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A REPORT TO THE
COMMITTEE ON SCIENCE AND ASTRONAUTICS
U.S. HOUSE OF REPRESENTATIVES
BY THE
NATIONAL ACADEMY OF SCIENCES

March, 1965

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PREFACE

In December 1963, the Committee on Science and Astronautics of the United States House of Representatives concluded a formal agreement with the National Academy of Sciences. The purpose of the agreement, which evolved into the first contract ever entered into by Congress and the Academy, was the production of a comprehensive study designed to throw into bold relief some of the more serious phases of policy which Government must consider in its decisions to support or otherwise foster research in America.

This report is the embodiment of that study. It has not been an easy one to undertake, requiring as it does the careful evaluation of an extremely complex and elusive relationship—that of Government, science, technology, society, and individuals, each to the other and each to all.

In carrying out the terms of the agreement and in developing the form and substance of the report, we in the Congress are particularly indebted to Representative Emilio Q. Daddario who, as chairman of our Subcommittee on Science, Research and Development, served as the congressional agent and focal point throughout, and to Dr. George B. Kistiakowsky who, as Chairman of the Academy's Committee on Science and Public Policy, served in similar fashion on behalf of the Academy.

It is my belief that this report represents not only genuine achievement and utility in itself, but a significant milestone in Congress' methods of gathering talented, objective assistance to its use.

GEORGE P. MILLER, *Chairman,*
Committee on Science and Astronautics.

NATIONAL ACADEMY OF SCIENCES

OFFICE OF THE PRESIDENT
2101 CONSTITUTION AVENUE
WASHINGTON, D.C. 20418

MARCH 19, 1965.

Hon. GEORGE P. MILLER,
Chairman, Committee on Science and Astronautics, House of Representatives, Washington, D.C.

DEAR MR. MILLER: In March of last year, as chairman of the Committee on Science and Astronautics, you asked the National Academy of Sciences for an advisory report to the Congress on certain fundamental questions related to the support of basic scientific research by the United States Government. The task of responding to your request I assigned to our Committee on Science and Public Policy under the leadership of its chairman, George B. Kistiakowsky. It gives me great pleasure to transmit herewith the resulting report.

The report has been prepared by a panel of 15 distinguished individuals whom we especially selected for this task, eight of them members of the Committee on Science and Public Policy. A summary of their principal findings and opinions, prepared by the Committee, precedes the 15 essays of the individual panel members.

As you well know, the effort to determine the desirable level of Federal support for basic research, and its wisest allocation among fields or activities, is beset with difficulties. Each panel member has sought, after intensive discussions with the others, to clarify the factors that he himself deems essential to the task. And the Committee has sought in its summary to capture both the similarities and the differences among their views.

The Academy is indebted to those who have labored with diligence and devotion to produce this report. We hope that it will prove helpful to those in the Congress who through their diverse responsibilities bear the crucial burden of determining both the extent of Federal support of basic research and the broad character of its distribution.

Sincerely yours,

FREDERICK SEITZ,
President.

PANEL ON BASIC RESEARCH AND NATIONAL GOALS

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H. W. BODE, Bell Telephone Laboratories
HARVEY BROOKS, Harvard University
FRANK L. HORSFALL, Jr., Sloan-Kettering Institute for Cancer Research
HARRY G. JOHNSON, University of Chicago
ARTHUR KANTROWITZ, Avco-Everett Research Laboratory
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SAUNDERS MacLANE, University of Chicago
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ROGER REVELLE, Harvard University
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ALVIN M. WEINBERG, Oak Ridge National Laboratory
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JOHN VERHOOGEN, University of California, Berkeley
ALVIN M. WEINBERG, Oak Ridge National Laboratory
ROBERT E. GREEN, National Academy of Sciences, Executive Secretary

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INTRODUCTION

The papers presented in this volume were prepared in response to a request addressed to the National Academy of Sciences by the Committee on Science and Astronautics of the U.S. House of Representatives. In its request, the House committee set out two extremely broad questions of fundamental importance to the Federal Government in connection with its scientific research and development program:

I. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?

II. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

The task of preparing replies to these questions was undertaken in behalf of the Academy by its Committee on Science and Public Policy. The committee has been aware of the significance and scope of these questions because of its work on the report, *Federal Support of Basic Research in Institutions of Higher Learning,* published in 1964. In view of the complexity of even the comparatively limited area covered in that report, the committee was impressed by the difficulties presented in responding to the questions now before it. An additional difficulty was created by the comparatively short time available, since the members of the committee can give only a portion of their time to the activities of the Academy. Finally, the committee was aware that its membership is heavily biased on the side of college professors engaged in scientific research and is less competent to deal with problems of research outside institutions of higher learning. These considerations led to the appointment of an ad hoc panel of 15 members for the present task, 8 of whom are currently members of the committee. All members of the ad hoc panel are associated with representative institutions and/or professions concerned with research in the sciences.

It has been traditional for groups of this kind to develop a consensus as a basis for unanimity in the public statement of their findings addressed to the executive branch of the government. We concluded that, in view of the nature of the legislative process, this may be less desirable in a response to a request from a congressional committee. Since the issues raised by the questions of the House committee are very complex, a response on the level of a common denominator of individual opinions might not be very useful. Therefore, the membership of the ad hoc panel was deliberately selected to secure diversity of viewpoint, and, following the summary which begins this volume, 15 papers are presented, each prepared by an individual member of the panel. For each of the papers only the individual author is responsible: neither the other members of the ad hoc panel, nor the committee, nor the Academy assumes responsibility for the opinions expressed, except where explicitly stated. Even the subject of each paper was largely left to the individual choice of the author. During its meetings, however, the ad hoc panel subjected each paper to intensive discussion and frank criticism. Observing the evolution of subsequent drafts of the papers, we believe that this has been an extremely fruitful procedure and that the present document is substantially "more than the sum of its parts."

The summary following this introduction, prepared by the Committee on Science and Public Policy, analyzes the 15 individual papers in the light of the 2 questions of the House committee. However, criticism of the individual positions has been avoided. The members of the ad hoc panel generally approve this summary as an objective statement of their views. We are aware, however, that it does not do full justice to all the individual essays, especially in the balance of emphasis upon particular points and in the detailed definition of areas of agreement and disagreement. The reader can correct these inevitable shortcomings only by reading the individual essays themselves.

The 15 papers are arranged alphabetically according to the names of their authors. We suggest that the paper by Hendrik Bode, "Reflections on the Relation Between Science and Technology," be read first, however, since it provides a broad historical sketch which serves well as background for the other papers. At the end of the volume are brief résumés of statistical data on research and development activities in the United States (app. A) and on those in some other highly developed countries (app. B). These have been included to reduce repetitious citation of such data in individual papers and to bring out the uncertainties and inadequacies of statistical information.

We hope that notwithstanding its limitations this volume meets some of the needs of the House committee that led to its request to the Academy, and that it furthers better understanding of science policy problems and issues. We recognize that our contribution is only a beginning, but hope that the many points left unclear or those on which disagreement exists can be further developed by oral statements of the individuals involved to the members of the House committee, or be taken up in future studies.

SUMMARY

To summarize 15 separately written essays on issues as complex as those raised by the House committee might seem at first sight to be an insuperable task. The task is made easier, however, because each paper was discussed and criticized extensively by the entire ad hoc panel. Although no author was compelled to respond to the criticism of his paper, most of the authors did, on rethinking, modify at least some of their original views, and to this extent there emerged many elements of a common position. This is not to say that all the authors agreed, even on some of the central questions; for example, on the question of whether the Government should support basic research at an increasing rate, the mathematician, MacLane, and the geologist, Verhoogen, take somewhat different views, as do the two economists Johnson and Kaysen. Nevertheless, a common viewpoint does permeate a surprising number of the essays. The purpose of this summary is, therefore, not to repeat in abbreviated form what is said so much better in the essays. Rather, it is to identify the common threads in the many different approaches to these problems, as well as to point up the sharpest areas of disagreement. The committee is encouraged that problems as difficult as those raised by the House committee can elicit fairly congruent analyses, and even similar answers, from men of widely different backgrounds.

The papers fall into three groups. Eight of them are concerned broadly with the questions as stated by the House committee. These papers, by Bode, Brooks, Johnson, Kaysen, Kistiakowsky, MacLane, Verhoogen, and Weinberg, try to lay down general principles and to examine specific tactical questions arising in connection with the support of science They tend to have a philosophic, political-scientific, or economic flavor. Three other papers, by Kantrowitz, Teller, and Willard, are primarily focused on the relation between education and research. The remaining papers, by Blinks, Horsfall, Pfaffmann, and Revelle, depict the state of certain particular fields of science—biology, medicine, the behavioral sciences, and the earth sciences. These papers give the flavor of the substance of science by showing the intellectual challenge, the material requirements, and the relevance to our society of some specific fields of science.

The choice of whether to write on the general questions or on a narrower topic was left to each writer. As a result, not all aspects of the general questions, and certainly not all the particular fields of science, were covered equally. It was decided to leave out of the summary most of the discussion of particular fields since the essays on specific fields cover so small a part of science. On the other hand, we have tried in this summary to present a somewhat more balanced analysis of the broader questions of strategy and tactics of Government support of basic research than emerges from any of the individual papers. Some of the issues, like the relation between education and research, are touched upon in almost all the papers, whereas the matter of geographic distribution appears explicitly in only one. In the summary the imbalance in the discussions of these questions is to some extent redressed.

Both questions put by the House committee involve the issue of allocation of resources. The first question was interpreted by most of the panelists as raising the issue: How should we allocate resources between science and the other activities of our society? The second question asks: How should we allocate our resources within science? In our summary we consider each question in turn.

Part One: The Allocation of Resources between Science and Other Activities

I. *Basic Science "as a Whole" Is Not the Issue*

We first restate Question I: *What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?*

The first question as stated makes certain implicit assumptions with which not all the authors agree. Verhoogen questions whether the U.S. "position of leadership" in applications of basic science is as firm as the question implies: "The United States has, without doubt, mastered the technology of many fields; but brilliant engineering achievements are not to be seen exclusively in the United States, and our technological supremacy does not extend to all fields." And the concern over our ability to convert basic research into application effectively is the main topic of Teller's and Kantrowitz's contributions and a major theme in Bode's essay. Nevertheless, the reservations concerning American leadership are much less pronounced than the affirmations of it. Thus Kistiakowsky speaks of the beneficial interaction between "chemical research and the welfare and the position of leadership of the American Nation"; and Brooks points out that the United States enjoys a highly favorable balance of trade both in payments for technical know-how and in exports of products based on sophisticated technology. Moreover, the authors, almost without exception, concede that in most of basic research per se, the United States today stands preeminent. As Teller says, "* * * the United States enjoys an unquestionable lead in pure science." MacLane states: "Mathematics in the United States has recently been strikingly successful * * *" and Kaysen adds: "Our own contribution to the stock of basic knowledge * * * has been so great that we cannot simply act as if the total were given independently of our own actions." Though Johnson argues that scientific leadership in basic research itself may confer a kind of international leadership that is appreciated only by a small elite of scientific sophisticates, Bode articulates the views of several others when he replies that science and technology, as intellectual fields, are important components in the struggle of cultures in which our country is engaged.

This colloquy, in touching upon the connection between basic research and its applications, brings out one of the most pervasive and essential points in the whole analysis. The question, as put, implies that basic research "as a whole" is a proper focus either for budgetary decision or for political action. With this several essayists sharply disagree. As Weinberg says, to bring order to our thinking about public support of research, it is first necessary to separate basic research done to support a practical mission from research done to further science. Brooks and Kistiakowsky (as well as Kaysen and Verhoogen) also find that dealing with basic research "as a whole" is impractical. They emphasize, however, the unpredictability of practical uses of basic research and therefore do not stress as much as does Weinberg the distinction between mission-oriented and nonoriented research. To them the distinction based upon who does the research (university, Government laboratory, or industry) appears particularly relevant.

Nobody on the ad hoc panel challenges the proposition that the purposes of Government, as opposed to the techniques of Government, are nonscientific. Thus, the question to which question I naturally leads: Why should our society support basic science at all, and the corollary question: how much basic science should we support? must be answered in terms that generally lie outside science. Brooks identifies four goals of society to which basic science contributes and which justify its support by society. Basic science, per se, contributes to culture; it contributes to our social well-being, including national defense and public health; to our economic well-being; and it is an essential element of the education not only of scientists but also of the population as a whole. In deciding how much science the society needs, one must decide how the support of science bears on these other, politically defined, goals of the society.

With these goals, and the relevance of science to them, the essayists, except for Johnson who expresses serious doubts, are in good agreement. In particular, most of the essayists, especially Willard, stress the importance of basic scientific activity in maintaining our system of scientific education, although Teller and Kantrowitz, and Bode with less fervor, insist that, though basic science is necessary, it has distorted the university's perspective toward applied science. Several essayists conclude that this is not the case at present, and that men trained in basic research subsequently play key roles in applied research and other practical activities (see especially Brooks). Kistiakowsky notes that "education and research in basic science form the best base from which young scientists can develop their skills in applied fields." This disagreement with Kantrowitz and Teller perhaps can be attributed to the much closer and older relations between industry and universities in chemistry than in physics. As Brooks notes, the situation in physics will probably tend in the future to become more like that in chemistry.

A major divergence could lie between the viewpoints that basic science is a sort of long-range investment or social overhead, supported primarily because it will eventually lead to applied benefits, and that basic science is a part of culture, as is music, or art, or literature. None of the panelists holds the one view to the exclusion of the other. The difficulty in tying basic science too strictly to applied missions is summarized by many of the panelists: Basic research is unpredictable. Thus no man was bright enough to know that Roentgen's experiments with cathode rays would lead to the discovery of X-rays, which in turn would lead to vastly important advances in medicine.

On the other hand, granting that basic research is part of culture, why should the society single out this branch of culture for particularly favored treatment? As Johnson says, "* * * insistence on the obligation of society to support the pursuit of scientific knowledge for its own sake differs little from the historically earlier insistence on the obligation of society to support the pursuit of religious truth, an obligation recompensed by a similarly unspecified and problematical payoff in the distant future." Johnson recognizes, however, as he states later in his paper, that "* * * if the public is convinced that a scientific culture is desirable, it is perfectly appropriate for the taxpayer's money to be used to support scientists and scientific research." And Verhoogen, supported by others, adds "* * * human beings * * * want to know," which is to say: Science as culture is in itself a valid goal of the society. Moreover, as Brooks puts it, science is a publicly verifiable enterprise, and therefore its claim to public support can easily be validated.

But the argument for public support of basic science because it is a distinctive element of our culture is conceded by most of the panelists to be less persuasive than is the argument based on useful application of basic science. As Brooks says, "the basic difficulty with the cultural motivation for Federal support of basic research is that it does not provide any basis for quantifying the amount of support required." On the other hand, basic research viewed as an overhead necessary for the accomplishment of politically defined goals of the society, such as better defense, or better transportation, or health and longevity, though still difficult to quantify, is at least related to goals of society whose importance has been subjected to prior political judgment.

Which brings us again to a central point on which there is general agreement: that basic research "as a whole" is a misleading notion. It does not help our scientific policy-makers to view the allocation problem as one of basic research as a whole versus other activities of Government. On this crucial point at least three of the panelists, Brooks, Kistiakowsky, and Weinberg, agree explicity. Thus Brooks says: "The basic thesis of this paper adds up to the conclusion that the concept of a total science budget, which is implied by the questions asked by the House Committee, is probably not a very meaningful or significant one. Only in the restricted area of academic basic research does the concept of a Government-wide 'science budget' make a certain amount of sense * * *. The rest of the 'science budget' ought to be considered in a different context, in which the value of research and development is judged in competition with other alternative means of achieving the same objectives." Kistiakowsky essentially agrees with this view, although he divides basic research somewhat differently. Weinberg puts it thus: "The expense of science as a means to achieve a nonscientific end should logically be assessed against the budget for achievement of that end, not against some mysterious budget labeled 'Science as a Whole' * * *. The remaining basic sciences * * * would then be properly included in a budget which I call the 'Intrinsic Basic Science Budget.' This activity of our society * * * should properly be balanced against other activities of the society—for example, education and foreign aid * * * the choice between intrinsic basic science as a whole and other, nonscientific, activities is the primary relevant political decision."

II. *The Government and the Market Place as Supporters of Research*

From the conclusion that basic research, either as culture or as a long-range investment for the achievement of society's other goals, is desirable, to the conclusion that Government must therefore provide large-scale support for basic research is a step that requires argument. Johnson's paper makes the necessity of such argument explicit: He points out that there are mechanisms in our society and economy that would provide financial support for basic research subdivisions, so that the question is not one of "all or nothing" but whether privately financed research would be adequate. As Johnson puts it, "In order to establish a case for Government support, it must be shown that basic research yields a social return over its cost that exceeds the return on alternative types of investment of resources. Alternatively, it must be shown that the amount of basic research that would be carried on in the absence of Government support would be less than what would be economically optimal." He agrees with Kaysen, however, that the market may not provide enough or the right balance of support. Therefore the society cannot rely upon the market completely and must supplement it, especially since much of science is a byproduct of higher education, which is certainly of immediate public concern. Hence the Federal funds become involved but the question remains whether the present commitment is too little or too much. He makes a case for more reliance on support of basic research by sources other than the Federal Government. Johnson also argues that since basic research done by one country is available to all, there are limits to how far the United States can profitably go in attempting to maintain leadership across the board in basic research.

