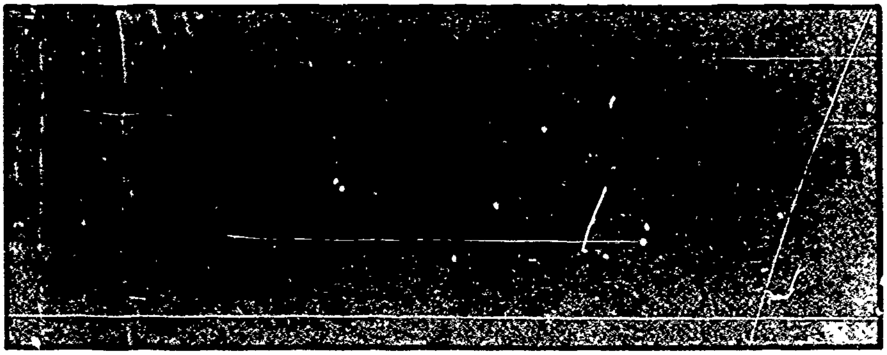
10. It is, of course, a long-debated question as to whether the fall of Adam and Eve ought to be considered "catastrophic," or more precisely, whether the Hebrew tradition considered it so. I do not mean here to be taking sides in this quarrel by my use of the term "catastrophic," and, in fact, tend to line up on the negative side of the questions, as put above. Curiously, as Aldous Huxley's *Brave New World* [(Harper & Row, New York, 1969)] suggests, the implicit goal of the biomedical technology could well be said to be the reversal of the Fall and a return of man to the hedonic and immortal existence of the Garden of Eden. Yet I can point to at least two problems. First, the new Garden of Eden will probably have no gardens; the received, splendid world of nature will be buried beneath asphalt, concrete, and other human fabrications, a transformation that is already far along. (Recall that in *Brave New World* elaborate consumption-oriented, mechanical amusement parks—featuring, for example, centrifugal bumble-puppy—had supplanted wilderness and even ordinary gardens.) Second, the new inhabitant of the new "Garden" will have to be a creature for whom we have no precedent, a creature as difficult to imagine as to bring into existence. He will have to be simultaneously an innocent like Adam and a technological wizard who keeps the "Garden" running. (I am indebted to Dean Robert Goldwin, St. John's College, for this last insight.)
11. Some scientists naively believe that an engineered increase in human intelligence will steer us in the right direction. Surely we have learned by now that intelligence, whatever it is and however measured, is not synonymous with wisdom and that, if harnessed to the wrong ends, it can cleverly perpetrate great folly and evil. Given the activities in which many, if not most, of our best minds are now engaged, we should not simply rejoice in the prospect of enhancing IQ. On what would this increased intelligence operate? At best, the programming of further increases in IQ. It would design and operate techniques for prolonging life, for engineering reproduction, for delivering gratifications. With no gain in wisdom, our gain in intelligence can only enhance the rate of our dehumanization.
12. The philosopher Hans Jonas has made the identical point: "Thus the slow-working accidents of nature, which by the very patience of their small increments, large numbers, and gradual decisions, may well cease to be 'accident' in outcome, are to be replaced by the fast-working accidents of man's hasty and biased decisions, not exposed to the long test of the ages. His uncertain ideas are to set the goals of generations, with a certainty borrowed from the presumptive certainty of the means. The latter presumption is doubtful enough, but this doubtfulness becomes secondary to the prime question that arises when man indeed undertakes to 'make himself': in what image of his own devising shall he do so, even granted that he can be sure of the means? In fact, of course, he can be sure of neither, not of the end, nor of the means, once he enters the realm where he plays with the roots of life. Of one thing only can he be sure: of his power to move the foundations and to cause incalculable and irreversible consequences. Never was so much power coupled with so little guidance for its use." [*J. Cent. Conf. Amer. Rabbis* (January 1968), p. 27.] These remarks demonstrate that, contrary to popular belief, we are not even on the right road toward a rational understanding of and rational control over human nature and human life. It is indeed the height of irrationality triumphantly to pursue rationalized technique, while at the same time insisting that questions of ends, values, and purposes lie beyond rational discourse.
13. It is encouraging to note that these questions are seriously being raised in other quarters—for example, by persons concerned with the decay of cities or the pollution of nature. There is a growing dissatisfaction with ethical nihilism. In fact, its tenets are unwittingly abandoned, by even its staunchest adherents, in any discussion of "what to do." For example, in the biomedical area, everyone, including the most unreconstructed and technocratic reductionist, finds himself speaking about the use of powers for "human betterment." He has wandered unawares onto ethical ground. One cannot speak of "human betterment" without considering what is meant by *the human* and by the related notion of *the good for man*. These questions can be avoided only by asserting that practical matters reduce to tastes and power, and by confessing that the use of the phrase "human betterment" is a deception to cloak one's own will to power. In other words, these questions can be avoided only by ceasing to discuss.
14. Consider, for example, the widespread acceptance, in the legal systems of very different societies and cultures, of the principle and the practice of third-party adjudication of disputes. And consider why, although many societies have practiced slavery, no slaveholder has preferred his own enslavement to his own freedom. It would seem that some notions of justice and freedom, as well as right and truthfulness, are constitutive for any society, and that a concern for these values may be a fundamental characteristic of "human nature."
15. Scientists may, of course, continue to believe in righteousness or justice or truth, but these beliefs are not grounded in their "scientific knowledge" of man. They rest instead upon the receding wisdom of an earlier age.
16. This belief, silently shared by many contemporary biologists, has recently been given the following clear expression: "One of the acid tests of understanding an object is the ability to put it together from its component parts. Ultimately, molecular biologists will attempt to subject their understanding of all structure and function to this sort of test by trying to synthesize a cell. It is of some interest to see how close we are to this goal." [P. Handler, Ed. *Biology and the Future of Man* (Oxford Univ. Press, New York, 1970), p. 55.]
17. When an earlier version of this article was presented publicly, it was criticized by one questioner as being "antiscientific." He suggested that my remarks "were the kind that gave science a bad name." He went on to argue that, far from being the enemy of morality, the pursuit of truth was itself a highly moral activity, perhaps the highest. The relation of science and morals is a long and difficult question with an illustrious history, and it deserves a more extensive discussion than space permits. However, because some readers may share the questioner's response, I offer a brief reply. First, on the matter of reputation, we should recall that the pursuit of truth may be in tension with keeping a good name (witness Oedipus, Socrates, Galileo, Spinoza, Solzhenitsyn). For most of human history, the pursuit of truth (including "science") was not a reputable activity among the many, and was, in fact, highly suspect. Even today, it is doubtful whether more than a few appreciate knowledge as an end in itself. Science has acquired a "good name" in recent times largely because of its technological fruit; it is therefore to be expected that a disenchantment with technology will reflect badly upon science. Second, my own attack has not been directed against science, but against the use of *some* technologies and, even more, against the unexamined belief—indeed, I would say, superstition—that all biomedical technology is an unmixed blessing. I share the questioner's belief that the pursuit of truth is a highly moral activity. In fact, I am inviting him and others to join in a pursuit of the truth about whether all these new technologies are really good for us. This is a question that merits and is susceptible of serious intellectual inquiry. Finally, we must ask whether what we call "science" has a monopoly on the pursuit of truth. What is "truth"? What is knowable, and what does it mean to know? Surely, these are also questions that can be examined. Unless we do so, we shall remain ignorant about what "science" is and about what it discovers. Yet "science"—that is, modern natural science—cannot begin to answer them; they are philosophical questions, the very ones I am trying to raise at this point in the text.



In recent years widespread public concern has developed in the United States over encroachments on privacy and confidentiality in many areas of American life. This concern has led to a proliferation of regulations and legislative proposals at local, state, and federal levels. Although the medical and epidemiologic research community has been highly successful in protecting the confidentiality of personal information obtained for research purposes, attention has nonetheless focused on possible threats to privacy that may be associated with research involving human subjects. The Privacy Act of 1974 (P.L. 93-579) was designed to ensure that personal information about individuals collected by federal agencies would be limited to that which is legally authorized and necessary and that the information would be maintained in a manner that would preclude unwarranted intrusions upon individual privacy. In addition, the act provided for creation of the Privacy Protection Study Commission, which subsequently made recommendations to Congress and to the President regarding various record-keeping practices in the private sector and the use of such records for research purposes (*1*).

In this article, we present an overview of the implications of the privacy and confidentiality issue for future medical and epidemiologic research in the United States. We first describe briefly the importance of population-based biomedical research; second, show how essential the use of medical records with individually identifiable information has been in the past; third, describe some of the health problems for which knowledge based on use of medical records is desperately needed; and finally, describe the safeguards that are in effect for protecting the rights to privacy of human research subjects.

The Society for Epidemiologic Research and the Association of American Medical Colleges are committed to protecting the confidentiality of the medical and personal data obtained in the course of research activities. We believe that privacy protection can best be accomplished through the regulations of the Department of Health, Education, and Welfare that are now in force and through new legislation based largely on the recommendations made by the Privacy Protection Study Commission in its report to Congress.

Population-Based Health Research

Epidemiology is the study of the distribution and dynamics of disease in human populations. Its purpose is to identify specific agents or factors, related to people and their environment, that may cause disease or identify people who are at high risk for developing a disease. In so doing, epidemiology provides the basis for public health programs designed to prevent and control disease. Prevention can be effected by reducing or eliminating exposure to a specific factor once its importance in producing disease has been demonstrated. There are at least two important reasons why identification of people at high risk for disease is desirable: (i) measures can be adopted to prevent such people from developing disease and (ii) medical supervision and screening tests can be provided when appropriate, so that if they do develop disease, their illnesses can be identified at an early stage.

Among the public health programs aided by knowledge resulting from epidemiologic investigations are those directed at preventing and controlling conditions such as infectious and cardiovascular diseases, cancer, and stroke. Epidemiologic methods are also essential for evaluating the efficacy of new preventive and therapeutic measures and any possible harmful side effects they may have. For example, the possible harmful effects of immunization against swine flu required epidemiologic investigations which, indeed, were responsible for demonstrating the relation between immunization and development of the Guillain-Barre syndrome (*2*).

Major Health Studies That Required Use of Medical Records

The major contributions of epidemiology to the understanding of disease have been based on studies in which medical and other vital records of many people were used, both for their data and for identifying those individuals appropriate for subsequent study. The studies were often conducted many years after the information was recorded. Their importance can be demonstrated by the following examples.

1) *Cancer*. Studies which demonstrated (i) the relationship of cigarette smoking to lung cancer, coronary heart disease, bladder cancer, and other conditions (*3-6*); (ii) an increased cancer risk associated with occupational exposure to substances such as asbestos and vinyl chloride (*7-14*); (iii) the increased risk of several types of cancer after exposure to radiation (*15-24*); (iv) that the ughters of women who received the hormone diethylstilbestrol (DES) during pregnancy have an increased risk of developing vaginal cancer (*25*); and (v) that women taking estrogens for menopausal symptoms have an increased risk of developing endometrial or uterine cancer (*26-28*).

2) *Cardiovascular diseases*. Studies which demonstrated (i) that high blood lipids, high blood pressure, and smoking shorten life expectancy, particularly through coronary heart disease (*29-34*); (ii) that women taking oral contraceptives have an increased risk of developing thromboembolism or stroke (*35-39*); and (iii) that administration of anticoagulants to patients with myocardial infarctions is associated with lower post-infarction mortality rates (*40, 41*).

3) *Infectious diseases*. Studies which led to the development of vaccines for poliomyelitis, measles, and other infectious diseases (*42-45*), and studies which showed that cases of polio subsequent to polio immunization in 1955 resulted from a vaccine lot contaminated with live virus (*46*).

4) *Child health*. Studies which demon-

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strated (i) that the administration of high concentrations of oxygen to premature infants results in blindness (47–49); (ii) that rubella (German measles) or other viral infection of the mother during pregnancy can produce congenital malformations in the infant (50–55); (iii) that radiation exposure of the mother during pregnancy is associated with an increased risk of congenital malformations and childhood cancer in her offspring (15–17); (iv) that Rh disease (erythroblastosis fetalis) in newborns can be prevented (56, 57); and (v) that comprehensive care programs for inner-city children and youth are effective in reducing rates of rheumatic fever (58).

These are but a handful of the studies which have produced important direct benefits for human health by identifying the factors associated with increased risk of disease, facilitating the development of preventive methods, and evaluating new ways of providing medical care and organizing health care delivery. (Some of these studies are considered in greater detail later.) It would be tragic if the potential benefits to society of such research were lost as a result of restrictions placed on the information available to researchers.

Information That Identifies Individuals

In order to carry out epidemiologic research, it is often necessary to identify individuals with specific diseases or disabilities, or individuals who share some common environmental exposure. Medical records are essential for identifying populations with specific diseases and for obtaining detailed historical, clinical, and laboratory information about individual patients. Access to these records is only a first step in identifying and evaluating patients with the particular disease under study so that they can subsequently be contacted and, with their informed consent, interviewed and studied further. Identification of individuals during the time the research is conducted is also essential to link records from different sources for a given person—records such as those kept by physicians, hospitals, and employers as well as birth and death certificates when appropriate. Identifying information about healthy persons (nonpatients) must also be made available for making comparisons when determining the cause of a disease. Without individually identifying information, which is essential for follow-up and record linkage, it would be virtually impossible to carry out epidemiologic studies of the etiologic characteristics, risk factors, natural history, and prognosis of a disease. Nor could studies be conducted in which new approaches to prevention, early detection, treatment, and delivery of health services are developed.

Patient Consent

Much population-based research would be very difficult to carry out if prior patient consent were required in order for the investigator to have access to medical records. The studies we described earlier were frequently conducted many years after the medical information was originally recorded, since the state of knowledge at the time the information was obtained may not even have permitted the study to be conceived. Thus patients' consents could not possibly have been obtained. In addition, reviewing medical records is often the first step used to identify patients with a given disease so that they may subsequently be traced, contacted, and, with their permission, studied further. Any requirement that patient consent be obtained before any medical record is reviewed would, therefore, be extremely destructive to medical and epidemiologic research.

Redisclosure

Provisions that allow redisclosure or rerelease of information for research or health statistics purposes (while maintaining safeguards of confidentiality and privacy) are extremely important, as the following example will illustrate. Because of the increasing importance of the problem of cancer, many cities and states are establishing cancer registries. These are lists of newly identified cancer patients, and they are designed to facilitate the long-term care of cancer patients, to alert health officials quickly to clusters of new types of cancer (which may reflect a new exposure to a carcinogen), and to permit investigators to identify all patients with a particular cancer so that the cause of that cancer can be investigated through well-designed epidemiologic studies.

Cancer registries generally obtain their data from hospitals and pathology laboratories. A registry would be of very limited use, however, were it not possible for the registry to go the next step and make its data available to legitimate cancer investigators. Thus rerelease of information with appropriate safeguards is essential.

A second example of the importance of rerelease concerns a large-scale study of women who had been exposed to x-rays during pregnancy. The study addressed the question of whether children from such pregnancies had an increased risk of developing cancer. Some years later, an investigator who had not been part of the original study team became interested in the reproductive histories of the children (who were by then fully grown) and in the status of their children's health. The children of the first study were contacted and asked a variety of medical and social questions. This was possible only because the data from the original study, including identifying information about individual subjects, were rereleased to the investigator. Throughout, rigid safeguards were maintained to protect the confidentiality of all data.

Research Use of Medical Records: Further Examples

Diethylstilbestrol and vaginal cancer. A few years ago, investigators in Boston demonstrated through an epidemiologic study that when mothers had been given DES during pregnancy to prevent miscarriage, female offspring from these pregnancies had an increased risk of developing a rare type of vaginal cancer when they reached adolescence (25). (This finding has important implications, since for many years DES was added to livestock feed in the United States.) Three features are particularly noteworthy: (i) the cancer did not appear in the person taking the medication but only in her female offspring; (ii) the cancer appeared some 15 to 20 years after exposure to DES, so it was necessary to go back many years to determine exposures and to identify the drugs taken in pregnancy; and (iii) the girls and young women who had this cancer were first identified through their medical records—only then could their mothers be contacted and studied further. If such use of medical records had been prohibited, or had been permitted only with the consent of the patient, this study—perhaps the first demonstration in human beings of transplacental carcinogenesis—would have been extremely difficult or impossible to carry out.

There may be other carcinogens that mothers should avoid during pregnancy because of the hazard to their children. To identify these agents, rigorous epidemiologic investigations in which medical records are used are needed to protect the health of women and their children.

Occupational cancers. Workers in certain industries are often subjected to high concentrations of potentially toxic substances. For example, workers exposed to vinyl chloride were shown to have an increased risk for liver cancer (*12-14*). This finding, which has now been confirmed, was obtained by reviewing and correlating the medical, work, and death records of large groups of employees in specific industries. Without access to these records it would have been impossible to identify and confirm vinyl chloride as a cause of cancer. Also, had there been a requirement that patient consent be obtained before the records were made available, these studies could not have been carried out because many patients had died by the time the study was done or had moved and could not be traced.

We have only begun to scratch the surface with respect to the toxic and cancer-producing potentials of substances to which workers are exposed during their daily labor. Any restriction that would preclude the identification of harmful substances and the documentation of their effects would be a major setback.

Oral contraceptives (the "pill"). Although the pill has been demonstrated to be a highly effective and convenient form of birth control and has been adopted by many women, many epidemiologic studies have demonstrated that women who take the pill for long periods of time increase their risk for blood clots, strokes, heart attacks, high blood pressure, liver tumors, gallbladder disease, congenital malformations in their offspring, and other conditions (*35-39*). These important findings were primarily the result of large-scale studies in which hospital and medical records were used—studies which, again, would have been impossible to carry out had patient consent been required. Epidemiologic studies of the effects of drugs like the pill are critical for protecting the health and well-being of the public.

Radiation. One of the questions posed by the accident at Three Mile Island in Pennsylvania has been, How serious is the potential risk to residents of the area who may have been exposed to radiation from the reactor? We know that high levels of radiation are extremely hazardous to human beings, but what is not known with any certainty is the extent of the hazard posed by low levels of radiation. In order to generate data on the hazards presented by low levels of radiation, it is necessary to collect information on a population that was exposed to low-level radiation. If such a population were identified, we would attempt to trace its members and obtain any relevant physician records, hospital records, or death certificates. For comparison purposes, it would be necessary to identify a similar but unexposed population and obtain similar records for its members to determine the rate of disease in that population. Only in this way could we determine whether the exposed group has more disease than the group that was not exposed. For the conclusions to be valid, complete records must be available for both groups. It would be necessary to know names and addresses, to have access to their medical and vital records with personal identifiers included, and to establish procedures for tracing, recontacting, and following up the members of both populations to determine all episodes of serious illness and death. If access to these records is restricted, Americans will be denied information on the hazards of low-level radiation. It is, therefore, essential that Congress ensure that legitimate medical and epidemiologic researchers have unhindered access to medical records. Such access, naturally, must be conditional on the demonstration by the investigator to his institutional review board (IRB) that he has provided adequately for protection of the privacy of the subjects in his study, as described below.

Existing Safeguards for Protecting Confidentiality

All epidemiologists and medical researchers have a major professional and personal responsibility to minimize invasion of privacy as much as possible and to protect vigorously the confidentiality of the data in their possession. The provisions of the National Research Act (P.L. 93-348) (especially its implementing regulations on protection of human subjects) codify an elaborate system of safeguards, currently in operation within the scientific community, to prevent violations of the rights of patients for purposes of research. Each investigator must justify to the IRB the rationale for subjecting any human research subject to any risk—including invasion of privacy—and must demonstrate the measures he or she is taking to ensure the confidentiality of all personal and medical data obtained in the course of the research (*59-63*). Investigators must assure the IRB that research data will be kept under lock and key, and must specify who will have access to the data, how and at what point in the research personal information will be effectively separated from other data, and whether or not the data will be retained at the close of the study, and if so, why. The IRB thoroughly reviews interview instruments and questionnaires, the consent statement, and any accompanying material—all of which must be sufficiently informative and understandable to enable the subjects to make an informed decision about participating. If the subjects are patients, they are regularly assured that their care will not be jeopardized in any way by their failure to participate. All subjects are assured that they are free to withdraw from a study at any time. Many of these provisions are spelled out in current regulations of the Department of Health, Education, and Welfare.

Immunity from Subpoena

It should be apparent that epidemiologists and other medical investigators are keenly sensitive to the challenge of ensuring confidentiality of data and protection of the privacy of research subjects. However, one important issue remains to be resolved, namely, the protection from subpoena of data gathered for research purposes. At present, no such protection is guaranteed in most research areas. Although an investigator may make the maximum effort to safeguard the confidentiality of information he obtains, such information remains subject to subpoena by a court of law. Therefore absolute assurance of confidentiality cannot be made to research subjects. Provision must be made for data obtained for research purposes to be immune from subpoena except under precisely stated extreme circumstances.

Current Challenges

Among the major public health problems in the United States are cancer, cardiovascular disease, and infectious diseases such as hepatitis, venereal disease, and influenza. Many cancers today are probably environmentally determined. In an interview some time ago, Dr. Arthur C. Upton, director of the National Cancer Institute, responded to a question about research needs in the cancer field by saying, "We need a lot more good epidemiology. It can tell us not only about environmental factors but also about genetic influences, and we really do need to know about both." Upton's comments could also apply to research in cardiovascular diseases, high blood pressure, stroke, neurological diseases (including epilepsy), diabetes, arthritis, digestive diseases—indeed, to virtually all chronic and many infectious conditions.

The effects on human health of new drugs and other chemicals in the environment can only be identified through epidemiologic and other investigations, most of which depend on the availability of medical records. The maintenance of health and improvement of protection from environmental hazards requires the facilitation of epidemiologic research and the continued availability of medical records.

Society has a vital stake in epidemiologic and other medical research. We must ensure that the dignity and privacy of subjects will be protected without hindering the advancement of knowledge of disease. The social contract that facilitates the existence of individuals within social groups requires that each individual occasionally yield some of his rights, including privacy and freedom of action, for the benefit of society as a whole. Compliance with traffic regulations and with income tax laws are but two examples of the interactive workings of the social contract. Each society must decide when a limited compromise of individual rights is justified by the potential benefits to be derived by the community as a whole. Investigations of the etiology and natural history of disease and of the effectiveness of preventive and therapeutic interventions are of great potential benefit to society, but the conduct of such studies requires that, with proper safeguards, individually indentifiable data from medical records continue to be made accessible for medical and epidemiologic research.

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During the first half of the 20th century, up until the late 1950's, American physicians enjoyed social esteem and prestige along witn an admiration for their work that was unprecedented in any age. Medicine was the model profession, and public opinion polls from the 1930's to the 1950's consistently confirmed that physicians were among the most highly admired individuals, comparable to or better than Supreme Court justices (*1*). Highbrow and mass media commentators alike associated medical practice with the "miracles" of science and made few adverse comments on the profession (2). By the 1970's, however, statesmen of medicine were writing unhappily about being "deprofessionalized" in the wake of attacks by articulate and knowledgeable critics, attacks that by 1981 were reflected specifically in substantial mistrust of the profession among the public at large (*3, 4*). One can conduct a historical postmortem of this unexpected turn of events by examining changes in direct public depreciations of the medical profession, using the different kinds and levels of criticism of M.D.'s as indicators of what happened.

The attitudes of leaders and shapers of opinion and of the public toward physicians did not translate directly into the behavior of patients. For economic and social reasons, amounts of money spent by Amcricans on medicine continued to increase dramatically even when attitudes changed. But, as was revealed both by polls and by a resurgence of alternatives to conventional medical practice, over time the critics not only affected doctors' sensibilities but also demonstrably damaged the social credibility of the profession as a whole (*4, 5*). Since public acceptance is necessary for a profession to function, the criticism had tangible effects.

A long and honorable tradition of denigrating doctors was known to Aristophanes and Molière and continued to flourish in 19th-century America (*6*). As late as 1908 a set of satirical "Medical maxims" in this tradition included, for example (*7*):

Diagnose for the rich neurasthenia, brainstorm, gout and appendicitis; for the poor insanity, delirium tremens, rheumatism and gall-stones . . . fatten the thin, thin the fat; stimulate the depressed, depress the stimulated; cure the sick, sicken the cured; but above all, keep them alive or you won't get your money.

But in those same early years of the 20th century, the tradition of doctor baiting tended to die out as the golden age of medicine dawned. Whereas the post-1950's resurgence of criticism that culminated in Ivan Illich's *Medical Nemesis* (*8*) recalled traditional themes such as physician greed, pretension, and imposition, the later critics were also responding to new and untraditional characteristics of both medical practice and American society (*9*). Moreover, the few particular criticisms that survived in the golden age helped shape and define the new deluge.

Evolution of the Medical Image

During the 19th century, physicians seeking to professionalize their calling were fair game for hostile comment, with quacks and sectarians on one side and the practitioners' actual therapeutic impotence on the other. Some aristocrats of medicine and the medical ideal they represented did enjoy high prestige, but most (often deservedly) did not. Occasionally, antimedical diatribes based on these earlier struggles persisted after the 1890's, along with other anachronisms like attacks on the germ theory of disease. But by and large, in the wake of medical, and particularly surgical, successes, publicity about the profession was favorable, and leaders of the American medical profession succeeded by the early 20th century in their campaign to persuade the public to want and expect uniformly well-trained, well-paid physicians who themselves set standards of practice (*10–12*).

So effective was favorable publicity about both science and doctors that Americans in general began to view extensive medical care as a life necessity. Expansion of hospital care at the beginning of the century was an important indication of the change.

After some years, publications of the Committee on the Costs of Medical Care (1928–1933) and other surveys generated much criticism of the medical profession—not for members' inferior technical performance or misbehavior but for their failing to make physician services of any kind available to more people through economic and organizational means (*13*). By the late 1930's the modern campaign for "socialized medicine" or compulsory health insurance had begun, and for many decades organized medical groups opposed any change in the structuring and financing of health care delivery (*14*). All parties to the controversy, however, continued to agree that medical care was highly desirable.

While many public figures attacked the American Medical Association (AMA) and state and local medical groups for their political activities, the public image of scientific medicine improved constantly (*15*). By the 1940's virtually everyone had heard of miracle drugs and many people knew that they owed their lives to them. As writer Evelyn Barkins (*16*) observed in 1952, "Most patients are as completely under the supposedly scientific yoke of modern medicine as any primitive savage is under the superstitious serfdom of the tribal witch doctor."

Ultimately, however, the socialized medicine debates undermined public confidence in medicine as a profession. The heavily financed publicity campaigns undertaken in the name of the AMA generated political statements that few people could tak seriously and raised questions about the claims of members of the profession acting in scientific and clinical roles (*17, 18*). Even before World War II the evident social insensitivity of physician groups such as the AMA tended to tarnish the doctor as a public figure, and many people began to associate the physician with another familiar stereotype, the small businessman, who was presumably not only grasping but slightly dishonest (*19*). As one writer of the early 1940's observed of organized medicine, its "social out-

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look turns out to be . . . scarcely distinguishable from that of a plumber's union" (*20*). Indeed, the actions of physician groups caused the Supreme Court in 1943 officially to refuse to recognize doctors' professional claims and instead to find physician groups, including the AMA, guilty of restraint of "trade" (*21*)

Beginning in the 1940's, a number of reformers within the medical profession worked to expose inferior medical practice and upgrade medicine to a level appropriate for the age of penicillin and high technology. Some of the self-criticism revealed through these efforts was repeated by the general press. The combination of internal criticism and external distrust eventually had a negative effect, just as the social environment for all professions turned from favorable to unfavorable.

The End of the Golden Age

The rare public doubters of the medical profession in the late 1940's and early 1950's gradually increased in number. By 1954, Herrymon Maurer, writing in *Fortune*, could cite a series of sensational articles in mass media magazines attacking not only moneymaking but incompetence in medical practice. Maurer's article was entitled, "The M.D.'s are off their pedestal" (*22*). A few more years had to pass, however, before the number of recriminations reached the threshold that marked the end of an era.

Despite the ineptitude of the campaigns against socialized medicine, the public image of the physician per se was very favorable in the proscientific post–World War II period. This image was reflected, for example, in the activities of Dr. Kildare (a stereotype later known as Marcus Welby, M.D.) who moved from the novel and motion picture to the television screen. Physicians showed up in over half of 800 Hollywood films surveyed in 1949 and 1950. But in only 25 instances was the doctor portrayed as a bad person, and when he was bad there were often extenuating circumstances. He was almost never a humorous character, either (*23*).

Around 1950 many physician organizations across the country began systematic campaigns to reduce the number of legitimate complaints of the public against physicians. Leaders in the profession had concluded that actual experiences of everyday Americans with medical care were the source of much of the antipathy directed toward the profession. An early and exemplary effort was that of the California Medical Association, which conducted a double program. First, California M.D.'s made medical care available (but on their own terms) to answer complaints about access to it. Second, and more important, they carried out a campaign to protect the public by hearing complaints against four types of abuses: (i) malpractice; (ii) "unnecessary or incompetent procedures"; (iii) excessive fees; and (iv) unethical acts of physicians. All over the United States grievance committees of local medical societies tried to adjust physician-patient disputes and effect some of the professional self-policing so notoriously absent theretofore (*24–26*).

Grievance committees were, in fact, but one facet of a major attempt of reformer physicians to get each practitioner to emphasize and upgrade his or her personal relationships with patients. The doctors set out to fight bad public relations as one did syphilis, one case at a time but with a cumulative effect (*27*). California M.D.'s in 1951 employed the psychologist Ernest Dichter to suggest how each practitioner should manage his or her patients. Every encounter between a physician and patient is, of course, an intensely and unabashedly narcissistic experience for the patient and therefore eminently suitable for psychological manipulation. A patient's gripes about high fees, for example, may mask a real grievance related to some personal slight inflicted by the doctor. Psychological studies and systematic research on patients, analogous to consumer surveys, both gave specificity to concerns about the individual doctor-patient relationship and helped inspire and shape programs to improve such relationships (*26, 28*). As an osteopath concluded in 1955, the trust of every patient had to be gained in order to overcome the belief that medicine was emphasizing business and quantity rather than service or quality (*29*). The popular press also soon reflected the medical campaigns, elements of which were familiar from earlier AMA publicity favoring the old family doctor as opposed to the cold, impersonal specialist. By 1959 an article in *Life* was popularizing this idea, portraying physicians favorably but still strongly emphasizing how much they needed to add sympathy to their science (*30*).

At the same time that physicians were working on their public relations in the 1950's, overt popular indictments were pushing the profession off the "pedestal." Exactly where and when the final shove came is not certain. In the third quarter of the 20th century there were no fewer than 20 investigations of the New York City health system, and in 1966, after it was clear that the medical profession was in trouble, journalist Martin Gross (*31*) traced the new criticism to the first of these investigations in the cultural center of the country (*32*). In 1965 an anonymous writer in *Consumer Reports* (*33*) dated modern criticism from the publication of a study conducted in 1956 in which investigators actually rated physician performance. Perhaps the most important date was 1958, when Richard Carter's *The Doctor Business* (*25*), the first of a number of muckraking books, appeared. Carter's exposé and others that followed it drew heavily on both public investigations and exposés that members of the profession had written for internal professional purposes. Whatever the source, clearly adverse criticism had entered a novel phase by the end of the 1950's, reflecting and also creating new social circumstances within which physicians practiced.

Indignant lay writers and reformer M.D.'s shared an elevated opinion about what physicians ought to be. They were, wrote a journalist in 1954 (*34*), supposed to be part of a double picture: "on the one hand, a group of dedicated and white-coated scientists, bending over test tubes and producing marvelous cures for various ailments, and, on the other, equally dedicated practitioners of medicine and surgery, devoting themselves to easing pain and prolonging human life, without thought of personal gain and at considerable self-sacrifice." Both the public and the profession, he noted, were beginning to notice substantial deviations from this widely held ideal and to become filled with "disillusionment . . . tinged with a bitterness which breeds public hostility" (*34*). Other observers traced the rising level of adverse comment to unrealistic hopes. As the 1950's ended, columnist Dorothy Thompson summarized for readers of the *Ladies' Home Journal* this growing public criticism of American physicians. There was bad hospital care, there were bad doctors, and there were excessive medical costs. But she went on to note the cause (*35*):

> In a rather profound sense the current attacks on the medical profession compliment it. People, it seems, *expect* more of physicians than they do of other professional men with the possible exception of the clergy. The medical profession has invited that expectation, and in the opinion of this writer, and with exceptions that only prove the rule, has deserved it.

In later decades, as Americans came to expect the medical profession to furnish comfort, happiness, and well-behaved children as well as health, the disillusionment grew.

Adapting to Change

Since ancient times, critics—and the public at large—have usually discriminated sharply between their own personal physicians, who command professional trust, and the medical profession as a whole, which does not and which is susceptible to harsh judgments (*36*). In the mid-20th century, however, doubts about medicine in general or "the doctor" intensified so much that even personal professional trust was often impaired, especially when a patient could not get the attention that he or she wanted. Critics at all levels who started by blaming the system, particularly the clinic and hospital, inadvertently raised questions about the M.D.'s who collaborated in the faulty operation of the institutions.

As professionals, physicians always functioned in part on the social level. When, in the 20th century, major changes occurred in the immediate social context within which medicine operated, the profession did not adapt quickly in either the formalities of practice or the self image it produced. One of the major new forces was the startling increase of chronic (as opposed to acute) diseases as the dominant concern in practice. A second new force was the growth of huge bureaucratic institutions, particularly hospitals, in the regular health care system. A third force was the greatly increased sophistication of consumers. And a fourth was the rise of psychological explanations for illness, leaving the physician dealing with the uncertainties of psychosomatics. All of these changes were well under way before the 1950's, and each helps to explain what happened to the golden age of medicine.

Critics and reformers outside the profession were also slow to respond to the changed situation. Carter's *The Doctor Business* (*25*), for instance, was targeted chiefly on the fee-for-service organization of medicine, and at most only a quarter of the volume was devoted to actual faults in health care. Even in 1960 in perhaps the most crucial of the new critical publications, *The Crisis in American Medicine*, the authors still tended to emphasize the economics of medicine even while recognizing that "Millions of people are bitterly dissatisfied with the medical care they are getting" (*37*).

What eventually transformed the criticism was the addition of another ingredient from society as a whole: widespread anti-institutional sentiment along with a general disillusionment with many aspects of American life (*38*). Among the target institutions were the professions, particularly professions based on expertise. In the mid-1950's writers in the highbrow and mass media began to paint negative or at least ambivalent images of many American institutions that in the 1940's had been beyond reproach: the city, the automobile, the large family—and the doctor. In making their unfavorable remarks about doctors, various kinds of public commentators drew from both past and then current concerns to focus on three aspects of the physician's function: the priestly, or sacerdotal role; the technical role; and the role of the physician as a member of the health care system.

The Sacerdotal Role

In the first half of the 20th century, when medical intervention was becoming increasingly effective, such critics as there were tended to concentrate not on the technical role of physicians but on their priestly functioning as they went through medical ceremonies and acted as wise and trusted personages. In this preoccupation, commentators reflected basic popular attitudes. In novels, for example, despite the shift of physician characters from priestly and scholarly roles to scientific, their most important duties still centered on nonphysical problems and relationships (*39*). Regardless of the passing of the old-fashioned family doctor, there was a well-understood public demand for a sympathetic personal relationship such as that furnished by the idealized country practitioner. "His successors have much to learn from him," observed an editorial writer in a typical comment as early as 1908. "At all events they must learn to be men, not merely scientists" (*40*). And even as the socialized medicine debate heated up, the impersonal system rather than individual M.D. performance was the subject of adverse comment.

In all of the criticism during the golden age, the emphasis on priestly personal functions of the physician, as opposed to effectiveness or even competence, is striking. As late as the 1950's, lists of common criticisms to which physicians were sensitive included most prominently "A failure to take a personal interest in the patient and his family," "Inability to get a doctor in cases of emergency," "Waiting time in doctors' offices," and other such items reflecting the continuing demand for personal attention (*41*). The only other conspicuous categories of complaint had to do with fees and failure to communicate with the patient. Only in later decades did the demand for competence become very conspicuous (*4*, *42*).

It is against this background of emphasis on the sacerdotal function of medical personnel that the great constant of criticism, greed, has to be viewed. Greed on the part of a physician violated a sacerdotal stereotype because most Americans expected that under ideal circumstances a physician was a dedicated professional who provided a service because the service was needed, not because it was profitable (*43*). Greed showed up earlier as a concern in attacks on quackery, fee-splitting, and then, to a small extent, physician financial interest in laboratory and drug store enterprises (*44*). But it was only after physicians had in general substantially increased their incomes that critics fastened on the evident wealth rather than specific fees of M.D.'s as evidence of unseemly grasping. This recent phase had to wait for the development of what David Horrobin has called "the politics of envy" in the late 20th century (*45*, *46*).

That physician greed was a constant in criticism meant that even in the recent period, when technical as well as priestly performance in medicine was again subject to question, the motive that critics identified in errant physicians was avariciousness. Why else would a rational M.D. commit undesirable acts and reduce the quality of the medical care that he was delivering? And in the continuing socialized medicine controversy, when the physician as entrepreneur was an issue, greed was, again, imputed to medical advocates of laissez-faire (*47*).

One area in which the public could and did react to physicians in their nontechnical roles was indifference to patients, epitomized in the contrast between house calls and clinic or hospital practice. Personal attention was the theme of the solo practice advocates both inside and outside the profession. It was the chief complaint of detractors of specialization, before and after the late 1950's. It was the object of the local grievance committees set up after World War II. And it was the subject of studies after mid-century by members of a new subspecialty, medical sociologists.

In an era of high technology, when the

secrets of medicine became increasingly inaccessible and incomprehensible to the public, responsiveness to the patient remained the one aspect of practice by which most people could judge the M.D. By the 1960's, case histories of patient mistreatment on a social, not technical, level were standard in the growing literature of criticism. But the critics who wanted atttention and care from the physician still did not usually specify what the care consisted of until well into the age of malpractice suits (*48*).

The Technical Role

Although the technical performance of the physician called forth little adverse comment before the 1950's, both the application of medical science and the individual competence of the M.D. in applying it had earlier been traditional and continuing subjects of recrimination. Kept alive for a time in the campaign against obviously incompetent nonphysician quacks, the theme of pretension and ineffective treatment continued to be an issue in occasional attacks on unnecessary surgery. Remarkable, however, was the fact that one type of criticism, that directed toward the laziness, negligence, and incompetence of M.D.'s, remained largely undeveloped for over half a century. There were a few stories about outright malpractice, and there were suggestions (usually made by M.D.'s trying to upgrade the profession, that many physicians were not keeping up with scientific literature (*49*). But no rash of damaging exposés appeared until after the 1950's.

One dark side of the physician as technologist was the fear that practitioners would impose too much medicine, not only forcing inoculations and surgery on unwilling persons but, indeed, using patients for experimental purposes. In the 1920's, Sinclair Lewis's *Arrowsmith* helped keep this traditional fear alive, but the physician as scientist who imposed on patients in the name of technique remained largely a literary figure. For decades, serious critics restricted themselves to the impersonality of the specialist, not his mania for medical intervention and innovation. Lay commentators, in fact, tended to write about fads in medicine in terms of progress and to ignore the discarded fashions. Publicists who did discuss faddism did so gently, like the 1928 humorist in *Collier's* who commented (*50*),

> An' now it's the gall bladder. Doctors are *mad* over it. The appendix, tonsils, teeth, auto-intoxication, acidosis—all are forgotten; an' the gall bladder is now the undisputed belle of the body. For a medical man it has all the lure an' emotional appeal of a Swinburne poem, a Ziegfeld chorus or a moonlight party in Hollywood.

By the 1960's and 1970's critics were saying that, as one of them put it, medical faddism reflected "the underlying bias of the technological mindset and its activity orientation . . . that newer must be better and that doing more must be better than doing less; hence the possibility of harm is always a second thought . . ." (*51*). By this time, then, deliberate risk had been added to lack of knowledge and skill. Moreover, the public ultimately developed a very high level of distrust of what critics had been characterizing as excessive use of drugs and surgery (*4*).

The Social Role

Beyond the priestly and technical requirements of medical practice, one of the well-understood demands society makes of any professionals in granting them special status has been that their activities be harmless to society (this is one reason that advertising, for example, cannot qualify as a profession). The traditional issue of whether the monopoly granted physicians was or was not antisocial became a crucial one in the 20th century. The reorganizers of American medicine at the turn of the century took pains to show that the newly licensed monopoly, "the medical trust," as early critics characterized it, that outlawed quacks and sectarians and vested licensure in the profession, *was* in the public interest (*52*).

Medical leaders succeeded in winning the public's trust and approval (*11*). Not even the failure of the self-policing that was a direct (though not essential) concomitant of the monopoly elicited much comment before the 1960's. Only insofar as physicians as a group failed to take positive action to provide medical care for all who wanted it, or as medical groups opposed institutional arrangements designed to improve and extend medical care, did criticism fall on the monopoly. Then, attribution of greed to physicians was one aspect of the accusation, but so also was conservatism, which was a characteristic of other monopolies that consistently drew criticism in modern America. It was not until the 1960's and 1970's that new, well-educated groups tried to break the monopoly by developing new kinds of "health care deliverers" and by introducing lay control. Such developments grew out of distrust of the intentions and and customs of the medical profession.

Attention to the social aspects of medicine was the qualitative characteristic that most clearly differentiated detractors of medicine before and after the 1950's. More recent critics not only decried the monopoly and maldistribution of medical care but also loaded physicians with responsibility for any number of social transgressions: exploiting menials, failing to provide incentives for improving health care delivery, encouraging unnecessary bureaucracies, increasingly setting arbitrary boundaries to illness, ignoring "positive" health, and in general, to use the term of the leading critic, Illich, "medicalizing" the whole society to the detriment of individual dignity and well being (*8*).

The Erosion of Professional Status

Physicians have always been sensitive to criticism (*53*). For half a century they were relatively free from public censure or actual interference in clinical and professional activities, and they enjoyed great public and personal admiration. Few people other than doctors knew about iatrogenic disease or the placebo effect. Criticism—and lack of it—reflected both the impression conveyed in public about the miracles of medicine and the persistence of the sacerdotal role of the physician, demanded by the public at all levels. But the physician as priest was already in some trouble by the 1930's. Attacks on impersonal specialism and on well-meaning social reformers' attempts to spread the technical benefits of medicine through prepayment (that is, insurance) and institutional reorganization laid a basis for doubts about the whole profession. Demand for a priest was still intense, as surveys even in the 1950's showed, but the profession in general was by then set in place to be the object of a more general social attack. This attack portended the end of generous funding for medical research and the end of such extremes of freedom of action as professionals might aspire to (*54*).

Commentators with a sense of the tragic, or even just of the ironic, can find in the 20th-century physician ample justification for their views. As sociologist Eliot Freidson pointed out at the beginning of the 1960's, conflict between patient and physician was inevitable because the function of the physician was to apply general knowledge to a particular individual, the patient (*55*, p. 175).

Applying knowledge involved trying to control the patient, and the patient in turn was interested in controlling his or her destiny (*26*). In attempting to maximize the client-professional trust that would permit patients to yield control, physicians emphasized the validity of their science—and in so doing created a sophisticated public. That public in turn became increasingly competent to expose shortcomings of the profession and to react when physician reformers spoke out about their colleagues' failures (*56*). "I wrote about . . . abuses and asked for changes," wrote District of Columbia internist Michael J. Halberstam in the mid-1970's. "And now changes are coming, but alas . . . they will probably be the wrong ones" (*2*).

One of the major results of the new criticism of the 1960's and 1970's, in which the technical as well as the sacerdotal function of the physician came into question, was therefore a series of demands for greater patient participation in the medical relationship, demands exacerbated by a resurgence of romantic individualism in the culture as a whole (*57, 58*). By 1972 one analyst (*59*) could add to the "engineering" and "priestly" models of health care and delivery two more, the "collegial" and the "contractual." Both of these last models involved patient participation and were flourishing in various settings (*59*).

Insofar as the entire society was moving toward social leveling, the high status necessary for professional authority was being eroded throughout most of the century (*55*, p. 187). By the 1960's even the popular image of the physician as portrayed on television reflected a change from a charismatic figure, who used mysterious powers to resolve problems, to a new type of hero, one with only ordinary endowments and who potentially could behave unheroically (*60*) But as early as the 1930's the sociologists, who surveyed Muncie, Indiana, as "Middletown" had commented that physicians, and lawyers, too, were increasingly less visible as independent community leaders. Older physicians continued to be aware of a change, but few could cite convincing detail as did J. A. Lundy of Worcester, Massachusetts, who in 1952 recalled the time when townspeople customarily tipped their hats to the physician (*61*). Another perceived sign of erosion of the physician's place was the fact that patients felt increasingly free to shop around for an M.D. who suited them (*62*). The loss was felt not by the technically oriented specialist whose bedside manner might be imperfect, but by the traditional family doctor. By the 1960's and 1970's physicians were complaining not only of lack of deference but of lay interference and assaults on professional privileges. The politics of envy were building in new ways upon traditions of criticism that had been muted in the first half of the 20th century but had not died.

Conclusion

The golden days of the medical profession can be defined by the amount and the content of criticism that the profession received—what little adverse comment there was, was often to the effect that highly desirable professional services were insufficiently available or that physicians had lapsed from their sacerdotal roles. In both cases the critics tended to fasten on the old theme of the doctor whose greed overcame his more professionally disinterested concern. The practice of medicine always involved M.D.'s in ambivalent relationships with both individual patients and society, and high-status professionals who could not or would not respond to patients' personal and selfish concerns of course generated complaints and could even become both personal and social scapegoats (*63*). But it was the continuing politics of the socialized medicine debate that first planted the seeds of major and pervasive mistrust. When, after World War II, physicians themselves spoke out to increase the beneficent results of medicine and upgrade the profession in the direction of the professional ideal, they unwittingly opened the door for the latter-day critics who attacked not only priestly pretension but technical performance. The influence of these critics combined with other social forces in movements that in the 1960's and 1970's tended to impair the trust and freedom that had once marked medical practice (*64*).

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The rapid development of molecular genetics and particularly the introduction of recombinant DNA technology have elicited much interest among scientists, physicians, and the public in general. The realization that scientists might be able to manipulate the heredity not only of lower organisms but also of our own species has led to much soul searching. Some observers maintain that mankind is at the threshold of new powers that are unlike any innovations ever faced before.

Where do we stand? Scientists and physicians need to be well informed about the current status of genetic manipulation so as to be able to inform the public regarding the scientific facts Sometimes incomplete knowledge and lack of understanding of various issues in this rapidly evolving subject have led to unwarranted emotional reactions and ill-advised resolutions designed to block the progress of investigative activity.

Genetic Manipulation in the Past

Genetic manipulation is not a new development. For several thousand years, human beings have attempted to control their environment by influencing the genetic characteristics of other species. The domestication of wild plants and animals is an example of genetic manipulation with the aim of producing better and more food. Other examples include the improvement of egg and milk yields from domestic animals. The domestication of dogs shows that even behavior has been manipulated genetically. Hunting dogs, herding dogs, and watch dogs are only a few of the many kinds that were produced purposefully by breeding for specific behavioral characteristics—a form of genetic manipulation.

Genetic manipulation by design has rarely been practiced in our own species. However, unplanned genetic selection for intelligence probably occurs frequently. Marital partners resemble each other in intelligence (at least as measured by IQ tests) because of assortative mating for this trait (*1*). While the exact contribution of heredity to IQ remains unknown, most informed observers accept that genes contribute to the variability of IQ. Therefore, the elevated IQ levels observed on the average among offspring of intelligent parents are an example of genetic selection based on social customs. Such an assertion does not deny that there is a significant environmental component under these circumstances. However, even if the genetic contribution to intelligence is relatively small, assortative mating for IQ would be expected to concentrate high IQ genes among the offspring of gifted couples.

Human breeding by design for high intelligence was recently suggested by a California millionaire who arranged to use sperm from Nobel Prize winners in the sciences for artificial insemination of self-selected volunteer women. One would expect statistically that the offspring of such a procedure would be more intelligent than the average. No other predictions regarding future

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achievements could be made. Presumably, such voluntary private undertakings on a small scale would cause few social problems and would have no significant effects on the human gene pool. However, attempts by governments to control human breeding must be viewed with alarm—particularly since such efforts would interfere with civil liberties and democratic ideals. The attempt by the Nazi government in Germany to institute breeding centers for selected Aryan men and women illustrates an ill-conceived undertaking based on pseudoscientific standards of race ideology and retrogressive notions about individual rights and dignity.

Indirect Manipulation of Human Genes

Medical therapy and certain public health measures affect the human gene pool indirectly by preserving deleterious genes that would otherwise be eliminated. Thus, successful treatment of certain genetic diseases such as diabetes, hemophilia, immune deficiency, certain types of congenital heart disorders, and others, allows the bearers of defective genes to have children. Some biologists and geneticists have warned about the "dysgenic" effects of these practices, fearing serious contamination of the human gene pool with harmful genes that might necessitate a major expenditure by future societies on treatment of the genetically infirm.

While there is some formal merit in such arguments, it is important to distinguish the human from other species (*2*). Human beings have a unique brain that allows "cultural inheritance," which, with the rapid dissemination of ideas, has facilitated our adaptation to a variety of environments. From a strictly biologic viewpoint, the necessity for humans to wear clothes is a deleterious trait, in that we lost the genes for hairiness that protected us against the elements. Development of the human brain enabled our ancestors to devise the necessary protection by the fabrication of clothes from animal skins first, from agricultural products later, and from synthetic fibers more recently. Cloth making and cloth wearing is a valuable part of human culture in all but the most primitive human societies and therefore cannot be considered a harmful trait in the human context.

What about wearing eyeglasses? In developed countries the wearing of eyeglasses because of genetically conditioned myopia or other refractive error is not particularly harmful except in limited occupational settings. The relatively high frequency of myopia and the need to wear eyeglasses represents loss of an adaptive biologic trait among civilized populations. Yet, in the absence of a nuclear holocaust that would relegate humans to a hunting and gathering existence, myopia is a trait that can be well supported by modern societies. Indeed, the existence of myopia and other refractive errors has created an industry of ophthalmologists, opticians, and spectacle frame makers. Analogously, in the distant future human beings might require injections and pills for a variety of genetic infirmities—a development that we currently view as unhealthy. However, our descendants might consider this state of affairs to be as "normal" as we consider the wearing of clothing or eyeglasses today. Thus, the characterization of human genetic traits as beneficial or harmful depends entirely on the environment in which the trait or traits operate.

The "New" Genetics

DNA has come to be recognized as the genetic material in organisms as far apart as viruses and humans. The basis of life on this planet is unitary and founded on the DNA genetic code. The "dictionary" of this genetic code is well worked out. Human hemoglobins have been useful for showing the effect of DNA mutations on gene function (*3*). Study of various genes has shown that the coding sequences in the DNA (exons) are interrupted by intervening sequences (introns) with yet unknown function. Before gene transcripts consisting of RNA can be translated into proteins according to the genetic specification laid down by DNA, these introns need to be spliced out. Mutations affecting the coding sequences as well as the splicing mechanism can give rise to genetic diseases (*3, 4*).

The discovery of restriction enzymes that cut DNA at specific base sequences has been a major development (*5*). Many different restriction enzymes—each splitting DNA at different specific sites—have been discovered. DNA that has been split by a given restriction enzyme can combine with any other DNA molecule cut by the same enzyme. It is therefore possible to join DNA molecules from different sources to produce so-called "recombinant DNA" consisting of parts of DNA from different species. Species barriers can therefore be crossed.

The technology necessary to determine the sequence of the component bases of DNA molecules has developed rapidly (*6*). Other methodologic advances have made it possible to sequence the amino acids in proteins with very small quantities of material (*7*). "Gene machines" that synthesize portions of genes or even whole genes already exist (*8*). Such synthesized partial genes can be used as probes to isolate genes of biologic interest (*9*).

What are the applications of the new biology and what problems do they raise?

Production of Enzymes and Protein by DNA Technology

Human genes that specify the synthesis of biologically important substances can be inserted into the DNA of microbiologic vectors, such as the intestinal bacterium *Escherichia coli*, where the human DNA becomes integrated. The small quantities of genetic material thus introduced can be amplified by growing the "engineered" microorganisms in culture (*10*). The gene products manufactured by the manipulated human genes in the microorganism can be collected. Human insulin already has been produced in this manner and has been safely used in diabetes therapy. Other polypeptides such as human growth hormone and interferon are under production. Various laboratories and companies plan to use DNA technology to manufacture vaccines and many other therapeutic and diagnostic agents. In principle, any polypeptide gene product could be synthesized by these new methods.

What are the ethical problems?

There was anxiety initially that genetic manipulation of *E. coli* could result in the creation of pathogenic bacterial strains that might cause mass epidemics (*11, 12*). Similarly, it was feared that genetic manipulation of certain human cell lines might cause the spread of cancer (*11, 12*). Scientists shared these fears with the public and there was much alarm. It was soon shown, however, that genetic exchange between microorganisms was not new and had occurred all along in evolution. Furthermore, it was demonstrated that the *E. coli* strains created by the genetic engineers are so enfeebled that they represent no danger in outgrowing *E. coli* organisms. Much additional experimentation over the last few years has suggested no unusual dangers of the new DNA technology. The initial anxiety regarding the safety of genetic manipulation has therefore receded, but it is important to understand that the rather remote potential dangers were first described by highly reputable scientists.

When the potential dangers of recombinant DNA were first discussed, medical microbiologists who were experienced in working with highly lethal human microorganisms had not been fully consulted by the molecular biologists who were not accustomed to strict microbial containment in their work. It is unlikely that medical microbiologists would have raised the kind of fears suggested by the molecular biologists. Similarly, cancer epidemiologists had not been fully consulted in the early stages of safety discussions about recombinant DNA. Some scientists now question whether hypothetical, frightening scenarios that appear farfetched in retrospect should have been shared publicly. Most observers agree that scientists should not make important decisions that affect the public without full disclosure. Although the DNA safety issue, because it dealt with the "stuff of life," was alarming to many people, it stimulated the interest of the public who therefore became better informed.

The new genetic technology has raised problems of corporate control. The involvement by university scientists in industry may lead to less open exchange as scientists try to capitalize financially on their findings. Secrecy may be necessary to allow a company a commercial advantage in bringing a certain product to market, but in basic science departments this could throttle the open communication that led to those very developments that can now be commercially exploited. The availability of ready money for commercially valuable research also may distort research objectives, leading to possible neglect of basic research. University administrators are eager to attract funds from industry at a time of decline in governmental grant support. Such problems are not entirely new, having been faced by faculties of chemistry, pharmacy, engineering, and electronics in the past. However, the engagement of basic biologists in industrial applications is rather novel, since previously applied scientists usually had been involved with industry.

An ethical issue faced in relation to the pharmaceutical industry is the understandable interest in manufacturing products with a potentially large market. Drugs or biologicals for treating rare diseases are less likely to be developed than those agents that will have a wide sale because of their effect on common diseases such as cancer and hypertension. Profits derived from vaccines against tropical diseases prevalent in Third World countries are likely to be much smaller than those obtained from widely sold products in developed countries. Developing countries cannot afford expensive biologicals. Such financial issues distort the priorities of product development in the commercial sector and require enlightened governmental financial aid.

Genetic Techniques in Diagnosis of Hereditary Disease

The new DNA technology has shown that differences in DNA sequences affecting the noncoding areas as well as differences in the intervening sequences (introns) are common among individuals *(13)*. DNA variants of either type have no known functional consequences in the expressed phenotype of the organism, but affect the length of DNA fragments defined by a given restriction enzyme. These variants are inherited by Mendelian segregation and can be traced through families. Their laboratory determination is not excessively difficult *(13–15)*. If such a DNA variant is located close to a defective gene and if the defective gene cannot be tested for directly, the DNA variant may be used as a marker to infer the presence of the linked gene that causes disease. It has been calculated that the visualization of 150 to 300 different DNA markers of this type randomly distributed over the 23 pairs of human chromosomes would yield a sufficient number of specific landmarks on each chromosome to allow detection of any disease-producing gene *(13, 16)*. Diagnosis by DNA markers usually requires study of the parents and of other affected and unaffected family members. With this information it is possible to assign the relationship of the DNA marker gene to the disease gene by using the principles of conventional genetic linkage analysis. In a few cases, such as in sickle cell anemia, where the specific mutation in the DNA is already known, certain restriction enzymes that recognize the abnormal DNA sequence at the mutant site can be used to demonstrate the mutation directly without family study *(17)*. Gene deletions that occur in some other hemoglobinopathies may also be recognized directly by using the appropriate probes without family study *(18)*.

It may thus be possible, by means of these innovations in DNA technology as well as by other advances in biochemical genetics, to detect susceptibility to and provide early diagnoses of a variety of hereditary diseases that currently cannot be detected until they are clinically manifest. Certain hemoglobinopathies can already be diagnosed prenatally by using amniotic fluid cells aspirated by amniocentesis *(19)*. Parents have the choice of abortion of fetuses affected with the genetic disease and may thus avoid the birth of an affected child. Although this option is favored by many couples, it is not acceptable to others for religious or other reasons.

If a predictive test is available, should it be applied to detect all family members that might be affected by a hereditary disease? For example, an appropriate test might be developed for detecting individuals at risk for Huntington's disease. This neurologic disease does not usually become manifest until middle age. If a test were available, it would be possible to assure one-half of the children of an affected parent that they would never be affected. My general philosophy in such situations is to strongly urge patients to be tested if the condition can be prevented or treated. In situations where a positive test would only provide knowledge but no further options for medical or reproductive management it may not be appropriate to insist on testing. Huntington's disease is such an example. Some medical geneticists, however, feel that if a reliable test is available it should be used to identify all members of a kindred who are at risk for developing the disease. Thus, individuals destined to get sick at a later date can order their lives and make appropriate reproductive decisions, while those free of the disease can continue their lives without undue anxiety. Rationally, such an approach makes good sense, but not every person wants to know. Should we not respect the right of the people to privacy and their desire to remain uncertain about their future health? If a testing program has been recommended, does a family member have the right to stop the program and thus prevent someone else in the kindred from knowing his or her susceptibility?

The clinical investigator and affected families might face a dilemma if a test for an untreatable, late-manifesting disease appeared promising. If, additionally, preventive management of such a disease became feasible, investigations of those who might later be affected would be required. The selection of such persons for study would then require the disclosure of information about which at least some individuals might rather have remained ignorant.

"Labeling" of individuals as carriers for genetic disease occurred in the United States when genetic screening for the sickling trait was introduced (*20*). Carriers of the trait who never develop any clinical problems were considered as mildly affected by the public or even by physicians who were unaware of the harmlessness of the carrier state for sickle hemoglobin. "Labeling" may be particularly serious if a given genetic trait sometimes, but not always, has undesirable consequences. Acrimonious discussions took place when studies of newborns were initiated to follow the developmental and psychologic consequences of sex chromosome aberrations such as XYY [for references, see (*21*)]. Critics of these studies raised the specter of "self-fulfilling prophecies" in view of early suggestions that the XYY state might always be associated with criminal behavior—a concept that turned out to be false.

Occupational restriction might be instituted for genetic reasons. Certain individuals may be at higher risk to toxic damage from specific chemicals because of inherited enzyme variations. Genetic testing in industry has already been discussed [see (*22*)]. Trade unions have criticized the introduction of such testing because management might use the testing as pretext to avoid cleaning up unhealthy industrial conditions. It is cheaper to exclude workers than to provide healthy working conditions for everyone. In a related problem, an executive might be passed up for promotion if it became known that he carried the gene for familial hypercholesterolemia with its high risk of premature heart attacks. Could one blame an industrial company for such action? Do individuals who know they carry such a gene have the right to withhold such information from employers?

"Predictive medicine," that is, the early detection of individuals at risk for a specific disease, will become increasingly possible with the new developments in DNA and genetic marker technology (*22, 23*) As public bodies assume a more direct role in the health system in many countries, confidentiality may become eroded and genetic information may be used by social and health planners to assign individuals their niche in society. As long as such knowledge only concerns genes affecting variables of physical health and as long as testing remains voluntary, society might be able to cope. But when we learn more about the genetics of personality and mental traits (*21, 24*), new problems could arise. At present, there are few clearcut genetic data in human behavioral genetics and there is no way to apply this knowledge in the foreseeable future. However, the recent claim that cognitive intelligence might be predicted by evoked auditory or visual responses (*25*) (that is, by presenting auditory or visual stimuli to an individual and measuring certain brainwave responses) suggests that advances in this area may soon bring new problems.

Gene Therapy

The replacement of a defective gene with its normal counterpart, if it were possible, would be applicable only in monogenic diseases where abnormal function of a major single gene is the principal cause of the disease. These diseases, while numerous, are individually quite rare. It should be emphasized, however, that the technical problems allowing the practical use of gene therapy have not yet been overcome. Gene therapy or gene manipulation could probably not be carried out in complex traits where many genes are involved in phenotypic determination. Thus, genetic manipulation would not be possible for traits such as skin color, hair shape, personality, or intelligence. However, if one or several major genes were the principal contributors to the variation of these traits, it might theoretically be feasible to manipulate them genetically. Currently, the nature and location of most genes affecting normal variation of body structure and function are unknown.

The procedure used to replace a defective gene is likely to be as follows. The normal gene to be used for gene therapy will first be isolated. After a small portion of the diseased tissue, such as bone marrow cells, are removed from the patient, the normal gene will be introduced into the patient's cells containing the defective genes. The nuclei of the target marrow cells will be induced to take up the normal gene by means of a variety of techniques. The genetically manipulated cells will then be reintroduced into the patient. It is postulated that the manipulated cells would have an advantage over the genetically defective cells which they would perhaps ultimately replace, thereby curing the patient. Before any such therapy could be successful, however, several conditions would have to be fulfilled (*26*). The transplanted gene would have to be taken up by the abnormal target cell and integrated into its nucleus, where it would have to remain and function normally. The expression of the introduced gene would have to be regulated to produce appropriate amounts of gene product. The engineered cells as well as the total organisms would have to be unharmed by the procedure (*26, 27*).

This scenario for gene replacement represents a new approach to somatic therapy in that the procedure will not affect genes in the germ cells of the ovaries or testes of the patient, but will affect only the somatic cells that have been manipulated. Patients whose cells have been engineered in this manner still will carry the abnormal gene in their gonads and, if they are able to reproduce, will transmit the defective gene to some of their descendants according to Mendelian principles.

Gene therapy of this type is therefore conceptually no different from any therapy in medicine that attempts to improve the health of a sick patient. The only difference is that DNA, rather than other biologicals, drugs, or surgery is used as the therapeutic modality. This point is important because some critics claim that gene replacement represents a revolutionary departure in medical treatment. In fact, gene therapy for diseased tissues is no different from any other therapy. No change in the genes of the reproductive organs is attempted.

What is the current status of gene therapy?

The best understood genetic system in humans is the hemoglobin gene complex, and the most common monogenic genetic disorders affect hemoglobin structure and function (*3, 4*). Sickle cell anemia and the various thalassemias cause severe anemia (*3*). It is now technically feasible to produce normal or abnormal human hemoglobin genes in the laboratory. Since hemoglobin is produced by certain bone marrow cells (that can easily be aspirated in a routine manner), normal isolated hemoglobin genes in the form of DNA can be added to the patient's abnormal erythropoietic marrow cells. After the normal hemoglobin DNA has been taken up, the manipulated marrow cells can be returned to the patient where they are expected to proliferate and produce normal hemoglobin. A cure, or at least a partial cure, by DNA therapy might therefore ensue.

How does this mode of therapy conceptually compare with other new and old treatments of the hemoglobinopathies?

Treatment of anemia by transfusion of

red cells is a well-recognized form of therapy. Transfusion historically was the first successful type of transplantation in medicine, and few ethical arguments have been raised against blood transfusions. A new type of experimental therapy is bone marrow transplantation (28). When the hemoglobin-producing cells of the bone marrow are genetically defective, marrow of appropriate tissue type (so as to minimize cell rejection) from a normal sib can be transplanted into the patient with hemoglobinopathy. It is hoped that the transplanted marrow cells will proliferate normally and synthesize the hemoglobin that the patient is not making properly. At least one case of thalassemia has already been successfully treated in this way (29).

No special ethical arguments are raised against bone marrow transplantation except those that apply to all of human therapeutic experimentation. A further logical step in the treatment of hemoglobinopathies, that is, the use of isolated normal hemoglobin genes rather than of entire donor cells to improve function of a patient's abnormal marrow cells, is conceptually no different from bone marrow transplantation. Gene therapy is therefore a natural therapeutic development that evolves from increasing understanding of disease mechanisms. Public unease about gene therapy can therefore be lessened by explaining that the nature of such therapy is not a radical departure from previous medical intervention. Gene therapy can be considered as a form of "euphenics" rather than the practice of "eugenics." The phenotype may be altered but not the genotype. Medicine has been proceeding in this manner since its beginnings.

Although the use of DNA in such projected therapy causes no new ethical problems, many problems are raised as with other types of innovative therapy. First, extensive animal experimentation is required to work out the details and to ensure safety of the proposed treatment. The severity of a disease is an important criterion in deciding when to introduce a new therapy. With mildly affected patients one would hesitate to initiate a completely new therapy that might have unanticipated side effects. However, with life-threatening diseases, one might be less hesitant to use new treatments, particularly if the patient is in the end stages of the disease and no alternative treatments are available. For example, for a patient with a terminal malignancy a new treatment based on rational principles that has not been worked out in all details in animals might be acceptable.

The timing of the introduction of a new therapeutic modality depends on many factors. Observers with different medical or scientific backgrounds might have different views. The recent controversy about the use of DNA therapy for two patients with thalassemia major in Italy and Israel is illustrative (30). Many scientists felt that these attempts were premature in the absence of full animal experimentation. The U.S. investigator who attempted the therapy was a medically qualified scientist who maintained that the patients had a life-threatening disease and that no meaningful alternative treatment was available. This incident was further beclouded by long delays in decision-making by a human experimentation committee in the United States that was considering the appropriateness of the planned treatment. Before obtaining a ruling from this committee, the investigator decided to carry out the experimental treatment in other countries. Permission by ethics committees abroad was obtained more rapidly, but, since recombinant DNA (rather than a nonrecombinant DNA technique as specified in the application) was used, further problems arose (31). The investigator lost grant support from the National Institutes of Health for transgressing the relevant regulations. The patients apparently were neither helped nor harmed by the procedure, but a full account has not yet been published. The case attracted much public attention because it represented the first attempted use of gene therapy in humans.

Medical pioneers in the past, such as Jenner and Pasteur, performed their respective studies on smallpox and rabies prevention on a single human subject without the safeguards we demand today. They were successful and established immunization schemes that wiped out dangerous and lethal diseases and saved many lives. In retrospect we honor their achievements, but there was no assurance at the time that the first vaccinated subjects would not suffer serious side effects or even contract the fatal diseases meant to be prevented. Current attitudes regarding human experimentation are more in keeping with our respect for human autonomy and dignity. It is conceivable, however, that the ethics committees that are now required to approve proposed experimental treatments in humans might be unduly cautious or conservative and might defer or prevent the introduction of innovative treatments with great potential impact. Since human subjects in the past have sometimes been abused by medical experimenters, our current system of safeguarding human subjects is clearly desirable. But let us hope that this system will not inhibit imaginative new approaches in the prevention or treatment of human disease.

Genetic Manipulation of Fertilized Eggs

Some recent technical developments may allow genetic manipulation of germ cells (32–38). In several experiments, isolated genes were introduced into mouse eggs shortly after fertilization when the male's genetic contribution was still present as a distinctive pronucleus. When rabbit or human DNA coding for hemoglobin or some other protein from a different species was injected into the mouse pronucleii, the foreign DNA could in some instances be detected in the mouse offspring that developed from the fusion of the manipulated pronucleus and the egg's pronucleus. Some mice actually synthesized the protein coded by the DNA of the donor species. In these cases, the transferred DNA functioned actively in cells that had differentiated after genetic manipulation of the fertilized egg. Furthermore, in some cases, the foreign genes had become incorporated into germ cells, since the specific protein synthesized under the signal of the transferred gene could be detected in offspring of the next generation and was again transmitted to the third generation. Means of overcoming the low efficiency of integrating foreign DNA are still needed, and ways must be found to target genes to the appropriate chromosomes. Reliable, time-specific expression is likely to depend on correct integration. While much remains to be done, these experiments show that genetic manipulation of germ cells is a distinct possibility.

