as I dined in the Great Hall. It was the 20th anniversary dinner of the National Science Board; Philip Handler, who as newly elected president of the Academy had completed the auditorium, was our host. When he rose to speak, he first looked about him and up into the dome with an expression of admiration and affection. I was moved and very grateful to my friend and presidential successor, for I saw that he had already sensed the rare qualities of this house that had first been envisioned by George Ellery Hale. They are a blend of that endless quest for knowledge and wisdom that is our mission here and the beauty of the building. It is a fitting home for those whose minds have been trained in the splendid discipline of science, but whose hearts and eyes take also delight in the triumphs of art and the beauties of nature. It is our heritage from the past, our legacy to the future.

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Applied Research for the Public Good—A Suggestion

Methods used for application of research to technology should be tried for social problems.

Harold Gershinowitz

Much of the discussion about the utilization of knowledge and understanding for the solution of such problems as poverty, pollution, urban blight, or excess population has been concerned with the needs for research, with the relative weight that should be given "basic research" as compared with "applied research," and with the proper geographical and organizational location for whatever research is to be done. In contrast, very little is said about the processes involved in the application of research, about the mechanisms by which successful research could be used for the solution of major social problems.

The sequence of basic research, applied research (and, for some disciplines, development), and use of research seems an obvious logical and chronological order, particularly to research workers. However, in those areas in which the application of research has been most successfully accomplished, the reverse order has been prevalent. If one's primary concern is with the use of knowledge to produce change, then the order should be (i) the use of existing knowledge to produce the desired change; (ii) when existing knowledge is incomplete or insufficient, applied research to find the necessary knowledge; (iii) basic research for the understanding of nature (including man and his works), with particular emphasis on those areas in which lack of fundamental understanding limits the scope of applied research; and (iv) basic research without regard to its possible relation to any area of application. I hasten to add that, from the overall standpoint of human society and culture, the last is not the least.

In the latter sequence, the most essential element in the application of research is the mechanism for transforming knowledge into the action that produces the desired change. In science-based industry, the process that brings this about is sometimes called innovation (*1*). This term is deceptively simple, and I propose to devote most of this article to an analysis of just what is involved in the "application of research," primarily in terms of institutions and relations. For much of what I say I claim no novelty—it is well known to both practitioners and theoreticians of industrial research management. These persons, however, constitute a rather special group, the members of which talk mostly to each other. My experience in dealing with public officials and social scientists has convinced me that it is important to restate the problems and the principles in terms relevant to their interests. Although my own experience has been principally in the application of the physical and biological sciences, I think that my conclusions are equally applicable to the social and behavioral sciences.

Considered in this light, the "application of research" is very different from research, even "applied research." It is a very complex interdisciplinary activity. The application of research is not a simple or straightforward process. It takes place at the interface between knowledge and action. At this interface there are barriers of language, of psychology, and of values. Fields of knowledge are fragmented in different ways than fields of action are. It is not generally realized that, even in those areas in which research and development are unambiguously technological in character, many of the problems involved in the application of research are not technological at all.

In order to understand better the nature of the problems with which one is confronted in the application of research, let us look more closely at the two activities on either side of it, applied research and administration. I include in administration both policy-making and responsibility for the execution of policies and programs.

A great deal has been written about the differences between applied and basic research, often with more passion than understanding. For a balanced analysis, see the 1967 report of the

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National Academy of Sciences (NAS) (2), which was prepared for the Committee on Science and Astronautics of the U.S. House of Representatives. For our purposes, the principal characteristic of applied research, and that which distinguishes it from basic research, is not its subject matter, nor the state of mind of its practitioners or sponsors, but its relation to those who use the knowledge acquired. There is a feedback into applied research of the results of its initial or tentative application. This feedback often gives new insight into the nature of the problems that the research is supposed to solve, and this insight can, in turn, change radically the nature of the proposed solutions. Without frequent and cooperative interaction of the doers and the users of research, the work itself can be misguided or misapplied, or both.