All the other general papers endorse the desirability of large-scale Federal support of basic research, and explicitly or implicitly reject the view that the private marketplace should be utilized more and Government sources less for seeking support for basic research. Thus Bode responds to Johnson's suggestion that foreign basic research can be transplanted by tracing the history of technology. In the early days, technology flourished with relatively little fertilization from basic science. Leadership in science was usually uncorrelated with leadership in technology; but as technology, especially military technology, has become more sophisticated, its reliance on basic science has grown. Today, the connection between basic science and technology is so close that to Bode it is unthinkable to maintain leadership in technology without maintaining an indigenous supporting basic science. It takes trained people of highest order to apply modern science to our sophisticated technology. Such people, who must be trained in our own, indigenous, educational and research system, will be available only if we have vigorous basic research of our own.

Kaysen responds to "But why the government?" by pointing out that, just because its contribution to our nonscientific goals is so unpredictable, basic research is a proper concern of the one element of our society responsible for the general welfare—the Federal Government. The marketplace always underinvests in social capital or social overhead. The economic and social benefits of basic research cannot be wholly recaptured by the private institution that finances it, but only by society as a whole. Hence the Government cannot rely either on the marketplace or on institutions with regional or specialized interests to support the volume of basic research that would benefit the economy as a whole. Moreover, some of the fruits of basic research, for example, those related to "military capability, fall directly within the sphere of Federal responsibility, and only the Federal Government can and will pay for them. This applies both to military requirements for applied research and development, and to the insurance value of the scientific reserve corps." This general view is shared by Brooks.

And, finally, Weinberg argues that much of the basic research supported by the Government is justified by its direct relevance to specific, politically defined goals of Government. For example, once the political decision is made by the Government, say, to desalt the sea economically, then the Government must do whatever is necessary in the judgment of those responsible for developing techniques of desalination to achieve it. If, for getting on with the job of desalting the sea, basic research on sea water seems more important than building another pilot plant, then the Government must see to it that basic research on sea water is done. Insofar as basic research is done to accomplish specific applied missions of the Government, such basic research is obviously the job of the Government. This kind of basic research constitutes a substantial part of the basic research conducted by the Government. It is only the other part, which is not so obviously relevant, about which there can be serious argument as to its relevance to the goals of Government.

III. *Mechanisms for Allocating Support*

We consider now the mechanisms for determining how much basic research Government ought to support. Since, as the previous discussion stresses, basic research "as a whole" is not a very useful concept, different mechanisms and different justifications are appropriate for the differently motivated segments of basic research. The entire mechanism for allocating support (and by implication, the total to be supported) turns out to be a collection of separate mechanisms (and separate subtotals).

The precise partitioning of all basic research into components is, of course, largely arbitrary. Basic research can be classified in terms of its motivation—as culture, as an adjunct of education, as a means to accomplish nonscientific goals of the society; of its sources of support, whether mission-oriented agency or science-oriented agency; of its performers, whether university, Government laboratory, or private industry; or of its character—whether "little science" or "big science." Any one of these classifications, if applied consistently, could cover all basic science, but none is wholly satisfactory; hence, the different classification schemes crisscross each other and are somewhat incongruent. For some purposes, one classification scheme is more convenient than another. In this discussion we shall use elements of all the schemes proposed in individual essays. Though this may cloud some of the underlying philosophic issues, it accords more nearly to the actual situation than does a strictly logical classification, and as Brooks puts it, in some cases leads more naturally to a basis for allocating resources to science.

For the purpose of answering the first question, i.e., how much should be allocated to basic research as a whole rather than to other activities of the society, the classification suggested by Brooks and Weinberg is germane: the larger part of basic research is tied to specific, nonscientific missions of agencies; a much smaller part is not directly relevant to missions. Let us consider first the larger part—the basic research done by mission-oriented agencies to accomplish their politically defined, nonscientific, missions.

As we have already said, such basic research is supported by the agency because the agency believes that basic research is a better place to allocate its resources than is some alternative, like procurement, transportation, or communications. This basic research is an overhead expense, since it is performed primarily not for the sake of the basic research but rather for the sake of the agency's mission. But how does one decide what fraction of the agency's resources should be allocated for basic research to accomplish even the most specific mission? Johnson argues that our understanding of cost/effectiveness of basic research is too insecure to allow even the mission-oriented agencies to decide what a reasonable allocation to mission-oriented basic research should be. Or, as Kaysen puts it, "It is in the very nature of an overhead that a nice calculation of the 'right' amount to expend on it is difficult." Difficult, but not impossible, suggest Brooks and Weinberg. Every business, in every part of its operation, makes the same sort of overhead calculation as goes into an agency's estimate of how much basic research it needs to accomplish its mission. Weinberg suggests that the amount of such support should be geared to an estimate of just how much the agency's applied mission derives from past basic research—perhaps 10 percent for agencies whose missions depend on scientific knowledge heavily, perhaps less for agencies that need science less. Brooks suggests that "10 to 15 percent of the applied effort might be a good rule of thumb for the basic research effort." These judgments of the percentage going to basic research, would be decentralized in the sense that they would be left primarily to the agency, but they would be reviewed, as is any overhead expense of an organization, by other interested parties in Government, like Congress, the Bureau of the Budget, and the Office of Science and Technology.

The basic research of an agency is thus related to the applied or developmental effort of the agency: The main problem, as indicated above, is to determine what fraction of the overall agency budget should go into basic research. But evidently, as pointed out by many of the panelists, and especially Brooks and Verhoogen, this method of supporting basic research as an overhead on applied missions, logical though it may be, also causes trouble. Basic research closely tied to an agency's nonscientific goals suffers from the vagaries of agency budgeting and agency management. If it is to be effective, basic research cannot tolerate such fluctuations, whether they are imposed by fluctuating budgets or by fluctuating opinions within an agency as to the relevance of particular basic research to the agency's mission. Mechanisms are needed both to smooth out the fluctuations in mission-supported basic research and to enlighten agency managements about basic research.

Here the National Science Foundation is viewed as playing a decisive role. The National Science Foundation is the sole agency of Government whose purpose is support of science across the board and without regard for immediate practical gains. If there is good basic science ready to be done but which does not as yet command support from some mission-oriented agency, then the National Science Foundation must be equipped to step in, if it chooses, to pick up the tab. Thus the National Science Foundation is viewed by Weinberg as being responsible for what he calls "Intrinsic Basic Science," the motives for which are relatively remote from politically defined missions. Since this is a social overhead whose connection with specific applied objectives of the society is distant and undefined, it would seem, as Kaysen stresses, that allocation of resources to this activity would be even more difficult than the allocation to mission-related basic research.

And, indeed, Weinberg insists that just this decision—how much should go for "Intrinsic Basic Science"—is the primary political decision that faces Congress. Moreover, he visualizes this decision as coalescing more and more with the decision as to the budget for the National Science Foundation if, as seems likely, more and more of this kind of science gravitates toward the National Science Foundation.

In the analyses of Brooks and Kistiakowsky, the National Science Foundation is also seen assuming the role of a "balance wheel" to soften the impact of variable research policies of mission-oriented agencies on "academic basic research" or "little science." Looking at the problem more from the point of view of the "performers," however, they realize that most of this type of research, although not immediately related to the practical missions, is nonetheless supported now by those mission-oriented agencies that choose to interpret their research tasks broadly.

As an illustration we may cite the finding, based on a recent survey by the chemistry panel of the Committee on Science and Public Policy, that the National Science Foundation contributes only 23 percent of Federal funds to support basic research in university chemistry departments. And yet this is typical "little science." In fact, the same individual "performers," that is, chief investigators, receive funds from the National Science Foundation and from mission-oriented agencies for research projects that are not different in kind and frequently overlap closely. This is related to the unpredictability of basic research (see Kistiakowsky): What to one may seem to be "science for science's sake," to another may have germs of exciting but as yet uncertain practical applications.

If this view is appropriate, the formal separation of "Academic Basic Research" into "intrinsic" and "not so intrinsic" would be very difficult from the points of view (far from uniform) of both the Federal agencies and the performers. Hence the recommendations of Brooks and Kistiakowsky that other agencies continue supporting academic basic research without a reappraisal of what is relevant to their missions (implied in Weinberg's arguments), but that the National Science Foundation grow into the role of the "balance wheel," assuring steady progress of such research.

Whichever breakdown turns out to be most useful, are there criteria for judging how much should go for this kind of science; i.e., how much, eventually, should go to the National Science Foundation or to any other agencies carrying responsibility for "Academic Basic Research"? The panelists generally take for granted that the present situation, which has led to our position of leadership, should be used as a baseline. Several of the panelists argue that the amount should increase on two general grounds: First, as Kistiakowsky and Bode suggest, our society is becoming increasingly scientific. As science helps to improve our material or personal well-being, our appetite for more improvements grows. Since the advanced technologies on which these improvements depend make ever greater demands on basic science, this trend will be reflected in an increasingly scientific culture and therefore an increasing effort in basic science. Moreover, as the wealth of our industrialized society increases, we can afford to put a larger share of our wealth into science that furthers the more sophisticated needs of the society. Second, basic science is inextricably related to the education of scientists, who are so essential to the modern industrial society. Our society is committed to the idea of providing its citizens with adequate education. Insofar as academic scientific research is connected with scientific education, the growth of our student body implies a growth of science. This is emphasized by Kistiakowsky, Willard, and Verhoogen. As Brooks says, one great advantage of tying support of science to education is that educationally justified science is the easiest to quantify: One may look mainly at the projections of population and guess how many more science students will be entering the universities some years hence, and make adequate preparations for this.

But a strict separation into mission-oriented basic research and intrinsic basic science hardly provides unequivocal clues as to how much science, particularly of the latter sort, we need. This separation is useful primarily because it suggests where in the Government the problem should be looked at. Mission-oriented basic research is the business of most agencies, whereas intrinsic basic research is the business of the National Science Foundation or of other agencies which, as Weinberg puts it, have become, in part, "little National Science Foundations." To find clues as to how much basic science we ought to support, as opposed to who should support it, we therefore return to the classification schemes used by Kistiakowsky, Brooks, and Kaysen, which divide science into "little science" and "big science." The way would be clear indeed if all "little science" and "big science" were congruent with all "intrinsic basic science." This is not the case. The professor and his small group of students, the typical performers of "little science," are often supported by a mission-oriented agency because it judges the professor's basic research to be fairly relevant to the needs of the agency. The totality of "academic science," or "little science," is very much larger than the National Science Foundation research budget.

"Little science" as defined in several essays is largely academic science. It is highly individualistic, and the performance varies greatly among the fewer than 100,000 people who do it. As evidenced by the agency records of rejected applications for funds (see Willard), far from all these people receive Federal support for research. Kistiakowsky's estimate is that in 1963 the total cost to the Government of "little science," including facilities and fellowships, was approximately $600 million. The opinions of panelists as to the desirable level of support vary somewhat. Verhoogen believes that every qualified scientist should be provided with adequate support, provided his activities are within the fiscal range of "little science" (e.g., $20,000 or less per annum) and provided his research is subjected always to the scrutiny of his peers. Brooks and Kistiakowsky (and less explicitly Kaysen) take the present situation as a satisfactory starting point, and the first two argue that a 15 percent annual increase will meet national needs.

Brooks's argument, which is the most explicit, is based on a forecast of the population of graduate students and faculty, and the 15 percent annual increase is suggested as minimal rather than as a necessarily adequate level to take care of expanding educational needs. Finally, MacLane, directing his attention to theoretical sciences, holds that every potentially original scientist should be provided with adequate support, while by implication he holds that the growth of the research budget should depend in some fashion on the growth in the number of outstanding scientists. Verhoogen's point of view calls for larger growth because: "in other fields (other than mathematics) we still very much need to assemble the verifiable facts on which new ideas may grow."

We now turn to "big science," some of which is "academic science" and some clearly mission-oriented (e.g., the scientific satellites). This science centers around research equipment, some of which is so costly that it in itself represents a significant element in the total national budget. As Kistiakowsky puts it: " 'Big science' is fiscally open-ended because the commitment of *scientific personnel per project* is rising comparatively slowly and the costs are concentrated in the engineering effort. * * *" Hence, the principle of supporting every good man in "little science" is not very useful for "big science." It is obviously impossible to provide every high-energy physicist with his own accelerator. As stressed by Kaysen, Kistiakowsky, and Brooks, the decentralized methods of allocation that characterize the panel system under which "little science" is governed must for "big science" be replaced by much more centralized planning and deliberation. *Each* "big science" project obviously demands special judgment and action and, since the size of each such expenditure is so great, the decisions will have to be made at the highest levels of Government with the strongest interplay between the political and scientific communities; in the words of Brooks, they are strategic decisions. Thus, although a total budget for "little science" can be arrived at a priori by adopting some such principle as: Support every *good* man (especially if he contributes to the educational process), or, use the present generally satisfactory situation as a basis for reasonable expansion, no a priori judgment can be made for "big science." Each instance of "big science" must be examined by itself, and must find its place not only as part of the science budget but also as part of the entire national budget.

IV. *The Self-Equilibration of Scientific Growth*

Support of "little science" at a level that assures every qualified scientist of adequate support may run the danger pinpointed by Johnson, who says, "Ultimately it (such a policy) relies on the self-equilibrating processes of the intellectual market in ideas and the commercial market in scientifically trained labor to prevent serious misallocations * * * the approach depends on a particular assumption * * *: that there is a limited and fairly readily identifiable group in the population that is capable of acceptable scientific performance, and a sharp difference in ability between this group and the rest. This assumption does not make economic sense in any long-run perspective: One would expect the supply of potential scientists, like the supply of any other kind of skilled labor, to vary in response to the income and career opportunities offered."

Most of the other panelists, insofar as they touch on this question, don't agree with Johnson. Their view is that the number of people interested and qualified in science is limited, and, because science is so demanding, will always remain limited. Moreover, the "self-equilibrating process of the intellectual market" is generally thought to work very well, especially in Kistiakowsky's view. The panel system, the internal criticism that characterizes the scientific community, the institutional standards established by the universites, at which so much of "little science" is performed—all of these keep basic science honest, keep it demanding, and will always keep it relatively small.

One of the panelists, MacLane, devotes his paper primarily to the question of standards in science; he is concerned that the growth of science budgets should not be so explosive as to erode these standards. He holds that the fruitfulness of science depends vitally upon the presence of relatively few top-quality scientists. Hence he argues that a first allocation of resources should be to support top-quality scientists, whatever their choice of subject or field. This might yield a system of allocations with multiple criteria: some basic research supported according to its relevance to the missions of various agencies; other basic research supported because scientists of proven excellence judge it worthy of pursuit.

Brooks, in a sense, imposes a similarly stringent set of qualifications on those eligible for support simply because they are really good. He estimates the number of truly outstanding "little scientists" as being only around 5 percent of all the active basic research scientists, and that their support is justified purely on cultural grounds. Brooks urges support for the rest of "little science," but not solely on cultural grounds or on the grounds of supporting good people; rather, it is on the basis that what the remaining "little scientists" do is necessary for our expanding educational system, and that it is germane to applied mission of Government.

Kaysen goes further along this line and recommends, in much the same spirit as Weinberg, that the support of basic science be defined as an overhead on total expenditures for applied research and development, with the proportion of the total set initially at its historic level of 9 percent.

V. *We Must Improve the Connection between Basic and Applied Science*

Since what emerges from the essays is a preponderant opinion that the primary justification for Government support of basic research lies, aside from education, in the expectation of payoff, we must examine more carefully the efficiency with which our Nation has been able to convert successes in basic research to practical advantage. This is the substance of the discussion by the physicists, Teller and Kantrowitz, and looms large in Bode's thinking. Teller puts the problem succinctly: "Most of our Federal expenditure is used to support applied science and the engineering developments based upon applied science. At the same time, most of our educational effort on the relevant graduate level goes into the support of pure science. As a result, the most massive expenditures of our Government suffer from inadequate technical leadership." Teller and Kantrowitz argue that the strong encouragement of basic research in the universities has created an environment that is uncongenial to applied research—so uncongenial that even universities that were organized to pursue applied science and engineering now turn out many graduates who have no taste for anything but pure science.

But many of the panelists believe the universities do play a notable role in maintaining our strength in applied research. For, as Brooks suggests, there is a steady flow of people trained in university-type research who go into applied science, "which has been one of the characteristic features of American science that has contributed to its vitality" (see also Kistiakowsky). This indeed is one of the important ways in which the results of basic science are converted into applied payoffs. And neither Teller nor Kantrowitz nor Bode wishes to disturb our position of leadership in basic research, established largely because the government has supported basic research at the universities so steadily. Rather, the former two suggest a new educational pattern for applied science in which the citadels of basic research, the universities, and of applied research, the industrial and Government laboratories, form joint entities devoted to graduate education in the applied sciences. The degree would be conferred and the academic standards would be maintained by the universities; the graduate thesis and much of the instruction would be the responsibility of the cooperating laboratory. Many such arrangements are springing up in the United States. However, since in many cases the laboratory is an agent of the Government, these arrangements often are hampered because of Government regulations concerning the propriety of using Government facilities for educational purposes. Explicit sanctioning of such arrangements by the Government is urged by both Teller and Kantrowitz.

Part Two: Allocation of Resources within Science

The second question, What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support, raises the question of allocations within science. It thereby involves possibly fewer elements of public, as opposed to scientific, policy than does the first question. In dealing with this question, the panelists tended to broaden its scope to include not merely the allocation among fields of science but also allocations among institutions engaged in science.

We have already examined various subdivisions of science—into mission-oriented and non-mission-oriented; into "big" and "little"; into "basic" and "applied"; into science at universities and science outside of universities—and we have discussed how the panelists have used various subdivisions, as they found appropriate, to discuss the total budget for all basic research. For making allocations within science, two subdivisions seem particularly appropriate: By field of science, and by institution; allocations within the first subdivision may be called "scientific choice," within the second "institutional choice." These choices are related to considerations of the sources of support and the performers.