Techniques for the manipulation of germ cells are currently used by investigators studying gene regulation who attempt to understand how genes are turned "on" and "off." Practical applications for such techniques may come in agriculture, since commercially useful traits, such as faster growth and higher milk yields, might be introduced into animal stocks by genetic manipulation of fertilized eggs. However, most of the valuable traits in livestock are polygenic, so that conventional breeding techniques would have the same end result. If genes such as those for growth hormone or prolactin have a major effect on normal growth or milk yield, respectively, it might be possible to obtain the desired results by injection of the appropriate genes into fertilized eggs rather than by the usual breeding techniques. It is difficult to visualize human applications of such techniques, since the genetic manipulation of human eggs would require prior knowledge of the genotypes of both the egg and the pronucleus of the sperm. Such genetic typing of germ cells is not possible with current technology. Nevertheless, the animal studies raise the possibility of future genetic manipulation in humans. Unlike the somatic therapy with DNA discussed earlier, use of such technology would constitute a definitive qualitative departure from other therapies since it would affect future generations. Extensive safeguards and public discussion would therefore be needed before these techniques were ever applied in humans.

Other Forms of Reproductive Engineering

Sex selection by physicochemical or immunologic separation of sperm carrying X or Y chromosomes has been discussed for many years but has not yet been achieved. When such techniques do become successful, they will be used initially for sex choice in animal breeding. Application to humans might also be relatively simple, and the possibility that the sex ratio of the population might become distorted has been discussed (*39*).

Human fertilization in vitro to bypass blocked Fallopian tubes has been achieved several times but is technically difficult (*40*). When the procedure was first introduced, there was much discussion about possible misuse of the technique since any sperm and any egg might be used for fertilization. It was feared that some women might hire themselves out as host mothers to bear embryos from other couples, that is, they would provide "wombs for rent" (*41*). I consider it unlikely that widespread abuse will develop and the method does permit infertile couples to have their own children.

"Cloning" has been widely discussed in the past (*42*) and was achieved in frogs some time ago. In this procedure the nucleus of a somatic cell is transplanted into an enucleated egg thereby allowing the exact reproduction of the genes of the individual from whom the transplanted nucleus was obtained. Recent developments suggest that a similar approach, with nuclei being transplanted from embryonic mouse cells, might be used to clone mice (*43*). Even if cloning of humans with nuclei from adult cells ever became possible it is unlikely that the procedure would be widely used. The occasional utilization of cloning of humans would be both startling and of some scientific interest since pairs of "identical twins" of different ages could be produced. While this procedure has occasionally raised emotionally charged reactions I do not believe that cloning in humans will cause grave societal problems in the future.

General Comments

There is agreement in most societies that medical practices that depart from current therapeutic modes or that introduce completely novel reproductive procedures require public discussion. Scientists should be accountable to the public before they utilize such innovations. To be able to make wise and informed decisions in these matters, people must have some knowledge of human biology, including genetics. This means that science education at all levels from elementary schools through college needs to be strengthened. Teachers must be trained to offer exciting and attractive courses in human genetics and biology. Nontechnical science courses in colleges need to be emphasized. The media can play an important part in this endeavor by explaining and reporting responsibly on new developments. Uninformed decision-making can lead to prohibition of laudable but not particularly dangerous innovations.

The new biologic revolution based on DNA has been with us for only one generation and genetic manipulation by gene splicing was developed less than 10 years ago. Neither scientists nor the public in general have absorbed the full impact of these developments. As more is learned about DNA and human genetics more problems are certain to arise. Nevertheless, well-informed human beings in enlightened democratic societies should foster the use of the new DNA technology in a responsible manner that will lead to better health and welfare for all.

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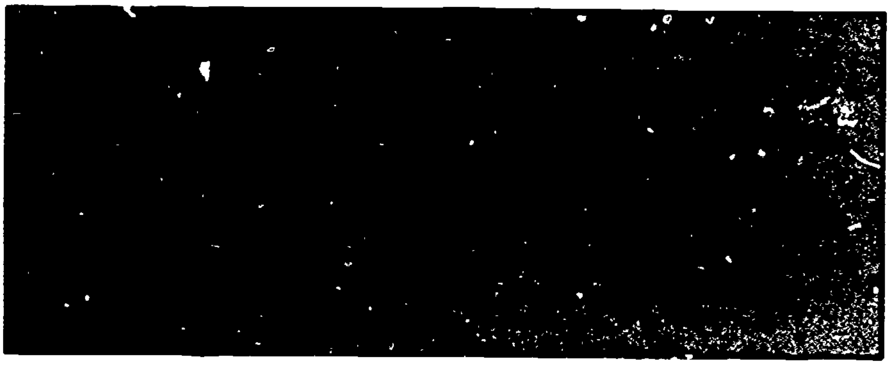

Is genetic screening a marvel about to free us from the scourge of genetic disease, or a menace about to invade our privacy and determine who may reproduce?

Genetic screening may be defined as a systematic search in a population for persons of certain genotypes. The usual purpose is to detect persons who themselves are at risk or whose offspring are at risk for genetic diseases or genetically determined susceptibilities to environmental agents (*1*). When an individual is diagnosed as having a genetic condition, the testing of relatives may be recommended. This "retrospective screening" differs from the screening of individuals without known affected relatives (prospective screening). Genetic screening may be undertaken also for research purposes unrelated to disease or the improvement of health. Retrospective screening and screening for research purposes will not be further considered here.

Genetic screening differs from nongenetic health screening in at least three important ways. First, whereas in both types of screening, identification of persons at risk may lead to the identification of others at risk, in the case of ordinary health screening the connection is often by physical proximity (contact) whereas in genetic screening it is by genetic proximity (kinship). Second, whereas in other forms of health screening the concern is about the subject being screened, in genetic screening the concern is often about the subject's offspring. Third, genetic screening carries an inherent risk of impairing self-image and perceived suitability as a marriage partner or parent.

Types of Genetic Screening

There are three principal types of genetic screening. Newborn screening seeks disease in the newborn. Fetal (prenatal) screening seeks disease in the fetus. Carrier screening seeks heterozygotes for genes for serious recessive disease. The three types have, respectively, a long established, a recently established, and a yet to be established place in health care.

Newborn screening

Newborn screening has focused largely on the detection of inborn errors of metabolism. An inborn error of metabolism is an inherited biochemical defect, classically a deficiency of an intracellular enzyme. Such deficiencies cause disease due either to the accumulation of the enzyme's reactant or its metabolites or to a deficiency of the enzyme's product.

Phenylketonuria (PKU) was the first condition for which newborn screening was widely adopted (*2*). Mass screening was feasible, despite the disease's low incidence by public health standards (1 in 11,500) (*3*), because of the discovery, by Guthrie in 1961 (*4*), of a bacterial growth inhibition assay for measuring blood phenylalanine. Before a newborn is discharged from the hospital, a sample of its blood is spotted onto filter paper and mailed to a regional laboratory (*5*). Despite the fact that most states made newborn screening for PKU mandatory before methods for diagnosis and treatment of the disease were firmly established, newborn screening for PKU remains a major triumph of genetic screening (*6*). A low phenylalanine diet begun in the first few weeks of life prevents marked mental retardation in affected children.

Phenylketonuria, initially thought to be a single disease, illustrates the phenomenon of genetic heterogeneity. High concentrations of phenylalanine in the blood of a newborn may have multiple genetic and developmental causes. In addition to classical PKU due to phenylalanine hydroxylase deficiency, there is a transient hyperphenylalaninemia due to hepatic immaturity. This abnormality disappears without treatment. More serious are some variant forms of PKU due to either a deficiency of dihydropteridine reductase or a defect in dihydrobiopterin synthesis (*2, 7*). These disorders require special procedures for diagnosis. Mental retardation is not prevented by phenylalanine restriction alone: a deficiency of monoamine neurotransmitters is also present. The efficacy of drug treatment for this deficiency is under study. At least eight causes for hyperphenylalaninemia in the newborn have been discovered (*7*), largely through newborn screening. In fact, any given clinical syndrome may have multiple genetic causes, each with its individual requirements for recognition and management. Such genetic heterogeneity, while providing valuable scientific insights into metabolic vagaries, makes genuine comprehensiveness of genetic screening programs an elusive goal.

With regard to classical PKU, questions remain. Must the diet be continued into adult life? A woman with undetected PKU and with unrestricted phenylalanine intake has a risk of producing children with severe mental and physical defects caused by high phenylalanine levels in the maternal blood (*8*). Although such women can have normal children if phenylalanine restriction is reinstituted prior to conception, their PKU may remain unknown to them or their physician. The Quebec Network of Genetic Medicine has instituted a register for all persons in Quebec Province known to have PKU (*9*). It contacts them on their 12th birthday to provide counseling about their reproductive options, that is, planned pregnancy with phenylalanine restriction, termination of an unplanned or untreated pregnancy, reliable birth control, sterilization, or adoption. This program illustrates how government can fulfill a need for long-term tracking that is difficult for private medicine because of the multiplicity of providers of care for one individual.

Other inborn errors frequently screened for at birth are galactosemia, branched-chain ketonuria (maple syrup urine disease), and homocystinuria. Like PKU these inborn errors may cause severe mental retardation or death which may be preventable by promptly instituted dietary treatment. However, the benefit of screening is less clear-cut because of other features, such as rapidity of onset of symptoms, complexity of treatment, or rarity of the condition (*10*).

A major recent addition to newborn

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screening is testing for hypothyroidism (11). In most cases this is due to a multifactorial deficiency of thyroid tissue, rather than to an inborn metabolic error. Mental and physical retardation can be prevented by treatment, consisting of thyroid hormone replacement. No special diet is required. It is significantly more frequent (one in 4000) (12) than other conditions usually screened for at birth.

Of the various types of genetic screening, newborn screening is the most widely practiced. The great majority of infants born in the United States are tested for the above conditions resulting in a marked decrease in the number of symptomatic children. The cost of PKU screening is more than offset by the savings in health care required (usually institutionalization) without screening (13). Testing for other inborn errors on the same blood sample entails little additional cost. Newborn screening represents one form of genetic screening in which government has effectively participated. State health departments, which are responsible for the supervision of newborn screening, have effectively pooled resources on a regional basis for greater efficiency (for example, New England states Northwestern states–Alaska).

Nevertheless, important issues remain (14). First, what additional diseases should be screened for? Several diseases proposed and some arguments made against them are adenosine deaminase deficiency (rare), tyrosinemia/tyrosinosis (rare except in certain populations), histidinemia (symptoms variable, treatment unproven), chromosomal disorders (15) (no intervention proposed), familial hypercholesterolemia (16) (treatment of unproven benefit), cystic fibrosis (treatment unsatisfactory), sickle cell disease (treatment unsatisfactory) (17), and Duchenne muscular dystrophy (treatment unsatisfactory) (18).

In the case of diseases for which treatment is ineffective, the argument has been made that neonatal diagnosis gives parents the opportunity to avoid the birth of a second affected child. However, the resulting decrease in incidence is small: assuming two-child families and abstinence from childbearing by all counseled couples, the reduction in incidence is only 1/8 (19). Second, should a second sample be obtained after hospital discharge because some cases may be missed by early discharge? Third, should states appropriate funds, not only for diagnosis but to ensure adequate treatment? Fourth, how can nongovernment laboratories which do the testing in some states be more effectively monitored? Fifth, should newborn screening be legally mandated or should informed consent be sought prior to testing, as in Maryland (20)?

Fetal Screening and Prenatal Diagnosis

Prenatal diagnosis of birth defects represents one of the most important practical advances in medical genetics in recent years. In most cases the fetal cells analyzed are obtained by amniocentesis, the removal of amniotic fluid containing sloughed fetal skin cells at 14 to 20 weeks of pregnancy. The commonest indication for fetal screening is a maternal age of 35 or greater because of the increased risk for an offspring with a chromosomal anomaly. The most common of these is Down's syndrome; the abnormality is paternal in origin in 30 percent of cases (21). Prospective parents exposed to mutagenic agents such as chemicals and x-rays often seek prenatal diagnosis but are difficult to aid because most birth defects are not detectable by chromosomal analysis.

Prenatal diagnosis may also be indicated if a previous child has had a chromosome abnormality, if either parent is a carrier of a chromosomal anomaly (most commonly a balanced translocation), if a previous child or a close relative has had a neural tube defect, if the mother is a known or a presumed carrier of a serious X chromosome–linked recessive disorder (for example, hemophilia or Duchenne muscular dystrophy), or if both parents are known carriers of a gene for a significant autosomal recessive disorder detectable in utero (for example, Tay-Sachs disease).

Cytogenetic, biochemical, and developmental disorders involve different methods of analysis of the amniotic fluid cells obtained. In the case of neural tube defects, the commonly used biochemical marker is an increased concentration of α-fetoprotein, found where the fetus' spinal canal is in direct contact with the amniotic fluid ("open" cases)

The gene product can be directly measured in a large number of conditions (22), usually by assay of enzymatic activity (for example, in mucopolysaccharidoses). Less satisfactory is the prenatal diagnosis of X chromosome–linked conditions in which the biochemical defect is not known. In the case of Duchenne muscular dystrophy, a devastating disorder uniformly fatal in young adult life, parents at risk must decide whether to abort any male fetus even though there is only a 50 percent chance that a given male fetus has inherited the X chromosome bearing the Duchenne gene.

Recently, analysis of DNA from cells in the amniotic fluid has permitted prenatal diagnosis of hemoglobinopathies. Sickle cell anemia in the fetus can be diagnosed by restriction enzymes Dde I (23) and Mst II (24) because the nucleotide substitution in the sickle gene eliminates a restriction site for each enzyme. Various forms of thalassemia may be diagnosed by detection of the change in DNA that is causing the disease, for example, a deletion (25). Because of the multiplicity of mutations that cause thalassemia, however, analysis of genetic linkage between globin gene loci and polymorphic restriction sites is often necessary (26). Synthetic oligonucleotide probes specific for normal or mutant nucleotide sequences have also been used (27). Prenatal diagnosis based on linkage to polymorphic restriction sites is expected to become possible for any single-gene disorder (28).

Amniocentesis cannot be performed before the second trimester of pregnancy, and by the time results are available fetal movement may have been felt. Diagnosis during the first trimester would be preferable, both because of greater patient acceptance and because pregnancy termination would then be safer. An alternative method of diagnosis is to obtain, transvaginally at 6 to 10 weeks of pregnancy, samples of chorionic villi. These villi are of fetal origin and may be used for the prenatal diagnosis of hemoglobinopathies (29) and chromosome abnormalities.

Prenatal diagnostic methods have enabled many couples with a known genetic risk to have healthy children. As a result, the incidence of certain genetic diseases, for example, Tay-Sachs disease (30), Down's syndrome (31), and, in some regions, thalassemia major (Cooley's anemia) (32) has been markedly reduced. Fetal screening in cases of advanced maternal age has been widely adopted. An important factor has been extensive media coverage leading to a demand for services. Lawsuits, brought against obstetricians by parents of children with birth defects detectable but not detected by prenatal diagnosis because it was not offered, have educated obstetricians beyond the plaintiff.

Many issues remain to be resolved. Some of these are technical, for example, the need for safer methods to sample fetal blood, required at present for the diagnosis of hemophilia and currently carrying a 4 to 5 percent fetal mortality. One approach under investigation is the detection and sorting of fetal blood cells from the maternal circulation by means of flow cytometric methods. Other issues require more information to be resolved, for example, what should a physician tell parents about the phenotype of their child when fetal chromosome analysis reveals a previously undescribed karyotype? A major controversy is whether every pregnant woman should be screened for elevated concentrations of serum α-fetoprotein which can signal an increased risk of a neural tube defect in her fetus. Among the issues are the ability of providers to follow a complex sequence of diagnostic steps in following up elevated α-fetoprotein values, government regulation of reagent use, and whether benefits will outweigh costs [in view of the incidence of the defect in the United States (one in 590 births)] (*33*).

The overriding issue in the promulgation of prenatal diagnosis for birth defects is, of course, the controversy over abortion. Many parents who find abortion unacceptable in other circumstances do choose to terminate a pregnancy in which the fetus is proved to have a serious birth defect. In fact, prenatal diagnosis has had a "pro-life" effect for couples who previously avoided pregnancy because of a genetic risk but now willingly conceive (*34*). Further, some couples choose prenatal diagnosis with no thought of termination but rather to prepare for the birth of a child with special needs.

The lower socioeconomic groups are still underserved by this new technology. At least part of this underutilization is inadequate access to these services and insufficient understanding of their benefits (*35*).

Carrier Screening

Carrier screening is the identification of heterozygotes for an autosomal recessive or X-linked recessive disease. To many people, "genetic screening" brings to mind chiefly carrier screening because it is this form of screening that has frequently been carried out through public appeals, whereas newborn and prenatal screening have been carried out in the course of regular health care.

Many considerations should be weighed in establishing carrier screening programs (*36*). First, the disease in question should be serious. Second, the test to be performed on the population at risk should be simple, relatively inexpensive, and sensitive enough not to miss positive individuals. If the test itself is not specific, then a backup test of adequate specificity should be available. Third, the individual identified as positive should have some options. For example, married couples identified as being at risk for a recessive disease for which there is no prenatal diagnosis might choose to take the risk, undergo artificial insemination, or adopt a child and forgo pregnancy. But providing such information might not be a service since all of the options might be unattractive. Fourth, the costs avoided should exceed the costs incurred. A major determinant of the cost is the frequency of the disorder in the population screened.

Tay-Sachs disease was the first disorder for which large-scale carrier screening was done in the United States. Tay-Sachs disease meets most of the criteria above. It is serious, being characterized by developmental delay, blindness, seizures, and paralysis; it is usually fatal by age 3; and it is without specific treatment. There is a satisfactory test for the carrier state. Prenatal diagnosis is available for the enzyme (hexosaminidase A) in amniotic fluid cells. The disease occurs predominantly in the Ashkenazi Jewish population. Kaback directed a program in the Washington/Baltimore area in the early 1970's with excellent results (*37*), and since then similar programs have been initiated in most large U.S. cities and many cities abroad. The relative incidence of Tay-Sachs disease has been significantly reduced by such programs (as well as by exogamy) and couples at risk have been able to have only healthy children.

In contrast to this generally successful experience with Tay-Sachs screening was the experience with sickle cell screening in the early 1970's. Some of the screening programs were politically motivated and lacked sufficient expertise, confidentiality, and provision for the counseling of subjects identified as positive. Positive individuals often suffered a decreased self-image (*38*). Positive children were often overprotected by parents. Individuals were sometimes discriminated against for purposes of marriage, employment, or insurance. A revealing study of a sickle screening program was conducted in Orchomenos, a Greek village where marriages were frequently arranged by parents, a conceivably ideal arrangement to take into account genetic knowledge. Nevertheless carriers were stigmatized as undesirable marriage partners, not only for other carriers, but for everyone (*39*). Another adverse result of the U.S. screening program was the exposure of nonpaternity, that is, the fathering of a child by someone other than the presumed father. Many states passed laws requiring sickle testing at birth, at school entry, or prior to marriage, laws leading to charges by blacks of attempted genocide.

In theory, it should be easy to avert some of these unfortunate results by providing accurate testing, adequate counseling, and strict confidentiality. These goals are difficult to achieve in public programs. A major reason is the regrettable fact that the average citizen lacks the background in biology and genetics to comprehend the significance of the carrier state.

Few diseases are common enough in the general population to merit carrier screening. Many genetic diseases have an especially high incidence in a particular ethnic, racial, or religious group (*40*). Screening only members of such a group involves difficulty in determining who is a member of that group and risks charges of discrimination. A group to be screened should have a partnership role in planning any screening effort. The most common serious autosomal recessive disease in Caucasians is cystic fibrosis, but at present there is no satisfactory test for the heterozygote.

The best age for carrier screening is arguable. The newborn identified as having sickle trait is not likely to benefit directly since reproduction is remote. However, if the parents are screened and found to be at risk for a child with sickle cell anemia, the information may be a significant benefit to the family unit, particularly now that prenatal diagnosis of sickle cell anemia by DNA analysis is safe and accurate.

A major issue in genetic screening is whether it should be legally mandated or voluntary. Arguments made for mandatory screening are higher compliance rates, lower unit cost, timely execution, and facilitation of record keeping of incidence and outcome. However, voluntary screening is more in keeping with the American tradition. It recognizes the fact that not all citizens will benefit equally, for example, those who do not condone termination of pregnancy may not view prenatal diagnosis as a benefit. Voluntary screening may also reduce the likelihood of adverse psychological effects if the screening is preceded by appropriate education about the benefits and risks of testing and if consent for testing is truly informed. Whereas in most states newborn screening is legally mandated, carrier screening is generally voluntary. A National Academy of Sciences Committee has condemned mandatory carrier screening (*41*).

A quite separate issue is whether genetic carrier screening should be a public or private matter. Th: Tay-Sachs and sickle cell screening p ograms described above were conducted publicly and involved temples, churches, fraternal organizations, and in some cases announcement by the media. Advantages of such sponsorship include the assistance of the voluntary organization in enlisting screenees in a group educational program prior to screening and voluntary personnel who may donate time and provide support for those found to be positive. However, an equally good case can be made for incorporation of certain types of screening into primary health care (*42*). For all its success the Tay-Sachs programs have screened only 10 percent of the adult target population in 10 years (*43*). Public screening efforts may involve subtle forms of coercion, for example, among me nbers of an extended family, and may risk stigmatizing carriers (*44*). A regular health setting is more likely to provide confidentiality and needed follow-up and to avoid duplication of testing. Voluntary groups may operate only intermittently and for some populations there may be no suitable organization. The success of the Tay-Sachs effort has been due in no small part to the high educational attainment of this population group and, to the extent that this characteristic does not apply to other population groups, other screening efforts may be less successful. Rosenstock (*45*) has observed that "systematic efforts to develop rational screening programs on a regional level are likely to pay greater health dividends than a series of unrelated opportunistic programs."

To claim that genetic screening is ideally provided by the primary health care sector is not to claim that this sector is now ideally equipped to shoulder the task. First of all, the poor integration among health care providers in the United States compared to most Western countries causes duplication of effort and lack of follow-up (*46*). Second, most medical practitioners, excepting family practitioners, are oriented to the care of the individual rather than to the care of the family as a unit. Much primary medical care today is rendered by the specialist. The pediatrician may make a genetic diagnosis but leave reproductive counseling to the obstetrician who may fail to take a family history. Preventive medicine as a whole has taken a rather lower hold on the practice of medicine than might be desired. For the most part, adult medical care still waits for the individual to appear with a "chief complaint."

Genetic knowledge among medical practitioners is deficient (*47*). A survey of pediatricians, obstetricians, and family practitioners in 1974 found that nearly three-quarters reported that no course in genetics had been available during their medical training and that, as a whole, these physicians were not ready to accept genetic screening (*48*). Although the percentage of medical schools with a formal course in genetics increased from 8.6 percent in 1955 to 75 percent in 1978, teaching is still primarily in the first 2 years of medical school and lacks adequate integration into clinical training (*49*).

Just as laymen have played a role in educating obstetricians about the benefits of prenatal diagnosis, so laymen must ultimately educate physicians as a whole about their desires for genetic screening. An important contribution toward this end has been made by Scriver and his colleagues who developed a genetics curriculum for Montreal high school students. In the context of learning about genetic differences among normal individuals, high school students had a 75 percent acceptance rate of carrier (Tay-Sachs) testing compared to a 10 percent acceptance rate among adults (*50*). Conducting genetic screening in public schools requires parental permission in most U.S. communities and can be criticized as risking coercion through peer pressure. However, public schools are ideal for educating the public about genetics. If citizens learn simple genetic principles as part of their high school education, they can better understand the significance of genetic tests offered later and help health care policy-makers decide what information will be useful. Childs and Hickman (*51*) have outlined how genetics could serve as a focal point for the teaching of human biology throughout elementary and secondary school years.

Informed consent is generally stated to be a requirement for genetic screening. This is abrogated by most states in the case of newborn screening where it is generally felt that the stakes are too high and time too short to make it voluntary (*20*). In favor of informed consent for most types of genetic screening is the recognition that genetic information is psychologically different from other health information in that it refers to an immutable part of oneself which may complicate marital and reproductive plans. Nevertheless, it is common for parents of a child born with a preventable birth defect to ask, "Why didn't you doctors tells us this could happen? We would never willingly have had a child like this!" The problem with requiring informed consent in the primary health care setting is the fact that it can be argued that as much time must be spent in obtaining informed consent as in educating the individual found to be positive about the significance of the result. Consequently, a case can be made for including appropriate genetic screening as a part of multiphasic health screening. The person to be screened could be informed as to purpose by means of fact sheets provided in advance and giving the person the opportunity to decline. Such a procedure is feasible only if adequate provision is made for counseling subjects found to be positive. Videotapes supplemented by written material to take home may reduce the total professional time required. A medical genetic paraprofessional, or "genetic associate," can be of enormous value in answering the many questions that genetic screening programs elicit (*52*).

The National Genetic Diseases Act of 1976 has provided federal money to 34 states to support genetic testing and counseling services, much of which has provided salary support for genetic associates (*53*). Since 1981, however, federal administrative changes and funding cuts have threatened the continuation of these programs.

In many cases reluctance to undergo genetic screening is the result of considering only the short-term risk of anxiety related to the testing procedure rather than the long-term risk of anxiety associated with the birth of a child with a serious genetic defect. In so far as this reluctance represents a lack of awareness, more attention might be devoted to persuading individuals to find out what they need to know about their genetic constitution. Such educational programs deserve as much effort and imagination as are now invested in persuading people to choose a given antacid.

Occupational Screening

Genetic screening in industry has two different rationales. The first is the identification of individuals at greater risk than the average worker for suffering adverse effects from industrial exposures. The second is the use of genetic tests (for example, chromosome analysis) to detect actual or potential genetic damage to the genetically normal worker.

Omenn (*54*) has listed criteria for traits for which occupational testing might be justifiable: (i) a sufficiently high prevalence of the trait in the worker population; (ii) a significant increase in the risk of morbidity in workers with the trait compared to those without it; (iii) the availability of a test to detect the trait which is reliable and inexpensive; and (iv) a clear understanding between management and labor about what action might be taken on the basis of test results and who would have access to this information. Omenn (*54*) has also listed traits known to be genetically polymorphic and which, if deficient, might predispose an individual to occupational morbidity upon exposure to chemical agents (*N*-acetyltransferase, plasma pseudocholinesterase, glucose-6-phosphate dehydrogenase, and methemoglobin reductase deficiency) and to inhaled pollutants (α_1-antitrypsin, arylhydrocarbon hydroxylase inducibility, metabolic conversion of nicotine, and plasma paraoxonase).

It was recently reported that 59 major corporations were considering adopting some kind of genetic testing of employees (*55*). Labor leaders and toxicologists have expressed concern that industry is putting a bigger emphasis on "weeding out the susceptibles" than on cleaning up the workplace.

Today, many people believe that the statement made by Cooper (*56*) a decade ago still applies:

> What is the current state of tests of hypersusceptibility? There is insufficient epidemiologic evidence to support the use of any of them as a criterion for employability without many qualifications. On the other hand, there is ample scientific evidence to support wider testing. Premature assumptions as to the necessity for such tests or overoptimistic claims for the benefits can actually impede testing. On the basis of what we now know, no employers should be regarded as liable or derelict for not choosing to screen his employees. If he screens all employees, he would have to consider whether he would be regarded as liable to criticism for using a positive test to deny employment, or conversely, for jeopardizing the health of an individual permitted to work with a positive test. If it is clearly understood that the appropriate application of tests of hypersusceptibility is still on trial, then progress can be made in studying them.

The workplace is only one example of an environment that may reveal genetically determined differences in susceptibilities among individuals. The morbidity to be reduced by genetic screening is thus not due to genetic factors exclusively, but rather due to an interaction between specific genetic and specific environmental factors. Childs (*57*) has deplored the tendency to categorize a disease as due exclusively to heredity *or* to the environment. He has emphasized that each patient presentation calls for assessing the separate contributions of genetic and environmental factors.

Screening Donors for Artificial Insemination

It is possible for a woman whose mate has a dominant gene for a serious genetic disease, or who shares with her mate a gene for a serious autosomal recessive disease, or who has no male partner, to bear a healthy child by means of artificial insemination. It is the responsibility of the physician performing artificial insemination to maximize the probability that the resulting child will be born healthy. Hence the genetic screening of the sperm donor should be particularly comprehensive. Such screening should include a complete medical history with information on any exposure to radiation or mutagenic drugs, a reproductive as well as family history, Rh typing if the recipient is Rh-negative, and testing for any heterozygous state commonly found in his or the recipient's ethnic group (*58*).

Population Aspects of Genetic Screening

Hohenemser *et al.* (*59*) have suggested a method for constructing, for any technological innovation, a profile of hazardousness. This profile reflects both hazards (threats to humans and what they value) and risks (quantitative measures of hazard consequences that can be expressed as conditional probabilities of experiencing harm). Since genetic screening can affect the genetic structure of offspring, the effects of such screening may be long lasting. However, unless choices were to be mandated on a large scale, no significant alteration in the genetic structure of populations would be likely. Hence, ensuring the availability of free choice should ensure continued genetic diversity of the population.

Limitations of Screening Capabilities

It has been estimated that, of every 200 newborns, approximately two will have a significant single gene disorder, one will have a chromosome disorder, eight will have a significant congenital malformation, two to four will have idiopathic mental retardation, and nine will have a multifactorial (partly genetic) disorder of later onset (for example, diabetes, coronary heart disease, psychoses) (*60*). Genetic screening can identify the risk of many monogenic disorders by study of the prospective parents and can identify chromosomal disorders and additional monogenic disorders from study of the fetus. But such screening prenatally cannot readily identify multifactorial disorders, including most instances of idiopathic mental retardation and congenital malformations.

In addition to the genetic factors transmitted from parents to child, the occurrence of new mutations must also be considered. New dominant mutations may cause genetic disease regardless of the screening of prospective parents. Methods to monitor human mutation rates are urgently needed because of increased environmental exposure to mutagenic agents.

Psychological Aspects of Genetic Screening

Discussions of genetic screening often refer to reproductive decision-making as though reproduction were necessarily preceded by deliberation. In fact, even for couples at genetic risk, many conceptions are not planned, others represent attempts to compensate for a deceased or defective child, still others constitute efforts, often unconscious, to demonstrate ability to bear a normal child. The average couple has difficulty with the concept of probability and may, while ignoring the actual risk of its occurrence (*61*), convert a statement of risk (for example, a 1 percent chance of having a child with a chromosome abnormality) into a binary statement (either it will or it will not happen). They then may visualize the worst outcome and judge whether or not they could cope with it (*62*). Additional information on the process of genetic counseling is provided elsewhere (*63*). Achieving the full benefits of genetic screening will require better methods for communicating risk information based upon a better understanding of how individuals deal with probabilities of adverse reproductive outcomes.

Ethical Aspects of Genetic Screening

Genetic screening programs, existing and proposed, have sparked many ethical debates over the past decade (*64*), The President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research has recently issued a report on the ethical, social, and legal implications of genetic screening, counseling, and education programs (*53*). The Commission enunciates five principles and makes some recommendations. Excerpts are:

1) *Confidentiality*. "Genetic information should not be given to unrelated third parties. . . ." However, adoption laws should be changed so that information about serious genetic risks can be conveyed to adoptees or their biological families without betraying anonymity.

2) *Autonomy*. "Mandatory genetic screening programs are only justified when voluntary testing proves inadequate to prevent serious harm to the defenseless, such as children, that could be avoided were screening performed. . . . The value of the information provided by genetic screening and counseling would be diminished if available reproductive choices were to be restricted. (This is a factual conclusion that is not intended to involve the Commission in a national debate over abortion.)"

3) *Knowledge*. "Decisions regarding the release of incidental findings (such as nonpaternity) or sensitive findings (such as diagnosis of an XY female) should begin with the presumption in favor of disclosure. . . ." An informed public requires, not just extensive genetic counseling services, but more intensive exposure to genetic principles in public schools.

4) *Well-being*. "Screening programs should not be undertaken until the [screening] test has first demonstrated its value in well-conducted, large-scale pilot studies. . . . A full range of prescreening and followup services for the population to be screened should be available before a program is introduced."

5) *Equity*. "Access to screening may take account of the incidence of genetic disease in various racial or ethnic groups within the population without violating the principles of equity, justice, and fairness."

The above precepts are concerned primarily with protecting the individual from undesirable effects of genetic screening. Such effects will be minimized if screening programs adopt the specific goal, not of reducing the incidence of a disease, but of maximizing options available to couples at risk for an affected child.

The larger ethical issues in genetic screening concern whether the benefits of a proposed screening program will outweigh the burdens, and, if this is judged to be likely, what priority to assign the program in competition for limited resources with other desirable programs of health care. As in other fields of medicine, a case-by-case analysis, as advocated by Toulmin (*65*), may be more helpful than abstract principles.

Legal Aspects of Genetic Screening

If a family had an undesirable reproductive outcome, such as the birth of a defective child, and there was reason to identify the family as at increased risk for such an outcome, and yet the physician did not inform them of their options (for example, prenatal diagnosis), then the family may decide to bring suit against the physician (*66*). Other examples of legal liability in the provision of genetic services include intervention which proves harmful, for example, artificial insemination resulting in the birth of a child with Tay-Sachs disease (*67*), or breach of confidentiality occurring when a physician notifies relatives of their genetic risk without the permission of the patient (*68*).

Conclusions

Genetic screening thus represents neither a panacea nor an anathema. Among its past accomplishments are reduction in the incidence of symptomatic inborn errors through newborn screening and, for many couples at risk for a child with a serious birth defect, provision of the option to avoid having a child with a defect that could have been detected. Still uncertain is the advantage to be gained by optimal provision of carrier testing. Individual differences in receptivity to genetic information and in reproductive preferences complicate policy-making.

If the promise of genetic screening is to be fulfilled, certain needs for the future are evident. First, research must be conducted on the best delivery mechanism for technologies already at hand. For example, now that it is possible for a couple at risk to avoid the birth of a first child with sickle cell anemia or Cooley's anemia by analyzing fetal DNA, what kind of screening program should be used to identify such couples? Comparisons are needed between various methods of delivery of genetic services (*69*). The roles of the health care provider, the voluntary organization, and local, state, and federal governments should be clarified and integrated (*70*). Legislative and executive branches need the advice of commissions which include medical geneticists to prevent hasty legislation and to provide for timely updating of health policy without requiring legislative revision, such as provided in Maryland by the Commission on Hereditary Disorders (*71*).

Kaback has distinguished three approaches to the control of genetic disease: cure, effective therapy, and prevention (*72*). Cure—that is, the correction of the intrinsic defect in germ line DNA—is not in the immediate future, despite the success of certain recent gene transfer experiments with the use of somatic cells in vitro. Prevention is a high priority primarily because truly effective therapy is not available for most genetic diseases.

The scientific community bears a responsibility, not only to expand knowledge but also to educate the public. Scientists must assist their fellow citizens in understanding the true promise of science, including what science cannot provide. Science provides options but individuals must choose among them.

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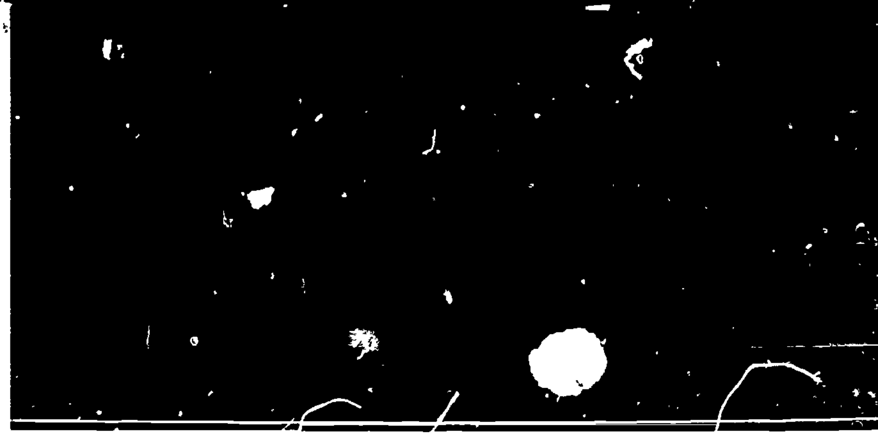


The scientific community generally agrees that objective measures of the hazards presented by toxic chemicals, food additives, or occupational exposures are needed to clarify the trade-offs that are a part of public and private regulatory decisions. The search for "risk indicators" to help both the policymakers and the public understand potential hazards has assumed great popularity among scientists. But conflicts arise over the appropriate role of scientists who assist in public-policy efforts designed to minimize the costs and hazards associated with technological change. Those who argue that scientists should help the government cope with the uncertainties presented by new technologies are challenged by colleagues who believe that these issues are inherently more political than scientific in nature. One outgrowth of the search for a means to balance scientific objectivity with politic ' action has been the increased emphasis on studies of risk assessment.

Chauncey Starr introduces one of the first quantitative efforts to respond to the question, How safe is safe enough? In his risk-benefit analysis, Starr measures the acceptability of voluntary and involuntary risks using data on accidental deaths arising from technological developments already in public use. He then extrapolates from these figures to estimate the level of risk of new technologies seeking to provide similar services.

Judge David L. Bazelon focuses on the invisible judgments often imbedded in risk assessments in his article on risk and responsibility. Judgments involved in government regulatory decisions affecting public health and safety, he notes, can no longer be delegated to so-called disinterested scientists. He recommends that society rely instead on procedural standards to help sort out scientific facts, inferences, and values in risk regulation

David Okrent develops a foundation for a science of risk assessment, describing the need to measure societal risks accurately, to examine and reevaluate priorities in risk decisions, and to determine the level of expenditure for risk reduction beyond which adverse economic and political considerations may be overriding. Chauncey Starr and Chris Whipple show that the cost-benefit type of risk analysis that was used for early evaluations of technological risks is limited by subjective interpretations of risk perceptions They recommend the use of quantitative risk criteria, such as safety targets, to improve the effectiveness of risk management.

Simon Ramo continues the discussion about risk management by pointing out that government regulations have often resulted in inequitable mitigation of technological hazards. He proposes a new regulatory approach that would separate the task of investigating a technological activity from the task of making decisions about the appropriateness of that activity.

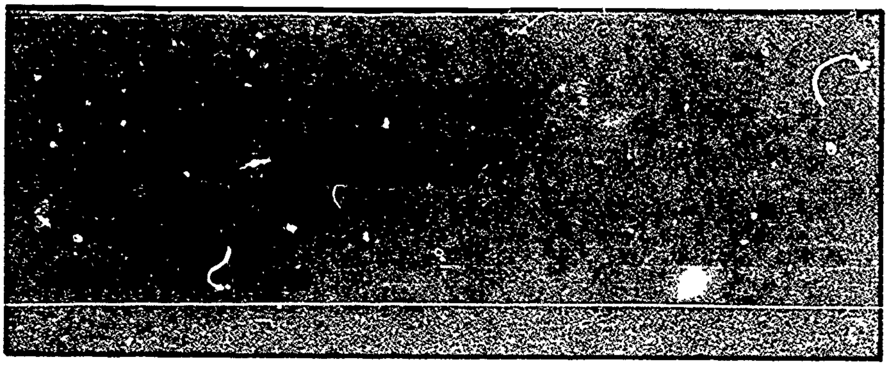
Paolo F. Ricci and Lawrence S. Molton take another approach. They review the judicial record on the use of risk analysis to establish environmental standards. The courts must interpret congressional intent in reviewing an agency's rule-making decisions, they note, and they identify issues emerging in recent judicial reviews of regulatory actions.

Sheila Jasanoff and Dorothy Nelkin take issue with the remedies proposed by advocates of a "science" of risk assessment. In their examination of litigation based on disputes over science and technology, they argue that proposed reforms frequently misconceive the problems faced by the courts because they concentrate on technical uncertainty rather than on the conceptual and policy issues at stake. Jasanoff and Nelkin prefer to rely more on traditional forums for conflict resolution than do some of the other contributors to this chapter.

C. Hohenemser, R.W. Kates, and P. Slovic construct a unique profile for technological hazards that incorporates quantitative physical, biological, and social descriptors. The profile, termed "hazardousness," was tested in pilot experiments on perception and appeared to capture a large fraction of the public's concern with hazards.

William D. Ruckelshaus appeals to the scientific community to make a greater effort to explain to the public the uncertainties involved in estimates of risk. He urges the development of a common statutory framework to replace the patchwork of laws and regulations that deal with environmental risks. He also endorses separating scientific risk assessment from the use of these assessments in regulatory decisions and management actions.

How can science most effectively contribute to public debates over the nature and significance of environmental, consumer, or occupational risks? The authors in this chapter present no uniform remedy or solution. Instead, the reader is left with the impression that efforts to extend science into politics have resulted in some politicization of science itself. Coupling scientific research with social and political reforms has raised new questions about the motivations, methods, and sponsorship of individual research efforts. The result has been increased public skepticism about science and calls for new accountability measures within the scientific community. —*RC*



The evaluation of technical approaches to solving societal problems customarily involves consideration of the relationship between potential technical performance and the required investment of societal resources. Although such performance-versus-cost relationships are clearly useful for choosing between alternative solutions, they do not by themselves determine how much technology a society can justifiably purchase. This latter determination requires, additionally, knowledge of the relationship between social benefit and justified social cost. The two relationships may then be used jointly to determine the optimum investment of societal resources in a technological approach to a social need.

Technological analyses for disclosing the relationship between expected performance and monetary costs are a traditional part of all engineering planning and design. The inclusion in such studies of *all* societal costs (indirect as well as direct) is less customary, and obviously makes the analysis more difficult and less definitive. Analyses of social value as a function of technical performance are not only uncommon but are rarely quantitative. Yet we know that implicit in every nonarbitrary national decision on the use of technology is a trade-off of societal benefits and societal costs.

In this article I offer an approach for establishing a quantitative measure of benefit relative to cost for an important element in our spectrum of social values—specifically, for accidental deaths arising from technological developments in public use. The analysis is based on two assumptions. The first is that historical national accident records are adequate for revealing consistent patterns of fatalities in the public use of technology. (That this may not always be so is evidenced by the paucity of data relating to the effects of environmental pollution.) The second assumption is that such historically revealed social preferences and costs are sufficiently enduring to permit their use for predictive purposes.

In the absence of economic or sociological theory which might give better results, this empirical approach provides some interesting insights into accepted social values relative to personal risk. Because this methodology is based on historical data, it does not serve to distinguish what is "best" for society from what is "traditionally acceptable."

The author is dean of the School of Engineering and Applied Science, University of California, Los Angeles. This article is adapted from a paper presented at the Symposium on Human Ecology held at Airlie House, Warrenton, Virginia, in November 1968.

Maximum Benefit at Minimum Cost

The broad societal benefits of advances in technology exceed the associated costs sufficiently to make technological growth inexorable. Shef's socioeconomic study (*1*) has indicated that technological growth has been generally exponential in this century, doubling every 20 years in nations having advanced technology. Such technological growth has apparently stimulated a parallel growth in socioeconomic benefits and a slower associated growth in social costs.

The conventional socioeconomic benefits—health, education, income—are presumably indicative of an improvement in the "quality of life." The cost of this socioeconomic progress shows up in all the negative indicators of our society—urban and environmental problems, technological unemployment, poor physical and mental health, and so on. If we understood quantitatively the causal relationships between specific technological developments and societal values, both positive and negative, we might deliberately guide and regulate technological developments so as to achieve maximum social benefit at minimum social cost. Unfortunately, we have not as yet developed such a predictive system analysis. As a result, our society historically has arrived at acceptable balances of technological benefit and social cost empirically—by trial, error, and subsequent corrective steps.

In advanced societies today, this historical empirical approach creates an increasingly critical situation, for two basic reasons. The first is the well-known difficulty in changing a technical subsystem of our society once it has been woven into the economic, political, and cultural structures. For example, many of our environmental-pollution problems have known engineering solutions, but the problems of economic readjustment, political jurisdiction, and social behavior loom very large. It will take many decades to put into effect the technical solutions we know today. To give a specific illustration, the pollution of our water resources could be completely avoided by means of engineering systems now available, but public interest in making the economic and political adjustments needed for applying these techniques is very limited. It has been facetiously suggested that, as a means of motivating the public, every community and industry should be required to place its water intake downstream from its outfall.

In order to minimize these difficulties, it would be desirable to try out new developments in the smallest social groups that would permit adequate assessment. This is a common practice in market-testing a new product or in field-testing a new drug. In both these cases, however, the experiment is completely under the control of a single company or agency, and the test information can be fed back to the controlling group in a time that is short relative to the anticipated commercial lifetime of the product. This makes it possible to achieve essentially optimum use of the product in an acceptably short time. Unfortunately, this is rarely the case with new technologies. Engineering developments involving new technology are likely to appear in many places simultaneously and to become deeply integrated into the systems of our society before their impact is evident or measurable.

This brings us to the second reason for the increasing severity of the problem of obtaining maximum benefits at minimum costs. It has been often stated that the time required from the conception of a technical idea to its first application in society has been drastically shortened by modern engineering organization and management. In fact, the history of technology does not support this conclusion. The bulk of the evidence indicates that the time from conception to first application (or demonstration) has been roughly unchanged by modern management, and depends chiefly on the complexity of the development.

However, what *has* been reduced substantially in the past century is the time from first use to widespread integration into our social system. The techniques for *societal diffusion* of a new technology and its subsequent exploitation are now highly developed. Our ability to organize resources of money, men, and materials to focus on new technological programs has reduced the diffusion-exploitation time by roughly an order of magnitude in the past century.

Thus, we now face a general situation in which widespread use of a new technological development may occur before its social impact can be properly assessed, and before any empirical adjustment of the benefit-versus-cost relation is obviously indicated.

It has been clear for some time that predictive technological assessments are a pressing societal need. However, even if such assessments become available, obtaining maximum social benefit at minimum cost also requires the establishment of a relative value system for the basic parameters in our objective of improved "quality of life." The empirical approach implicitly involved an intuitive societal balancing of such values. A predictive analytical approach will require an explicit scale of relative social values.

For example, if technological assessment of a new development predicts an increased per capita annual income of x percent but also predicts an associated accident probability of y fatalities annually per million population, then how are these to be compared in their effect on the "quality of life"? Because the penalties or risks to the public arising from a new development can be reduced by applying constraints, there will usually be a functional relationship (or trade-off) between utility and risk, the x and y of our example.

There are many historical illustrations of such trade-off relationships that were empirically determined. For example, automobile and airplane safety have been continuously weighed by society against economic costs and operating performance. In these and other cases, the real trade-off process is actually one of dynamic adjustment, with the behavior of many portions of our social systems out of phase, due to the many separate "time constants" involved. Readily available historical data on accidents and health, for a variety of public activities, provide an enticing stepping-stone to quantitative evaluation of this particular type of social cost. The social benefits arising from some of these activities can be roughly determined. On the assumption that in such historical situations a socially acceptable and essentially optimum trade-off of values has been achieved, we could say that any generalizations developed might then be used for predictive purposes. This approach could give a rough answer to the seemingly simple question "How safe is safe enough?"

The pertinence of this question to all of us, and particularly to governmental regulatory agencies, is obvious. Hopefully, a functional answer might provide a basis for establishing performance "design objectives" for the safety of the public.

Voluntary and Involuntary Activities

Societal activities fall into two general categories—those in which the individual participates on a "voluntary" basis and those in which the participation is "involuntary," imposed by the society in which the individual lives. The process of empirical optimization of benefits and costs is fundamentally similar in the two cases—namely, a reversible exploration of available options—but the time required for empirical adjustments (the time constants of the system) and the criteria for optimization are quite different in the two situations.

In the case of "voluntary" activities, the individual uses his own value system to evaluate his experiences. Although his eventual trade-off may not be consciously or analytically determined, or based upon objective knowledge, it nevertheless is likely to represent, for that individual, a crude optimization appropriate to his value system. For example, an urban dweller may move to the suburbs because of a lower crime rate and better schools, at the cost of more time spent traveling on highways and a higher probability of accidents. If, subsequently, the traffic density increases, he may decide that the penalties are too great and move back to the city. Such an individual optimization process can be comparatively rapid (because the feedback of experience to the individual is rapid), so the statistical pattern for a large social group may be an important "real-time" indicator of societal trade-offs and values.

"Involuntary" activities differ in that the criteria and options are determined not by the individuals affected but by a controlling body. Such control may be in the hands of a government agency, a political entity, a leadership group, an assembly of authorities or "opinion-makers," or a combination of such bodies. Because of the complexity of large societies, only the control group is likely to be fully aware of all the criteria and options involved in their decision process. Further, the time required for feedback of the experience that results from the controlling decisions is likely to be very long. The feedback of cumulative individual experiences into societal communication channels (usually political or economic) is a slow process, as is the process of altering the planning of the control group. We have many examples of such "involuntary" activities, war being perhaps the most extreme case of the operational separation of the decision-making group from those most affected. Thus, the real-time pattern of societal trade-offs on "involuntary" activities must be considered in terms of the particular dynamics of

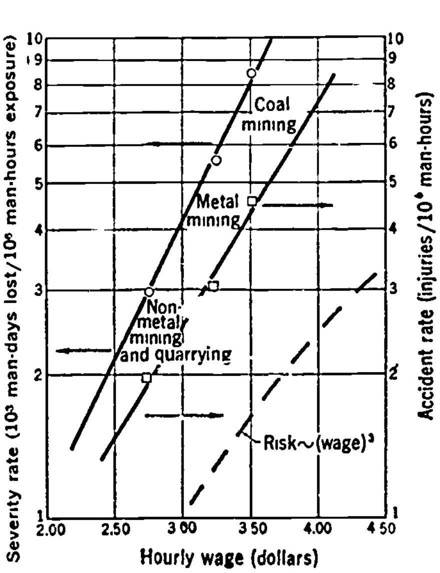


Fig. 1. Mining accident rates plotted relative to incentive.

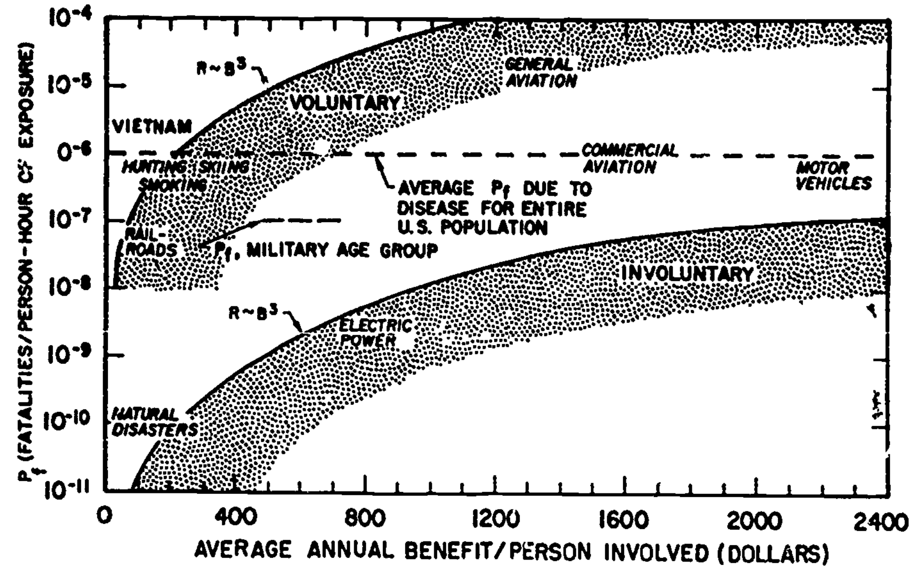


Fig. 2. Risk (R) plotted relative to benefit (B) for various kinds of voluntary and involuntary exposure.

approach to an acceptable balance of social values and costs. The historical trends in such activities may therefore be more significant indicators of social acceptability than the existent trade-offs are.

In examining the historical benefit-risk relationships for "involuntary" activities, it is important to recognize the perturbing role of public psychological acceptance of risk arising from the influence of authorities or dogma. Because in this situation the decision-making is separated from the affected individual, society has generally clothed many of its controlling groups in an almost impenetrable mantle of authority and of imputed wisdom. The public generally assumes that the decision-making process is based on a rational analysis of social benefit and social risk. While it often is, we have all seen after-the-fact examples of irrationality. It is important to omit such "witch-doctor" situations in selecting examples of optimized "involuntary" activities, because in fact these situations typify only the initial stages of exploration of options.

Quantitative Correlations

With this description of the problem, and the associated caveats, we are in a position to discuss the quantitative correlations. For the sake of simplicity in this initial study, I have taken as a measure of the physical risk to the individual the fatalities (deaths) associated with each activity. Although it might be useful to include all injuries (which are 100 to 1000 times as numerous as deaths), the difficulty in obtaining data and the unequal significance of varying disabilities would introduce inconvenient complexity for this study. So the risk measure used here is the statistical probability of fatalities per hour of exposure of the individual to the activity considered.

The hour-of-exposure unit was chosen because it was deemed more closely related to the individual's intuitive process in choosing an activity than a year of exposure would be, and gave substantially similar results. Another possible alternative, the risk per activity, involved a comparison of too many dissimilar units of measure; thus, in comparing the risk for various modes of transportation, one could use risk per hour, per mile, or per trip. As this study was directed toward exploring a methodology for determining social acceptance of risk, rather than the safest mode of transportation for a particular trip, the simplest common unit—that of risk per exposure hour—was chosen.

The social benefit derived from each activity was converted into a dollar equivalent, as a measure of integrated value to the individual. This is perhaps the most uncertain aspect of the correlations because it reduced the "quality-of-life" benefits of an activity to an overly simplistic measure. Nevertheless the correlations seemed useful, and no better measure was available. In the case of the "voluntary" activities, the amount of money spent on the activity by the average involved individual was assumed proportional to its benefit to him. In the case of the "involuntary" activities, the contribution of the activity to the individual's annual income (or the equivalent) was assumed proportional to its benefit. This assumption of roughly constant relationship between benefits and monies, for each class of activities, is clearly an approximation. However, because we are dealing in orders of magnitude, the distortions likely to be introduced by this approximation are relatively small.

In the case of transportation modes, the benefits were equated with the sum of the monetary cost to the passenger and the value of the time saved by that particular mode relative to a slower, competitive mode. Thus, airplanes were compared with automobiles, and automobiles were compared with public transportation or walking. Benefits of public transportation were equated with their cost. In all cases, the benefits were assessed on an annual dollar basis because this seemed to be most relevant to the individual's intuitive process. For example, most luxury sports require an investment and upkeep only partially dependent upon usage. The associated risks, of course, exist only during the hours of exposure.

Probably the use of electricity provides the best example of the analysis of an "involuntary" activity. In this case the fatalities include those arising from electrocution, electrically caused fires, the operation of power plants, and the mining of the required fossil fuel. The benefits were estimated from a United Nations study of the relationship between energy consumption and national income; the energy fraction associated wtih electric power was used. The contributions of the home use of electric power to our "quality of life"—more subtle than the contributions of electricity in industry—are omitted. The availability of refrigeration has certainly improved our national health and the quality of dining. The electric light has certainly provided great flexibility in patterns of living, and television is a positive element. Perhaps, however, the gross-income measure used in the study is sufficient for present purposes.

Information on acceptance of "voluntary" risk by individuals as a function of income benefits is not easily available, although we know that such a relationship must exist. Of particular interest, therefore, is the special case of miners exposed to high occupational

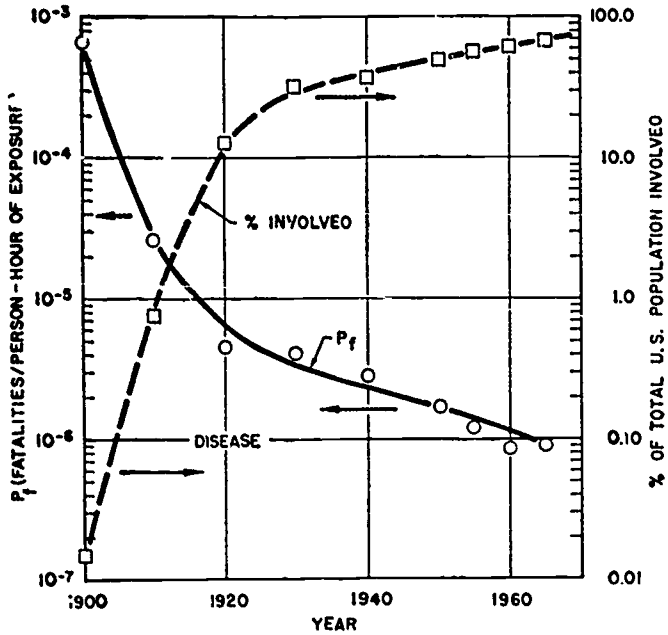


Fig. 3 (above). Risk and participation trends for motor vehicles.

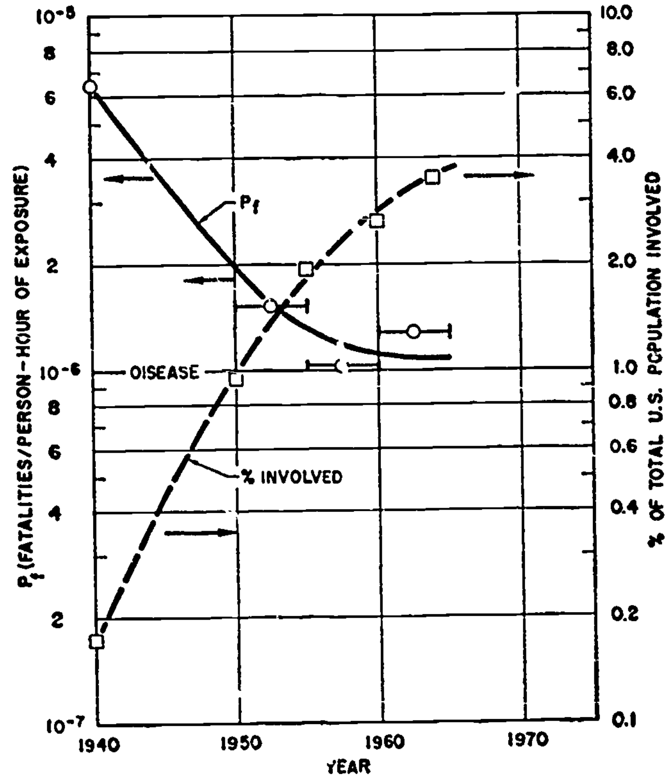


Fig. 4 (right). Risk and participation trends for certified air carriers.

risks. In Fig. 1, the accident rate and the severity rate of mining injuries are plotted against the hourly wage (*2, 3*). The acceptance of individual risk is an exponential function of the wage, and can be roughly approximated by a third-power relationship in this range. If this relationship has validity, it may mean that several "quality of life" parameters (perhaps health, living essentials, and recreation) are each partly influenced by any increase in available personal resources, and that thus the increased acceptance of risk is exponentially motivated. The extent to which this relationship is "voluntary" for the miners is not obvious, but the subject is interesting nevertheless.

Risk Comparisons

The results for the societal activities studied, both "voluntary" and "involuntary," are assembled in Fig. 2. (For details of the risk-benefit analysis, see the appendix.) Also shown in Fig. 2 is the third-power relationship between risk and benefit characteristic of Fig. 1. For comparison, the average risk of death from accident and from disease is shown. Because the average number of fatalities from accidents is only about one-tenth the number from disease, their inclusion is not significant.

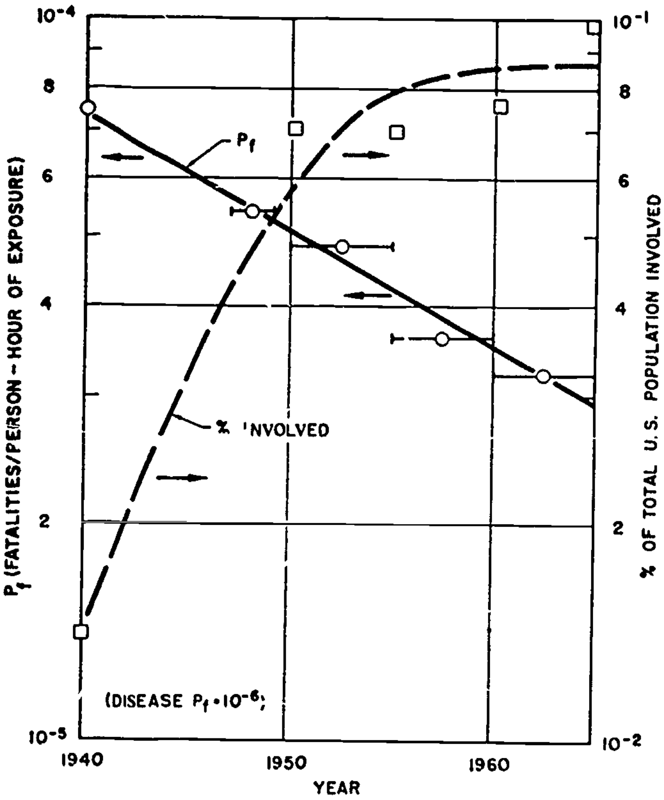
Several major features of the benefit-risk relations are apparent, the most obvious being the difference by several orders of magnitude in society's willingness to accept "voluntary" and "involuntary" risk. As one would expect, we are loathe to let others do unto us what we happily do to ourselves.

The rate of death from disease appears to play, psychologically, a yardstick role in determining the acceptability of risk on a voluntary basis. The risk of death in most sporting activities is surprisingly close to the risk of death from disease—almost as though, in sports, the individual's subconscious computer adjusted his courage and made him take risks associated with a fatality level equaling but not exceeding the statistical mortality due to involuntary exposure to disease. Perhaps this defines the demarcation between boldness and foolhardiness.

In Fig. 2 the statistic for the Vietnam war is shown because it raises an interesting point. It is only slightly above the average for risk of death from disease. Assuming that some long-range societal benefit was anticipated from this war, we find that the related risk, as seen by society as a whole, is not substantially different from the average nonmilitary risk from disease. However, for individuals in the military-service age group (age 20 to 30), the risk of death in Vietnam is about ten times the normal mortality rate (death from accidents or disease). Hence the population as a whole and those directly exposed see this matter from different perspectives. The disease risk pertinent to the average age of the involved group probably would provide the basis for a more meaningful comparison than the risk pertinent to the national average age does. Use of the figure for the single group would complicate these simple comparisons, but that figure might be more significant as a yardstick.

The risks associated with general aviation, commercial aviation, and travel by motor vehicle deserve special comment. The latter originated as a "voluntary" sport, but in the past half-century the motor vehicle has become an essential utility. General aviation is still a highly voluntary activity. Commercial aviation is partly voluntary and partly essential and, additionally, is subject to government administration as a transportation utility.

Travel by motor vehicle has now reached a benefit-risk balance, as shown in Fig. 3. It is interesting to note that the present risk level is only slightly below the basic level of risk from disease. In view of the high percentage of the population involved, this probably represents a true societal judgment on the acceptability of risk in relation to benefit. It also appears from Fig. 3 that future reductions in the risk level will be slow in coming, even if the historical trend of improvement can be maintained (*4*).



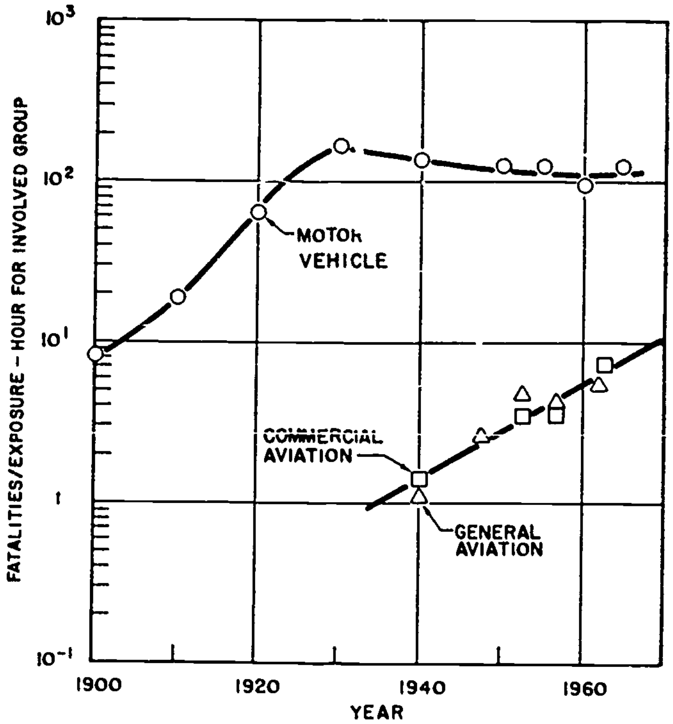


Fig. 5 (left). Risk and participation trends for general aviation. Fig. 6 (above). Group risk plotted relative to year.

Commercial aviation has barely approached a risk level comparable to that set by disease. The trend is similar to that for motor vehicles, as shown in Fig. 4. However, the percentage of the population participating is now only 1/20 that for motor vehicles. Increased public participation in commercial aviation will undoubtedly increase the pressure to reduce the risk, because, for the general population, the benefits are much less than those associated with motor vehicles. Commercial aviation has not yet reached the point of optimum benefit-risk trade-off (*5*).

For general aviation the trends are similar, as shown in Fig. 5. Here the risk levels are so high (20 times the risk from disease) that this activity must properly be considered to be in the category of adventuresome sport. However, the rate of risk is decreasing so rapidly that eventually the risk for general aviation may be little higher than that for commercial aviation. Since the percentage of the population involved is very small, it appears that the present average risk levels are acceptable to only a limited group (*6*).

The similarity of the trends in Figs. 3–5 may be the basis for another hypothesis, as follows: the acceptable risk is inversely related to the number of people participating in an activity.

The product of the risk and the percentage of the population involved in each of the activities of Figs. 3–5 plotted in Fig. 6. This graph represents the historical trend of total fatalities per hour of exposure of the population involved (*7*). The leveling off of motor-vehicle risk at about 100 fatalities per hour of exposure of the participating population may be significant. Because most of the U.S. population is involved, this rate of fatalities may . sufficient public visibility to set a level of social acceptability. It is interesting, and disconcerting, to note that the trend of fatalities in aviation, both commercial and general, is uniformly upward.

Public Awareness

Finally, I attempted to relate these risk data to a crude measure of public awareness of the associated social benefits (see Fig. 7). The "benefit awareness" was arbitrarily defined as the product of the relative level of advertising, the square of the percentage of population involved in the activity, and the relative usefulness (or importance) of the activity to the individual (*8*). Perhaps these assumptions are too crude, but Fig. 7 does support the reasonable position that advertising the benefits of an activity increases public

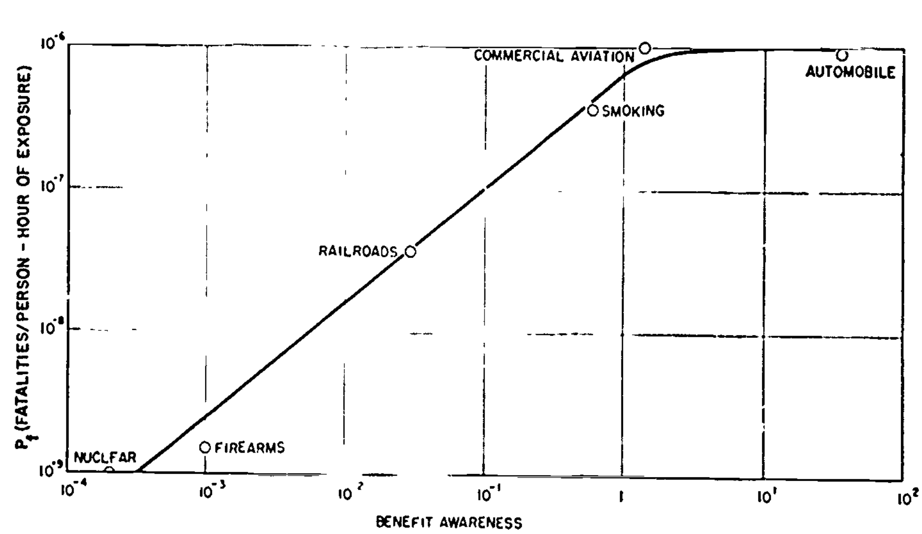


Fig. 7. Accepted risk plotted relative to benefit awareness (see text).

acceptance of a greater level of risk. This, of course could subtly produce a fictitious benefit-risk ratio—as may be the case for smoking.

Atomic Power Plant Safety

I recognize the uncertainty inherent in the quantitative approach discussed here, but the trends and magnitudes may nevertheless be of sufficient validity to warrant their use in determining national "design objectives" for technological activities. How would this be done?

Let us consider as an example the introduction of nuclear power plants as a principal source of electric power. This is an especially good example because the technology has been primarily nurtured, guided, and regulated by the government, with industry undertaking the engineering development and the diffusion into public use. The government specifically maintains responsibility for public safety. Further, the engineering of nuclear plants permits continuous reduction of the probability of accidents, at a substantial increase in cost. Thus, the trade-off of utility and potential risk can be made quantitative.

Moreover, in the case of the nuclear power plant the historical empirical approach to achieving an optimum benefit-risk trade-off is not pragmatically feasible. All such plants are now so safe that it may be 30 years or longer before meaningful risk experience will be accumulated. By that time, many plants of varied design will be in existence, and the empirical accident data may not be applicable to those being built. So a very real need exists now to establish "design objectives" on a predictive-performance basis.