The Users of Research

The users of research are, of course, the policy-makers and the managers. Their viewpoints, their temperaments, and, ultimately most important, their responsibilities are very different from those of the research worker. Whereas novelty, or to put it more kindly, new knowledge, is the objective of the research worker, stability is the objective of the administrator. The more enlightened administrators realize that stability can be achieved only by the gradual incorporation of change, but there are very many, particularly in the intermediate levels of bureaucracy and management, who believe that the status quo is the best of all possible states. I have seen on the wall in the office of the manager of a very large petroleum refinery a framed motto: "The better is the enemy of the good."

On the other hand, the research worker is no paragon. He is intolerant of those whose interests, viewpoints, and backgrounds are different from his own. He is skeptical of the ability of the man of action to make proper use of the knowledge that he, the researcher, has worked so hard to obtain.

These and other characteristics of applied research, of the system in which research must be applied, and of the inhabitants of each of these worlds have made it necessary, where research is being successfully applied, to set up rather formal organizational relationships and methodologies in order to reduce the interfacial barriers.

Some of these factors have been discussed in the aforementioned NAS report (2). Most of the applied research in the physical sciences in the United States is done in the laboratories of industry, and the technological prowess of the United States bears witness to its success. But it is pointed out in the NAS report that, in the life sciences and in the behavioral and social sciences, studies of the application of research to practical problems usually are done in, or in close association with, an academic institution.

This last fact is, in my opinion, largely responsible for the lack of success in applying theoretical knowledge, or even problem-oriented research, to the public issues of our time. The magnitude of the problems and the complexity of the organizations and institutions with which they are involved, make it even more essential than it is in industry that some mechanism be provided for the continuous and intimate interaction of the research worker with those who ultimately use the research.

The academic environment is not conducive to such interrelationships. In fact, as seems to be the case with classified military research, the existence of the necessary bonds may be detrimental to the university and to its primary responsibilities to the community (3). I consider undirected basic research to be essential to the future of mankind, both in a cultural and in a practical sense, and the university is the principal place in which such research should be done (4). I recognize the very proper concern of the academic community with the important social issues of our time. I believe very strongly, however, that the most important and valuable contributions it can make are in the category of basic research for the understanding of nature (including man and his works), with particular emphasis on those areas in which lack of fundamental understanding limits applied research. In this category, the channels of communication are still scholar to scholar, not scholar to man of affairs, as they are in applied research.

This is not to say, or even to imply, that university faculties are incapable of participating effectively in the process of innovation. However, most of those who have been successful have made their contributions while acting outside of the university environment and structure, as heads of operating agencies in the Executive Branch of the federal government and as staff members of mission-oriented laboratories. The difficulty lies with individuals in an institutional setting, not with their capabilities as individuals.

Innovation of Technology

In the minds of many of those who are most concerned about our environment and social problems, there is a distrust of technology—because it has contributed so much to undesirable environmental change. The use of technology is associated with industry and with the military, both of which are presumed to operate on principles that do not take into account public welfare and protection of the physical environment.

To my mind, this attitude only beclouds the real issues and problems. During the past three decades there have been developed, particularly in the United States, remarkably effective methods of conducting applied research and of putting to use the results of applied research. In order to solve our major social and environmental problems, it will be necessary to apply all the existing knowledge of both physical and social scientists and to do research in both areas in order to increase that body of knowledge. The techniques and procedures that have been developed to transform scientific and engineering knowledge into technology can be useful for our social problems as well. It would be foolish to refuse to use matches to light a stove because the same match, if applied to a cigarette, can start a chain of events that might lead to cancer.

The quest for solutions to social problems should involve applied research in a sense that has not usually been understood by the social scientist—a continued and close interaction between those who do the research and those who must make the decisions and policies that result in the application of research. The quest should also include rapid and continuous exchange of information and knowledge between those doing the research and those who are doing the things that research has indicated to be necessary for the solution of the problems. To achieve these interactions, it may be necessary to change not only the methodology of research but also the structure of the organizations and institutions involved in the researching and in the solving of social problems. In American industry and in some governmental agencies it has been possible to make these modifications in methodology and structure. This success provides an example and, possibly, a model.