I. *The Criteria for Scientific Choices*

The problem of "scientific choice," that is, deciding how to allocate funds to different fields of science within a total science budget, has been debated publicly since 1958. As a means of clarifying the issues in this debate, several committees of the Academy and some groups directly sponsored by Federal agencies have prepared reports dealing wih the opportunities and requirements of specialized fields of modern science. These include reports on oceanography, atmospheric sciences, and high-energy physics. Several major efforts to cover other areas of science have been undertaken since 1962 by groups of experts in cooperation with the Committee on Science and Public Policy and sponsored mainly by the National Science Foundation. We refer the reader to one such effort—a report, *Ground-Based Astronomy: A Ten-Year Program* (National Academy of Sciences, 1964), as an example of what can be achieved by a highly competent group (working on a part-time basis, of course) in approximately 18 months. Other such reports, on the academic uses of computers, on physics, chemistry, and the plant sciences, are in various stages of completion and are expected to be available before the end of 1965.

These reports will still leave many major scientific areas unexamined, and thus will not provide sufficient basis for the formulation of a balanced answer to the second question of the House committee. As an expedient to bridge the gap, four members of the ad hoc panel prepared papers on scientific areas in which they have special competence. Of these four essays, those by Blinks and Horsfall deal with essentially the same scientific area—life and biomedical sciences—but from quite different viewpoints. The essay by Blinks emphasizes the unity and interdependence of life sciences and brings out the great gains in our knowledge of life processes that can accrue from biological research. Horsfall's article describes the impressive breadth of scientific fields that now have relevance to health problems, and establishes ties between basic and applied research in these fields. Revelle's essay traces the close connection between research in earth sciences and future progress and conservation in many areas of the civilian economy. Finally, Pfaffmann's essay stresses the point that many areas of behavioral sciences have advanced to a stage where objective scientific research is feasible and is rewarding from the point of view of social benefits; also that such research, no less than research in natural sciences, requires major investment of financial resources.

Since these essays do not pretend to be full committee reports, and since they do not begin to cover all fields of science, the panelists felt they did not have information on which to base recommendations for allocation of resources among fields. Instead, most of the panelists who spoke of this problem tried to lay down principles for making the judgments on allocation among fields that the House committee asked for. The underlying strategy suggested by many of the panelists, notably Kistiakowsky, Kaysen, and Brooks, was to separate "big science" from "little science," and to use different criteria of choice for them. Briefly, these panelists recommended that allocations within "little science," or, almost synonymously, "academic basic science," should be made by the free play of the scientific marketplace of ideas. The prior assumption, already discussed in Part 1, is that, starting with the present situation, which has given us leadership, every really good man, especially if he helps the educational process, should be supported. No one knows as well as he does what is a fruitful or useful direction for his basic research; his work is continuously scrutinized and monitored by his scientific colleagues; and hence he should be allowed to decide what facet of science to pursue. The total allocation within "little science," broadly, is the sum of innumerable individual judgments by individual scientists. Such a self-equilibrating system of allocation is almost the only one that can ensure continued long-term viability for our preciously individualistic "little science."

Kaysen sharpens this strategy: He also accepts the play of the scientific marketplace within fields of basic science—e.g., in physics or in chemistry—but he pleads for a more deliberate comparison of fields by a mechanism that takes into account the total number of active researchers and the number of new Ph. D.'s produced in a field to guide allocations to that field.

The criteria for choice in "big science" must be very different, partly because of the open-endedness of "big science," already mentioned, and partly because any single decision may affect a large sector of science. Thus, whether a 200-Gev accelerator is built or is not built is a matter that will profoundly affect the long-range future of American high-energy physics. Three of the panelists consider in some detail the problem of allocation of resources within "big science." Kaysen recommends that the proposed projects be scrutinized as to their competitive merits by a group composed of the representatives of the funding agencies and of "performers" in various sciences. The evaluation process must involve the fusion of the elements of technical "ripeness" or urgency of particular projects, and political considerations. Kaysen suggests further an ingenious "tax" on "little science" to finance a part of the costs of "big science." "This cost-sharing arrangement would appear as another useful administrative control device, directed toward making those representatives of any (scientific) field not themselves too directly concerned with using large facilities sensitive to their costs in terms of their own interests." Brooks classifies the entire research funds into three categories: (1) the capital costs of "big science" facilities; (2) the operating costs of such facilities needed to make them available to the scientific community; (3) the strictly scientific costs of research involving the facilities. This last item he treats as part of "little science" and segregates it from (1) and (2). Item (2) has occasionally been insufficiently allowed for in the past, with serious effects for progress of science, and

Brooks urges its careful consideration when making decisions regarding (1). "The decisions regarding allocations under (1) are the only decisions regarding allocations between fields of science that should be made at the highest levels of Government * * *. They are the basic investment decisions of the Federal Government * * * that determine the scientific priorities for many years ahead * * * in which the price of error is highest." Brooks goes on to say that he agrees with Kistiakowsky's criteria for ordering the priorities of "big science" projects. Kistiakowsky starts from Weinberg's criteria (published in *Minerva,* Winter 1963) but revises them for "big science." The following factors emerge as most important in making the decisions that Brooks calls strategic: (a) commitment of qualified scientists to a project; (b) the relevance of the project to adjacent and significant branches of science; (c) the potential impact of the project on practical applications; (d) its impact on national prestige and international influence; (e) the broad cultural impact of the proposed undertaking.

All the panelists who deal explicitly with the subject emphasize that decisions regarding allocations within "big science" must be centralized at a high level in Government, and that decisions regarding allocations within "little science" must be decentralized.

II. *The Criteria for Institutional Choices*

But, Kistiakowsky insists, even in "little science" the scientific choices cannot really be separated from institutional choices. Where the "little science" is done, whether in Government laboratories, or universities, or industrial laboratories, must affect, if nothing else, the degree of permissiveness that the agency can allow in the support of the research.

Kistiakowsky and several other panelists argue that general, non-mission-oriented, basic research in the universities, above all, needs strengthening even at the expense of such basic research in other institutions. Perhaps Willard states the case most explicity: First, that because of our growing population, education in general and education in science in particular must increase. The need for more education is inexorably growing, and must take precedence over other needs. Second, that the universities have proved in the past that they provide the climate (the "ecology" as Brooks puts it) most conducive to distinguished achievement in basic research. Thus, the Government gets its best money's worth for a dollar spent on undirected basic research at the university and, if institutional choices are to be made in disbursement of funds for such basic research, the university should have first claim.

The nonuniversity members of the panel do not choose to respond precisely to this challenge. Kistiakowsky strongly supports relevant basic research in the Government and industrial laboratories—all created for practical purposes—while rejecting the substitution of general research for a valid applied mission. Verhoogen, who makes a strong case for basic research in the universities, argues for continued vigorous basic research in the Federal laboratories on three grounds: First, because "their research (e.g., meteorology and weather prediction) is commonly of a kind and scope that cannot be carried out in universities. That is, it is "big science," a point on which no panelist disagrees. Second, competition in science is good, and Government agencies may set standards of excellence in research that private institutions should equal or surpass. Finally, it is difficult * * * to ensure that science will move forward with the necessary vigor on all fronts * * *. By a judicious choice of its own research program, the Federal Government can to some extent correct the imbalance."

The seeming divergence between Kistiakowsky's view and Verhoogen's is not very sharp. Both agree that mission-related basic research is necessary in the Government and industrial laboratory; the point at issue is really where one draws the line between mission-related basic research and non-mission-related basic research. Kistiakowsky would possibly draw the line more sharply than some of the other panelists, and would tend to keep out of, or at least not expand in, the mission-oriented laboratories, some parts of basic research that are now pursued in such institutions. Brooks, in much the same vein, suggests that the amount and kind of basic research in Federal laboratories should be primarily the decision of the laboratory management, subject to the constraint that the total budget of the laboratory be governed by the importance of its practical mission and its long-term success in accomplishing it.

III. *Geographic Distribution of Research*

The matter of institutional choice inevitably leads to consideration of geographic distribution of research support by the Government. If one supports only excellence, and if excellence exists, as MacLane implies, at only a limited number of established centers ("One cannot have more centers than the population of scientists allows."), how does the country redress imbalances in its economic and cultural growth that are connected with geographic distribution of scientific activity? This is a question asked explicitly only by Johnson; yet it is obliquely implied in several other papers. Johnson urges a deliberate policy of locating scientific research in the backward areas of the country to encourage their industrial development: "So long as public funds are allocated to the support of basic research, the geographic allocation of the funds should take account of the social effects of their expenditure."

That this issue is not discussed in the other general essays probably results from the fact that the Committee on Science and Public Policy had already taken a stand on the matter. In its 1964 report, *Federal Support of Basic Research in Institutions of Higher Learning*, the Committee on Science and Public Policy recommended special Federal action to assist selected institutions in attaining higher levels of excellence, emphasizing at the same time the great difficulty of making the right choices.

To some extent, the project system of granting Federal support to science may have slowed down a process of dispersion of scientific resources, which might not have been slowed down under different systems of support. Brooks recommends a gradual transition to a situation in which about 25 percent of the costs of "little science" goes to supporting people, 25 percent is institutional support, and about 50 percent is project support.

The problem of geographic distributions of Federal funds for research is taking on increasing importance in the eyes of Congress. Whether it will tend to go away of its own accord, or how much the process of diffusion of scientific excellence can be accelerated without sacrificing the excellence itself, are questions on which additional thought is needed beyond what was given in the report, *Federal Support of Basic Research in Institutions of Higher Learning.*

IV. *The Age of Biology and the Crisis in Physical Sciences*

Though the panelists could not fully address themselves to the question of scientific choice (inasmuch as the necessary data were not available), nevertheless two themes in this connection are discernible. One impression created by several panelists is that the next decade ought to be the age of biological science. As Weinberg puts it, "The National Institutes of Health seems * * * to be the Government agency, the achievement of whose mission is most directly and obviously dependent on a great push in our understanding of an underlying basic science (biology), and whose mission will continue to enjoy greatly expanding public support." Or as Blinks says, "Many physical scientists feel that support should be even greater for biology (than the 15 percent per annum suggested for 'little science' as a whole) as it enters an era of unprecedented fruitfulness."

On the other hand, Weinberg sees the physical sciences confronted with a deep financial crisis: "* * * the necessity of expanding basic physical science research in order to further the missions of * * * the Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration is not * * * obvious * * *." Yet, since orderly expansion of these sciences is a necessary element in the general growth of the sciences—in education if nowhere else—"* * * basic research in the physical sciences is faced with a crisis. Most of its support has come from the mission-oriented agencies, but these agencies (faced with stationary budgets) will probably not expand their support of basic research as fast as our capacity to do basic research (in the physical sciences) expands."

Part Three: Government Decisions and Actions

What specific actions do the panelists call for from the Government agencies and from Congress? Two kinds of actions seem to be called for by various panelists: In one group are additional staff studies and statistical analyses; in the other are broad and, in some instances, crucial decisions that affect our whole governmental organization for science. These latter actions in a sense constitute a sort of operational answer to the questions put by the House committee.

I. *Statistics on Research*

Statistics on research and development in the Federal Government are complex, and sometimes misleading, not because the dedicated statisticians and analysts who amass these figures are incompetent, but rather because the situation is inherently so complicated. As Brooks says, "A recent report of the Organization for Economic Cooperation and Development has remarked that most countries have better statistics on poultry production than they do on the activities of their scientists and engineers. To some extent this is inevitable since the product of scientific activity is an elusive entity which defies measurement." Brooks then goes on to analyze some of the difficulties: It is very hard to decide what is basic, what is applied; what is academic, what is nonacademic; what is research, what is education; or even what is Federal, what is non-Federal. There is probably no easy way to improve our statistical picture of research in the Federal Government: essays such as Brook's point out the complexities and may help Government administrators bring their data together in more useful ways.

One aspect of research statistics is particularly troublesome: this has to do with estimating the cost/effectiveness ratios of research. Here it is not so much a matter of statistics. Rather, as Johnson implies, there are exceedingly difficult questions on which professional economists are still groping for ways to make progress. He suggests that additional research be done on this group of questions. Pfaffmann echoes this in urging wider support of behavioral sciences research generally on the economic and social implications of science and technology.

A few specific statistical studies are suggested by some of the essayists. For example, Weinberg suggests that the Government try to establish how the support of basic research in each field of science is now distributed among the agencies of Government. Since some basic science is mission-oriented and some is not, it would be useful to know how the agencies differ in their assessment of relevance of basic research to their missions. Kistiakowsky notes how uneven this assessment now is.

II. *The Role of the National Science Foundation: Conclusions*

Two really major conclusions emerge from these papers. The first is that Government should recognize that, on the whole, science in the United States today enjoys preeminence, and that what is done in the future should be based on expanding and improving the present situation. Though some illogicalities may exist, of course, in the conduct of so large an activity as Government-supported science, it has so far been effective and there is no reason to change it drastically. In a sense, this constitutes the simplest answer to the first question. The more detailed suggestions by various panelists: To look at academic research separately from other research, or to provide support as broadly as possible for "little science," or to treat "big science" differently from "little science," are inherently complex and are made against a background of belief that the Government's scientific policies in the past have been generous and responsible, and that U.S. science has done very well indeed.

The second essential point that runs through at least half the papers is the belief, stated either explicitly or implicitly, that the role of the National Science Foundation during the next decade should become much greater than it has been in the past, especially in the physical sciences. The crisis in the physical sciences has already been alluded to. As the handmaiden mostly of mission-oriented agencies such as the Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration, whose missions are not likely to expand in the immediate future, these sciences are caught in a squeeze. Yet, as many of the panelists have argued, the physical sciences should expand, though perhaps not as rapidly as the biological sciences.

The reasons for such expansion are: (1) That biological and environmental sciences, to which Government is already heavily committed, will increasingly depend on advances in and people from the basic physical sciences; (2) that although military and space research and development expenditures appear to be declining, the Nation must continue to build up its stock of knowledge and people in the physical sciences, on which future advances in military and space technology will rest if a later emergency requires renewed emphasis on these fields; (3) that the physical sciences rather than the biological sciences have been the major source of past improvements in civilian technology, and support of them is, therefore, important for the further growth of productivity in the U.S. economy.

Two courses for providing increased support to the physical sciences are open, and probably both should be followed. The first, recommended by several panelists, is that the mission-oriented agencies, at times such as this when budgets are rather stationary, should devote a larger fraction of their budgets to basic research. This implies that they incline toward a broader interpretation of what kinds of basic research they deem relevant to their missions than is sometimes the case now; or even that Congress extend the mission of the agency to include the pursuit of certain branches of basic science, if this is necessary.

The second course, which by no means excludes the first, is to make the National Science Foundation a much larger agency than it now is—so large that it can eventually become the "balance wheel," or even the main "umbrella," for the support of basic research—especially in the physical sciences—that is too remote to merit support from the mission-oriented agencies. Such a specific policy with respect to the future growth of the National Science Foundation involves a major political decision by Congress and by the executive branch, as formidable and far-reaching as its decision has been with respect to expansion of the National Institutes of Health.

OPPORTUNITIES AND REQUIREMENTS IN THE LIFE SCIENCES

by Lawrence R. Blinks
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In addressing myself to the question of how much support should be made available to science by the Federal Government, I felt I might best contribute by indicating some of the accomplishments, present requirements, and future opportunities of the life sciences. This may provide some background for judgments concerning support needed by biology in comparison with some other sciences, such as astronomy, physics, and chemistry, which are making large-scale appraisals of their requirements.

Biology is closely related to astronomy, geology, physics, and chemistry, for living organisms, which are its subject matter, are but extraordinarily involved and intricate assemblies of physical particles, and chemical molecules. They were shaped by cosmic and geological history, and have affected the earth, in turn, by laying down great limestone deposits and forming fossil fuels such as coal and oil.

Life still gets its daily energy from the earth's star, the sun, trapped through the activity of land plants and water-living algae. Man now probes out to our sister planets, and may some day venture to reach them.

Like other sciences, which show us how to find minerals, utilize nuclear energy, and produce useful chemicals, biology has application. It is the basis of agriculture, forestry, medicine, and some of the most fundamental industries and arts. All of these profit from the evolution of general biological principles, derived from intellectual curiosity as to how life originates, develops, and functions.

Many principles turn out to be the same for men, for higher plants and animals, one-celled protozoa, bacteria, and algae. Discoveries concerning the simpler and more accessible organisms that the biologist studies can often be extended to the cure of disease, the supply of food, and the control of human population. These are surely three of the most important challenges facing mankind in the next generation and the next century; and they must be solved largely by biological procedures. Thus, it is essential that progress in the biological sciences be supported if America, and the world, are to meet these important challenges.

The present state of biology reflects some striking trends. The greatest of these is the breakdown of barriers that formerly isolated biology from physics and chemistry, and divided it into several disciplines, such as botany, zoology, and microbiology. This unifying trend is reflected in the establishment of biology, biochemistry, and biophysics departments in many American universities. It is the consequence of two other trends—the discovery of more and more common features of all life, giving a fundamental unity to biology; and increasing success in the attempts to describe life at physicochemical and molecular levels.

This now well-recognized unity of biology permits rapid progress by concentrating research on the simplest and most readily controlled organisms. These include viruses, bacteria, and other unicellular creatures like algae and protozoa, as well as the isolated cells of higher organisms (including man).

The "molecularization" of biology is rapidly bringing the causal analysis of structure and function to its most basic level. The implications of this are tremendous for understanding the hereditary makeup of organisms (including man). Knowledge is being created and tools forged for immensely powerful biological applications, which may lead to a revolution comparable in importance to or even greater than that of the nuclear age in physics.

Although these new developments are bearing such exciting fruits, the importance and usefulness of other aspects of biology should not be minimized. Failure to keep these other branches active could lead to attenuation of many motivating ideas of physicochemical biology itself, and to the loss of much theoretical and practical advance. Two branches in danger of possible atrophy (because they may appear less glamorous at present) are supracellular biology and "systematic" biology.