Let us first arbitrarily assume that nuclear power plants should be as safe as coal-burning plants, so as not to increase public risk. Figure 2 indicates that the total risk to society from electric power is about 2×10^{-9} fatality per person per hour of exposure. Fossil fuel plants contribute about ⅕ of this risk, or about 4 deaths per million population per year. In a modern society, a million people may require a million kilowatts of power, and this is about the size of most new power stations. So, we now have a target risk limit of 4 deaths per year per million-kilowatt power station (*9*).

Technical studies of the consequences of hypothetical extreme (and unlikely) nuclear power plant catastrophes, which would disperse radioactivity into populated areas, have indicated that about 10 lethal cancers per million population might result (*10*). On this basis, we calculate that such a power plant might statistically have one such accident every 3 years and still meet the risk limit set. However, such a catastrophe would completely destroy a major portion of the nuclear section of the plant and either require complete dismantling or years of costly reconstruction. Because power companies expect plants to last about 30 years, the economic consequences of a catastrophe every few years would be completely unacceptable. In fact, the operating companies would not accept one such failure, on a statistical basis, during the normal lifetime of the plant.

It is likely that, in order to meet the economic performance requirements of the power companies, a catastrophe rate of less than 1 in about 100 plant-years would be needed. This would be a public risk of 10 deaths per 100 plant-years, or 0.1 death per year per million population. So the economic investment criteria of the nuclear plant user—the power company—would probably set a risk level 1/200 the present socially accepted risk associated with electric power, or 1/40 the present risk associated with coal-burning plants.

An obvious design question is this: Can a nuclear power plant be engineered with a predicted performance of less than 1 catastrophic failure in 100 plant-years of operation? I believe the answer is yes, but that is a subject for a different occasion. The principal point is that the issue of public safety can be focused on a tangible, quantitative, engineering design objective.

This example reveals a public safety consideration which may apply to many other activities: The economic requirement for the protection of major capital investments may often be a more demanding safety constraint than social acceptability.

Conclusion

The application of this approach to other areas of public responsibility is self-evident. It provides a useful methodology for answering the question "How safe is safe enough?" Further, although this study is only exploratory, it reveals several interesting points. (i) The indications are that the public is willing to accept "voluntary" risks roughly 1000 times greater than "involuntary" risks. (ii) The statistical risk of death from disease appears to be a psychological yardstick for establishing the level of acceptability of other risks. (iii) The acceptability of risk appears to be crudely proportional to the third power of the benefits (real or imagined). (iv) The social acceptance of risk is directly influenced by public awareness of the benefits of an activity, as determined by advertising, usefulness, and the number of people participating. (v) In a sample application of these criteria to atomic power plant safety, it appears that an engineering design objective determined by economic criteria would result in a design-target risk level very much lower than the present socially accepted risk for electric power plants.

Perhaps of greatest interest is the fact that this methodology for revealing existing social preferences and values may be a means of providing the insight on social benefit relative to cost that is so necessary for judicious national decisions on new technological developments.

Appendix: Details of Risk-Benefit Analysis

Motor-vehicle travel. The calculation of motor-vehicle fatalities per exposure hour per year is based on the number of registered cars, an assumed 1½ persons per car, and an assumed 400 hours per year of average car use [data from *3* and *11*]. The figure for annual benefit for motor-vehicle travel is based on the sum of costs for gasoline, maintenance, insurance, and car payments and on the value of the time savings per person. It is assumed that use of an automobile allows a person to save 1 hour per working day and that a person's time is worth $5 per hour.

Travel by air route carrier. The estimate of passenger fatalities per passenger-hour of exposure for certified air route carriers is based on the annual number of passenger fatalities listed in the *FAA Statistical Handbook of Aviation* (see *12*) and the number of passenger-hours per year. The latter number is estimated from the average number of seats per plane, the seat load factor, the number of revenue miles flown per year, and the average plane speed (data from *3*). The benefit for travel by certified air route carrier is based on the average annual air fare per passenger-mile and on the value of the time saved as a result of air travel. The cost per passenger is estimated from the average rate per passenger-mile (data from *3*), the revenue miles flown per year (data from *12*), the annual number of passenger boardings for 1967 (132×10^6, according to the United Air Lines News Bureau), and the assumption of 12 boardings per passenger.

General aviation. The number of fatalities per passenger-hour for general aviation is a function of the number of annual fatalities, the number of plane hours flown per year, and the average number of passengers per plane (estimated from the ratio of fatalities to fatal crashes) (data from *12*). It is assumed that in 1967 the cash outlay for initial expenditures and maintenance costs for general aviation was $\$1.5 \times 10^9$. The benefit is expressed in terms of annual cash outlay per person, and the estimate is based on the number of passenger-hours per year and the assumption that the average person flies 20 hours, or 4000 miles, annually. The value of the time saved is based on the assumption that a person's time is worth $10 per hour and that he saves 60 hours per year through traveling the 4000 miles by air instead of by automobile at 50 miles per hour.

Railroad travel. The estimate of railroad passenger fatalities per exposure hour per year is based on annual passenger fatalities and passenger-miles and an assumed average train speed of 50 miles per hour (data from *11*). The passenger benefit for railroads is based on figures for revenue and passenger-miles for commuters and noncommuters given in *The Yearbook of Railroad Facts* (Association of American Railroads, 1968). It is assumed that the average commuter travels 20 miles per workday by rail and that the average noncommuter travels 1000 miles per year by rail.

Skiing. The estimate for skiing fatalities per exposure hour is based on information obtained from the National Ski Patrol for the 1967–68 southern California ski season: 1 fatality, 17 days of skiing, 16,500 skiers per day, and 5 hours of skiing per skier per day. The estimate of benefit for skiing is based on the average number of days of skiing per year per person and the average cost of a typical ski trip [data from "The Skier Market in Northeast North America," *U.S. Dep. Commerce Publ.* (1965)]. In addition, it is assumed that a skier spends an average of $25 per year on equipment.

Hunting. The estimate of the risk in hunting is based on an assumed value of 10 hours' exposure per hunting day, the annual number of hunting fatalities, the number of hunters, and the average number of hunting days per year [data from *11* and from "National Survey of Fishing and Hunting," *U.S. Fish Wildlife Serv. Publ.* (1965)]. The average annual expenditure per hunter was $82.54 in 1965 (data from *3*).

Smoking. The estimate of the risk from smoking is based on the ratio for the mortality of smokers relative to nonsmokers, the rates of fatalities from heart disease and cancer for the general population, and the assumption that the risk is continuous [data from the *Summary of the Report of the Surgeon General's Advisory Committee on Smoking and Health* (Government Printing Office, Washington, D.C., 1964)]. The annual intangible benefit to the cigarette smoker is calculated from the American Cancer Society's estimate that 30 percent of the population smokes cigarettes, from the number of cigarettes smoked per year (see *3*), and from the assumed retail cost of $0.015 per cigarette.

Vietnam. The estimate of the risk associated with the Vietnam war is based on the assumption that 500,000 men are exposed there annually to the risk of death and that the fatality rate is 10,000 men per year. The benefit for Vietnam is calculated on the assumption that the entire U.S. population benefits intangibly from the annual Vietnam expenditure of $\$30 \times 10^9$.

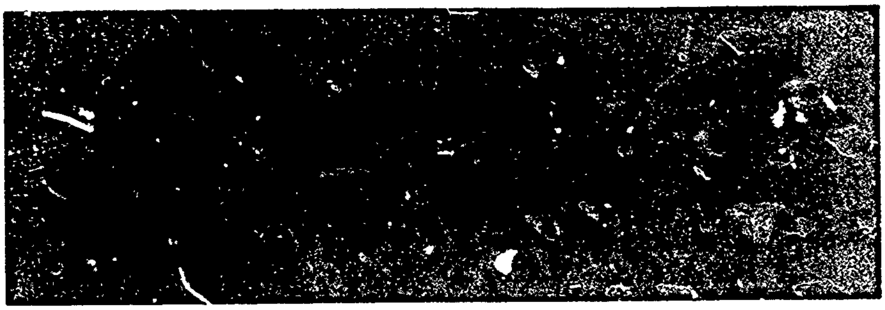
Electric power. The estimate of the risk associated with the use of electric power is based on the number of deaths from electric current; the number of deaths from fires caused by electricity; the number of deaths that occur in coal mining, weighted by the percentage of total coal production used to produce electricity; and the number of deaths attributable to air pollution from fossil fuel stations [data from *3* and *11* and from *Nuclear Safety* 5, 325 (1964)]. It is assumed that the entire U.S. population is exposed for 8760 hours per year to the risk associated with electric power. The estimate for the benefit is based on the assumption that there is a direct correlation between per capita gross national product and commercial energy consumption for the nations of the world [data from Briggs, *Technology and Economic Development* (Knopf, New York 1963)]. It is further assumed that 35 percent of the energy consumed in the U.S. is used to produce electricity.

Natural disasters. The risk associated with natural disasters was computed for U.S. floods (2.5×10^{-10} fatality per person-hour of exposure), tornadoes in the Midwest (2.46×10^{-10} fatality), major U.S. storms (0.8×10^{-10} fatality), and California earthquakes (1.9×10^{-10} fatality) (data from *11*). The value for flood risk is based on the assumption that everyone in the U.S. is exposed to the danger 24 hours per day. No benefit figure was assigned in the case of natural disasters.

Disease and accidents. The average risk in the U.S. due to disease and accidents is computed from data given in *Vital Statistics of the U.S.* (Government Printing Office, Washington, D.C., 1967).

References and Notes

1. A. L. Shef, "Socio-economic attributes of our technological society," paper presented before the IEEE (Institute of Electrical and Electronics Engineers) Wescon Conference, Los Angeles, August 1968.
2. *Minerals Yearbook* (Government Printing Office, Washington, D.C., 1966).
3. *U.S. Statistical Abstract* (Government Printing Office, Washington, D.C., 1967).
4. The procedure outlined in the appendix was used in calculating the risk associated with motor-vehicle travel. In order to calculate exposure hours for various years, it was assumed that the average annual driving time per car increased linearly from 50 hours in 1900 to 400 hours in 1960 and thereafter. The percentage of people involved is based on the U.S. population, the number of registered cars, and the assumed value of 1.5 people per car.
5. The procedure outlined in the appendix was used in calculating the risk associated with, and the number of people who fly in, certified air route carriers for 1967. For a given year, the number of people who fly is estimated from the total number of passenger boardings and the assumption that the average passenger makes six round trips per year (data from *3*).
6. The method of calculating risk for general aviation is outlined in the appendix. For a given year, the percentage of people involved is defined by the number of active aircraft (see *3*); the number of people per plane, as defined by the ratio of fatalities to fatal crashes; and the population of the U.S.
7. Group risk per exposure hour for the involved group is defined as the number of fatalities per person-hour of exposure multiplied by the number of people who participate in the activity. The group population and the risk for motor vehicles, certified air route carriers, and general aviation can be obtained from Figs. 3–5.
8. In calculating "benefit awareness" it is assumed that the public's awareness of an activity is a function of *A*, the amount of money spent on advertising; *P*, the number of people who take part in the activity; and *U*, the utility value of the activity to the person involved. *A* is based on the amount of money spent by a particular industry in advertising its product, normalized with respect to the food and food products industry, which is the leading advertiser in the U.S.
9. In comparing nuclear and fossil fuel power stations, the risks associated with the plant effluents and mining of the fuel should be included in each case. The fatalities associated with coal mining are about ¼ the total attributable to fossil fuel plants. As the tonnage of uranium ore required for an equivalent nuclear plant is less than the coal tonnage by more than an order of magnitude, the nuclear plant problem primarily involves hazard from effluent.
10. This number is my estimate for maximum fatalities from an extreme catastrophe resulting from malfunction of a typical power reactor. For a methodology for making this calculation, see F. R. Farmer, "Siting criteria—a new approach," paper presented at the International Atomic Energy Agency Symposium in Vienna, April 1967. Application of Farmer's method to a fast breeder power plant in a modern building gives a prediction of fatalities less than this assumed limit by one or two orders of magnitude.
11. "Accident Facts," *Nat. Safety Counc. Publ* (1967).
12. *FAA Statistical Handbook of Aviation* (Government Printing Office, Washington, D.C., 1965).



Risk Regulation: A Problem for Democracy in the Technological Age

In 1906, Congress enacted the Pure Food and Drug Act, the first general food and drug safety law for the United States. Commenting on the provisions of the act, the House committee observed: "The question whether certain substances are poisonous or deleterious to health the bill does not undertake to determine, but leaves that to the determination of the Secretary . . . under the guidance of proper disinterested scientific authorities, after most careful study, examination, experiment and thorough research."

This statement reflected a deep faith in the ability of "disinterested" scientists to determine for society what substances posed an unacceptable risk. More than 70 years of regulation have called into question that naïve faith. We are no longer content to delegate the assessment of and response to risk to so-called disinterested scientists. Indeed, the very concept of objectivity embodied in the word disinterested is now discredited. The astounding explosion of scientific knowledge and the increasing sophistication of the public have radically transformed our attitude toward risk regulation. As governmental health and safety regulation has become pervasive, there is a pressing need to redefine the relation between science and law. This is one of the greatest challenges now facing government and, indeed, society as a whole.

Risk regulation poses a peculiar problem for government. Few favor risk for its own sake. But new risks are the inevitable price of the benefits of progress in an advanced industrial society. In order to have the energy necessary to run our homes and our factories, we incur risks of energy production, whether they be the risks of coal mining, nuclear reactor accidents, or the chance that a tree will fall on a man felling it to produce firewood. In order to have mobility, we risk auto accidents and illness from air pollution. In order to have variety and convenience in our food supply, we risk cancer or other toxic reactions to additives.

Ironically, scientific progress not only creates new risks but also uncovers previously unknown risks. As our understanding of the world grows exponentially, we are constantly learning that old activities, once thought safe, in fact pose substantial risks. The question then is not whether we will have risk at all, but how much risk, and from what source. Perhaps even more important, the question is who shall decide.

In our daily lives we do not confront the trade-off between dollars and lives very directly or self-consciously. But when we make societal policy decisions, such as how much to spend to eliminate disease-producing pollutants, we are painfully aware that we must make what Guido Calabresi has called "tragic choices."

In primitive societies these choices were often made by the tribal witch doctor. When the need to choose between cherished but conflicting values threatened to disrupt the society, the simplest path was decision by a shaman, or wizard, who claimed special and miraculous insight. In our time shamans carry the title doctor instead of wizard, and wear lab coats and black robes instead of religious garb.

But ours is an age of doubt and skepticism. The realist movement in law effectively stripped the judiciary of its Solomonic cloak. So, too, the public has come to realize the inherent limitations of scientific wisdom and knowledge. We have been cast from Eden, and must find ways to cope with our intellectual nakedness. To the basic question of how much risk is acceptable—a choice of values—we have learned that there is no one answer. To the problem of how much risk a given activity poses, we have learned that even our experts often lack the certain knowledge that would ease our decision-making tasks. Often the best we can say is that a product or an activity poses a "risk of risk."

Who Decides? Scientists v. the Public

Under these circumstances, the questions of who decides and how that decision is made become all the more critical. Since we have no shaman we must have confidence in the decision-making process so that we may better tolerate the uncertainties of our decisions.

Courts are often thrust into the role of authoritative decision-makers. But in recent years there has been growing concern about the ability of the judiciary to cope with the complex scientific and technical issues that come before our courts. Critics note, quite correctly, that judges have little or no training to understand and resolve problems on the frontiers of nuclear physics, toxicology, hydrology, and a myriad of other specialties. And the problem is growing. Hardly a sitting in our court goes by without a case from the Environmental Protection Agency, the Food and Drug Administration, the Occupational Safety and Health Administration (OSHA), or the Nuclear Regulatory Commission (NRC). These cases often present questions that experts have grappled with for years, without coming to any consensus.

But the problem, of course, is not confined to the judicial branch. Legislators are daily faced with the same perplexing questions. They, too, lack the expertise to penetrate the deepest scientific mysteries at the core of important issues of public concern. This problem ultimately strikes at the very heart of democracy. The most important element of our government, the voter, simply cannot be expected to understand the scientific predicate of many issues he must face at the polls.

Some well-meaning scientists question the wisdom of leaving risk regulation to the scientifically untutored. They wonder, to themselves if not aloud, whether the public should be permitted to make decisions for society when it cannot understand the complex scientific questions that underlie the decisions. Some scientists point with relish to the contradictory and seemingly irrational response of the public to risk. They observe the public's alarm at the prospect of nuclear power and note that the same public tolerates 50,000 automobile deaths a year. They decry the Delaney clause, which singles out cancer among all serious risks and imposes a rigid ban,

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regardless of countervailing benefits.

Scientists are also concerned by the growing public involvement in decisions that, in the past, were left entirely to the scientific community. Many scientists believe that regulation has intruded too deeply into the sanctum sanctorum. The controversy ranges from the periphery of scientific pursuits, such as OSHA regulation of laboratory work conditions, to the heart of the scientific enterprise, such as the conflict over recombinant DNA research. Regulators are accused of stifling creativity and innovation in the name of the false g[illegible] f safety. Science, once invoked as an ally to progressive government, more and more views the political process with hostility and disdain.

In reaction to the public's often emotional response to risk, scientists are tempted to disguise controversial value decisions in the cloak of scientific objectivity, obscuring those decisions from political accountability.

At its most extreme, I have heard scientists say that they would consider not disclosing risks which in their view are insignificant, but which might alarm the public if taken out of context. This problem is not mere speculation. Consider the recently released tapes of the NRC's deliberation over the accident at Three Mile Island. They illustrate dramatically how concern for minimizing public reaction can overwhelm scientific candor.

This attitude is doubly dangerous. First, it arrogates to the scientists the final say over which risks are important enough to merit public discussion. More important, it leads to the suppression of information that may be critical to developing new knowledge about risks or even to developing ways of avoiding those risks.

It is certainly true that the public's reaction to risk is not always in proportion to the seriousness of the threatened harm discounted by its probability. But the public's fears are real.

Scientists must resist the temptation to belittle these concerns, however irrational they may seem. The scientific community must not turn its back on the political processes to which we commit societal decisions. Scientists, like all citizens, must play an active role in the discussion of competing values. Their special expertise will inevitably and rightly give them a persuasive voice when issues are discussed in our assemblies and on our streets. But the choice must ultimately be made in a politically responsible fashion. To those who feel the public is incapable of comprehending the issues, and so unable to make informed value choices, I respond with the words of Thomas Jefferson:

> I know no safe depository of the ultimate powers of the society but the people themselves; and if we think them not enlightened e[illegible]h to exercise their control with a wholesc[illegible] discretion, the remedy is not to take it from them, but to inform their discretion.

Scientist, regulator, lawyer, and layman must work together to reconcile the sometimes conflicting values that underlie their respective interest, perspectives, and goals. This cooperation can be achieved only through a greater understanding of the proper roles of the scientific, political, and legal communities in addressing the public regulation of risk. Only then can we achieve a program of risk regulation that accommodates the best of scientific learning with the demands of democracy.

Sorting Out Scientific Facts, Inferences, and Values in Risk Regulation

The starting point is to identify the fact and value questions involved in a risk regulation decision. In determining questions of fact, such as the magnitude of risk from an activity, we as a society must rely on those with the appropriate expertise. Judges and politicians have no special insights in this area. Where questions of risk regulation involve value choices such as how much risk is acceptable, we must turn to the political process.

But even this formulation leaves many problems unanswered. There is no bright line between questions of value and of fact. Even where a problem is appropriately characterized as one of scientific fact, consensus and certainty may very often be impossible even in the scientific community. Many problems of scientific inference lie in the realm of "trans-science" and cannot be resolved by scientific method and experimentation.

The recent National Academy of Sciences (NAS) report on saccharin vividly illustrates the problem of separating fact from value in risk regulation. Although there is a reasonable scientific consensus on the effects of saccharin in rats, the important question of human risks and the appropriate response to those risks remain controversial. On the basis of uncontroverted animal experimental data, the NAS panel could not conclude whether saccharin should be considered a substance posing a "high" risk of cancer, or only a "moderate" risk. Yet this lack of consensus should not surprise us. As Philip Handler, president of the NAS, observed in his preface to the report, "the difference of opinion which led to this ambivalent statement is not a differing interpretation of scientific fact or observation; it reflects, rather, seriously differing value systems."

Handler's statement reveals a critical issue in risk regulation. When the debate over saccharin is couched in terms of the degree of risk, it sounds as though there is a scientific issue, appropriate for resolution by trained scientists. In fact, however, the terms moderate and high do not conform to any differences in experimental data, but rather correspond to the scientists' view of the appropriate regulatory response.

The growing use of analytic tools such as cost-benefit analysis magnifies the chance that unrecognized value judgments will creep into apparently objective assessments. Even the most conscientious effort by experts not to exceed their sphere of competence may be inadequate to safeguard the validity of the decision-making process. Outside scrutiny may be imperative.

The Role of Courts

It is at this point that courts can make their contribution to sound decision-making. Courts cannot second-guess the decisions made by those who, by virtue of their expertise or their political accountability, have been entrusted with ultimate decisions. But courts can and have played a critical role in fostering the kind of dialogue and reflection that can improve the quality of those decisions.

Courts, standing outside both scientific and political debate, can help to make sure that decision-makers articulate the basis for their decisions. In the scientists' realm—the sphere of fact—courts can ask that the data be described, hypotheses articulated, and above all, in those areas where we lack knowledge, that ignorance be confessed. In the political realm—the sphere of values—courts can ask that decision-makers explain why they believe that a risk is too great to run, or why a particular trade-off is acceptable. Perhaps most important, at the interface of fact and value, courts can help ensure that the value component of decisions is explicitly acknowledged, not hidden in quasi-scientific jargon.

This role does not require, as some have suggested, that courts intrude excessively into an agency's processes. The demands of adequate process are not burdensome. Surely it is not unreasonable to suggest that agencies articulate the basis of their decisions or that they open their proceedings and deliberations to all interested participants and all relevant information.

These requirements are in everyone's best interest, including decision-makers themselves. If the decision-making process is open and candid it will inspire more confidence in those who are affected. Futher, by opening the process to public scrutiny and criticism, we reduce the risk that important information will be overlooked or ignored. Finally, openness will promote peer review of both factual determinations and value judgments.

Coping with Uncertainty

Risk regulation in itself carries risks. No problem of any significance is so well understood that we can predict with confidence what the outcome of any decision will be. But there are two different kinds of uncertainty that plague risk regulation. Some uncertainty is inherent in regulating activities on the frontiers of scientific progress. For example, we simply do not know enough about the containment potential of salt domes to know with confidence whether they are adequate for storing nuclear wastes for thousands of years. In the face of such uncertainty society must decide whether or not to take a chance—to wait for more information before going ahead with nuclear production, or to go forward and gamble that solutions will be found in the future.

The other kind of uncertainty that infects risk regulation comes from a refusal to face the hard questions created by lack of knowledge. It is uncertainty produced by scientists and regulators who assure the public that there are no risks, but know that the answers are not at hand. Perhaps more important, it is a false sense of security because the hard questions have never been asked in the first place.

In the early days of nuclear plant licensing, for example, the problem of long term waste disposal was never even an issue. Only after extensive prodding by environmental and citizens' groups did the industry and regulators show any awareness of waste disposal as a problem at all. Judges like myself became troubled when those charged with ensuring nuclear safety refused even to recognize the seriousness of the waste disposal issue, much less to propose a solution.

I expressed these concerns in *Natural Resources Defense Council v. Nuclear Regulatory Commission* (*1*). In that case our court was asked to review the NRC's quantification of the environmental effects of the uranium fuel cycle, including the "back end" of the cycle, waste disposal and reprocessing.

The NRC concluded that those effects are "relatively insignificant." Yet the only evidence adduced in support of its assessment was the testimony of a single NRC expert. Most of the testimony was conclusory and the expert gave little or no explanation of the underlying basis for his optimism.

To my mind, that testimony, without more, provided an inadequate basis for making critical nuclear plant licensing decisions. My objection was not founded on any disagreement with the expert's conclusions. For all I knew then or know now, he may have been accurate in minimizing the risks from nuclear waste disposal. Nor do I criticize the NRC for failing to develop foolproof solutions to the problem of waste disposal. What I found unacceptable was the almost cavalier manner with which the NRC accepted the sanguine predictions and refused to come to grips with the limits of the agency's knowledge. I stated (2):

> To the extent that uncertainties necessarily underlie predictions of this importance on the frontiers of science and technology, there is a concomitant necessity to confront and explore fully the depth and consequences of such uncertainties. Not only were the generalities relied on in this case not subject to rigorous probing—in any form—but when apparently substantial criticisms were brought to the Commission's attention, it simply ignored them, or brushed them aside. Without a thorough exploration of the problems involved in waste disposal, including past mistakes, and a forthright assessment of the uncertainties and differences in expert opinion, this type of agency action cannot pass muster as reasoned decisionmaking.

The "thorough exploration" that I found lacking is particularly important in technically complex matters such as nuclear waste disposal. Since courts lack the expertise to assess the merits of the scientific controversy, "society must depend largely on oversight by the technically trained members of the agency and the scientific community at large to monitor technical decisions." There were a number of avenues open to the NRC for the kind of exploration that permits meaningful oversight—but the agency adopted none of them.

The Supreme Court unanimously reversed our decision (*3*). They felt that we had imposed extra procedures on the NRC beyond those required by law for so-called informal rule-making under the 1946 Administrative Procedure Act. They returned the case to our court, however, to determine whether the record supported the substantive conclusions of the NRC.

Whether the Supreme Court's decision represents a fair reading of what our opinion in fact required the agency to do, I leave to the legal scholars. My own view is that the Supreme Court's decision will have little impact because many of the new laws governing risk regulation explicitly direct agencies to use decision-making procedures that supplement the minimal requirements of informal rule-making under the Administrative Procedure Act. Statutes such as the Clean Air Act Amendments of 1977, the Clean Water Act of 1977, and the Toxic Substances Control Act of 1976 include procedural and record-enhancing features that will contribute substantially to the quality and accountability of agency decisions.

A Structured Approach to Decision-Making Under Uncertainty

I have never believed that procedures per se are a cure-all for solving regulatory problems. Rather, procedural safeguards serve an instrumental role, and it is the fullness of the inquiry that is paramount. If the inquiry is comprehensive and conscientious without additional procedural safeguards, it provides the best record we can hope for in making the difficult choices we now face. Conversely, even when all the procedural niceties are observed, if there is no commitment to a candid exploration of the issues, the predicate for good decision-making will be lacking.

Agencies are now revising their procedures to increase the availability of expert advice without abdicating agency responsibility for value decisions. Agencies have begun to encourage and fund public intervenors. These steps have increased the range of the administrative process, and have forced the agencies to wrestle with the difficult questions which might otherwise escape public scrutiny. Restrictions on ex parte contacts have increased our confidence in agencies' impartiality and fairness. The visibility of decision-making processes and decisions themselves has been enhanced by Congress' and the courts' commitment to openness, through the Freedom of Information Act, the Advisory Committee Act, and the Sunshine Act. I am confident that the courts will continue vigorously to carry out Congress' mandate that decisonmaking be honest, open, thorough, rational, and fair.

The Problem of Delay

Considering all relevant data and viewpoints is essential to good decisions. This is why I am concerned by recent proposals to shorten the decision-making process for licensing nuclear reactors. I have no doubt that some of the current delay is unnecessary, and it may be that current proposals do not affect critical deliberative processes. I do not express any views on specific proposals. I only want to caution that in speeding up the process, we must take care not to sacrifice the valuable and productive safeguards that have come to be built into the decision-making process.

I do not favor delay caused by an unthinking rejection of progress. Delay from unjustified fear of the future can in the long run cause more harm than the risks it prevents. But delay that is necessary for calm reflection, full debate, and mature decision more than compensates for the additional costs it imposes. The Alaska Pipeline was embroiled in extensive controversy in our courts, primarily by environmental groups who questioned whether sufficient attention was given to safety issues. The litigation imposed substantial costs, both the rising expenses for building the pipeline and the cost of postponing a major source of domestic energy. But in the subsequent attorneys' fees proceedings the companies themselves conceded that the litigation produced substantial safety improvements in the pipeline that Congress ultimately approved. Sometimes the benefits of delay can be dramatic. The American experience in avoiding the tragedy of thalidomide is a poignant but not unique example.

By strengthening the administrative process we provide a constructive and creative response to the inherent uncertainties of risk regulation. Approaching the decision to take or to step back from risks such as nuclear power is like coming to a busy intersection with our view partially obscured. Our instincts tell us to proceed with caution, because intersections are dangerous. Ultimately, the importance of our journey and the desirability of our goal may lead us to brave the traffic and pull out into the highway. But even when we decide to proceed, we should not omit the moment of reflection to observe the passing cars, and look both ways.

References and Notes

1. *Natural Resources Defense Council v. Nuclear Regulatory Commission*, Fed. Rep. 2nd Ser., vol. 547, p. 633 (D.C. Circuit 1976), reversed *sub nom. Vermont Yankee Nuclear Power Corp. v. Natural Resources Defense Council* (*3*). Nothing in these remarks should be taken to intimate any views of the merits of this case in its present posture, on remand from the Supreme Court.
2. *Ibid.*, p. 653.
3. *Vermont Yankee Nuclear Power Corp. v. Natural Resources Defense Council*, U.S. Rep., vol. 435, p. 519 (1978).

Comment on Societal Risk

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The terms "hazard" and "risk" can be used in various ways. Their usage in this article is defined by the following simple example.

Three people crossing the Atlantic in a rowboat face a hazard of drowning. The maximum societal hazard in this case is three deaths. Three hundred people crossing the Atlantic in an ocean liner face the same hazard of drowning, but the maximum societal hazard is 300 deaths. The risk to each individual per crossing is given by the probability of the occurrence of an accident in which he or she drowns. The risk to society is given by the size of the societal hazard multiplied by the probability of the hazard. Clearly the hazard is the same for each individual, but the risk is greater for the individuals in the rowboat than in the ocean liner (*1*).

Some general observations follow:

1) Society is not risk-free and cannot be. No energy source is free of risk, either to the environment or to the public. This includes solar energy (*2*). Measures toward achieving "soft" energy, zero increase in energy consumption, and even conservation inherently carry risk.

2) There are large gaps in society's understanding of risks and the economics of risk management (*3*). Risk-benefit analysis, in the legislative process and elsewhere, is an important tool in decision-making, and should be judiciously employed. Procedures are needed to ensure proper disclosure of assumptions, uncertainties, unaggregated results, and so forth, and to ensure impartial evaluation and review of any important risk-benefit decisions.

3) The consequences of two different hazards may vary greatly with respect to their measurability. This problem (together with the possible erroneousness of the raw data), has led to a questioning of the desirability of using such information in making decisions. Furthermore, some hazards, such as the greenhouse effect or the effect of energy policies on the chance of war (*4*), introduce risks that can be difficult to quantify. Nevertheless, for many societal hazards, risk quantification, albeit imperfect and frequently containing large uncertainties, is usually possible and desirable. Decisions still have to be made, and they are likely to be better if they are made with the benefit of more complete information—keeping in mind the need for judgment as to when the inability to completely quantify may lead to unwarranted dependence on estimates. Consideration should be given to requiring risk quantification, as practical, for societal endeavors.

4) Society uses the word safe in a vague and inconsistent fashion. Efforts to reduce risk are not necessarily made in the most cost-effective way. Our priorities should be reevaluated.

5) In view of their statistically smaller contribution to societal risk, major accidents may be receiving proportionately too much emphasis compared to other sources of risk, such as chemical residues, pollutants, and wastes.

6) Society's resources are limited. When resources are lavished on a

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needed service, less is available for use in measures that reduce the number of injuries and premature deaths. Thus a more expensive source of electricity carries an economic penalty compared to a cheaper source. Above a particular level, expenditure of resources on additional programs to reduce risks to health and safety may be counterproductive because of adverse economic and political effects.

7) Congress should take the lead in establishing a national risk management program that is equitable and more quantitative.

The Need for Information

There are few published assessments of the many hazards and risks to which society is exposed. And there are still fewer risk assessments that (i) provide a detailed statement of the assumptions made in arriving at the conclusion, (ii) point out any uncertainties in the results, and (iii) have the benefit of a detailed evaluation by a competent independent body.

For example, it is difficult to find published quantitative estimates of the risks posed by the thousands of large dams in the United States. In fact, the safety of such dams is generally poorly known, particularly with respect to the more serious, lower probability modes of failure. The situation is the same for facilities in which large amounts of hazardous chemicals are stored.

We also know little about the risks created by emissions of substances into the atmosphere, by the disposal of liquid and solid wastes, by coal-fueled electric power stations, by residues and additives in our food, by occupational environments—and the list could go on.

Nevertheless, substantial improvement can be made in our knowledge about risks and the costs of their reduction.

Examples of Hazard and Risk Estimates

Canvey Island. An interesting and significant risk study, "Canvey: summary of an investigation of potential hazards from operations in the Canvey Island/Thurrock area" (5), was released in June 1978 by the Health and Safety Executive of the British government.

Canvey Island lies in the Thames River and is 9 miles long and 2.5 miles wide. It has 33,000 residents and seven large industrial complexes, including petroleum, ammonium nitrate, and liquefied natural gas facilities. The largest risk of death from an accident at one of these industrial facilities was estimated to be about 1.3×10^{-3} (1 in 800) per year for some of the nearest Canvey residents. This risk is about five times as large as the average risk of dying in an automobile accident in the United States. The average risk of death from an accident at these installations was estimated to be about 5×10^{-4} (1 in 2000) per year for all the island's residents. This is about twice the risk of death from an auto accident in the United States. The chance of 1500 people being killed in a single accident was given as more than 1 in 1000 per year. The chance of 18,000 being killed in a single accident was given as 1 in 12,000 per year.

It was stated that these estimates probably erred on the side of pessimism by a factor of 2 or 3, but probably not by a factor of 10. The Health and Safety Executive recommended that improvements be made that would reduce the likelihood of each of the risk estimates by a factor of 2 or 3. With these improvements, it was judged, the risks would be acceptable.

My discussions with British experts in safety assessment have given me the impression that they doubt the practicality of obtaining improvement, for every large facility in the British chemical industry, by more than a factor of 10 over the risks estimated for Canvey. However, the British are making it a matter of national law that safety assessment reports be submitted by each industrial facility utilizing or storing more than a particular quantity of a hazardous chemical. Notification is still required if some specified lesser quantity is stored. The Health and Safety Executive will have the responsibility for evaluating the risk assessment and deciding on the acceptability of the risk.

Japan is also instituting safety design requirements for chemical plants, requirements that become increasingly strict in proportion to the number of deaths that might occur if there is a serious accident.

Should not the United States be developing some systematic approach to these and other societal risks? I have little doubt that we have many chemical installations posing risks not unlike those at Canvey.

Dams in California. Limited studies of ten California dams by our group at the University of California, Los Angeles, indicated that up to 250,000 deaths could result from catastrophic failure of the largest of these dams (6). Historically, large dams have failed (although not necessarily suddenly and in gross fashion) at a rate of about 1 in 5000 per year. However, our crude estimates of the failure rate for some of the dams studied were as large as 1 in 100 per year.

During the San Fernando Valley earthquake in 1971, the Van Norman Dam nearly failed catastrophically due to soil liquefaction, a phenomenon recognized only after its construction. Had the reservoir been full, the dam would have failed (7), possibly causing 50,000 to 100,000 fatalities.

The state of California has had a dam-safety law since the 1971 earthquake. The law specifies that the safety of each state-controlled dam must be reviewed and a finding of "safe" made. However, under the law the state need not publicize the risk it is imposing when it determines that a dam is safe. And, of course, the maximum possible number of fatalities remains unchanged by any finding.

Earthquakes in California. California faces serious safety questions concerning the possibly catastrophic effect of earthquakes on its cities. This is also true in other states, but in California the problem is acute. On 17 March 1976 the U.S. Geological Survey advised Governor Brown of the relatively large likelihood that a major earthquake in Los Angeles would kill many thousands of people, primarily from collapse of seismically substandard buildings and from dam failure. A report prepared for the Federal Disaster Assistance Administration makes equally gloomy predictions (8). To my knowledge, seismically substandard buildings have not been posted as hazardous in Los Angeles, nor have instructions been issued on where to go in the event of dam failure. The city of Los Angeles, of course, has been grappling with the problem of seismically substandard buildings for years (*9, 10*). Seismic retrofit or building condemnation is very costly. So far as I know, the state of California has not devoted significant financial resources to this problem.

Liquefied natural gas (LNG). The LNG technology has been the object of increasingly intense safety review in the past few years. One of the few large proposed U.S. chemical installations for which a serious, detailed risk study has been published was the LNG facility for Los Angeles (*11*), Oxnard, or Point Conception, California.

The study, which was performed under a contract for the corporation requesting to build the facility, has been a subject of considerable controversy. It did not include a self-critique in which assumptions were clearly identified and uncertainties critically evaluated. And it did not establish quantitative criteria against which to judge the acceptability of the risk at each of the proposed sites.

Although other risk estimates for these proposed facilities have been issued (most project larger risks), a detailed study and evaluation by an independent group is not, to my knowledge, available.

The state of California has imposed very stringent siting requirements for LNG facilities, but California makes no systematic assessment of the hazards from large chemical installations, some of which may pose risks similar to or greater than those from the previously proposed LNG facility in Los Angeles harbor. Thus, in a state that is relatively advanced in its efforts to control risk to the public, it is difficult to find a uniform rationale for the standards, priorities, and resources used in this job.

The Flood at Big Thompson Canyon

Some may call the flood at Big Thompson Canyon, Colorado (*12*), a natural disaster. But if most of the fatalities could have been prevented by proper advance planning or emergency action, I am unwilling to shrug off the event as a natural disaster, seemingly beyond our control and not to be compared with accidents in man-made facilities.

During the evening of 31 July 1976, an intense thunderstorm stalled over a small portion of Big Thompson Canyon, dropping ten or more inches of rain in a 3-hour period. Because of the steep mountain topography, the runoff quickly formed a virtual wall of water that displaced everything in its path. Of about 4000 people in the canyon, 139 died and 4 were never found. Property damage exceeded $41 million.

The area was totally unprepared for such an event. Efforts to evacuate were made, but they were obviously inadequate. Was the loss of life the result of a natural catastrophe that could not be avoided? It might have been avoided altogether by restrictions on building in the floodplain—a controversial matter. Accepting the de facto use of the floodplain, the loss of life could still have been minimized with the benefit of some prior analysis, a reasonably direct method of measuring and monitoring rainfall, and a suitable warning system.

I do not recall any congressional investigation of the matter. Colorado has since imposed restrictions on rebuilding in the floodplain in Big Thompson, but these restrictions are being fought. There are many similar canyons all along the Front Range of the Rockies, including one that opens onto Boulder, Colorado. What safety precautions are being taken for these canyons and for other similar "natural" hazards? Is this question being given the same priority as new LNG facilities or nuclear power plants?

Expenditures to "Save a Life"

The expenditures made by society to save a single life vary to a remarkable degree. Morlat (*13*) estimated that in France, $30,000 was being spent per life saved through road accident prevention and about $1 million per life saved through aviation accident prevention. Sinclair (*14*) estimated that in Great Britain the expenditures ranged from $10,000 for an agricultural worker to $20 million for a high-rise apartment dweller.

Comparable disparities among implicit values of life are easily found in the United States. In a report prepared by the National Academy of Sciences (NAS) for the Senate Committee on Public Works in March 1975, estimates were made of the health costs of the pollutants from coal-fired electric generating plants (*15*). A figure of $30,000 per premature death was used (*15*, p. 611) "rather than the value of $200,000 used in highway safety." The reasoning for this choice was that "most of the deaths occur among chronically ill, elderly people, and the amount by which their lives are reduced may be only a matter of days or weeks." This value of life was then used as the reference value for cost-benefit trade-offs that provided a basis for evaluating the merits of various approaches to control of emissions from coal plants, including the timing of such controls. The estimated number of premature deaths resulting from the activities of coal plants was lower in the NAS report than the highest estimate given in other publications (*16*). The actual value is quite uncertain.

On the other hand, in its "As low as reasonably achievable" (ALARA) criterion for routine releases of radioactivity from a nuclear power plant, the Nuclear Regulatory Commission (NRC) employs $1000 per man-rem as the expenditure limit for making improvements. On the basis of estimates from the BEIR report (*17*), this translates into more than $5 million per premature death deferred. Furthermore, this death would probably occur after the age of 50; hence, if one remained consistent with the philosophy promulgated in the NAS study, the $5 million derived from the NRC criterion should be compared to a value less than the $200,000 quoted in the NAS report.

The societal risk from the disposal of hazardous liquid and solid wastes is substantial. I doubt that society is using the same risk-acceptance criteria or value of life in its choice of criteria for disposal of radioactive and nonradioactive wastes. I believe that a similarly large discrepancy exists with respect to regulation of the transportation of hazardous radioactive and nonradioactive materials.

Resource Allocation

Resources for the reduction of risks to the public are not infinite. At some point, a greater improvement in health and safety is to be expected from a more stable and viable economy than from a reduction in pollution or the rate of accidents. For example, Siddal (*18*) recently showed a direct correlation between increased life expectancy and improved economic circumstances in Great Britain.

Perhaps Congress should initiate appropriate studies to enable a reasonably accurate evaluation to be made of the proper level of expenditure for risk reduction. Within such a level of expenditure, if we fail to devote our resources to those risks in which the most reduction is achieved per dollar, we are not optimizing the effect of our capital outlay (*19, 20*). Of course, one must ensure that there are no gross inequities; that no individual is knowingly left exposed to a risk significantly greater than some upper limit of acceptability.

Each individual or group that makes recommendations, or otherwise takes actions affecting national priorities, bears some responsibility for any adverse effects. Thus an individual who effects the banning of DDT in a tropical country may inadvertently cause far more deaths than he defers, since the incidence of malaria will then increase. Similarly, if coal-burning electric generating plants are found to cause far more premature deaths than nuclear power plants (in agreement with most published estimates), an individual or agency that successfully advocates the construction of coal-burning plants instead of nuclear power plants may be responsible for unnecessary deaths. If the media should present an unbalanced perspective on some aspect of risk in society, and this causes risk-reduction priorities to be set inefficiently and even wrongly, the responsible media would, in effect, be contributing to the causing of premature deaths that might otherwise have been averted.

Approaches to Risk Acceptance

Lowrance (*3*) said, "A thing is safe if its risks are judged to be acceptable."

The Van Norman Dam was presumably considered to be safe before it nearly failed in 1971. Was it safe?

The Los Angeles *Times* some years ago editorialized concerning the proposed Auburn Dam, saying, "Let's build it if it's safe." What does safe mean in the context of an Auburn Dam whose failure was estimated by an experienced engineer to be capable of killing 0.75 million people (*21*)? We cannot prove that there is zero probability of its failure. What estimated failure probability is acceptable? What level of uncertainty in this estimate is acceptable? Will it be possible to demonstrate that such a safety goal can be achieved?

The NRC licensed reactor No. 2 at Three Mile Island before the accident there. Hence it had determined that "there is reasonable assurance that the activities authorized by this operating license can be conducted without endangering the health and safety of the public." However, the NRC did not provide an estimate of the residual risk remaining after the inclusion of required safety features. And the NRC still has not qualified its definition of "reasonable assurance" with substantive numbers.

The approaches society might use in coping with "How safe is safe enough?" include (i) nonintervention (rely on the marketplace), (ii) professional standards (rely on the technical experts), (iii) procedural approaches (muddle through), (iv) comparative approaches (reveal or imply preferences), (v) cost-benefit analysis, (vi) decision analysis, and (vii) expressed preferences (rely on public perception of risk).

It is to be anticipated that any generally accepted approach would incorporate facets of most of the above, as appropriate It is not proposed that quantitative risk-acceptance criteria can or should represent the whole approach. However, they should play an important role.

It is not easy to develop a workable, defensible set of quantitative risk-acceptance criteria that also allow for benefits, societal needs, equity, economics, political and social effects, and so forth. As a result, few specific proposals have been published. In an effort to stimulate discussion on the subject, Okrent and Whipple (*22*) described a simple quantitative approach to risk management that incorporates the following principal features:

1) Societal activities are divided into major facilities or technologies, all or part of which are categorized as essential, beneficial, or peripheral.

2) There is a decreasing level of acceptable risk to the most exposed individual (for example, 2×10^{-4} additional risk of death per year for the essential category, 2×10^{-5} for the beneficial category, and 2×10^{-6} for the peripheral category).

3) The risk is assessed at a high level of confidence (say 90 percent), thereby providing an incentive to obtaining better data. (The expected value of risk must be smaller the larger the uncertainty.)

4) Each risk-producing entity is subjected to risk assessment in terms of both the individual and society. The assessment is performed under the auspices of the manufacturer or owner but must be reviewed and evaluated independently; the decision on acceptability is made by a regulatory group. (For practical reasons, there would be some risk threshold below which no review was required.)

5) The cost of the residual risk is internalized, generally through a tax paid to the federal government, except for risks that are fully insurable and, like drowning, readily attributable.

6) The government, in turn, redistributes the risk tax as national health insurance or reduced taxes to the individual.

7) Risk aversion to large events would be built into the internalization of the cost of risk, but with a relatively modest penalty. If a technology or installation poses a very large hazard at some very low probability (and many do), case-by-case decisions are made, with considerable emphasis on the essentiality of the venture.

8) An ALARA criterion on risk is required, although an incentive to reduce risk and associated uncertainties would already be provided by establishing a suitable level for the risk tax.

This quantitative approach to risk management is, of course, untested. It may be both too complex and too simple. It is subject to the obvious difficulty of defining what constitutes a risk-producing entity. However, there has been all too little real discussion of the question, "How safe is safe enough?" Comar (*23*) suggested a "de minimus" approach, and there are a few other proposals. But, more typically, entire symposia are held on risk management without so much as mentioning the subject of quantitative risk criteria.

In conclusion, if our priorities in managing risk are wrong, if we are spending the available resources in a way that is not cost effective, we are, in effect, killing people whose premature deaths could be prevented. There is some optimal level of resources that should be spent on reducing societal risk, a level beyond which adverse economic and political effects may be overriding. Finally, there is need for the development of a national approach to risk management, one that Congress, the President, and the public can support.

References and Notes

1. This example is, of course, simplified with regard to the hazards faced on such a journey. Those in the rowboat face the possibility of dehydration and starvation, while those on the ocean liner are subject to fire and falling overboard. Other hazards can be imagined.
2. In 1957 I designed my own passive solar home in northern Illinois. It was very well insulated and had the tightest storm windows available. Not till 20 years later did I become aware that I had thereby been exposing my family to increased indoor air pollution. I have learned to be a skeptic about risk. I am particularly skeptical of those who advocate a particular technology as benign, or attack a technology as too risky, without presenting a detailed, quantitative risk evaluation, without making a choice among feasible alternatives, and without placing the risks in some broader societal perspective.
3. Modern risk-benefit thinking had its real birth with the classic paper by C. Starr [*Science* **165**. 1232 (1969)]. For background material, see Committee on Public Engineering Policy [*Perspectives on Benefit-Risk Decision Making* (National Academy of Engineering, Washington, D.C , 1972)]; W. W. Lowrance [*Of Acceptable Risk: Science and the Determination of Safety* (Kaufmann, Los Altos, Calif., 1976)]; W. D. Rowe [*An Anatomy of Risk* (Environmental Protection Agency, Washington, D.C., 1975)]; A. J. Van Horn and R. Wilson ("The status of risk-benefit analysis," discussion paper, Energy and Environmental Policy Center, Harvard University, Cambridge, Mass., 1976); D. Okrent, Ed. [*Risk-Benefit Methodology and Application: Some Papers Presented at the Engineering Foundation Workshop* (UCLA-ENG-7598, University of California, Los Angeles, 1975)]; National Academy of Sciences Forum [*How Safe is Safe? The Design of Policy on Drugs and Food Additives* (National Academy of Sciences-National Research Council, Washington, D.C , 1974)], D. Okrent [*Final Report A General Evaluation Approach to Risk-Benefit for Large Technological Systems and Its Application to Nuclear Power* (UCLA-ENG-7777, University of California, Los Angeles, 1977)].
4. I favor a national energy approach that emphasizes conservation and the wise use of all domestically available resources. Diversity will better ensure resiliency Too much discussion has made it appear that the United States must choose between solar and nuclear energy or between the "soft" or "hard" energy paths. We need solar and nuclear and coal energy.
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9. K. A. Solomon, D. Okrent, M. Rubin, *Earthquake Ordinances for the City of Los Angeles, California: A Brief Case Study* (UCLA-ENG-7765, University of California, Los Angeles, 1977).
10. Earthquake disaster studies have also been prepared for Salt Lake City and the Puget Sound area. Most major U.S. cities are vulnerable to catastrophic damage from an earthquake, although the likelihood is highest in Los Angeles and San Francisco.
11. *LNG Terminal Risk Assessment Study for Los Angeles, California* (SAI-75-614-LJ, Science Applications, Inc., Los Angeles, 1975).
12. D. B. Simons *et al.*, *Flood of 31 July 1976 in Big Thompson Canyon, Colorado* (National Academy of Sciences-National Research Council, Washington, D.C., 1978).
13. G. Morlat, "Un modèle pour certaines décisions médicales," (Cahiers du Séminaire d'Econometrie, Centre Nationale de la Recherche Scientifique, Paris, 1970).
14. C. Sinclair, P. Marstrand, P. Newick, *Innovation and Human Risk: The Evaluation of Human Life and Safety in Relation to Technical Change* (Centre for the Study of Industrial Innovation, London, 1972).
15. "Air quality and stationary source emission control," prepared for the U.S. Senate Committee on Public Works by the Commission on Natural Resources, National Academy of Engineering, Washington, D.C., 1975.
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17. *Report of the Advisory Committee on the Biological Effects of Ionizing Radiation* (National Academy of Sciences-National Research Council, Washington, D.C., 1977).
18. E. Siddall, *Nuclear Safety in Perspective* (Canatom, Toronto, Ontario, Canada, 1979).
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[illegible] of Risk Decisions

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Technology creates many risks. Determining which risks are acceptable is an important national issue. It pervades major sectors of our economy: In food production we face decisions about pesticides and preservatives; transportation risks are increasingly regulated; and a central issue in energy policy is the controversy over the risks from power plants. Regardless of whether the seriousness of technological risk is only now being recognized, or, alternatively, that the preoccupation with risk and regulations is an overreaction, it is clear that the cost to society of the conflict over accepting technological risks is great. These costs stem from the anxiety suffered by those who are dismayed by the conflicting information about these risks, and from the litigation, misplaced investment, retrofits, and costly delays that result from industry's inability to predict the acceptance of risk by the public.

Risk assessment is growing in importance as a system design tool. The final configuration of all technical systems is the outcome of a common design sequence. The first task of a system designer is the development of a workable basic concept. The second task is reducing the vulnerability of the system to failures of component parts, including human participants. The final task is balancing the benefits and risks of the new system, starting with the internalized economic costs. The external effects have rarely been analyzed, and it is only in recent decades that we have become deeply concerned with this difficult but important part of the design process.

Risks created by technical systems arise either from routine external effects considered acceptable at the time of design, or from abnormal conditions that are not part of the basic design concept and its normal operation. Most abnormal events usually impair or stop the operation of the technical system, and may threaten the operators. The usual external effect is the loss of operational benefits to the users of the output. The major internal consequences of failures are borne by the operating institution. The timely diffusion within the institution of information about such failures usually stimulates rapid modifications to reduce the ratio of failure costs to benefits. Less frequently, a failure results in effects outside the institutional boundary, creating a public risk—and a potential cost to the public. These external costs are usually difficult to evaluate, and here the informational mechanism for system modification is usually cumbersome and slow. In recent years such modifications have been made because of an increasing public concern over the inherent risks and costs arising from previously acceptable external effects, both occasional and routine. For these reasons, the importance of risk as a design criterion is increasing.

The basic truisms about risk are readily recognized. First, everyone knows that risk taking is an accepted part of life. Living can be fun, but it is also dangerous (just how dangerous can be difficult to measure). Second, everyone reacts differently to risks taken voluntarily and to risks that are imposed by some outside group. Third, decisions imposing risks on us are being made all the time. This results in the fourth truism: a conflict is inherent when a group imposes a risk on others. Historically, such conflicts have been resolved by compromise, but rarely to everyone's satisfaction.

It is, therefore, characteristic of the functioning of an organized society that conflicts arise from the balancing of public benefits and involuntary risks to the individual. Because such conflict is unavoidable, our problem is how to manage and minimize it.

How should group decision processes operate to minimize social costs and maximize social benefits? Group processes range from anarchy to dictatorship. In most of the industrial world, we enjoy a medium between these, but the processes for decision-making have themselves become contentious issues.

Social costs include intangibles, and the question immediately evident is what costs are included and how are they weighted. It is obvious that if we have a decision process, and if we know how to determine costs to the individual, we still have a problem with the full disclosure of all the social costs. What is full disclosure? Do we include the options for societal risk management as part of full

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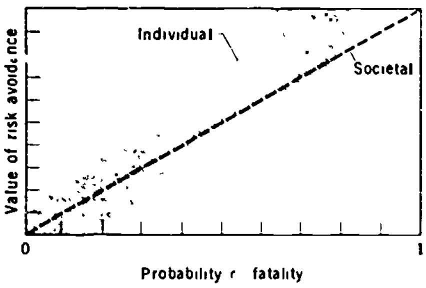


Fig. 1. Value systems for risk.

disclosure: that is, the cost of the alternatives for managing the risk? Does it include all present events, future events, the people who get the benefits, and the people who bear the costs? We have geographic distributions, time distributions, demographic distributions—all of these are included by the term full disclosure. Where do we draw the boundaries?

Decisions are not made by institutions; the decision process involves people. The government typically works through agencies and committees, so that, in fact, it is a few people in the agencies and a few people on the committees who really decide what happens. How do we allocate the responsibility and the costs of bad decisions? How do we functionally connect authority, responsibility for outcomes, and costs?

After we establish the social costs, how do we set out priorities? How do we determine the relative merits of various outcomes? That is a subject for a separate study, because, of course, value systems depend on culture, background, economic status, and all kinds of psychological factors.

Part of the problem in risk assessment comes from confusions that arise during discussions of the subject—confusions about reality, analysis, and individual perceptions. Reality is what has happened or what will happen. Analysis is a process based on collected data, anecdotal cases, and statistics, any of which may or may not be correct; and, based on these, we invent simplified models to predict an outcome. The result, of course, is a large uncertainty in the predictions.

What is the intuitive perception of the individuals involved? Involuntary risks are perceived differently by individuals. Their perceptions may be far from reality. So, in discussing public acceptance of risk, we have to distinguish between the uncertain reality of what may occur, the uncertain analysis of predicting it, and the variable perception of its potential. Similar confusions exist, incidentally, over social costs and social benefits, which are also involved. As an illustration, who in the year 1900 could have predicted the social costs and benefits of the automobile?

Finally, people's perceptions of probabilities are frequently in gross error. The accident at Three Mile Island proved very little about probabilities of such events. The inadequacy of such single events for providing probability numbers can be explained analytically, but the political response and the public perceptions are often based on single events. So even if a professional group develops analytic answers, it has difficulty persuading the public to accept them.

Recognizing all these difficulties, it is nevertheless important to explore the subject of risk management in order to improve the quality of decision-making.

Analytic and Judgmental Approaches

A question implicit in the term acceptable risk is "acceptable to whom?" Certainly congressional approval of any method for making risk-benefit decisions establishes its legitimacy, but a public consensus is needed to sustain its use. Defining this consensus is difficult because there are technologies that are favored by a majority, or at least by a plurality, but are opposed by extremely motivated individuals and groups (for example, those who fight water fluoridation and nuclear power). Because of our experience with other political issues in which similar divisions of public opinion occur (abortion, gun control), we know that we should not be optimistic over the prospects that a regulatory approach can neutralize these controversies. Problems such as these raise issues, such as the definition of majority versus minority rights and the scope and limit of due process, that are well beyond those normally associated with risk management.

Congress has not defined "acceptable" risk levels, except for the few cases in which a zero risk approach was mandated. Far more frequently (1), Congress delegates responsibility for judging risk acceptability to regulatory agencies with the criteria that protection be provided against "unreasonable" risks. The methods by which these agencies interpret "reasonableness" range from a formal analysis of risk, benefits, and alternatives to purely subjective evaluations.

Analytic Approaches

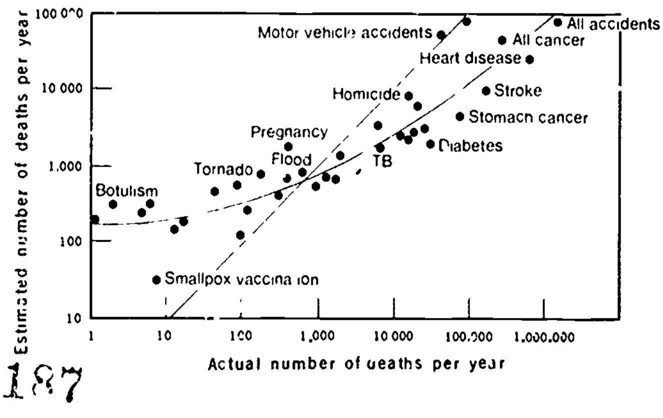
The attraction of analytic methods (cost-benefit analysis, decision analysis) is their capacity to make explicit the assumptions, value judgments, and criteria used for making a decision. The analytic approaches are considered logically sound and sufficiently flexible to accept any value system. Given a specific set of values and criteria, a cost-benefit analysis could ideally indicate the decisions that would best balance technological risk and benefit (assuming that both tangible and intangible costs and benefits are included). But in reality it is difficult to measure group values, and at best the analytic methods can only be used to reach a rough approximation of the social cost and benefits that characterize a decision.

The debate over the relative merits of these approaches generally focuses on the effects of incomplete information (omitted and uncertain risks, benefits, and values), neglect of distributional effects, and other errors of simplification. It is not our intent to review the merits of these methods as commonly practiced; that has been done elsewhere (2–6).

Physical Versus Financial Risk

Because of our use of the term risk as the probability of either financial or physical damage, we may tend to uncritically allow the use of premises about the acceptance of the risk to "life and limb" (7) to be based on an analogy to financial risk taking.

Fig. 2. Comparison of perceived risk with actual risk. [Courtesy of the American Psychological Association]



From the societal viewpoint, the presumption that risk equals cost may be valid in most cases. For example, the cost of the risk of death is sometimes calculated as being equal to the discounted net earnings of those killed. This method, now out of favor, operates as if the loss of lives were equivalent to the breakdown of productive machines.

Similarly, the value assigned to resilience (*8*) leads to a desire to avoid catastrophic accidents that parallels the strategy in which investments are diversified in order to limit losses under adverse conditions. Perhaps recognizing the differences between these two types of risk, Zeckhauser (*9*) argued that, on a per fatality basis, the social cost of multiple-fatality accidents is lower than that of a single fatality because fewer survivors are affected. For example, the social cost of the loss of a city or a family is less than that of an equivalent number of independent, dispersed fatalities. Although the basis for this argument is apparent, it is also incomplete. For example, it ignores the value placed on the continuation of a family line: the importance of this value is evident in the draft deferment that was given to sole surviving sons. Similarly, Wilson (*10*) noted,

> Small accidents throughout the world kill about 2 million people each year, or 4 billion people in 2000 years. This is "acceptable" in the sense that society will continue to exist, since births continually replace the deaths. But if a single accident were to kill 4 billion people, that is, the population of the whole world, society could not recover. This would be unacceptable even if it only happened once in 2000 years.

Another example in which the ability to generalize from financial cost-benefit analysis has been questioned is when physical risks are distributed across time. Arrow (*11*) argued that these risks should be treated as other costs, and discounted accordingly; other analysts of the issue have questioned the validity of this approach, and looked for alternative methods for guidance on how to judge equity in intergenerational risk trade-offs (*12*).

Application of Expected Value to Individual Risk Assessment

Although the above analogy may be valid for the collective view of the cost of risk, it may not apply to the intuitive evaluation of risk. As Fig. 1 illustrates, the individual and societal evaluations of risk are quite different (*8*). In the societal view, the presumption of a linear relation between risk and the cost of that risk may be quite valid. But as Howard (*13*) pointed out, the individual evaluation of that cost is necessarily nonlinear, and becomes infinite as the probability approaches unity.

The use of an expected value or expected utility model is based on the premise that expected cost is simply the product of the probability of an outcome and the evaluation of that outcome. But in the individual's view of the risk of death, this is not valid, for this product is very large or infinite.

It is often presumed that the individual evaluation of the cost of risk is linear over a probability range of interest (*9, 13*), but there is little firm evidence to support any hypothesis about the shape of this curve, as far as we know. Under the common conventions of risk analysis, the slope of the curves in Fig. 1 is referred to as the value of life. The politicians' old saw that life is of infinite value can be reconciled if this refers to the individual evaluation of one's own life. This viewpoint is not inconsistent with the assignment of finite costs to risks. it is the application of an expected value model that is inappropriate for this evaluation.

A second drawback with the application of expected value to individual risk evaluation stems from the tendency by analysts to seek to accommodate differences of opinion entirely within the assignment of utility (*14*). This is because in most decision or cost-benefit analyses, the probability estimates are considered roughly valid because they are based on available data, engineering models (such as fault trees), and expert opinion. But in a study of public attitudes about nuclear power, the bulk of the disagreement was found to be due to different beliefs about accident probability (*15*). Although it may be perfectly valid to base public policy on expert estimates and data, the attempt to reconcile differences in the assignment of costs and values is misdirected if, in fact, the controversy over technological risk is due to divergent beliefs about probability.

Intuitive Versus Analytical Risk Assessment

We now consider the implications of the premise that risk acceptance is ultimately inseparable from the psychology of risk perception and evaluation. A corollary to this premise is the assumption that when the results of intuitive risk assessments differ significantly from those of the analytical methods, conflict follows.

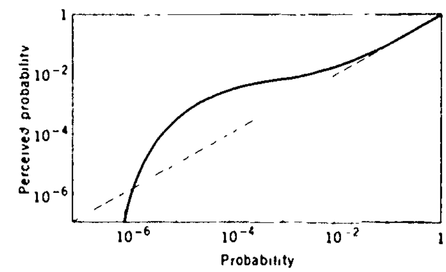


Fig. 3. Perception of probability.

It seems clear that intuitive and quantitative risk-benefit assessments can produce quite different results, even given the capacity of the analytical approaches to accommodate complex values relating to different risk attributes. The differences of opinion over probability assignments are not limited to those risks for which data are not available; many people intuitively fear travel by airplane more than by automobile, yet aviation is safer. Explanations of this effect focus on the degree of individual control over risk (*16*), the conditional probability of survival given an accident, and the catastrophic nature of airplane accidents (*17*).

The difficulty that arises from these differences in assessment stems from the dual meaning of acceptable risk. The analytical methods help regulators set standards that implicitly define acceptable risk. But the intuitive individual assessments of acceptability can overrule these decisions through the political process. The repeal of the seat belt interlock regulation and recent congressional action to prevent a ban on saccharin are cases in which public opinion resulted in a policy change.

Intuitive Risk-Benefit Analysis

Given the role of individual judgments of (physical) risk and benefit in determining the political acceptability of specific technologies, it seems particularly valuable to try to understand intuitive risk-benefit analysis. Efforts to develop this understanding were made by Starr (*8, 18, 19*), whose approach was based on a study of historically accepted risk (revealed preferences) and by Fischhoff *et al.* (*17*) and Slovic *et al.* (*20*), whose approach was usually based on risk-taking behavior as determined by questionnaire (expressed preferences). An additional source of information is the study by Lawless (*21*) of many controversies over technology. If we assume that many of these controversies arose because of intuitive estimates of unrea-

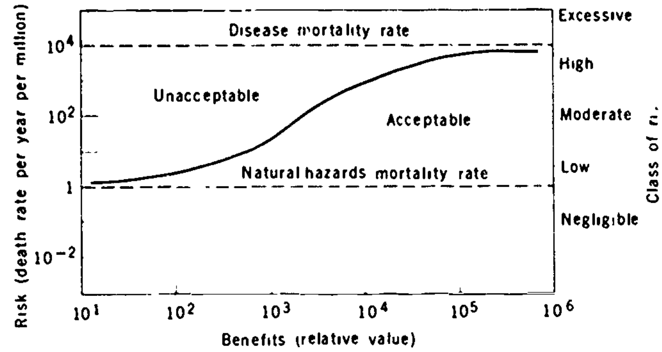


Fig. 4. Risk-benefit pattern for involuntary exposure.

sonably high risk (not true in all the cases described; some cases, such as the thalidomide tragedy, were due to late identification of a risk), then the common characteristics of risk and benefit in these controversies may indicate important factors in the intuitive risk process. Lawless did this, and his findings confirm those of other studies in identifying catastrophic potential and lack of individual control over risk as "factors that influence the impact of the threat."

Understanding the Intuitive Process

There is an attraction to try to develop an understanding of intuitive risk-benefit decisions by constructing parallels to the analytical methods. This approach leads to a model of intuitive decision-making in which subjective judgments of the probability and consequence of undesirable outcomes are somehow combined to produce a perceived risk; parallel judgments provide a perceived benefit; the two are then compared to provide intuitive judgment of acceptability.

This model is quite broad; it does not specify the intuitive procedures for arriving at either perceived risk or benefit, or for their comparison. Even so, the available evidence suggests that this model may be incorrect. First, studies of intuitive decision-making in general (not limited or applicable to physical risks alone), have identified numerous decision-making rules that do not follow the model described above (*22*). Second, there is evidence to indicate that benefits are not intuitively evaluated independently from risks.

In the survey of subjective risk and benefit by Fischhoff *et al.* (*17*), perceived risk and perceived benefit were negatively correlated, due principally to the subjective evaluation of a number of things as high in risk and low in benefit (handguns, cigarettes, motorcycles, alcoholic beverages, nuclear power). When subjects were asked to judge "the socially acceptable level of risk," those who first took the benefits into consideration consistently reported higher levels of acceptability than did subjects who first evaluated risk, which reinforces the view that risks and benefits are not evaluated independently.

Despite the limitations of the perceived risk-perceived benefit view of deciding risk acceptability, we know of no better way to attempt to understand the intuitive processes for risk decisions. Support for this approach stems from the fact that the acceptability of a risk has been found to increase with increasing benefit both by Starr (*18*) and Fischhoff *et al.* (*17*).

Benefits

Little work has been done to characterize the perceived benefits of technological activities. Starr (*18*) found a correlation between risk and benefit awareness," which he described as a crude measure of public awareness of social benefits. This measure was based on the relative level of advertising, the percentage of the population involved in the activity, and a subjective judgment of the usefulness of the activity. The survey by Fischhoff *et al.* (*17*) included a subjective ranking of benefits, but no attempts were made to relate perceived benefit with any characteristics of that benefit.

Probability Perception

By far the most studied and best understood component of intuitive risk-benefit analysis is risk perception and evaluation. There is excellent literature on the subjective estimation of probability (*23*).

One aspect of the interpretation of probability that has been noted repeatedly is the intuitive handling of very low probabilities. As Mishan (7) noted: "One chance in 50,000 of winning a lottery, or of having one's house burned down, seems a better chance, or greater risk, than it actually is." The same observation was made by Selvidge (*24*). Lichtenstein *et al.* (*25*) found similar results (Fig. 2) when they asked people to estimate the number of fatalities from specific causes annually in the United States: "The full range of perceived risk is only about 10,000 while the corresponding actual range is closer to 1,000,000." Similar results were found in another survey in which risk was ranked subjectively (*17*).

The influence of this perception is important when we recall that the expected value or expected utility model calculates that a change in event probability by a factor of 1000 produces a change in expected value or utility by a like amount. If the probability is perceived as having changed by a much smaller amount, then it would not be surprising to find that an intuitive evaluation of risk is less sensitive to probability changes. This can be extremely important for low-probability, high-consequence risks, because probabilities lying below an intuitively understandable range may be overestimated.

We postulate that this is only true to a point (see Fig. 3). Although we selected these scales judgmentally, their chief purpose is to illustrate that, at some low level of probability, the intuitive interpretation goes from "low" or "unlikely" to "negligible" or "impossible." This hypothesis can be used to explain behavior regarding seat belt use and perhaps smoking. In a study of seat belt use, Slovic *et al.* (*26*) noted that if the decision to wear seat belts is approached on a per trip basis, "we might expect that many motorists would find it irrational to bear the costs (however slight) of buckling up in return for partial protection against an overwhelmingly unlikely accident." They observed that "change of perspective, towards consideration of risks faced during a lifetime of driving, may increase the perceived probabilities of injury and death and, therefore, induce more people to wear seat belts. . . . Such differing perspectives may trigger much of the conflict and mutual frustration between public officials and motorists, each believing (with some justice) that their analysis of the situation is correct.' Similarly, Jacobson (*27*) referred to carcinogenic "chemicals which pose minuscule hazards to individuals, but significant hazards to the population as a whole." This last point supports our premise that much conflict over technological risk is due to differences between intuitive and analytical risk-benefit analyses. If the hypothesis that perceived probability is effectively zero for some risks is valid, then the perceived risk of a short automobile trip without seat belts or of one cigarette may be zero.