With some risk of oversimplifying, one can summarize the conditions that have led to successful use of research as follows.

1) The formulation of a program of research starts with the definition of a problem in terms that conform to the language, subject matter, and doctrines of conventional scholarly discipline.

2) There is a mechanism for continuing communication between those attempting to solve problems and those faced with the problems. This involves having, at a high level and in the part of the organization that would be responsible for using the results of research, a receiving and transmitting post in which the often disparate language of research, problem solving, and action are reconciled. In most organizations, this is the research director and his staff.

3) There is often an individual, sometimes called the "technical entrepreneur," who assumes responsibility for ensuring that the results or recommendations of research are put to use, or are at least examined adequately and justly before being rejected.

4) The utilization of research is facilitated (although not made automatic) if those who are to use it have some of the responsibility for planning and financing the research.

It is in the large, science-oriented corporations of the United States and their laboratories that one finds these conditions most nearly met. There is no single formula, however. Corporations vary greatly in structures and in objectives. The adaptation of research programs to corporate objectives and the utilization of research by the corporation involve interactions that work both ways and make necessary the occasional revisions not only of the research program, but even of the corporate objectives and structure. In a complex corporation that makes or mines most of its raw materials, manufactures both intermediates and consumer products, and sells to both industries and consumers, there are often conflicts of interest among its various parts. The successful corporation functions through the cooperative efforts of different kinds of activities and interests. The implementation of research often requires the willing cooperation of several elements of the corporate structure. For this reason, research is usually on the same corporate level as, for example, manufacturing and marketing, but with many horizontal channels of communication below the executive level. Research operates most effectively when it occupies a position in the organizational structure that permits its workers to be continuously aware of corporate problems and to participate in the planning process. This should also be true of research for the public good.

Innovation for the Public Good

An organizational structure that makes it possible for individuals with partial or divided responsibilities, sometimes even with conflicting interests, to work toward a goal of mutual benefit is worth considering as a way in which to attack some of the major social problems of our day, especially those that do have a substantial technological content—and it is hard to think of many that do not.

In this process of consideration we must answer the following two questions: What is the organizational unit that corresponds to the corporation, and are the techniques of applied research for the public good sufficiently like those of applied research for technological progress to justify an analogous organizational or institutional structure?

Taking up the second question first, let us consider the nature of applied research in the social and behavioral sciences. In the usual manifestations of their conclusions and recommendations, social and behavioral scientists tend to be more doctrinaire than physical scientists. As a result, innovation is regarded by social scientists as an exemplification of a theory, rather than as an experiment, the results of which may help to improve or modify a theory. Two years ago, I sat in on a meeting of experts that was being held in Paris under the auspices of the Organisation for Economic Co-operation and Development (OECD). One of the speakers expounded at some length on the inapplicability of the physical sciences and technology to urban planning and similar social problems. The arguments he used, although couched in different language, were essentially the same as those that I, when I was an industrial research director, was accustomed to hearing from "practical" businessmen and plant operators, who did not believe that research could help them solve their problems. My comments that experimental research could be useful were condescendingly dismissed by the chairman as "an eloquent affirmation of the American ideal of empiricism." I plead not guilty to being an empiric, and I am still puzzled by the description of empiricism as an ideal. The point of my criticism was simply that the social scientist too often designs, and sometimes builds, a city or a social structure from first principles, with no room for, or even thought of, modification as experience in its use is acquired.

At another meeting of the OECD, a representative of the British Ministry of Housing was describing a "new town," telling how the authorities had successfully resisted pressures to build a shopping center in the American style. When asked how the inhabitants of this new town felt about this issue and about the ways in which housing, shops, pubs, and other recreational facilities had been provided for them, the speaker responded with a blank stare. It seemed never to have occurred to him that a new town could be regarded as an experiment, that the suitability to its objectives of what had been provided could be used to modify future towns. In the physical sciences and in engineering, each experiment, each pilot project, and, ultimately, each full-scale operation is carefully studied and examined to see what modifications should be made in the next.