Although molecules are the structural basis of cells, and the laws of physics and chemistry govern their functioning, most of biology is supracellular. At each level of supracellular organization, there are important problems that can be investigated only at that level. Cells in isolation do not usually show either the same form or the same function as cells integrated into tissues—e.g., muscle, nerve, glands; wood, pith, cortex. Combinations of tissues introduce further aspects of form and function, as in brain, heart, eyes; root, stem, leaves. Organs combine to form organ systems—for circulation, communication, digestion in animals; for food production, transport, and storage in plants. The next level is the integration of organ systems into complete individuals—animals, plants, humans; these, in turn, constitute populations—families, herds, hives, cities, grasslands, forests, swamps. These populations interact not only with their physical milieu—air, water, soil, light, temperature, etc.—but with each other, and, above all, with man himself.

Each of these levels of biological integration has its own principles and methods of investigation, which indeed involve physics and chemistry, but also include many strictly biological concepts—inheritance, variation, succession, evolution, behavior, competition, survival—which are not reducible as yet to their physicochemical origins. Indeed, it is a fact of scientific history that physicochemical principles often become applicable to biological materials only after strictly biological methods have defined the problems, and have identified favorable organisms on which to attack and solve them. Hormones, like adrenalin or plant-growth substances, could not have been anticipated by chemists until studies of animal and plant behavior suggested the presence of such transportable messengers. Modern molecular genetics could not have developed until strictly biological methods of breeding analysis and microscopic observation, and patient biological experimentation, had led to and validated the chromosome and gene theories of heredity. Nor is it likely that the organic and biological chemist would have chosen the fruitfly, the bread mould, or the bacterium *E. coli* for the study of chemical genetics, without the knowledge of the biology of these organisms which showed them uniquely adapted to quick and significant experimentation.

Physiologists, embryologists, endocrinologists, ecologists, marine biologists, and other life scientists are exposing other facts and principles which are prerequisites to the physical and chemical explanation of phenomena at supracellular levels of organization. It is, therefore, essential to assure the vigorous pursuit of each of these more complicated levels of biology. Only thus can there be a truly integrated, theoretical science extending to all manifestations of life—a biological unity.

On the other hand, the students of biological diversity—the taxonomists and systematists who compare organisms, name them, group them, and establish their relationships—are exposing the other "face" of biology. In the midst of an overall biological unity, there exist literally millions of species of plants, animals, and microbes, each with its own distinctive form, function, and interaction with its neighbors. This grand view of the order of nature—the synthesis and visible result of millions of years of change—provides the picture of how evolution has proceeded, and hence aids in understanding how it may be directed in the future by man. It also discloses the raw materials from which to develop useful new plants and animals (or even microbes—to supply new antibiotics).

The exploration and classification of the world of existing organisms also provides experimental biologists with the organisms most suitable for their research, and guarantees to each of them that he is using the same plants, animals or microbes described in the work of others. Only thus can one arrive at insights into what is generaly valid, or what is special for given organisms. On the study of diversity, therefore, depends the ultimate discovery of valid unifying principles, and the definition of many significant problems.

We may now turn to some of the particular subdisciplines of biology to indicate some of their needs, and requirements for support.

Taxonomy and Systematics

The task of identifying, describing, classifying, and relating in their family trees the millions of species of plants, animals, and microbes is, after two centuries of effort, probably only half finished. Many parts of our planet, such as the rich tropical jungles, polar regions, high mountains, deserts, and the depths of the ocean, are still in need of further exploration. Organization of the necessary expeditions is often very expensive. Moreover, modern taxonomists have come to appreciate the advantages of bringing living organisms into the laboratory, and supplementing gross descriptions of adults with studies of breeding relations (genetics), development (embryology), and even physiology and biochemistry (e.g., for immunological reactions). The biochemistry of microorganisms is often their most certain identification.

Among the needs in the immediate future are larger numbers of systematic biologists with a flair for intuitively grasping the relationships of species, well trained not only in careful observation and description, but also in statistics, genetics, population behavior, embryology, and chemistry. This is a large order indeed, and would make the ideal systematist perhaps the most well-rounded of all biologists. Unfortunately he is often just the opposite. But modern "biosystematics" is an interesting and exciting discipline, which should increasingly attract able young workers. They must be subsidized.

Furthermore, not only must type specimens of all known organisms, with duplicates for study, be deposited and catalogued in central museums and herbaria; but also they should be kept alive in zoos, botanical gardens, or controlled-climate facilities. Thus they will be available, alive, for reference and future study, instead of becoming pickled or dried "vouchers." Accomplishment of this is expensive. Organisms proving useful for experimental study should be maintained, often in many genetic strains, in type culture collections, or as viable seed, from which researchers can obtain them at will. Such storehouses of genetic material constitute a major source for agricultural experimentation, for eventual improvements of existing plants and animals (e.g., as to disease resistance), and for potentially important additions to our list of useful organisms. The precious genetic materials brought back alive by expensive and laborious expeditions (or developed in the laboratory by mutation) must not be lost, expensive though their preservation may be.

Unforeseeable, but almost certainly valuable, additions to knowledge and practical utilization will require well-staffed and well-equipped collecting expeditions, especially to little-explored regions of the earth and the seas. Desert laboratories and oceangoing laboratories will be needed, along with arctic and tropical stations, as expedition headquarters. These are as essential to biological exploration as observatories are to astronomical exploration—and are becoming as costly.

Genetics and Molecular Biology

The study of heredity has become a central and unifying discipline of biology. It makes contact with and leads to virtually every aspect of the science. It has provided a concrete basis for the species concept, the central idea of taxonomy: those organisms that can interbreed and produce fertile progeny constitute a species; those that cannot belong to a different species. Although this definition has its limitations and difficulties, it has proven to be of great value, and has, as pointed out earlier, made breeding tests of great value to taxonomists.

Genetics leads to all other aspects of biology because of the obvious fact that heredity determines the characteristic properties of every organism. This includes its system of reactions to other organisms and to its physical and chemical environment. To understand fully how an organism develops and functions and reacts, one must understand its hereditary mechanism and how it operates.

It is well known that the hereditary mechanism resides chiefly in genes, a strictly biological concept worked out long before their molecular constitution was known. Genes are arranged end-to-end in linear series in the chromosomes of the nucleus. The laws of heredity are the rules—the same for all organisms—by which the genes are transmitted from parents to offspring, including the rules by which sexual reproduction leads to various combinations of genes. Evolution is the process by which new genes (mutations) and new genic combinations arise and are propagated by natural or artificial (i.e., man-directed) selection. Development from the egg is fundamentally the process that leads to the different action of various genes of the set in different cells of the body—a process affected by cellular interactions and environmental influences (such as disease or poisons). Adult physiology and behavior are due to the functional interactions of direct and indirect products of genic activity.

No wonder that the gene and genic action have become central in modern biology! Progress along these lines has been continuously accelerating during the past quarter-century, especially with the shift of attention from more complex organisms such as flies and corn—or mice and men—to simpler ones such as fungi, unicellular plants and animals, bacteria, and viruses. The essential genic material has been shown to be nucleic acid, especially deoxyribonucleic acid (DNA). Its structure—a linear, irregular sequence mostly of four kinds of chemical units—was only recently discovered, as was the main feature of how it can reproduce itself so exactly. A gene turns out to be a length of DNA: hundreds to a thousand or more such units in a sequence peculiar to that gene—each gene having a different sequence. A mutation turns out to be a change from one of the four kinds of units to another, at any one of the hundreds of positions in a gene; or the insertion, removal, or transposition of one or more units.

Genes were soon shown to act by somehow controlling the formation of proteins—one of the most distinctive chemicals found in living organisms, formed only by organisms or their parts. Proteins are also long-chain molecules, but they consist of 20 kinds of units— again in a sequence that is unique for each kind of protein. Recent evidence shows that there are three times as many nucleic-acid units in a gene as there are protein units in the protein molecule controlled by the gene. This and other evidence led to the conclusion that each sequence of three nucleic-acid units (a triplet in a gene) somehow "codes" for a given protein unit. This code—the Rosetta Stone of life—was partly deciphered a few years ago, and further study of it proceeds apace. Concurrently, biologists have been discovering the mechanism by which the code operates upon protein formation: It is first transcribed onto a closely related chemical messenger (RNA) which carries the genic message out from the nucleus into the cell body; in the process it combines with a minute particle, the ribosome, which is the "protein factory." The order having been received, the protein is custom-built to the nuclear specifications. The sequence of triplet nucleic acid is translated, from one end to the other, into a sequence of corresponding protein units until the whole protein is completed. The translation involves a very complex machinery, much of which is already understood. It is one of the most complex of programed factories.

This marvelous and exquisitely controlled process, so briefly and inadequately described here, is literally the core of modern molecular biology (just as the nucleus is the core of the cell). It is the study of protein synthesis under genic control. And why are proteins so important? Not only are they major buildings blocks of the cell, making up many important structures; but also they include such important molecules as hemoglobin, the red oxygen carrier of the blood; the contractile myosin of muscles; and all known enzymes. The latter, of course, are the catalysts responsible for practically every characteristic vital reaction, from digestion through respiration to excretion. Many aspects of nerve and muscle activity, as well as fermentation and photosynthesis in plants, involve dozens of stepwise enzyme catalyses. If the formation of an enzyme is blocked by a mutation (which garbles the genetic code), then some step of an important function, such as respiration, fails, and the whole process may stop. (This biological dictum, "one gene, one enzyme," was first well established in a common bread-mould.)

To carry on work of this sort requires increasingly complex apparatus: instruments that count the scintillations produced by radioactive chemical tracers; others that separate molecular species by high-speed centrifugation, adsorption, or electrical migration; still others that analyze the molecule by its characteristic adsorption of infrared or ultraviolet radiation. These are now standard and indispensable tools for genetic and molecular researches, and they may cost $10,000 apiece and upward. No longer does a $500 microscope suffice, nor do the breeding cages, milk bottles, and test tubes of earlier genetics. To keep American biology out in front (and it is indeed leading the world in the particular fields discussed here) it will be absolutely essential to supply investigators with these and ever more complicated instruments. Biology, while not yet as expensive as nuclear physics or astronomy, must be at least as well provided with tools as is modern chemistry; indeed it uses many of the same tools.

Given the tools—and well-trained brains—the results will be not only of fascinating theoretical importance, but also often of practical value as well. In the not-to-distant future, individuals who have inherited a gene that makes a defective protein (such as an enzyme or a hormone) may be provided with a proper one, for it is possible even now to have the right genes make the right proteins in a test tube. It may even prove possible to change the defective gene into a normal one, though that still seems far away despite some encouraging discoveries with microorganisms. In any case, the strategically promising research for the health-oriented and agricultural sciences is that which goes to the heart of the problem: to the chromosomal genes—their nature, action, and mutation; and to the mechanisms that control their differential activity.

It seems clear that study of the biological nucleus must yield as important results as has the study of the nuclei of chemical elements. It is desirable that such fundamental study should go forward at the maximum possible speed.

Cytology and Cellular Physiology

Moving from nuclear to cellular biology, we again must consider structure and function. Both involve genetics and molecular biology, for the study of chromosomes is one of the classic topics of cytology (cell structure) and the study of protein synthesis and nucleic-acid metabolism are within the established domain of cell physiology. This is a good example of how the old barriers between biological disciplines happily are vanishing.

But there is much more to these domains. Cell physiology embraces the membrane control of substances passing both in and out of the cell (and its parts); the accumulation of salts; the loss of waste materials; the trapping of solar energy by plants, and its utilization in synthesizing cellular materials. It includes the intake of materials by animal or bacterial cells; respiration or fermentation of foods; the mechanisms of ameboid and ciliary motion; muscle contraction and the transmission of the nerve impulse; and the generation of heat, electricity, and light.

These are examples of the complex and highly patterned multienzyme cycles by which the living machinery operates.

Enormous strides have been made recently by the use of radioactive tracers to follow the pathways in the cell taken by various essential molecules, and their inclusion in strategic structures (including the nucleus itself). Modern methods for breaking up cells with the least possible damage to component parts (expert "watch-grinding") have led to the isolation of organelles, such as nuclei, plastids, mitochondria, ribosomes, spindle fibres, and internal membranes. It has become clear that different parts of the cell's working are closely associated with this or that organelle; thus respiration occurs in mitochondria, photosynthesis in plastids, protein synthesis in ribosomes (as already described). The detailed molecular organization of these structures, indeed of every component of the cell, thus becomes basic to an understanding of their function. It has been possible to isolate many enzymes from the constituent particles, but until recently it was not possible to "see" at molecular levels.

Cytology was limited for years by the lenses of the light microscope, which had indeed revealed many of the subcellular entities, but scarcely their basic structure. All this was drastically changed about 25 years ago, when the electron microscope was invented. It is still being perfected, but under ideal conditions it can reveal particles as small as 5 to 10 angstrom units; that is to say, a few hundred-millionths of an inch. These are about 500 times smaller than objects just visible in the light microscope, which in turn are about 500 times smaller than objects just discernible by the naked eye. It is obvious that various sciences (including colloid chemistry and geology) could utilize such magnification, but its most valuable application has probably been in biology, to reveal fine structures below the limits of light microscopy. The cutting of extremely thin sections, and the application of mineral stains or shadowing techniques, have opened up a previously unexplored microcosm in the cell. Now the beautifully clear electron-micrographs reveal the fine details of cilia, membranes, chromosomes, ribosomes, mitochondria, and plastids; and have shown the remarkably organized structures of bacteria and viruses, mostly below the resolving power of the ordinary microscope. Thin membranes or laminations a few molecules thick can be discerned, and the ordered aggregation of macromolecules into fibers or flagellae clearly seen.

Another powerful tool for molecular analysis has also recently been exploited in biology, though applied for a longer time in physics and crystallography. This is the X-ray, used for discovering molecular arrangement in organized fibers such as those of cotton or wool, and in cartilage or muscle. With the aid of the highest-speed computers, X-ray crystallographers have gone far toward elucidating the exact conformation of all the subunits in some proteins, such as myoglobin from muscle.

While this technique is only now coming into use, its promise for the future is enormous.

One difficulty is the paucity of personnel trained to interpret the complex X-ray photographic patterns, by means of mathematics, chemistry, physics, and the lore of computers. Another is the great expense of the tools—not only of the computers (up to $500,000) but also of the X-ray machines.

Electron microscopes also cost up to $40,000 each (as against $1,000 for a good light microscope). The time is actually at hand when one of every four laboratory biologists could well utilize an electron microscope in his daily work.

Centrifuges and oscilloscopes were adequate a generation ago; now it is X-ray equipment, electron microscopes, electron spin resonance instruments, and mass-spectrometers. Tomorrow the computer will be added. It is clear that large sums of money will have to be made available for such biological instrumentation if American biology is to retain its present eminence.

Development

Like the rest of biology, embryology—the study of development—has in recent years taken on a new character. Not only have the new knowledge and methods of molecular biology been exploited, but at least two new strictly embryological approaches have been developed.

On the one hand, the development of adequate media and techniques has made it possible to explant cells and tissues into cultures in glass, where they may be maintained for long periods away from the parent organisms. This permits the study of interactions between diverse kinds of body cells, how they "recognize" each other, how they respond to each other, and what pattern of organization they set up when they come together again, as they do, after artificial separation. These isolated, simplified, and controlled systems are giving new insight into the decisive intercellular events that occur during the much more complex development of an intact embryo. On the other hand, the development of new, delicately precise instruments, the micromanipulator and microforge, permit analytical operations not heretofore possible.

We have referred above to the all-important nucleus, which transmits the genetic code from generation to generation. What is its role during the change from a relatively undifferentiated egg into the highly developed adult? One way to approach this problem is to remove the nucleus from one cell or inject it into another. Thus nuclei can be removed from eggs, and others introduced from embryos at later stages of development—and vice versa. Such procedures have revealed that nuclei undergo progressive and very persistent changes as they "age" during the process of development. Much more can be learned by such techniques about the alteration of the genetic message with time. It can already be stated (from work on micro-organisms) that there are "gene-control" mechanisms that regulate the activities of many specific genes; this results in inhibition or stimulation of various kinds of protein production. It is surmised that similar changes occur during development of higher organisms, so that a given gene is only active in certain cells and at certain times. This has recently been verified in full by studies of giant chromosomes, in which the position of a given gene, as well as its state of activity or inactivity, can be directly seen. Such analysis, which is only beginning, promises to produce important new knowledge. It is closely relating development to the orderly control of gene action.

The molecular analysis of development is also being advanced by recent successes in culturing explanted cells, in the use of radioactive tracers to follow cellular interactions and transfer of materials, and in very sophisticated physicochemical studies on the regeneration and repair of wounds and excisions. (This is a sort of "delayed embryology" by which damaged tissues can be reconstituted.)

In the opinion of many biologists, one of the greatest areas for spectacular advances in the near future will be the analysis of development. The importance of such knowledge for human biology has been widely publicized in recent years in connection with thalidomide injury in babies and disturbances in development resulting from certain mild diseases like German measles in pregnant women. It is clear that animals must be used in the study of such questions.

Physiology of Organs and Organisms

This discipline deals with the highly diversified functioning of the adult organism, whether plant, animal, or human. It is an old and well-established discipline which has had molecular aspects much longer than has the rest of biology. The explanation of the manifold activities of organs, like heart, brain, kidney, and muscle, or root, stem, leaf, and fruit, was always sought in terms of chemistry and physics. Modern physiology continues to develop along such traditional lines, but with the added advantages of more sophisticated apparatus, and with the fertilization of new ideas from other life sciences. Thus hormones, long known to change the activity of cells, are now suspected of doing so, in part, by controlling the action of specific genes in certain cells of the body; excellent evidence for this has been shown in an insect. Many investigators, indeed whole institutes, have recently begun to search for the mechanism of learning and memory in the production of specific nucleic acids or proteins. The physiology of the brain, nerves, and sensory organs, and of behavior mechanisms in general, has entered a new phase with the application of biophysics and of specially designed computers. The analog computer has been helpful in analyzing the time course of the nerve impulse. It is quite possible that the phenomenal success that crowned the study of viruses and bacteria during the last quarter-century may be matched in the years ahead by the study of the neuron, the individual nerve cell. Curiously enough, the generation of electric impulses like those of nerve in certain plant cells has been of value in understanding the conduction mechanism.