This nonlinearity in probability perception indicates that even something apparently as basic as the unit of exposure used to evaluate risk can be influential. In his analysis, Starr (*18*) commented, "The hour-of-exposure unit was chosen because it was deemed more closely related to an individual's intuitive process in choosing an activity than a year of exposure would be."

Accepting, at least tentatively, the relation between perceived and actual probability (Fig. 3), we can see a basis for the controversy over catastrophic risks. As mentioned above, high-consequence, low-probability risks are of particular concern if their probabilities are overestimated subjectively. But when part of the public believes the probability is low, and another part believes it to be negligible, these beliefs lead to radically different evaluations. This may be the case with nuclear power and other risks of this type, and may be a key reason for the controversies over these risks.

Risks Distr[illegible] ver Time

Given the [illegible]rent nonlinearities in risk evaluation depending on the unit of measurement, it seems reasonable to look for other perceptual factors related to the units in which risks are expressed. A number of distinctions can be considered: risks can be immediate or delayed, cumulative or ephemeral, and can affect future generations or our own or both. There is little evidence to indicate how these factors are handled. Fuchs (*28*) cited evidence that individual discount rates for financial and physical risk are positively correlated. But the fact remains that benefits and risk may be discounted at different rates. For decisions with very long-term implications, the use of a variable discount rate, declining with time, may more accurately reflect the value given to future risks and benefits than a constant discount rate (*29*). This is an area that seems particularly worthy of attention, for many risk controversies are about risks that are persistent or cumulative, such as carcinogens.

Predicting Risk Controversies

Because of the work to define the factors influencing perceived risk, it is now possible to anticipate the kinds of risk likely to generate controversy. Catastrophic potential and lack of individual control, particularly once an accident occurs or a risk is identified, are apparently the most important risk characteristics. When the uncertainty associated with risks is great, data concerning the uncertainty not forthcoming, and expert opinion apparently divided, apprehension by the public is understandable. Haefele (*30*) termed these risks hypothetical, and described nuclear power as the "pathfinder" for these risks. Certainly there are many risks with the characteristics described above (for example, toxic chemicals and recombinant DNA research). Whether decisions can be made about these risks without the high degree of controversy and the resulting high social cost associated with the nuclear debate remains to be seen.

Quantitative Criteria for Risk Acceptance

In May 1979, the Advisory Committee on Reactor Safeguards (ACRS) recommended "that consideration be given by the Nuclear Regulatory Commission [NRC] to the establishment of quantitative safety goals for overall safety of nuclear power reactors" (*31*). The ACRS further recommended that "Congress be asked to express its views on the suitability of such goals and criteria in relation to other relevant aspects of our technological society. . . ." A similar suggestion, accompanied by proposed criteria, was made by Farmer (*32*) in 1967; the criteria were expressed by a curve relating acceptable accident frequency with accident magnitude. Subsequent proposed criteria for acceptable risks, not necessarily limited to nuclear power, have been made by Starr (*18*), Bowen (*33*), Rowe (*34*), Okrent and Whipple (*35*), Wilson (*36*), and Comar (*37*). Currently efforts are under way within the NRC, the ACRS, and elsewhere to develop quantitative criteria for risk acceptance and to consider the many issues raised by this approach.

Incentives to Develop Quantitative Criteria for Acceptable Risks

The dissatisfaction with current regulatory systems for risk management provides impetus to develop new methods. Theoretically, quantitative criteria for acceptability would resolve many specific criticisms. One criticism stems from the fact that in several cases, a zero-risk goal has been established. This denies the concept of a trade-off between risk and benefit, and ignores the difficulty or impossibility of reaching zero risk. Further, improvements in technology have permitted identification and estimation of risk at levels far below those that were possible when specific zero-risk laws were passed; risks we might consider negligible are not treated in the regulatory process differently from much higher risks. As Hutt (*38*) argued,

> Until quite recently, a no-risk food safety policy was widely thought to be an achievable goal. . . It is now clear that it is literally impossible to eliminate all carcinogens from our food. Moreover, many of the substances which pose a potential risk are part of long-accepted components of food, and any attempt to prohibit their use would raise the most serious questions both of practicality in implementation and of individual free choice in the marketplace.

A suggested way of handling this problem would be to set a level below which risks would be ignored, provided some benefit were associated with the risk. This low level would serve as a quantitative standard for acceptability of the risk.

A second criticism of regulatory approaches is that decisions are often made arbitrarily. Such a charge is not surprising considering that several regulatory agencies have a mandate to protect the public from "unreasonable" risk, without congressional guidance on how to judge reasonableness. The objections are enhanced when regulators are believed to be overly accommodating or hostile to the regulated industry. Certainly, one way to reduce the influence of bias and arbitrariness is to institute a numerical definition of "reasonable." Perhaps the time required for risk decisions would also be reduced by the availability of clear, relatively simple criteria.

Often, regulatory authorities specify the technology for meeting risk targets, rather than the targets themselves. The drawback of this approach is that there are no incentives to develop more efficient methods of controlling risk. The establishment of risk targets alone could stimulate the development of a variety of creative methods or risk control.

Finally, another criticism of current risk management is that the effort required to control risk (as measured by the cost per life saved) varies considerably from one risk to another; this wastes both lives and money (*9*). Assuming that the total funds allocated for risk reduction could be transferred freely between different risk reduction opportunities (which is certainly not always possible), the maximum number of lives that could be saved nationally is found when the marginal co of saving a life is uniform among the opportunities. Thus the comparative marginal cost-effectiveness of each opportunity for saving lives would become the guiding principle in the allocation of resources, and the value of life would be implicit in the total national allocation of funds. There would, of course, need to be a national allocation of resources to such "life-saving" endeavors, but as with military budgets, a common-sense consensus judgment is likely to be as reliable as any analytic formula.

Applications for Risk Criteria

One of the pitfalls in trying to develop regulatory approaches for managing risk is the desire to use the same method to tackle a number of different risks. There are different types of risk decisions, and no single regulatory method seems applicable to all of them.

The use of cost-effectiveness criteria serves as an example. This issue arises when, a priori, the technology is found acceptable but the specific operating point is left to be decided. An example of this type of decision is the determination of allowable levels of a pollutant in automobile exhaust. In this case the issue involved is not the relative risk and benefit of transportation, nor the selection of a transportation technology (automobile versus mass transit). For this simplified type of decision the only issue is the marginal trade-off between the social cost of the risk and the cost of controlling it. For these cases, two kinds of quantitative criteria can be considered; the first is the standard for judging cost-effectiveness described above. There is nothing new in this approach; it is simply cost-benefit analysis in which the metric for judging the social cost of risk has been specified. The second quantitative criterion is more pragmatic: it is a lower risk limit below which no regulatory action would be taken. This could be useful in allocating a regulator's time and would help prevent the highly visible cases in which the nuisance aspects of regulation are intuitively greater than the benefits of that regulation.

The next level of difficulty in risk decisions is the choice of the best method for obtaining a specific benefit. In these cases the benefits need not be analyzed, it is presumed that the benefits are sufficiently great to justify any of several alternatives. For example, in the often heated debates over the selection of energy production technologies, it is generally assumed that, under any proposed policy, energy services will be provided (such services include conservation). For this decision, the dominant issues are the costs and risks associated with each alternative. The difficulty in making these choices is often due to the qualitatively dissimilar character of the risks (for example, air pollution risks from coal mining and burning versus nuclear reactor accident risks). It is difficult to see a role for quantitative criteria in making comparisons of the type needed. One could establish a maximum permissible risk level that would serve to screen out excessively risky alternatives, but the selection of a technology generally depends on some aggregation, either explicitly or implicitly, of the components of the social cost of each alternative. Presumably, after one alternative is selected, the decision is reduced to the determination of the preferred operating point discussed above.

A complete risk-benefit decision requires that the relative social cost of the risk be compared with the associated benefit. A pragmatic application of quantitative criteria for these cases was suggested by Starr (*19*) and by Starr *et al.* (*8*), and is illustrated in Fig. 4. This risk-benefit curve reveals the commonly proposed characteristics for risk criteria: a lower limit for concern about risk (in this case, the natural-hazards mortality rate), an upper limit for acceptability (set by the average disease rate), and provision for risk-benefit trade-off between these limits.

We should not overestimate the capacity of simple criteria, such as those illustrated in Fig. 4, to reduce risk conflict costs. Many, if not most, risk estimates include significant judgmental inputs. There are often substantial disagreements over risk estimates, the methods used to arrive at risk estimates, and the competency, integrity, and motivation of the experts providing subjective risk estimates. What is needed for the application of quantitative criteria for risk acceptance is a standard of proof for determining whether the criteria have been met. Although many different approaches to this issue have been recommended (including peer review, scientific courts, and quantitative methods for resolving differences between experts), the ultimate responsibility for judging the competency of risk analysis still resides with the regulatory agency responsible for managing the specific risk.

Conclusion

Analytical approaches to decide risk-benefit issues ideally come closer to maximizing net social benefits than any other approach. The usefulness of these methods in making assumptions and values explicit justifies their application. But a necessary condition for applying their results to specific decisions is a social consensus on the relative benefits and costs of the proposed actions. For specific types of risk, in which intuitive evaluations of risk and benefit contradict analytical evaluations, the necessary consensus may not develop, but rather a conflict requiring political resolution is likely to result.

When the conflict arises from a disagreement over the level of risk rather than the value assigned to that risk, efforts to reduce the cost of conflict by incorporating values into an expected utility approach will be unsuccessful. Quantitative risk criteria appear quite attractive in this respect, because the key to the acceptability of a technology under the proposed method is the level of risk. Assuming that the estimated risk became the central point in the debate, the public might have more confidence in the regulatory system if their concern were directly addressed.

We see significant value in trying to understand the intuitive risk-benefit process. The evaluation of its outcome could reduce anxiety and cost if used as a tool in the design of technical systems. This is already the case, as when we use more stringent criteria for nuclear power and commercial aviation than for a more commonplace risk (*39*, *40*). The balance between individual and group risk-benefit decision methods is fundamental to the development of national policies on risk acceptance. It is customarily achieved through the political process, and is not amenable to quantitative analysis.

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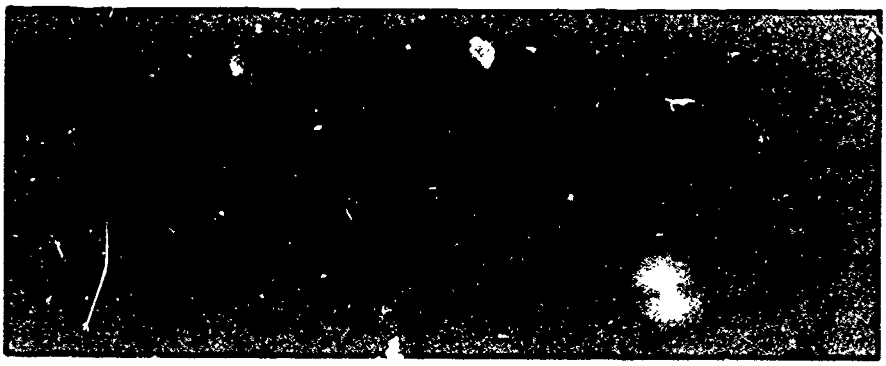

Success in satisfying the requirements and aspirations of the American citizenry depends greatly on the wise employment of advancing science and technology. The potential gains from proper use of these tools include reduced costs of production, the discovery of new resources and invention of substitutes for those in shrinking supply, and the design of new products whose manufacture would create needed jobs. Unfortunately, there is a deterrent to our full realization of the fruits of technology. It is that technological activities produce negatives along with positives. Build any machine or set up any process and, along with the benefits, detrimental consequences also may result. Appreciation of possible disbenefits is now so widespread that government regulation of technological activities is a permanent policy, even if in practice it is an ambiguous one, difficult to implement.

Critics of present technological regulation abound. They complain that the regulation often does not provide needed, minimum protection; over-regulation is frequent; Congress has created bad regulatory legislation; the courts are called upon to do what they cannot and should not be asked to do; agencies sometimes have conflicts of interest; regulators often make inadequate investigations and stall to play it safe; value judgments are confused with economic or scientific factors, an unintegrated hodgepodge of disconnected decisions dominates; balanced decisions, with the risks and benefits of all alternatives compared, are rarely made. Whether these criticisms are justified is itself a value judgment, my own being that all have considerable validity.

In this article I will discuss the nation's present pattern of regulating technology-based activities, arguing that it is overly beset with shortcomings. I will propose a new approach which I believe merits consideration for two reasons: (i) it satisfies some of the criteria fundamental to any more satisfactory system, and (ii) it constitutes beginning theoretical support for the belief that superior systems are inventable.

Difficulty of Technological Regulation

Before considering the shortcomings of present regulatory policy, it is essential to recognize the inherent difficulty of technological regulation. To begin with, defining accurately what hazards are tolerable is essentially impossible. The unwanted ills conceivably present are too numerous and not always quantifiable. Even if for every activity we could measure every possible menace, we would not learn thereby what threshold level of impairment is acceptable. What we define as tolerable must depend on how much we are willing to risk losing. What degree of lowering of our life expectancies or vigor or joy of natural surroundings are we willing to countenance? Since people differ in value judgments even if they agree on the facts, how can we specify the limit of harm? Should we merely insist that the disbenefit be negligible, how shall we define negligible?

Just as identifying detriments is a beginning to sound regulation, so is a listing of rewards. How much of a gain should we insist on before we are willing to accept a given risk? Industry's dollar costs of meeting regulations are eventually paid by all of us, and most such costs can be estimated. But we cannot readily put economic worths on improvements in health or prevention of accidents. No marketplace sets a price for an extra year of life or a month's supply of breathable air. Knowing that the decision-making required of us involves listing the pluses and minuses for each alternative, the lists perpetually incomplete, the items often unmeasured, we sit in the ridiculous position of pitting one alternative against another. How are we to balance the gains against the risks with limited knowledge of each and no clear weighing

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scale? Despite this quandary, we have created regulatory agencies to limit hazards.

Has regulation protected us from serious harm? Tens of thousands of chemicals are being manufactured and a thousand new ones are added every year. Billions of pounds of some are produced annually. Analyses have suggested that more of these substances might be hazardous than we have recognized. Before ethylene dichloride was found to be a strong carcinogen, 100 billion pounds of it was produced. Vinyl chloride reached a rate of 5 billion pounds a year before tests showed it to cause cancer. A 1975 report of the National Academy of Sciences stated that 1 billion pounds of toxic matter was being introduced yearly for pest control and that the government's knowledge of the potential harm was superficial. The Environmental Protection Agency (EPA) has completed examination of only a token part of the 50,000 chemicals for which testing is required by the Toxic Substances Control Act. Uranium mining has claimed the lives of miners because of radon gas–induced lung cancer.

In principle, most cancer of environmental origin should be preventable. One tough problem in identifying environmental carcinogens is that it may take 25 years before the influences are felt. This is true not only of many industrial chemicals but of low levels of radiation, radium, coal mine environments, asbestos, and others. Another problem is in instituting practical controls even when we have positively identified hazards—for example, smoking (lung cancer), fat in the diet (colon and breast cancer), and charcoal-broiled steaks (containing charred protein, a mutagen).

Clearly, the investigatory task of identifying and measuring hazards is an enormous one. Regulation is unsatisfactory partly because we have not faced up to the science and technology part of the task. Usually the technical experts and laboratory facilities required are more than the agencies are in a position to assemble. Inadequate budgets and inspection powers often limit the making of studies leading to good regulations and the policing needed to ensure adherence to them. Restricted in investigatory capability, but anxious to protect against hazards, agencies sometimes hold back on approvals. Delays may curb harmful effects of new developments but also may deny us benefits. Regulation frequently involves voluminous, costly documentation on minor issues and long negotiations, the required industrial bureaucracy matching the government's.

Meanwhile, regulatory action sometimes appears to have results exactly opposite to those intended. For instance, the current clean air offset requirement mandates that "old pollution" has to be cut down before superior plants can be built in the same area. Since it is not always practical to make an old plant pollute less, this rule means that up-to-date, efficient, low-pollution plants are discriminated against in an established region and will not be created there, while old plants that pollute heavily are allowed to remain. This discourages investment in new technologies that underlie cleaner plants.

Two decades ago it took 5 years and $1 million to work an average new drug through the regulatory mill. Today the typical cost is nearer $20 million and the time is approaching 10 years. In this period the rate of new drug introductions by U.S. firms has fallen by 50 percent. Should we be thankful that we are paying the added price in time and money to prevent hasty introductions of bad drugs? Or are we allowing needless suffering and deaths that new good drugs might prevent? We do not know, because no group has the function of answering these questions. During the 1970's, U.S. drug companies increased their annual R & D budgets in foreign countries from under $50 million to over $250 million. Trials with volunteers are permitted by other countries, who view differently the balance between the dangers of new drugs and the values they might provide. If pharmaceutical R & D moves abroad, then foreign countries, not we, will be penalized by the hazards, but they will be the early beneficiaries of the health benefits and financial returns. Perhaps it has worked out that our pattern has afforded us great protection and cost us little in missed gains. If so, that would be largely accidental, because no group is clearly charged with comparing these broad alternatives.

Conflicting Roles of Regulatory Agencies

Can a regulatory agency be an adequate investigator of negatives if it is simultaneously an arranger of a flow of positives? For instance, is the Nuclear Regulatory Commission (NRC) in business partly to see that the nation obtains nuclear energy, or does it exist solely to protect us against the negatives of the nuclear approach? If both, does it not have a conflict of interest? Moreover, is it reasonable to assume that the NRC can balance nuclear reactor hazards against energy needs unless it is assigned the duty and given the means to determine how much energy the nation requires and is expert on alternatives such as coal and solar, on the politically acceptable level of oil imports, and on the potential of more conservation effort? The NRC often has counseled the suppliers and utilities and can claim never to have had to turn down a request for a license. When it also takes upon itself the role of safety expert and the public's protector, the perplexity is natural. Immediately after the Three Mile Island event other utilities operating similar reactors considered closing down temporarily. The NRC was looked to for a decision about a shutdown. Here the mission of the NRC as a protector was understandably perceived by some to be in conflict with its also being a party to providing uninterrupted electric power.

There is an opposite side to this conflict of interest coin. Those wishing to get on with technological activities are frequently frustrated by the negating activities and indecisiveness of government agencies. Sometimes the critics of an agency are politically powerful, and the ultimate effect of their criticism is to cause the agency to depart from its mission of protection and seek to appear to be using a more even approach. In so doing, it compromises its role as a protector. Consider, for example, the political difficulties of the Occupational Safety and Health Administration (OSHA). It was not created either to promote industrial development or to slow it. Industry is disturbed by the costs of meeting OSHA's standards and the large staffs needed to deal with OSHA. The criticism has become so great that OSHA is now on the defensive. It may have to create for itself an image of balanced decision-making or face lower funding and restricted jurisdiction. This handicaps OSHA in fulfilling its mission of worker protection. At the same time, its actions will hardly be viewed by industry as contributing to expansion. Thus, all concerned—workers, industry, and government—will continue to be unhappy with OSHA.

Trade-offs Between Benefits and Harms

Decision-making on technological operations can hardly be sound unless it includes examining alternatives. Also, the decisions, once made, must be enforced. There is no such thing as zero risk, so to seek it can only generate an expensive bureaucracy with no chance of succeeding. Comparing imperfect options and balancing risks and gains, both in arriving at rules and policing adherence to them, is key. Severity in regulation is not necessarily an error on the safe side, because it can also have a negative impact on productivity and employment. It can hurt our ability to compete in the world market, lower return on investment, raise prices, discourage new investment, and decrease average incomes. People who are made poorer suffer from health problems just as surely as do those who are not protected from health hazards.

Starting with the Pure Food and Drug Act of 1906, we have added laws governing therapeutic drugs, cosmetics, medical devices, occupational environments, pesticides, children's sleepwear, automotive safety, nuclear emissions, and pollutants in water and the atmosphere. All this regulatory effort is narrowly focused and disconnected. Effects of specific regulations on other government programs and overall national economic, physical, and social health would enter the deliberations if responsibility for comparing alternatives broadly accompanied government regulation. In some instances agency policies include finding evidence of positive benefits before allowing a new product on the market. However, usually the legislation setting up regulating agencies is silent on defining trade-offs. Congress has actually sometimes forbidden balanced decisions by the agencies and required unbalanced ones. The Clean Air Act specifically precludes the deliberate weighing of benefits against harms. The so-called Delaney Amendment to the Food and Drug Act tells the Food and Drug Administration that it must not consider the cost impact when making regulations.

The trade-off between improving the environment and increasing the energy supply is typical. The economy's being slowed by too low an energy supply is bad; allowing more pollution and accidents is also bad. With no one in charge of balancing positives and negatives, the government has taken several unrelated and conflicting actions. If coal use is expanded, then energy shortages may be eased, but environmental impairment and safety hazards will increase. The government first set a low ceiling price on natural gas, discouraging further exploration and simultaneously increasing demand. In an unrelated act, it then imposed drastic controls on coal. To cut air pollution, it mandated changeovers to oil and gas for utilities using coal. A little later, reacting to actions by the oil-exporting nations, it required greater use of coal. The government introduced strong air pollution restrictions on automobiles without considering the impending oil shortage. The EPA's isolated auto emission rules raised the demand for unleaded gasoline and lowered MPG (miles per gallon) performance. Less gasoline is produced from a barrel of crude in making unleaded fuel, so more refinery capacity was needed. At the same time, new restrictions were placed on refineries. While one agency thus pushed the demand for oil upward, another discouraged the expansion of capacity. Government policy in energy has preached conservation, encouraged dissipation (by keeping conventional fuel prices low), made development of new domestic energy sources through private investment less attractive, then started government-funded programs to pursue new energy alternatives.

The automobile pollution problem is a good example of the need to consider the inevitable impact of a ruling or inaction on the rest of the economy. The automotive industry employs more people, constitutes a higher fraction of our gross national product, uses more materials, consumes more energy, and influences our way of life more than any other industry. Regulations affecting the design of a car have an enormous effect not only on air pollution and accidents but also on unemployment, the national economy, and our international competitiveness. Government controls have much to do with the price of cars. The price influences the rate at which the public shifts from older (lower MPG, more polluting, less safe) cars to more desirable ones. Government actions dominate manufacturers' decisions as to where to put available funds—to meet regulations, enhance productivity, or improve the product. Yet little evidence exists to suggest that federal regulation of the industry has been based on weighing overall national gains and costs.

Seat belts, safety glass, collision-proof door latches, and the energy-absorbent steering column were the first mandatory safety requirements. The regulatory bureaucracy then invented 5-mile-per-hour bumpers, the airbag, and interlocking of seat belts with the ignition. The public vetoed the last two. The new bumpers perhaps reduced repair bills after some accidents, but they cost consumers $1 billion for the adornment and required hundreds of millions of gallons of extra gasoline annually to handle the added weight. It is not evident that any safety benefit has been attained. To this day, we do not know whether auto standards are in the right range, everything considered.

A different kind of example is the Georges Bank project, off the coast of Massachusetts, where the Labrador Current and the Gulf Stream converge and stir up nutrients. The fish catch there over the next 20 years is believed to be worth $3 billion to $4 billion. Geologists estimate that during the same period $10 billion of oil and gas can be obtained from the area. The government is about to sell petroleum leases amidst controversy over potential harm to the fishing. Many agencies are involved and the pattern for setting standards is confused. No one group has the responsibility to compare alternatives.

Delays due to Indecision

The eastern United States has a refinery capacity for less than a quarter of the oil it consumes. No new refinery has been built on the East Coast for more than 20 years, and petroleum products must be shipped from a distance, using energy and adding pollution from its dissipation. Those seeking to locate a new refinery on the East Coast have contested for many years with those striving to prevent it. Involved, in addition to those who would operate the facility, are numerous citizen groups, the Department of Energy, the Department of Interior, the National Oceanic and Atmospheric Administration, the Commerce Department, local government groups, the Army, the General Accounting Office of Congress, the EPA, the Coast Guard, and others. None has decision power.

To secure approval for a pipeline from California to Texas, the Sohio Company spent 5 years obtaining 700 separate permits from regulatory authorities. Then, seeing no end of legal challenges, the company gave up. But perhaps we badly need the pipeline. Who knows, and who is to say?

Another example particularly shows how our decision-making affects us internationally. California competes with Japan for liquefied natural gas (LNG) from South Alaska, Indonesia, Chile, Malaysia, Australia, and other Pacific locations. Anticipating a decline in U.S. gas supplies, California gas companies commenced arrangements 10 years ago for LNG deliveries from sources offering two or three decades of supply, well before the Japanese made similar contracts. The gas started flowing to Japan in 1977. The earliest the United States can now receive this gas is 1983, the period having been used up to get approval on a terminal site for the tankers. Similarly, the Alaskan North Slope gas pipeline project was blessed by the Canadian Prime Minister, the U.S. Congress, and the President in 1977. Many more years will be needed to complete approvals. Perhaps this slowness gives us worthwhile protection. It would be easier to be convinced of that if the decision process appeared so thorough as to require the time for selection of the wisest alternative. It seems instead to be a hodgepodge of fragmented confrontations.

It is interesting to compare Canada and the United States on similar technological projects, Canadian tar sands and U.S. oil shale. Both are huge energy resources but need additional development. Two commercial plants are in operation in Canada, a cooperative effort by government and private industry. There is nothing comparable in the United States. Canada began passing environmental protection legislation years before similar U.S. action, so they are not ignoring the problem, but they seem able to match reasonable environmental protection with desired use of the resources. Permits required in the United States for a single project in oil shale number in the hundreds. One disapproval is enough to halt action. Again, if our procedure results in well-balanced decisions, it will be partly fortuitous.

The Department of Agriculture estimates that if pesticides were banned, crops would decline 30 percent and food prices would rise 75 percent. Millions of people around the world would go hungry because U.S. food would no longer be available to them. Unregulated use of pesticides is unthinkable, but the standards should be based not alone on the dangers of their use but also on the disbenefit of their nonuse. A recently introduced herbicide is said to be environmentally superior to existing ones. It was the result of 20 years of research and approval effort. Should the regulatory process be accelerated, the gains expected to exceed the potential harm of premature approvals? To weigh probable benefits against risks is not now a required regulatory procedure.

Role of the Judiciary

Interested parties now commonly seek appeal from regulations through the courts. Litigation has become so frequent that regulations are often rendered academic, their application requiring the step of winning in court. The U.S. Court of Appeals recently struck down a regulation by OSHA on the handling of benzene. Regulations originating several decades ago limited the allowable molecular concentration of benzene in industrial establishments to 100 parts per million. This was later lowered to 10 parts per million. Then OSHA ruled that the concentration should be decreased to 1 part per million. Would adhering to these more severe standards save 100 lives, or even one life, annually? OSHA had not performed tests to answer such questions; it was going on the assumption that if holding benzene in the air to a low value is good, then reducing it to a lower value must be better. On the other hand, it was quickly ascertained that OSHA's new standard would lead to industry expenditures of more than $500 million. Immediately a value issue arose: some certain and large economic penalties versus some possible, but perhaps totally absent, health benefits. OSHA assumed that industry spending to meet regulations is not to be a criterion when the agency seeks to protect human lives. But surely some price is prohibitive and some of the expected benefits must be measurable, the court decided, ruling that OSHA could not apply the more severe standards. (The case recently was heard by the Supreme Court, which backed up the decision of the Court of Appeals.)

The inadequacies of the regulation process, while making the role of the courts more important, has also caused their function to be less distinct. Industry often complains that the courts unduly delay and interfere with industrial development. Labor and environmentalists argue that the courts defer valid prosecutions by regulatory agencies and are too subservient to industry groups. Accusations are common that judges, without adequate knowledge of the highly technical matters involved, misuse their injunctive power, available at the beck and call of environmentalists at times and of industry groups at other times.

While fundamental questions exist as to the appropriate role of the judiciary in technological regulation, the legislation produced by Congress is in any case a key factor in the frequency and substance of actions before the courts. Congress has set up a new agency almost every time a new harm has surfaced, the empowering legislation occasionally directing the agency to do something bordering on the impossible, such as essentially eliminating a risk. The laws governing the agencies do not tell them whether to tolerate a trivial hazard when the cost of removing it is enormous while banning it may deny us a great benefit. By creating many narrow agencies and ignoring the impacts of an agency's regulations on the rest of the nation's activities, Congress has almost neglected its constitutional role as overall policy-maker and has created a base for isolated, piecemeal, and inconsistent decisions.

No really effective legal foundation for control exists over most regulatory agencies. Each new empire is constructed to be independent of elected officials, those who must answer to the voters. Although Congress has committees to oversee the work of regulatory agencies, these committees seem not to spot over-regulation or under-regulation readily, or an agency's lack of motivation or responsibility to compare alternatives, if that is what Congress intended, or the fact that an agency is inadequately funded to obtain facts essential for sound regulation. It has been estimated that $100 billion to $200 billion per year is spent by industry to meet government regulations, an amount comparable with the nation's annual capital investment or the federal tax revenues from business. Thus Congress could be excused for spending roughly as much time studying regulatory costs, to make sure they are justified, as pondering taxes. With a budget in mind to meet regulations, Congress could apply its own value judgments to the comparison of costs with benefits.

Admittedly, some benefits result when regulatory matters get to the courts. In bringing unresolved matters there, industry, environmentalists, and numerous other government and private groups help bring to the surface a very critical issue in technological regulation: the survival of democratic institutions in a technological age. This is more important than the courts' curbing bureaucratic excesses or catching inadequacy of expertise. If Congress did its job properly, creating an adequate organization to handle technological regulation, the role of the courts would become less cloudy. The courts could then concentrate on being the guarantor of the rights of the individual in a technological democracy.

Present court activity in technological regulation does not have this focus. Reflecting the unsatisfactory state of court actions on technological activities, David Bazelon, Appellate Judge of the Washington, D.C., Circuit Court of Appeals, recently suggested (1) that in cases involving technological and scientific hazards the courts should restrict themselves to the role of watchdog over the expert. The courts should ensure, he wrote, that the experts have fully considered all the evidence, but should refrain from inserting themselves as the final arbiters of complex scientific questions. This may be what, in practice, the judge must do in certain cases, but it does not mean that the courts can shy away when experts testify. Any proposed technological activity must meet the test of justice and fairness to the individual, which every judge swears to uphold, or our concept of democracy will not survive. Society's interests, whether or not an issue is based on technological matters, may occasionally deserve to outweigh an individual's rights. But the presumption underlying the Bill of Rights and the Constitution is still the supremacy of the individual. Historically, it has been the courts' duty to uphold fundamental values on which the majority or the powerful may be seeking to trample. In administering justice, the court is the expert. No matter how we organize technological regulation, society will feel frustration with court decisions much more deeply if the courts fail to articulate and implement basic concepts of justice than if they fall short in ensuring that decisions are scientifically accurate.

This may be all the more evident once it is fully accepted that a decision concerning a technology-based issue is not merely a matter of risk assessment. If it were, we could turn it over to expert calculators. Broad problems arising out of scientific and technological advance are never matters of science and technology alone. Instead, like all other important issues, they are dominated by their social, ethical, and political dimensions (2). Jeremy Bentham, in the early days of the industrial revolution, enunciated a political theory of social policy based on the idea that one could compare the number of people benefited with the number harmed to arrive at an appropriate decision. He hoped that legislation could be based on such a quantitative criterion. (Bentham believed his system would protect the working class since no policy affecting laborers adversely could prevail, the large number harmed exceeding the small number of company owners.) But now it is clear that a program might benefit a large number but cause the death of a few. Is this just according to our ethical and legal standards? In many technology-related issues to come before the courts, the decision may hinge on this point.

Take only one example, the nuclear energy option. It can be given some useful evaluation through cost-effectiveness analysis, but it also involves matters that cannot be judged in terms of numbers. The analysis is likely to disclose only what is good for the majority. What the nuclear route does to a minority may always have to be for the courts to ponder and is definitely not a matter for technical experts alone (3).

A Proposed New Approach

With the foregoing description of the problems of technological regulation in mind, let us consider a regulatory organization that may be superior to present approaches as to timeliness of action, the reaping of benefits from technological advance, protection against hazards, and the minimizing of court actions on items best handled through legislation.

One route to improvement lies in decisively separating two duties of the government in regulation of technology: (i) to investigate and make recommendations concerning negatives and (ii) to balance the good against the bad aspects of various alternatives and make decisions. The proposed approach starts with a competent organization to discover, study, assess, provide recommendations, and present cases regarding all hazards to safety, health, and the environment. To remove conflicts of interest, we would relieve this investigatory unit of any responsibility for considering positives and attempting balanced decisions. Decision-making would not be its business. When it comes to clean air and water, nuclear safety, toxic chemicals, occupational health and safety, purity in food or drugs, and the rest, the group should be equipped with the experts, tools, and budgets needed to track down all the detriments of existing or proposed activities. The operations of the Federal Bureau of Investigation (FBI) are a useful guide. The FBI is an investigatory agency; it investigates crime and finds criminals. It does not try or sentence lawbreakers. It does not decide whether capital punishment is proper or whether jails should punish or rehabilitate. When it finds culprits it turns them and the evidence it has found over to another part of the government.

If we accept the value _ a separate, unambiguous mission to investigate disbenefits, it is sensible to bring all such activities together in one agency. Every potential harm to humans and the environment requires for discovery and evaluation an array of measurement equipment, laboratory facilities, field offices, and expertise in chemistry, physics, biology, engineering, toxicology, statistics, and other disciplines. Efficiency and flexibility of organization would result if the experts were all in one strong unit. No need would exist for Congress continually to perceive a new danger and launch still another agency to handle it.

In all technological activities, some entity presumably always exists, such as a drug manufacturing firm or an electric utility, that wants to move forward with a product or a project. These groups and the proposed technological FBI may often be opposing parties, one interested in the advantages of some activity it wishes to engage in, the other ready to say what detriments the activity will bring. Perhaps these two interested parties will agree that the activity is safe or, conversely, should be held up until identified disbenefits can be diminished. If they do not agree, then a decision board is needed to settle the issue.

We see at this point that the FBI model for our proposed agency to handle technological negatives is inadequate, and that in some respects the Department of Justice, which includes the FBI, offers a more complete analogy. This is because the new agency would be expected to bring actions before the decision board. It would present its evidence of hazards and recommend action by the board to disapprove or mandate the modification of technological activities. In this analogy the agency would resemble the office of the U.S. Attorney, which decides whether to take a case to court and, if it does, then prosecutes it. The party carrying on the technological activity would be analogous to the defendant, and the decision board would be like the judge. (If this analogy is applied crudely to a present regulatory agency, it would say that the agency acts as both the prosecutor and the judge.) It would be hoped that most often the investigatory agency and the technological operator would "settle out of court."

The decision board, unlike the investigatory agency, should have the role of comparing alternatives, balancing the good against the bad, and the duty to connect the case before it to other national interests. It should have the unquestioned responsibility for banning or approving the challenged technological operation. The pluses and minuses of the activity and the alternative for regulating it would be argued as thoroughly as possible in the board's hearings, after which the board would make its decision. While a single agency may be the best way to manage the discovery and evaluation of hazards and the presentation of recommended means to handle them, we should not try to solve all society-technology interactions with one decision board, because of the expected number of cases and the wide array of value judgments to be included.

The decision boards should be regarded as extensions of the Executive Branch—that is, of the President—a conclusion arrived at by focusing on the intended board's missions. We want the boards to (i) constitute a credible and effective representation of the electorate, (ii) integrate the members' values to form criteria for judging the options and use these value judgments for decision-making, and (iii) compare the alternatives, seeking to balance benefits and risks. The President should appoint the members with the consent of the Senate, naming citizens of outstanding competence and character, with staggered, substantial terms of office. This process will cause the boards to be inherently political, as they should be, responsive to the country's goals and priorities, thus fulfilling (i) and (ii). By the congressional legislation setting up each board, by other pertinent legislation affecting the issues the boards will ponder, and by the boards' operating competently, (iii) will be satisfied.

The President, Congress, or the new investigatory agency would be empowered to call on a decision board to handle a question. Other private or public groups or individuals could also request the board's consideration of an issue (*4*). Because the boards would be expected to ponder every case on a broad basis, they would be expected to overlap frequently, one board being assigned the decision responsibility but typically calling on another board for advice. The legislation setting up the boards would provide the first approximation to defining their jurisdictions and would name the activities (such as a nuclear reactor installation) specifically requiring their approval. In case of doubt as to which board should handle a particular issue, the assignment should be made by the President.

As the boards go about their tasks of rendering decisions, the courts will sometimes be sought out by interested parties. If the legislation setting up the boards is competently written, their decisions will be interfered with by the judicial only when they overstep their charter or ignore other pertinent legislation or fail to be just and fair. For example, a board may make a decision it regards as superior to any alternative for the good of the nation, but may overlook an injustice to some citizens caused by that decision. The Constitution and the courts will always be with us, and decisions reached in any way may end up in the courts. But we can do better than to encourage the present trend of relying on litigation to settle most important issues.

The need for the decision boards and the investigatory agency described, or some superior approach, can be summarized by quoting the words of John G. Kemeny, president of Dartmouth College and chairman of the presidential commission on the accident at Three Mile Island (*5*). "Our decision-making process is breaking down. The problem is whether our current political process can handle the complex issues of modern society—highly technical questions of science and technology that also involve value judgments. . . . I am still a believer in democracy, but I think some changes will have to happen in the practice of it. We have to have a forum for effective discussion of highly technological issues, so that there is a clear consensus on what science and technology say about an issue. Then the political process can make the value judgment."

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3. For example, in New Mexico, the source of 50 percent of U.S. reactor uranium, the mining may benefit the majority of Americans but be hazardous to the Navahos who live there (A. Ramo, Los Angeles *Times*, 1 June 1980, part 5, p. 5).
4. This article is focused on federal regulation, but state and local regulation is also important. The latter will be aided by improved federal policy, organization, and performance. To some extent, the lower levels can also emulate the improvement at the federal level.
5. J G Kemeny, *Newsweek*, 19 November 1979.

Risk and Benefit in Environmental Law

Paolo F. Ricci and Lawrence S. Molton

[illegible]

Currently, more than ten federal statutes contain statements about risk and benefit assessment; the process of setting environmental standards increasingly rests on the analysis of risk. The courts have made numerous references to risk-benefit analysis in reviewing the actions of regulatory agencies. Yet the issue of what role risk-benefit assessment should have in energy or environmental policy is neither clear nor settled. One reason is that many of the questions that arise in attempting to evaluate risk are what Weinberg (*1*) has called "questions which can be asked of science and yet which cannot be answered by science . . . they transcend science." Another is that decisions on how to measure or reduce risk are not purely scientific ones, they are policy choices as well, enveloped in the controversy that usually accompanies policy disagreements.

In carrying out this rule-making function, regulatory agencies are frequently required to postulate answers to trans-scientific questions. These include the extrapolation to low doses of results obtained at high doses of a substance; the interpretation of carcinogenic potencies measured in different animal species to infer possible effects on humans; the meaning of benign tumors; the use of epidemiologic, in vivo, and in vitro studies as evidence for establishing health

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effects; the choice of a dose-response model.

Regulatory agencies, such as the Environmental Protection Agency (EPA) and the Occupational Safety and Health Administration (OSHA), promulgate standards that reflect their policies on health risk. In doing so, they must consider the enabling statute, the intent of Congress, and the procedural requirements of the Administrative Procedure Act. The statutes vary greatly in their description of the risk analysis that the agency must perform. Because of the frequent ambiguity of the statutory language and the discretion given to agencies in choosing objectives and methods of implementation, litigation and judicial review are often the ultimate source of regulatory policy.

The statutes have thus created an "uneasy partnership" (*2*) between administrative agencies and the U.S. Courts of Appeal. The courts must scrutinize the procedures used by the agencies in forming regulations and also examine the substance of the agency policy. Agency findings of fact must be supported by evidence in the record. Environmental regulation is a difficult area for courts to oversee because of its technical content, and judges do not agree on the role of the courts in reviewing energy or environmental decisions. The positions of two judicial scholars of administrative law illustrate this. The late Judge Leventhal of the D.C. Circuit Court of Appeals believed that the courts have a "central role of ensuring the principled integration and balanced assessment of both environmental and non-environmental considerations in federal agency decision-making" (*3*). In his view, the decisions taken by the agency are reviewed by competent judges, who apply uniform standards of review, thus limiting the dependence of results on the court that hears the case. A different view was expressed by Judge Bazelon, also of the D.C. Circuit, who stated: "Because substantive review of mathematical and scientific evidence by technically illiterate judges is dangerously unreliable, I continue to believe we will do more to improve administrative decision-making by concentrating our efforts on strengthening administrative procedures" (*4*).

Considering the technical complexity of the assessment of health risks, it is understandable that courts would be cautious about interposing their judgments on these issues. Nevertheless, the last decade has brought closer judicial scrutiny of environmental policy, as courts have required more formal and rigorous administrative proceedings. Under the "hard look" doctrine of administrative law, the agency must analyze the evidence, describe its methodology, and explain the rationale for its decision (*5*).

In this article we discuss the treatment in law of several key issues in risk assessment: the meaning of the 1980 Supreme Court ruling that OSHA must demonstrate that a standard is needed to remedy a "significant risk," the burden of proof of the significance of risk under conditions of scientific uncertainty, and the resolution of the conflict between the desire for accuracy and the need to reduce hazardous exposures. We then review approaches that have been taken in balancing the economic costs against the benefits of risk reduction.

Significance and Risk

In the 1980 Supreme Court ruling in *Industrial Union* v. *American Petroleum Institute* (*6*), the court required OSHA to demonstrate, before it issues a standard, that "it is reasonably necessary and appropriate to remedy a significant risk of material health impairment." Lower courts had recognized that not all risks can be eliminated by regulation; some are too slight (*de minimis*) to be considered.

In a case involving the use of acrylonitrile in beverage containers (*7*), the court interpreted the legal definition of additive as requiring that a substance migrate into food "in more than insignificant amounts." Although the court said the second law of thermodynamics guarantees that some acrylonitrile will enter the beverage, it decided that this fact alone does not make acrylonitrile an additive.

In cases under the Clean Air Act and the Toxic Substances Control Act the courts recognized that the regulatory agency has the power, "inherent in most statutory schemes, to overlook circumstances that in context may fairly be considered *de minimis*" (*8*). However, if the EPA wishes to ignore low levels of a substance, it must find "the concentration at which there are only trivial benefits to be derived from regulation"(*9*).

In previous OSHA cases the lower courts did not apply close scrutiny to agency regulation of uncertain risks. For example, in a 1975 case the court upheld a reduction in the standard for workplace exposure to vinyl chloride from 50 to 1 part per million. It did not request that OSHA calculate the number of cancers to be expected from either exposure level or adopt a model of carcinogenesis; the opinion does not mention the problem of extrapolation from animal tests (*10*). Another court in 1978 upheld the standard for coke oven emissions, acknowledging that no safe level of exposure could be shown (*11*). A strong expression of this approach came in a 1976 opinion by J. Skelly Wright, now Chief Judge of the D.C. Circuit, upholding an EPA regulation restricting lead as a gasoline additive. He said that "a determination of endangerment to public health is necessarily a question of policy that is to be based on an assessment of risks and that should not be bound by either the procedural or the substantive rigor proper for questions of fact" (*12*). The agency was left free to adopt risk-averse regulations.

In contrast, the Supreme Court ruling in *Industrial Union* (*6*) requires OSHA to develop better evidence of the risks of exposure. The Occupational Safety and Health Act calls for safe employment, and the court noted that " 'safe' is not the equivalent of 'risk-free.' There are many activities that we engage in every day—such as driving a car or even breathing city air—that entail some risk . . . nevertheless, few people would consider these activities 'unsafe.' Similarly, a workplace can hardly be considered 'unsafe' unless it threatens the workers with a significant risk of harm."

The court based its decision primarily on economic considerations, that is, a recognition that regulation of low-level exposures is very costly. It pointed out that under OSHA's rules, once a substance was determined by specific evidence to induce cancer in animals, or in humans who experienced extremely high exposures, it must be regulated. Since the National Institute of Occupational Safety and Health (NIOSH) had 2415 substances on its list of suspected carcinogens and OSHA listed 269 of them as carcinogens subject to regulation, following this course "would give OSHA power to impose enormous costs that might produce little, if any, discernible benefit." The court concluded that Congress did not intend to give OSHA such broad power. After a review of the legislative history of the Occupational Safety and Health Act, the court rejected the view that "the mere possibility that some employee somewhere in the country may confront some risk of cancer is a sufficient basis for . . . the expenditure of hundreds of millions of dollars to minimize that risk." It then proceeded further: if OSHA were correct in arguing that it need not characterize a risk as significant, the statute might be unconstitutional as an overly "sweeping delegation of legislative power" (*13*).

Conclusively showing the significance of risks from low-level exposures would require resolution of issues that range from the choice of a dose-response model to the definition of "acceptable" risk. OSHA's rule-making allowed it to avoid this problem; it did not establish a safe exposure level of a substance. OSHA required standards to be set at the lowest level feasible. But this approach incorporated other assumptions about carcinogenesis, such as the no-threshold hypothesis. The extent to which a finding of significance may rest on such assumptions is unclear. The Supreme Court refused to consider what factual findings are necessary to establish significance. In footnotes to its opinion (*6*), it stated that animal studies could support "a conclusion on the significance of the risk" and that epidemiologic evidence, even if insufficient to generate a dose-response model, "would at least be helpful" in deciding whether a risk is significant.

However, with animal studies there are still the questions of extrapolation and interspecies comparison, and appropriate epidemiologic studies may be nonexistent, so the evidence suggested by the court may still not resolve the controversy.

On some occasions, however, OSHA regulates a well-defined risk. The Supreme Court considered what level of known risk is significant by bounding the concept. It noted that a one per billion risk of cancer from chlorinated water would not be significant, while a one per thousand risk of death from inhaling gasoline vapor would be. Somewhere in between lies significance; where that point lies must require case-by-case determination.

Under other statutes, courts confronted with a known risk have often ruled that if the probability or severity of harm is very low, the risk should not be regulated. In nuclear power plant licensing, the courts have noted that there is a small probability of a major accident from a meltdown, but because the chance of such an occurrence is so low, it need not be factored into the environmental impact statement (*14*). Similarly, a nuclear plant exposes some persons to low-level radiation; this guarantees that some risk exists, but it is of such low severity that courts and agencies have found it "clearly acceptable under existing conditions" (*15*). The D.C. Circuit acknowledged that this risk might at some future time be proved severe, but did not find that possibility sufficient to justify refusing a license. As the court added in a later case, "even the absolute certainty of *de minimis* harm might not justify government action . . . whether a particular combination of slight risk and great harm, or great risk and slight harm, constitutes a danger must depend on the facts of each case" (*16*).

Burden of Proof

When the risk is uncertain, what is the burden of proof that an agency must satisfy to demonstrate significance? The law usually requires that a fact or an overall finding (such as civil liability) be supported by a "preponderance of the evidence" or "more likely than not" (51 percent). The normal rule of administrative law is that "it is the proponent of a rule or order who has the burden of proof in administrative proceedings" (*6*). With the exception of the Federal Insecticide, Fungicide, and Rodenticide Act (FIFRA), which places the burden on the registrant to prove that a pesticide is safe, environmental statutes follow this principle. In earlier cases OSHA was not required to demonstrate the significance of a risk. But in the benzene case the Supreme Court held that "the burden was on the agency to show, on the basis of substantial evidence, that it is at least more likely than not that long-term exposure to 10 ppm of benzene presents a significant risk of material health impairment" (*6*).

While this principle applies to the overall finding of significance, it does not apply to the components of the analysis. The basic principles of scientific proof must be followed wherever there is a consensus among scientists. For a specific piece of evidence to be valid, such as the induction of tumors in mice by chemical X ingested under conditions Y, the findings must be replicable and be statistically significant at commonly prescribed levels. A finding of carcinogenesis must be "proven" with 95 percent confidence or greater. However, there are some issues in the measurement of risk about which there is no scientific consensus. Here the agency is held to a lower burden of proof than "a preponderance of the evidence."

The Supreme Court ruling allowed OSHA to adopt conservative assumptions about carcinogenesis "so long as they are supported by a body of reputable scientific thought" (*6*). It also stated that the agency has no duty to calculate the exact probability of harm. Therefore OSHA may regulate carcinogens by estimating the risk, and characterizing it as significant, on the basis of the most conservative dose-response models. Allowing the agency to choose answers to transscientific questions that are not necessarily the most popular or most logical, but are supported by some experts, would allow OSHA to assume significance even where a preponderance of the evidence does not support such a finding.

Factual Accuracy

Risk assessment by regulatory agencies reflects a tension between two basic goals of regulation. McGarity (*17*) has described these as factual accuracy and result orientation. To achieve the first goal, the agency should wait until sufficient data have been accumulated before imposing regulations. The second goal requires agencies to implement policies that Congress considers socially desirable. An agency may choose to endorse a particular result and acknowledge that factual accuracy is impossible, or it may regulate only where it can be accurate. To avoid this choice, an agency faced with a risk of uncertain magnitude may choose to defer regulatory action until more studies are completed that will better define the risk. If the agency has underestimated the risk, delay will prove to have unnecessarily injured some; if it has overestimated it, delay will avert the imposition of excessive costs. In a case where the agency lacked complete evidence of the environmental impact of its action, the D.C. Circuit held that the agency must give "full and careful consideration" to delay, but may proceed if it decides that delay "is outweighed by the benefits of proceeding" (*18*).

When further research is unlikely to produce additional knowledge, delay is inappropriate. Thus a court contemplating the risks of low-level radiation from a nuclear plant refused to consider that the exposure might be proved hazardous because "There is no indication that either possibility could be rendered other than speculative during the foreseeable future" (*18*).

Sometimes delay is inappropriate because Congress has clearly chosen the result-oriented approach in the relevant statute. The EPA must establish ambient air quality standards for pollutants even where the current state of knowledge makes this difficult; it must make do with the best information available (*19*). But in other cases, such as occupational exposure to suspected carcinogens about which little research has been done, it is difficult to decide whether to delay. Thus far, the courts have given no clear guidance.

The Supreme Court appeared to endorse both factual accuracy and result orientation in the benzene case. As described in the opinion, if the agency is explicit in its choice of models, it may attempt to eliminate almost all cancer risks by adopting a "one-hit" theory of carcinogenesis. But the court has also called for stronger evidence, better documentation, and clearer proof that a risk exists; it found the evidence of low-dose benzene carcinogenicity inadequate (*20*).

Balancing

A critical question is the extent to which economic costs should be weighed against the benefits of risk reduction. Statutes dealing with the environment and energy differ in their approach to this issue, and the courts are wrestling with it. There are four types of statutory frameworks, as described below.

1) One class of statutes requires that the agency balance cost and benefits. Some statutes explicitly require cost-benefit analysis. The most important example is the National Environmental Policy Act (NEPA), which mandates "balancing of the environmental costs of a project against its economic and technological benefits"; a numerical cost-benefit analysis is required in cases where other methods provide inadequate detail. More recently, the Outer Continental Shelf Lands Act, amended in 1978, requires offshore drilling to be done with the safest technology, except when "the incremental benefits are clearly insufficient to justify the incremental costs of using such technologies" (*21*). Similarly, FIFRA requires suspension of pesticides when there is "unreasonable risk to man or the environment, taking into account the economic, social, and environmental costs and benefits" of the pesticide. Similar language appears in the Toxic Substances Control Act (*22*).

When the "unreasonable risk" language appears, the courts have imposed balancing as a prerequisite to regulation. Under the Consumer Products Safety Act (CPSA) and the Federal Hazardous Substances Act (FHSA), the courts have held that such language "necessarily involves a balancing test like that familiar in tort law" (*23*). This balancing formula, called "Learned Hand's algebra" after the great jurist of the 1920's, has three components: the burden of the regulations, the probability of harm occurring from the product or conduct at issue, and the severity of the harm if it occurs. A regulation is valid if the severity of the injury, factored by its probability, outweighs the burden of regulation. This allows the courts to make a subjective assessment of the imposition on the consumer.

2) A second approach to balancing costs and benefits appears in the Clean Water Act. The EPA must consider costs, but they are much less central to the decision than under the first approach. In establishing phase I (1977) effluent standards, the agency must consider the total cost of standards, including potential unemployment and dislocation. It need not make a quantitative comparison of cost and benefit, and it is to impose the standard unless the marginal level of effluent reduction is "wholly out of proportion" to the cost. For phase II (1987) standards, the total cost need not be compared to benefit, but only considered. One court of appeal has required a cost-effectiveness analysis of alternative strategies to implement phase II controls.

When an individual polluter wishes a variance from the effluent standards, the result is different. The U.S. Supreme Court ruled in 1980 that the economic capability of an individual plant to bear the costs of a phase I standard may not be considered (*24*). But under phase II individual economic hardship will justify a variance. The reasoning is that phase I standards already incorporate costs, because they are calculated on the basis of the best control system now in use; segments of industry that have not attained this level should be required to do so. Individual consideration is appropriate for phase II because such cost analysis has not yet been performed.

3) A third approach is to ignore costs and focus on the issue of health risk. The Delaney clause of the federal Food, Drug, and Cosmetic Act, which provides that no additive can be approved "if it is found to induce cancer," is an example. In theory, once tests demonstrate the carcinogenicity of a substance, no consideration of its benefits or the costs of its removal is relevant; the additive is banned. This approach is used in other statutes, although not with the same clean-cut rejection of balancing. For instance, the Clean Air Act requires the establishment of primary national ambient air quality standards solely as a function of health risk. Considerations of economic or technological infeasibility cannot be used in formulating these standards (*25*).

4) It is the Occupational Safety and Health Act that has been the main focus of the debate over balancing. Regulation under this act raises very difficult questions about the assessment and acceptability of health risk, and the answers to the problems of low-level occupational exposure to toxic substances will influence policies in many other areas. Section 6(b)(5) of the statute provides that standards must assure "to the extent feasible" that "no employee will suffer material impairment of health." The Courts of Appeal had interpreted the word "feasible" to require only that the technology existed and could be installed without destroying the industry. In 1974 the D.C. Circuit ruled that standards were feasible even though they were financially burdensome to the employer and reduced his profit margin; even a standard that bankrupted some individual employers could be feasible (*26*). But in a groundbreaking 1978 ruling on OSHA's benzene standard, the Fifth Circuit endorsed cost-benefit balancing (*27*). It held that the benefits of a standard must bear a "reasonable relationship" to its costs, because the statutory definition of health standards as "processes reasonably necessary to provide safe or healthful employment" implied a balancing of costs and benefits. However, in other contexts the phrase "reasonably necessary" only requires that the agency action bear a rational relationship to the statutory purpose (*28*). In 1979 the D.C. Circuit upheld OSHA's cotton dust standard and held that OSHA need not balance costs and benefits (*29*).

When the benzene case reached the Supreme Court in 1980, the main opinion avoided the issue of balancing. Then in 1981 the court ruled in *American Textile Manufacturers Institute (ATMI)* that balancing was inappropriate under its reading of the legislative history of the Occupational Safety and Health Act. The court held that Congress had performed balancing and intended to place "the 'benefit' of worker health above all other considerations save those making attainment of this 'benefit' unachievable. Any standard based on a balancing of costs and benefits by the Secretary that strikes a different balance than that struck by Congress would be inconsistent with the command set forth in §6(b)(5)" (*30*). The court also held that interpretation of the phrase "reasonably necessary" to require balancing of costs and benefits "would eviscerate the to the extent feasible requirement."

The court ruled that feasibility, not cost-benefit consideration, is the only factor that takes precedence over worker health. It defined feasible as "capable of being done." It refined the definition of economic feasibility, but still left some aspects uncertain. For instance OSHA conducted studies to estimate the cost of complying with the new standards and concluded that the cost would not seriously threaten the textile industry and that the industry would maintain "long-term profitability and competitiveness." The court refused to decide whether a standard that threatens this status is feasible.

Obviously, an analysis of these economic questions requires estimates of costs. The precision needed for these estimates is not certain. In *ATMI*, the cost studies were based on a hypothetical dust standard that was less strict than the one actually adopted; thus the cost estimates were too low. The studies also overestimated the cost by miscalculating the amount of synthetic fibers used. OSHA claimed it could not generate more precise figures unless industry was willing to release proprietary data. It then assumed that the overestimate of cost roughly equaled the underestimate due to the hypothetical standard. The court admitted that a cost estimate based on the actual standard "surely would be preferable," but held that the lower court had the power to accept OSHA's estimate under the circumstances. The court concluded that OSHA "acted reasonably" and that the lower court had not "misapprehended or grossly misapplied" the test for substantial evidence.

The *ATMI* case should resolve the meaning of "to the extent feasible" for this issue, but feasible has a different meaning in other contexts. The feasibility of a standard, as just discussed, is not the same as the feasibility of various methods employed to achieve the standard. Two Courts of Appeal implied that this provision necessitates a cost-effectiveness analysis of possible solutions. Thus the employer might avoid expensive engineering controls by demonstrating that they are not feasible because they are not cost-effective (*31*). In *ATMI* the Supreme Court noted that if two methods that achieved the same reduction of health risk were both feasible, the more burdensome method might not be "reasonably necessary." So it appears that cost-effectiveness is needed for OSHA standards.

The *ATMI* decision also implied that the "reasonably necessary" language might require cost-benefit balancing for other hazards. The feasibility principle of section 6(b)(5) only reflects the intent of Congress to regulate toxic materials as much as possible, it does not necessarily apply to safety or noise standards, for example. Therefore standards in those areas might require "some form of cost-benefit analysis." Again, the court did not decide this issue.

However the problems are resolved, if balancing is to play a role in environmental law, some attempt must be made to value human life and health. Society, either implicitly or explicitly, places a dollar value on the preservation or saving of a life. The most dramatic example is the jury award. Juries make death awards in auto accidents, product liability suits, and medical malpractice cases. Statutory compensation systems such as the federal black lung disease program or state workers' compensation also put a price on injury or death. Legislative decisions to finance programs whose effects are documented also value life because they expect to save a certain number of lives for a certain number of dollars. Examples are mobile cardiac emergency units and drinking water treatment plants (*32*). Human life is not the only aspect that is difficult to evaluate, and balancing under environmental statutes often involves comparing intangible costs and benefits. It is through balancing that the courts attempt to weigh such factors as scenic beauty, preservation of animal life, and quality of life

Prospects for Risk Assessment

There is no doubt that agencies and courts will continue to be troubled by risk questions. The easiest administrative policy—eradication of risk to the greatest extent possible—has been declining in popularity as we have become aware of the finite nature of our resources. The costs of environmental regulation seemed less burdensome at a time when the United States had greater economic advantages. Yet the calculus of risk involves basic values that will always be weighted differently by different individuals. Decision-makers select a policy that implicitly weighs health, quality of life, economic opportunity, and environmental amenities. Consensus is almost impossible, yet we have no alternative but to seek increasingly rational approaches (*33*).

Risk-benefit assessment is still in development. Better methodology and better procedures in the agencies and courts are urgently needed. The distributive effects of regulation (which groups are benefited and burdened by a policy), and the trade-offs between present and future generations should be considered. Recent articles have called for new procedures in policy-making: generic rule-making on transscientific issues, full disclosure of the uncertainties contained in all risk decisions, and the use of permanent special masters to advise appellate courts in these cases (*34*). More accurate and just results may be possible in the future.

References and Notes

1 A Weinberg, *Minerva* 10, 209 (1972)

2 This phrase originated with Judge H. Friendly in *Associated Industries of New York State v Department of Labor*, 487 Fed Rep , 2nd ser 341, 354 (2nd Cir , 1973) The partnership is designed to "check extravagant exercises of the agency's authority to regulate risk" (29, p. 649)

3 H Leventhal, *Univ Pa Law Rev* 122, 509 (1974), p 555

4 D Bazelon, *Ethyl Corp v Environmental Protection Agency*, 541 Fed Rep 2nd ser 1, 67 (D.C Cir 1976) (Bazelon, Circuit Judge concurring).

5 For a description of the history of standards of review, see *National Lime Association v Environmental Protection Agency*, 627 Fed Rep , 2nd ser, 416, 451 (D C Cir , 1980) The phrase "hard look" originated with Judge Leventhal in *Greater Boston Television Corp v Federal Communications Commission* (FCC), 444 Fed Rep , 2nd ser 841 (D C Cir , 1970) and *Pikes Peak Broadcasting Co v FCC*, 422 Fed Rep 2nd ser 671 (D C Cir., 1969) The doctrine is defined and described in detail in W Rodgers, *Geo Law J* 67, 699 (1979), pp 705–707

6 *Industrial Union Department, AFL-CIO v American Petroleum Institute*, 100 S Ct 2844 66 L Ed , 2nd 268 (1980)

7 *Monsanto v Kennedy*, 613 Fed Rep , 2nd ser 947, 955 (D C Cir , 1979)

8 *Environmental Defense Fund v EPA*, 636 Fed Rep 2nd ser 1267, 1283 (D C Cir , 1980)

9 *Ibid* , p 1284

10 *Society of the Plastics Industry v OSHA*, 509 Fed Rep , 2nd ser 1301 (2nd Cir , 1975)

11 *American Iron and Steel Institute v OSHA*, 577 Fed Rep 2nd ser 825 (3rd Cir , 1978)

12 *Ethyl Corp v EPA*, 541 Fed Rep , 2nd ser 1, 24 (D C Cir , 1976)

13 The delegation doctrine is a basic principle of constitutional law that blocks Congress from delegating too much of its authority to other branches of government Key New Deal legislation was invalidated by the courts because it gave the executive too much authority *Schechter Poultry Corp v United States*, 295 U S 495 (1935) (National Industrial Recovery Act unconstitutional) In general, courts interpret a statute narrowly if this will avoid a finding of unconstitutionality Justice J P Stevens' plurality opinion in the benzene case raised the possibility that an open-ended grant of authority to OSHA might violate the nondelegation principle Limiting OSHA's authority avoided the issue

14 *Carolina Environmental Study Group v United States*, 510 Fed Rep , 2nd ser 796 (D C Cir , 1975) (class 9 reactor accidents "almost totally unworthy of consideration in environmental impact statement)

15 *Citizens for Safe Power v Nuclear Regulatory Commission* (NRC), 524 Fed Rep , 2nd ser 1291, 1300 (D C Cir 1975) (low-level radiation from Maine Yankee plant acceptable)

16. *Ethyl Corp v. EPA*, 541 Fed Rep 2nd ser 1, 18 (D C Cir , 1976)

17 T. McGarity, *Geo Law J* 67, 729 (1979)

18 *Alaska v Andrus*, 580 Fed Rep , 2nd ser 465, 473 (D.C Cir 1978) (cost of proceeding without more data may be outweighed by benefits National Environmental Policy Act fails to specify level of certainty needed for environmental estimates). *Citizens for Safe Power v. NRC*, 524 Fed Rep , 2nd ser 1291, 1300 (D C Cir , 1975)

(halting plant until studies settle issue would "bring essential economic development to a standstill")

19 *Natural Resources Defense Council* v. *Train*, 545 Fed Rep. 2nd ser 320 (2nd Cir. 1976) The fact that current knowledge makes this difficult is "irrelevant." See note 5 in the court's opinion.

20 The Supreme Court examined the record in detail Studies of U S rubber workers exposed to pure benzene as a solvent in the 1940's and 1950's showed a ninefold increase in leukemia A NIOSH study of workers at two plants in Ohio in the 1940's showed a significantly higher incidence of leukemia However, exposures were usually in excess of 100 parts per million, and OSHA concluded that no dose-response relation could be determined Only one study of low-level exposure was found, a Dow Chemical study that showed three leukemia deaths in the work force (0.2 expected) The three had been exposed to 2 to 9 parts per million benzene Because the three workers were probably exposed to other occupational carcinogens and no leukemia deaths appeared in other groups of workers with higher exposures, OSHA did not claim that this study demonstrated cause and effect The Supreme Court concluded that evidence in the record of "adverse effects of benzene exposure at 10 parts per million is sketchy at best" and said OSHA had not shown significant risk.

21 *Columbia Basin Land Protection Association* v. *Schlesinger*, 643 Fed Rep. 2nd ser 585, 594 (9th Cir. 1981). *Outer Continental Shelf* Lands Act, 43 U.S Code, sect 1347(b)

22 FIFRA, 7 U S Code, sect 136(bb), Toxic Substances Control Act, 15 U S Code, sect, 2605(a)

23 The quotation is from *Forester* v. *Consumer Products Safety Commission* (CPSC), 559 Fed Rep. 2nd ser 774 789 (D C Cir. 1977) (FHSA), see *Aqua Slide "N" Dive* v. *CPSC*, 569 Fed Rep. 2nd ser 831 (5th Cir. 1978) (CPSA)

24 The "wholly out of proportion" phrase originated with Senator Muskie [*A Legislative History of the Water Pollution Control Act Amendments of 1972* (93rd Congress, 1st Session), p. 170]. *EPA* v. *National Crushed Stone Association*, 49 U S. Law Week 4008 (1980). The case dealt only with variances from phase I standards, however, the court indicated (note 10 of Justice White's opinion) that it supported the view that cost-benefit analysis is unnecessary for phase II

25 In construing the National Ambient Air Quality Standards provision, the Supreme Court held that "claims of economic or technological infeasibility may not be considered by the Administrator in evaluating a state requirement that primary ambient air quality standards be met in the mandatory three years" [*Union Electric Co* v. *EPA*, 427 U.S 246, 265 (1976)].

26. *Industrial Union Department, AFL-CIO* v. *Hodgson*, 499 Fed Rep. 2nd ser. 467 (D.C Cir., 1974) A later court expanded on this, noting that massive dislocation or "crippling" of an industry would be unacceptable, but it upheld coke oven regulations estimated to cause a 13 percent drop in earnings for industry [*American Iron and Steel Institute* v. *OSHA*, 577 Fed Rep., 2nd ser. 825 (3rd Cir., 1978)

27. *American Petroleum Institute* v. *OSHA*, 581 Fed Rep. 2nd ser 493 (5th Cir., 1978) affirmed on other grounds, sub nom. *Industrial Union Department, AFL-CIO* v. *American Petroleum Institute* (6).

28 Means-end analysis is common in judging the constitutionality or validity of regulations. The Supreme Court held (in a case under the Truth-in-Lending Act) that a statute allowing such rules "as may be necessary" only requires that the means chosen by the agency reasonably relate to the purpose of the statute [*Mourning* v. *Family Publications Service*, 411 U S. 356, 369 (1972)]. Whether in the Consumer Products Safety Commission or Occupational Safety and Health Administration, "reasonably necessary probably was not intended to limit agency authority.

29 *AFL-CIO* v. *Marshall*, 617 Fed Rep 2nd ser 636, 665 (D.C Cir. 1979)

30 *American Textile Manufacturers Association* v. *Donovan*, 49 U.S. Law Week 4720 (1981)

31. *International Harvester* v. *Occupational Safety and Health Review Commission*, 628 Fed. Rep. 2nd ser. 982, 988 (7th Cir., 1980), citing *Turner* v. *Secretary of Labor*, 561 Fed Rep., 2nd ser. 82 (7th Cir. 1977). *RMI Co* v. *Secretary of Labor*, 594 Fed. Rep. 2nd ser. 566 (6th Cir. 1979) requires ad hoc balancing to determine whether the means selected to meet the standard is feasible

32 For examples of the costs per life saved of these and other funding decisions, see W. Rodgers, *Harvard Environ Law Rev* 4, 191 (1980), pp. 194–195.

33. Executive Order 12291, signed by President Reagan in early 1981, is intended, among other things, to "reduce the burdens of existing and future regulations, increase agency accountability for regulatory actions," with certain specific exclusions, through regulatory impact analysis Such impact analysis should contain information on potential net benefits to include "evaluation of effects that cannot be quantified in monetary terms" and the identification of distributional effects.

34. T McGarity (*17*); D. L Bazelon, *Science* **211**, 792 (1981). J Yellin, *Harvard Law Rev*. **94**, 489 (1981).

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Science, Technology, and the Limits of Judicial Competence

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Resolution of scientific and technological controversies occupies an increasingly important position in the agenda of the federal courts. Government efforts to regulate problems related to technological advances have given rise to a new brand of litigation that focuses directly on issues debated among scientific experts. Legislation to control environmental and health risks, such as the National Environmental Policy Act, the Clean Air Act, and the Toxic Substances Control Act, require decisions based on the "best scientific information" as well as relevant social and economic considerations. Science itself has become a focus of litigation as advances in biomedical science lead to controversial research and clinical practices that are challenged in the courts. Thus, scientists are frequently called on to provide technical evidence in order to prove the rationality of administrative decisions or to establish the legitimacy of innovative research practices.