Consider one urban problem, that of population density. There are two positions on this problem: (i) crowding people together is bad, it results in psychological tensions, and it raises problems of waste disposal, transportation, and lack of recreational areas; and (ii) people like to be near other people, especially those who are like themselves. High density of population makes it easier to provide schools, transportation, and shopping and recreational facilities, as well as optimum use of urban space in both a physical and a sociological sense.

Sometimes these two positions are stated in the following terms: (i) Man's racial inheritance makes it necessary for him to have direct contact with the land, with animals, with growing things—without occasional recourse to Nature he deteriorates; and (ii) Man is a social animal who prefers the surroundings with which his childhood and youth have made him familiar. Urban children suffer "cultural shock" when they are taken to the country. Recreation is necessary and desirable, but, to be stimulating and accessible, it should be designed for urban environments.

It is easy to see that an urban planner who belongs to the first school will design a very different city than will a planner who belongs to the second school (*5*). There probably are already in existence cities or parts of cities that embody these different philosophies. We have to be able to design experiments, or methods of analysis of what already exists, that will help us decide whether the first, second, neither, or some combination of views represents the character of human beings and human society.

Social Experimentation

Such experiments require, first, pressure to have them done; second, some ideas about what might be done; third, an organizational structure that would make it possible to tie the research to real problems; and fourth, enough money to do them properly. The last may be the least difficult to achieve. It takes little more money to do and then reflect on the results than just to do and walk off.

The design of experiments and the analysis of experimental data have almost become sciences in their own right. I do not wish to give the impression that all social scientists are ignorant of these techniques or that all physical scientists are adept. In both cases, only those who have been trained in the methods of research realize that experiments can be planned so that negative results can be as informative as positive results. The go or no-go tests of the "practical" technologist or technician waste resources and often inhibit further experimentation. Too many social experiments are one-shot affairs. At the first of the two OECD meetings that I mentioned earlier, there was expressed a dislike of experimenting with human beings. There was no recognition of the fact that every social innovation is an experiment with human beings and that it would be far better to recognize that fact and to design the experiment in such a way as to minimize the effects and make it possible to correct the errors, if not for the original group affected, at least for the subjects of the next innovation.

Even the refusal to adopt an innovation is an experiment, an experiment to determine whether what was suitable or valid 10 or 20 years ago is still valid today. I recognize that there are important moral and ethical objections to experimenting with human beings without their assent. These objections do not vanish just because one does not acknowledge that a chosen course of action is an experiment. In many problems involving the public good, the interactions between the research worker and the user of research should include also the consumer, the individual who would be affected by the proposed innovation. I know that this may sound impractical and idealistic but, in the long run I would prefer to have the "tragedy of the commons" (*6*) dealt with by the processes of democracy than by the fiats of the well-meaning.

The resistance to explicit experimentation rests not only on ethical and moral grounds, but on political grounds as well. There is the fear that the public official who approves an experimental project that fails is liable to become the scapegoat and be defeated in the next election. While there are grounds for this fear, I think that they are often exaggerated. Well-planned experiments provide for modification and self-correction as they proceed, and few are disastrously unsuccessful. One of the major needs with which we are faced in our attempts to improve the public good is the need to convince our citizenry, and their elected representatives, that change is a fact of life and that, while physical adaptation to change is taken care of by the processes of evolution, cultural change involves the conscious choice of alternatives. In the absence of infallibility, the choice among alternatives requires a certain amount of conscious experimentation.

It would be an exaggeration to say that no social science innovations are considered to be experimental, in the sense that lessons or knowledge can be drawn from their successes or failures. It sometimes seems, however, that, when social science innovations are considered experimental, it is the opponents, rather than the proponents, of an action who examine it in that way in the hope of finding fault. Consider, for example, the use of the interest rate to control the economy. The results of a rise in the interest rate are always watched very closely by those who maintain that the supply of money is not the crucial factor in inflation.

During the past few years, there has been an increasing awareness among social scientists that experimentation is an important part of the process of the application of research (*7*). However, I am concerned with problems of organization and coordination rather than with how research in the social and behavioral sciences should be carried out. To a substantial extent, the methods of applied research cannot help but be influenced by the nature of the organizations and institutions through which or within which they operate. It does seem that, in at least some areas, the techniques of applied research for the public good are sufficiently like those of applied research for technological progress to justify an analogous organizational or institutional structure. I have in mind not only "technological fixes" (*8*), but true social answers to social problems.