The situation created by the increasing proportion of old people in our population has stimulated research into the problems of the aged (gerontology). All kinds of organisms, from bacteria and protozoa through plants and animals to man, display changes with age, and biologists, studying organisms with relatively short lifespans, may contribute to an understanding of human aging. The oldest living things such as the redwoods and certain pines, when compared with short-lived aspens or cottonwoods, may have something to tell us about aging.

Among the topics of interest in the overall physiology and behavior of animals is the migration of birds, which seem to accomplish a kind of celestial navigation by reference to stars. This is just beginning to be understood. So is the remarkable ability of other animals, notably the bat, to catch food or avoid obstacles completely in the dark; this it does by emitting characteristic high-pitched "squeaks," or pulses of sound, which are echoed by objects in the environment. It is almost impossible to "jam" this remarkable echo-sounding device. Porpoises employ a somewhat similar sound-echo system in the water; and electrical impulses are broadcast by some fishes for the same purpose. Some marine animals seem to be responsive to polarized light. All these navigational devices are suggestive of further developments and application, even by man himself.

Ecology

Change is inherent in the history of the earth, and during millions of years, organisms have had to adapt to altered environments. This has been a condition of the selection process, which has, with genetic mutation, aided the evolution of present-day forms, with their astonishing ability to fit almost every climatic niche, from swamp to plain to highest mountain. Some forms of life exist in all earth's environments. Caves, the dark depths of the sea, the driest desert, the coldest snowfields—all support some peculiarly adapted organisms—if not large, then microscopic. The study of the means by which extremes of light, humidity, temperature, or pressure are tolerated is one of the important biological disciplines and one of great significance to man. It has for half a century been more a matter of observation than of experimentation. But with the development of facilities for controlling the environment (phytotrons, biotrons, climatrons, marinostats), these are becoming matters for experimentation as well. And much has already been learned about the effects of such important variables as length of day and night temperatures upon such various phenomena as flowering of plants, migration of birds, and hatching of insect pupae. A few minutes difference in day length, or exposure to a brief flash of light in the middle of the night, can trigger astonishing biological results. (The color of the light is also of extreme importance, infrared often being able to oppose the effect of red light, and vice versa.)

It is obvious that such control of the environment requires equipment whose building and maintenance is both elaborate and expensive. Highly important results can be expected from such facilities.

Other environmental factors (besides these physical ones) are biological. These include not only the obvious ones of predator and prey, or grazer and grazed, but the more subtle one of competition for space. Even a slight advantage possessed by one species over another may give it quick dominance. Here is involved heredity and the diversity of species, and also behavior, the study of which is coming more and more into biological realms under the name of "ethology." The "territoriality" of many animals that defend strongly delimited areas is not without its human implications. The excretion of poisonous substances into soil gives some plants a territorial protection, especially in the desert.

The preservation of forest or desert, seashore or mountains, for the use and enjoyment of present and future human generations is highly dependent upon a knowledge of ecology. Small environmental changes—draining, flooding, air and water pollution, sometimes grazing and trampling—may become disastrous to a beautiful or useful plant cover, or may drive away interesting or valuable animals. Conservation of natural resources is frequently dependent upon understanding of the factors necessary to maintain an adequate environment for survival. It is highly important that our few remaining natural areas be maintained for such study, as well as for the use and enjoyment of people. Most of the remaining wildlands are in Federal control, and should be adequately protected for scientific as well as recreational and watershed purposes.

Pathology and Disease Control

Like humans, plants and animals are attacked by a great variety of diseases. Even some bacteria have virus infestations (e.g., bacteriophage). While the control of disease can often be accomplished by chemical means such as sprays, many types of biological control are also employed. Often there are intermediate hosts, which carry the disease through one of its stages. Well-known examples are the transmission of malaria and other diseases by mosquitoes. It was a triumph of biology when this was discovered, allowing for biological control and later physical or chemical warfare against these diseases. Many other insects such as fleas and lice, as well as the spider-like ticks, are carriers of disease. Snails are intermediate hosts for the liver fluke of sheep, and the barberry bush for white pine blister rust. The recognition of such carriers requires a strictly biological procedure, involving the ecological tracing of the chain. More subtle problems remain, such as the marked specificity of the host—intermediate or final. Why are some species immune and others susceptible? Why must an alteration of infection occur in so many instances? What is the genetic and biochemical basis for susceptibility or resistance? Biological techniques, such as genetic selection for disease resistance in commercially important plants and animals, can be coupled with quarantine measures and the introduction of counter-parasites, which can destroy the organisms causing disease. Novel techniques such as the broadcasting of irradiated males (rendering their progeny nonviable) have already wiped out whole populations of insect pests. The sensitivity of insects to extremely dilute odorous molecules may be utilized as well.

The importance of disease is not limited to medicine and agriculture; it may cause the fall of wild populations in a few years: Witness the disappearance of the American chestnut forests and the Bermuda juniper within a few years. Wild animals such as deer are subject to diseases that cause wide fluctuations of population. Not only bacteria and viruses but also fungi and parasitic worms are important causes of disease. The whole problem of parasitism and the changes of physiology that parasites display must be better understood. Immune reactions in invertebrates are proving to be different from those in higher animals.

Fortunately the electron microscope, mentioned above, is proving immensely valuable in the study of viruses, and the tools of the biochemist as well as those of the geneticist are elucidating the characteristics of virus multiplication in cells. Nonmedical entities, such as the virus-causing tobacco mosaic disease, are still advantageous for the study of many of these problems. Ecology is an especially important aspect of disease study and control—especially in the tropics.

It is clear that disease is a strictly biological entity, not found in physics, chemistry, or geology; its study, while involving all the tools that other sciences can supply, remains a largely biological discipline. Students must be trained in its various aspects, and exploration must continue to uncover disease-resistant organisms and to supply information on intermediate hosts. Breeding stocks of grains and other crop plants must be maintained to anticipate newly evolving or arriving strains of bacteria, fungi, and viruses, which constantly arise to plague organisms temporarily protected by sprays, insecticides, or antibiotics. Eternal vigilance is the price of safety, here as in warfare. And the expense, perhaps our greatest biological one (except medicine), must continue to be met.

Space Biology

This brief account cannot close without mention of one of the most exciting prospects in the future of the life sciences—the exploration of life on other planets, or "exobiology," as it is sometimes called. No one can say whether organisms will be found there; some of the space probes raise doubts as to whether life as we know it may exist on our nearest planet neighbors, Venus and Mars. But it still remains an intriguing prospect for many biologists. The techniques of discovery may have to be subtle and indirect, for it is unlikely that living samples, or even preserved ones, will be brought back within our generation. But physical and chemical tests for certain compounds, such as enzymes and characteristic pigments, known only from living systems, may tell us whether life is there. The task is tremendous, taxing all the ingenuity of some of America's best biologists; and it is certain to be immensely expensive. Some people question whether the money so spent would not better go to elucidating terrestrial biology, curing disease, improving crops, and similar worthy activities. Doubtless both purposes should be served.

Meanwhile, the present announced national policy, requiring the effective sterilization of all probes sent to the planets, must be maintained, for once terrestrial bacteria, viruses, and higher organisms are deposited upon Mars or Venus, they may multiply so rapidly that all chance of discovering native fauna and flora will vanish—and with it one of the greatest opportunities that biology will ever have had.

Summary

This brief account, necessarily selective and leaving many interesting developments in biology untouched, has, I hope, showed needs and opportunities in some of the life sciences. More biologists are required and should be trained under appropriate fellowship support. Biological instrumentation, borrowing more and more heavily from physics, chemistry, and mathematics, becomes increasingly expensive. Facilities for environmental experimentation must be extended and supported. Exploration and the maintenance of discovered organisms from all over the world (including the oceans) must be kept up. Both biological oceanography and exobiology involve expensive activities, but both national prestige and well-being demand that efforts in these fields be continued and increased. Disease must be controlled in forest, field, and farm. Advances in understanding the immensely complex biological system can help solve the problems of food, population, and medical care for the Nation and the world. The prospects are really great that the next generation may see a biological revolution as far-reaching in effect as that produced by nuclear physics and electronics in the generation just past.

Thus life sciences should be supported at least as substantially as the physical sciences have been in recent decades, and with a minimum rate of increase of 15 percent per year, indicated as being desirable in other papers in this series. Many physical scientists feel that support should be even greater for biology as it enters an era of unprecedented fruitfulness.

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REFLECTIONS ON THE RELATION BETWEEN SCIENCE AND TECHNOLOGY

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Summary

This paper gives a quasi-historical review of the relation between science and technology. It also attempts to describe some of the outstanding characteristics of science and technology in recent decades. Throughout most of their history the relation between science and technology has been quite loose, and applications of science to technology in many areas have been casual and dilatory. In recent times science and technology have been growing steadily closer together. However, the deliberate effort to apply science to technology on a broad scale and with maximum exploitation of comparatively new science is essentially a phenomenon of the war and postwar years. Since modern science now has more to offer technology that it ever had before, this trend is full of promise, particularly if scientific research can be kept at a sufficiently high level and in sufficiently close contact with the body of technological activity. However, the systematic application of science to technology on the present scale is a relatively new idea in human affairs, and raises many problems of its own. It appears likely that the benefit to the country from basic scientific research will depend at least as much on the skill with which we manage to solve these problems as it does on the basic research effort itself.

Introduction

Undoubtedly most of the papers in this collection will begin by commenting on the great difficulty of preparing an adequate answer to the questions posed by the House Committee on Science and Astronautics, and the fact that any one paper can provide only a partial response to these questions. This paper is no exception. Certainly it has caused the author great difficulty, and it is only in a fragmentary sense responsive to the questions raised by the House committee.

In other respects, the aims of the present paper are still more limited than those to be expected from the other papers in the collection. In particular, I do not attempt to confront the central question raised by the House committee, but instead concern myself with some preliminary issues that seem important. Thus, this may be thought of as a sort of essay that should find its value, if any, as background for the other papers.

The central question raised by the House committee is that of the appropriate level of Federal support of basic research. This question is raised against a backdrop of other considerations: the country's position of world leadership, its activities in science and technology generally, and the beneficial end-effects that stimulation of science and technology, through basic research, will have on the country's cultural, economic, and military positions. Although science and technology are coupled in the statement of the problem, it must be assumed that they may be related to the various end-effects in different ways. Thus, the cultural life of the country should be most closely related to scientific advances, and its economic life to advances in technology, while in the present age our military position may be critically dependent on both factors. Similarly, all these elements may enter in various ways into the country's overall position of leadership.

In posing the issue of basic research against this background, one obviously assumes that the interplay among all these factors is reasonably well understood, so that the impact of changes in the level of basic research on the various end-results can be calculated, at least approximately. This paper is written in the belief that this takes much too much for granted. The interrelations among the various factors are too complex and too little understood for such a computation to be made. The practical connection between basic scientific advances, on the one hand, and technology, on which so much depends, on the other, appears to be particularly uncertain and complicated.

Thus, my fundamental position is simply that a satisfactory answer to question 1 cannot be obtained through consideration of basic research alone. The end-results in which the House committee is interested depend in a complex way on the total technological and economic structure. It is not necessarily true that an increase of basic research by itself would lead to a significant increase in these beneficial applications. The outcome would depend primarily on the overall structure. The primary problem confronting the country, then, is that of maintaining a scientific and technological establishment that works in a coherent and effective way. The central problems in such an establishment may well lie in fields of applied science and technology, rather than in basic research.

My treatment is largely historical. Basic research has been with us at least since the time of the Greeks, and technology for several millennia more, so that there is plenty of material to draw on to show that there has been no unique or necessary connection between the two. At the same time, the historical approach makes it relatively easy to isolate some of the striking new characteristics of contemporary science and technology, and thus to indicate some of the considerations that should be important in a systematic approach to the country's present-day problems. The treatment is intended to be merely suggestive, however; a definitive treatment would be beyond my capabilities.

The Question of Leadership, and Other Issues

Before attempting the historical résumé described above, it may be worth while to interpolate one other introductory section. The questions posed by the House committee are about the support of basic science, and they are addressed to a group most of whose members are identified primarily with scientific research. Thus, one might naturally expect a "scientific" answer. In fact, however, the questions imply value judgments and factual backgrounds that lie outside the universe of discourse appropriate for pure science. Thus, one has the choice of a narrow answer that stays within the scientific sphere, or a broader answer with a correspondingly larger seasoning of personal opinion and judgment.

There are two areas in particular in which the issue just raised is important to an understanding of the present paper. The first has to do with leadership, particularly leadership in scientific research. To a professional scientist, research leadership is an end in itself, to be measured by professional judgment, and requires no further justification. In this paper, on the other hand, leadership in any of the areas mentioned by the committee will be related to the international influence of the United States, with secondary emphasis on our domestic well-being. In other words, we shall be concerned with the Nation's ability to shape the world so that it will remain hospitable to western ideas during the present time of troubles.

Obviously, military strength, as mentioned in the House committee's question, is an important element in the United States' international position. In the long run, however, the future must be decided by voluntary acceptance of the basic elements in our culture, rather than by simple force. Thus, strength in science and technology is both an important element in our domestic affairs and an important aspect of our position of international leadership.

The other area has to do with the question of the values of basic research in terms of its economic, cultural, and military applications—the general field of this paper. It is worth noting that this is not quite a fair question to pose to a pure scientist, even if adequate information were available to answer it. All definitions of basic research agree in the statement that the actual motivation for basic research must be simple curiosity about an interesting and challenging aspect of nature. If the work is motivated in any more direct way, it is no longer "basic." Thus the basic researcher is almost necessarily driven to the comfortable, if unexamined, dogma that basic research always pays its own way in the long run. To ask him to examine the issue further is like asking a young lover to give some sensible reasons why the adored person is really so charming.

To stay within the logical confines of basic science, one cannot readily do more than make a more or less detailed exploration of possible lines for basic research, either within some fields of science or in science as a whole. Such an examination cannot furnish a complete answer to the House committee's questions in a logical sense, but it might well furnish an adequate answer for practical working purposes. For example, one might turn up the result that research possibilities in many areas are so promising that the country cannot afford not to pursue them with all qualified workers. In addition, one might find that the number of people in the United States qualified for and interested in doing basic research is in any case so limited that support for them is never likely to be a big item in the Nation's budget. This would make the support of basic research at most a matter of choice of areas to emphasize, rather than one of overall level.

It may help in the understanding of this paper to say that the hypothetical conclusion just stated is essentially what I believe to be true. Of course, one must make qualifications. "Science spectaculars" and "big science" generally must be left out of such a conclusion. Their values must be calculated separately, perhaps in terms of their direct contributions to the country's international prestige, as discussed earlier.

If the support for basic research is to be essentially open-ended, scientists must see to it that quality standards are kept high. The research blanket must not be allowed to cover large areas of plodding, uninspired work, only marginally "publishable" and in the long run merely a complication for the information-retrieval problem. The maintenance of appropriate standards, however, is a job the scientific community should do for itself. They are not readily imposed from without. Subject to these limitations, there seem to be no pressing reasons for trying to restrict budgetary support for basic research. Thus, the fundamental object of this paper is not to suggest limits for pure research but to urge that adjoining areas get adequate attention.

I. *Science and Technology in Historical Perspective*

A somewhat impressionistic version of the history of science and technology might be imagined as a graph containing two curves representing, respectively, the relative rates of advance in the two areas at various times in the past. The science curve would start several millennia before the birth of Christ, to reflect work, primarily in astronomy, in Egypt and Asia Minor. The first conspicuous feature, however, would be a big bump a few centuries before the beginning of the Christian era, to represent the great achievements of the Greeks during their Golden Age.

The Greeks of that period, however, were not noteworthy for technology, and the technology curve would still be at a low level for some time. The technology curve does rise a little later, however, so that for a century or so science and technology flourished together, as the Greek world merged into the Alexandrian. On the other hand, the science curve flattens out rapidly just before the birth of Christ, as the Romans, an eminently practical people, with no particular taste for or interest in science, became dominant in the Mediterranean world. The Roman world, on the other hand—in particular, the urbanized world of the Roman Empire—was quite competent and interested in technology, so that the technology curve continues at a substantial level for some time. After the fall of Rome, western European civilization rapidly decayed and both curves go negative, to indicate that previous knowledge and skills were actually being forgotten at a faster rate than new knowledge and skills were being generated.

The science curve does not rise significantly until the middle of the 16th century with the Copernican revolution, followed in the 17th century by the great age led by Galileo and Newton. This is generally regarded as the beginning of modern science. One need only think of Galileo with the pendulum, the telescope, the elements of mechanics; Newton with gravitation, the calculus, light; Harvey with the circulation of the blood; Levenhoek with the discovery of bacteria, using the first primitive microscopes; and so forth.

The technology curve rises much earlier, perhaps as early as the year 1100. Beginning then and continuing through the year 1500 or later, there was a steady improvement in the arts and skills by which people lived. In degree of advance, it meant as much as the Industrial Revolution meant much later in transforming the United States in the 19th century from the colonial period to a modern industrial state. The inventions and the new skills were numerous. To cite only those of special military value, the crossbow and the longbow were both developed in this time; gun powder and firearms, including primitive artillery, were also invented. So-called "Greek fire" appeared at the siege of Constantinople in the mid-15th century. A more subtle but perhaps more important advance was the development of a better metallurgy. This permitted lighter but stronger armor and much better swords and lances. Advances in shipping and navigation were equally important. The mariner's compass was invented. So also was the decked-over ship, propelled by square or fore-and-aft rigging. Such ships were far more rugged and far more maneuverable, especially in rough weather, than were the earlier light, open boats propelled by oars with only auxiliary sail power. The classic Battle of Lepanto, in which the Venetians destroyed the power of the Turks in the Mediterranean, was a victory for the new ships over the old. So also, in a way, was the defeat of the Spanish Armada, since the Spanish fleet had gone only halfway down the new path. These developments in ships, of course, underlay the great age of discovery that began near the end of the Middle Ages.