The resulting surge of science-related disputes into the judicial arena has produced a set of difficult and highly visible problems for the courts, and it is widely believed that the traditional processes of adjudication are no longer capable of handling many of these disputes. Introducing a panel on science and the law at the 1978 annual meeting of the American Bar Association, a legal scholar remarked (*1*):

> Traditional legal techniques, education and institutions, may soon be the same kind of anachronism in an age of science-based technology that canon law institutions became with the decline of temporal religious authority. . . . What may be required is a reform of existing structures which is no less comprehensive than the reforms that freed Anglo-American law from the technicalities of writs and those that freed science from the grip of Aristotle.

Some scholars have argued that scientific and technical disputes fall outside the limits of judicial competence and that courts should therefore be content with a greatly reduced role in such controversies, limiting themselves to reviewing the adequacy of procedures for collecting and analyzing scientific evidence Judge Bazelon, for example, has suggested that courts reviewing actions of administrative agencies can do no more than verify that major technical issues are addressed in agency decision-making, that decisions are based on a consideration of all the relevant factors, and that the data and reasoning supporting administrative decisions are entered into the public record (*2*) Others seek a more substantive role for the courts, pointing out that it is virtually impossible in practice to avoid scientific and technical issues, because courts have to acquire some understanding of the basis of agency decisions sim-

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ply to evaluate the adequacy of the underlying administrative procedures (3). Lawyers and scientists subscribing to this view have put forward a variety of proposals designed to increase the scientific competence of judicial decisionmakers and provide better technical input into the judicial process.

In this article we consider some recent litigation in an effort to understand why controversies with a large scientific or technical component seem to place an unusual burden on the adjudicatory process. These controversies fall into two major classes: (i) those in which ethical issues have been raised by scientific advances, particularly in the biological sciences, and (ii) those involving societal risks and perceived deficiencies in the government's effort to mitigate these risks through regulatory action. In part, the problems encountered by the courts stem from the scale and complexity of the issues involved. A "good" decision is hard to reach without evaluating tradeoffs whose ultimate consequences the courts are ill-equipped to consider in the framework of a conventional adjudicatory proceeding. The high level of uncertainty involved in the regulation of scientific and technological developments compounds the difficulty. Disagreements exist about the magnitude of risk, the appropriateness of measuring techniques, and the reliability of data. Because of its great visibility, the problem of technical uncertainty has become the main focus of proposals intended to mitigate the difficulty of adjudicating scientific and technical disputes. We have examined some of these proposed reforms and conclude that they frequently concentrate on the question of technical uncertainty to the exclusion of the conceptual and policy issues at stake.

Scientific Disputes That Strain the Adjudicatory Process

Theoretical and technical advances, especially in the biological sciences, have made possible clinical applications and research procedures that are controversial on religious or moral grounds. Activities such as fetal research, in vitro fertilization, resuscitation of terminally ill patients, and the creation of living microorganisms through recombinant DNA techniques are perceived by some as having the potential to change the "normal" state of nature, alter the genetic structure of man, threaten cherished values, or even violate natural law. Opposition frequently crystallizes around particular applications of such research as attempts are made case-by-case to define limits through administrative appeals and, increasingly, through litigation. Opponents of particular applications seek judicial support for their moral or religious positions by invoking the traditional power of the courts to prevent or compensate for injurious activity.

In this kind of litigation the conflicting values that underlie a dispute are often masked by scientific issues, and the dynamics of adversary litigation seldom permit a separation or clear identification of the values at stake. This is what happened, for example, in the Del Zio case, in which a plaintiff sued Columbia University, Presbyterian Hospital, and the chairman of Columbia's department of obstetrics for refusing her permission to undergo a voluntary in vitro fertilization procedure. During the trial, the qualifications and scientific credentials of doctors who had agreed to perform the procedure became the subject of debate. Attention was focused not only on their past performance as researchers but also on particular technical decisions, such as the use of temperature charts to determine the time of ovulation and of test tubes rather than petri dishes for fertilization. Relatively little attention was paid to what some have seen as the basic issue in the case, the conflict between Mrs. Del Zio's desire to have a baby, even with the aid of controversial scientific techniques, and Columbia University's prior agreement with the federal government not to permit human experimentation without adequate review (4). The litigation reduced the ethical issues involved in in vitro fertilization to a debate about what constitutes competent clinical work. This is ironic in view of the fact that adjudication is probably far better suited to weighing competing values and interests than to settling disputes among scientific experts.

A similar blurring of scientific, social, and moral concerns is evident in many of the "right to die" cases that are making their way into the courts. That courts have a legitimate role to play in this area is apparent from a careful reading of the Massachusetts Supreme Judicial Court's decision in the case of the *Superintendent of Belchertown State School v Saikewicz* (5). Here, the plaintiff was a 67-year-old, severely retarded man suffering from a fatal form of leukemia. The issue before the court was whether life-prolonging treatment should be administered to Saikewicz. Chemotherapy, the treatment routinely available to and accepted by most competent persons with the same disease, could have led to a remission lasting up to 13 months. The judges decided, after balancing the factors for and against treatment, that the plaintiff, acting through his guardian ad litem, could properly refuse such procedures. The central question in the case concerned the extent of an incompetent person's right to refuse life-prolonging treatment, taking into consideration the state's countervailing interest in preserving human life by any available means. Whatever one thinks of the particular balance struck by the court, it must be recognized that the weighing of competing interests carried out in this case constituted an appropriate functioning of the adjudicatory process.

Most "right to die" cases that reach the courts present considerably less clear-cut issues for adjudication. Typically, these cases arise when physicians refuse to discontinue treatment of terminally ill patients until they are assured by a court of law that the decision may be taken without fear of prosecution. Judicial approval is sought even though both the doctors and the patients' representatives agree from the outset that further treatment would be futile. One result is that the courts are converted into forums where litigants seek to establish the meaning of death in scientific terms. Moreover, it has been argued that resort to the courts in these cases is socially destructive, because court-ordered immunity from prosecution in effect permits the medical profession to escape the responsibility it should assume in making life or death decisions (6).

No doubt the existence of legally valid definitions of life and death would make such decisions considerably less painful for doctors and for the families of patients, but it is questionable whether litigation is an appropriate avenue for establishing such definitions. For one thing, it would be unreasonable to expect sporadic litigation to aid the development of generally recognized biological criteria for defining concepts such as brain death. A scientific consensus could only be reached if the medical community worked actively toward establishing such criteria and ratified them through consistent professional practice. There is every indication that the courts would respect the results of such an effort and would not compel treatment beyond a point where responsible medical opinion would declare the use of life-prolonging techniques to be useless (6). However, until a consensus exists, it is perhaps inevitable that a certain number of life-termination decisions will be challenged in the courts. In the meantime, by seeking protection against the threat of lawsuits, doctors may actually delay the attainment of a scientific consensus.

With respect to the ethical issues, although moral, social, or religious scruples may underlie the positions adopted by parties to a lawsuit, the technical rules of litigation virtually ensure that these will not themselves become the subject of courtroom debate. Most "right to die" cases essentially ignore the social or religious aspects of dying and focus instead on the technical definitions of death. In the Del Zio case, the social and ethical questions related to in vitro fertilization did not surface during litigation. Similarly, in the recent controversy over the patentability of living microorganisms, legal arguments have necessarily focused on the intended coverage of the patent laws and the distinction between an invention and a living organism, not on the morality of extending the concept of proprietary rights to the creation and commercial use of new life forms (7).

Technological developments in areas outside the biological sciences do not directly interfere with the processes of life and death, but frequently pose risks to human health, safety, and welfare that generate controversies. Environmental groups and individuals have increasingly turned to litigation to prevent or minimize such risks, but for a variety of reasons this type of litigation strains the adjudicatory process almost to the breaking point.

To begin with, such cases give rise to problems that have little to do with their scientific or technical dimensions. Courts are confronted with voluminous records and lengthy procedural wranglings, just as they are in large antitrust cases or other litigation involving major corporate entities and multiple parties. The special flavor of recent technology-related litigation, however, derives from its unique policy context. Government regulation of major technologies has to take into account a conflicting array of scientific, social, and economic considerations and of public and private interests. The trade-offs considered in the course of regulation are so complex that industry, private citizens, and special interest groups all find ample opportunity to raise questions about the scientific or technical validity and procedural fairness of individual decisions, as well as the underlying social values they seem to represent. Litigation growing out of this context takes many forms. Exposure limits for particular toxic substances, siting decisions and the environmental impact statements they are based on, methodologies such as cost-benefit analysis used to evaluate trade-offs, are all subject to challenge in the courts. And when technological failures occur, as at Love Canal or Three Mile Island, numerous and varied claims are filed against public and private entities by persons seeking compensation.

In the American legal system, basic rules of adjudication, such as those for determining standing or for assigning liability, have largely evolved out of a framework of two-party litigation. These procedures tend to break down in modern technology-related litigation, where the complexity of the issues makes it difficult to determine precisely who those affected are, how they have been injured, and by what agency. Liability is hard to apportion because of the confusion of public and private responsibility in the management of large technological enterprises. In the welter of facts, assumptions, and values represented in such litigation, it is almost impossible for judges to perform the painstaking analysis and balancing of conflicting values appropriate to the adjudicatory process.

The conceptual difficulties created for the courts by scientific and technical controversies are mirrored in the novel legal theories developed by the plaintiffs. For example, demonstrators at nuclear power plant sites have attempted to defend themselves against trespass charges by resurrecting the old "lesser of two evils" doctrine. In the course of judicial proceedings they have argued, with occasional success, that the crime of trespassing is less evil than the dangers of nuclear power and that trespassing is therefore justified as a means of dramatizing the greater evil. By invoking such a defense, litigants seek to ensure that some discussion of values will be injected into an otherwise routine proceeding for dealing with a minor infraction of the law. In another example, opposition to the use of nuclear energy resulted in nothing less than a "lawsuit to end atomic power." In *Honicker v. Hendrie*, the plaintiff's lawyers prepared a brief arguing that the harmful effects of ionizing radiation justify closing down all nuclear fuel cycle operations immediately (8). Legal support for this position was derived from an array of national and international sources of law, principles adopted during the Nuremberg trials, covenants of the United Nations, provisions of the U.S. Constitution. As a social manifesto, and even as an indictment of nuclear power, the resulting document makes fascinating reading, but one does not have to engage in sophisticated legal reasoning to see why it could not carry the day in court. In its audacious reliance on litigation to effect large-scale social change, the *Honicker* case drastically, and perhaps intentionally, overstepped the dividing line between adjudication and policy-making. Not unexpectedly, the Supreme Court rejected Honicker's petition against the Nuclear Regulatory Commission without comment, reaffirming its earlier, constitutionally based judgment that a policy decision concerning nuclear power must ultimately be left to Congress and the states.

Evidentiary Problems

The strains created by litigants wishing to compel policy formulation through the adjudicatory process are compounded by the uncertainty that pervades scientific and technological controversies. Efforts by the government to prevent or reduce harm from scientific and technological activities require decisions to be made at the frontiers of scientific knowledge, often on the basis of incomplete evidence. Challenges to these decisions bring into the courts disputes concerning the quality and interpretation of data that cannot be resolved definitively on the basis of current scientific knowledge. Examples of such questions abound. What is a "safe" standard for human exposure to low-level radiation? How can data from animal toxicity tests be extrapolated to human beings? How does noise affect human health and well-being?

Although issues like these are frequently raised in litigation, it is important to recognize that courts reviewing administrative decisions dealing with such questions are not themselves in the business of coming up with the "right" answer. It is not the correctness of the decision that is at issue, but the substantive and procedural adequacy of the record that supports it. The major function of the court is thus to ensure that the decision-making body, usually a federal regulatory agency, has not engaged in "arbitrary and capricious" action (9) and that due process has been afforded to all parties.

Basic authority to develop a scientific record and to make the necessary factual determinations in such cases is lodged in the administrative agency, which has at its disposal powerful procedures for generating evidence. As Judge Bazelon commented in reviewing the Atomic Energy Commission's rule-making process on nuclear waste disposal (*10*):

> Many procedural devices for creating a genuine dialogue on these issues were available to the agency—including informal conferences between intervenors and staff, document discovery, interrogatories, technical advisory committees comprised of outside experts with differing perspectives, limited cross-examination, funding independent research by intervenors, detailed annotation of technical reports, surveys of existing literature, memoranda explaining methodology.

The role of the court is not to dictate the choice of particular procedures, but to make sure that the agency uses all the means at its disposal to generate a full record of relevant facts in support of its regulatory decision. Although it may often be difficult for the courts to determine what facts are most relevant and whether a "genuine dialogue" has been created by the agency, it seems clear that the reviewing court does not need independent access to the same fact-finding mechanisms that are available to the agency.

Too great an emphasis on the uncertainty of technological impacts can lead both scientists and regulators to recommend inaction, pending the development of better evidence of risk and causation. But decisions to protect human health and welfare need not invariably depend on scientific proof of harm. It is possible to obtain relief at common law from odors, noise, and other nuisances even when their effects on health or well-being are not scientifically well understood. The California Supreme Court recognized this last year in upholding an award of damages for distress caused by airport noise. Compensation was approved for "a sense or feeling of annoyance, strain, worry, anger, frustration, nervousness, fear, and irritability" produced in neighbors of the airport (*11*). By refusing to insist on medical evidence that exposure to noise causes ill health, the court confirmed that, at least in cases involving demonstrable harm from technological enterprises, a scientific rationale does not have to be provided to justify relief. In this case, judicial power was exercised to prevent an adjudicable conflict over values from being converted into a scientific dilemma.

However, the element of technical and scientific uncertainty often seems to encourage litigants to translate questions of social value into a technical discourse. It is assumed that the resolution of uncertainty will automatically clarify social choices and resolve value conflicts related to scientific and technological advances. Thus recommendations for improving the adjudication of scientific or technical disputes focus more and more on the technical competence of the courts

Some Proposed Reforms

The courts face a diversity of problems as they are drawn into the resolution of problems related to scientific and technological advances. Technical uncertainty, a diversity of regulatory policies, and a complex array of social, moral, and religious questions complicate the judicial resolution of such disputes. Yet almost all recent proposals for judicial reform are narrowly directed toward improving the scientific literacy of lawyers and judges and clarifying technical information used as a basis for judicial decisions.

Proposals to enhance the technical competence of the courts range from those suggesting basic changes in the adjudicatory process to those calling for the introduction of scientific advisory and training programs. It has been argued that the structure of litigation should be changed to recognize the crucial role of expertise; that because technical knowledge is necessary to evaluate risks and technical causation, interaction between lawyers and experts should take place at every stage in the process of litigation. The authors of one proposal for reform, in the area of product liability, seek a "seriated" trial format in which the question of technical causation would be debated before any claim for damages is considered. This would allow the theory of liability to develop consistently with an "expert" evaluation of the technical data; the intention is to make the litigation process "more responsive to technological realities" (*12*).

In another call for structural change, Judge Leventhal proposed setting up a cadre of scientific experts who would act as aides to appellate judges, helping them to understand problems of scientific methodology and to assess substantive data (*13*). More extreme reformers would establish a system of special courts equipped to deal with technical matters and run by expert judges able to deal with questions of statistical reliability and the performance of complex technologies (*14*).

Other suggestions for improving the ability of the courts to deal with technical information include the appointment of science advisers and special masters or changes in the training of lawyers, judges, and their clerks. Special masters or science advisers would be set up in ad hoc positions, depending on the need for special expertise. One proposal would extend a system now used in the Court of Customs and Patent Appeals to all the federal courts, buttressing them with a staff of technical advisers trained in both science and the law (*15*). In effect, all these recommendations would equip the courts with sufficient expertise to consider scientific and technical claims more intelligently.

Such attempts to improve the competence of the courts, however, do not confront a common problem, namely, that the technical evidence presented for consideration is often inadequate, confused, and controversial. Accordingly, proposals have also been developed for clarifying technical issues and scientific arguments before they enter the courts. Their object is to create a scientifically sound basis for decisions, to develop criteria by which to assess the adequacy and competence of information, and to arrive at a consensus on controversial technical questions that represents the best judgment of the scientific community.

The science court, a well-publicized proposal for dealing with technical disputes, was intended as an adversary forum in which scientists with different views on controversial issues would argue in structured debates before unbiased scientific judges. Debate would be limited to questions of fact: judges would give opinions only on factual matters, leaving social value questions for the political or traditional legal arena. It was assumed, however, that the opinions of these judges would be authoritative enough to provide a basis for adjudicatory decisions. Indeed, proponents of the science court claimed that this procedure would make it possible "to find truth among the conflicting claims made by sophisticated advocates when there is serious controversy within the technological community" (*16*). Similar beliefs have generated calls for a "technological magistrature" and for a new profession of "certified public scientists" who would make independent technical evaluations of scientific disputes (*17*).

Alternatively, legal scholars have proposed a systematic use of scientific bodies, such as the National Academy of Sciences, to resolve controversial technical questions. Their scientific findings and risk assessments would serve as a basis for judicial decisions.

A somewhat different approach, but one also intended to improve the technical information available to the courts. seeks to accommodate technical uncertainty rather than resolve it. Advocates of this approach stress that uncertainty requires open ventilation of the differences in expert opinion. For example. the decision in *Calvert Cliffs' Coordinating Committee. Inc.* v. *United States Atomic Energy Commission* pointed out that poorly financed intervenors may lack the wherewithal to marshal technical evidence and thus be at a disadvantage in administrative proceedings and in the courts (*18*). The problem could be ameliorated by distributing resources so as to allow all sides to air their concerns and to present expert data in support of their positions. Measures for promoting this include the funding of technical intervenors and the distribution of scientific resources to citizen groups.

Analysis

Are such reforms likely to resolve the problem of judicial competence? Will they enhance the ability of the courts to deal with the characteristics of science and technology that have strained the adjudicatory process? Although the proposals described above would certainly improve the technical competence of the courts. we believe that they hold little promise of solving the more basic problems involved in scientific and technological litigation.

We have suggested that advances in science and technology. especially in the biological sciences. have created new conceptual problems that cannot be resolved by analogy to existing legal precedents. Proposals to enhance technical competence do not confront these new problems. Furthermore. proposals that seek to develop factual justification for ethical decisions often represent an extension of scientific rationality to inappropriate areas. In an effort to provide legitimacy for judicial decisions. scientific data are used to resolve questions that have little to do with science at all. Questions of aesthetics. of human dignity. and of religious belief underlie many allegedly scientific disputes. Practices such as involuntary sterilization or fetal research threaten what Tribe (*19*) has called "fragile values"—values that are nonquantifiable. intangible. resistant to categorization. The empirical or logical deductive methods of science have little to do with such issues and may even "squeeze out" important values by subjecting them to precise definition. In such cases. using science to resolve disputes will not satisfy the parties involved.

Some proposals also seem to ignore the fact that the technical uncertainty underlying many disputes is genuine; in many cases the evidentiary basis for definitive resolution simply does not exist. In these cases. scientists operate less as neutral parties than as advocates. providing evidence on both sides of technical disputes. Proposals that enhance the role of scientists in adjudicatory procedures may bring areas of technical disagreement into sharper focus. but will not necessarily lead to resolution.

The use of expert forums in settling legal disputes can be questioned from still another perspective. The belief that scientific expertise is inherently removed from value considerations and that scientists are therefore political celibates is an anachronistic and even dangerous one. Expert forums may limit the role of dissent by giving a dominant place to establishment views on controversial topics. Such consensus-building procedures may also perpetuate misconceptions about the relation between facts and values in controversial areas where questions of value are difficult to distinguish from questions of fact. Furthermore. the need for urgent action in controversial areas may lead to undue reliance on expert opinion. When policymakers and the courts need quick answers. tentative scientific judgments may be treated as definitive conclusions and the qualifications intended by scientists may be lost (*20*).

In the end. proposals to bolster judicial competence in technical areas fall short. for the problems faced by the courts in dealing with controversies in these areas cannot be attributed simply to lack of judicial expertise. They also reflect the failure of the policy process to recognize fully the public and multifaceted character of modern scientific and technological development. In the absence of controlling policy principles. broad questions that follow from scientific and technological activities will continue to reach the courts in the artificial guise of two-party adversarial litigation. Equipping the courts with scientific and technical support may facilitate the adjudication of these issues; however. it may also divert attention from the public responsibility for major policy decisions and encourage the conversion of moral and political questions into technical debates among experts. As our strongest institution for defending fragile values. the courts should guard against such overextension of scientific expertise. However. the fundamental choices involved are not simply matters to be resolved by adjudication; they call for setting priorities and evaluating the public will. clearly a political. not a judicial. role.

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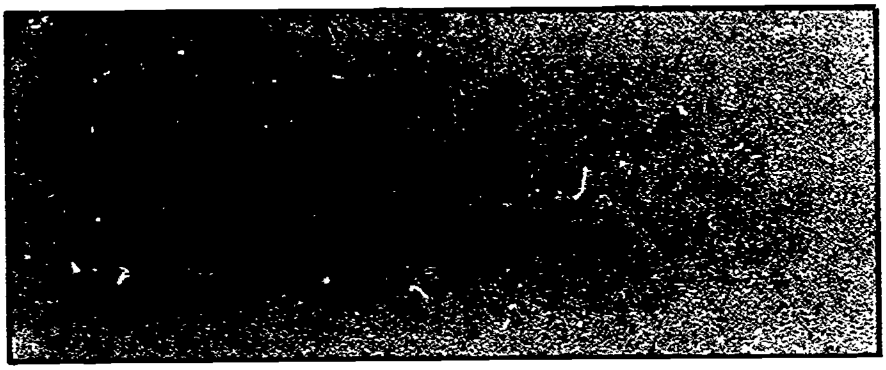

Each year the hazards associated with technology lead to illness and death, as well as varying environmental, social, and economic impacts; these effects correspond to a significant fraction of the gross national product (*1*, *2*). Despite the burden imposed by technological hazards and the broad regulatory effort devoted to their control, there have been few studies comparing the nature of technological hazards in terms of generic characteristics. Most investigators have produced case studies (*3*), comparative risk assessments of alternative technologies (*4*, *5*), comparative lists of hazard consequences (*6*, *7*), or comparative costs of reducing loss (*8–10*).

A first step in ordering the domain of hazards should be classification. Today technological hazards are classified by the source (automotive emissions), use (medical x-rays), potential for harm (explosions), population exposed (asbestos workers), environmental pathways (air pollution), or varied consequences (cancer, property loss). One scheme is chosen, usually as a function of historical or professional choice and the relevant regulatory organizations, even though most technological hazards fall into several categories. For example, a specific chemical may be a toxic substance, a consumer product, an air or land pollutant, a threat to worker health, or a prescription drug. Indeed, a major achievement has been the cross-listing of several of these domains of hazardous substances by their environmental pathways (*11*).

We have sought to identify common differentiating characteristics of technological hazards in order to simplify analysis and management of them. Technological hazards may be thought of as involving potentially harmful releases of energy and materials. We characterized the stages of hazard causation by 12 physical, biological, and social descriptors that can be measured quantitatively; we then scored 93 technological hazards and analyzed the structure of correlations among them. In this article we present a highly condensed account of our analysis (*12*).

Measures of Hazardousness

We should first distinguish between the terms hazard and risk. Hazards are threats to humans and what they value, whereas risks are quantitative measures of hazard consequences that can be expressed as conditional probabilities of experiencing harm. Thus, we think of automobile usage as a hazard but say that the lifetime risk of dying in an auto accident is 2 to 3 percent of all ways of dying.

We conceive of technological hazards as a sequence of causally connected events leading from human needs and wants to the selection of a technology, to the possible release of materials and energy, to human exposure, and eventually to harmful consequences (Fig. 1). To differentiate among types of hazards, we defined 12 measures for individual hazards and applied them to the appropriate stage in this chain. We selected descriptors (Fig. 1 and Table 1) that would be applicable to all technological hazards, comprehensible to ordinary people, and could be expressed by common units or distinctions.

One variable describes the degree to which hazards are intentional, four characterize the release of energy and materials, two deal with exposure, and five apply to consequences (Fig. 1). Only one descriptor, annual human mortality, is closely related to the traditional idea of risk as the probability of dying; the others considerably expand and delineate the quality of hazardousness. Four descriptors require categorical distinctions and eight use logarithmic scales (Table 1). Logarithmic scales are practical for cases where successive occurrences range over a factor of 10 or more in magnitude and where estimated errors easily differ by the same amount. Logarithmic scales may also match human perception better than linear scales, as seen by the success of the decibel scale for sound intensity and the Richter scale for earthquake intensity.

Hazards were selected from a variety of sources (*3*, *8*, *13*) and, after scoring, were found to be well distributed on the 12 scales (Fig. 2). Where appropriate, hazards were scored by reference to the scientific literature. Many cases were discussed by two or more individuals, or referred to specialists for clarification. When the results of this scoring were

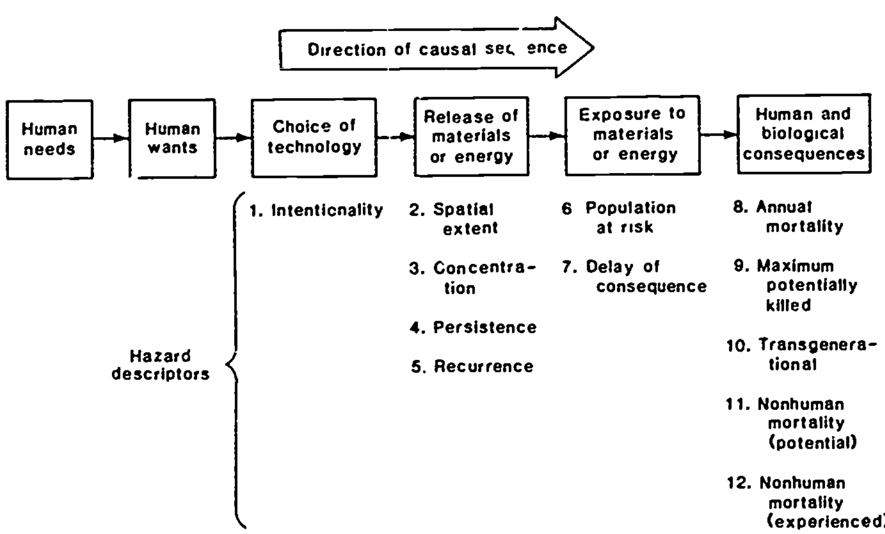

Fig. 1. Causal structure of technological hazards illustrated by a simplified causal sequence. Hazard descriptors used for classifying hazards are shown below the stage to which they apply.

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Table 1. Hazard descriptor scales.

Technology descriptor

1. Intentionality. Measures the degree to which technology is intended to harm by a categorical scale: 3, not intended to harm living organisms; 6, intended to harm nonhuman living organisms; 9, intended to harm humans.

Release descriptors

2. Spatial extent. Measures the maximum distance over which a single event has significant impact on a logarithmic scale, $1 < s < 9$, where $s = \log_{10} d + 1$ rounded to the nearest positive integer, and d is the distance in meters.

3. Concentration. Measures the concentration of released energy or materials relative to natural background on a logarithmic scale, $1 < s < 8$. For materials and nonthermal radiation $s = \log_{10} R + 2$ rounded to the nearest positive integer where R is the average concentration of release divided by the background concentration. For mechanical energy, $s = \log_2 a + 0.68$ rounded to the nearest positive integer where a is the acceleration to which individuals are exposed measured in units of the acceleration of gravity. For thermal energy, $s = \log_2 f + 0.68$ rounded to the nearest positive integer where f is the thermal flux expressed in units of the solar flux.

4. Persistence. Measures the time over which a release remains a significant threat to humans on a logarithmic scale, $1 < s < 9$, with $s = \log_{10} t + 1$ rounded to the nearest positive integer where t is the time measured in minutes.

5. Recurrence. Measures the mean time interval between releases above a minimum significant level on a logarithmic scale identical to that used for persistence.

Exposure descriptors

6. Population at risk. Measures the number of people in the United States potentially exposed to the hazard on a logarithmic scale, $1 < s < 9$, with $s = \log_{10} P$ rounded to the nearest integer where P is the population.

7. Delay. Measures the delay time between exposure to the hazard release and the occurrence of consequences on the logarithmic scale defined for persistence.

Consequence descriptors

8. Human mortality (annual). Measures average annual deaths in the United States due to the hazard on the logarithmic scale defined for population at risk.

9. Human mortality (maximum). Measures the maximum credible number of deaths in a single event on the logarithmic scale defined for population at risk.

10. Transgenerational. Measures the number of future generations at risk from the hazard on a categorical scale: 3, hazard affects the exposed generation only; 6, hazard affects children of the exposed generation and no others; 9, hazard affects more than one future generation.

11. Nonhuman mortality (potential). Measures the maximum potential nonhuman mortality on a categorical scale: 3, no potential nonhuman mortality; 6, significant potential nonhuman mortality; 9 potential or experienced species extinction.

12. Nonhuman mortality (experienced). Measures nonhuman mortality that has actually been experienced on a categorical scale: 3, no experienced nonhuman mortality; 6, significant experienced nonhuman mortality; 9, experienced species extinction.

Table 2. Factor structure. Factor loadings are the result of varimax rotation (*20*).

Factor	Variance explained	Hazard descriptors: Name	Factor loading
Biocidal	0.21	Nonhuman mortality (experienced)	0.87
		Nonhuman mortality (potential)	0.79
		Intentionality	0.81
Delay	0.21	Persistence	0.81
		Delay	0.85
		Transgenerational effects	0.84
Catastrophic	0.18	Recurrence	0.91
		Human mortality (maximum)	0.89
Mortality	0.11	Human mortality (annual)	0.85
Global	0.11	Population at risk	0.73
		Concentration	−0.73
Residual		Spatial extent	

checked for consistency, changes of 1 or 2 points were made in 8 percent of the scores and 3 points or more in a few scores (< 1 percent). We therefore believe replicability to be within ± 1 scale point in most cases.

Hazard Classification

Many investigators have developed descriptive classifications of technological hazards (*14–19*). Though mindful of this work, we based our classification entirely on the causal structure descriptors defined in Table 1.

Energy versus materials hazards. A simple, but significant, distinction is the division of hazards into those resulting from energy releases and those from materials releases. Comparison of 33 energy hazards and 60 materials hazards reveals four striking differences (*12*). (i) Energy releases persist for short-periods, averaging less than a minute; materials releases persist on the average for a week or more. (ii) Energy hazards have immediate consequences, with exposure-consequence delays of less than a minute; materials hazards have exposure-consequence delays averaging 1 month. (iii) Energy hazards have only minor transgenerational effects; materials hazards affect on the average one future generation. (iv) Energy hazards have little potential nonhuman mortality; materials hazards significantly affect potential nonhuman mortality.

Reducing the number of dimensions. In addition to simple division of hazards by release class, we explored the extent to which hazards may be grouped according to causal structure. Using principal component factors analysis (*20*), we derived five orthogonal composite dimensions (factors) that account for 81 percent of the variance of the sample. This means that the causal structure of each of the 93 hazards, and probably others to be scored in the future, can be described by five variables, rather than by 12.

The relation of the derived factors to the original set of descriptors is summarized in Table 2. The names given to the factors—biocidal, delay, catastrophic, mortality, and global—are intended to aid the intuition and are related to the descriptors that define each factor. The first four factors use descriptors whose scores increase as the factor increases (positive factor loadings), but the factor global is different. Because of negative loading of concentration, hazards scoring highest on global are high in population at risk and low in concentration (that is, diffuse) (Table 2). The factor global thus defines a special combination of hazardousness with widespread exposure and a concentration of release that is modest with respect to background.

Several tests indicate that the factor structure does not change significantly when hazards are added and deleted from the sample, or when scoring changes comparable to the estimated

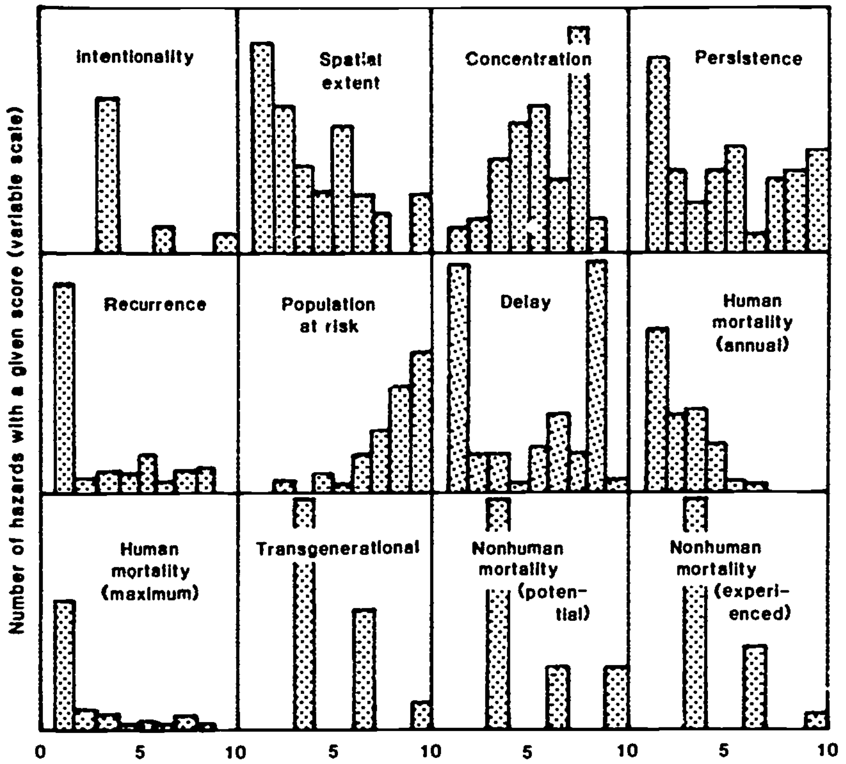


Fig. 2. Descriptor frequency distributions for 93 hazards.

scoring errors are made. Thus an initially chosen set of 66 hazards yielded the same factor structure as the final 93; changing 10 percent of the scores by 1 to 3 scale points had no significant effect. Furthermore, removal of 24 hazards with the most extreme factor scores produced only minor changes in factor structure, an unexpected finding since extreme scores often dominate the analysis.

Scores of the 93 hazards and the derived factor structure are summarized in Table 3. Individual descriptor scores have been grouped by factor into a 12-digit descriptor code, and extreme scores on each factor have been identified through a five digit factor code, with the use of truncated factor scores (*12*).

Inspection of Table 3 permits quick identification of dimensions that dominate hazardousness in specific cases. For example, commercial aviation (crashes) is high in the catastrophic factor and nondistinctive in the other four; power mower accidents are extreme in none of the five factors; nuclear war (radiation effects) is extreme in four.

The results of the coding in Table 3 led naturally to a seven-class taxonomy with three major groupings (Table 4). The first major group, multiple extreme hazards, includes cases with extreme scores in two or more factors; the second, extreme hazards, has cases with extreme scores on one factor; the third group, hazards, contains all the other cases. The group into which a hazard falls depends, of course, on the cutoff for the designation extreme. Although the location of the cutoff is ultimately a policy question, our preliminary definition is arbitrary (*21*).

How appropriate and useful is our approach to hazard classification? To succeed it must describe the essential elements that make specific hazards threatening to humans and what they value, reflect the concerns of society, and offer new tools for managing hazards. On the first point, we invite the review and evaluation of specialists; on the second and third points, we have additional evidence that we discuss below.

Comparing Perceptions

Although the scores for 93 hazards are the result of judgments, we relied on explicit methods, a scientific framework, and deliberate efforts to control bias. These attributes are not necessarily part of the judgments made by the general public. Indeed many scientists believe that lay judgments about hazards vary widely from scientifically derived judgments (*22*).

Because policies governing various types of hazards are determined to a large extent by people who are not scientists or hazard assessment experts, it is important to know whether lay people are able to understand and judge our hazard descriptors and whether these descriptors capture their concerns. The results of a pilot study that we conducted with 34 college-educated people (24 men and 10 women, mean age 24) living in Eugene, Oregon, are interesting.

To test the perceptions of these people we created nontechnical definitions and simple scoring instructions for the causal descriptors of hazards and asked the subjects to score our sample of 93 hazards (*12*). After an initial trial, concentration was judged to be too difficult for our respondents to score. For similar reasons, 12 of the less familiar hazards were omitted. The subjects then scored 81 hazards on 11 measures from our instructions and their general knowledge, reasoning, and intuition.

The results indicate reasonably high correlations between the scores derived from the scientific literature and the mean judgments of our lay sample (r = .65 to .96) (*12*). But despite these high correlation coefficients (Fig. 3), deviations of a factor of 1000 between scientific and lay estimates were encountered, suggesting that there were strong biases among our subjects for some descriptors and some hazards. The subjects also tended to compress the hazard scale, systematically overvaluing low scoring hazards and undervaluing high scoring hazards. Because this effect appeared in the scores of individual subjects, it was not an artifact of regression toward the mean. Similar effects were reported by Lichtenstein *et al.* (*23*) in comparisons of perceived risk with scientific estimates of annual mortality.

To test whether our descriptors by causes of hazards would capture our subjects' overall concern with risk, we collected judgments of perceived risk, a global risk measure whose determinants have been explored in other psychometric studies (*13, 19*). Subjects were asked to consider "the risk of dying across all of U.S. society," as a consequence of the hazard in question, and to express their judgment on a relative scale of 1 to 100. Modest positive correlations between perceived risk and our descriptor scores were obtained in 9 of 12 cases (Table 5). Each hazard descriptor thus explains only a small portion of the variance in perceived risk.

The five factors from Table 2 also

Table 3. Descriptor and factor codes for 93 hazards. The descriptor code for each hazard consists of a digit for each descriptor, and represents scores on the scales defined in Table 1. To help visualize the factor structure, descriptors have been grouped by factor in the order defined in Table 2. The factor code consists of a single digit for each factor, and identifies extreme scores by "1" and non-extreme scores by "0", and also follows the order defined in Table 2. Hazards with two or more extreme factors are identified with *.

HAZARD	DESCRIPTOR CODE	FACTOR CODE
ENERGY HAZARDS		
1. Appliances - fire	333-333-42-3-95-2	00000
2. Appliances - shock	333-113-21-3-95-1	00000
3. Auto - crashes	333-113-11-5-96-2	00010
4. Aviation - commercial - crashes	333-113-63-3-97-	00100
5. Aviation - commercial - noise	333-213-11-1-85-5	00000
6. Aviation - private - crashes	333-113-32-4-97-4	00010
7. Aviation - SST noise	333-313-41-1-76-5	00000
8. Bicycles - crashes	333-113-11-3-84-2	00000
9. Bridges - collapse	333-113-53-1-95-3	00000
10. Chainsaws - accidents	666-113-11-1-74-2	10000
11. Coal mining - accidents	333-233-53-3-64-3	00000
12. Dams - failure	693-423-74-2-85-5	10100*
13. Downhill skiing - falls	333-113-21-2-63-1	00000
14. Dynamite blasts - accidents	333-113-32-2-65-3	00000
15. Elevators - falls	333-113-52-2-96-2	00000
16. Fireworks - accidents	333-113-31-1-83-2	00000
17. Handguns - shootings	369-113-41-4-96-1	10010*
18. High construction - falls	333-113-71-1-28-2	00000
19. High voltage wires - electric fields	333-173-11-1-74-3	00000
20. LNG - explosions	363-213-85-1-86-5	00100
21. Medical x-rays - radiation	333-189-11-4-92-2	00011*
22. Microwave ovens - radiation	333-173-11-1-84-2	00000
23. Motorcycles - accidents	333-113-11-4-76-2	00010
24. Motor vehicles - noise	333-213-11-1-83-3	00000
25. Motor vehicles - racing crashes	333-113-52-2-67-2	00000
26. Nuclear war - blast	699-213-87-4-98-6	10110*
27. Power mowers - accidents	333-113-21-2-73-2	00000
28. Skateboards - falls	333-113-11-3-73-1	00000
29. Skydiving - accidents	333-113-51-2-48-1	00000
30. Skyscrapers - fire	333-425-53-3-85-4	00000
31. Smoking - fires	333-433-32-3-85-1	00000
32. Snowmobiles - collisions	333-113-41-2-73-2	00000
33. Space vehicles - crashes	333-313-84-1-98-5	00100
34. Tractors - accidents	333-113-41-2-74-2	00000
35. Trains - crashes	333-213-53-3-84-3	00000
36. Trampolines - falls	333-113-51-1-74-2	00000
MATERIALS HAZARDS		
37. Alcohol - accidents	333-313-11-4-95-2	00010
38. Alcohol - chronic effects	333-486-11-5-85-1	00010
39. Antibiotics - bacterial resistance	666-563-11-3-97-1	10000
40. Asbestos insulation - toxic effects	333-583-11-3-56-3	00000
41. Asbestos spray - toxic effects	333-583-11-1-83-3	00000
42. Aspirin - overdose	333-456-11-3-97-1	00000
43. Auto - CO pollution	333-346-11-2-94-4	00000
44. Auto - lead pollution	663-976-11-2-95-5	01000
45. Cadmium - toxic effects	663-986-11-2-74-6	01000
46. Caffeine - chronic effects	333-566-11-1-95-1	00000
47. Coal burning - NO_x pollution	693-566-11-3-95-7	10000
48. Coal burning - SO_2 pollution	693-563-11-4-94-7	10010*
49. Coal mining - black lung	333-483-11-4-64-3	00010
50. Contraceptive IUD's - side eff.	333-763-11-2-67-1	00000
51. Contraceptive pills - side eff.	333-586-11-3-74-1	00000
52. Darvon - overdose	333-556-11-4-77-1	00010
53. DDT - toxic effects	996-886-32-1-87-5	11000*
54. Deforestation - CO_2 release	696-993-11-1-91-9	10001*
55. DES - animal feed - human toxicity	333-586-11-1-93-1	00001
56. Fertilizer - NO_x pollution	393-686-11-1-93-9	00001
57. Fluorocarbons - ozone depletion	393-883-11-1-97-9	00000
58. Fossil fuels - CO_2 release	393-993-11-1-92-9	00001
59. Hair dyes - coal tar exposure	333-286-11-1-87-1	00000
60. Hexachlorophene - toxic effects	666-363-11-2-87-1	10000
61. Home pools - drowning	333-223-41-3-83-1	00000
62. Laetrile - toxic effects	333-553-11-1-55-1	00000
63. Lead paint - human toxicity	333-773-11-3-75-2	00000
64. Mercury - toxic effects	663-986-13-2-85-5	01000
65. Mirex pesticide - toxic effects	696-886-22-1-67-5	11000*
66. Nerve gas - accidents	669-836-73-1-77-5	10100*
67. Nerve gas - war use	699-836-87-3-97-7	10100*
68. Nitrite preservative-toxic eff.	336-786-11-1-91-1	00001
69. Nuclear reactor - radiation release	363-969-86-1-96-7	01100*
70. Nuclear tests - fallout	663-989-73-3-91-9	01101*
71. Nuclear war - radiation effects	699-989-88-4-97-9	11110*
72. Nuclear waste - radiation effects	363-989-15-1-82-6	01001*
73. Oil tankers - spills	663-763-61-1-15-6	00000
74. PCB's - toxic effects	663-976-13-1-97-6	01000
75. Pesticides - human toxicity	996-886-12-2-97-5	11000*
76. PVC - human toxicity	333-486-11-2-77-4	00000
77. Recombinant DNA - harmful release	393-869-97-1-97-9	01100*
78. Recreational boating - drowning	333-223-51-4-83-2	00010
79. Rubber manufacture - toxic exp.	333-986-11-3-57-4	01000
80. Saccharin - cancer	333-486-11-1-87-1	00000
81. Smoking - chronic effects	333-486-11-6-85-1	00010
82. SST - ozone depletion	393-893-11-1-93-9	00001
83. Taconite mining-water pollution	663-983-11-1-67-6	00000
84. Thalidomide - side effects	333-456-51-1-17-1	00000
85. Trichloroethylene-toxic effects	333-983-11-1-87-4	00000
86. Two,4,5-T herbicide - toxic eff.	696-886-22-1-77-5	11000*
87. Underwater construction - accidents	333-223-61-1-44-3	00000
88. Uranium mining-radiation	333-989-12-2-64-5	01000
89. Vaccines - side effects	696-556-11-2-84-1	10000
90. Valium - misuse	333-566-11-3-87-1	00000
91. Warfarin - human toxicity	666-653-11-1-87-1	10000
92. Water chlorination-toxic eff.	666-583-11-1-97-5	10000
93. Water fluoridation-toxic eff.	333-786-11-1-82-5	00001

showed modest positive correlations with perceived risk. Because the factors are linearly independent, the summed variance of the factors may be used to determine the total variance explained. With the sample of 34 Oregonians we find that our descriptors account for about 50 percent of the variance in perceived risk.

Perhaps the most striking aspect of these results is that perceived risk shows no significant correlation with the factor mortality. Thus, the variable most frequently chosen by scientists to represent risk appears not to be a strong factor in the judgment of our subjects.

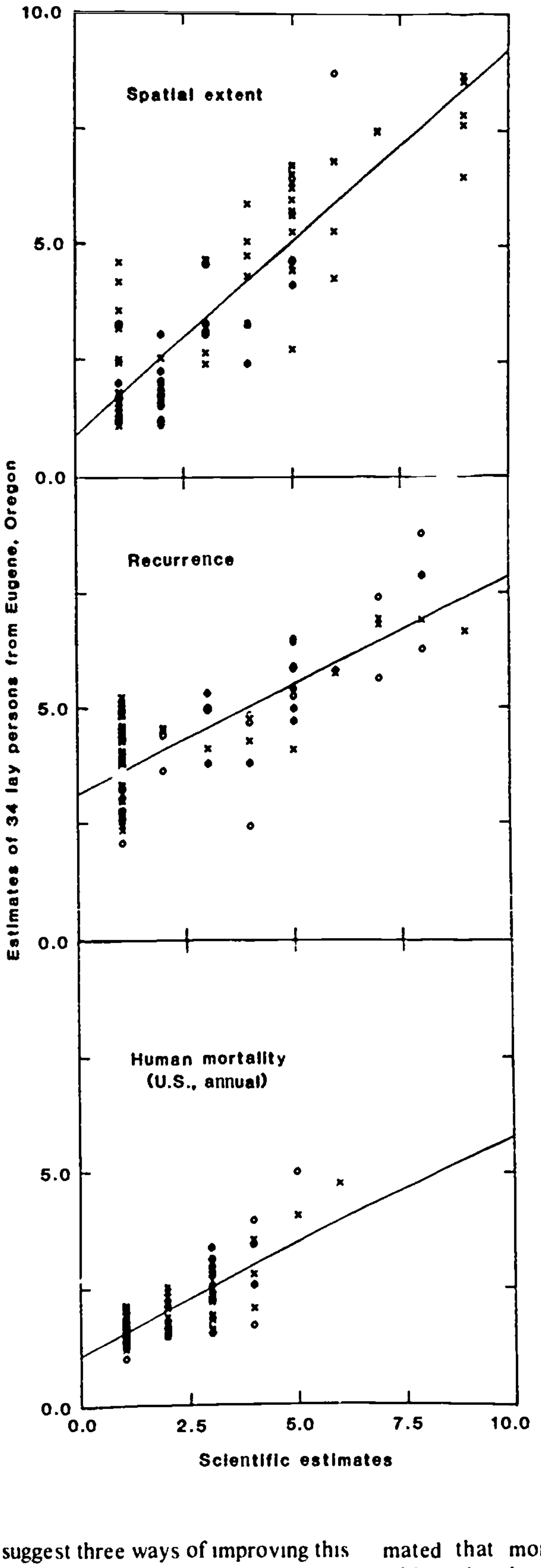
When average ratings from the 34 subjects were used instead of descriptor scores, correlations with perceived risk increased substantially, and factor scores derived from the subjects' descriptor ratings explained 85 percent (not 50 percent) of the variance in perceived risk. It appears, therefore, that the hazard descriptors were well understood by our nonexpert subjects and that they captured most of the global concern with risk that is expressed in the variable perceived risk. Larger and more representative groups must be tested before the results can be generalized.

Applications to Managing Hazards

In addition to improving our understanding of hazards, our conceptualization of hazardousness may help society select social and technical controls to ease the burden of hazards. Though detailed discussion of hazard management is beyond the scope of this article, we

Table 4 A seven-class taxonomy.

Class	Examples
Multiple extreme hazards	Nuclear war (radiation), recombinant DNA, pesticides
Extreme hazards	
Intentional biocides	Chain saws, antibiotics, vaccines
Persistent teratogens	Uranium mining, rubber manufacture
Rare catastrophes	LNG explosions, commercial aviation (crashes)
Common killers	Auto crashes, coal mining (black lung)
Diffuse global threats	Fossil fuel (CO_2 release), SST (ozone depletion)
Hazards	Saccharin, aspirin, appliances, skateboards, bicycles



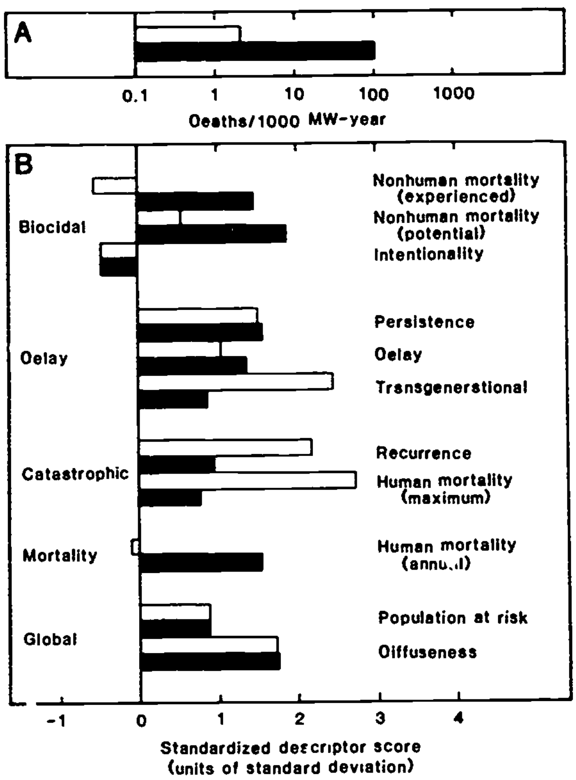


Fig. 3 (left). Scatter plots with linear regression lines indicating the correlation between mean lay judgments and our estimates of hazard descriptors. The three cases illustrate the generally high degree of correspondence between the two types of judgment, occasional deviations by as much as a factor of 1000, and except in the case of spatial extent, a significant compression of the scale of lay judgments. (x) Materials hazards; (O) energy hazards. Fig. 4 (above). Comparison of nuclear (light bars) and coal-fired (black bars) electric power by using Inhaber's analysis (A) and our hazardousness factors and descriptors (B).

can suggest three ways of improving this process.

Comparing technologies. Basic to hazard management are comparisons and choices among competing technologies. For example, for electricity generation, coal and nuclear power are frequently compared, and the hazards of each are invariably couched in terms of mortality estimates. Inhaber (*4*) has estimated that mortality rates associated with coal technology are 50 times those for nuclear power technology (Fig. 4A). Such one-dimensional comparisons have created considerable dissatisfaction because they ignore other important differences, including other aspects of hazardousness, between the two technologies (*24*).

Our factors and descriptors for hazardousness offer a partial solution by allowing a multidimensional hazard profile to be applied to coal and nuclear power (Fig. 4B). This profile was obtained from combined descriptor scores for each of several hazard chains that make up the total hazard of coal and nuclear power (*12*). Coal still exceeds nuclear in human mortality, as expected from Inhaber's analysis, and it also ex-

Table 5. Correlation of causal structure descriptors with psychometrically determined values of perceived risk for 81 hazards. Only values of r at greater than 95 percent confidence level are given.

Descriptor	r
Technology descriptor	
Intentionality	28
Release descriptors	
Spatial extent	.57
Concentration	—
Persistence	42
Recurrence	—
Exposure descriptors	
Population at risk	42
Delay	.30
Consequence descriptors	
Human mortality (annual)	—
Human mortality (maximum)	.53
Transgenerational	.43
Nonhuman mortality (potential)	.53
Nonhuman mortality (experienced)	.30
Factors	
Biocidal	.32
Delay	.41
Catastrophic	.32
Mortality	—
Global	.30
Variance explained (Σr^2)	.50

ceeds nuclear in nonhuman mortality, that is, environmental effects. Nuclear power, on the other hand, dominates in possible transgenerational effects and the catastrophic factor. The two technologies show little difference in persistence, delay, population at risk, and diffuseness.

The profile of hazardousness developed from the 12 hazard descriptors seems to capture the complexity of choice in energy risk assessment and management better than the common mortality index. The problem of choice remains, as does the question of how should society weight the different dimensions of hazardousness.

Hazard of the week. Analysis of national news media shows that 40 to 50 hazards receive widespread attention each year (*25*). In theory, each new hazard goes through a sequence that includes problem recognition, assessment, and managerial action. Often there is need for early managerial response of some kind. Our descriptors of hazardousness provide a quick profile that allows new hazards to be grouped and compared with others that have similar profiles. Such comparisons may provide industrial or governmental managers some immediate precedents, as well as a warning of unexpected problems, a range of suggested managerial options, and, at the very least, a measure of consistency in public policy.

We tested this use of the profile by scoring a new hazard, tampons—toxic shock syndrome. The profile of this hazard was most similar in structure to that of the profiles of contraceptive intrauterine devices (IUD's)—side effects; and then to aspirin—overdose; Valium—misuse; and Darvon—overdose. Indeed, subsequent regulatory response to the hazard associated with tampons has paralleled that to IUD's, the hazard in our inventory closest in structure to tampons.

Triage. As a society we cannot make extraordinary efforts on each of the 100,000 chemicals or 20,000 consumer products in commerce. If our causal structure and descriptors reflect key aspects of hazards—threats to humans and what they value—then our taxonomy provides a way of identifying those hazards worthy of special attention. Cases with multiple extreme scores (Table 3) lead naturally to a proposal for triage: extraordinary attention for multiple extreme hazards, distinctive effort for each of the groups of extreme hazards, and an ordered, routine response for the remainder.

Although we regard the suggestion of triage as an important outcome of our analysis, it is well to remember that many of the extreme hazards, such as nuclear weapons, are among a group that has defied solution for a long time and that special efforts expended on them may produce few concrete results. This leads some to argue that society should focus its effort on cases of proven cost-effectiveness—cases with the maximum reduction in hazardousness per unit expenditure.

We regard neither triage nor adherence to cost-effectiveness criteria as adequate foundations for managing hazards; rather, we see them as the horns of a familiar dilemma—whether to work on the big questions where success is limited, or to work on the normal, where success is expected.

Summary and Conclusions

All taxonomies are based on explicit or implicit assumptions, and ours is no different. We assume that technological hazards form a single domain, that they are defined by causal sequences, and that these are usefully measured by a few physical, biological, and social descriptors. Our picture leads us to distinguish between energy and materials releases and provides a method for constructing profiles of hazardousness that considerably extend the conventional concept of risk as annual human mortality. Our profiles of hazardousness appear to be comprehensible to lay people and to capture a significant fraction of our subjects' concern with hazardousness. This suggests that some conflict between experts and lay people may be resolved by clarifying the definition of hazardousness.

We expect that our approach can improve the quality and effectiveness of hazard management. In particular, it may help in comparing the hazards expected from competing technologies as well as provide a quicker, more orderly response to new hazards and offer society a rational approach to triage. Yet to be resolved is the assignment of weights to the different descriptors of hazard.

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26. The research reported in this article was conducted by an interdisciplinary team including P. Collins, R. Goble, A. Goldman, B. Johnson, C. Hohenemser, J. X. Kasperson, R E Kasperson, R W. Kates, M P. Lavine, M. Morrison, and B. Rubin at Clark University, and B. Fischhoff, M Layman, S. Lichtenstein, D. McGregor, and P. Slovic at Decision Research (a branch of Perceptronics). The research team received help in conceptualizing hazards from R C. Harriss, NASA Langley Research Center, and T. C. Hollocher, Brandeis University. Research support was provided by NSF grants ENV77-15334, PRA79-11934, and PRA81-16925

[illegible] Risk, and Public Policy

We are now in a troubled and emotional period for pollution control; many communities are gripped by something approaching panic and the public discussion is dominated by personalities rather than substance. It is not important to assign blame for this. I appreciate that people are worried about public health and about economic survival, and legitimately so, but we must all reject the emotionalism that surrounds the current discourse and rescue ourselves from the paralysis of honest public policy that it breeds.

I believe that part of the solution to our distress lies with the idea that disciplined minds can grapple with ignorance and sometimes win: the idea of science. We will not recover our equilibrium without a concerted effort to more effectively engage the scientific community. Frankly, we are not going to be able to emerge from our current troubles without a much improved level of public confidence. The polls show that scientists have more credibility than lawyers or businessmen or politicians, and I am all three of those. I need the help of scientists.

This is not a naive plea for science to save us from ourselves. Somehow, our democratic technological society must resolve the dissonance between science and the creation of public policy. Nowhere is this more troublesome than in the formal assessment of risk—the estimation of the association between exposure to a substance and the incidence of some disease, based on scientific data.

Science and the Law at EPA

Here is how the problem emerges at the Environmental Protection Agency EPA is an instrument of public policy, whose mission is to protect the public health and the environment in the manner laid down by its statutes. That manner is to set standards and enforce them, and our enforcement powers are strong and pervasive. But the standards we set, whether technology- or health-related, must have a sound scientific base.

Science and the law are thus partners at EPA, but uneasy partners. The main reason for the uneasiness lies, I think, in the conflict between the way science really works and the public's thirst for certitude that is written into EPA's laws. Science thrives on uncertainty. The best young scientists flock into fields where great questions have been asked but nothing is known. The greatest triumph of a scientist is the crucial experiment that shatters the certainties of the past and opens up rich new pastures of ignorance.

But EPA's laws often assume, indeed demand, a certainty of protection greater than science can provide with the current state of knowledge. The laws do no more than reflect what the public believes and what it often hears from people with scientific credentials on the 6 o'clock news. The public thinks we know what all the bad pollutants are, precisely what adverse health or environmental effects they cause, how to measure them exactly and control them absolutely. Of course, the public and sometimes the law are wrong, but not all wrong. We do know a great deal about some pollutants and we have controlled them effectively by using the tools of the Clean Air Act and the Clean Water Act. These are the pollutants for which the scientific community can set safe levels and margins of safety for sensitive populations. If this were the case for all pollutants, we could breathe more easily (in both senses of the phrase); but it is not so.

More than 10 years ago, EPA had the Clean Air Act, the Clean Water Act, a solid waste law, a pesticide law, and laws to control radiation and noise. Yet to come were the myriad of laws to control toxic substances from their manufacture to their disposal—but that they would be passed was obvious even then.

When I departed EPA a decade ago, the struggle over whether the federal government was to have a major role in protecting our health, safety, and environment was ended. The American people had spoken. The laws had been passed; the regulations were being written. The only remaining question was whether the statutory framework we had created made sense or whether, over time, we would adjust it.

Scientific Realities

Ten years ago I thought I knew the answer to that question as well. I believed it would become apparent to all that we could virtually eliminate the risks we call pollution if we wanted to spend enough money When it also became apparent that enough money for all the pollutants was a lot of money, I came to believe that we would begin examining the risks very carefully and structure a system that would force us to balance our desire to eliminate pollution against the costs of its control. This would entail some adjustment of the laws, but not all that much, and it would happen by about 1976 I was wrong.

This time around as administrator of EPA, I am determined to improve our country's ability to cope with the risk of pollutants over where I left it 10 years ago. It will not be easy, because we must now deal with a class of pollutants for which it is difficult, if not impossible, to establish a safe level. These pollutants

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interfere with genetic processes and are associated with the diseases we fear most: cancer and reproductive disorders, including birth defects. The scientific consensus is that any exposure, however small, to a genetically active substance embodies some risk of an effect. Since these substances are widespread in the environment, and since we can detect them down to very low levels, we must assume that life now takes place in a minefield of risks from hundreds, perhaps thousands, of substances. We can no longer tell the public that they have an adequate margin of safety.

This worries all of us, and it should. But when we examine the premises on which such estimates of risk are based, we find a confusing picture. In assessing a suspected carcinogen, for example, there are uncertainties at every point where an assumption must be made: in calculating exposure; in extrapolating from high doses where we have seen an effect to the low doses typical of environmental pollution; in what we may expect when humans are subjected to much lower doses of a substance that, when given in high doses, caused tumors in laboratory animals; and finally, in the very mechanisms by which we suppose the disease to work.

One thing we clearly need to do is ensure that our laws reflect these scientific realities. The administrator of EPA should not be forced to represent that a margin of safety exists for a specific substance at a specific level of exposure where none can be scientifically established. This is particularly true where the inability to so represent forces the cessation of all use of a substance without any further evaluation.

Functions of Regulatory Agencies

It is my strong belief that where EPA, OSHA (the Occupational Safety and Health Administration), or any other social regulatory agency is charged with protecting public health, safety, or the environment, we should be given, to the extent possible, a common statutory formula for accomplishing our tasks. This statutory formula may well weigh public health very heavily, as the American people certainly do.

The formula should be as precise as possible and should include a responsibility for assessing the risk and weighing it, not only against the benefits of continued use of the substance under examination, but against the risks associated with substitute substances and the risks associated with the transfer of the substance from one environmental medium to another through pollution control practices. I recognize that legislative change in the current climate is difficult. It is up to those of us who seek change to make the case for its advisability.

But my purpose here is not to plead for statutory change; it is to speak of risk assessment and risk management and the role of science in both. It is important to distinguish these two essential functions, and I rely here on a recent National Academy of Sciences report on the management of risk in the federal government. Scientists assess a risk to find out what the problems are. The process of deciding what to do about the problems is risk management. The second procedure involves a much broader array of disciplines and is aimed toward a decision about control.

In risk management it is assumed that we have assessed the health risks of a suspect chemical. We must then factor in its benefits, the costs of the various methods available for its control, and the statutory framework for decision. The NAS report recommends that these two functions—risk assessment and risk management—be separated as much as possible within a regulatory agency. This is what we now do at EPA and it makes sense.

Risk Assessment

We also need to strengthen our risk assessment capabilities. We need more research on the health effects of the substances we regulate. I intend to do everything in my power to make clear the importance of this scientific analysis at EPA. Given the necessity of acting in the face of enormous scientific uncertainties, it is more important than ever that our scientific analysis be rigorous and the quality of our data be high. We must take great pains not to mislead people about the risks to their health. We can help to avoid confusion be ensuring both the quality of our science and the clarity of our language in explaining hazards.

I intend to allocate some of EPA's increased resources to pursuing these ends. Our 1984 request contains significant increases for risk assessment and associated work. We have requested $31 million in supplemental appropriations for research and development, and I expect that risk assessment will be more strongly supported as a result of this increase as well.

I would also like to revitalize our long-term research program to develop a base for more adequately protecting the public health from toxic pollutants. I will be asking the outside scientific community for advice on how best to focus those research efforts.

In the future, this being an imperfect world, the rigor and thoroughness of our risk analyses will undoubtedly be affected by many factors, including the toxicity of the substances examined, the populations exposed, the pressure of the regulatory timetable, and the resources available. Despite these often conflicting pressures, risk assessment at EPA must be based only on scientific evidence and scientific consensus. Nothing will erode public confidence faster than the suspicion that policy considerations have been allowed to influence the assessment of risk.

Risk Management

Although there is an objective way to assess risk, there is, of course, no purely objective way to manage it, nor can we ignore the subjective perception of risk in the ultimate management of a particular substance. To do so would be to place too much credence in our objective data and ignore the possibility that occasionally one's intuition is right. No amount of data is a substitute for judgment.

Further, we must search for ways to describe risk in terms that the average citizen can comprehend. Telling a family that lives close to a manufacturing facility that no further controls on the plant's emissions are needed because, according to our linear model, their risk is only 10^{-6}, is not very reassuring. We need to describe the suspect substances as clearly as possible, tell people what the known or suspected health problems are, and help them compare that risk to those with which they are more familiar.

To effectively manage the risk, we must seek new ways to involve the public in the decision-making process. Whether we believe in participatory democracy or not, it is a part of our social regulatory fabric. Rather than praise or lament it, we should seek more imaginative ways to involve the various segments of the public affected by the substance at issue. They need to become involved early, and they need to be informed if their participation is to be meaningful. We will be searching for ways to make our participatory process work better.

For this to happen, scientists must be

willing to take a larger role in explaining the risks to the public—including the uncertainties inherent in any risk assessment. Shouldering this burden is the responsibility of all scientists, not just those with a particular policy end in mind. In fact, all scientists should make clear when they are speaking as scientists, ex cathedra, and when they are recommending policy they believe should flow from scientific information. What we need to hear more of from scientists is science. I am going to try to provide avenues at EPA for scientists to become more involved in the public dialog in which scientific problems are described.

Lest anyone misunderstand, I am not suggesting that all the elements of managing risk can be reduced to a neat mathematical formula. Going through a disciplined approach can help to organize our thoughts so that we include all the elements that should be weighed. We will build up a set of precedents that will be useful for later decision-making and will provide more predictable outcomes for any social regulatory programs we adopt.

In a society in which democratic principles dominate, the perceptions of the public must be weighed. Instead of objective and subjective risks, the experts sometimes refer to "real" and "imaginary" risks. There is a certain arrogance in this—an elitism that has ill served us in the past. Rather than decry the ignorance of the public and seek to ignore their concerns, our governmental processes must accommodate the will of the people and recognize its occasional wisdom. As Thomas Jefferson observed, "If we think [the people] not enlightened enough to exercise their control with a wholesome discretion, the remedy is not to take it from them, but to inform their discretion."

Interagency and International Coordination

Up to this point I have been suggesting how risks should be assessed and managed in EPA. Much needs to be done to coordinate the various EPA programs to ensure a consistent approach. I have established a task force with that charter.

I further believe we should make uniform the way in which we manage risk across the federal regulatory agencies. The public interest is not served by two federal agencies taking diametrically opposed positions on the health risks of a toxic substance and then arguing about it in the press. We should be able to coordinate our risk assessment procedures across all federal agencies. The risk management strategies that flow from that assessment may indeed differ, depending on each agency's statutory mandate or the judgment of the ultimate decisionmaker.

But even at the management stage there is no reason why the approaches cannot be coordinated to achieve the goal of risk avoidance or minimization with the least societal disruption possible. I have been exploring with the White House and the Office of Management and Budget the possibility of effecting better intragovernmental coordination of the way in which we assess and manage risk.

To push this one step further, I believe it is in our nation's best interest to share our knowledge of risks and our approach to managing them with the other developed nations of the world. The environmental movement has taught us the interdependence of the world's ecosystems. In coping with the legitimate concerns raised by environmentalists, we must not forget that we cope in a world with interdependent economies. If our approach to the management of risk is not sufficiently in harmony with those of the other developed nations, we could save our health and risk our economy. I do not believe we need to abandon either, but to ensure that it does not happen, we need to work hard to share scientific data and understand how to harmonize our management techniques with those of our sister nations.

In sum, my goal is a government-wide process for assessing and managing environmental risks. Achieving this will take cooperation and goodwill within EPA, among Executive Branch agencies, and between Congress and the Administration, a state of affairs that may partake of the miraculous. Still, it is worth trying, and the effort is worth the wholehearted support of the scientific community. I believe such an effort touches on the maintenance of our current society, in which a democratic polity is grounded in a high-technology industrial civilization. Without a much more successful way of handling the risks associated with the creations of science, I fear we will have set up for ourselves a grim and unnecessary choice between the fruits of advanced technology and the blessings of democracy.

How to balance the need for scientific openness against threats to national security resulting from that openness is one of the most difficult issues in the emerging relations among science, technology, and society. Controls over unclassified scientific communication and publication have been proposed and justified by arguments resembling those articulated in the years following the Manhattan Project and World War II, when scientists and military officials debated the merits of removing the cloak of secrecy from the field of atomic physics and nuclear research.

Most of the research areas currently being singled out for controls lie within the field of applied science, but restrictions have also been proposed for basic research projects, including some related to lasers, code-related mathematics, high-energy physics, and materials research. Scientists receiving research support from government agencies not directly linked to military objectives, such as the National Science Foundation, have occasionally been advised that their research proposals and reports were routinely circulated to defense or intelligence agencies for review and information.

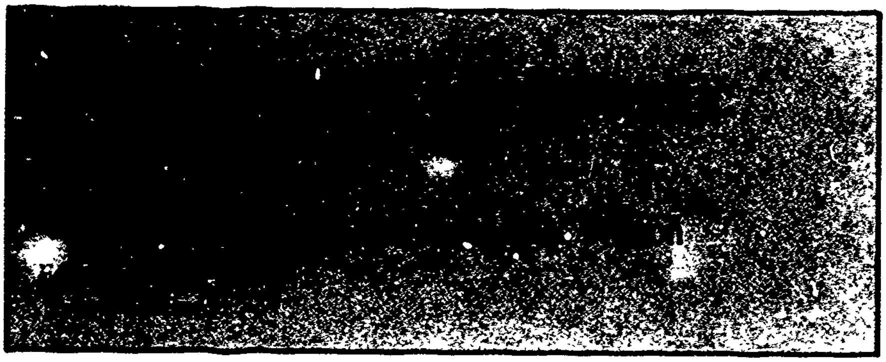
The debate over national security controls on scientific communication may be an early warning of increasing strains developing within the American science–federal government partnership. New calls from government officials for caution and restraint in the dissemination of sensitive research findings, the scientific currency of the twenty-first century, highlight an increased emphasis on protecting substantive national investments in sources of military and commercial advantage.