The Complexity of Social Problems

The design of an organizational structure is not an easy task. It is easier to point out the flaws in existing structures than to build or even propose new ones. Everyone but the most fanatical knows that our environmental and social problems are complex, both with respect to origins and to effects. Rarely is a problem the result of wanton, irresponsible action. Pollution, for example, is a by-product, not a product, of man's activities. No one deliberately sets out to make waste. What is annoying or hazardous to some people results from an action or process that benefits or is convenient for someone else. The causes of urban decay are even more complex, ranging from such broad issues as racial discrimination and migration from agricultural areas to more particular, but not lesser, issues such as inept municipal administration.

The federal government is fully aware of the complexity of environmental problems. The reorganizations of the past 2 years are manifestations of this awareness. Nevertheless, it still seems that the organizational and coordinating mechanisms within the federal government have been designed for the application of agreed-upon principles, rather than for the elucidation of the real nature of the problems and the promotion, performance, and use of research. Problems are treated for alleviation of symptoms, not of causes.

For example, it seems to be assumed that the best way to reduce that portion of air pollution which is due to automobiles is to reduce the emission from individual engines. Comparatively little attention is being paid to such alternatives as a combination of urban design and intraurban transportation, which would reduce the concentration of automobiles in cities and alleviate not only air pollution, but also congestion and a substantial proportion of urban noise. It is assumed that air pollution which comes from power plants is best eliminated by legislating the sulfur content of the fuels used by these plants. It may be that generating power at the mine site and transporting electricity long distances might be more economical when all social costs are taken into account. More support for research on reducing the cost of electric power transmission might well be worth while. It is assumed that the best way to stop pollution of rivers is to remove the contaminants at their source. This is, of course, the easiest solution to enforce by law, but it might be more efficient and more economical in some cases to combine effluents from a number of sources and then treat them all in one plant, as is done in part of the Ruhr valley (*9*).

These official policies and attitudes are a consequence not of ignorance or stupidity, but of the distribution of authority and responsibility in ways that do not meet the problems of our day. It is not helpful to denounce this state of affairs and call for an immediate and radical redistribution of authority and responsibility. The agencies of the Executive Branch and the committees of the Congress have developed over the years not merely vested interests but real competence in their respective areas. Reallocations of responsibility have not always been successful. Giving the Department of Health, Education, and Welfare (HEW) responsibility for air quality and solid waste disposal did not work out very well. In theory, this seemed a great improvement over previous fragmentation. In fact, however, HEW had great difficulty in fulfilling its responsibilities. The result of better air and better disposal of solid wastes would indeed be the improvement of human health and welfare, but the mechanisms through which these must be achieved are primarily technological, requiring competence in engineering and economics rather than in biology and medicine, the two areas of HEW's greatest competence. For HEW, the focus of interest is traditionally the individual human being, rather than the individual in the complex of society and culture that constitutes his total environment.

I have introduced HEW not as a whipping boy but as an example. Most government departments are confronted with broad interactions of technology, industry, commerce, and human welfare that result in both benefit and harm to the citizens of the country. The arbitrary assignment of responsibility to a single agency, or even the setting up of a new superagency, is not the answer to complex problems. I think that the development of an organizational and institutional structure through which we could manage our environment will come not from the attempt to treat the symptoms or manifestations of malfunction, but from an attempt to bring our intellectual resources to bear on the origins of the problems and on means of removing or alleviating undesired by-products of otherwise desirable modes of action. In many cases, of course, action cannot be postponed. We must continue to treat the symptoms of malignancy in order to survive long enough to find the remedy.