Following this burst, there was a coasting period for technology. New inventions were made and new processes found, of course, but they appear not to have had a profound effect on human life until the onset of the modern Industrial Revolution, which can be dated perhaps from the invention of Watt's steam engine about 1765. This led, in the first instance, to the application of steam power to weaving, spinning, and other industrial tasks. The first steamships and steam locomotives followed soon thereafter. It is hard to imagine many comparable developments that could have had such a technological impact, and one must consequently think of the technology curve as first rising to a high peak and then slackening somewhat after the first few decades of the 19th century. However, technological progress was rapid throughout.

Following the Newtonian epoch, there was also a coasting phase in pure science. Although much good work was done in the 18th century, the first notable upturn did not take place until about the year 1800. In mathematics, this was led by Gauss, commonly regarded as one of the three or four great mathematicians of all time, followed by Cauchy, Weierstrass, Riemann, and others. Systematic chemistry, based on Dalton's atomic theory, began. In physics, many fundamental discoveries were made, especially by Faraday and others in electricity. Much was also accomplished in the theory of heat and heat engines, culminating in the enunciation of the first and second laws of thermodynamics about the middle of the century. This work is of particular relevance to our modern industrial age, which depends so largely on mechanical and electrical power.

Even as abbreviated a sketch as this one is sufficient to establish the fact that, until about the middle of the last century, the connection between science and technology was very loose. In general, the times in which science flourished do not coincide with those during which technology was making most rapid progress. When they did flourish together they did not necessarily flourish in the same place. Unless we wish to go back as far as the Alexandrian world, the only real exception is furnished by England near the beginning of the 19th century, and even here it is probable that practitioners in the two fields had little contact with one another.

Another important fact emerges when we review this history in more detail. This is that, in a certain sense, science was far more indebted to technology than technology to science throughout this period. There were, of course, exceptions, but on balance the scientist was in the position of relying on technology, or, more broadly, on the world of practical experience generally, for his tools and much of his information. Technology "was there first." For example, the invention of both the telescope and microscope depended on a flourishing industry in spectacle lenses that already existed. Magnetism was known as an empirical fact, and had been used as the basis for the navigator's compass for centuries before 18th and 19th century physicists got around to studying the phenomenon. Watt's steam engine was invented without the benefit of the Carnot cycle, or Joule's work, and so on.

Thus, the work of the scientist was largely to refine and systematize the knowledge that technology in some sense already had. Of course, technology eventually profited thereby. The scientific understanding gained in the Newtonian epoch led to many advances in navigation, marine and civil engineering, and medicine. However, science was not yet in a position to contribute many actually new things to the world's stock.

II. *Science and Technology in Historical Perspective*

Science and technology begin to draw gradually closer together, and science begins to take the lead in some areas, as we enter the second half of the last century. Tables 1 and 2 give listings of some of the principal advances in science and technology over a 14- or 15-year period about a century ago.

TABLE 1.—*Some outstanding contributions to science, 1859–73*

Darwin	Theory of evolution	1859
Helmholtz	Basic theories of vision and hearing	ca. 1860–62
Kekulé	Structural theory of chemical compounds	1858–66
Pasteur-Koch	Germ theory of disease	1866–68
Mendel	Theory of heredity	1865
Mendeleev	Periodic table	1871
Maxwell	Electromagnetic theory	1873
Kirchhoff-Bunsen	Spectroscopic analysis	1859

TABLE 2.—*Some outstanding technological advances about 1860*

[Nonmilitary]	
Beginning of synthetic drug and dye industry	1856
Bessemer process	1856
Industrial dynamo	1860–70
First transatlantic cable (Kelvin)	1865
First plastic	1870
[Military]	
Improved explosives (smokeless powder, dynamite, etc.)	1855–65
Armored ships	1855–62
Machinegun (Gatling, Hotchkiss)	1861–72
Self-propelled torpedo (Whitehead)	1866

It is sometimes said that the present age of science is an unprecedented one—that man has never before advanced so rapidly. It takes only a brief glance at table 1, however, to show that the 14 or 15 years between 1859 and 1873 were at least equally rich. For example, the year 1859 is the year of publication of Darwin's *Origin of Species,* followed 5 or 6 years later by the *Descent of Man.* These are the equivalent in biology of the Copernican revolution in astronomy, as far as man's understanding of his place in nature is concerned. The year 1873 is the time of publication of Maxwell's *Electricity and Magnetism.* This includes, as an incidental, the whole of the electromagnetic theory of light. Almost more important for our modern age is the fact that it involves the basic theory of radio propagation. The fact that there could be such things as electromagnetic waves—radio waves—was verified with great difficulty 15 years later by Hertz. Without Maxwell's prediction, the field might have been undiscovered for many years.

Between the books of Darwin and Maxwell were a number of other only slightly less important advances. In chemistry, for example, Mendeleev enunciated the periodic law and Kekulé laid the systematic foundations of organic chemistry. In other areas, Mendel enunciated the so-called Mendelian laws of heredity, Pasteur enunciated the germ theory of disease—followed quickly by the work of the microbiologist Koch in isolating and identifying many of the most serious disease-causing organisms—Kirchhoff initiated systematic spectroscopy, essential to many fields, and so on. In spite of the richness of present-day science, it would be hard to contend that it has made a better record in any recent 14 years.

Table 2 gives some sample activities in the technological field that were going on at the same time. Here we begin to see some interconnections between technology and contemporary of earlier science. This is obvious, for example, in the chemical fields. The technological application of the dynamo was clearly a dilatory appreciation of the importance of Faraday's pioneer work nearly 40 years earlier. The telegraph cable may similarly be thought of as an outgrowth of scientific work during the first half of the century. The work of Pasteur and Koch was, of course, almost immediately applicable in medicine and public health. On the whole, however, this was an age in which the relation between science and the great bulk of technology was still quite remote.

As a matter of passing interest, table 2 also includes a listing of some of the principal activities in military technology during the same period, approximately contemporary with our Civil War. The idea of applying science and technology in warfare, which has been such a conspicuous feature of recent years, is, of course, not entirely new. It has been given sporadic attention on many occasions in the past, and was the subject of relatively lively interest at the time of the Civil War. What was involved at that time, however, was clearly the 19th century's technology—not its science.

As we enter the 20th century, the connection between science and technology gradually becomes closer. Tables 3 and 4 give a comparison of representative accomplishments in the two fields near the turn of the century. Several of the items in table 4—notably those related to prime movers—were, of course, quite remote from any contemporary, or near-contemporary, scientific activities, as measured by the other table. However, such engineering work could now rest on properly laid theoretical foundations in pure science, dating, in fact, largely from the first half of the 19th century. It did not need to depend primarily on empiricism or intuition, as it did in Watt's time.

TABLE 3.—*Some outstanding contributions to science, 1897–1905*

Thomson	Discovery of the electron	1897
Curie	Discovery of radium	1898
Hilbert	Foundations of geometry	1899
Planck	Quantum theory of radiation	1900
Einstein	Special theory of relativity	1905
	Quantum theory of photoelectricity	1905
Freud	Psychoanalysis	1904
Pavlov	Conditioned reflex	1904

TABLE 4.—*Some outstanding technological advances about 1900*

Marconi	Radio, practical experiments	1895–1902
Curtis	Steam and gas turbines	1896
Rudolf Diesel	Diesel engine	1897
Wright Brothers	Powered airplane	1903
Fleming	Radio tube	1904

In other items of table 4, notably Marconi's work and the Fleming valve, the advance was obviously an offshoot of a relatively recent scientific discovery. In still other areas, technological applications of earlier scientific discoveries were imminent, but had to wait a little longer for further scientific work. For example, Mendel's pioneering work in genetics, which had been overlooked for a generation, was rediscovered at this time and became the subject of further active research. This led within a few years to the systematic experimental work on new strains of plants and animals that are so important in our present-day agricultural economy. In chemistry, the plastics industry—now so elaborately developed—had made a slow and halting start in the 19th century. It finally began to make consistent progress with the invention of bakelite in 1907.

The decade between 1900 and 1910 was also the period when the large industrial laboratory was first established in this country. (Similar laboratories, principally in the drug and dye industry, had previously existed in England and Germany.) Du Pont, General Electric, and the Bell System all established substantial central laboratories at this time.

The individual inventor or engineer of the 19th century was likely to be a man whose primary training had been in drafting or the shop. The new industrial laboratories of the early 20th century showed how much more effective a better knowledge of science and the systematic application of scientific method could be in attacking technological problems. A series of public or quasi-public institutions, such as the Bureau of Standards, the Rockefeller Institute for Medical Research, and the Hygienic Laboratory of the Public Health Service (which later became the National Institutes of Health), were also either established originally or reestablished with broadened charters at this time. They performed a similar function in bridging the gap between pure science and its applications in their particular fields.

The major scientific advances near the turn of the century, as listed in table 3, are, however, interesting not so much for their immediate applications as for another reason. This also was a golden period for science, but it was primarily a germinal period, in which science made new starts whose ultimate implications were great but which lay well in the future. Pavlov's work on the conditioned reflex, which was begun in 1904 and has been a cornerstone of the behavioral sciences for the last few decades, is one example. We are all deeply immersed, of course, in the world of "electronics," ushered in by Thomson's discovery of the electron as a separate physical particle in 1897.

In several instances, new starts began with the discovery that the great achievements of preceding eras could not be taken quite at face value, but were subject to reconsideration or qualification. For example, 1898 is the year of the discovery of radium, which appeared to be a source of infinite energy, thus refuting the first law of thermodynamics so laboriously established in the middle of the 19th century. To reestablish the law, one had to postulate a special atomic world, which led finally to modern atomic and nuclear physics. The year 1899 was the year of the publication of *Foundations of Geometry* by David Hilbert, a German mathematician who is generally regarded as the proponent and formulator of the modern abstract school of mathematics. One of Hilbert's tenets is that mathematics exists whether or not it is in correspondence with the real physical world. Mathematicians were led to this position by the discovery of a number of so-called "pathological" cases such as Peano's space-filling curves, which, in defiance of ordinary intuition, were curves including all the points in a square or a cube. The year 1900 saw the enunciation of Planck's quantum hypothesis, involving the assumption that in some ways nature has to proceed in steps and cannot be continuous.

The period near 1900 was also a time of reappraisal of the theory of light, based on the negative results of the famous Michelson-Morley experiment some years earlier. This experiment, in a sense, left Maxwell's electromagnetic theory of light unchallenged, but without foundation. It indicated, in effect, that light may indeed be a wave motion, but that the so-called ether, which had been postulated as the substance in which the wave traveled, could not possibly exist. Physics was rescued in 1905 with the enunciation of Einstein's first theory of relativity. The theory includes the famous mass-energy equivalence now used to compute ultimate yields in nuclear weapons. Einstein in that same year also gave the quantum theory its first real support by establishing a quantum theory for the photoelectric effect. This further undermined Maxwell by pointing out that, on the atomic scale, light behaved like waves but also like bullets.

There is a consistent intellectual style to all these discoveries. Whereas the universe of Newtonian mechanics was obviously logical and straightforward, these new discoveries indicate a natural world that is in a sense perverse and subjective, that is full of paradoxes and not necessarily straightforward in any obvious way. Anyone who has had to accept the quantum hypothesis, or the relativistic barrier to infinite speed, or relativistic foreshortening, or the equivalence of mass and energy, or Peano's space-filling curve, can no longer believe that nature is made up of simple straightforward elements obviously accessible to the intuition of an untutored mind. Instead, it is subtle, sophisticated, and even deceptive. Freud's enunciation of the principles of psychoanalysis in 1904 is included in table 3 because it represents the same sort of change in point of view. Human behavior is no longer to be understood only through the obvious and rational aspects of the human mind, as the 18th and 19th centuries would have had it, but instead is to be studied by penetrating deeply into a hidden regime with a perverse logic of its own.

In the short run, this change to a more profound and subtle sort of science may have weakened the impact of science for the uses of the ordinary world. In the long run, however, it was full of promise. It meant that by digging deeply enough we could expect to turn up new phenomena and new relationships not readily predictable from ordinary experience. Thus science could play more and more the role of an innovator in technology, a creator of new devices and new ways of doing things. It was less and less confined to improving on and systematizing known technology, as it had been in its early days. The change has, of course, come about gradually, but it is one of the primary reasons for the strong impact of science in the present day.

Science and Technology in the United States

We can conveniently continue this sketch of the relation between science and technology by confining our attention to the United States, the area of particular interest to us. It is also convenient to center particularly on the years extending roughly from 1930 to 1940—the last decade before World War II. Since the war made vast changes in both science and technology and in their effects, this period is a natural point of departure for any more contemporary problems.

The situation in the United States, at least up to 1930, is of additional interest because it exemplifies so well the rather casual and distant relation between science and technology during most of their history. We have, of course, long been conspicuous in technology. Until recent years, however, the United States has not been a country of key importance in the world of science. We had occasional noteworthy men but they were very few in comparison with the number in older countries.

Table 5 illustrates this. It gives a count of Nobel Prize winners in the sciences (Nobel Prizes are given in physics, chemistry, physiology, and medicine) in the years 1900–30. One notices that scientifically the United States was able to give countries like Sweden and Holland quite good competition, and we actually led Switzerland, but we were hardly anywhere in the major league competition represented by Germany, England, and France.

TABLE 5.—*Nobel Prizes in science, 1901–30*

Germany	27	Denmark	4
England	15	Austria	3
France	11	Switzerland	3
Sweden	6	Italy	2
United States	6	U.S.S.R.	2
Holland	6		

In contrast, our contributions to technology have been important for many years. A rough documentation of this, using invention rates as an index of contributions, is attempted in table 6. Column A is a listing of the nationalities of the authors of major inventions from colonial times to the present as given in a recent popular almanac. Such a tabulation can, of course, be assailed on grounds both of probable chauvinism in the choice of inventors to whom credit is given and of lack of discrimination in choosing important inventions. As a corrective, column B gives the average annual patenting rate for the years 1930–39 in the countries concerned (*1*). Although the margin is somewhat more modest, this column also shows the United States in a leading role. It should be noted, however, that the result is due in large part simply to the fact that the United States is a big country. In proportion to population the Swiss invention rate, for example, is much higher than ours.

To describe adequately this period in our history we should make several further remarks. One is that the stature of American science had in fact been growing steadily ever since the late 19th century, when several universities had reorganized their graduate instruction along the lines of the best European models, and 1930 is about the last possible year when one could conceivably ascribe any inferiority to American science. During the 1930's, for example, the United States finished approximately in a triple tie with England and Germany for Nobel awards in science, and since then we have been doing still better.

Another point worth making is that in many areas bridges between science and technology did exist in the United States during this period. These bridges existed in the various industrial and quasi-public laboratories mentioned in the preceding section, the agricultural experiment stations, and the like. The growth of industrial laboratories is perhaps particularly noteworthy. At the onset of the depression there were approximately 1,600 such organizations of various shapes and sizes. With a few notable exceptions, however, this activity was very much on the applied side, with little coupling to advancing areas in science.

Finally, it is worth pointing out that straightforward technological advances, whether brought about by science or by simple invention, were important in this era, but they were not the most conspicuous aspect of the American economy. First in importance were the management and organizational aspects of production. Mass-production techniques, based on time and motion studies, materials-flow studies, assembly line techniques, and so forth, were the dominant elements of the economy. They tended to lead to production techniques that were fairly similar even among industries producing quite different kinds of goods. We all remember how quickly American industry shifted from peacetime to wartime production under the urgencies of the two world wars.

TABLE 6.—*Invention rates in various countries*

	A Total on Selected List 1600-present	B Average Annual Rate 1930–39
United States	203	38, 300
Great Britain	58	9, 050
Germany	32	14, 600
France	29	9, 550
Italy	14	3, 900
Switzerland	....	3, 130
Sweden	4	1, 030

The years during and after World War II have seen a revolutionary change in American attitudes toward both science and technology. As we all know, they are now very much in the forefront of our thinking, and we aspire to a position of international leadership in these areas that is quite different from the one we had enjoyed earlier.

The shift in the position of the United States in pure science is exemplified by table 7, which gives, in comparison with table 5, a tabulation of Nobel Prize winners for the second 30 years of the Nobel Prize awards.

TABLE 7.—*Nobel Prizes in science, 1931–60*

United States	33	Austria	4
England	18	Sweden	2
Germany	14	Italy	2
Switzerland	5	U.S.S.R.	2

We see here that the United States now has taken a leading role, which would be still more marked if we confined the comparison to the years following the war. The sudden change may be attributed in part to the maturation of our own institutions, as noted earlier, and in part to a very substantial infusion of first-rate European scientists that took place because of the political troubles of the 1930's. As in the case of patents, however, the lead is in large part merely a reflection of our large population. We also benefit from our great material resources; the areas in which we lead tend to be those requiring relatively expensive experimental equipment rather than those that can be dominated by the paper-and-pencil theorist.

In technology, the precipitating cause of the change was clearly our military experience during World War II, confirmed and extended by the military and space program since. The application of relatively new science to military technology was obviously a vital element in many of our undertakings, while the vast scale and ambitious goals of such activities as the Manhattan Project, the ICBM program, and the current Apollo project have led the public to the belief, perhaps unwarranted, that "science" can do almost anything when pursued with sufficient determination and budgetary support.