Above all, the power of nuclear weapons continues to be a strong reminder of the important role scientists play in developing new instruments of war. This chapter includes a 1982 statement presented to Pope John Paul II by presidents of scientific academies and other scientists from all over the world. The statement expresses the belief that science offers the world no real defense against the consequences of nuclear war. It urges nations to renounce first use of nuclear weapons and to prevent further proliferation of these weapons as steps toward a true world peace.

William D. Carey's editorial on science and national security expresses a growing disenchantment with Defense Department policies that target the open communication processes of American science as a source of vulnerability to U.S. national security interests. His exchange of letters with then Deputy Secretary of Defense Frank Carlucci examines the pros and cons of proposed controls. Robert A. Rosenbaum *et al.*, Mitchel B. Wallerstein, and Donald Kennedy develop strong arguments for continued openness in science. The editorial by former defense research chief Richard D. DeLauer takes a more conciliatory approach to the issue and outlines a possible strategy for limiting the extent of government controls.

Additional perspectives on the debate over the need for national security controls on scientific communication are also offered. James R. Ferguson suggests that scientific communication may indeed not be entitled to full First Amendment protection since it is a form of action as well as speech. F.A. Long emphasizes the need for innovative U.S. technology and the consequent need for rapid dissemination of technical information. He suggests that these approaches, rather than controls, should be the focal point of U.S. defense policy.

While some scientists and government officials were arguing the merits of government restrictions in science, others were emphasizing the importance of maintaining scientific exchange as a confidence-building measure between the United States and Soviet governments. Victor F. Weisskopf, Robert R. Wilson, and William D. Carey examine the search for new principles to guide scientific communication in the face of human rights violations and for standards of responsible conduct for scientists in dealing with incidents of repression and injustice. —*RC*



Our scientific relations with the Soviet Union are rapidly deteriorating. We have almost reached the point of the cold war situation 25 years ago. The reasons are clear enough: the persistent violations by the Soviet government of the human rights of scientists such as Orlov, Shcharansky, and many others, the persecution of Sakharov, and now the invasion of Afghanistan.

Many scientists in this country and elsewhere, aghast at these outrages, have resorted to one of the measures available to them: refusing to attend conferences and to participate in collaborative scientific projects. It is assumed that the Soviet leaders are so strongly interested in scientific contacts with the West that they will change their policy. We fear that it will not work that way. Most of the contacts took place in the fundamental sciences or in applied fields removed from weapons technology. These areas are not important enough to Soviet leaders to make them yield to external pressures. The primary victims are our colleagues in the U.S.S.R., for they lose a precious window on the world that was opened to them—and to us. Another victim is scientific progress, since we lose the personal contacts that are so important, particularly in those fields in which one side has more results than the other.

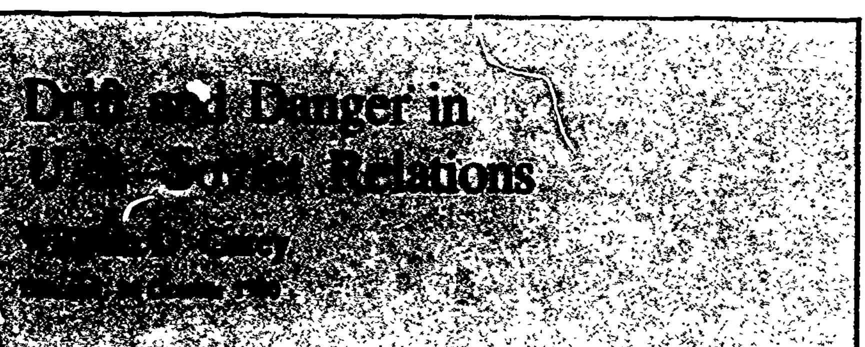
But there are deeper arguments against a boycott of scientific relations. Science is supranational and supraideological—the concern of humankind as a whole. It should stand above political turmoil and serve, as it has in the past, as a bridge for mutual understanding and peace in a divided world. Directly and indirectly, scientific contacts have led to actual disarmament measures—the test ban, for example, or the arms control talks.

We should not lose contact with some of the best elements of Soviet society, a group that basically agrees with our value scale and may have a significant influence on future developments in the Soviet Union. If, as we hope, the present situation will not lead to a catastrophe, there is a chance that, sooner or later, the character of the Soviet regime may change again for the better. We should leave our bridges intact for this eventuality.

Unavoidably, scientific contacts will be weakened in the near future because of the understandable reactions of many U.S. scientists against the recent happenings. However, the U.S. National Academy of Sciences' official suspension of bilateral agreements is a step in the wrong direction. Restrictions on scientific communication are not the right answer to the restrictions the Soviet government has imposed on some of their scientists. Repressive actions usually incite hostility, which often leads to misunderstandings, dislike, and retaliation. Not all of the Soviet scientists will understand the reasons for our actions when we no longer go there and talk to them openly and vigorously, as many of us have done in the past. We may have done the cause of human rights in the U.S.S.R. more of a disservice than a service.

The only appropriate way for the scientific community to deal with any kind of problem, scientific or human, is through reason and discussion: one scientist speaks or writes to another or addresses a meeting of scientists, be it an official one or one organized by refuseniks. Collaborative experiments offer unique opportunities for reaching a mutual understanding, especially through personal contacts during the hours of relaxation. In times of political tensions, we should extend collaborations—not cut them back.

The real problem is the danger of nuclear war. If we cannot learn how to rationalize our differences, how to resolve them by argument rather than by threats and by cutting off relations, then we are really lost. The least we scientists can do is show the power of reasoning. Despite its frustrations, only by reason will both human rights and peace flourish on this small planet.



The dismantling of meaningful scientific exchanges with the Soviet scientific community, provoked by the military occupation of Afghanistan and the subsequent internal exile of Sakharov, calls for second thoughts. It is not as though the uses of the scientific exhanges have zero value to diplomacy while advancing science. The fact is that in the Soviet system the scientific elites not only are extraordinarily valued professionally but also enjoy that rarest of socialist graces, an edge of independence. Turning a blind eye to that fact is shortsighted.

The temper of U.S.-Soviet political relations slides from bad to worse. As tensions increase, the risks of overreaction with ... ful consequences are enhanced. There is, on this side, talk of retargeting schemes, of new attack weapons hatched from supertechnology, and more than a hint of *Star Wars*. From the other side comes political adventurism and an internal tightening of the screws, but mostly a menacing silence. As far as can be judged, not a single diplomatic card is being turned up by either side, or by third parties that could bring the principals together to cool the temperature of growing crisis. Drift, governments need to be reminded, is the worst of all policies.

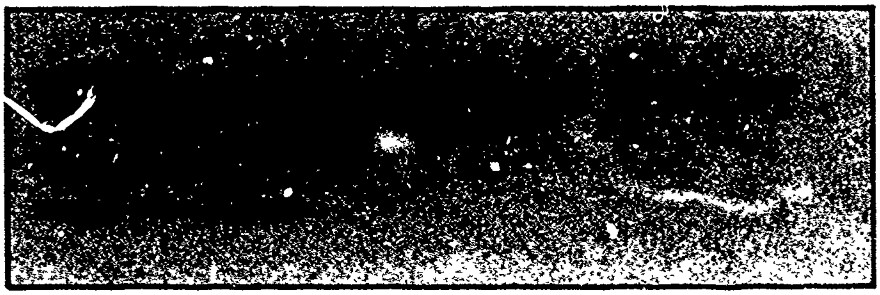
The evidence is that the boycott technique, as applied to scientific exchanges with the Soviets, is availing next to nothing. It has not relieved the besieged defenders of Afghanistan any more than it has restored such civil and professional rights as Sakharov ever enjoyed. When an instrument of policy turns up useless, the sooner it is put down the better. But that is a hard thing to do when it has been invested with the authority of the establishment. Such is the price that is paid for going too far and leaving no exit.

One does not have to eat crow, much less condone the behavior of the Soviets in repressing dissent through violence and police action, to press for a reopening of scientific traffic. The United States maintains what pass for friendly relations with a large class of nations whose political virtue does not meet the standards that we can afford, and we stomach it for reasons of expediency. When it comes to the matter of the Soviets, the difference is not trivial: it is central. The groundwater of a smoldering enmity is heating up, and it cannot be allowed to flash to steam. The conscience of science, however justified its outrage at Soviet behavior, nevertheless has the greater burden of striving to prevent the ultimate outrage, the dimensions of which are better known to scientists on both sides than to distracted and unprotected publics.

It violates no confidences to report that leaders in science in both countries view the present tension with undisguised alarm. Although the driving forces and emotions at work are remote from science, the blow, when and if it comes, will be a confrontation between Western science and technology and its Eastern equivalent. It is this tragic failure that is to be avoided, and when the politics of diplomacy are paralyzed, then a form of science as diplomacy can no longer wait to be exercised. This is what troubled scientists on both sides are now signaling to one another, and for good reasons.

The position is that we are very nearly out of safety valves as the nuclear superpowers drift toward impasse. If scientific responsibility is more than an idle phrase, it requires participation in the pursuit of peace and conflict resolution. The quarantining of Soviet science, however principled, defeats the chances for engaging a concerned and far from impotent cohort of opinion and influence in a dialogue of reason.

Disagree as we will with the actions and the philosophy of the Soviet system, we can find common ground in the shared dread of a collision of power. We should get on with it, before time runs out.



In an open society the relationship between science and the military authorities is a touchy business at best. Science is rightly expected to enhance the national security, and it responds willingly. Equally, the military authorities are expected to respect the values, standards, and methods of science as an open and productive process. Yet, when the climate of national security is overtaken by hyperanxiety this qualitative balance is easily destabilized by judgmental mistakes, and that is what has now happened.

The brochure on *Soviet Military Power* that has been released with much publicity by the Department of Defense goes beyond documenting the U.S.S.R.'s formidable military assets. It addresses what may be termed collateral sources of Soviet military know-how. These sources, in the department's opinion, include high technology that has been transferred by the industrialized free world. Also helpful to the Soviet military, we are informed, are bilateral scientific exchanges initiated under detente. Next come "student exchanges," along with the inter-academy exchanges that predate the government-to-government agreements. Omitting nothing, the Defense Department's distress blankets scientific conferences and symposia, unclassified research reports, and the "professional and open scientific literature." The military authorities seem convinced that the infrastructure supporting the U.S. scientific and technical enterprise caters to Soviet military power and comprises a large pane in the window of vulnerability.

If all this actually reflects the view from the Pentagon, it calls for swift revision. What is sadly missing is the recognition, which surely exists in thoughtful quarters of the defense establishment, that lively but responsible communication in science is essential to the growth and development in science on which both national security and economic potential rely. "National security" is not the simplistic proposition that it is made out to be, and it is in the best interests of those directly responsible for it to realize that laying heavy hands upon scientific discourse is counterproductive and self-denying. Even the maligned exchanges with the Soviets have their uses, and no one supposes that they should or do involve sensitive information. To put it more strongly, it is only sensible to carry on these exchanges where both sides hold first-class rank, including such areas as condensed matter physics and astrophysics. It is a profoundly disturbing mistake to put out the notion that Soviet scientific capability is inferior to ours. We know better.

The operative premise of our military leaders is that the U.S. window of vulnerability must be closed with all possible speed. That premise is buttressed by a substantial national consensus. But if, beyond rebuilding strategic and tactical military assets, it extends to clamping down on legitimate scientific conferences and symposia as well as the open literature of science, the quality of science's interface with the military will go downhill swiftly and tragically. Scientists are well aware that information of genuine national security value must be protected. That is not the point. What is at issue is the balance between protection and overprotection. Difficult as that riddle may be to untangle, it must be dealt with responsibly and by no means solely from a military mind-set. One wants to believe that the Defense Science Board would have taken a different view of these matters had it been asked.

The issues raised here ought to be pondered, as well, by the Commerce and State Departments, where work goes on behind closed doors on regulations to tighten controls on the international transfer and exchange of scientific and technical information. Slamming the window may indeed stop the draft, but at the expense of fresh air and light. More than 30 years ago, Senator Brien McMahon, sponsor of the Atomic Energy Act of 1946, spoke eloquently of the need for a sane balance between two necessary but competing types of security, "by concealment" and "by achievement." Burying knowledge in silos of secrecy serves the one well, the other very badly.

On 9 October 1981, in a letter addressed to The Honorable Frank Carlucci, Deputy Secretary of the Department of Defense, Mr. William D. Carey, Executive Officer and Publisher of Science, criticized statements by the Department of Defense concerning scientific exchanges, conferences, and the unclassified, open scientific literature. Mr. Carey's letter and the reply he received from Mr. Carlucci are printed here verbatim.

I must tell you that the otherwise excellent brochure on *Soviet Military Power* went off the rails badly, in my opinion, in contending (pp. 80–81) that U.S.-sponsored scientific exchanges and scientific communication practices enhance Soviet military power.

I am dismayed to find the Defense Department indicting inter-Academy exchanges, student exchanges, scientific conferences and symposia, and the entire "professional and open literature" as inherently adverse to U.S. military security interests. These normal and well-accepted fora for advancing scientific progress constitute the primary infrastructure of U.S. and worldwide communication in science, and without them the U.S. technology base would go stale very quickly.

The Defense Department should know, by this time, how scientific practice is conducted and how necessary unimpaired communication in science is to advancing the state of the art and improving our own essential capabilities. I find it deplorable to have our Defense Department taking a public and well-advertised stance that exchanges and the open scientific literature constitute still another window of vulnerability and a free asset handed to our principal adversary.

It is also somewhat astonishing to have the Defense Department charging that bilateral U.S.–Soviet scientific and technical exchanges are giveaway channels benefiting Soviet military power. These bilateral exchanges, as you must know, are legitimized by formal intergovernmental agreements initiated by President Nixon and continued by his successors. Whether the Defense Department likes them or not, they constitute the present foreign policy of the United States. As to the merits, it is very important to U.S. interests to be well-posted on the quality of Soviet scientific research. The contact we have through the bilaterals has left no doubt as to Soviet excellence in fields that matter to us, including metallurgy, condensed matter physics, theoretical physics, astrophysics, geophysics, and cancer research. Nobody is arguing that the exchanges should involve security-related fields of science. Elsewhere, in fields where both sides are equally good, it is to our country's advantage to pursue the exchanges. The DOD paper shows an extremely disappointing grasp of what the exchanges are all about.

If I seem exercised by the position taken by the Defense Department in *Soviet Military Power* it is because I *am* exercised. In particular, that position strikes in a deadly way at the dependence of scientific progress on open communication and shared information. Our own military power will be diminished, not enhanced, if the wellheads of scientific communication are sealed and new knowledge confined in silos of secrecy and prior restraint.

Mr. Carlucci's reply

This is in reply to your recent letter in which you state that the Department of Defense (DOD) views the inter-Academy exchanges, student exchanges, scientific conferences and symposia, and the entire professional and open literature as inherently adverse to U.S. military security interests.

Be assured the DOD is well aware of how scientific practice is conducted and fully recognizes the importance of unimpaired scientific communications to the mutual benefit of all parties concerned. In our considered view, however, the exchanges to date, in the main, have not been reciprocal. Rather, it is quite apparent the Soviets exploit scientific exchanges as well as a variety of other means in a highly orchestrated, centrally directed effort aimed at gathering the technical information required to enhance their military posture.

Because of the importance I attach to this complex issue, I want to respond in some detail and thus ask your indulgence. Illustrative examples follow which, at least in part, indicate the basis for our concern.

The energy bilateral agreement began with 14 subtopics. The U.S. promised and delivered the large magnet and magnetohydrodynamics (MHD) channel details as well as a great deal of information on other topics. The Soviets promised but did not deliver data on geothermal energy and energy resources, consumption, production, and forecasting. Consequently, the U.S. Department of Energy has been curtailing its participation. The only topic still active is the one on MHD.

Under the S&T bilateral agreement, the Soviets had been sending large numbers of scientists to the U.S. in the field of chemical catalysis, but the U.S. was gaining virtually nothing in return. Consequently, in 1980 the U.S. terminated the one-sided exchange. It now appears, however, the Soviets will try to use the inter-Academy exchange or other means to acquire the information they deem vital.

Another example of their persistence was demonstrated in the electrometallurgy subtopic of the science and technology bilateral agreement. The Soviets wanted to establish an exchange in the fields of superplasticity and fracture mechanics. A concerned U.S. government scientist succeeded in stopping the exchange in these militarily related topics. However, it was dismaying later to find that the Soviets had acquired the information under the auspices of a new subtopic on corrosion.

One of the provisions common to many of the government-to-government bilateral agreements encourages the establishment of separate agreements between individual companies in the west and entities of the Soviet government, primarily the State Committee for Science and Technology and the Ministry of Power Engineering. These are sometimes referred to as the "Article IV" Agreements and in the case of the U.S. involve a large number of companies that are among the world's leaders in areas in which we know the Soviets to be deficient. The degree of concern with this situation was such that previous policy was altered to the extent that the Export Administration Act of 1979 now requires that companies file notice with the Department of Commerce when such agreements are signed.

We also have evidence that the Soviets are misusing scholarly exchanges. In the area of graduate student and young faculty exchanges, administered by the International Research and Exchange Board (IREX), the U.S. sends young master's and doctoral level students, mostly in humanities, primarily to two universities, Moscow St te and Leningrad State. For the most part, the USSR sends senior, experienced, technical people. Almost all possess *Kandidat* degrees; some come from closed military research institutes, and attend any of a hundred or so U.S. universities. In accordance with the openness of our society, Soviet students are granted academic freedom and, with almost automatic government approval, can travel practically at will. Conversely, American students in the Soviet Union are much more isolated and restricted in their travel and professional contacts.

Soviet weakness is not in basic research, which, as you point out, is on the whole excellent. Rather their weakness is in putting technology into production. It is therefore not surprising to us that the scientists the Soviets nominate are often directly involved in applied military research. For example, in 1976–77 S. A. Gubin's course of study involved the technology of fuel-air explosives. Mr. Gubin studied this topic at one of our leading universities under a professor who was a consultant to the U.S. Navy on fuel-air military explosive devices. As a parenthetical comment, one must admire their ability to determine so precisely where to send their "students." Gubin, incidentally, during his stay ordered numerous documents pertaining to fuel-air explosives from the U.S. National Technical Information Service. When he finished his study, he returned to his work in the USSR developing fuel-air explosive weapons.

In the case of K. H. Rozhdestvensky, it was not until several months after his departure that we learned his research paper was concerned with the "wing-in-ground effect" aerodynamic vehicle. This vehicle has significant potential military applications and indeed, the Soviets have been attempting to develop a wing-in-ground effect machine for quite a number of years.

T. K. Bachman, a psychologist, came to study the interface between man and machine. In the opinion of U.S. researchers, this field was directly applicable to the design of heads-up displays which optimize the amount of data presented visually to a military weapon system operator. Bachman attended several very significant conferences on this topic and was able to observe state of the art demonstrations of such work funded by the Department of Defense.

This graduate student/young faculty exchange is such that each year it is recommended that at least one-half the Soviet nominees not be allowed to pursue their desired topic of study at all or that significant modifications be made in their study program. This is because the information the Soviets seek is either embargoed by law or militarily sensitive. Hence our concern.

In the senior scholar exchange program also administered by IREX, each side sends a number of scholars for a total of 50 man-months per year. As with the graduate student/young faculty program, the Soviets nominate physical scientists, while the U.S. nominates scholars specializing in the arts, literature and history. Until a few years ago, most Soviets in this program conducted very basic research. No objections were voiced to such courses of study. Currently, practically all the Soviet nominees propose to study in fields having military application. Some examples of proposed research topics in 1981 are:

- Properties of adhesive joints of polymers.
- Macromolecular materials and composite materials (two nominees).
- Preparation of micro-tunnel diodes in gallium arsenide by annealing and/or molecular beam epitaxy.
- Theory of computer science and programming methodology.
- Thin-film metals in semiconductor technology.
- Semiconductor and infrared technology, ion implantation, radiation defect analysis and infrared detector techniques and materials.
- Machinability of difficult to machine materials.

Largely as a result of dissident physicist Andrei Sakharov's ill treatment, the U.S. Academy of Sciences imposed a moratorium on joint symposia and other high level contact with the Soviet Academy of Sciences. Nonetheless, individual exchanges are still permitted and the Soviets continue to nominate scientists to study and conduct research in topics that are either embargoed or militarily sensitive. Of 25 scientists nominated by the Soviets during 1980, 11 topics proposed offered a significant potential for loss of critical U.S. technology. There was a somewhat lesser, but nevertheless real, degree of concern over the remaining 14 topics.

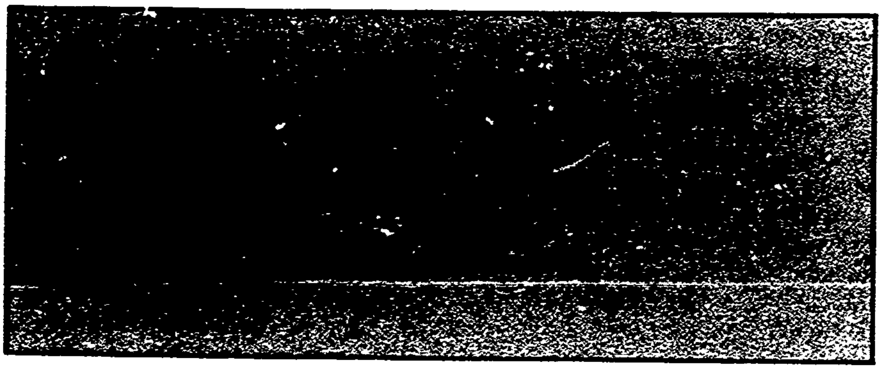
With regard to scientific conferences and symposia, the Department of Defense has become increasingly concerned over the type and volume of defense-related information openly provided. As you will undoubtedly recall, the concern has been such that Soviet Bloc scientists were prevented from attending the First International Conference on Bubble Memory Materials and Process Technology, and the Conference on Laser and Electro-optical Systems/Inertial Confinement Fusion early in 1980. This denial was precipitated by the revelation that Hungarian physicist, Gyorgy Zimmer, provided the Soviets the scientific knowledge on magnetic bubble memories gained as a result of his frequent visits to U.S. laboratories. A thorough review then followed which resulted in an official U.S. policy restricting Soviet attendance at U.S. conferences and symposia. This policy was widely publicized in a number of scientific journals.

With regard to professional and open literature, U.S. and western applied and basic research papers are usually quite explicit in explaining their purpose and are published promptly and without censorship. The author's affiliation and the sponsor of the research are almost always provided. This is contrasted with Soviet and other Communist country publications where the purpose and goal are usually not mentioned or deliberately obfuscated. Authors' affiliations frequently are not provided and the sponsor of the research is rarely identified. In addition all articles are subject to censorship.

I would also point out that it is and has been for many years U.S. policy to make available all unclassified government sponsored research to anyone in the world at very nominal cost through the National Technical Information Services (NTIS) of the Department of Commerce. Similar Soviet research holdings are maintained by the All-Union Institute of Scientific and Technical Information (VINITI) which is jointly administered by the State Committee for Science and Technology and the Soviet Academy of

Sciences. The NTIS assisted the Soviets in setting up VINITI under the auspices of the bilateral agreement on Science and Technology, yet the holdings of VINITI are not released to anyone outside the Soviet Union.

Other examples abound, but I trust that these will suffice to provide you with the context within which our views were framed. The Department of Defense favors scientific, technical and educational exchanges and the free exchange of ideas in basic and fundamental science. However, since the military posture of this nation relies so heavily on its technical leadership, the Defense Department views with alarm the blatant and persistent attempts, some of which have just been described, to siphon away our militarily related critical technologies. I note this is precisely your point when you state "nobody is arguing that the exchanges should involve security-related fields of science." By the very nature of our open and free society, we recognize that we will never be able to halt fully the flow of militarily critical technology to the Soviet Union. Nevertheless, we believe that it is possible to inhibit this flow without infringing upon legitimate scientific discourse. I hope that this letter has allayed your concerns and look forward to additional views you may wish to offer.



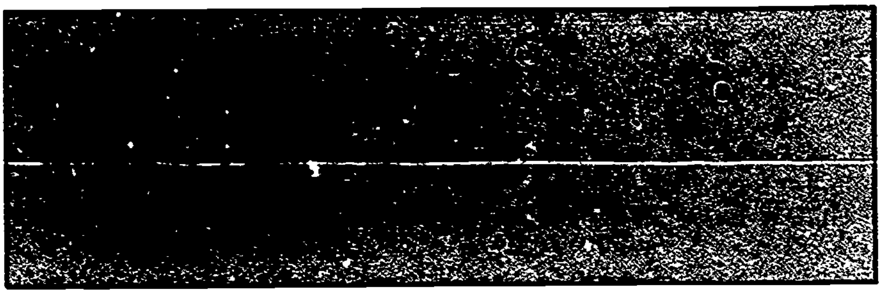
Fundamental science in the United States is a successful venture, but a peculiar one. Its success is attested by the dramatic increase in scientific awards won by Americans and by the contributions it makes to our technological productivity. Its peculiarity lies in the organizational arrangements for its sustenance. It is an activity pursued in one culture, supported out of another: more than two-thirds of the nations's fundamental science is done in the research universities, with the government sponsoring nearly all of it. Given the contrast in style and values between the two cultures, it is hardly surprising that this patronage arrangement sometimes generates quarrels. The current one is a debate over the government's efforts to regulate scholarly activities related to critical technologies.

The problem is that the Communist bloc nations have obtained and then copied U.S. innovations in military technology The solution proposed by the Administration—based on the hypothesis that these leaks partly involved academic exchanges—is that universities restrict the access of foreign students and visitors to, and dissemination of research results in, certain fields of study. These restrictions are sought by applying to fundamental research regulations originally intended for devices or industrial processes.

The universities have countered that if unpatrolled academic visits and the participation of foreign students in research programs contribute to technology leakage, they do so minimally, and that interdicting academic exhange would therefore yield trivial benefits to national security. We have also emphasized the potential damage to the scientific enterprise from government efforts to restrict its openness—especially when these coincide with efforts of private sponsors to expand secrecy for proprietary reasons. It surely will be difficult to resist the latter if we are forced to accept the former.

There is, fortunately, encouraging progress to report. The Department of Defense has created a forum with the universities to examine research restrictions, among other things The National Academy of Sciences will be studying the matter as well. The Department of State and the Department of Commerce are responsible for the regulations under which the current restrictions have been mandated. The State Department has revised its instructions on the handling of Soviet visitors, so that their academic hosts will not be required to shield them from exposure to particular unclassified projects. That department, which is responsible for the International Traffic in Arms Regulations, has also adopted an appropriately narrow view of their scope. We are informed that the regulations will apply only to technical data significantly and directly related to specific items on the Munitions List—the limitation set out by the United States Court of Appeals for the Ninth Circuit in *U.S. v. Edler Industries*.

These are promising signs of change, and they should be encouraged; there are other ways to meet our national security objective. If a Soviet scientist is viewed with such alarm that universities must be asked to police his visit, then the Department of State can apply visa controls. And if a technology has such military value that exposure in an open environment presents clear risks to national security, the government can classify the technology—thereby permitting the universities to decide in advance whether they can accept the restrictions that come along with the work. But to apply a burdensome set of regulations to a venture that has gained such great strength through its openness will cost the nation more than it can be worth.



In the mellow last days of August, even as the National Academy of Sciences' panel on Scientific Communication and National Security rolled up its sleeves to work on its report, our military authorities launched a surprise strike resulting in the suppression of papers scheduled to be read at a major conference on optical instrumentation.

Exercising its oversight of Defense-funded research and development contracts, the military summarily embargoed the presentation of about 100 papers whose titles in a number of instances unwisely included language suggestive of military applications. Although the affair is being smoothed over, there can be little doubt that the continued prospects for open discussion of leading-edge unclassified work now dwell in a no-man's-land of confusion and disarray, subject to further incursions at any time. The humiliation visited on the sponsoring engineering society is no small matter and one that will be taken to heart by other scientific and engineering organizations. Of more significance, if the raid at San Diego was more than an aberrant case of fractured communications, is an emergent tilt toward reliance upon preemptive powers. Should this be so, we are seeing a new face of the defense research funding system which, over many decades, contributed on an enviable scale to the open search for and sharing of knowledge.

Such a transformation would go far to undo the postwar terms of reference that assured comity and stability to relationships between the scientific and technical communities and the defense establishment. If acceptance henceforth of Defense Department support for significant but unclassified work must carry with it implicit or explicit acquiescence in the suppression of disclosure, will scientists and engineers be prepared to travel that road with the specter of ambushment no farther distant than the next professional meeting? What conference planner will consider inviting foreign participants lest they be suspected carriers of unclassified tidings to delight an insatiable KGB?

It is not just the unfortunate handling of the affair at San Diego that is unnerving. The timing is no less to be deplored. What has been needed is a breathing spell to reduce the tensions and the controversy of last winter, and an opportunity for balance to be struck between the needs for national security and the requirements for scientific and technical communication. The latest failure of restraint undeniably constitutes a setback to peacemaking efforts.

The relationships between the government and the scientific and technical communities continue to be sorely troubled as the fixation on the "hemorrhage" of technology hardens. Even as the pipeline war has unsettled the Atlantic diplomatic consensus, it appears that the crucial domestic consensus between science and national defense is being tested severely. It becomes increasingly clear that a formula must be found to set up an institutional umpire with authority to see to it that checks and balances are put in place and understood on both sides. It will not do to continue to have a variety of government agencies taking matters into their own hands without coordination indifferent to the consequences.

There is one other, and quite vital, point that must not be lost sight of. When a proper concern for the national security is burdened by clumsy execution, something is subtracted from the fundamental respect that is owed the necessary goal of safeguarding defense secrets. Once confidence in the judgment and the management of the security process is shaken, its integrity is served badly. The defense authorities have very good reason to know that the scientific community has proved its respect for the national security through three hot wars and a long cold war. That respect must be reciprocated.

Declaration on Prevention of Nuclear War

Conference on Nuclear Warfare

[illegible]

On 24 September 1982, this statement was presented to His Holiness, Pope John Paul II, by an assembly of presidents of scientific academies and other scientists from all over the world convened by the Pontifical Academy of Sciences to consider the issue of nuclear warfare.

I. Preamble. Throughout its history, humankind has been confronted with war, but since 1945 the nature of warfare has changed so profoundly that the future of the human race, of generations yet unborn, is imperilled. At the same time, mutual contacts and means of understanding between peoples of the world have been increasing. This is why the yearning for peace is now stronger than ever. Mankind is confronted today with a threat unprecedented in history, arising from the massive and competitive accumulation of nuclear weapons. The existing arsenals, if employed in a major war, could result in the immediate deaths of many hundreds of millions of people, and of untold millions more later through a variety of aftereffects. For the first time, it is possible to cause damage on such a catastrophic scale as to wipe out a large part of civilization and to endanger its very survival. The large-scale use of such weapons could trigger major and irreversible ecological and genetic changes, whose limits cannot be predicted.

Science can offer the world no real defense against the consequences of nuclear war. There is no prospect of making defenses sufficiently effective to protect cities since even a single penetrating nuclear weapon can cause massive destruction. There is no prospect that the mass of the population could be protected against a major nuclear attack or that devastation of the cultural, economic, and industrial base of society could be prevented. The breakdown of social organization, and the magnitude of casualties, will be so large that no medical system can be expected to cope with more than a minute fraction of the victims.

There are now some 50,000 nuclear weapons, some of which have yields a thousand times greater than the bomb that destroyed Hiroshima. The total explosive content of these weapons is equivalent to a million Hiroshima bombs, which corresponds to a yield of some 3 tons of TNT for every person on earth. Yet these stockpiles continue to grow. Moreover, we face the increasing danger that many additional countries will acquire nuclear weapons or develop the capability of producing them.

There is today an almost continuous range of explosive power from the smallest battlefield nuclear weapons to the most destructive megaton warhead. Nuclear weapons are regarded not only as a deterrent, but there are plans for their tactical use and use in a general war under so-called controlled conditions. The immense and increasing stockpiles of nuclear weapons, and their broad dispersal in the armed forces, increase the probability of their being used through accident or miscalculation in times of heightened political or military tension. The risk is very great that any utilization of nuclear weapons, however limited, would escalate to general nuclear war.

The world situation has deteriorated. Mistrust and suspicion between nations have grown. There is a breakdown of serious dialogue between the East and West and between North and South. Serious inequities among nations and within nations, shortsighted national or partisan ambitions, and lust for power are the seeds of conflict which may lead to general and nuclear warfare. The scandal of poverty, hunger, and degradation is in itself becoming an increasing threat to peace. There appears to be a growing fatalistic acceptance that war is inevitable and that wars will be fought with nuclear weapons. In any such war there will be no winners.

Not only the potentialities of nuclear weapons, but also those of chemical, biological, and even conventional weapons are increasing by the steady accumulation of new knowledge. It is therefore to be expected that also the means of nonnuclear war, as horrible as they already are, will become more destructive if nothing is done to prevent such a war. Human wisdom, however, remains comparatively limited, in dramatic contrast with the apparently inexorable growth of the power of destruction. It is the duty of scientists to help prevent the perversion of their achievements and to stress that the future of mankind depends upon the acceptance by all nations of moral principles transcending all other considerations. Recognizing the natural rights of humans to survive and to live in dignity, science must be used to assist humankind towards a life of fulfillment and peace.

Considering these overwhelming dangers that confront all of us, it is the duty of every person of good will to face this threat. All disputes that we are concerned with today, including political, economic, ideological, and religious ones, are small compared to the hazards of nuclear war. It is imperative to reduce distrust and to increase hope and confidence through a succession of steps to curb the development, production, testing, and deployment of nuclear weapons systems, and to reduce them to substantially lower levels with the ultimate hope of their complete elimination.

To avoid wars and achieve a meaningful peace, not only the powers of intelligence are needed, but also the powers of ethics, morality, and conviction.

The catastrophe of nuclear war can and must be prevented. Leaders and governments have a grave responsibility to fulfill in this regard. But it is humankind as a whole which must act for its survival. This is the greatest moral issue that humanity has ever faced, and there is no time to be lost.

II. In view of these threats of global nuclear catastrophe, we declare.

- Nuclear weapons are fundamentally different from conventional weapons. They must not be regarded as acceptable instruments of warfare. Nuclear warfare would be a crime against humanity.
- It is of utmost importance that there be no armed conflict between nuclear powers because of the danger that nuclear weapons would be used.
- The use of force anywhere as a method of settling international conflicts can entail the risk of military confrontation of nuclear powers.
- The proliferation of nuclear weapons to additional countries seriously increases the risk of nuclear war and could lead to nuclear terrorism.
- The current arms race increases the risk of nuclear war. The race must be stopped, the development of new, more destructive weapons must be curbed, and nuclear forces must be reduced, with the ultimate goal of complete nuclear disarmament. The sole purpose of nuclear weapons, as long as they exist, must be to deter nuclear war.

III. Recognizing that excessive conventional forces increase mistrust and could lead to confrontation with the risk of nuclear war, and that all differences and territorial disputes should be resolved by negotiation, arbitration, or other peaceful means, we call upon all nations:

- Never to be the first to use nuclear weapons;
- To seek termination of hostilities immediately in the appalling event that nuclear weapons are ever used;
- To abide by the principle that force or the threat of force will not be used against the territorial integrity or political independence of another state;
- To renew and increase efforts to reach verifiable agreements curbing the arms race and reducing the numbers of nuclear weapons and delivery systems. These agreements should be monitored by the most effective technical means. Political differences or territorial disputes must not be allowed to interfere with this objective;
- To find more effective ways and means to prevent the further proliferation of nuclear weapons. The nuclear powers, and in particular the superpowers, have a special obligation to set an

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Participants in the Conference on Nuclear Warfare included E. Amaldi, Italy I Badran, Egypt A Balevski, Bulgaria: A Berkoc, International Council of Scientific Unions, F Benvenuti, Italy, C Bernhard, Sweden, O Bikov, Soviet Union, B. Bilinski, Poland, C Chagas, Brazil E De Giorgi Italy B Dinkov, Bulgaria, G Hambraeus, Sweden, T Hesburgh and H Hiatt, United States D Hodgkin, International Pugwash Conference, S Hsieh, Taipei, A Huxley, England, S Iijima, Japan, S Isaev, Soviet Union P Jacquinot, France, W Kalweit, German Democratic Republic, M Kazi, Pakistan, S Keeny, United States, K Komarek and F Konig, Austria, J Labarbe, Belgium J Lejeune and L Leprince-Ringuet, France R Levi Montalcini, Italy, M. Lora-Tamayo, Spain, T Malone, United States, G Marini-Bettolo Italy, M Menon, India, G Montalenti, Italy, M Peixoto, Brazil, J Peters, Belgium, G Porter, England F Press United States, G Puppi, Italy, B Rifai Indonesia, W Rosenblith, United States P Rossano Italy P Rudomin, Mexico, B Rysavy, Czechoslovakia, I Saavedra, Chile, V Sardi, Venezuela T Shin, Korea, E Simpson: South Africa: J Sirotkovic, Yugoslavia, L Sosnovski, Poland, A Stoppani, Argentina, J Szentagothai, Hungary; S Tanneberger, German Democratic Republic C Townes, United States, E Velikhov, Soviet Union, W Watts, Ireland, and V Weisskopf, United States example in reducing armaments and to create a climate conducive to nonproliferation. Moreover, all nations have the duty to prevent the diversion of peaceful uses of nuclear energy to the proliferation of nuclear weapons;

● To take all practical measures that reduce the possibility of nuclear war by accident, miscalculation, or irrational action.

● To continue to observe existing arms limitation agreements while seeking to negotiate broader and more effective agreements.

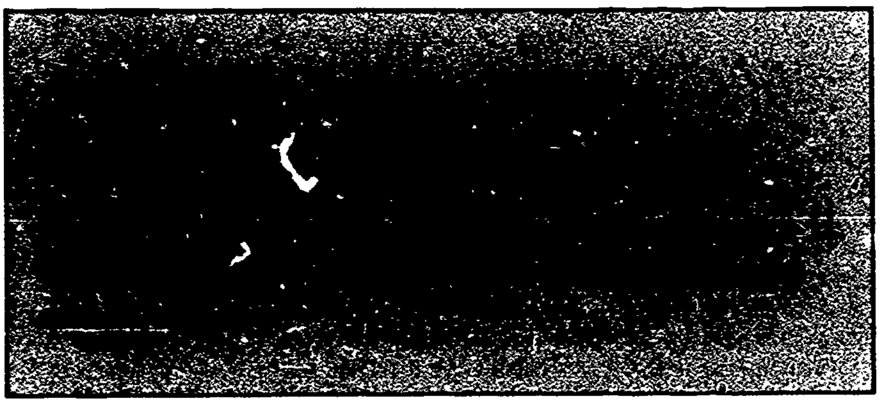
IV. Finally, we appeal:

1) To national leaders, to take the initiative in seeking steps to reduce the risk of nuclear war, looking beyond narrow concerns for national advantage; and to reject military conflict as a means of resolving disputes.

2) To scientists, to use their creativity for the betterment of human life, and to apply their ingenuity in exploring means of avoiding nuclear war and developing practical methods of arms control.

3) To religious leaders and other custodians of moral principles, to proclaim forcefully and persistently the grave human issues at stake so that these are fully understood and appreciated by society.

4) To people everywhere, to reaffirm their faith in the destiny of humankind, to insist that the avoidance of war is a common responsibility, to combat the belief that nuclear conflict is unavoidable, and to labor unceasingly towards insuring the future of generations to come.



A recent report (*1*) on the network of statutes and regulations which have been invoked by government officials to restrain unclassified research and travel and publication by academic researchers concluded that these restrictions abridge academic freedom significantly beyond the needs of national security. It was also argued that the nation's security is ill-served by the restrictions in that barriers to learning from others, as well as the suppression of innovative work whenever its originality might be useful even to the industrial or technological progress of other nations, are necessarily discouraging to the maintenance of research leadership within the United States.

A recent event tends to justify such criticism. A university professor submitted two papers for presentation, and subsequent publication, to the 26th Annual Technical Symposium of the Society for Photo-Optical Instrumentation Engineers meeting in San Diego in August 1982. The professor's research, supported by a grant from the Air Force, was not classified, in accordance with the university's stated policy "to undertake only those research projects in which the purpose, scope, methods, and results can be fully and freely discussed." As he had done routinely in the past, the professor also sent the papers to the program officer in the Air Force who told him, a week before the symposium, that his papers had not been cleared and therefore should not be presented. The professor, while vigorously protesting, withdrew the papers.

Certain research conducted in universities may have immediate and direct national security implications. Some of that work is undertaken pursuant to Department of Defense contracts. Universities generally recognize that such arrangements may compromise their commitment to academic freedom, and they vary in their policies respecting the wisdom and acceptability of such arrangements. The American Association of University Professors (AAUP) has thought it inappropriate to condemn faculties and universities for making such arrangements per se, but it has regularly expressed concern that inconsistency with respect to academic freedom is a genuine danger that all academic institutions should weigh carefully in the research and restrictions they accept.

The implication of the earlier report (*1*) was to favor a limited classification system, to the extent that it might minimize uncertainty and provide a less random threat to academic freedom. Ideally, a clear and circumspect classification system should state what research and publication must necessarily be treated in confidence according to needs of national security that are plain and compelling. It should enable universities and their faculties to make informed decisions about their research. Very different, and strongly objectionable, is a classification system that sweeps within it virtually anything that might conceivably be useful industrially, technically, or militarily to at least someone and that is administered by officials who feel compelled to classify as secret any information about which they have doubts.

Here we review briefly the recent changes introduced into the classification system by Executive Order 12356, issued by President Reagan on 2 April 1982. A recent report of the National Academy of Sciences Panel on Scientific Communication and National Security (*2*) concluded that a national policy of security through openness is much preferable to a policy of security by secrecy. We agree. We believe the enlargement of the classification system as stated in Executive Order 12356 is seriously mistaken. It poses an unwarranted threat to

This article is adapted from a report issued in October 1982 by the American Association of University Professors' Committee A on Academic Freedom and Tenure. The report was prepared by Committee A's Subcommittee on Federal Restrictions on Research. The members of the subcommittee are R. A. Rosenbaum, professor of mathematics, Wesleyan University, Middletown, Connecticut 06457, *Chair*; M. J. Tenzer, professor of political science, University of Connecticut, Storrs 06268; S. H. Unger, professor of computer science, Columbia University, New York 10027; W. Van Alstyne, professor of law, Duke University, Durham, North Carolina 27706; and J. Knight, associate secretary, American Association of University Professors, Washington, D.C. 20036.

academic freedom and hence to scientific progress and the national security.

Summary of Recent Changes

Executive Order 12356 is the most recent presidential executive order prescribing a system for classifying and declassifying information on the basis of national security concerns. President Franklin Roosevelt issued the first such order in 1940. Succeeding executive orders were signed by Presidents Truman, Eisenhower, Nixon, and Carter. In their details, these earlier executive orders differed on such matters as what information was to be classified, for what period of time, and according to what standards. Their similarities, however, are more noteworthy than their differences. They sought to preserve the public's interest in the free circulation of knowledge by limiting classification authority, by defining precisely the purposes and limits of classification, and by providing procedures for declassification.

By contrast, Executive Order 12356 significantly broadens the authority of government agencies to classify information as secret. It removes a previous requirement for classification that damage to the national security be identifiable. It resolves doubts about the need to classify in favor of classification. It permits indefinite classification. It provides for reclassification of declassified and publicly released information. It expands the categories of information subject to classification to include nonclassified research developed by scientific investigators outside the government.

Main Provisions

The preamble to Executive Order 12356 states that the "interests of the United States and its citizens require that certain information concerning the national defense and foreign relations be protected against unauthorized disclosure." To prevent "unauthorized disclosure," the order establishes three levels of classification: top secret, secret, and confidential. The standards for top secret and secret are the same as in previous executive orders. However, Executive Order 12356 omits the earlier qualifying word "identifiable" in describing the damage to the national security that can justify classification at the lowest, or confidential, level. The text reads: "confidential shall be applied to information, the unauthorized disclosure of which reasonably could be expected to cause damage to the national security." At a congressional hearing, a Deputy Assistant Attorney General explained the deletion of the requirement of identifiability as follows:

> Every new qualifier or adjective, such as "identifiable," added to the requirement of showing "damage" or any other requisite element of proper classification, raises new uncertainties or areas of ambiguity that may lead to litigation. . . . [T]he requirement of "identifiable" damage may be construed to suggest that disclosure must cause some specific or precise damage, a requirement that the government might not reasonably be able to meet in some cases. . . . Provisions of such orders should be simple, general, less complex and require no more precision than the subject matter reasonably allows. The requirement of "identifiable" damage fails on all these counts.

In the event that a government official is uncertain about the security risk of some information, the doubt will be resolved in favor of classification pending a final determination within 30 days. In addition, if there is doubt about the level of classification, the information will be classified at a higher level, also pending a final decision within 30 days. Once the information is classified, it can remain so at the discretion of government officials "as long as required by national security considerations." There is no provision in Executive Order 12356 for justifying the need for classification beyond a stated period of time. (President Nixon's executive order called for automatic declassification after 30 years, unless it was determined that continued classification was still necessary and a time for eventual declassification was set; President Carter's executive order established a 6-year declassification period.) The latest order makes no comment on whether declassifying information is generally desirable.

If information is declassified, it may be reclassified under Executive Order 12356 following the requirements for classification. Information that has been properly declassified and is in the public domain apparently may remain "under the control" of the government (the order defines information as "any information or materials . . . that is owned by, produced by or for, or is under the control of the United States Government") and thus can be reclaimed by the government.

The executive order provides for limitations on classification. It states that "basic scientific research information not clearly related to the national security may not be classified." Early drafts of the order had not included this provision; it first appeared in the executive order issued by President Carter. It was retained mainly as a result of protests from the scientific community. However, it is not clear what this provision actually safeguards.

Sanctions for violations of the executive order may be imposed on the government's "contractors, licensees, and grantees."

Comments

National security obviously requires some classification of information as secret. It is also obvious that freedom to engage in academic research and to publish the results is essential to advance knowledge and to sustain our democratic society.

The possibility for friction between classification and academic freedom is always there. The friction can be reduced if classification is invoked before research has begun and is cautiously applied for a limited period of time and only to matters of direct military significance. Classification defeats its own purpose, however, if it imperils the freedoms it is meant to protect. In our judgment, Executive Order 12356 does exactly that. It gives unprecedented authority to government officials to intrude at will in controlling academic research that depends on federal support. It allows classification to be imposed at whatever stage a research project has reached and to be maintained for as long as government officials deem prudent. Academic research not born classified may, under this order, die classified.

The provision in the executive order that "basic scientific research information not clearly related to the national security may not be classified" carries the suggestion that it may be classified if it is determined by the government to be "clearly related to the national security." This standard for classification is looser still than "could be expected to cause damage to the national security." We may be reading too much into this provision; we hope that it will be interpreted as an exemption and nothing more. Unfortunately, even with its most favorable gloss it is a weak safeguard for scientific inquiry. The government official who cannot fix a clear relationship between scientific research and national security but nonetheless has doubts could still classify government funded or contracted research consistent with other provisions in the executive order.

In the pursuit of knowledge, academic researchers should not have to look backward either in hope of favor or in fear of disfavor. In an era of reduced federal support for research except in the area of national security, and with investments in research programs and facilities significantly reliant on previously allocated federal funds, academic researchers are under great pressure to submit to classification no matter how restrictive or apparently arbitrary the demand. The adverse effects on academic freedom and thus on the advancement of knowledge and on the national security can be grave.

The executive order can inhibit academic researchers from making long-term intellectual investments in research projects that are potentially classifiable. It can serve to foster unnecessary duplication of research efforts. It is likely to inhibit the sharing of research methods and results with professional colleagues, because something that a government official can call harmful to the national security might unwittingly be revealed. Classification, or the worry that it might be imposed, could result in the isolation of academic researchers, cut off from the free exchange of ideas and exposure to constructive criticism. Those concerned in government with the uses of new knowledge are not likely to obtain the benefit of the widest possible evaluation of their plans and projects. All of these consequences of the executive order are likely to be felt outside as well as within the field of research in which classification is imposed.

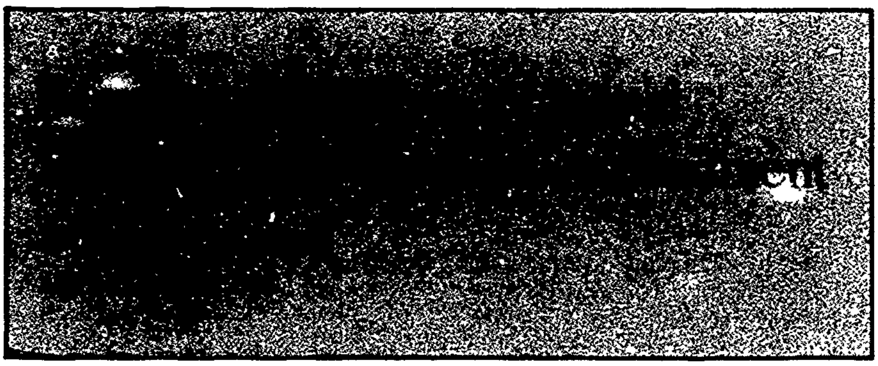
The government has not put forward any compelling reasons for instituting a system of classification that is so at odds with previous systems. The government's own reports, including reports issued by the Department of Defense, seriously question the cost, effectiveness, and need for more classification. They draw particular attention to the dangers of overclassification.

Executive Order 12356 requires drastic revision in order to be tolerable to a community of scholars committed to free inquiry. The application of the order to nonclassified information, which is already subject to potential restraints under existing laws and regulations, is at best superfluous. The heavy emphasis on classification is misplaced: the provision for reclassification should be removed and the standards for classification rewritten so that they do not sweep unnecessarily broadly and thereby significantly threaten academic freedom.

If the government's executive order or its successor continues to deny due recognition to the need of the independent research scholar for academic freedom, the cost will be borne not only by the researchers who are affected but by the nation as a whole.

References and Notes

1. R. A. Rosenbaum, M. J. Tenzer, S. H. Unger, W. Van Alstyne, J. Knight, "Federal restrictions on research: Academic freedom and national security," *Academe: Bull. Am. Assoc. Univ. Prof.* **68**, 17a (September-October 1982).
2. National Academy of Sciences, *Scientific Communication and National Security* (Washington, D.C., 1982), vols. 1 and 2.
3. The present address of S. J. Unger is Thomas J. Watson Research Center, P.O. Box 218, Yorktown Heights, N.Y. 10598.



It is now apparent that the American scientific community is approaching a critical point in its relations with the federal government. Until recently, the conduct of most scientific work in this country proceeded on a well-founded assumption that it would remain free from official intrusion or state regulation (*1*). Since 1979, however, the federal government has frequently acted in the name of national security to impose restraints on important aspects of the scientific endeavor. Most notably, in an effort to curb the export of "militarily useful" technologies, the Administration has applied the existing set of export controls to domestic scientific symposiums, university research programs, and even the presentation of scientific papers (*2*, pp. 97–107; *3–5*).

This effort to restrict the dissemination of applied scientific knowledge has sparked heated debate. The government maintains that the normal avenues of scientific communication often contribute to a "technology leakage" that enhances the military capabilities of the Soviet Union (*6, 7*). What underlies this view is the belief that the American military must depend on the technological superiority of its weapons systems to offset the quantitative superiority of the Soviet Union (*6, 7*). The critics charge that restraints on scientific expression are both ineffectual as a means of curbing the transfer of technology and inconsistent with the requirements of scientific progress (*2*, pp. 42–45; *4, 8*). In this view, America's technological supremacy is due in large part to policies that promote the free circulation of scientific and technological information.

The debate has thus far addressed the government's effort to control the export of applied scientific knowledge as a broad question of public policy. It seems likely, however, that the major issues in the controversy will soon be tested under narrower, legal principles in a court of law. If so, the government will almost certainly rely on one of two congressional statutes as authority for its restraints on the transmission of technological knowledge.

One of the statutes is the Arms Export Control Act (*9*), which empowers the State Department to license the export of all military articles listed in the International Traffic in Arms Regulations (*10*). As defined by those regulations, the relevant articles consist not only of war-making devices such as aircraft and explosives but also of "any information" used in the production of military arms (*10*, sect. 125.01). Equally important, the regulations broadly construe the term "export" to include the noncommercial transmission of information in domestic settings such as scientific symposiums (*10*, sect. 125.03; *11*).

The other statute is the Export Administration Act of 1979 (*12*), which differs from the arms regulations in two respects. First, it authorizes the Com-

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merce Department to license the export of "dual use" technologies that are subject to both military and civilian applications. Second, it deals principally with the export of technologies to "controlled countries" such as the Soviet Union, Poland, and East Germany. Like the arms regulations, however, the Export Administration Act restricts the domestic release of any information used in the production of commodities having a military value (*13*). Furthermore—and again like the arms regulations—the Export Administration Act imposes stiff criminal penalties on those who willfully violate its licensing requirements (*12*, sect. 2410).

In these statutes Congress has provided considerable authority for governmental restraints on the export of "militarily useful" technologies. This fact alone, however, will not end the legal inquiry in cases where the government has invoked the statutes to restrict the open, domestic communication of applied scientific knowledge. On the contrary, in such a case, a major issue will arise concerning the validity of the legislation under the free-speech clause of the First Amendment.

To resolve this type of issue, the Supreme Court has consistently relied on a well-defined analytical framework designed to determine whether the state's interest in regulation is sufficiently important to justify an abridgment of First Amendment freedoms. In the rest of this article, I will examine the ways in which the Court's mode of analysis can accommodate the difficult First Amendment issues arising from the imposition of restraints on the open, domestic communication of technological knowledge (*14*).

First Amendment Fundamentals

Like other guarantees in the Bill of Rights, the free-speech clause of the First Amendment stakes out a zone of individual freedom by identifying a specific activity to be protected against unwarranted governmental intrusion. The enforcement of such guarantees is left to the Supreme Court, the branch of government removed from public accountability and vested with the power to invalidate official acts that encroach on the protected freedoms. This power of judicial review, however, carries the risk that the Court will frustrate the democratic process by freely substituting its own preferences for the enacted will of the public's elected representatives. Accordingly, under prevailing constitutional theory, the Court's power is properly exercised only when its decisions are rigorously based on principles derived from the text of the Constitution (*15*, *16*).

These larger considerations have often guided the Court in deciding cases arising under the free-speech clause of the First Amendment. Rejecting the notion that all speech is absolutely immune from official regulation, the Court has determined the degree of protection to be accorded to various categories of expression by looking to the major values that underlie the free-speech guarantee. These values, according to the Court, can be summarized in three propositions. First, the right of free speech advances the citizen's interest in self-fulfillment by enabling him to realize his full potential through the free expression of opinions, beliefs, and ideas. Second, the guarantee of free speech serves an important social function by promoting the widest possible circulation of socially useful information. Finally, the right of free speech is essential to a democratic form of government, for it ensures that all information bearing on various policy issues is fully disseminated to the public (*17*, *18*).

Though the Court has not yet adjudicated the issue, it seems clear that scientific communications contribute to each of these interests and thus warrant as much protection as political tracts, literary works, or any other variety of speech. Indeed, a system of free scientific expression not only enables scientists to draw on the work of colleagues but also tests the validity of hypotheses against current data and opposing views. In these ways, it promotes the discovery of scientific truth and fosters the intellectual advances that contribute to the collective wisdom (*2*, pp. 42–45; *19*, *20*).

In the case of purely technical data, however, more difficult questions arise. For example, does technical information having only military uses warrant the same degree of constitutional protection as political speech or basic scientific knowledge? In all likelihood the Court will answer in the negative, for it has previously held that analogous "lesser" forms of expression do not stand on the same constitutional footing as more traditional varieties of speech. For example, the Court has held that commercial advertising occupies a "subordinate position in the scale of First Amendment values" and thus warrants only a "limited measure" of constitutional protection (*21*; *22*, pp. 651–656).

Most forms of technological knowledge, however, are subject to a wide range of uses, some of which have military value but most of which contribute directly to the material welfare of the community. This point is clearly illustrated by many of the "militarily critical" technologies that have been cited by the Department of Defense—for example, laser technology, semiconductors, computer hardware, and infrared technology (*23*). Given the obvious social value of such technological achievements, the Supreme Court will probably hold that the broad category of technological knowledge warrants a full measure of constitutional protection, while noting an exception for information that is subject only to military applications (*19*).

Once this larger question is decided, the Court will not assess the social value of the technical data at issue in a given challenge to a governmental restraint. Rather, it will simply note that the information in question falls within the category of fully protected speech and will then turn its attention to the government's countervailing interest in regulation. At this point, a crucial issue will arise: given the strong constitutional presumption in favor of free speech, just what burden of proof must the state carry to justify its imposition of restraints on the information? Or, to put it in legalistic terms, what standard of review will the Court apply to the government's stated justification for the challenged restrictions?

Determining the Standard of Review

To determine the relevant standard of review, the Court will focus on two broad questions. First, does the government have a possessory interest in the underlying information? If so, the Court will apply a mere "reasonableness" standard to any governmental restraints imposed on government employees in an effort to preserve the secrecy of the data. Thus, for example, in *Snepp* v. *United States*, a recent case involving a book published by a former CIA agent, the Court broadly upheld the state's power to impose "reasonable restrictions" on the dissemination of governmental information obtained by government employees (*24*). In addition, the Court pointedly noted that this general principle applies "even in the absence of an express agreement" between the government and the employee (*25*, p. 507).

In like manner, the Court will probably sustain any reasonable restraints imposed on the dissemination of information resulting from the government-funded research of private parties. Indeed, in such a case, the government's restraints will likely be upheld on either of two grounds: (i) the state, by financing the underlying research, acquires a property interest in the resulting information or (ii) the researcher, by accepting the public financing, agrees to restrictions that might otherwise be constitutionally impermissible (*19, 26*).

On the other hand, if the state attempts to regulate the dissemination of nongovernmental information by private parties, the Court will apply a far more demanding standard of review. In such a case, the weight of the state's burden will be determined by a second line of judicial inquiry focusing on the precise way in which the government has restricted the free-speech right.

On this issue, there are two major possibilities: either the state has imposed a "subsequent punishment"—usually in the form of criminal penalties—on individuals who have already published the restricted information, or it has blocked the dissemination of the data by issuing a "prior restraint." In the case of a subsequent punishment, the Court will uphold the action only if the state can demonstrate a "compelling" interest in regulation (*22*, p. 602; *27*)—a burden of proof that stands as the modern analog of the well-known "clear and present danger" test formulated by Oliver Wendell Holmes (*28*). In the case of a prior restraint, the Court will apply an even more demanding standard of review, since the government is seeking to block the timely dissemination of information and ideas. Indeed, on the evidence of the so-called *Pentagon Papers* decision (*New York Times* v. *United States*) the Court will uphold the restraint only if the government can show that a "grave" and "irreparable" harm will almost surely result from publication of the data in question (*29*).

Clearly, under either standard of review the state is faced with an exceedingly difficult task. Nevertheless, the Court has indicated that in some "exceptional" cases, principally in the area of national security, the government's interest in regulation may be sufficient to warrant a direct infringement on fully protected speech (*29*). The remaining question, therefore, is: Just how will the Court assess the importance of the state's concerns to determine whether they are adequate to justify an abridgment of First Amendment freedoms?

Weighing the State's Interest in Regulation

The Court has held that the strength of the government's interest in regulation is determined in large part by two independent factors: the nature of the harm that the state is seeking to avert and the likelihood of its occurrence (*30*, p. 843). In particular, the crucial inquiry centers on whether the "gravity of the 'evil,' discounted by its improbability, justifies such invasion of the free speech right as is necessary to avoid the danger" (*31*). With this approach, the seriousness of the threatened danger will affect to some extent the showing required of the government on the "likelihood of occurrence."

The Court has long recognized that "no governmental interest is more compelling than the security of the Nation" and that this interest sometimes requires the state to protect the secrecy of certain kinds of information (*24*). On the facts of a given case, however, the state could not rely on the mere assertion of a national security threat, for the Court will make its own inquiry into the nature and magnitude of the harm said to result from publication of the data at issue (*30*, p. 843).

The state's argument on this score will undoubtedly stress the unique nature of technical knowledge and, in particular, the unique way in which this variety of speech can harm the public. Under classic First Amendment theory, most forms of human communication contribute to the larger social exchange of opinions, beliefs, and ideas and do not threaten in any way the material welfare of the society. Indeed, on this theory, the speech of an individual generally cannot cause any harm to the community except by influencing others to adopt an erroneous or misguided position. The theory further holds that the government has no genuine interest in suppressing a "dangerous" idea, since the alleged error or fallacy can be exposed through an additional exchange of views (*22*, pp. 605–606; *32*).

These general considerations, however, do not always apply to technological knowledge, which often gives rise to dangers of a more immediate and tangible kind. In particular, technical know-how, although rarely contributing to the general exposition of ideas, often confers the power to alter the material conditions of life in important new ways, some of which may prove harmful (*19, 20, 33*). For example, in the case of new technologies having military applications, the underlying know-how can provide a hostile nation with the capability of committing harmful acts it would not otherwise be able to commit.

This point was clearly underscored by the decision of a federal district judge in *United States* v. *The Progressive* (*34*). In that case, the government asked the judge to enjoin a magazine from publishing an article outlining the design of a hydrogen bomb. In granting the injunction, the judge stressed that the case differed in important ways from the *Pentagon Papers* case, which dealt with a classified history of the Vietnam War (*29*). Most notably, according to the judge, the case before him concerned "information dealing with the most destructive weapon in the history of mankind, information of sufficient destructive potential to nullify the right to free speech and to endanger the right to life itself" (*34*). Thus convinced that publication "could pave the way for thermonuclear annihilation of us all," the judge found that the government had met its heavy burden of justifying a prior restraint (*34, 35*).

In the case of nonnuclear technologies, the government has also invoked the name of national security to limit the dissemination of technical information having possible military applications. For example, at a recent international symposium on optical engineering, the Department of Defense blocked the presentation of a large number of unclassified papers on topics ranging from microelectronics to infrared technology (*2*, pp. 106–107; *36*). In so doing, the department underscored its concern that advanced work in "critical" technologies could aid a foreign adversary in the development of more effective weapons systems (*36*). The National Security Agency has recently monitored the efforts of research cryptographers to develop undecipherable computer communication codes (*2*, pp. 120–125). According to agency officials, the free publication of this work could threaten the inviolability of codes used by the American military or provide a hostile power with an impenetrable communication system (*2*, p. 123; *7*).

In the light of these examples, it is useful to rank the various types of national security information according to the nature and magnitude of the dangers posed by the resulting capability. This effort applies, however, only to those cases in which the government has first demonstrated two important points: (i) that the information at issue is indeed subject to the asserted d ngerous use and (ii) that the information is not currently available to the receiving nation from another source (*19*).

Assuming these facts can be established, the most serious danger would arise from technical capabilities that could alter in major ways the current balance of international military power. This category would include technologies that directly conferred on the Soviet Union a new offensive capability or an effective countermeasure to American weapons systems. It would also include technologies that exposed the United States to new threats by providing a smaller adversary with a destructive power that it had not possessed before.

These are examples of "sudden and disastrous giveaways" (*37*). There are other capabilities that, if acquired by a hostile nation, could result in a number of lesser harms to the nation's security. Most significant is the wide range of militarily useful technologies that could enable a foreign adversary to add incrementally to its current military strength by (i) directly improving the performance of its weapons systems, (ii) enhancing its communications network, or (iii) increasing its knowledge of American military capabilities (*38*). Examples of such technologies are electrooptical sensors, solid rocket propulsion systems, satellite technology, navigation and guidance subsystems, microprocessors, and microelectronics (*2*, pp. 18–20; *6*, pp. 5–15).

A less immediate harm would result from technologies that enabled a foreign adversary to improve its military research and development. The most significant are technologies associated with the use of the computer for correlating experimental data with theoretical models (*39*). Other well-defined technical methodologies are used to "guarantee reliability, explore the limits of design, and reveal new phenomena that can affect the next generation of weapons" (*39*).

A slightly different harm to national security would result from technologies that enabled a foreign power to upgrade its manufacturing capability in industries of military importance. For example, microelectronics and computer technologies are important in the development of in-flight guidance systems (*6*, p. 13), while precision ball bearings are important in the production of missiles and other military hardware (*6*, p. 7).

Finally, it is possible that the export of some technical capabilities could undermine foreign policy goals that are closely linked to the nation's security. The export of some types of technical knowledge, for instance, might undermine a trade embargo designed to influence the international behavior of the Soviet Union.

Turning to the question of the likelihood of occurrence, the Court will address the probability that a third party will use the information at issue to develop the new capability. This line of inquiry will consider both the complexity of the technology and the skills of the receiving nation. The need for the inquiry arises in part from the fact that the impersonal transmission of technical knowledge is rarely an effective method of transferring technology (*38*; *40*, p. 29). As a general rule, the normal channels of intellectual communication convey only the broad outlines of technical design and theory (*40*, pp. 67–73). What is usually not published or codified is the body of associated know-how that constitutes the art of the technology (*40*, p. 73), typically including methods of operation, organization, and manufacturing procedures. This is particularly true of emerging technologies with few previous applications (*40*, pp. 73–74).

Accordingly, the Court's inquiry into the likelihood of occurrence will focus on the ability of the receiving nation to absorb the knowledge at issue and put it to use. For example, if the receiving nation has a high level of technical expertise in the relevant area, the government could show with virtual certainty that that nation will put the information to an immediate military use. If the receiving nation lacks any of the needed skills or resources, the state could show only a possibility that the knowledge will be put to a significant use in the foreseeable future.

Together with the gravity of the threatened harm to national security, the Court's finding on the likelihood of occurrence will generally determine whether the state's interest in regulation is sufficient to warrant the restriction of First Amendment rights. Assume, for instance, that the government can show that the Soviets have sufficient skills to acquire a new military capability by exploiting an American breakthrough in directed energy weaponry. On these facts, the Court will no doubt agree that the government's concerns are sufficiently compelling to warrant an abridgment of First Amendment freedoms. This will probably hold true, moreover, even if the government concedes that the Soviets will eventually acquire the capability anyway, since the maintenance of a military lead time can be highly advantageous (*41*). On the other hand, if the threatened harm to the nation's security is less serious, the state's case will be correspondingly weakened, and all the more so if the receiving nation is shown to lack the requisite skills or resources to absorb the technology.

Less Restrictive Alternatives

The crux of conventional First Amendment analysis lies in the Court's effort to determine whether the restricted information gives rise to a substantial danger and thus warrants governmental regulation. However, if this issue is resolved in the state's favor, the Court will pursue a further line of inquiry focusing on the government's regulatory technique. In particular, the Court will determine whether the restraints on speech imposed by the state are more extensive than necessary to serve its underlying concerns (*42*). Accordingly, even if the government can demonstrate a "compelling" interest in regulation, the Court will invalidate the challenged restraints if it finds that a "less restrictive alternative" could serve the asserted interest equally well.

A useful illustration of this principle is offered by *Central Hudson* v. *Public Service Commission of New York* (*43*). In that case, the Public Service Commission of the state of New York issued an order prohibiting all public utilities from promoting the use of electricity. The commission reasoned that such a ban would decrease the demand for electricity and thus further the state's interest in the conservation of energy resources. The Supreme Court agreed that this interest was sufficient to warrant some restriction of commercial speech but found that the state's blanket prohibition was more extensive than necessary to further that interest. The Court noted, for example, that the commission's order prevented utilities from promoting electrical services that would reduce energy consumption by diverting demand from less efficient sources. On this ground, therefore, the Court found that the commission's order was unconstitutional.

This type of inquiry might well become relevant if recent proposals to alter the export control statutes are passed into law. For example, under one such proposal (*44*), the Arms Export Control

Act would be amended to cover communications of any kind—technical or otherwise—dealing with any of a broad range of restricted technologies (*4*). This type of regulation, however, would clearly be more extensive than necessary to safeguard the nation's security, since many communications dealing with the restricted technologies have no military value. Consequently, any regulatory scheme based on this proposal would be subject to a stern First Amendment challenge on the grounds that there are less restrictive alternatives.

Conclusion

What is most striking about the Court's method of First Amendment adjudication is that it takes into account virtually all the commonsense perceptions that have informed the general policy debate on the government's effort to control the export of scientific and technical knowledge. Indeed, if the Court applies its standard analysis to this issue, it will not only give due weight to the value of scientific freedom but will also examine critically the nature and magnitude of the threatened harm to national security. In addition, it will address a variety of other considerati ins, such as the technical skills of the receiving nation and the reasonableness of the regulatory technique. By incorporating each of these factors into a method of adjudication that formally allocates the burden of proof, the Court's approach provides a well-defined analytical framework for accommodating the claims of scientific freedom with the legitimate interests of national security.