The attempt to analyze a problem in terms of the possible causes and to study solutions in terms of their potential effects is the usual procedure in the physical sciences and technology. To do otherwise is mere troubleshooting—an activity that attracts a certain type of individual whose abilities and value should certainly not be denigrated, but that is not attractive to the scholar, whose primary interest is the advancement of the frontiers of knowledge. It has been difficult to persuade scholars in both the physical and the social sciences to work on problems of the environment. Part of the reason is that these problems are usually discussed in such terms as smog, garbage disposal, and so on. Garbage by any other name may smell as bad, but problems formulated in the imprecise language of the common man are not attractive to the scholar. In order to prevent us from creating problems for tomorrow, we must do more than alleviate the problems of today. We must do research and in order to do research we must attract the interest of scholars. To solve the problem of smog, we must understand the chemical reactions that take place in the atmosphere. The problem of garbage disposal involves the solutions to problems of operations research, the economics and technology of the collection and reuse of materials, and the biochemistry of the degradation of complex compounds.

Thirty years ago, the same difficulty in attracting scholars existed in industry. Now it is rare to find a major company in which the relation of research to the operations and objectives of the company are not understood. I have been citing the methods of industry only as examples—I am not proposing that the research be done by industry. It is hard to see how research for the public good can be sponsored by any but governments. The alternatives must usually be chosen in terms of values, and often in terms of trade-offs.

Institutions for Social Innovation

The institution, therefore, should be governmental, or at least be closely affiliated with an operating governmental agency. The recital of problems and complications that I have just completed leads to an intriguing thought about the nature of such an institution. It is always tempting to draw up a grand design for a new kind of organization. In this case, I think it would be an error. I have already pointed out the complexity of our social and environmental problems and the ways in which responsibility for parts of each is divided among a multitude of agencies. If I am right in claiming that success in the application of research in industry has resulted largely from the interaction of applied research with the process of application, then it is desirable that a test of this thesis be made in a governmental institution or agency that has direct responsibility for social problems.

In many ways, the structure of a municipality or of a public authority seems to be more adaptable to experimentation in the application of research to social or environmental problems than are federal or even state governments. In the first place, there are some clear common interests and an obvious interaction among competing interests. The commercial core, the middle-class residential areas, and the ghettos of a city have different problems, but it is not hard to see that the solution to the problems of one might aggravate the problems of the others. Second, there is more immediate contact between those governing and those being governed. The politics of city government, which is often detrimental to optimum management, is potentially well suited to measuring the effects of innovation. Third, within the city council (or equivalent body) and within the administrative structure of the mayor's office, there are frequent opportunities for those whose interests or responsibilities are affected by innovations to converse with each other.

A public authority such as the New York Port Authority or the Tennessee Valley Authority is also suited to innovation, but in a different way from a municipality. It has clearly a defined area of responsibility, but usually has broad powers to make changes in contingent areas. These powers are sometimes used brutally, but it is possible to have wise and intelligent cooperation between the authority and the citizens who are affected by its activities. The freedom of many public authorities from political pressures and the large funds available to them enable them to be dramatically innovative on occasion.

A Pilot Problem-Oriented Laboratory

I have implied that one result of applied research might be a change in the planning agency's mode of operation or even in its organization, in order to use effectively the results of research. This is another reason that the first institutions for the application of research to society's problems should be small. What I am proposing is in itself an experiment. It should be regarded as a pilot plant for the design of bigger efforts. The success of one or more of a series of small experiments could even bring about the creation of political and sociological environments or institutions that would bring problems which transcend municipal or regional boundaries under centralized or coordinated planning and control, thus making possible the application of research to such problems. Even in industry the most successful research laboratories started as experiments. They did not spring full grown from the head of whatever god represents corporate management. They grew in size and developed in structure in response to the demands on them and as the organizations sponsoring the research were themselves reshaped in response to their responsibilities.

One of the most important results of a program to apply research to public problems may be the clear definition of what modifications of laws and institutions are essential to progress. In order to achieve this kind of result, the interaction among applied research, administration, and policy-making should be close and continuous. This is a relationship difficult to maintain when the applied research is done in a university or in a think tank. There is still another reason, perhaps even more important, for having the applied research an integral part of the organization that is to use the research. A significant element in the motivation of an applied research group is the realization of achievement. The kinds of research needed to provide the knowledge for problem-solving are not always those that are in vogue in academic circles. The individual engaged in research in a university looks to his peers for approval, and their acceptance of his work as significantly contributing to their knowledge is an important incentive.