The success of the Manhattan Project was particularly critical. During World War I, most Americans would have conceded leadership in science and the applications of science to Germany. But the success with the atomic weapon, added to superior performance in radar and other fields, gave the country a quite different view of itself and its potentialities. At the same time, the fact that such a large and complex undertaking could be organized and carried through to completion on a short time scale has lent encouragement to the planning of ambitious projects ever since. "Forced development" has become an established concept, particularly for military purposes. The fact that the Manhattan Project depended on a quite recent discovery in pure physics emphasized the importance of a close coupling between technology and pure science, to permit the rapid exploitation of scientific advances.

On the other hand, a somewhat more gradual evolution toward a science-based technology should probably have been expected even if the war had not broken out. As we have already seen, the pioneer industrial laboratories of the early 20th century were constantly finding more imitators. Moreover, after a long period of gestation, the scientific upheaval near the turn of the century was beginning to bear fruit in many fields. By the early 1930's, for example, it had carried us to the beginnings of modern solid-state physics, so important for contemporary electronics. In atomic theory, it had led us to the point where the basic discoveries about atomic energy could be made. In genetics, the work begun just after the turn of the century had already led by the 1930's to the introduction of commercial hybrid corn. The science that was beginning to emerge by the outbreak of the war had considerably more to offer technology than had its predecessors of a generation or two earlier. It was richer and more diversified, with greater depth of understanding and more lines of attack to offer in any given situation. It produced greater probability of turning up new and unexpected phenomena that might lay the foundation for substantial steps forward in technical method or even totally new areas in technology. Under such circumstances, it seems certain that competitive forces would have produced increasingly close ties between science and technology in any event.

The U.S. position in science and technology is, of course, an important factor in its general position of international leadership for many reasons. Both science and technology are directly involved in our military posture. In addition, technology, through its effects on our economic life, also acts indirectly to help provide the financial support both of our military establishment and of our foreign-aid policy. As we saw in the introduction, however, U.S. leadership in the long run cannot be based upon military strength or even upon foreign aid. It must depend on voluntary recognition by other nations of our culture as a healthy and successful one, worth emulating at least in many of its aspects. Thus our successes in science and technology, as conspicuous elements of our total culture, are important to achievement of leadership.

In reflecting on these questions it is natural to think first of technology. American goods flood the world's markets and carry with them the national image. The fact that they are generally well made is all in our favor. So also is the fact that they increasingly represent advanced technology—that jet transports and data-processing machines, for example, are typical American export items.

It is also important, however, to include leadership in basic science as a vital element in our position. In fact, in the underdeveloped countries, intellectual communication with science on a global scale may be better than it is with technology, if only because science is so internationalized. Thus, Nobel Prize winners in the United States are by no means a negligible factor in influencing the world to follow our lead. Cultural leaderships also helps us in other ways. The fact that foreign students come

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increasingly to the United States to be educated is a factor in our favor if we assume, as we must, that they generally return home as our friends. Cultural leadership also helps if it means that educated people all over the world must learn to read English because so much that is important appears in that language.

The struggle of cultures is, of course, one that the country must wage on a broad front. Science and technology, however, are important components. Expenditures in science and technology may be indirectly effective for our position of international leadership, in addition to their more direct values. Thus, such expenditures can properly be compared in value with the sums spent for information agencies, direct aid, or other similar means of strengthening our international position.

Changes in the Nature of Science

With this long background, we can now begin to ask what there is about contemporary science and technology that distinguishes them from the science and technology of past generations, and which poses particular problems for the present day. The writer can hardly claim to make a definitive statement about these questions, but will attempt only to offer some general remarks.

To turn first to science, it is clear that, while we live in a distinguished era, there have been very distinguished epochs of science in the past. In a certain qualitative sense, our era is probably no more distinguished than certain of its predecessors. Nevertheless, it far outstrips them in other respects. Science now is bigger; it covers more fields; there are more interconnections; there are more technical resources on which it can draw so that there are more avenues of useful exploration open to research at any one time. (Of course, there are also more research workers to exploit them.)

These changes seem in a sense to be merely a reflection of the growth in the body of science itself. As the body of science grows, there are relatively more and more interrelations, and in a sense one approaches "critical mass" just as one does as one brings nuclear material together in an atomic bomb.

There are several ways in which the effects of this gradual maturing of science can be discerned. The first is that, as any body of science becomes more thoroughly understood, it is possible to make more imaginative jumps into the unknown to explore more and more obscure effects. The better established the base camp, in other words, the longer the foray one can mount from it. In the early years of science, most of its material came through casual observation. It is only as theory becomes well established that one can make a very specialized and elaborate experiment with hope of success. The discovery of Neptune is the classic instance of a theoretical prediction that led to an experimental finding that otherwise would almost certainly have remained unknown, or at least unrecognized. The prediction of radio propagation by Maxwell, later verified by Hertz, which was referred to earlier, represents another such instance. As we feel more confident in our basic understanding, similar occasions in which one can commit a considerable experimental effort on the basis of a refined theoretical prediction become more and more common. For example, chemical purity in most substances is measured in hundredths or thousandths of a percentage point. In transistors and other semiconductors, on the other hand, the impurity level, at least for critical contaminants, must be of the order of millionths of a percentage point. Such purity is not found in nature or created by ordinary refining. Obviously, no one would undertake the job of finding ways of providing such pure materials without the sort of theoretical understanding that tells him that achievement of such purity would really provide a new kind of performance. Similarly, the modern maser will not "mase" unless conditions are just right. It takes an elaborate deductive process to see that a unique result may be possible if the required conditions are met. In another field, hybrid corn and similar modern plants are obtained from long programs of inbreeding and recrossing, which no practical plant breeder of the past, without the aid of modern genetic theory, would have had the confidence to undertake. Organic synthesis represents still another field in which elaborate and protracted experimental programs can be undertaken only because of the depth and completeness of our theoretical understanding.

A second general fact of our present situation is that, as science grows more mature, more and more areas lend themselves to successful scientific attack. Mathematics and physics, which are perhaps the oldest and best-established sciences, are so because in some ways they are simpler than most other sciences. The problems they confront could be clearly defined at an earlier period. For example, physics is simpler than chemistry. The theory of the atomic nucleus may be complex enough, but the number of kinds of organic molecules is far greater than the number of nuclear species. In turn, chemistry is surely simpler than biology, and biology than the behavioral sciences. Thus, the more complex disciplines necessarily lag behind the simpler ones, and are added one by one to the areas in which really active progress can be expected as time goes by. Lavoisier and Priestley, in displacing phlogiston by oxygen in the mid-18th century, were doing what Galileo was doing to the Aristotelian hypothesis about falling bodies near the beginning of the 17th century. Similarly, Dalton's atomic hypothesis near the beginning of the 19th century did for chemistry something like what Newton's laws of motion did for physics near the end of the 17th century.

More complex areas have had to wait still longer. In certain aspects of biology and the behavioral sciences, quantitative scientific progress perhaps dates from Karl Pearson, who is also one of the pioneers associated with the famous peak near 1900, which I spoke of before. The fact that more and more areas are becoming accessible to scientific investigation, more or less in the style of the physical sciences, is thus one of the outstanding features of our scientific time.

Another major trend is in the growth of interdisciplinary fields. These are fields that lie at the borderline between two disciplines and that make use of the ideas and methods of both. At one time science was becoming more and more fragmented, but the tide is clearly turning. Physical chemistry, the well-known border strip between physics and chemistry, has been joined by another field, chemical physics. Biochemistry has been a flourishing area for many years. Biophysics is much younger, but shows many signs of vitality. There is even a recognized field of biomathematics. This sort of junction, involving the massive importation of the basic concepts of one field into another, frequently yields enormous results. The importation of the ideas of modern physics, particularly in atomic theory and quantum physics, for example, has revolutionized the fundamentals of chemistry.

The final conspicuous aspect of our times is the massive employment of tools (as distinguished from concepts) drawn from one scientific field, or from engineering, in another. The most obvious example of a field that is almost completely dependent on other fields for its tools is astronomy. Ordinary visual observation of the stars goes back, of course, for many millennia, but progress beyond that point had to wait for the contribution of the telescope by physics. The next revolutionary tools for astronomers were photography and spectral analysis. Long time exposures, rather than visual observation, are, of course, the basic observational techniques that astronomy now uses in coping with the dim and distant parts of the universe. Spectral analysis, with its indications of chemical composition, relative motions, physical conditions of pressure, density, and other factors, has been almost equally important.

Spectrum analysis was included in the list of achievements in the golden age between 1859 and 1873 because of its unique value as a tool. In addition to its contribution to astronomy, it has been uniquely valuable to chemists, replacing many of their previous methods of analysis and giving them information not otherwise available. As a tool, it also served physicists themselves in many ways. For example, the long catalog of spectrum observations was the primary source material for the development of the modern theory of the atom.

The history of the use in astronomy of tools from other areas is by no means over. Modern control theory of the highest order is required to position big telescopes with sufficient exactness. Radio telescopes give astronomers a new way of charting the universe. If we succeed in putting a telescope into orbit or on the moon, astronomers will have a new observational tool, independent of the earth's atmosphere, of almost incalculable value.

The importance of this process, by which some of the sciences (or engineering) furnish tools for other sciences, is extremely marked at the present time. One has only to observe what radioactive tracers have meant to chemistry, the life sciences, and many forms of engineering process control, or what the discovery of naturally decaying elements—radio isotopes of various sorts—has meant in historical and geological dating, in periods running from the formation of the continents to the construction of your great grandfather's Windsor chair.

High-energy physics furnishes another example. With respect to experimental equipment, this field is really an exercise in advanced electrical engineering. It could not be attempted without the tools furnished by electrical engineering. Its huge experimental installations represent the classic example of the sort of large-scale expensive resource that scientists of the past did not ordinarily have, and that are indispensable for certain kinds of investigations.

The role of electronics and electronic instrumentation in many other areas is worthy of comment. Comparison of a relatively simple instrument like the electrocardiograph with the old-fashioned stethoscope shows how important even simple new tools can be. Automatic or quasi-automatic electronic instrumentation has revolutionized experimentation in many areas, permitting experimenters to take many times more data in a given time than they could only a few years ago.

While these examples are taken from the natural sciences, the impact of such new tools on psychology and the behavioral sciences is also very great. In many cases, moreover, the contribution is not limited to simple instrumentation. For example, the concepts of information theory, originally developed for communication engineering, have turned out to be unexpectedly fruitful in these fields.

A final example of the widespread application of new tools is furnished by the modern computer. In many cases it serves as a substitute for experiments that could be made only with difficulty, if at all. In others, it provides the only good way of coping with the enormous mass of data produced by the new instrumentation. In the planning of very elaborate experimental situations, involving substantial forays into unknown ground, it provides an almost indispensable way of investigating the situation thoroughly before one starts. In one or another of these ways the computer appears capable of substantially accelerating the rate of progress in many scientific areas.

An analogy between science and mining is a suggestive one here. In the beginning, science exploited nuggets and rich superficial pockets just as primitive man satisfied his small needs for metals. These most accessible deposits of metals were quickly used up, but vastly greater quantities of the world's ores still remained for exploitation through systematic utilization of large-scale equipment capable of exploiting poorer ores or of reaching rich deposits well below the surface. In science, the comparable era of systematic "mining" has just begun.

Since the basic topic of this paper is the relation between science and technology, one further remark may be in order. The instrumentation called for by modern science may be relatively simple, as in recording equipment, or very elaborate, as in space experiments. In all cases, however, it is properly defined as technology, and is the product of essentially technological effort. Thus, the increasing use of instrumentation in science is an important bridge between science and technology.

Some General Problems in the Relation between Science and Modern Technology

I have just given a brief account of some of the characteristics of modern science. Logically we should now continue with a similar discussion of modern technology. The discussion of science laid stress on the fact that modern science is increasingly likely to turn up new and unexpected phenomena, which might form the foundation for a substantial step forward in technical method, or even a whole new area in technology. Thus, we might expect the sketch of modern technology to show it in an attitude of passive, though attentive, waiting, ready to run off promptly with any new discovery and exploit it without further ado. If this simple picture were accurate, the application of science to technology would be a relatively straightforward matter, and could be easily discussed.

Unfortunately, the actual application of science to technology is considerably more difficult than this. It is likely, even under the best circumstances, to call for more initiative and active effort in technology than the simple picture would suggest. The subject is a very complicated one, however, because of the great variety of technical and economic situations in which applications of science may take place. An adequate treatment of all these possibilities would be well beyond the scope of this paper. For the sake of formal completeness, however, this section will give a brief sketch of a few of the questions that make the whole matter so complicated, and which must be borne in mind in any final appraisal of our overall progress in the application of science to technology. The logical thread of the paper, then, will be resumed in the following sections with a description of the characteristics of modern technology in limited areas where the interplay between science and technology is most apparent, and reasonably general statements can be made.

The most obvious reason for the complexity of the subject is the fact that the various sciences find their ways to eventual impact on the life of the society through quite different channels. For example, the physical sciences are most closely related to manufacturing and to certain portions of the service industries, such as transportation, electric power, and communications. Thus, a new discovery in the physical sciences is likely to find application, in general, through the methods available in these parts of our economy. The biological sciences, on the other hand, have only a tenuous relation with manufacturing, but they are very closely tied to agriculture and medicine. Our methods of capitalizing on scientific advances in these areas, however, are quite different from those in the typical manufacturing industry. The behavioral sciences represent still a different problem, with different channels for application, subject to different constraints.

To simplify our discussion we may restrict ourselves to the single field of manufacturing industry. With related areas in the service industries, as noted above, this still covers the bulk of the Nation's economy, including almost all our activities in military and space fields. This restriction, however, still leaves a great variety of possibilities. Table 8 illustrates the situation. It is an abbreviated form of a standard table giving research and development expenditures as a percentage of net sales in various sectors of manufacturing industry proper. The four industries—aerospace, electrical machinery and communications, chemicals, and scientific instruments—whose expenditures exceed the average for all manufacturing industry are listed at the top of the table. The rest of the table gives a few samples of the remaining industries whose research and development expenditures are below the overall average. The categories are, of course, quite broad. For example, "Chemicals" includes everything from bulk industrial chemicals to pharmaceuticals, and "Machinery" everything from construction machinery to office equipment.

TABLE 8.—*Research and development industry—1961*

	Sales $Billions	R&D $MILLIONS			R&D as Percent of Sales
		Federal	Company	Total [1]	
Aircraft and missiles	16.4	3,537	385	3,957	24.2
Electrical equipment and communications	23.1	1,533	861	2,404	10.4
Professional and scientific instruments	5.3	176	208	384	7.2
Chemicals	23.4	224	845	1,073	4.6
All manufacturing industry	246.0	6,313	4,480	10,872	4.4
Machinery	20.0	292	600	896	4.4
Motor vehicles and other transportation	28.0	802	609	192	2.9
Primary metals	20.0	16	143	160	.8
Food	35.0	4	101	105	.3

[1] Includes minor amounts from other sources.

Source: NSF 64-9.

It takes only a moment's inspection to see that the various industries differ quite widely in terms of research and development. For example, on the average, the first 4 industries spend about 10 times as much for research and development, in proportion to their sales, as do the others. The ratio between the aerospace industry and the food industry is about 80 to 1. Some of this disparity is, of course, a reflection of Government expenditures for military or space purposes. The differences, however, are substantial even when Government funds are subtracted.

The fact that an industry does not spend a large sum on research and development does not, of course, inevitably mean that it is not spending as much as its situation justifies. It is, however, one index to the flexibility and speed with which it is likely to respond to basic scientific advances. It is noteworthy that the four top entries in table 8 have all been recognized historically as "science-based" industries. The chemical and electrical industries, in particular, were pioneers in the establishment of industrial laboratories, and have long experience in the applicaton of new science to technology.

In the rest of this paper, we shall have tacitly in mind science-based industries of the sort found near the top of table 8, possibly excluding bulk chemicals. These are the areas in which the impact of a scientific advance on technology is most clearly evident, and in which the interaction between science and technology occurs under the most favorable circumstances. They are, of course, also critical areas for our defense effort.

The restriction to the science-based industries can perhaps also be justified in part by the argument that they are areas of particular interest for the United States' international position. This is obviously true in the military sphere. It also tends to hold for our normal export trade, since the more technological industries seem to be increasingly the arenas in which advanced industrial nations now compete. As we saw earlier, the most significant exports of the United States are now jet transports and data-processing machines, rather than, say, wheat and typewriters, and ships and transistor radios have tended to replace cotton goods as exports of the Japanese.

The science-based industries are also of special importance because of their indirect impact on the rest of domestic industry. To illustrate, if we look at a random sample of current technological advances in industry broadly, we will probably find that many of them stemmed ultimately from improvements in materials. Thus, they are likely to have been contributed by the chemical industry. A good example is furnished by the use of synthetic materials in the textile industry. In mechanical engineering one is likely to encounter techniques like explosive forming or light-weight actuator and control systems originated or perfected for aerospace applications. The random sample is also likely to include a number of examples of automation, in the form of either automated processing in the factory or advanced bookkeeping and data handling in the office. In either case, there is a fair chance that the advance rests ultimately on modern electronics, perhaps with an assist from the instrumentation industry. The machinery industry—the fifth entry in the list of table 8—is also one whose effect on the technological progress of other industries may be profound, because it creates so many of the tools they use.

To carry this discussion further, we need to turn now to some of the difficulties that may present themselves whenever we consider an actual application of science to technology. These obstacles may arise for the science-based industries as well as the others, and indeed the discussion is intended primarily as a preliminary for the sketch of these industries in the following section. It is included here, however, because it also helps, indirectly, to explain why one must expect such varied results in different areas.