References and Notes

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8. W. D. Carey, *Science* **215**, 139 (1982).
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10. International Traffic in Arms Regulations, *Title 22, Code Fed. Reg.*, parts 121–128 (February 1976).
11. Specifically, the regulations state that an "export" occurs "whenever technical data is *inter alia* . . . disclosed to foreign nationals in the United States (including plant visits and participation in briefing and symposia)" (*10*, sect. 125.03).
12. Export Administration Act of 1979, *50 U.S. Code Appendix*, sect. 2401–20 (1979).
13. *Title 15, Code Fed. Reg.*, part 379 (1982).
14. The Supreme Court has not yet decided whether the First Amendment protects the speech of American citizens in foreign countries [*Haig* v. *Agee*, 453 U.S. 280, 306–310 (1982)].
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19. J R Ferguson, *Harv. Civ Rights—Civ Liberties Law Rev.* 16, 519 (1981).
20. ———, *Cornell Law Rev* 64, 639 (1979)
21. *Ohralik* v. *Ohio State Bar Assoc.*, 436 U.S. 447, 456 (1978).
22. L Tribe, *American Constitutional Law* (Foundation Press, Mineola N Y , 1978).
23. Office of the Secretary of Defense, *Fed. Reg* **45**, 65 014 (1 October 1980).
24. *Snepp* v *United States*, 444 U S 507 (1980) See also *Haig* v *Agee* 453 U.S 280 (1981)
25. *Snepp* v. *United States* (*24*). One caveat should be noted here The Court will apply a more rigorous standard of review to governmental restraints imposed on a publisher who has received confidential information from a government employee (*30*).
26. Given the government's current emphasis on curbing the transfer of technology, it seems likely that the Department of Defense will be placing an increasing number of contractual restrictions on research that it supports (*3*).
27. *First National Bank of Boston* v *Bellotti*, 435 U S 765 (1978).
28. *Schenck* v. *United States*, 249 U.S. 47 (1919).
29. *New York Times Co.* v. *United States*, 403 U.S. 713 (1971) (per curiam). In the *Pentagon Papers* case, the Court found that the government's claims of grave harm to the national security were insufficient to justify a prior restraint on the publication of a classified history of U.S. activities in Vietnam.
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33. H. Jonas, *Philosophical Essays* (Univ. of Chicago Press, Chicago, 1974).
34. *United States* v. *The Progressive, Inc.*, 467 F.Supp. 990, 995 (W.D. Wis.), appeal dismissed, 610 F.2d 819 (7th Cir. 1979).
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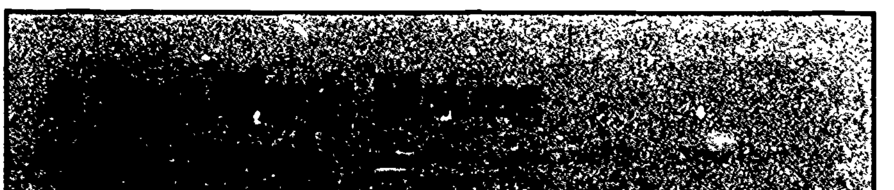

It has been said, perhaps too often and too loudly, that science is an objective process one that is value-free. In our time, when science is being employed most conspicuously as an adjunct of politics and strategic national purposes, a vacuum of internal values tends to be invaded by prevailing external values. Not surprisingly, the eventual recognition of what is taking place produces a level of discomfort that expresses itself, within the strictures of science's methodologies, in concerted displays of scientific responsibility. The conscience of science comes, a step at a time, to life.

Despite admonitions from Rome that believing scientists have the duty to look themselves in the eye when they apply brainpower to weapons systems, scientists are justified in doing what is necessary to offset the unmistakable progress of an unpredictable adversary. But what must be added is that scientific responsibility has another dimension, and it is to look squarely at the consequences of violence in the application of scientific knowledge.

It has been a very good thing for the integrity of science, and a sign of courage, that some 40 scientists of high standing have gone public with their considered estimates of the global atmospheric effects and long-term biological consequences of nuclear war(*1*). Whether such a weapons exchange would be small or vast in its scale, they believe, the effects on the biosphere would be lasting and literally deadly. In effect, life-support systems would be cut, and the diminished surviving populations would have little chance in a darkened and sunless environment.

Some four decades ago in the heat of war and its enforced secrecy, scientists prepared the nuclear weapons that we, exploded without warning upon civilian populations. It says a good deal for the emergence of the scientific conscience that, in a difficult age of superpower hatreds and technological gusto, the present warning is timely, unvarnished, and stark. Nor is it the first of its kind. Health scientists have made clear the absurdity of assuming that there would be a medical care system after a major attack and have been stumping the country to put the message across.

There remains the question of who is listening and how deeply these warnings penetrate and adhere to the nation's thought. For a few days, the news of potential biological catastrophe is the stuff of media prominence, only to be quickly displaced by the next catastrophe. The society is exhausted and news-numbed. No special session of the U.N. General Assembly is called to digest and reflect on the appalling meanings of the scientists' findings. If alarms have shaken the American and Soviet tacticians ostensibly seeking a breakthrough in nuclear arms control negotiations, it is a well-kept secret. The drift continues, and the world is ablaze with "small" wars and threats of larger ones. What does this signal to concerned scientists? For all that is obvious about science as a universal force, as a trusted partner in the works of society and governments, can it be supposed that science cannot make a difference in the one matter that transcends all the others? This is not a conclusion that scientists will swallow.

Among the endless arguments centering on arms control agreements, no issue is more vexing than that of verifying compliance, especially as new weapons are promised to the arsenals of both sides. What the cluster of scientists concerned with biological effects have done very well is to nail down, as far as scientific method can do it, the probabilities of consequences of an exchange of nuclear weapons on the biosphere. Even allowing for the constraints imposed on scientific opinion in the Soviet Union, it is fair to assume that the same conclusions are held in that quarter. Here, then, is a new basis for dialogue and for an alternative run at restraint. It is a run worth making.

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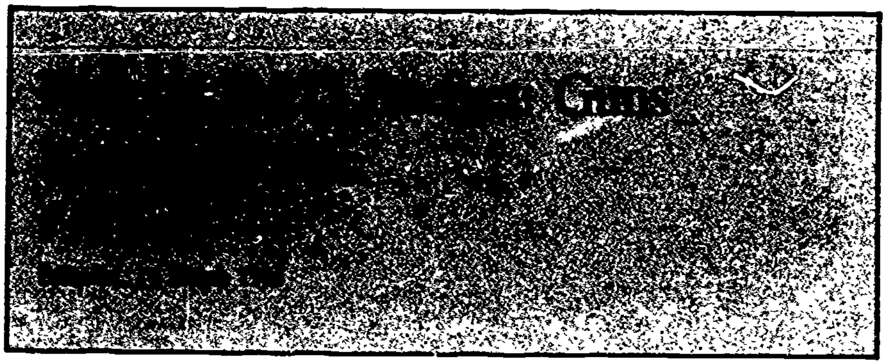

U.S. scientists and engineers are generally aware that federal funding for R&D for the military has increased sharply in recent years. What is less appreciated is that federal funding for the rest of the nation's R&D effort has considerably decreased. Using words from a classic phrase, R&D funding for "guns" is up and R&D funding for "butter" is down.

The National Science Foundation compilation *(1)* of federal R&D funding for fiscal years 1980 through 1984 by budget function, corrected for inflation with official deflators (fiscal 1984 is set at 100), reveals the following in billions of constant dollars

Category	Fiscal year budget					Increase 1980–1984 (%)
	1980	1981	1982	1983	1984	
Total R & D	$39 0	$39 2	$39 6	$40 4	$45 7	17
National defense	$19.4	$21.7	$24 2	$26 2	$32 0	65
All other R & D	$19.6	$17.5	$15 4	$14.2	$13.7	−30

Figures for fiscal 1983 and 1984 are estimates. However, the opposite trends of support are obvious. NSF lists 15 nondefense budget functions that obtain federal support for R&D. Of these, only one, general science, which is primarily basic research, shows an increase in constant dollars between fiscal 1980 and 1984, the 4 -r increase is a modest 7 percent. In P dent Reagan's recent budget proposals for R&D for fiscal 1985, the dominance of funding for the military continues.

The rapid increase in R&D for the military is not surprising; it was almost inevitable, given the large expansion of military budgets. The surprise is the magnitude of the decrease in support of nondefense R&D. This has occurred in the face of rising concern about the international competitiveness of our industries and the need for increasingly innovative U.S. technology. One response to these concerns was passage of the Economic Recovery Tax Act of 1981, which provided U.S. industry with a 25 percent annual tax credit for incremental R&D expenditures. Partly as a result, industry funding of R&D rose between 1980 and 1984 at about 6 percent per year in constant dollars.

There are fields of effort where contributions by industry are small or fragmented and where federal support of R&D is essential. These include health (other than drugs), energy, housing, agriculture, environmental protection, and natural resources. Basic research, which supplies the fundamental knowledge on which industrial R&D builds, also requires federal support, since industry's contribution is slight.

Will the pressure for increased military R&D ease soon? The answer is almost surely no, since large increases in budgets for the military are proposed for the next several years, and there is no reason to expect the fraction for R&D to decrease. The most likely future is intensified pressure on all other federal budgets, including those for R&D. What then is to be done to obtain more adequate federal support for civilian R&D? Three efforts suggest themselves: develop more persuasive arguments to federal agencies and Congress on the need for more support for R&D in nonmilitary areas, emphasize the need for more basic research, particularly in areas that supply the scientific base for our industries; and urge greater effectiveness in the federal government's civilian R&D support programs, with less emphasis on such research spectaculars as the Manned Space Laboratory and tighter constraints on the burgeoning expenditures for military R&D.

Scientists and engineers have a particular responsibility to understand these problems and make their recommendations known. What is at stake is the future prosperity of our nation.

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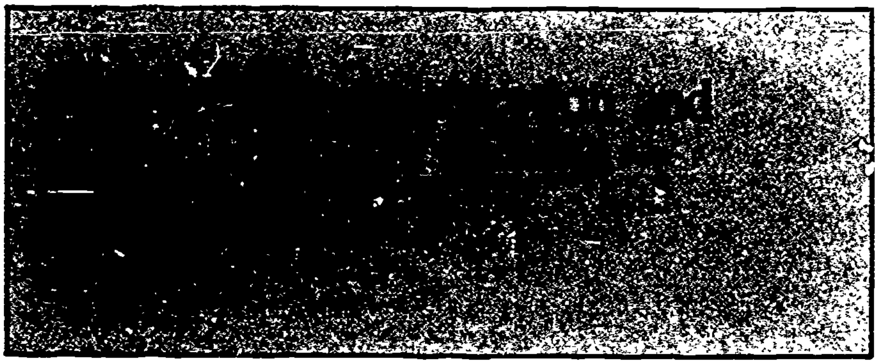

In recent years, U.S. national security has come to depend increasingly on lead time over its adversaries in areas of high technology—that is, areas in which technological progress is closely related to advances in basic science. Many of these technologies (for example, high-speed electronics and computer-based encryption techniques) have commercial as well as military applications. This has led some in the past and current administrations to fear that the open U.S. research community could become a source of militarily significant technology to the Warsaw Pact.

Such concerns were reflected both in policy proposals and public pronouncements aimed in part at the scientific community. Public statements in early 1982 included warnings by senior officials in the Department of Defense, the Department of Commerce, and the Central Intelligence Agency that Soviet military intelligence might find easy access to sensitive information through what one called the "soft underbelly" represented by the American academic community (1).

Conflicting Perspectives on the Problem

There are sharply differing views within our society regarding the extent of technology loss and the best solutions to the problem. Some interests speak with a louder and more effective voice than others in terms of the formulation of Administration policy, recognizing that there is a good deal of overlap in the emphasis accorded by each.

Clearly, what lies at the heart of public debate is concern over the loss of military advantage the United States might suffer if it does not block the efforts of the Soviet Union and its allies to gain access to Western science. Some who are concerned with national security believe the speed and seriousness of this technology transfer require new policies and reassessment of current regulations. The concern here is really twofold. First, by targeting Western research, the Warsaw Pact countries avoid a large part of the massive R & D investment the United States has committed to the development of sophisticated military systems. Second, the nature of the research process in nonclassified settings has changed, with the result that universities in particular are now becoming increasingly attractive targets for foreign intelligence efforts. Those sharing this concern argue that the imposition of limited controls is preferable to classification, which would remove such work entirely from the campus. They suggest that, when universities choose to accept the presence of defense-related research on campus, for whatever mix of intellectual and economic motivations, controls must be considered part of the price.

A second perspective focuses on the critical importance of maintaining vigorous, open scientific communication both within the borders of the United States and across international boundaries. According to this view, even limited restrictions on the free flow of information affect feedback, delay the discovery of errors and duplication, hinder critical evaluation of scientific efforts, and, as a result, undermine the pace of scientific discovery. The price of achieving short-term security by restricting the communication of ideas and information would be the pace and effectiveness of both our research effort and the transfer of that research into application. This perspective argues, in sum, that secrecy about existing knowledge can never replace the development of new ideas as a means of protecting national security.

Those adhering to this view also suggest that openness pays dividends of other kinds. For one thing, U.S. efforts to limit the flow of information by restricting scientific exchanges with the Soviet Union inevitably will limit the flow of information from them to us. Not only does American science benefit from these interactions, but it can also be argued that for intelligence purposes they help gauge accurately the state of Soviet scientific advancement.

A third perspective focuses on the educational impacts of restrictions on access to and dissemination of scientific and technological information. Here again there are two separate arguments. First, those who provide advanced scientific and technical training point out that it is functionally impossible to separate completely education from research activities. Moreover, if the only experience a young scientist or engineer gains is in the classroom, rather than the laboratory, the next generation of American investigators will be substantially less prepared to maintain the superior levels of productivity and achievement that have characterized the U.S. scientific effort since World War II.

But there is another important dimension to the educational perspective, the cost to the U.S. academic system of excluding foreign nationals from sensitive areas of science and technology. The number of foreign students in higher education in the United States increased substantially during the 1970's, at both the undergraduate and graduate levels. Among the factors underlying this trend were increased foreign demand and in-

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creased recruitment of foreign students by U.S. institutions in order to augment domestic enrollment (Fig. 1). The data are even more striking at the postdoctoral level (Fig. 2). The postdoctoral sector is particularly significant because it is a major source of junior faculty and research talent in many American universities.

A fourth major view is that expressed by private industry. Many leaders of U.S. technology-based industries have argued that if industries are to adapt to rapidly changing business conditions, they must have access to scientific and technological information from all parts of the world. Beyond the loss of information, industrial leaders express concern about other economic impacts resulting from a lack of openness, including regulatory costs, loss of sales, loss of reliability as a trading partner, and reductions in the pace of innovation.

A fifth voice in the debate may be characterized somewhat broadly as the constitutional and cultural perspective. Many concerned about the continued vitality of the American political system believe it can flourish only in an atmosphere of openness. First, they point out that fundamental constitutional questions may be raised when the government seeks to restrict the rights of citizens to speak or to publish. It is argued that freedom of speech is not just a legal right, it is essential to the maintenance of an informed electorate. Second, it is suggested that visits by Soviet and East European scientists expose them to the U.S. culture and political system which, over time, may contribute to political and social change within the Warsaw Pact.

Key Findings and Recommendations of the Corson Panel

In spring 1982 discussions between representatives of the National Research Council and the DOD led to the establishment of an ad hoc panel of the Committee on Science, Engineering and Public Policy (COSEPUP), chaired by Cornell University president-emeritus Dale R. Corson. The panel included scientists, former defense and national security officials, and research administrators in industry and universities (2). Its mandate was to examine the evidence of technology leakage and methods for controlling it, and to seek policy measures by which the competing national goals of national defense and intellectual freedom could be accommodated satisfactorily.

After reviewing evidence on the benefits and costs of control measures, the panel concluded that a national strategy of "security by secrecy" would weaken American technological capabilities, because there is no practical way to restrict international scientific communication without also disrupting domestic scientific communication. A national strategy of "security by accomplishment"—one that emphasizes protecting the U.S. technology lead by promoting scientific productivity—has far more to recommend it (*3*).

The panel heard extensive briefings by the intelligence community, some at high levels of classification. While noting that the available evidence left much to be desired, the panel reported that it had found no case of significant damage to security associated with research dissemination (*3*, pp. 13 and 41). The panel also added two caveats. First, it observed that the evidence on leakage and the associated damage to U.S. security was still only fragmentary and anecdotal, in part because the problem had only recently been identified (*3*, p. 14). Second, it pointed out that Soviet intelligence efforts were extensive and that universities and other research sites were indeed targeted. It also noted (but took no position on) intelligence community arguments that research facilities are likely to be more heavily targeted in the future (*3*, p. 21).

The panel suggested that the class of research information of greatest potential concern is not the explicit findings in research reports but rather know-how—the detailed understanding of equipment use or operational procedures normally gained only by direct participation in a research project (*3*, p. 42). It follows that protection of sensitive research information is achieved better by preventing sustained access to research projects than by preventing dissemination of written research reports.

The Corson Panel examined five types of control mechanisms: (i) classification, (ii) export control regulations, (iii) controls on foreign visitors, (iv) restrictions on government contracts, and (v) voluntary prepublication clearance. Its general review of these mechanisms led the panel to two broad conclusions:

1) Where controls are deemed necessary, the government should use contract restrictions in preference to export control regulations. The contract mechanism has the advantage of informing a researcher of his or her obligations in advance while leaving application in the hands of the most technically qualified government personnel. Export controls, devised for controlling the movement of tangible objects, are ill-suited to the control of information flow.

2) The government's effort is uncoordinated and spread too broadly across too many diverse technologies to be practicable. An effort spread this thin cannot be effectively administered, and it raises unnecessary fears among researchers working in areas with no military relevance. The panel suggested that the government adopt a strategy of building "tall fences around narrow areas"; contract controls, for example, should be restricted to a few gray areas that justifiably cannot be either classified or completely open. It defined such technologies as those in which all of four criteria apply (*3*, p. 49).

- the technology is developing rapidly, and the time from basic science to application is short,
- the technology has identifiable direct military applications, or it is dual-use and involves process or production-related techniques,
- transfer of the technology would give the U.S.S.R. a significant near-term military advantage, and

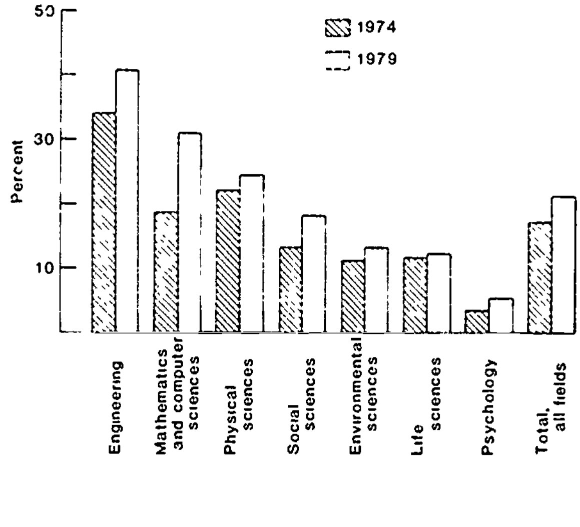

Fig. 1. Percentage of foreign students in full-time, graduate science and engineering programs in doctorate-granting institutions, 1974 and 1979 (*16*)

• the United States is the only source of information about the technology, or other friendly nations that could also be the source have control systems as secure as ours.

The Corson report was released on 30 September 1982, and it was received favorably by top Administration officials, university administrators, and other members of the U.S. science policy community.

The Situation in 1984

More than 18 months have elapsed since the Corson report was issued. Much of the hope that surrounded its release has faded and there have been at least four attempts to formulate a new policy:

1) An interagency review of the Corson report's implications has yet to be completed. The effort began with National Security Study Directive 14 82 (now renumbered as NSSD 1-83), which was signed by President Reagan and issued by then National Security Advisor William Clark in December 1982. The terms of the review have twice been altered, and there have been multiple changes of personnel at the Office of Science and Technology Policy (OSTP), which is responsible for coordinating a major section of the report. In addition the study has been conducted at the classified level without the benefit of outside input. It is not known how the product, reportedly near completion, compares in scope or substance with the effort originally requested by the President. OSTP officials see an advantage in the release of some sort of unclassified document, but they are unsure as to either the date of its public availability or its comprehensiveness.

2) Given the delays in the interagency policy review, the Department of Defense (DOD) has moved to complete an internal policy review that was begun in 1981. Accordingly, a Steering Committee on Technology Transfer was established within DOD to focus on contracts, visa controls, emerging technologies, scientific conferences, publications, and rules for exemption to the Freedom of Information Act. The work of the Steering Committee and its subpanels has now been largely completed and its recommendations are being implemented.

3) A provision included in the 1984 Defense Authorization Act permits the Secretary of Defense to protect certain kinds of unclassified technical data in the possession or under the control of the DOD that otherwise would be subject to release to foreign nationals under the terms of the Freedom of Information Act. Additional proposals have been circulated within the DOD to seek broader authority to protect sensitive technical data produced by other federal agencies (for example, NASA or the Department of Energy) by facilitating their transfer to DOD control.

4) A Presidential directive, "Safeguarding National Security Information" (NSDD 84), was issued in March 1983. That directive would have required both government officials and those under contract to the government with access to sensitive compartmented information, which is information classified at levels above top secret, to submit for prepublication clearance anything they write that bears upon national security matters. The directive was withdrawn in February 1984 after substantial opposition arose in the Congress and among the general public.

While the various intra- and interagency policy studies have been under way, a series of incidents have occurred over the past 16 months similar to those that provided the original impetus for creation of the Corson panel. The principal distinction between now and the pre-Corson environment is that most of the incidents relate to the withdrawal of papers from meetings, rather than the denial or restriction of visas or other restrictions.

The Development of Controls

Since the early 1940's, the federal government has added steadily to the means by which it can prevent—or at least slow—the loss of militarily sensitive scientific information. For example, the government has the authority, established through a series of executive orders, to impose security classification on sensitive research conducted by its own employees or undertaken by private parties at public expense. The Executive Branch has also been assigned a substantial amount of legislative authority over the years through which it can attempt to control all aspects of scientific communication.

The Atomic Energy Act of 1946, for example, precluded public dissemination of most of the results of the Manhattan District Project or subsequent atomic research, particularly through a "born secret" provision that automatically classifies research on atomic energy at its creation. The government has also been able to use the authority vested in export control legislation to limit the release of products, processes, and technical data to potentially adversary nations. The Export Control Act of 1949, which has been renewed since 1969 as the Export Administration Act, required the Department of Commerce to prevent the export of goods that might enhance either the economic or military potential of communist countries. The Export Administration Act is implemented through the Export Administration Regulations and through a comprehensive list of products and processes known as the Commodity Control List. Since 1979, the Department of Commerce administrators have also relied for guidance on the Militarily Critical Technologies List, which is prepared by the DOD and based on a 1976 report of the Defense Science Board, commonly called the Bucy report (4).

Another method of controlling the export of security-related data is the Arms Export Control Act of 1976. This Act is implemented by the Department of State through the International Traffic in Arms Regulations. These regulations control the export of military systems, including technical data relating to the "design, production, manufacture, repair, overhaul, processing, engineering, development, operation, maintenance or reconstruction . . . of implements of war on the U.S. Munitions List" or "any technology that advances the state of the art or establishes a new art in any area of significant military applicability" (5).

In order to control the movement of militarily sensitive goods at the international level, the Coordinating Committee for multinational export controls (CoCom) was established by informal agreement in 1949. It comprised all the NATO countries except Iceland and Spain, plus Japan. CoCom has provided a forum for

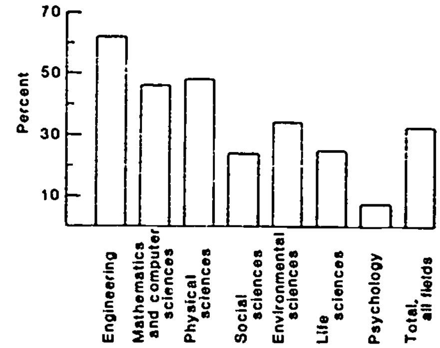

Fig. 2. Percentage of foreign postdoctoral researchers in science and engineering programs in doctorate-granting institutions, 1979 (16, p. 21).

the voluntary coordination of trade controls on exports to the Warsaw Pact countries. The United States is currently engaged in efforts to strengthen the effectiveness of the CoCom mechanism

An Update of the Key Issues

Evidence on the extent of technology leakage. Knowledge about technology leakage and its effects on national security has not changed significantly in the 18 months since the Corson Panel was briefed by the U.S. intelligence community (*6*). In recent months the principal activity has been the identification of the ways in which technology leakage can occur so that a comprehensive control effort can be fashioned. No major initiative has been undertaken to better characterize the relative importance of sources, channels, or types of information that leaks out or the relative significance of scientific communication within the large picture. The intelligence community reports no cases during this period in which loss through the U.S. scientific community has led to identifiable damage to national security. However, intelligence officials remain concerned about the small percentage of Soviet intelligence acquisitions that involve the American research community.

Classification controls. There is now better information on the extent to which military research is classified or otherwise restricted. As part of the report of its subcommittee on publications, the DOD Steering Committee on National Security and Technology Transfer determined how publications in federal information centers were classified or disseminated in terms of their subject area or source (Tables 1 and 2).

The data support the contention that universities are responsible for less sensitive research than is done in other settings, no matter what field of technology is involved. The study also found that all classified reports from universities and approximately 50 percent of the limited reports from universities were generated in off-campus facilities affiliated with the universities. Meanwhile, through Executive Order 12356, issued in April 1982, the Administration changed the thrust of its classification policy, stating that restrictions are to be imposed in all cases where reasonable doubt exists about the need for classification. It also expanded the number of categories of potentially classifiable information and made it possible to reclassify information previously made public.

Export controls. The Export Administration Act, and the attendant Export Administration Regulations (EAR), remains the principal regulatory instrument for controlling the flow of sensitive technical data, particularly that of a proprietary nature across our borders. The Export Administration Act came up for renewal during the first sessions of the 98th Congress. Bills were passed in both Houses early in the second session, with the Senate version tending to be more restrictive than that of the House. A conference version of the Export Administration Act of 1984 is expected before the end of the current session.

While the language of the new export act has been debated, the Administration has proceeded with vigorous efforts to control unwanted technology transfer. It can be stated that, in general, the Department of Commerce presently considers scientific communication to be a relatively small—albeit significant—aspect of the overall technology control problem. Since 1982 the EAR have not been invoked to prevent the dissemination of the results of academic research. On the other hand, modifications to the technical data regulations under consideration for incorporation into the EAR could significantly alter this situation. If implemented, they would eliminate the general licensing exemption granted to some "scientific and education data." They would also require a validated license for the export of virtually all "critical technical data," a new term identified in the draft wording, which is not publicly available. Since the definition of "export" in this draft includes presentation of papers at symposia where foreigners were present, the hiring of a foreign researcher, and so on, the proposed rules, if adopted, would have a significant impact on U.S. scientific intercourse.

The Administration has also stepped up its export control enforcement effort, principally through two channels. The first, Operation Exodus, is an effort by the Customs Service since late 1981 to spot-check high-technology goods being readied for shipment. It has resulted in the detainment and seizure of some 2300 foreign-bound shipments worth approxi-

Table 1. Distribution restrictions on DOD reports by source, 1979 through 1983 (*13*).

Source	Total	Classified (%)	Limited (%)	Public (%)
DOD laboratories	61,694	12	44	44
Universities	23,119	1*	4	95
Industry	32,806	21	35	44
Nonprofit	5,609	17	15	68
Total	123,228	13	33	54

*Generated at research institutes associated with universities

Table 2. DOD reports withheld from public release, by subject (13)

Field	Total	Classified (%)	Limited (%)	Public (%)
Missile technology	2,524	57	32	11
Ordnance	6,740	32	47	21
Military sciences	8,099	38	33	29
Navigation, communication, detection and countermeasures	13,490	40	28	32
Aeronautics	5,082	13	53	34
Propulsion and fuels	3,252	14	48	38
Space technology	905	17	44	39
Nuclear science and technology	1,259	24	34	42
Energy conversion (nonpropulsive)	1 055	3	54	43
Electronics and electrical engineering	12,424	3	50	47
Materials	5,643	1	46	53
Methods and equipment	2,288	3	42	55
Agriculture	82	1	44	55
Mechanical, civil, industrial, and marine engineering	9,284	5	35	60
Biological and medical sciences	10,093	1	32	67
Physics	12,812	6	25	69
Behavioral and social sciences	10,529	2	20	78
Earth sciences and oceanography	4,671	1	21	78
Atmospheric sciences	3,078	1	16	83
Chemistry	4,042	—	14	86
Astronomy and astrophysics	584	—	13	87
Mathematics	5,292	—	5	95

mately $149 million and eventual indictments in 221 cases, only 28 of which involved so-called dual-use technology (7). The second effort to enforce controls involved creation of a new post within the Department of Commerce, known as the Office of the Deputy Assistant Secretary for Export Enforcement, which referred 37 dual-use technology export cases to the Justice Department for prosecution in fiscal 1983 (7). This two-track approach reflects uncertainty between the Customs Service of the Treasury Department and the Export Administration of the Department of Commerce over which agency has lead responsibility for export enforcement.

The other principal export control mechanism is the Arms Export Control Act and the attendant International Traffic in Arms Regulations (ITAR). Revision of the ITAR, which has been pending for more than 2 years, still has not been completed, although a draft of the revised regulations is now said to exist. A likely target period for release of a new ITAR is mid-to-late 1984. The ITAR is administered by the State Department on the basis of the Munitions Control List, which is maintained by the DOD. However, there appear to be no instances in which the ITAR has been applied to written or oral scientific communication since the release of the Corson report.

Controls on foreign visitors. In May 1983, after an interagency review, Under Secretary of State William Schneider announced a new visa policy for handling cases of individuals suspected of technology acquisition. Schneider essentially reaffirmed that the existing visa law can and should be used to limit the loss of information obtained by foreign visitors. Moreover, he indicated that action may now be taken on a visa solely on the basis of a visitor's potential to be a source for technological loss. Therefore, depending on the nature of the risk identified, an applicant may be (i) denied a visa, (ii) offered a conditional visa, or (iii) given an unconditional visa. In cases of conditional visas, the restrictions may be imposed either by the relevant department or by the Immigration Service of the Department of Justice—outside of the visa process—as a condition of entry.

The State Department's principal concern is commercial trade visits, with only secondary attention paid to those involved in academic research. Again, depending on the assessment of the risk involved, a sponsor may be asked to modify a visitor's program, or alternatively, the visitor's freedom to travel may be restricted. Because the Visa Bureau does not track technology transfer cases per se, it is not possible to provide a quantitative assessment of scientific visits approved, denied, or made conditional. However, Table 3 provides an indication of the trend in advisory recommendations made between 1981 and 1983 by COMEX, the interagency Committee on Exchanges. This period may be somewhat anomalous, due to rising tensions with the Soviet Union and East Europe over Poland and other matters, but the data reveal (i) a decline in the total number of cases reviewed, (ii) a slight decline in the percentage of cases in which significant concern was expressed, (iii) a decline in the percentage of cases recommended for program denial, and (iv) an increase in the percentage of cases recommended for program modification. COMEX recommendations are not necessarily adhered to by the State Department, but they prevail in a majority of the cases.

An interesting aspect of the visa and scientific exchange matter involves the People's Republic of China. The COMEX data reveal that between 11 and 25 percent of the cases from 1981–1983 in which significant concern about technology loss was expressed by the committee involved Chinese students or scientists. But, because visa and export control policies toward the People's Republic have been liberalized substantially, none of the COMEX recommendations for program denials involved the Chinese. The programs of some visiting Chinese were, however, modified.

Contract controls. The Corson Panel recommended that where controls on unclassified scientific information are warranted, they can best be accomplished by means of a priori contract constraints. This mechanism was examined by the DOD Steering Committee on National Security and Technology Transfer, and a new policy has emerged on "international transfers of technology, goods, services and munitions" (DOD directive 2040.2) (8). This directive articulates a number of new mechanisms for establishing standard definitions of what is militarily sensitive and for resolving appeals of contractually imposed restrictions. It also establishes the Panel on International Technology Transfer, as the highest level appeal mechanism for resolving differences within the DOD on technology transfer policy, and it creates two subpanels: (i) Export Control Policy—a first-level appeal structure for resolving differences on export control policy matters, and (ii) Research and Development—a first-level appeal structure for resolving differences on technical standards, definitions, and the dissemination and exchange of technical information, including appeals of "technology transfer research cases."

Table 3. COMEX recommendations for visiting scientists' programs, 1981 to 1983 (*14*).

Programs recommended	1981	1982	1983
For approval	92	56	33
For approval with modification	225	57	133
For denial	55	35	28
Total	372	148	194

With regard to sensitive research undertaken in academic settings, the DOD Steering Committee has made a number of additional recommendations. First, it has clarified its policy on review of research papers produced by DOD contractors, distinguished by budget category and the nature of the research (Table 4). The point of these policies is to give the researcher written notice of review procedures before he or she signs a DOD contract.

Two aspects of Table 4 merit special attention: (i) the 60-day prior review requirement for basic research is more restrictive than recommended by the Corson Panel, which called for simultaneous review by the publishers and DOD, and (ii) the 90-day prior review and right to require changes for sensitive exploratory research or development are also far more restrictive than the Corson Panel's recommendation of simultaneous review for both basic and applied research.

In addition, the DOD Steering Committee has recommended the permanent implementation of a series of six dissemination-control stamps, already approved on an interim basis by Secretary of Defense Caspar Weinberger, that clarify the standards used in circulating unclassified documents produced through DOD contracts (or at government laboratories) and held by the Defense Technical Information Center (DTIC) and other secondary distribution facilities (9).

Finally, COMEX is updating an existing DTIC database in order to inform researchers of each other's work and to determine quickly the number and type of DOD contracts in force on a given university campus plus their level of classification or restriction.

Voluntary prepublication review. An

agreement for voluntary submittal of papers for simultaneous review by the National Security Agency (NSA) and professional journals, developed by the Public Cryptography Study Group of the American Council on Education and the NSA, appears to be working in a manner that is reasonably satisfactory to all parties. The NSA reports that 200 papers have been submitted for review since completion of the agreement. Of this total, nine papers have been challenged, six have been modified, and three have been withdrawn. Pursuant to the agreement, a six-member appeals committee has been established, consisting of four academic researchers and two former NSA officials. To date, there have been no appeals of the NSA review decisions (10).

Implementation of gray area criteria. Perhaps the most important recommendation of the Corson Panel concerned the need to build tall fences around narrowly circumscribed technologies that could be identified as meeting the four principal criteria set forth in the report. Pending the public release of the interagency NSSD report, which was coordinated by the OSTP, it is impossible to determine with certainty the extent to which these recommendations have been adopted as official policy.

There are indications, however, that the government is moving toward the adoption of a broader approach than was recommended by the Corson report. Consider the following factors: (i) there has been little progress in streamlining the Militarily Critical Technologies List; (ii) a new, unclassified Militarily Significant Emerging Technologies Awareness List (METAL) is being created that will identify for purposes of monitoring certain frontier technologies just appearing on the horizon but not yet embodied (11); (iii) the definition of "threat assessment factors" proposed to the DOD Steering Committee on National Security and Technology Transfer for identifying militarily significant emerging technologies are substantially more comprehensive than the Corson Panel criteria, and (iv) continuing efforts are under way within the Coordinating Committee for multinational export controls to identify additional technologies that are to be proscribed or restricted for export to Warsaw Pact countries.

Conclusions

This review of the current status of scientific communication and national security suggests three major conclusions.

1) The government has not found it possible to act in a manner compatible with the major principles set forth in the Corson report. Although the DOD appears to be implementing the panel's recommendation that the best form of control is the research-funding contract, both the stringency and the reach of restrictions either proposed or in force go considerably beyond the panel's recommendation. Moreover, if the draft wording for new technical data regulations were incorporated into the EAR, then export controls could come to supplant DOD-imposed contractual restrictions as the principal mode of control contrary to recommendations of the Corson Panel.

2) The continuing lack of effective government-wide coordination raises important risks, including (i) disparate agency policies that do not adequately balance national goals, (ii) wasteful allocation of national resources among programs of varying effectiveness, and (iii) confusion and skepticism in the research community. Given the successive delays in the National Security Council review, policy initiative has reverted back to the individual agencies (most notably DOD), whose missions typically reflect only one among the many relevant national objectives. Once put into place, these uncoordinated initiatives will be difficult to adjust. The government's lack of central coordination also represents a missed opportunity to set reasonable priorities among the many offices (OSTP counted 44 of them) responsible for addressing the many parts of the technology transfer problem. There is a danger that the lack of effective government-wide coordination will undermine the perceived legitimacy of government programs among the research community.

3) There is little progress toward an objective understanding of the technology leakage problem and the effects of control measures. This research failed to demonstrate any improvement in knowledge about the actual effectiveness or adverse effects of control on scientific communication.

One feature that makes the policy dilemma in this area particularly difficult is that the costs of being wrong, which are potentially great, accrue only after a delay and in ways that are difficult to measure accurately in the short term. If U.S. controls on scientific communication are too lax, the extensive Soviet intelligence effort may obtain at relatively low cost information not otherwise accessible. Even then, however, it may be difficult to determine precisely how the transferred data have contributed to the Soviet military posture. On the other hand, excessively tight controls may have effects that, while subtle and indirect, are also pervasive. Perhaps the largest risk in this regard is the long-term changes that controls are likely to cause in the demographic distribution of scientists and engineers among the various disciplines and subfields.

The threatened or actual government intervention has already had an impact on the number of papers put forward for publication or presentation at conferences in certain fields (12). If the present climate of uncertainty continues, we may witness an increasing migration of the best minds away from those areas of science and engineering where controls are (or may be) imposed—the very fields where new talent is most critical to U.S. technological lead time. Furthermore, if the United States acts to restrict further the flow of people and ideas in frontier areas of science and technology, other advanced industrialized countries may find it necessary to do the same. In this

Table 4. Review policy for research papers produced by DOD contractors (15).

Budget items	Nonsensitive research	Sensitive research
Basic research*	Simultaneous submittal to contract officer and to publisher. DOD has no right to require changes or to restrict publication.	Manuscript must be submitted to contract officer 60 days prior to submittal to publisher. Researcher retains option of whether or not to publish.
Exploratory research and advanced technological development†	Same rules as for basic research.	Manuscripts must be submitted to contract officer 90 days prior to submittal to publisher. DOD retains the right either to require changes before allowing publication or to block publication outright.

*DOD budget category 6.1. †DOD budget categories 6.2 and 6.3

respect, to the extent that U.S. national security continues to rely on technological superiority, by disrupting the invisible colleges, the channels of informal communication that speed the pace of innovation and scientific discovery, the nation may risk sacrificing its best hope for continued long-term security in the belief that controls are necessary to maintain short-term strategic advantage.

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7. J. Zonderman, "Policing high-tech exports," *New York Times Sunday Magazine*, 27 November 1983, p. 103.
8. C. W. Weinberger, "International transfers of technology, goods, services, and munitions," DOD directive 2040.2, 29 December 1983.
9. The range of classifications is as follows: (i) release only on approval of originator, (ii) release within DOD only; (iii) release to DOD and DOD contractors (who must apply for formal approval) only; (iv) release to U.S. government only; (v) release to U.S government and U.S. government contractors only, and (vi) release to the general public. This system will replace the previous arrangement that allowed only two options: public release or U.S. government only.
10. Office of Policy, National Security Agency, personal communication, 15 March 1984.
11. Creation of METAL was recommended by the subcommittee on the monitoring of emerging technologies of the DOD Steering Committee on National Security and Technology Transfer. The subcommittee urged the establishment of an unclassified watchlist to which a technology might be assigned during the period that it was still at the stage of basic research. Subcommittee on monitoring of emerging technologies, "Report to the DOD steering committee on national security and technology transfer." Department of Defense, Washington, D.C., 29 December 1983.
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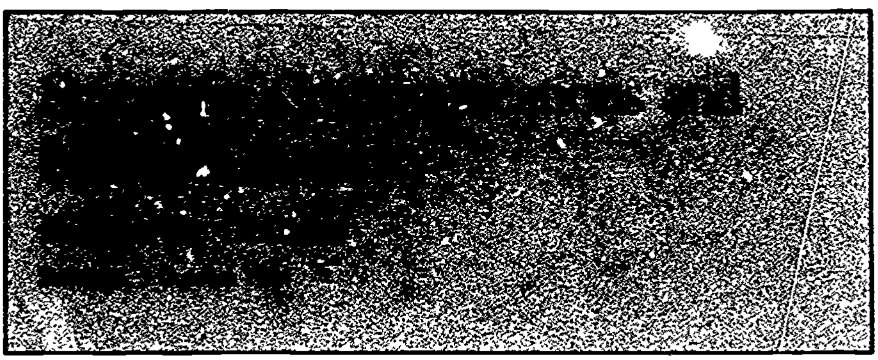
14. From data provided by the interagency Committee on Exchanges, Technology Transfer Intelligence Committee, 5 January 1984.
15. Office of Research and Laboratory Management, Deputy Under Secretary of Defense for Research and Engineering, personal communication, 20 December 1983.
16. Division of Science Resource Studies, *Foreign Participation in U.S. Science and Engineering Higher Education and Labor Markets* (NSF-81-316, National Science Foundation, Washington, D.C., 1981), p. 5.

17. I thank Lawrence E. McCray for his thoughtful contributions to this article, and also for their helpful comments Dale R. Corson, Rosemary Chalk, Ruth L. Greenstein, Allan R. Hoffman, John M. Logsdon, Richard A. Meserve, Robert L. Park, Victor Rabinowitch, Harold C. Relyea, Walter A. Rosenblith, Eugene B. Skolnikoff, and Philip M. Smith. The views expressed are those of the author and do not represent a deliberative process of or a position taken by the National Academies of Sciences or Engineering, the Institute of Medicine, or the National Research Council.



The conflicting imperatives of national security and open scientific communication have been the subject of a vigorous and sometimes emotional national debate. Differing priorities have led to incompatible conclusions. In times of peace and security, the maximum freedom of speech and communication has served this nation well, in times of great peril, national security considerations have temporarily displaced those precious freedoms. In this period of world history when nations in competition may win or lose by their technologies, both in combat and in commerce, how should we order our priorities regarding national security and scientific communications?

We must begin by recognizing the distinction between science and technology, between knowledge and know-how. Nature yields her secrets to anyone imaginative enough to ask the right questions, regardless of nationality. All participants benefit in the testing of new scientific hypotheses and the exchange of scientific information. Nor can the flow of ideas be stopped at national borders. On the other hand, know-how is a precious commodity leading to the commercial or military products that determine the fortunes of nations in peace and in war. Yet sometimes it is hard to tell where scientific knowledge leaves off and engineering know-how begins.

The potential for unintentional disclosure of national security information through the publication of basic research results is virtually nonexistent, and the benefits of such an open publications policy far outweigh the risks. The treatment of university R&D more applied in nature has been the subject of intensive discussions between university and Department of Defense representatives in the DOD–University Forum over the past 2 years. (The forum participants are drawn about equally from the academic and defense communities.) To put the matter in perspective, about 80 percent of R&D on university campuses sponsored by DOD falls in the category of basic research. The discussions have therefore focused on the other 20 percent, only a very small fraction of which has been of security concern.

The forum discussions have contributed greatly in formulating new policy that will provide for completely unrestricted publication (without delay) of all unclassified fundamental research carried out in any laboratory (university or industrial). Henceforth, consistent with U.S. statutes, the primary way to restrict the publication of contracted fundamental research will be to classify it. (The rules for classification are well understood. The DOD currently has no classifed basic research on university campuses, and this situation is expected to continue. The government's power to classify is not new, it has not been and will not be invoked on university campuses except in the rarest of circumstances involving special reasons of national import, and with complete prior agreement of the university involved.)

The quickest way to disseminate research results is through meetings, conferences, and symposiums, often sponsored by scientific and engineering societies. Many meetings held in the United States are international, and it is important to keep them so. For government sponsored or cosponsored technical conferences, admission should likewise not depend on nationality but only on security considerations—it is in the best interests of all allied countries to share their technologies to lighten the burden of mutual defense. I also believe that no unclassified technical conference requiring an invitation should exclude individuals from allied nations who can contribute to the success of the conference.

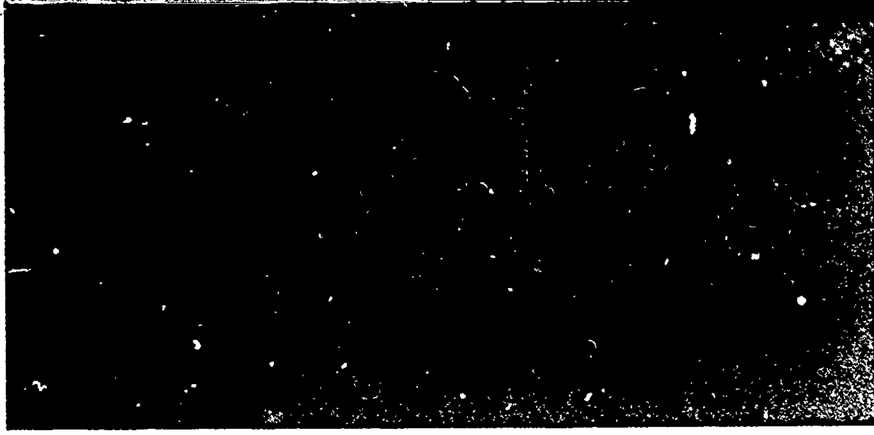
The freedom to publish scientific and educational material is vital for progress in science and engineering. Ultimately the relationships among academia, government, and industry will depend on the trust and understanding among the people who work together and depend on one another.

In addressing problems posed by the changing roles of scientists in contemporary society, the preceding chapters develop key principles of scientific freedom and responsibility. In many cases, these principles derived from broader discussions of the nature of citizenship, the role of the expert in a democracy, and the duty of experts to warn society when unintended consequences of individual actions threaten others. As the possible risks associated with new scientific developments became more apparent, leaders from the scientific community and other responsible officials sought ways to diminish potential dangers while at the same time providing the benefits of new research discoveries to the larger society. The framework of environmental laws and regulations that evolved in the 1970s is built upon many of these earlier discussions about the nature of risk and the manner in which it should be allocated throughout society as a whole.

What issues will influence the objectives and practice of science in the years to come? The status of the research university and other research centers as emerging sources of economic and political advantage will certainly be a dominant factor in future discussions about the role and purpose of science in society. If the frontiers of discovery are seen as frontiers of profit and power as well as of intellectual excitement, vigorous efforts to direct scientific talent and other research resources towards private interests can be expected. The privatization of science may, for example, help to promote the rapid development of new sources of energy, food production, and medical advances. At the same time, concerns about the corrupting influences of personal gain will surely raise new questions of responsibility and accountability in the conduct of science: What criteria should be used in directing public subsidies, in the form of government funds, tax credits, and relaxed regulations, toward selected areas of research and development? What standards of accountability should guide future government investments in basic research? How will the basic values traditionally associated with scientific research, such as the sharing of research data, freedom of travel and communication, and so on, be affected by an increasingly competitive global economy?

In the 1960s and 1970s, scientists began to incorporate principles of informed consent, environmental protection, and — recently — animal rights into their research policies and procedures. What new concerns will shape the conduct of research in the next decades? Will the increasingly competitive nature of scientific research weaken the professional norms that have shaped traditional research practices? If so, what new standards will replace them?

Informed reflection on these and related issues requires the accumulation of much experience with the changing forms of modern science. If history is a reliable guide, the ethical problems of the future will build upon those of the past. It is hoped that the insights and perspectives offered in this volume will assist those who struggle with future dilemmas about the meaning of rights and duties in science. —*RC*



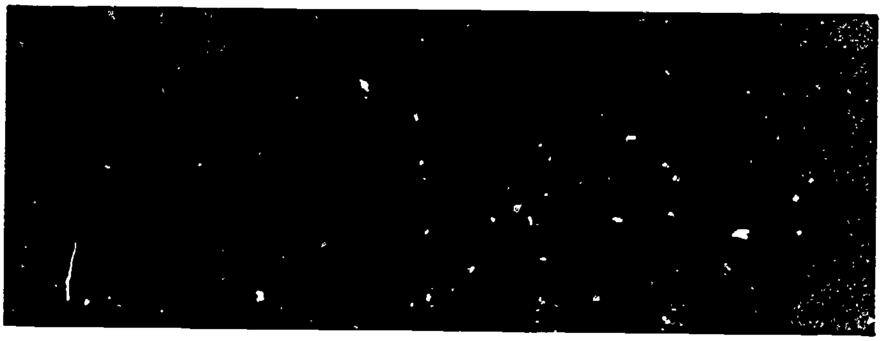
In the past four decades, the American Association for the Advancement of Science has organized four separate committees to explore issues related to the rights and duties of scientists and to scientific freedom and responsibility. These four committees included the Special AA S Committee on Civil Liberties for Scientists, appointed in 1948 and chaired by Maurice B. Visscher; the AAAS Interim Committee on the Social Aspects of Science, formed in 1955 and chaired by Ward Pigman; the 1960 AAAS Committee on Science in the Promotion of Human Welfare, chaired by Barry Commoner; and the ad hoc AAAS Committee on Scientific Freedom and Responsibility, organized in 1970 and chaired by Allen Astin. This ad hoc committee was succeeded in 1975 by a standing AAAS Committee on Scientific Freedom and Responsibility, which continues to sponsor programs and activities on behalf of the Association.

Each of these four committees prepared a written report that was published in *Science*. In some cases a committee's report might also be published as a separate document by the Association, as in the case of the 50-page report, *Scientific Freedom and Responsibility*, prepared by John T. Edsall in 1975.

The AAAS committee reports provide a valuable analysis of issues that shaped the relationship between science and technology and society in each decade. In the late 1940s, AAAS concerns were provoked by security restrictions and secrecy in the aftermath of World War II. In the 1950s, in the wake of *Sputnik* and a new resurgence of public interest in the contributions of science to war and peace, scientists sought to develop an appropriate place for their professional skills and expertise in the management of public affairs.

In 1960, with the formation of the Committee on Science in the Promotion of Human Welfare, AAAS addressed the disparity between military and civilian research in science and technology and the problems of environmental and urban pollution. Finally, in 1975 in cooperation with its affiliated societies, the Association sought to develop broad principles of scientific freedom and responsibility to protect and assist "whistle-blowers" — scientists and engineers who were punished as a result of actions intended to foster the public's interest.

These four reports provide a unique historical record of the changing dimensions of the social contract between science, technology, and society. As such, they provide a window on emerging areas of consensus defining the broad principles of social responsibility, autonomy, and integrity in science that shape professional conduct in modern times. — *RC*



On December 30, 1947 the AAAS Council passed a resolution instructing the President of the Association to appoint a Special Committee on Civil Liberties for Scientists. Maurice B. Visscher was named chairman, and with Philip Bard, Robert E. Cushman, Richard L. Meier, and James R. Newman as members, and Walter Gellhorn as consultant, the Committee completed its investigations and submitted a 77-page report of findings and recommendations in December 1948. The full text was referred to the Council, which voted by an overwhelming majority to publicize the findings, and it is planned ultimately to make the complete report available at cost to those who want access to it. Announcment will be made in Science *when Maurice B. Visscher and E.C Stakman have concluded editorial revisions and the report is ready for distribution. Meanwhile, by vote of the Executive Committee at its meeting July 7, the conclusions and recommendations are published herewith.*

There is at present a tendency in public thinking to relate scientific activity almost wholly to military activity, exposing scientists more than most occupational groups to sustained and stringent limitations upon their professional freedom. Fearful lest these limitations exceed justifiable bounds, jeopardize the national welfare, and infringe the rights of scientists, the American Association for the Advancement of Science, in December 1947, created a Special Committee on the Civil Liberties of Scientists.

The present report embodies its conclusions and recommendations with respect to three main areas:

1. Restrictions on research and scientific information;
2. Measures to assure the personal reliability of scientists having access to confidential data;
3. Inquiries relating to the "loyalty" of scientific workers in federal employment.

Conclusions

I

Secrecy is damaging to both science and democracy. In both, progress and the detection of error depend upon open discussion and free interchange of ideas among widely divergent and widely separated groups.

Yet today, in the United States, we have within the body of science large regions of secrecy. We endorse the statement of the President's Scientific Research Board, which in its 1947 Report on Science and Public Policy said: "Strict military security in the narrow sense is not entirely consistent with the broader requirements of national security. To be secure as a Nation we must maintain a climate conducive to the full flowering of free inquiry. However important secrecy about military weapons may be, the fundamental discoveries of researchers must circulate freely to have full beneficial effect. . . .Security regulations, therefore, should be applied only when strictly necessary and then limited to specific instruments, machines or processes. They should not attempt to cover basic principles of fundamental knowledge."

II

No matter how the area of secrecy may be delimited, there will undoubtedly remain some matters of scientific cognizance which should be kept confidential. So long as national policy dictates that secrecy be observed, the reliability of persons to whom these matters are entrusted must be assured; hence inquiries into the character and attitudes of these persons are warranted.

If national as well as individual interests are to be protected, however, improvements must be achieved in the policies and procedures of our present security clearance programs as they affect scientists who will be entrusted with classified information.

The Atomic Energy Commission and the National Military Establishment are the chief agencies concerned with the trustworthiness of scientists who have access to "restricted" or "classified" data. Neither of these agencies furnishes the affected scientist any statement of the reasoning underlying a conclusion which is adverse to him; neither one sets forth charges in a precisely formulated fashion; neither one requires that testimony used against an individual be made known to him, or that even casual and nonofficial informants be identified and produced for examination; neither one provides for the making of specific findings of fact; neither one undertakes to record and publish its opinions in a way which makes possible any public understanding or analysis of the determinations made.

In some respects the procedures of the Atomic Energy Commission are more fully elaborated than those of the National Military Establishment, though the military clearance of the latter may affect literally millions of employees of private industry engaged in the planning of production of articles for military use. A military determination that clearance should not be granted a civilian scientist is subject to appeal to the Industrial Employment Review Board (IERB), composed of Army, Navy, and Air Force officers. Proceedings of the IERB are themselves "classified," which means that even the immediately affected employee is forbidden to discuss them, keep notes about the handling of his own case, or possess a copy of the record of the hearing. Despite the fact that its decisions have a drastically important impact upon the lives and careers of civilians entirely outside the public service, the tribunal is exclusively military in its composition and there is no opportunity for review of its judgments by an appellate body differently constituted. Such subjection of the destinies of civilians to military tribunals is contrary to national tradition. Quite apart from procedural inadequacies, the present organization for deciding security clearance cases is open to basic criticism.

The Atomic Energy Commission has recently manifested a tendency to require security clearance not only for those scientists who themselves have access to restricted data, but also for their fellow scientists with whom they may have personal contact. This is graver in its implications than even the serious procedural and administrative imperfections already noted. At Brookhaven National Laboratory, for example, where only perhaps one-tenth of the scientific personnel works within the area of secrecy, all scientists must be cleared as a condition of employment. This apparently reflects a yielding to uninformed or sensationalist legislators and others who tend to exaggerate the problem of "keeping our atomic secrets." The effect of the excessive precautions is to discourage participation in important research activities closely linked to the nation's well-being. Scientists are increasingly reluctant to commit their personal and professional reputations to those who have brought frivolous charges against respected colleagues. Moreover, the delays and expense often involved in obtaining security clearance deter qualified persons from entering the atomic energy program.

So far as disclosures of evidence reveal, the problem of faithless scientific personnel in this country appears to be markedly less grave than the public has been led to suppose. Moreover, informed scientists are in broad agreement that restricted data cannot be readily transmitted to unauthorized persons. In the circumstances which exist rather than those which are fancied to exist, the stringent application of personnel security clearance should be limited to smaller numbers of scientists rather than extended to ever larger groups. If nothing is done to reverse the present trend to require security clearance of scientists who do not have or desire to have access to restricted data, it is likely that many of the most penetrating and original scientific minds will be turned to pursuits unrelated to further development of the atomic energy program. Work in that field will be shunned by men of ability and pride if they are constantly treated as objects of suspicion and possible calumny.

III

Executive Order No. 9835 provides that no person shall be employed in a federal post if he is bel ·ved to be disloyal to the government of the United States. This Loyalty Order does not supplant existing provisions for summary removal of employees on security grounds. Entirely without reference to security consideration, the Order seeks to assure "complete and unswerving loyalty to the United States" on the part of all those who are in its service.

No one doubts the importance of faithful discharge of duty by public officials. No one questions the propriety of the government's demanding that its employees be loyal to their jobs and to the democratic institutions they serve. The Loyalty Order is, however, basically objectionable because it seeks to determine the employee's loyalty by inquiring into his supposed thoughts and attitudes, which are established in large part by imputing to him the beliefs of his associates.

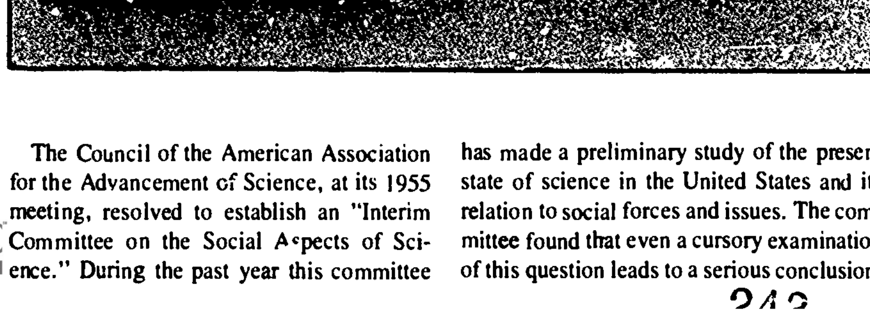
If the Loyalty Order is to be retained, a drastic revision is essential. Instead of focusing on an employee's associations, it should focus on his behavior in overt acts. Legislation already on the statute book. amply protects the federal service against retention of employees who advocate overthrow of the government.

Insofar as the Loyalty Order purports to deal with such matters as espionage, sabotage, and disregard of instruc 'ons, it is wholly superfluous, since conduct of that character is not only criminal but is also fully subject to administrative disciplinary action under existing law and regulations. The failure to confine the Loyalty Order to matters of objective proof has engendered a feeling of insecurity in public employment and may be expected to lessen the vigorous intellectual independence which is a prime condition of sound scientific work as it is of an imaginative civil service. "Experimentation there may be in many things of deep concern," Judge Cardozo once wrote, "but not in setting boundaries to thought, for thought freely communicated is the indispensable .ondition of intelligent experimentation, the one test of its validity." Unless there is elimination of the Order's present emphasis on attitude rather than conduct, the nation will suffer heavily from the present loyalty program.

Even if the Loyalty Order were to be continued without revision of its underlying philosophy, important changes in administrative methods are urgently needed. The present loyalty boards discharge simultaneously the functions of advocacy and adjudication. The content of the charges they issue and the conduct of the proceedings over which they preside do not assure that the facts and their implications will be fully explored. The organizations with which an employee may be identified are finally and conclusively characterized by the Attorney General without either the employee's or the organization's having any opportunity whatsoever to establish that the Attorney General was not fully informed. These and other procedural deficiencies can be corrected readily. So long as they remain, they accentuate the possibility of error in the loyalty program.

The fundamental shortcomings in the Loyalty Order, however, are not procedural. Rather, they are to be found in the very conceptions which the Order expresses. Refinement of administrative methods and gentility of official behavior are important, to be sure. But they are not basic. Until the Loyalty Order deals with the way employees act, rather than with the way they supposedly think, we shall inhibit the freedom and encourage the insecurity of our public servants. The cost will in the end be borne not by the employees who are deprived of their normal freedom to believe and behave as they wish within the limits law has set. It will be bome by the nation as a whole.

As President Truman recently asserted, "Continuous research by our best scientists is the key to American leadership and true national security. This work may be made impossible by the creation of an atmosphere in which no man feels safe against the public airing of unfounded rumors, gossip, and vilification."



The Council of the American Association for the Advancement of Science, at its 1955 meeting, resolved to establish an "Interim Committee on the Social Aspects of Science." During the past year this committee has made a preliminary study of the present state of science in the United States and its relation to social forces and issues. The committee found that even a cursory examination of this question leads to a serious conclusion: that there is an impending crisis in the relationships between science and American society. This crisis is being generated by a basic disparity. At a time when decisive economic, political, and social processes have become profoundly dependent on science, the discipline has failed to attain its appropriate place in the management of public affairs.

The committee believes that this question

Members of the committee are Ward Pigman, associate professor of biochemistry, University of Alabama Medical Center, *chairman*, Barry Commoner, professor of botany, Washington University, Gabriel Lasker, associate professor of anatomy, Wayne State University, Chauncey D Leake, professor of pharmacology, Ohio State University; Benjamin H. Williams, Industrial College of the Armed Forces.

demands the most urgent attention of the AAAS and of scientists generally. The present interim report is not intended as a complete consideration of the many interrelated problems encompassed by the area which the committee has studied. Rather, the report represents a sampling of some of the issues which the committee has found to serve as useful points of departure in developing an analysis of the situation. Because of the importance of this matter, the committee believes that any decision on the manner in which the AAAS can best deal with it should be based on extended and broadly conducted discussion among natural and social scientists and other interested persons. The report which follows is intended as one means of initiating this discussion.

Such an undertaking comes at an opportune time. We are at the start of a period in which science holds the promise of making unprecedented improvements in the condition of human life. Any action taken now to assist the orderly growth and beneficial use of science will be of lasting significance.

New Scientific Revolution

A cursory examination shows that society has become far more dependent on science than ever before for the following reasons.

Accelerated growth of scientific activity. The volume of scientific research and development conducted in this country has been increasing at an astonishing rate. In 1930, expenditures for science were estimated at $166 million, in 1953, the amount was more than $5 billion. Allowing for the change in the value of the dollar, this represents approximately a 15-fold increase in research expenditures over the 23-year period. The number of active scientists in the United States in 1930 was 46,000, the present number is probably about 250,000. All estimates of future needs for scientific research and personnel indicate that this growth will continue at an accelerated pace. This rate of growth sets scientific activity apart as the second most rapidly expanding sector of our social structure, military activities being first.

Increased use of scientific knowledge. It is characteristic of the present era that the previously formidable gap between scientific knowledge and its application to practical problems has become considerably reduced. It is now commonplace that calculations based on physical theory move quickly from the scientist's laboratory across the engineer's drafting board and on to actual industrial production. Since 1940 we have experienced a series of classic examples of almost immediate conversion of a scientific advance to a process of large practical impact upon society: antibiotics, synthetic polymers, nuclear energy, transistor electronics, microwave techniques, electronic computers. The greatly narrowed gap between laboratory and factory results from a distinctively new role of research in industry. Scientific investigations were previously regarded by industry as a kind of exotic garden to be cultivated in the hope of producing an occasional rare fruit. In contrast, research has now become a deliberate instrument of industrial development; scientific investigations are consciously undertaken as a means of achieving desired economic gains or, as in several notable industrial laboratories, for the purpose of contributing to our fund of basic scientific knowledge.

Recent advances in science have also created completely new industries. Four major industries—chemical, electronic, nuclear energy, and pharmaceutical—represent direct extensions of laboratory experience to an industrial scale. This type of direct transformation of scientific experience to industrial operation is probably unique in human history. Earlier industrial developments were based more on empirical experience than on laboratory science.

Social Position of Science

Science is but one sector of our culture. It is one of the institutions of society, and to a considerable degree society itself governs the development of science. In the present situation, social forces influence the development of science in the following key ways.

Social demand for technologic advances. From the evidence already cited it is clear that there is a strong social demand for at least some kinds of scientific progress. The fact that industry has made unprecedented investments in research is practical evidence that this type of scientific work is seen as a desirable activity by industrial managers. Government scientific activity, which perhaps reflects a wider range of social forces, has also been very intense in the past 20 years. Accelerated support for scientific research is evident from the increased scale of military research, the growing activities of the National Science Foundation, the greatly increased support for medical research by the National Institutes of Health, and the increasing share of philanthropic funds from private agencies now devoted to research on health and social problems. The following generalizations may be made concerning the distribution of the enhanced research support now enjoyed by American science.

1) The major part of research support goes into applied research and development rather than into basic science. In industrial research, the ratio is about 97/3; in universities, about 50/50; in federal agencies (including support for research done elsewhere), about 90/10.

2) Support is heavily slanted toward physical sciences. In 1954, federal research support was divided as follows: physical sciences, 87 percent; biological sciences, 11 percent; social sciences, 2 percent. Industrial research is at least as heavily weighted in this direction.

3) At present a very large part of our total research activities are for military purposes. Of the estimated federal expenditures for research in 1957 ($2.5 billion), about 84 percent is earmarked for matters related to national security.

4) Colleges and universities, which are the site of much of our basic research activities, have become dependent on federal funds for the greater portion of their research support (60 to 70 percent in 1954).

Some of the effects of these factors upon the character of scientific research are discussed in the section on "Internal situation of science."

Public interest in science. There are indications that the public interest in science is not commensurate with the important role of science in society.

1) Shortage of scientific personnel: We face a major crisis with respect to present and future shortages of scientific personnel. In effect, this means that the social environment in the United States does not elicit a maximum interest in science on the part of those individuals who have the capability of doing scientific work, or that our social organization does not permit them to receive the necessary training. This problem is closely connected with the more general question of the present state of public education in the United States. The content of public education has been subjected to a good deal of criticism recently, especially with regard to science and mathematics. Many scientists feel that an official state requirement for graduation from high school which calls for 1 year of "general" mathematics and for 1 year of "general" science cannot be regarded as proper recognition of the importance of science.

2) Attitudes toward scientific work. To some degree the foregoing difficulties reflect a broader problem—that is, a traditional disregard for abstract thinking. More than a century ago De Tocqueville observed "Hardly anyone in the United States devotes himself to the theoretical and abstract portion of human knowledge." He said that the immediately practical aspects of life were, however, fully appreciated. The same generalization appears to be true today. So-

called "practical" men of public affairs and business frequently disregard the advice of scientists and prefer instead to rely on "common sense," but the latter is often construed to mean what Einstein has called "a deposit of prejudices laid down in the mind prior to the age of 18." This problem, particularly as it relates to a lack of interest in scientific careers, has attracted considerable attention of late. Recent surveys indicate that the general attitude exemplified by popular epithets such as "eggheads" and "longhairs" is well rooted in the opinions of young people.

3) Science in the public press and other media: By all standards, science receives an unduly small share of the budget of newspaper space or broadcasting time. The number of books and magazines devoted to disseminating public information about science is correspondingly small The immediate reasons for this state of affairs are manifold. It is clear, however, that the situation reflects a rather low level of interest in science on the part of the public, or, more probably, of those who attempt to judge the public mind for purposes of directing the media of information.

Internal Situation of Science

How has its recently accelerated rate of growth and the general nature of the social influences upon it affected the character of American science? Some brief and approximate answers may be made.

Unbalanced growth. The growth of our scientific organization has not been an orderly process. Growth has been based less on internal needs of science than on the interest of external agencies in possible practical results. In a sense, the speed and direction of the development of science have been determined by the users of science rather than the practitioners of science. Agencies which use scientific knowledge (for example, industrial management, military establishments, medical agencies) have undertaken to encourage, and pay for, scientific research of a sort which seems to promise information that might be useful to their own specific purposes. This disproportionate growth of the physical sciences as compared with biological and social sciences to some degree reflects the interests and superior financial resources of the industrial and military agencies that support science.

The effects of this unbalanced development are already being felt. Generally speaking, we sometimes find ourselves embarking upon new ventures, based on advances in chemistry and physics, before we are adequately informed about their consequences on life or on social processes. Some of the resultant difficulties which we have already made for ourselves are described in the section on "Major social issues of scientific origin: signs of trouble."

It should be recalled that this unbalanced growth takes place within the framework of a shortage of personnel. This situation has very naturally given rise to a somewhat disorganized competition for students, which further accentuates the disparate pattern of development of the various sciences.

Inadequate progress in basic research. It is well known that the creative source of all technologic advance is the free inquiry into natural phenomena that we call basic, or "pure," science However, as has already been indicated, the great bulk of our present research activities represents the development of practical applications of the knowledge generated by previous advances in pure science. It has been pointed out repeatedly that many of our current technologic advances are based on the application of accumulated basic knowledge which is perhaps 20 to 30 years old. The progress of basic science does not appear to be keeping pace with the development of applied science Some observers even feel that there has been an absolute decline in the amount of highly creative research of the type that leads to major advances in our knowledge of nature. They point out that our present understanding of the structure of atoms and molecules and of the behavior of living cells goes back to great illuminating propositions that are 25 years or more old.

Difficulties in scientific communication. New information is the major goal of scientific research, and communication of information is vital for all scientific progress. However, the rapid, rather disordered growth of science has placed a severe strain on the channels of scientific communication.