The individual at work in a problem-oriented institution has a dual audience. To a considerable extent, particularly if his work is primarily basic research, he still has the peer audience of academia. But he also has the audience of those who are paying his way and who expect to benefit from his work. The feeling that what he is doing is being used is important to the research worker. Workers in industrial research laboratories who have a substantial amount of freedom of choice in their projects, as well as ample facilities, often feel uneasy and out of place if they cannot see the relevance of what they are doing to the objectives of the organization that employs them. In spite of a tendency to regard industrial research workers as mercenaries, a very high percentage of them are inspired by the commercial activities and stated objectives of their employers—there are geologists sincerely interested in finding more oil; organic chemists in developing better plastics, fibers or antibiotics; and biologists in increasing yields of crops. When a commercial objective is coupled with a problem that poses a real scientific challenge, the result is highly motivated research. When he is a member of the team, the researcher realizes that good results in his own activity will be recognized and used and will benefit him. If he is a member of a consulting firm or of a university faculty, he will not usually have that kind of feeling.

If there is any merit in the arguments I have put forth, it seems to me that one or more experiments should be undertaken to see if the principles and organizations that have proven so successful in industrial and military science and technology are useful for the problems of society. In simplest terms, this would be a problem-oriented research establishment closely allied to those who are confronted with the responsibility for solving the problem and those who are part of the problem.

As an initial step, it would be desirable to set up a study team to choose both sites and problems. The composition of the team would depend on the scope of the area to be studied. The team should probably be competent in industrial research management, engineering, political science, economics, sociology, psychology, and ecology. This team would be charged with reformulating the problems from common language into professional jargon. A team working with a common objective finds little difficulty in communication. Face-to-face discussion greatly facilitates mutual understanding. The function of the industrial research director would be to provide guidance that would permit the team to design programs applicable to more than one situation or geographical area and, in addition, to design the organization in such a way that continuity of effort over a long time could be assured. This continuity would require a balance of short-range and long-range problems, in order to give both researchers and users of research a sense of challenge and an early sense of achievement.

Conclusion

The techniques that have been developed for the application of physical science to technology have been outstandingly successful. It seems worth applying them or their analogs to both the physical and social sciences in order to benefit the public. To do this, it will be necessary to bring together politicians, administrators, and research workers in a manner that encourages their interaction and cooperation. There is no magic formula for accomplishing this. The methods that have been successful are as diverse as the corporations or mission-oriented agencies in which they have been used. The successful methods will probably be as diverse as the governments and other sociopolitical entities that make use of them. The common element is the recognition that the application of research is a complex operation, involving continuing interaction and feedback, and is not a simple, orderly process of transmitting information from one place to another.

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5. This is, of course, a very simplistic picture. To a considerable extent, however, the first point of view is held by the advocates of garden cities, such as the British "new towns," and permeates I. McHarg's *Design with Nature* (American Museum of Natural History, New York, 1969). The argument for high density of population in cities and towns is well presented by W. H. Whyte [*The Last Landscape* (Doubleday, New York, 1968)]. A comprehensive and jargon-free critique of most of the schools of urban sociology is given by L. Reissman, *The Urban Process* (Free Press, New York, 1964).
6. G. Hardin, *Science* **162**, 1243 (1968).
7. Two recent reports examine and evaluate some specific examples of applied research in the social sciences. The first [*Policy and Program Research in a University Setting, A Case Study* (National Academy of Sciences, Washington, D.C., 1971)] examines the work done for the Office of Economic Opportunity by the Institute for Research on Poverty at the University of Wisconsin. It considers not only the specific issue, but discusses, more broadly than I have in this article, the problems involved in using university-based institutes as behavioral science research resources for mission agencies. The second report [*Behavioral and Social Science Research in the Department of Defense: A Framework for Management* (National Academy of Sciences, Washington, D.C., 1971)] also treats not only the specific question, but the more general problem of how to do research. It points out: "The idea of field testing social science hypotheses is not yet widely accepted by either researchers or policy makers. Yet there is a sufficient number of successful applied social science research on a large scale, and the testing of social hypotheses, to make it impossible to deny the potential value of such work" (p. 29). Another NAS report to be published in 1972 is the "Study conference on research strategies in the behavioral and social sciences on environmental problems and policies." This conference was held under the joint auspices of the Division of Behavioral Sciences of the National Research Council and the Environmental Studies Board of the National Academy of Sciences–National Academy of Engineering.
8. A. Weinberg, *Bull. At. Sci.* **22**, 4 (1966).
9. For a more complete discussion of the complexity of environmental problems, see H. Gershinowitz, in *Environmental Quality and Safety* (Academic Press, New York, 1972), vol. 1, pp. 1–9.