1) Communication among the divisions of science. The problem of adequate dissemination of the results of current research has become a matter of great concern. The growth of our research establishment and the resulting increase in the numbers of scientific communications have made the problem of "keeping up with the literature" quite serious. It is now widely recognized by scientists that the existing system of publication and distribution does not fill their needs. Published articles and monographs have not kept up with the current knowledge in many fields. The number of journals is insufficient (publication delays of 1 year are common), and methods of abstracting, indexing, and reviewing are inadequate. It is becoming rapidly more difficult for scientists to find out what their colleagues know. The situation is particularly bad with respect to articles printed in foreign languages (Russian, especially) which investigators too frequently incapable of reading. Some observers have already urged the establishment of scientific information centers from which subscribers could receive transmitted reproductions of teletyped abstracts obtained by electronic scanning devices. Such centers would require government investment of about $150 million. That proposals of this magnitude are under current discussion is an indication of the severity of this problem.

Proper communication among scientists is

Resolution adopted by the Council of the American Association for the Advancement of Science at its 1956 meeting in New York

WHEREAS one of the purposes of the AAAS is "to improve the effectiveness of science in the promotion of human welfare, and to increase public understanding and appreciation of the importance and promise of the methods of science in human progress," and

WHEREAS the present rapid advance of science is accompanied by social problems of unprecedented magnitude that affect human welfare;

THEREFORE BE IT RESOLVED that in recognition of the responsibility of scientists to participate in deliberations regarding the use made of new scientific knowledge, the Council of the AAAS authorizes the president to continue the work of this committee by appointing an enlarged group for the purpose of defining the problems, assembling the relevant facts, and suggesting a practical program, to be submitted to the AAAS board of directors, to implement the objectives of the AAAS in this regard.

not, however, merely a matter of developing proper recording, cataloging, and searching devices. Face-to-face meetings, which bridge the barriers of specialization, are an obvious necessity for the ordered growth of human knowledge. There is a widespread feeling among scientists that scientific meetings which bring together investigators from different fields of science are a necessity. But, with some distinguished exceptions, such meetings have been difficult to establish thus far.

2) Imposed restrictions of free communication: Although government support has been a major source of recent scientific growth, it has been accompanied by influences which are in some respects inimical to the basic needs of science. Complete freedom of communication, regardless of national boundaries, is an essential aspect of science; nevertheless, along with government support American science has been burdened with practices that restrict the free flow of information. The interchange of scientific information is sometimes restricted unduly by the overclassification of data that affect national security. It must be acknowledged that at certain times, and with certain types of data, restriction of exchange of information is necessary, so long as scientific progress continues to have military activity as one of its chief values. The immediate problem is to limit such restrictions to a minimal area. The ultimate problem is to free society as a whole and thereby science itself from the tyranny of war.

Not all artificially imposed restrictions on communication result from government requirements. There is an understandable tendency on the part of industry to protect its investment in research by restricting distribution of its results. As a greater share of research is taken on by industry, especially in those areas where expensive, complex operations are involved, this problem will become of greater significance. It is ironical to note that a recent Conference on the Practical Utilization of Recorded Knowledge—Present and Future (at Western Reserve University, January 1956), which devoted a good deal of attention to the problem of improved dissemination of knowledge, found it necessary to hold part of its deliberations behind closed doors and to refrain from publicizing the full record of these "confidential" sessions [*Am. Scientist* (April 1956)].

Unrestricted communication is but one facet of the free intellectual environment that is as important to scientific creativity as it is to all other fields of human endeavor. If society is to benefit broadly and effectively from the efforts of modem science, and if science in tum is to be enriched by contributions from other fields, the social order must provide the greatest possible intellectual, social, and personal freedom for scientist and nonscientist alike at every level of the social structure.

Major Social Issues of Scientific Origin: Signs of Trouble

How well have we solved those social issues which are most closely related to scientific or technologic knowledge? Most of our successes are self-evident. Scientific knowledge is being applied to the development of a new industrial system capable of greatly increasing, in both quantity and quality, the total wealth of man. We are creating a remarkable establishment for medical and related research, which has given us mastery of many human ills and has prolonged the span of life. Nevertheless, scientific problems which influence social processes have become an arena of serious difficulties. In some situations our enhanced ability to control nature has gone awry and threatens serious trouble. Some examples follow.

Radiation dangers. It is hardly necessary to point out at this time that the difficulties created by the dispersion of radioactive materials from nuclear weapons have caused considerable concern in this country and throughout the world. Regardless of one's attitude toward the necessity of setting off nuclear explosions for testing purposes, there is considerable evidence that this aspect of human control over nature is a potential danger to life. The recent controversy over the immediate significance of this problem shows that we have not yet developed methods for the orderly determination of the facts, in an area in which such facts may influence the health of the whole population of the earth.

Food additives. The enormous growth of industry based on organic synthesis, coupled with the already mentioned tendency toward rapid exploitation of scientific knowledge, has resulted in a great increase in the number of man-made compounds now used in foods or otherwise ingested or absorbed by human beings. The period of use of many of these substances has been rather short, and possible undesirable long-range biological effects have not yet had time to appear. Laboratory methods for studying delayed biological effects such as carcinogenicity are unfortunately difficult to manage and equivocal in interpretation. Consequently, the establishment of certification procedures which might assure the public that a given additive is harmless is a difficult matter which has been the subject of considerable discussion and controversy. Nevertheless, additives are in use, and the problem of making a reasonable determination of their safety must be faced.

A parallel situation exists in connection with the health hazards that arise from the dissemination of fumes, smogs, and dusts by industrial plants and from automotive and other combustion processes. The harmful biological effects of these agents usually appear a long time after the commercial usefulness of the process is established and large-scale operations are in effect. By then remedial procedures are very diffuclt to carry out.

In these cases the use of substances resulting from scientific advance has already outstripped the base provided by our scientific knowledge. Information on the biological effects of a new substance is acquired at a very much slower pace than the rate at which new substances are made or put into use. It is probably inevitable that biological research will move more slowly than either chemistry or physics, but it should be expected, therefore, that we would put correspondingly more effort into research on biological phenomena. The opposite is the case. Less than about 10 percent of our total research expenditure goes into biology and medicine.

Natural resources. The natural resources contained in the crust of the earth comprise the major source of our wealth, and it is a matter of concern that they be properly used. The natural laws which regulate the character and behavior of these resources lie within the domain of the various sciences. However, social decisions actually control what is done with our resources. It has been pointed out by Paul B. Sears that these decisions are really in the hands of scientists. Under these circumstances large-scale changes in our natural resources have occurred without proper consideration for the consequences which might be expected from a knowledge of natural laws.

An illuminating example cited by Sears is the recent flood disaster in New England. He points out that the widespread damage caused by these floods was a direct consequence of the unplanned crowding of housing areas into the river flood-plains. This was a failure to recognize and act upon physical events easily foreseeable from a relatively simple knowledge of the landscape. The declining water-table caused by irrigation practices illustrates a similar disregard for natural laws. In these and more complex instances, the harmful outcomes of the given practice can be predicted by appropriate technical analysis.

These examples show that social factors condition the use to which scientific knowledge is put. Perhaps the most striking example of this phenomenon is modern warfare, which represents a social decision to use the power of scientific knowledge for purposes of destruction and death.

Some Conclusions

The present state of science and its relation to the social structure of which it is a part are characterized by the following general features.

1) We are witnessing an unprecedented growth in the scale and intensity of scientific work. Research has placed in human hands the power to influence the life of every person in every part of the earth.

2) This growth has been stimulated by an intense demand for the practical products of research, especially for military and industrial use. Agencies which use the products of research are willing to provide financial support and other forms of encouragement for science but show a natural tendency to favor those fields and aspects of science which most nearly relate to their needs.

3) The public interest in, and understanding of, science is not commensurate with the importance that science has attained in our social structure. It cannot be said that society provides good conditions for the proper growth of science. The effort to explain the nature of science to the public is slight compared with the public attention now given to other, less consequential areas of human activity. Interest in science as a career is so restricted that a serious and worsening personnel situation has arisen.

4) For reasons such as those just cited, science is experiencing a period of rapid but rather unbalanced growth. Basic research, which is the ultimate source of the practical results so much in demand, is poorly supported and, in the view of some observers, lacks vigor and quality. Areas more remotely connected with industrial and military applications, such as biology and the social sciences, are also not being adequately supported. The present period of rapid, unplanned growth in research activities is precipitating critical difficulties in connection with the dissemination and analysis of scientific information.

5) The growth of science and the great enhancement of the degree of control which we now exert over nature have given rise to new social practices of great scope and influence, which make use of new scientific knowledge. While this advance of science has greatly improved the condition of human life, it has also generated new hazards of unprecedented magnitude. These include the dangers to life from widely disseminated radiation, the burden of man-made chemicals, fumes, and smogs of unknown biological effect which we now absorb, large-scale deterioration of our natural resources, and the potential of totally destructive war. The determination that scientific knowledge is to be used for human good or for purposes of destruction is in the control of social agencies. For such decisions, these agencies and ultimately the people themselves must be aware of the facts and the probable consequences of action. Here scientists can play a decisive role: they can bring the facts and their estimates of the result of proposed actions before the people.

Need for Action: The Role of the Organizations of Science

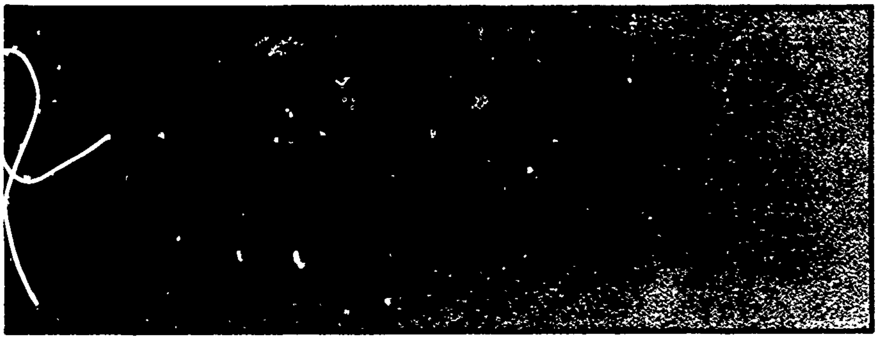
This appears to be a critical time for review of the general state of science and its relation to society. We are now in the midst of a new and unprecedented scientific revolution which promises to bring about profound changes in the condition of human life. The forces and processes now coming under human control are beginning to match in size and intensity those of nature itself, and our total environment is now subject to human influence. In this situation it becomes imperative to determine that these new powers shall be used for the maximum human good, for, if the benefits to be derived from them are great, the possiblity of harm is correspondingly serious.

As scientists we are particularly concerned with determining how we should meet this situation, both as individuals and through our organizations. In marked contrast to other associations, scientific societies seldom consider the social and economic position of their group. Action taken on social problems with a scientific or technologic base are sporadic and usually forced. Yet the democratic system is operated to a considerable extent under stimulus from groups, each representing the views and interest of its members.

Business and labor are not backward in presenting their opinions on social questions that affect them. They make sure that in the final decision their views have been considered. There are many who think that the viewpoint of scientists should also be stated publicly. In fact, if others express their opinions and scientists do not, a distorted picture will be presented, a picture in which the importance of science will be lacking and the democratic process will become to that extent unrepresentative.

The need for action is serious and immediate. Consider, for example, the situation related to the biological hazards of radiation. It is now 6 months since the radiation committees of the National Academy of Sciences issued a report that called for a series of immediate actions including, among others: (i) the institution of a national system of radiation exposure record-keeping of all individuals; (ii) vigorous action to reduce medical exposure to x-rays; (iii) establishment of a national agency to regulate disposal of radioactive wastes; (iv) establishment of an international program of control and study of radioactive pollution of the oceans; (v) considerable relaxation of secrecy about dissemination of radioactivity. In addition, the committees pointed out that "The development of atomic energy is a matter for careful integrated planning. A large part of the material that is needed to make intelligent plans is not yet at hand. There is not much time left to acquire it."

There is no evidence that these urgent pleas for action have yet met with any significant response. Clearly, this is a matter that requires the persistent attention of all scientists. It exemplifies the pressing need that scientists concern themselves with social action. In this situation, the AAAS carries a special responsibility. As one of our past presidents, Warren Weaver, has said. "If the AAAS is to be a vigorous force for the betterment of science, it cannot continue in the face of crucial situations with closed eyes and a dumb mouth." This responsibility has already been recognized. What is needed now is a way to meet it.



For nearly two decades, scientists have viewed with growing concern the troublesome events that have been evoked by the interaction between scientific progress and public affairs. With each advance in our knowledge of nature, science adds to the already immense power that the social order exerts over human welfare. With each increment in power, the problem of directing its use toward beneficial ends becomes more complex, the consequences of failure more disastrous, and the time for decision more brief.

The problem is not new, either in the history of human affairs or of science. What is without past parallel is its urgency.

Four years ago, the report of the AAAS Interim Committee on the Social Aspects of Science (*1*) stated: "We are now in the midst of a new and unprecedented scientific revolution which promises to bring about profound changes in the condition of human life. The forces and processes now coming under human control are beginning to match in size and intensity those of nature itself, and our total environment is now subject to human influence. In this situation it becomes imperative to determine that these new powers shall be used for the maximum human good, for, if the benefits to be derived from them are great, the possibility of harm is correspondingly serious."

The Interim Committee also concluded that "there is an impending crisis in the relationships between science and American society. This crisis is being generated by a basic disparity. At a time when decisive economic, political, and social processes have become profoundly dependent on science, the discipline has failed to attain its appropriate place in the management of public affairs."

In the last few years the disparity between scientific progress and the resolution of the social issues which it has evoked has become even greater. What was once merely a minor gap now threatens to become a major discontinuity which may disrupt the history of man.

Recent events have lent substance to the conviction of our committee and of its antecedent groups—and we believe to that of scientists generally—that scientists bear a serious and immediate responsibility to help mediate the effects of scientific progress on human welfare, and that this obligation should be reflected in the program of the AAAS.

In the present report we endeavor to translate this conviction into action by suggesting a general approach and some specific procedures which may serve as a guide for the development of a AAAS program on the role of science in the promotion of human welfare.

Now, as in 1956, our premises are these (*2*):

1) We are witnessing an unprecedented growth in the scale and intensity of scientific work.

2) This growth has been stimulated by an intense demand for the practical products of research, especially for military and industrial use.

3) The public interest in, and understanding of, science is not commensurate with the importance that science has attained in our social structure. It cannot be said that society provides good conditions for the proper growth of science.

4) For reasons such as those just cited, science is experiencing a period of rapid but rather unbalanced growth. Basic research, which is the ultimate source of the practical results so much in demand, is poorly supported and, in the view of some observers, lacks vigor and quality.

5) The growth of science and the great enhancement of the degree of control which we now exert over nature have given rise to new social practices, of great scope and influence, which make use of new scientific knowledge. While this advance of science has greatly improved the condition of human life, it has also generated new hazards of unprecedented magnitude.

An Estimate of the Present Situation

Since 1956 this general pattern has taken on some new features which concern us at this time.

1) The conscious exploitation of science for military advantage continues at an accelerating rate. But in recent years this process has merged with another, equally important trend: science is being pressed into the service of international politics. Scientific accomplishment per se has become an accepted—and at present dominant—factor of prestige among nations. The philosophy of "getting ahead of the Russians" (or Americans), which once referred only to military matters, now includes scientific achievements as well. This rivalry has strongly motivated the recent intensification of government support for scientific research.

2) The rapid emergence of political independence among the "underdeveloped" nations of the world, and their natural desire to exploit modern technology, has added to the importance of international exchange of scientific knowledge and personnel. Perhaps one reason for the rivalry for scientific preeminence among the more advanced nations is the expectation of political advantage from this exchange.

3) Certain recent scientific advances add directly to the ease with which our knowledge of nature can be applied to the control of human beings and of social organization. Development of new psychotomimetic drugs and psychological techniques have suggested, to some, effective means for controlling the behavior of social groups. Progress in the science of cybernetics and the development of automation techniques result in new capabilities for direct control of social and economic processes.

4) Despite some recent effort toward improvement, there is no reason to alter the earlier conclusion that our present

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social environment does not favor the development of an understanding of science, or of science's aims and needs. The increasingly spectacular practical achievements of science have only accentuated misconceptions about the relative significance, for the growth of science, of practical results and the advancement of basic knowledge. To many people physical science means nuclear energy and rockets. The public is sometimes led to expect that biological and medical research will conquer every human ailment—will overcome death. There is a tendency to equate scientific progress with a sum of money and a number of people. There is insufficient appreciation of the significance of basic research, or of the conditions in which it can flourish.

The situation appears to be this: We are witnessing an unprecedented and accelerating rate of growth in man's power over his environment. Science, the instrument which produces this power, is being consciously exploited for industrial, military, and political purposes. At the same time there is little recognition of the internal needs of science, or of its purposes as a discipline of the human mind.

In this situation it is inevitable that the inner strength of science should suffer, for what is essential to the proper growth of science is often in conflict with the conditions of its service to military and political affairs.

An important example of this effect is the matter of "competition." The military and political advantages, to a nation, of scientific progress within its own borders are self-evident. Yet, it is a truism—but nevertheless a vital one—that nature is the same everywhere, and that the study of nature is an activity of the whole human race. Any effort to divide science into fragments which are delimited by national boundaries, and dominated by a local social philosophy, will inevitably restrict the free discovery and communication of new knowledge that is the substance of scientific progress. A "nationalistic" science is an anachronism which cannot long continue without damage to science, and eventually to the nation.

What, then, is the scientist's responsibility to his own nation's scientific effort? Clearly, we need to understand that what science contributes to the national purpose is measured by what it adds to the sum of human knowledge; science serves the nation by serving humanity.

A further examination of the effects of the present social uses of science on life inside the house of science itself leads to even more disturbing conclusions. There is some evidence that the integrity of science is beginning to erode under the abrasive pressure of its close partnership with economic, social, and political affairs.

In recent controversies about fallout and the detection of nuclear explosions, partisanship on the part of some scientists for a particular political approach to the problem has been so intense in some instances as to cloud—at least in the public mind—the identity between science and an objective regard for the facts.

The grim international competition for "supremacy" in scientific accomplishment also endangers the integrity of science. Unseemly claims of priority may be encouraged. Premature reports of new scientific discoveries, which will occur to some extent in any circumstances, may be permitted to acquire a semblance of credibility.

An illustration—as yet unrealized—is the matter of "the creation of life." Some scientists believe that the properties of life are inherent in the chemistry of nucleic acid, and would regard the artificial synthesis of a reproducible nucleic acid or nucleoprotein molecule—which may occur in the reasonably near future—as the "creation" of life. Other scientists would disagree with this interpretation because they believe that nucleic acid, nucleoprotein, or anything less than a living cell is not "life," for the reason that it is not a self-sufficient replicative agent.

Under ordinary circumstances this difference of opinion would be occasionally debated among scientists and finally resolved when the weight of evidence on one side or the other became sufficiently strong, or when a new and more acceptable idea emerged. However, in the present circumstances this matter may take another course. There is some evidence that a claimed "creation of life" based on the test-tube synthesis of an infectious molecule might be regarded by a government as a scientific accomplishment of great political importance—a kind of "biological Sputnik." In this case, scientists may be hard pressed to persuade government officials—and perhaps even some of their colleagues—that the discovery should be given an interpretation which is less dramatic but more in keeping with the divided scientific opinion of its significance.

It is evident that the accelerating progress of science has evoked a number of serious problems that affect both the social order and the internal situation of our scientific establishment. Having become a major instrument in political affairs, science is inseparably bound up with many troublesome questions of public policy. That science is valued more for these uses than for its fundamental purpose—the free inquiry into nature—leads to pressures which have begun to threaten the integrity of science itself.

Scientists' Approaches to Their Social Responsibilities

It can be seen from the foregoing discussion that the scientific community is faced with numerous problems that very seriously affect the development of science and the future of society. How have scientists responded to this challenge?

Since World War II there has been a considerable growth in scientists' participation in political affairs. The growth has been intermittent, and based on a variety of views of the scientists' relation to social problems.

The Federation of American Scientists, initiated by scientists involved in the wartime atomic bomb project, is frankly designed to give scientists a direct voice in discussions of political matters that relate to science. The Pugwash movement and the less formal groupings represented by the *Bulletin of the Atomic Scientists* take the view that scientists can serve a useful function in proposing political solutions to the international problems that result from the applications of modern science. This approach results in a deliberate effort, on the part of these scientists, to persuade government agencies to follow a recommended course of action. A third group is typified by the Society for Social Responsibility in Science, which takes the view that scientists have a moral responsibility to try to limit to ethical uses the applications of science and technology.

In the 1956 Presidential campaign, *ad hoc* groups of proponents for both political parties developed among scientists. During the past year both parties have organized scientific advisory committees, which will presumably provide the "scientific authority" for the positions that these parties will take in the 1960 campaign.

Finally, some scientists take the view that their proper role with respect to questions of policy that are related to science is to bring to public attention the relevant facts and scientific principles and an explanation of the limits of accuracy and alternate interpretations that apply. Thus informed, the citizen is prepared to make his own choice between possible solutions. This approach has been the basis for the formation in St. Louis of the Citizens Committee for Nuclear Information, a group of scientists and citizens devoted to the dissemination of information about the radiation problem. A group of scientists with a similar purpose, the Scientists Committee for Radiation Information, has been organized in New York City under the auspices of the New York Academy of Science.

This account shows that scientists are trying to meet their social responsibilities in a variety of ways. It also suggests that no single approach has as yet won the active participation of more than a very small part of the scientific community. Nor has there been a sustained development of these activities. Indeed, in the last few years activity on these matters within the scientific community has been relatively slight.

If we regard participation in the resolution of public issues related to science as a part of the scientists' professional responsibilities, we must conclude that the scientific community has not yet developed a consistent, widely supported way of meeting this obligation.

A Suggested Approach

This committee and its antecedent groups have developed a distinctive approach to the matter of how scientists and their professional organizations (and in particular, the AAAS) can best function with respect to the public issues that involve the progress of science.

To begin with, we suggest that the issues fall into two classes with respect to where, in our social structure, their ultimate solutions lie. Certain problems—for example, the effects of public policy on the development of science itself—are matters in which scientists, as scientists, have a particular interest, responsibility, and experience. In the solution of these problems, the opinions of the scientific community should carry some special weight, and scientists should accept the obligation to develop and explain these opinions.

The more difficult problems are those which do not so exclusively concern scientists but which have a broad relation to public policy and affect all citizens equally. An example of such a problem is public policy in relation to nuclear energy, but this is only the most obvious of a growing class of troublesome issues.

It is our view that such problems are in essence social and political. We expect the choice among alternative solutions of these problems to be made through the normal, democratic processes of social and political decision-making, in which all citizens participate.

In this respect the general issues that relate to scientific developments do not differ from other social and political questions. The difference between them lies at a less fundamental level. In the case of the more familiar questions of public policy, the facts which the citizen, or the government official, requires to make an informed choice between alternative solutions are relatively accessible and the consequences of different solutions are more or less apparent. On the other hand, the factual background and implications of the issues with which we are concerned involve scientific and technical data, often in areas relatively new to science, which are in themselves complex. Many citizens are neither familiar with science generally nor well informed about the specific developments which are at the root of present public issues. Scientists as well as other citizens often lack the relevant scientific facts and are unable to visualize the effects of alternative courses of action. In these circumstances, there is little reason to hope for informed decisions about questions of public policy that relate to science.

In our view, this deficiency is a major cause of the difficulties that now impede the proper development of public policy on science-related issues. This conclusion can be documented in detail from recent experience regarding public policies on radiation hazards, food additives and insecticides, the significance of space exploration, the nature of modern warfare, the population question. This list also illustrates the importance which such issues have assumed in public affairs.

The foregoing analysis leads to a distinctive view of the part which the scientist and his professional organizations should play in the social processes involved in the resolution of science-related issues.

With respect to the process of decision-making, the scientist's role is simply that of an informed citizen. Like any other citizen, the scientist is free to express his opinions regarding alternative solutions for matters of public policy and will perhaps join with like-minded citizens in a group effort to foster the solution he prefers. This role does not derive from the scientist's professional competence or obligations but only from his citizenship, and therefore it bears no direct relationship to his professional organizations.

But in the matter of providing citizens with the knowledge required to make informed decisions on science-related public issues, the scientist and his organizations have both a unique competence and a special responsibility. As the producer and custodian of scientific knowledge, the scientific community has the obligation to impart such knowledge to the public.

The scientific community has another special competence (which derives naturally from its concern with new and potentially significant attributes of nature), for attempting to detect incipient problems before they become unnecessarily acute. For example, the likelihood that the relation between nutrition and the development of cancer would eventually become a practical problem for the food industry—a matter which is at present agitating farmers and food processors—has been apparent from the work of investigators in many countries for the past 15 to 20 years.

Early detection of such problems is one of the most important direct contributions science can make toward their solution. Too often the most serious obstacle to the solution of such issues is that they are recognized only after the commitment of massive and essentially irreversible economic and social investments. If the Los Angeles area were now about to be settled, it would be a relatively simply matter, given our present knowledge about the causes of smog, to make plans that would prevent a future smog problem. How much more costly is the real situation, which may require that an entire community's reliance on gasoline-powered transport be altered! In its fields of competence, foresight is a capability and, in our view, a responsibility of the scientific community.

It follows, then, that the scientific community should accept the obligation to determine how new advances in our understanding and control of natural forces are likely to affect human welfare, to call these matters to public attention, and to provide for the public and its social and political agencies the objective statements of the facts and of the consequences of alternative policies that are required as the basis for informed decisions on the relative merits of proposed courses of action.

At what point in the social process should the scientific community enter as an agency of information? One view is that, since most social decisions are executed by government, the scientist's function is to inform and advise government departments and officials. The government does, of course, need such advice, and a number of useful methods of providing it have been evolved. In these instances, scientists serve only by invitation. Inevitably, the general content of the information that is provided and the tenor of the advice that is offered are to some degree conditioned by the particular interests of the requesting agency, which determines what questions are asked and who is given an opportunity to answer them.

Such a relationship does not wholly fulfill the scientist's social role, as we see it. In dealing with social issues, the scientific community must demonstrate its responsibility and its inherent regard for truth and objectivity and must zealously preserve the freedom of thought and communication that is essential to the pursuit of these goals. Accordingly, we believe that the scientific community ought to assume, on its own initiative, an *independent* and *active* informative role, whether or not other social agencies see any immediate advantage in hearing what the scientist has to say.

We believe, also, that what scientists have to say about the social implications of science should be addressed directly to the general public. Our traditional preference for democratic procedures requires that the citizen be sufficiently informed to decide for himself what is to be done about the issues that scientific progress has thrust upon us. Furthermore, our command over natural forces—for example, the destructive potential of nuclear war—is now so great as to create social and moral questions of such great moment that no social agency ought to intervene between the issue and the public.

In sum, we conclude that the scientific community should, on its own initiative, assume an obligation to call to public attention those issues of public policy which relate to science, and to provide for the general public the facts and estimates of the effects of alternative policies which the citizen must have if he is to participate intelligently in the solution of these problems. A citizenry thus informed is, we believe, the chief assurance that science will be devoted to the promotion of human welfare.

Questions of Immediate Importance

Many specific problems command the attention of scientists who are concerned with meeting their responsibility to mediate the interaction between science and society. In choosing certain issues for emphasis, we have adopted the view that it is more important to learn how to deal with the difficult problems than with the simpler ones.

The problems presented below have been chosen with this in mind. Other issues may appear to be more important to other observers; we shall welcome our colleagues' recommendations in this regard.

1) *The social consequences of technological progress.* It is characteristic of the present situation that scientific advances lead to a very profound level of control over our environment and to widespread effects on nature. Often the benefits which are the original aim of a particular application of science are accompanied by secondary effects that cause unanticipated harm. The application of new scientific advances calls for social decisions which weigh the benefits against the disadvantages, and the public needs to have the facts relevant to such a decision. The scientific community faces an immediate need for developing the necessary educational programs. Important examples of such problems include: (i) the general effects of technological advances, such as that of automation on industrial development, or of rapid social changes on health; (ii) the effects of radiation from military and peaceful applications of nuclear energy; (iii) the effects of new organic insecticides, food additives, and food colors on animals and man; (iv) artificial control of the weather; and (v) population control.

2) *The association of scientific research and military activities.* Military usefulness is, at present, a dominant motivation in the social support of scientific research and has a profound effect on the development of our scientific establishment. Any significant change in the pattern of military activities—disarmament, for example—is likely to cause serious changes in research opportunities. The close association of science with recent military advances tends to foster a public image of science and the scientist which is not in keeping with the inherent goals of the discipline. The secrecy associated with military applications may restrict the development of science. Some observers regard the problem of preventing the catastrophic application of the power of science in war as a matter which overshadows all others.

There is an obvious need for the scientific community to give attention to the wide range of problems arising from the close linkage of science and military activity. The role of science in possible efforts toward disarmament and the practical impact of disarmament on scientific research are of immediate concern.

3) *International aspects of science.* Science figures prominently in the intense political rivalry among the major nations of the world. This use of science tends to conflict with its basic international character, and means must be found to resolve this difficulty. A useful innovation has been the development of collaborative international scientific programs, such as those associated with UNESCO, the World Health Organization, CERN, and the IGY. A number of proposals for similar programs in medicine, space research, and oceanography have been made. This area is a fruitful field for developing new ways to foster a sound development of collaborative science. Of particular importance are international programs to provide scientific and technological assistance to underdeveloped nations.

4) *Government support for scientific research.* This problem has received considerable attention in the last few years, but two recent discussions [the Parliament of Science (March 1958) and the Symposium on Basic Research

(May 1959)[1] have shown that no adequate solution is in sight. The basic difficulties seem to be the absence of any over-all rationale in the support of science and the overemphasis on projects that give promise of immediate practical results. We find, as a consequence of this emphasis, that the major part of governmental research support (about 87 percent in the projected 1961 budget) is in the military area, that basic research is inadequately supported, that the pattern of support is not conducive to the development of free inquiry into nature, and that the narrow base of support is distorting the development of science as a whole, in our universities in particular. Various solutions have been proposed, none of which has as yet received really wide support in the scientific community. A specific example is the proposal for the establishment of a federal Department of Science. Clearly, further analysis and discussion of this problem are greatly needed.

5) *How can scientists best meet their social responsibilities*? From what has been said above it is clear that the scientific community has not yet developed a widely accepted means of performing its function in connection with public issues related to science. It would be useful, therefore, to stimulate discussion among scientists on how such activity can best be developed and to encourage efforts which seem to promise success.

6) *The integrity of science.* As science becomes more deeply involved in the frequently discordant affairs of public life and in highly competitive social endeavors, we may expect a growing pressure toward relaxation of the traditional rules for the conduct of science: objective, open communication of results; rigorous distinction between fact and hypothesis; candid recognition of assumptions and sources of error. It is these rules which permit science to progressively increase our understanding of and control over nature. Without them science becomes useless, and even dangerous to the social order. If the scientific community is to accept the obligation to participate in public affairs, means must be found to strengthen the discipline's rules of conduct. Some observers favor the adoption of a code of ethics; others propose less direct means of maintaining scientific objectivity. To begin with, there is a clear need for candid discussion of this problem.

Proposed Procedures for AAAS Consideration

We wish to confirm the general procedural approach suggested by our antecedent committee in its 1958 report to the AAAS Board of Directors. In summary we suggest that AAAS activities follow these steps with respect to any given issue:

1) *Stimulation of discussion, within the scientific community, of issues relating science and human welfare.* Such discussion would result in the identification of issues which the scientific community regards as of most immediate interest and would serve as a guide for the development of a specific program. We urge our colleagues to submit articles and comments for publication, and we welcome direct communications to this committee as well. Symposia and other discussions on these matters should be organized in connection with the AAAS annual meeting.

2) *Assembly of the facts relevant to a given issue.* We propose that a detailed report directed toward the scientific community be prepared after an issue has been identified. Such a report would include the relevant data, a discussion of assumptions and sources of error, and a description of the expected consequences of alternative courses of action. The report would not recommend a specific course of action. The report would be made widely available to the scientific community, either directly or by publication in a suitable journal. For the preparation of such reports we would rely on *ad hoc* committees, conferences, and symposia.

3) *Preparation and dissemination of reports for the general public.* We propose that the content of the report should then be translated into forms suitable for distribution to the public through all available channels. This step is part of the program of the AAAS Committee on Science and the Public and would be carried out in accordance with plans which that committee is developing. An important aspect of these activities is the proposed appointment of a new member of the AAAS staff to supervise work in this area. Our own committee will cooperate in this stage of the program.

4) *Development of liaison between scientists and the public on a local basis.* It is expected that, as the foregoing program progresses, citizens will develop an increasing interest in learning more about the facts relevant to a particular issue directly from scientists in their own community. Many scientists report an increasing demand from local civic groups for lectures on contemporary issues. As already noted, in some communities scientists have formed organizations devoted to informing the public about radiation problems. The British Association for the Advancement of Science has organized local groups to meet public demand for information on these and related problems. The success of these activities suggests possible extension, both geographically and with respect to the types of issues considered. Our committee may serve a useful function in stimulating such developments.

Conclusion

In this report we have reviewed the momentous problems which result from the interactions between the social order and the progress of science. We conclude that the scientific community bears a serious responsibility for helping to solve these problems, and have suggested a program that might accomplish this aim.

Such a program does not yet exist, and it would be appropriate for the AAAS to help bring it into being. The task is not an easy one. It will add to the scientist's burden of work; it will require from the citizen more attention to public affairs; it will demand new social inventions. But we believe that a society capable of producing the enormous new powers of science ought to be capable of finding the means of comprehending their effects on the social order. And we are confident that with such understanding, science—as an expression of the creative gifts of the human mind—will flourish, and the power which it endows will be turned more fully to the promotion of human welfare.

References and Notes

1. *Science* 125, 143 (1957).
2. Premises quoted from the report of the Interim Committee [*Science* 125, 147 (1957)].

Problems of scientific freedom and responsibility are not new; one need only consider, as examples, the passionate controversies that were stirred by the work of Galileo and Darwin. In our time, however, such problems have changed in character, and have become far more numerous, more urgent, and more complex. Science and its applications have become entwined with the whole fabric of our lives and thoughts. On the one hand, basic science has enlarged our intellectual horizons—one need only mention as examples the vistas opened up by the formulation of quantum mechanics and the discovery of the genetic code. Applied science has largely freed mankind from the terrors of infectious disease; but these terrors have been replaced by new terrors, also the fruits of science. These include not only nuclear weapons with their incomparable powers of devastation but also such organic chemicals as the dioxins, accidentally discovered as contaminants of certain herbicides, which can kill guinea pigs at dosages as low as 600 parts per trillion (*1*) and are perhaps comparably poisonous for human beings. The unprecedented rate of population growth in our time, largely a consequence of advances in medical science, sanitary technology, and transportation, threatens the ecological balance of the earth and raises the specter of catastrophic famines.

These and other problems have led to intense and often bitter disputes among members of the scientific community, and between scientists and policy-makers. As examples we may recall the controversies over the biological hazards of nuclear weapons testing, over the use of defoliants and herbicides in the Vietnam War, over the effects of DDT and other chlorinated hydrocarbon pesticides, over the antiballistic missile program, over the supersonic transport, and over the extent of the hazards from nuclear power plants. In these conflicts the contestants are often unevenly matched, with powerful industrial and governmental forces on one side and a small group of critics on the other. There have been attempts to suppress important scientific data that appeared unfavorable to the policies of some powerful organizations. If wise policy decisions are to be made amid such pressures, there is a compelling need for fair hearings, due process, and public access to all relevant information.

The American Association for the Advancement of Science (AAAS) is deeply concerned that such issues should be dealt with, and resolved, in a responsible fashion. It would of course be far beyond the capacity of the Association to act as a court of appeal in such disputes, except in very rare and special cases. However, because of its concern with the policy issues involved, the Board of Directors of the AAAS charged this Committee to consider the following matters:

1) To study and report on the general conditions required for scientific freedom and responsibility.

2) To develop suitable criteria and procedures for the objective and impartial study of these problems.

3) To recommend mechanisms to enable the Association to review specific instances in which scientific freedom is alleged to have been abridged or otherwise endangered, or responsible scientific conduct is alleged to have been violated.

The full text of our report in response to this charge is being issued as a special publication of the AAAS. Here we set forth the major features of that report more briefly.

The Scientific Community:

Its Rights and Responsibilities

The American scientific community, as we define it, includes a wide range of very diverse individuals—basic scientists in universities, research institutes, and government laboratories; engineers; workers in medicine and public health; graduate students and technicians working on scientific problems; and teachers of science in colleges and secondary schools. The discussion that follows applies primarily to scientists involved in basic or applied research, but in large measure it is relevant to the whole scientific community.

The Committee concluded, early in its deliberations, that issues of scientific freedom and responsibility are basically inseparable. Scientific freedom, like academic freedom (*2*), is an acquired right, generally accepted by society as necessary for the advancement of knowledge from which society may benefit. Scientists possess no rights beyond those of other citizens except those necessary to fulfill the responsibilities arising from their special knowledge, and from the insight arising from that knowledge.

Later we shall have much to say of the activities often referred to as "whistle blowing" in which issues of freedom and responsibility are inextricably intermingled. Whistle blowing involves situations in which a scientist, engineer, physician, or other expert becomes aware of hazards arising from some process, material, or product, or becomes aware of possible improvements in technology or procedure that deserve to be adopted but are being neglected. Issues of public safety are frequently involved, and often the whistle blower works for the marketer of the process or product. Some may argue that persons with expert knowledge have a "right" to release information in their possession, if such release is in the public interest. Others would say that it is the responsibility of such experts to release the information, even though they might prefer to remain silent. Both rights and responsibilities are clearly involved here, but it seems clear to us that the responsibilities are primary

Presumably the potential whistle blower will begin by reporting his concern to his

This article is an abbreviated version of the report of the AAAS Committee on Scientific Freedom and Responsibility, prepared on behalf of the Committee by John T Edsall The members of the Committee, which was established in 1970, are: Allen V Astin, Director Emeritus of the National Bureau of Standards and Home Secretary of the National Academy of Sciences, Chairman John T. Edsall, Professor of Biochemistry Emeritus, Harvard University; Walter J Hickel, former Governor of Alaska and former U S Secretary of the Interior, John H Knowles, President of the Rockefeller Foundation, Earl Warren, former Chief Justice of the U S Supreme Court (who died in July 1974), and Dorothy Zinberg, Lecturer in Sociology, Harvard University Mary Catherine Bateson, Associate Professor in the Department of Sociology and Anthropology, Northeastern University, served on the Committee until early 1972.

employer and urging that corrective measures be taken; if the matter can be settled without appeal to outside authority, so much the better. If this step fails, however, and the concerned employee decides that he has the responsibility to make the matter public, he faces obvious risks that may include the loss of his job. If whistle blowers are to be encouraged to take such risks—and we believe that they should be encouraged, when serious issues are involved—they must be assured of some form of due process in passing judgment on the issues that they raise. This would call for the presence of outside independent members on any board that passes judgment on the issues, and should also include some right of appeal. We return to these matters later.

Should There Be Forbidden Areas in Basic Research?

Those for whom the advancement of knowledge is a supreme value might believe that, in basic research as distinct from applied science and technology, no subject should be declared off limits. Yet there are clear inhibitions on some kinds of research involving human beings, and indeed animals. Today we are increasingly conscious of the need for informed consent in studies of human physiology and behavior that may involve risk to the experimental subject. With young children, informed consent is impossible to obtain; parents or guardians must take the heavy responsibility of giving consent for the child. Some experiments may endanger the health, or even the lives, of the participants; some psychological experiments could be regarded as morally degrading or psychologically damaging. In such cases, review of the proposed experiments by a qualified panel of experts may provide more effective protection to the subjects than the attempt to obtain their informed consent, although we would insist on the importance of the latter.

Some experiments are justified, even if they involve great risks. The experiments that conclusively demonstrated the transmission of yellow fever by mosquitoes involved the death of one subject, Dr. Jesse Lazear. Those who took part knew they were risking their lives. Such heroic experiments are fortunately seldom called for, but they may well be needed from time to time in the future.

Recently, in a statement probably unprecedented in the history of science, a group of eminent molecular biologists, headed by Paul Berg of Stanford Univer- have deliberately renounced, for the being, certain experiments on the transplantation of foreign genes into bacteria because of potential though as yet unproven hazards to human health (*3*). This group has spoken with authority as the Committee on Recombinant DNA Molecules of the Assembly of Life Sciences of the National Academy of Sciences. Their views have received wide public attention; of course, this committee has no police powers to enforce its recommendations, but its influence is great and as yet appears not to have been seriously challenged. The members of the committee are well aware of the dilemma; the experiments that are, for the present, being renounced are not only of great scientific interest but also might make positive contributions to human health and well-being. The decision involves an expert balanced judgment of probabilities and risks (*3a*).

The grounds cited for refraining from these experiments—to protect human beings from possible new and dangerous infections—are quite different from the ethical problems of present or future "genetic engineering," as with the possible production of multiple copies of people with identical genotypes by cloning. The suggested threats here are not so much to health as to human integrity, dignity, and individuality. It seems to us proper to be on the alert for such possible threats, but we see no justification as yet for attempts to impose restrictions on the freedom of genetic research. We hold that the dangers today are remote, and that they are decisively outweighed by the great benefits that such research can bring.

Restrictions on Needed Research: Fetal Research as an Example

As we have said above, the advancement of knowledge by research must often be balanced against risks to experimental subjects that may be involved in gaining that knowledge. The complexity of the issues involved emerges from the diverse points of view that have been collected and set forth in much recent discussion, notably in a comprehensive book (*4*) and in a symposium (*5*).

In some important instances we believe that current restrictions on research have gone too far. Thus the National Research Act of 1974 has, at least temporarily, banned research on any "living" human fetus, either before or after induced abortion, except in the very unlikely event that the experiment is intended to save the life of that particular fetus (*6*).

We strongly oppose such restrictions. Research on the human fetus, over the past two decades, has yielded major benefits for human health. Behrman (*7*) has pointed out how many diseases that we can now diagnose or treat, or both, would have been unmanageable if a ban on fetal research had been in effect. Especially notable is Rh disease, which is now totally preventable, thanks in considerable part to fetal research. The human fetus is extremely susceptible to many drugs, as the thalidomide disaster dramatically demonstrated. Likewise, the fetus is far more susceptible to radiation damage than the adult. Research on fetuses that do not survive abortions can help us to discover how to give protection from such harmful agents to thousands of other fetuses that are destined to reach full term and to grow into healthy adults. We would urge that fetal research not only be permitted but intensified, subject to careful peer review of the research projects involved.

The Conflict between Science and Secrecy

Many scientists, especially those employed in industrial firms and in some government laboratories, spend much or most of their time in work classified as secret. Often the grounds for secrecy appear compelling. Nevertheless, science inevitably suffers from the imposition of secrecy on a research project. The reasons were well stated in the report of the AAAS Committee on Science in the Promotion of Human Welfare (*8*, p. 177):

> Free dissemination of information and open discussion is an essential part of the scientific process Each separate study of nature yields an approximate result and inevitably contains some errors and omissions Science gets at the truth by a continuous process of self-examination which remedies omissions and corrects errors This process requires free disclosure of results, general dissemination of findings, interpretations, conclusions, and widespread verification and criticism of results and conclusions

That report provided specific examples to illustrate these general conclusions and showed that secrecy almost always impedes scientific progress; in applied science and technology it frequently permits hazards to develop that could be eliminated if information were publicly available We believe that, with rare exceptions, data that provide a significant advance in fundamental science should not be kept secret, except in a major war situation, as with the atomic bomb in World War II Even in such cases information should remain classified only for a limited and specified time: it should then be released automatically, unless a strong specific case can be made for withholding a particular piece of information for a further limited time. We should look at claims of "national security" with a very critical eye; such claims, as we have good reason to know from recent experience, often serve to cover up governmental ineptitude or corruption.

Technology and Innovation: Their Multiple and Complex Effects

When we turn from the problems of basic science to those of applied science and technology, the problems of freedom and responsibility become even more formidable. Whatever the intentions of technological innovators, the results of innovation are always more complex than the innovators intended, and usually more complex than they could even imagine. These facts, in our time, have created a compelling need for the assessment of major technological innovations, and for their critical evaluation and control. The so-called "side effects" of innovation are often deleterious and not infrequently are so pronounced that they dominate the primary effect that was intended.

The history of the use of DDT provides an example of such complexity (*9*). Its use brought a dramatic halt to a cholera epidemic in Naples in World War II; its initial success in destroying agricultural pests was spectacular. Only a few biological experts warned of trouble at that time Gradually problems appeared, as insect pests developed resistance to DDT, as, in many cases, the pesticide destroyed the natural enemies of the pests, as the long persistence of DDT in soil was discovered, and its progressive concentration in food chains led to the killing of great numbers of certain birds and fishes. All this has led, in the United States, to the banning of DDT for nearly all uses Yet in countries where malaria is widespread, the spraying of house interiors with DDT has proved the most effective antimalarial technique available. In Sri Lanka (Ceylon) malaria had almost disappeared by 1963, when the DDT program was stopped. Then, in 1968 and 1969, there was an explosive increase in the number of infections. In 1967 only 3,465 cases had been reported; in 1968 the number rose to 425,937, and in early 1969 the rate of infection was even higher (*10*). Other factors were at work, of course, the World Health Organization attributed the rise in the incidence of malaria in large part to conditions that were unusually favorable to the breeding of the *Anopheles* mosquito and to unusual human population movements that helped to spread the infection (*11*). Nevertheless, until we have something better, DDT appears to be an essential component of any major program of malaria control.

Other complexities have arisen in connection with programs for international development. Dams and irrigation schemes have vastly increased the incidence of bilharziasis (schistosomiasis), a debilitating disease for which there is as yet no effective cure (*12*). The disastrous effects of the recent catastrophic droughts in the Sahel region, south of the Sahara, have been accentuated by some technological innovations, notably, for example, by the drilling of thousands of deep boreholes which have tapped the abundant water far below the surface. The resultant wells encouraged a great increase in the size of the cattle herds, pasture instead of water became the limiting factor in numbers of cattle. As pasture dried up in the drought, countless thousands of dead and dying cattle were clustered around the boreholes, while the surrounding land, for miles around, was ravaged by trampling and overgrazing. These wells, drilled by men of good will and technical skill in order to bring more water to the people and cattle of the Sahel, became a major factor in intensifying a great human and natural disaster (*13*).

These illustrations could be multiplied a hundredfold. Let us add at once that we are not among those who consider that there is something inherently evil about technology. Technology has been indispensable for the rise of all civilizations, and new technology is essential for the survival of our own civilization. As an example we need only mention the need for the development of new energy sources solar and geothermal energy and nuclear fusion, for instance that may be less polluting than fossil fuels and less hazardous than nuclear fission.

Likewise we reject the notion of the so-called "technological imperative" the idea that we must pursue new technological possibilities, wherever they may lead Thousands of projects may be technically feasible, including the destruction of all human life on Earth Even among those projects that appear attractive at first sight, careful appraisal may lead to the conclusion that they will do more harm than good. The U.S. Congress drew such a conclusion in 1971, when it voted to cut off funds for the supersonic transport program, which it had been generously funding a few years earlier.

In summary, the development of new technologies is indispensable, the training and encouragement of gifted and imaginative technologists deserves a high priority among our national needs, but the multiple repercussions of new technology need to be critically evaluated before they are introduced and constantly monitored after their first introduction. Many schemes that are technically brilliant must be rejected because their wider impact would, on the whole, be more damaging than beneficial. In some cases it would be preferable, in the eyes of some thoughtful scientific policy advisers, not to carry a project from the stage of research even into preliminary development, lest pressures would then arise that would lead to its full development.

Conflicts Involving Scientific Freedom and Responsibility

We turn now to the second item in the charge to this Committee, which calls for us "to develop suitable criteria and procedures for the objective and impartial study of these problems" The conflicts that call for such study generally lie in the realm of applied science and technology, including medicine and public health In most instances they involve the right, and the responsibility, of an employee to warn concerning the dangers inherent in some product or process with which he has become familiar in th[illegible]urse of his work; that is, the "whistle blowing" activity of which we have spoken earlier. The employer may put intense pressure on the employee to keep quiet; the employee may be fired, or life may be made so uncomfortable that he decides to resign. Interested parties may seek to suppress scientific data that are vital for the proper resolution of a controversy. On the other hand, the employee may be acting not in the public interest but out of prejudice or spite against the employer. To be an "objective and impartial" judge in such circumstances is not easy; one person's "objectivity" looks like bias to another. In all these cases scientific evidence is an essential part of the whole situation, but wise decisions involve complex human factors, ethical judgments, and standards of value that go far beyond purely scientific argument. We consider briefly a few specific cases, before discussing criteria and procedures.

The problem of standards for exposure to radiation. A few years ago, Drs. J. W. Gofman and A. R. Tamplin, who worked at the laboratories of the Atomic Energy Commission (AEC) in Livermore, California, claimed that existing standards for exposure to ionizing radiation were far too tolerant, and would permit a large increase in the number of deaths from cancer if exposures rose to the allowed levels. They publicized their views widely. The chief authorities in the AEC sharply opposed these views and held them to be invalid. Gofman and Tamplin eventually left the AEC, after what they believed to be considerable harassment, and eventually the AEC did impose considerably stricter radiation standards for the protection of workers within its jurisdiction. A committee appointed by the National Academy of Sciences, after intensive study, produced a report, commonly known as the BEIR (Biological Effects of Ionizing Radiation) Report (*14*), which stands at present as the most authoritative statement on the hazards of ionizing radiation. In our full report we consider this controversy at somewhat greater length.

The case of the BART engineers. A major feature of the Bay Area Rapid Transit (BART) system in the San Francisco Bay area was to be the Automated Train Control (ATC) system, for which a contract was awarded to the Westinghouse Electric Corporation in 1967. Beginning in April 1969, three of the engineers on the project became increasingly concerned about what they saw as serious defects in the system. They expressed their concerns to the management but drew no significant response except vague warnings not to be "troublemakers." Late in 1971 the three engineers decided to take their case to the Board of Directors; this led to a public hearing in February 1972, after which the Board voted 10 to 2 in favor of the management and against the engineers. The managers then told the three engineers that they could choose between resigning and being fired; they refused to resign and were summarily dismissed. Subsequent events vindicated their concern. The ATC system failed on several occasions; the failures were so dangerous that the system could not be used, and it became necessary to control the trains in the traditional manner.

The California Society of Professional Engineers (CSPE) initiated an inquiry. However, BART's top management refused to meet with them, or to offer explanations to anyone. Then CSPE undertook a full investigation of the firings, which brought out much disturbing information, and the California State Legislature set in motion a study, resulting in a report (the "Post Report") which essentially confirmed the warnings of the three engineers. Although CSPE took tentative steps toward a court action on behalf of the engineers, the society did not follow through on this. The engineers themselves launched a suit against BART for $885,000, the outcome of which we have not yet learned (*15*).

The case of data suppression concerning the carcinogenicity of vinyl chloride. In May 1970 an Italian investigator, Dr. P. L. Viola, reported at a cancer congress that high concentrations of vinyl chloride caused cancer in rats. Up to that time there apparently been no research in the [plas]tics industry concerning the possible hazards of vinyl chloride, although tens of thousands of workers were exposed to it. More than a year later, the Manufacturing Chemists Association (MCA) in the United States initiated its own research and in October 1972 entered into an agreement with the plastics manufacturers, who sponsored the European research, to share information but not to reveal it without the consent of the European manufacturers. In August 1972 Dr. Cesare Maltoni of Bologna found angiosarcomas and various tumors in experimental animals exposed to vinyl chloride concentrations as low as 250 parts per million (ppm). At that time the allowed exposure limit for workers in the United States was 500 ppm. In January 1973 U.S. scientists visited Dr. Maltoni and learned of his findings. However, they did not notify the National Institute of Occupational Safety and Health (NIOSH) concerning these extremely ominous findings, nor did they make any attempt to warn the public or the workers exposed to vinyl chloride. In late January 1973 NIOSH requested information on possible hazards associated with occupational exposure to 23 chemical substances, including vinyl chloride. On 7 March, MCA responded by recommending a precautionary label that made no mention of toxic effects on animals or people; in other words, it appears to have deliberately deceived NIOSH regarding the true facts. Apparently MCA has claimed that the withholding of data was due to their agreement with the European manufacturers to keep the data confidential until an agreement for their release could be worked out. The fact remains that, because of the suppression of these data, tens of thousands of workers were exposed without warning, for perhaps some 2 years, to toxic concentrations of vinyl chloride. On 22 January 1974, B. F. Goodrich announced that three polyvinyl chloride workers, in just one of their plants, had died of angiosarcoma of the liver since 1971. On the same day MCA revealed the Maltoni data to NIOSH. The Occupational Safety and Health Administration, on 5 April, in an emergency action lowered the permissible exposure level from 500 to 50 ppm; drastic further lowering, to 1 ppm or less, is in prospect (*16*).

The evidence here seems clear: a considerable number of scientists were aware of the hazards of vinyl chloride long before the facts were made available to NIOSH or to the public; yet they kept quiet and gave no warning. As the Federation of American Scientists states, "... industrial scientists who fail to challenge conspiracies of silence within their firms are not rebuked; rather, they are often quietly rewarded for their loyalty."

In industrial medicine and public health similar situations often arise. In recent books Paul Brodeur (*17*) and Rachel Scott (*18*) have described the medical problems of workers exposed to asbestos particles, beryllium dust, and other hazards. Many of these workers develop cancer, or fatal respiratory or other diseases, over the course of 10 or 20 years; the risk is high. Yet the company physicians who look after the workers sometimes join their employers in minimizing the need for more rigorous standards of health protection. This appears to be a clear abdication of the prime responsibility of physicians or public health workers to place the health of the people for whom they are responsible before all other considerations. Obviously a doctor who is paid by a commercial enterprise will find it very difficult to act contrary to the policy of the company. The doctor who works for an independent inspecting agency will be in a much stronger position. Even so, we know from experience that regulatory agencies often become the subservient allies of the organizations that they are supposed to regulate and may collaborate with the commercial organization in concealing the hazards.

Criteria and Procedures for the Resolution of Conflicts

Having now indicated the character of the problems, we now turn to criteria and procedures that may aid in resolving them. We approach the problem of establishing criteria by asking questions rather than by offering answers. How will the proposed decision or procedure affect human health and safety, and the general quality and amenities of life, for all the people concerned? Will a decision to require a drastic cleanup of operating conditions in a certain industry cause much of the industry to close down, with loss of jobs and production? How should such risks be balanced against considerations of safety and health? What are the possible large-scale environmental effects of the present operations and of the proposed changes? Have possible future effects been carefully considered, as, for instance, with the widely used aerosols, which liberate Freons that, in a decade or two, might (but again perhaps might not) destroy much of the ozone in the stratosphere? These are some of the problems that will have to be faced in the formulation of criteria. Obviously it is far easier to impose suitable regulations on a new enterprise than on a powerful industry that is already established, with a huge capital investment and with faults of operation and production that are already deeply entrenched. Thus the need for foresight in technology assessment is overwhelming; yet foresight is always imperfect and needs constantly to be corrected by further experience.

We turn now to procedures: how is the whistle blower to be assured of a fair hearing, without fear of reprisals, and with a good prospect that his recommendations, if they are found to be sound, will actually be put into practice? Many scientific and engineering societies have developed codes of ethics relating to the responsibility of employers and to the professional and personal conduct of scientific and technical employees. A highly articulate expression of such concerns is to be found in a statement on "Employment Guidelines," which has now been adopted by at least 20 engineering and scientific societies. For the most part, it is concerned with the general principles that should govern relations between employers and employees, but it also contains the significant statement: "The professional employee should have due regard for the safety, life and health of the public and fellow employees in all work for which he/she is responsible. Where the technical adequacy of a process or product is involved, he/she should protect the public and his/her employer by withholding of plans that do not meet accepted professional standards and by presenting clearly the consequences to be expected if his/her professional judgment is not followed" (*19*, p. 59).

The formulation of such a declaration is a significant event. How much it means depends, of course, on the effectiveness with which it is applied. Moreover, these guidelines, like most such codes of ethics that we have seen, lack a very important ingredient, namely, a provision for the arbitration of disputes. The protection of individuals from arbitrary action by authority is deeply ingrained in English common law, and the U.S. Constitution provides that "no person shall ... be deprived of life, liberty, or property without due process of law." We believe that some form of due process should be an essential part of any employer-employee agreement or contract, to protect the employee from arbitrary action by the employer, allegedly based on professional or personal misconduct. A minimum requirement for such due process would involve a hearing by a board, including independent members, with the right of appeal to some reasonably neutral but professionally qualified higher authority. Codes of professional ethics are likely to be ineffective unless some type of due process is provided for the resolution of disputes. Without this, scientific freedom is likely to be abridged. We therefore strongly recommend that all employment contracts involving scientific or professional employees include such provisions for the review of disputes through hearing and appeal processes. Provision for neutral or third-party participation is important, particularly when issues of public interest are involved.

Professional Societies as Protectors of the Public Interest

How active can, and should, professional societies be in actively fighting on behalf of their members who are attempting to defend the public interest? Most such societies have in the past remained aloof from conflicts of this sort and have often taken the attitude that the purity of their devotion to the advancement of their respective sciences would somehow be contaminated if they entered the public arena to contest such issues. We believe that such attitudes are no longer appropriate. The scientific community can no longer remain apart from the conflicts of our time, where so many technological decisions are being made that vitally affect the well-being of society. We are not proposing that professional societies should take public stands on large general political issues, such as the legitimacy of the Vietnam War; individual members of the societies, when their concern is aroused, should deal with these matters by other mechanisms. However, in matters directly related to the professional competence of members of the society, where the public interest is clearly involved, we believe that the societies can and should play a much more active role than they have in the past. They can deal with such issues by setting up committees of inquiry in cases where a serious violation of scientific freedom or responsibility is suspected, by publicizing the results of the inquiry in professional journals, and, if necessary, in the more popular journals and in the news media, and by calling the matter to the attention of governmental bodies, as with the California Legislature in the BART case. They can on occasion launch lawsuits on behalf of their members who have apparently suffered injustice when acting on behalf of the public interest.

In stating this, our major new proposal for dealing with "the objective and impartial study of these problems," we are aware of the difficulties that the proposal will face. The most serious problems are those of time and money. Most professional societies have limited funds; many operate more or less on a shoestring. They keep members' dues fairly small; otherwise members drop out, particularly in times of economic hardship. The fighting of difficult cases, on behalf of members involved in controversies, can be a very expensive business, especially if the case goes into the courts. In any event, responsible scientists would be required to spend substantial amounts of precious time serving on hearing panels, studying large bodies of evidence, and preparing reports.

When a professional society does fight for the rights of its members, it is more likely to be concerned with defending their status and pay than to be acting primarily on behalf of the public interest as its primary motive. The impetus to take actions of the latter sort is likely to be much less strong than the desire to provide direct help to members of one's own professional group.

These are powerful obstacles to our proposals, but they are not insuperable. Societies that share common interests, but which may be individually too weak financially to support such activities, may band together in groups to finance the necessary operations. There are increasing pressures upon scientists, engineers, and other members of the scientific community to face these public issues and deal with them effectively. These pressures come both from the public and from within the ranks of the scientists themselves. We are well aware of the mistrust and hostility toward science that is manifest in many quarters; one reflection of this attitude is the decline in government support of scientific research in recent years. Such hostility will almost certainly grow unless scientists exhibit greater concern for preventing the misuse of science and technology. As these concerns become more intense, it should become easier for the professional societies to obtain additional funds to finance the expenses of lawsuits, hearing panels, and other activities undertaken in the defense of the public interest. Whether government funds could or should be available for such purposes is open to question; but it is likely that some of the major private foundations, either those now in existence or those yet to be created, will see the urgency of supporting such public service activities. The need for these activities may also lead to the creation of other social mechanisms for dealing with these problems, of a sort that we cannot now foresee. We look to increased activity of the professional societies as the most hopeful approach to the problem in the immediate future.

The Role of the AAAS in the Defense of Scientific Freedom and Responsibility

We now consider the third charge from the AAAS to this Committee: "To recommend mechanisms to enable the Association to review specific instances in which scientific freedom is alleged to have been abridged or otherwise endangered, or responsible scientific conduct is alleged to have been violated." The domain of the AAAS is very broad, including all of the sciences and a large number of professional societies which belong to it as affiliates. Hence, by its nature the AAAS cannot undertake the role that we have envisaged for the professional societies in the preceding section. The number of possible appeals to the AAAS, if it were to undertake the responsibility of reviewing particular instances of alleged threats to scientific freedom and responsibility, would be immense. If it were to agree to handle some such cases, it would have to be rigorously selective and deal only with those that were at once so important and so broad in scope that they would fall outside the domain of any individual society.

Because of its multiple affiliations with the professional societies, the AAAS can play an important part in coordinating many of the activities of the societies. A number of the societies, for example, are now formulating, or revising, codes of ethics for their members, which will involve policies for dealing with issues such as we have been discussing here. The AAAS can help to provide an exchange of information on these matters among the different societies and thereby promote a more unified approach to these complex issues.

The AAAS is already playing a significant role in dealing with alleged abridgments of scientific freedom, and alleged violations of responsible scientific conduct, through active discussion of such matters in *Science*, chiefly in the News and Comment section, but also in some of the lead articles and in the Letters section. Since *Science* is so widely read, both inside and outside the scientific community, this is one of the most important channels now available for bringing such problems to the attention of the public. To focus public attention on such problems, of course, is not to resolve them, but it is an essential step toward such resolution. We recommend that *Science*, without any drastic change of its present editorial policies, enhance its coverage of such matters, particularly by inviting distinguished academic, industrial, and government scientists who are well informed on some of these controversial issues to set them forth in its pages. In some cases, it will be desirable to present two or more articles by different authors, expressing more or less contradictory points of view. In scientific controversies, it should not be necessary for the champions of different views to operate like adversaries in a court of law; the opposing sides presumably should be able to find a large area of agreement about scientific facts that are not in dispute. The real disagreements in such cases usually turn not on the scientific facts but on the relative weight to be given to different *kinds* of scientific facts, and on extrascientific issues involving political judgment and broad general perspectives on human nature and human motives. These factors always enter into the practical decisions that must be made in applications of science and technology. When a scientist or technologist states a case for action of a certain sort on such an issue, it is important that he make clear the general presuppositions from which he starts. He may, of course, be unconscious of these presuppositions; if so, *Science* should as a matter of editorial policy bring them to the surface. This is important, both for the rational discussion of the issues involved and for the maintenance of public confidence in the honesty and objectivity of scientists.

On rare occasions the AAAS may and should become actively involved in broad issues that are important and controversial, as it did when it decided to conduct an investigation of the effects of defoliants and herbicides in Vietnam. This was an important attempt to obtain scientific evidence on an issue that had aroused passionate controversy (*20*). The National Academy of Sciences, at the request of Congress, later undertook a more extensive study of the same problems, with increased financial support provided by the Department of Defense. The resulting report of the National Academy committee (*21*) greatly extended and amplified the AAAS report; the findings of both are for the most part reasonably concordant, but some discrepancies have given rise to controversy (*22*). These disagreements can be resolved later, when it is possible to conduct studies of the Vietnamese forests on the ground in a peaceful setting; as long as war continues, only aerial observations are possible. In any case the AAAS study performed a valuable service and was in no way rendered superfluous by the later study of the National Academy of Sciences.

This raises the more general question: What should be the relation between the AAAS and the National Academy in matters such as we have considered in this report? The National Academy has special prestige and a unique relation to the U.S. government. It, and its committees, can speak on many issues with far more authority than the AAAS, and it can generally command much more financial support for its investigations. However, the AAAS, with its much broader membership, is more widely representative of American science in general, and its greater independence from governmental ties gives it a greater freedom of action. Both organizations clearly have very important and somewhat different roles to play in the maintenance of scientific freedom and responsibility.

The problems we have been considering here will certainly continue and will probably become more numerous and more acute in the years to come. We hope that the concern of the AAAS will continue. We suggest that, not more than 5 years hence, the AAAS should reexamine the whole problem, perhaps by setting up a committee similar to this one, to see where we stand at that time. Alternatively or in addition, it might set up a committee to receive complaints concerning violations of scientific freedom and responsibility and refer them, when possible and desirable, to appropriate bodies for further study and possible action. Such a committee should not itself serve as a judicial body; its functions should be to refer complaints for possible action elsewhere and to analyze the information received, with an eye to possible recommendations concerning future policy initiatives by the AAAS in the light of this information. The terms of reference of such a committee would need to be very carefully drawn, to prevent the committee from being overwhelmed by a mass of unmanageable complaints.

References and Notes

1 *Perspective on Chlorinated Dibenzodioxins and Dibenzofurans* (Environmental Health Perspectives, Experimental Issue No. 5, Department of Health, Education, and Welfare Publication No. NIH 74-128, National Institute of Environmental Health Sciences, Box 12233, Research Triangle Park, N.C., September 1973) Several articles deal with the toxicity of the dioxins See particularly table 4 in the article by R. Baughman and M. S. Meselson, pp 27-35, which summarizes the results of earlier workers.

2 V Earle, Ed , *On Academic Freedom* (American Enterprise Institute, Washington, D C., 1971)

3. P Berg *et al* , *Science* 185, 303 (1974); N. Wade, *ibid* , p. 332.

3a In February 1975, an international conference of experts at the Asilomar Conference Center in California devoted several days of intensive discussions to this problem and concluded with a recommendation to lift the voluntary moratorium on most of these proposed genetic experiments. However, such experiments were to be done only subject to rigorous precautions for containment of the possibly hazardous organisms, the stringency of the precautions increasing with the estimated risk in different categories of experiments Indeed, for many of the possible experiments, the required conditions cannot be met without the development of new organisms and new techniques, so that the moratorium is in effect being continued for such experiments. See N. Wade, *Science* 187, 931 (1975).

4 J Katz, Ed., *Experimentation with Human Beings* (Russell Sage Foundation, New York, 1972)

5. "Clinical Studies in the Human The Ethical and Scientific Problems," symposium by A. C. Barnes, R Q Marston, B Barber, and J Katz [*Fertil Steril* 23, 593 (1972)].

6. B. J. Culliton, *Science* 185, 426 (1974)

7 E F Behrman, *New York Times* (9 June 1974), opposite editorial page

8 AAAS Committee on Science in the Promotion of Human Welfare "The integrity of science," *Am Sci* 53, 174 (1965).

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10 These figures are from *World Health Organization Relevé Epidemiologique Hebdomadaire* (8 August 1969)

11. R. Garcia, *Environment (St Louis)* 14 (No 5), 2 (1972)
12 For numerous examples of this and many other problems in international development schemes, see *The Careless Technology Ecology and International Development*, M. T. Farvar and J. P Milton, Eds. (Natural History Press, Garden City, N.Y., 1972).
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15. Our description is taken from a more detailed report by S. H. Unger ["The BART Case: Ethics and the Employed Engineer," *Inst Electr. Electron Eng Comm Soc Implications Technol Newsl No 4* (Sept 1973), p 6 (V Klig, Ed, 497 Park Avenue, Leonia, N J 07605)]
16. Our discussion is based on *Fed Am Sci Prof Bull* 2 (No. 8), 1 (1974)
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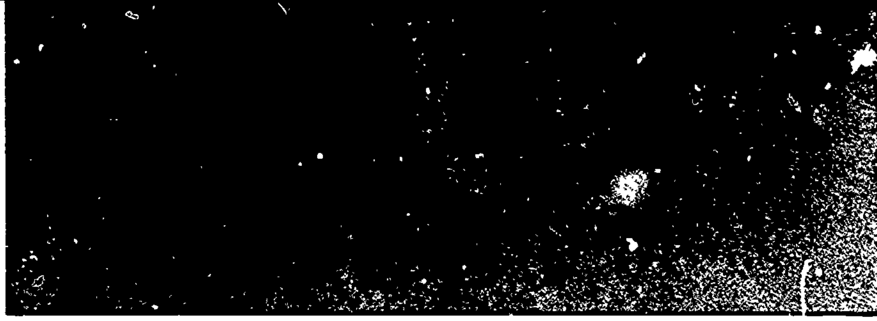
20. M. S. Meselson, A H. Westing, J. D. Constable: Herbicide Assessment Commission of the American Association for the Advancement of Science, Background Material Relevant to Presentations at the 1970 Annual Meeting of the AAAS [reprinted in the *Congressional Record* 118 (No 32), S3226-S3233 (3 March 1972)]
21 Committee on the Effects of Herbicides in Vietnam, *The Effects of Herbicides in South Vietnam* (National Academy of Sciences, Washington, D C, 1974)
22 Concerning this controversy, see, for instance, D Shapley [*Science* 183, [illegible] (1974)] and C Norman [*Nature (Lond)* [illegible], [illegible] (1974)]

260



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