NEWS AND COMMENT

National Cancer Act: Deciding on People, Policies, and Plans

During the last year and a half, scientists and politicians have been busy fighting over and bragging about the new, official U.S. commitment to the conquest of cancer. On 23 December 1971, President Nixon signed into law the National Cancer Act, which endows the National Cancer Institute (NCI) with privileged status and $1.6 billion to spend in the course of the next 5 years.

Intermingled with lavish and optimistic words of praise for this new enterprise is the often repeated caveat that biomedical research is a notoriously uncertain undertaking, that even the imprimatur of the White House and all that money cannot *guarantee* success. Be that as it may, no one, including the most sophisticated scientist, is going into this without some expectation of tangible results, and, among the public, expectations are great indeed. Consider, for example, an exchange between Representative Daniel Flood (D–Pa.), chairman of the House Committee on Appropriations' subcommittee on labor, health, education, and welfare, and Carl Baker, outgoing director of the NCI.

FLOOD: Every time the phone rings, I expect to pick it up and have you tell me that we have broken through in cancer virus [research].

BAKER: I don't think it happens as a breakthrough like that.

or again

FLOOD: What day are you going to tell us, what month and year, "Here, Hallelujah," as you have done with polio and measles?

BAKER: I don't think it is going to come that way.

To be sure, Flood's questions reflect a simplistic view of the cancer problem, but, just as surely, they represent the thinking of many members of Congress and the public. His notion that we are on the verge of a breakthrough in cancer research is, one must admit, not something he made up out of whole cloth. It is logically derived from the special pleading and hoo-ha that has attended the passage of what was billed earlier as a "cancer cure program." Cure or not, everyone is impatient for something to happen.

In this atmosphere of great expectations, the cancer effort must get off the ground—soon. In December, Nixon declared, "With the enactment of the National Cancer Act, the major components for our campaign against cancer are in place and ready to move forward." The President was a bit premature. What he had was an outline, but neither the characters nor the script for the anticancer drama. Now, however, all the key players have been cast—or will be as soon as the appointment of Frank J. Rauscher, Jr., as Baker's successor is officially announced. The script, in the form of the National Cancer Plan, which will detail the ways to spend the money, is nearly completed.

An essential feature of the new cancer act is the direct tie it creates between the NCI and the White House. The law provides for a structural reorganization that makes the director of the NCI responsible to the President—not to the Secretary of Health, Education, and Welfare (HEW) or to the head of the National Institutes of Health (NIH), as before. Both the Secretary of HEW and the NIH director have thus lost control of the NCI budget. Under the new provision, they may comment on the budget, but neither may change it by so much as a comma. It will go straight from Rauscher's desk to the President.

Another direct line to the White House has been opened by the creation of the National Cancer Panel, a triumvirate of one layman and two scientists, to oversee the entire operation of the NCI, reporting any bureaucratic quagmires to presidential advisers Ken Cole and James Cavanaugh, and, if need be, to Nixon himself. "The President," says Cavanaugh, "wants to be sure that this cancer effort does not become tangled in red tape. We plan to follow its activities fairly closely." (This being so, a number of cancer researchers have expressed fear that the program may be too carefully controlled from on high, but, as yet, it is too soon to say whether this will be the case.)

Benno C. Schmidt, who originally ad-