From the technological side, the principal difficulty arises from the fact that many scientific advances are, in a sense, incomplete. They point the way to an advance in technology, in other words, but a great deal more must be supplied both in science and engineering before the step can actually be taken. Frequently the additional advances must be drawn from many fields. The classic example is furnished by the Manhattan Project referred to in an earlier section. Nuclear energy is, of course, based on discoveries in pure physics made even before the war. To make a successful bomb, however, also required substantial contributions from chemistry and chemical engineering, electronics, and mathematics, as well as much more work in physics. The further development of the atomic-energy field has, of course, required continued work in all these technical areas, and more.

The Manhattan Project is an extreme case, but similar situations on a more modest scale are quite common. Further examples are given in the next section. In some cases one must simply wait for concurrent advances in several fields. In others, it may be possible to fill the gaps, but only at the cost of considerable additional effort. When we deal with scientific advances less revolutionary than nuclear energy, the scope and difficulty of this additional effort may overshadow the scientific advance itself, so that the real choice is between expending the effort to exploit one scientific advance or another. Expenditure for applied research rather than pure research, in other words, turns out to be the critical factor (*2*).

Other problems are economic in nature. For example, it is obviously necessary for a technological advance to be justified in terms of the new investment in physical capital than it may require. Again, the Manhattan Project, with its very large expenditures for the Oak Ridge plant and similar facilities, is a good, if extreme, illustration. In normal industry, the weight of such a consideration may depend on the particular activity involved. For example, the question may not be a difficult one in the pharmaceutical industry, where the shift from one drug to another of related sort can be made readily, using substantially the same productive equipment. It is, however, obviously more important in capital-intensive areas such as transportation, power, and communications, mentioned earlier, where capital charges are high and physical equipment may last for many years. Problems of this sort are made much more serious by a high rate of technological progress. When technological progress is slow, it may be sufficient to incorporate technical advances in equipment that needs renewal anyway. As we postulate increasing rates of technical growth, so that technological obsolescence comes more and more before natural wearout, however, this solution becomes less satisfactory. We must debit technical advance with the value of the scrapped equipment.

A related question has to do with the possible incompatibility between a proposed technological advance and the general usages and organization of an industry. This may cover a broad field, including such technically irrelevant considerations as specific building codes, labor union requirements, national distribution networks, and the like. In utilities and transportation it may include such things as standardized hardware and operating practices. In manufacturing industry, it includes the general management and organization of the productive process. The automobile industry, with its elaborate network of suppliers, assembly plants, and service facilities, is an example. Whatever the area, an effective and smooth-working economic organization is likely to be as important for the ultimate efficiency of the system as the basic technology itself. If a technological change is really to be an advance, we must find some way of introducing it without too much disruption of the going system.

The overall competitive structure of an industry is evidently important in all such questions. Broad innovations, whether in usage or in basic technology, are likely to be expensive, and any individual company engaged in such projects is necessarily working, in part, for the benefit of its industry as a whole. Thus the more fragmented and competitive an industry is, the more likely it is that technological advances will be of the limited and specific sort that small concerns, under high competitive pressure, can afford.

All these considerations point in the same general direction; that is, that industry cannot always take advantage of isolated scientific advances as they occur. Exploitation of an advance must be reconciled with all the facts of life concerning capital obsolescence, existing production organization, and so on. In general, technological advance should be easiest in new industries, just as modern cities tend to grow in suburban zones rather than in central areas. It is also easiest when the advance has a limited and specific impact, like the substitution of a new drug, or a new measuring instrument, or a new finish, for an old one, and does not call for a long series of interrelated changes.

When these conditions are not met, we must expect that technological advances may take place in rather large quantum steps, after the accumulation of technological possibilities have provided us with the potential for really significant progress. The "activation energy," in other words, may be quite high. This does not mean that the benefits of scientific progress are lost—merely that they are postponed. To carry through such large changes in an orderly way, however, obviously calls for good planning and the assurance of adequate financial and organizational support.

A final point may be worth mentioning. The application of science to technology does not happen of itself; it has to be brought about through some agency. In many cases, the industrial laboratory type of organization seems to be the natural means. On the other hand, it is not clear that the industrial laboratory fits all situations. To flourish, such a laboratory should be fairly large, with a technical mission that is well defined and yet broad enough to maintain a diversified intellectual atmosphere, with stable financial support, and without overwhelming competitive pressures. These conditions may not be met in all circumstances. Alternative solutions such as trade associations, research institutes, and Government-sponsored laboratories come readily to mind, and evidently need to be considered in any complete survey. They are, however, beyond the scope of this paper.

Changes in the Nature of Technology

We saw in the preceding section that modern technology as a whole is too heterogeneous to admit any simple description. However, if we confine our attention to the science-based industries near the top of the list in table 8, it is possible to paint a reasonably coherent picture.

The science-based industries naturally reflect, to some extent, the characteristics we have previously ascribed to modern science itself. For example, as modern science is becoming interdisciplinary, we may expect its applications to be even more interdisciplinary. Thus in many technological situations we may need substantial teams of scientists and engineers to encompass the required skills. As experimental precedures in a single science are increasingly likely to depend upon a mixture of tools and methods borrowed from other sciences, we can expect a corresponding hybridization of tools and methods in technology. The fact that science frequently takes long steps forward nowadays has its counterpart in the fact that technological projects are frequently quite ambitious. When we look at these characteristics from the technological side, however, they tend to have a different flavor. In addition, we find that contemporary technology has some distinctive aspects of its own.

We can describe the characteristics of the science-based industries most conveniently by contrasting them with the traditional view of the American economy. This was, briefly, that America owed her prosperity in part to the skill of her people and in part to her natural resources. The natural centers of industry were near coal and iron deposits—the principal raw materials—and near good shipping, because of the importance of cheap transportation of bulk products. This meant primarily the northeastern part of the United States, as it meant England and the adjacent parts of the continent for European industry. Mass production of standard items, usually consumer goods, by well-standardized production techniques was the most conspicuous achievement of American industry, and provides a yardstick against which we can set the science-based industries.

It takes only a moment's thought to realize that the traditional picture has very little relevance to much of our present economy. It does not, for example, explain the booming city of Phoenix, Ariz., which has no coal, no iron, no water transportation. Phoenix exists for other reasons. It makes high-value products, based principally on electronics. People like to live there, and transportation requirements for such high-value articles are of minimal importance. The specialized production techniques required by the industry can be practiced there as well as anywhere else. Very similar statements could be made for most other science-based industries. They are located where they are for a variety of reasons—tradition, industry centralization, congenial surroundings, intellectual atmosphere—but seldom on account of any very compelling physical considerations.

We have laid stress on these geographical considerations because they bear so directly on the House committee's concern with the United States' position of international leadership. In the traditional view, we think of the economic and industrial position of the United States as a reflection in part of her generous endowment of natural resources. Obviously, in such highly technological areas, these considerations no longer apply. If an electronics industry can take root in Phoenix, it can take root in Libya, the Congo, Tokyo, Bombay, or anywhere else, so far as objective physical factors are concerned. Thus, the position of the United States in such fields depends entirely on the technical and management skills of its people.

Another contrast with the traditional view becomes apparent when we turn to the details of production processes. Typically, in the science-based industries, one deals with relatively small quantities of (sometimes) very expensive raw materials, and subjects them to a complex fabrication process. The problems of materials and parts handling, production flow, and so forth, which are so conspicuous in normal mass production, are of little importance. Instead, one is concerned primarily with very elaborate process control for a wide range of processes. The "clean room" and the instrumentation center are standard equipment.

The complexity of these processes can be illustrated by a simple look at the range of materials involved in typical cases. The aerospace industry, for example, must deal with a considerable variety of structural materials in addition to the conventional steel and aluminum, for reasons connected with weight, high-temperature strength, dimensional stability, and other qualities. The fact that beryllium, let us say, is perhaps a thousand times as expensive as steel, and raises metallurgical and fabrication problems all its own, is not decisive when beryllium solves a critical problem. Similarly, it turns out that the communications-electronics industry makes purposeful use, in one way or another, of at least half of all the chemical elements, including a number that were little more than chemical curiosities a generation ago. In some cases they are used only in trace amounts, but the traces are important.

The discussion of science given in an earlier section made the point that in relying more heavily on instrumentation and other equipment, experimental science was in some ways drawing more closely to technology. Similarly, in dealing with comparatively small quantities of material in very elaborate and carefully controlled ways, technology begins to take on some of the character of experimental science. As in many areas of experimental science, advances frequently turn on the discovery of clever techniques to deal with hitherto elusive or intractable phenomena.

The development of such sophisticated technologies carries with it two implications worth noticing. First, as such technologies become more specialized and more difficult, they become less and less accessible to traditional mass-production techniques. Thus it is necessary to cultivate them directly at whatever level the country's interests demand. This is especially important because of their significance for defense. One estimate (*3*) puts the fraction of defense material needs currently requiring special-purpose production facilities as high as 90 percent. This is in contrast to the situation in World Wars I and II, where half or three-quarters of military equipment was essentially peacetime goods and most of the rest could be obtained from converted peacetime equipment. It obviously implies that we are not likely to win future wars by World War II's "production miracle."

The other implication has to do with the kind of effort required to develop such technologies in the first place. We took note in the last section of the large and varied technical effort required, in the Manhattan Project, to go from the initial discovery of nuclear fission to a workable bomb. On a more modest scale, a similar history occurs repeatedly in the science-based industries. A good example is furnished by the development of the transistor. The transistor itself arose from a deliberate and aggressive effort to exploit the new field of solid-state physics. To support the work, however, one needed competence in a variety of related areas. Crystallography was one, for example, since crystal structure is basic to the solid state. Methods of growing very large and nearly perfect single crystals was another, since without such units one could not easily know what he was doing. Since very minute impurities of the order of 1 in 100 million have appreciable effects in semiconductors, a separate investigation of the means of making substances to such extraordinary degrees of purity was needed. Once the basic transistor discovery was made, it was still necessary to put it to practical application by aggressive specialized work in many fields. This involved investigations of production techniques for making the necessary materials, for protecting the units, for developing circuit designs and auxiliary devices to permit their particular properties to be used successfully in circuits, and so on. As in the Manhattan Project, the work on supporting technology was not over when the first transistor was operated, or the first bomb exploded. A continuing effort on a wide range of supporting technology has been necessary to carry both fields forward.

A somewhat similar situation exists in the aerospace field. A ballistic missile, for example, is a complex of systems designed to give structural strength, heat shielding, guidance and control, and propulsion. All of these are supported by complex technologies, and the history of advance in the missile field has been largely a history of successive improvements in these technologies. In some cases this effort has been a matter of straightforward engineering, but in others the problems have been so difficult and have required such frequent return to first principles that they are best characterized as "applied research." Military considerations almost always lead in the long run to a large premium for moderate increases in weapon performance. In addition, rocket performance tends in any case to be abnormally sensitive to small changes, especially in propulsion and weight allowances. The inevitable result is great pressure to achieve the highest possible refinement of technology in every aspect of the design.

An industrial laboratory, or similar organization, is the natural means to carry forward the sort of technological effort described in this section. Such a laboratory, if it is of adequate size, can maintain the necessary array of skills to attack either new or old technologies on a broad front, including interdisciplinary problems as they arise. It can also offer the scientific perspective to see what most needs working on in new and comparatively ill-defined areas. Such a laboratory is also able to maintain a close coupling with the world of science, particularly if it is in a position to do a reasonable amount of basic research itself. Thus, it is in a particularly good position to recognize a relevant scientific advance, and perhaps to supply promptly the many bits and pieces that may be called for if the advance is to be turned to practical account.

The fact that so many industrial laboratories find it to their advantage to dedicate a significant fraction of their total effort to basic research (10 percent or so is a representative figure for the larger laboratories) is of special interest for the purposes of this paper. It evidently implies the desirability of very close coupling between basic science and technology in the modern world. The days when the United States could subsist on imported science and homegrown technology are well behind us.

Complexity and Small Numbers

There is one other important way in which the science-based industries tend to differ from the traditional mass-production operations. It has been reserved for a separate section because it leads naturally into a discussion of some related questions concerning large single technological projects. The traditional American economy was an ever-expanding one in which the number of units of any sort increased steadily. This is what "mass production" meant. In consumer goods this still tends to be true. If we look in other areas, however, we find that technological advance often goes in another direction. There is a tendency for greater outputs to be obtained from increase in the size and complexity of individual units and the improvement of utilization factors. Thus, actual numbers of individual units may remain about constant or even diminish.

Most of us are accustomed to the fact that construction machinery, such as earth-moving machinery, seems to grow larger every year. A few large, if expensive, units are more efficient than a larger number of small units. A similar tendency appears to hold in the industries we are talking about here, except that the growth is now not primarily in gross physical size but in speed and in complexity and refinement of design. For example, the total computer capacity of the country has been growing recently by a large factor each year. The growth, however, appears to be due primarily not to increased numbers of computers but to great increases in the speed and working capability of the most advanced types. (A very fast computer must be quite small in overall dimensions, for basic physical reasons, so that working capacity tends to increase, in fact, through the use of larger and larger numbers of very small, but highly refined, components.)

In the long-distance communications before the war, the maximum traffic that could be supported by a pair of conductors was about a dozen simultaneous conversations, or perhaps a few hundred for a cable full of conductors. The corresponding figure that the art would support now is of the order of three or four thousand for the pair of conductors, or some tens of thousands for a complete cable. Of course, both the cable and conductors and the supporting electronic equipment would be much more elaborate, but the overall system nevertheless shows a net gain in efficiency. A comparison of the numbers and performance of our present fleet of B–52's with our World War II B–29's shows the same trend in the military sphere.

The most familiar example, however, is in air transport. In the mid-1930's, the DC–3 was the bellwether of transport planes. The total production, including a very large military production during World War II, was about 11,000 (*4*). After the war these planes continued in use for short flights (about 1,400 are still in service). Their long-distance flights, however, were gradually taken over by larger four-engine planes typified by DC–6's and Constellations. A thousand or so such planes were made in the decade after the war. This seems a relatively small number, considering the great increase in the amount of air traffic. However, each plane could carry several times as many passengers as the DC–3, and was also substantially faster, so that it could provide at least five or six times as many passenger miles per day.

In recent years these propeller-driven planes have, in turn, been displaced for long-distance flights by the modern subsonic jets. This has again entailed a reduction in total numbers. There are about five or six hundred jets of the longest-range types in the country, but each is roughly twice as large and twice as fast as the DC–6's and Constellations, so that they represent considerably more traffic capacity. The supersonic transport plane, if it arrives, will provide a further step in the same direction. It will be still larger, and, of course, significantly faster than a subsonic jet. One estimate places the world market for such planes as low as 200.

Such trends as these have several consequences. With so small a number of articles to be built, the approach to the production problem obviously must differ greatly from the traditional mass-production tack. In some areas, such as electronics, design ingenuity allows us to reclaim some of the savings of quantity production through designs that consist largely of replications of standard subassemblies, and can be automated. However, in other areas, preparation for production consists largely of the preparation of appropriate jigs and patterns. The numbers involved are too small to make it worth while to automate the actual process of fabrication.

Another implication is more important for the purposes of the present inquiry. Obviously, many more engineering hours must be put into the design of these very complex items than were required for their simpler predecessors. As the number of produced items dwindles, however, this greater engineering investment must be supported by smaller and smaller numbers of finished products. Thus, the cost relation between production and engineering development moves farther and farther from the proportions that obtained historically. In some areas of the aerospace industry, for example, there are now supposed to be only five production workers for each research and development employee. In the past, a ratio more like 50 to 1 would have been expected, even in such technologically advanced areas. Where these technological trends obtain, we must expect development costs to be a substantial fraction of overall production costs, and not a meager fraction as they have normally been in the past. This also has an indirect bearing on the desirable scale of support for basic research. It means that when there is a reasonable prospect that the research will benefit development efforts significantly, we are justified in a higher level of activity than we might have been willing to contemplate in the past.

A final point is the fact that the total development bill must be paid before even one unit is available. There is no way of "easing into" the situation. One cannot test the design or the market in a small way. We saw in the last section that the replacement of physical capital by technologically more advanced equipment could be expected to take place in quantum steps, after an accumulation of research results had provided such a large potential for improvement that the costs of replacing still-usable equipment could readily be borne. Obviously, large development charges represent an influence that tends to make the quantum step still larger. In some instances the estimated rate of technical advance may also enter the calculation. In other words, one may elect to defer an attractive development for a few years in the hope of doing still better later on. This makes the quantum step larger yet.

The importance of accumulating an adequate base of new science and technology before proceeding with a development shows itself with particular clarity in systems that depend on advanced technology in several different areas. An example is furnished by a communications satellite like Telstar or Relay. This is cited in one of the other papers as a technological advance that depended on a totally unexpected research result—in this case the discovery of the maser. It is true that the maser, though not quite indispensable, is a very effective contributor to the efficiency of the satellite system. On the other hand, a number of other elements are also important. For example, solar batteries, as the source of power for the electronics in the vehicle, are critical elements. They were also an unexpected result from research, this time from semiconductor physics. Similarly, solid-state circuitry itself is also indispensable, as are many of the technical modulation schemes and other specifically communications aspects of the system. The dependence of the system upon advances in launching rockets is, of course, also obvious.

The success of such a project thus depends on the existence of a full storehouse of interrelated techniques on which to draw. The depth and adequacy of the supporting technology are what count. In this respect, the communications satellite, or the supersonic transport, are at least broadly akin to the atomic bomb and the transistor, which we discussed earlier.

On the other hand, even when such a storehouse exists there may be many remaining problems. The formulation of a suitable system, with the right combination of elements and the best "trade-offs" among the various aspects of the design, may itself be an engineering challenge of considerable difficulty. In a commercial situation, this planning or "systems-engineering" phase must include all the factors of cost, timing, probable markets, and other factors mentioned in the air-transport discussion. Moreover, the full storehouse is a somewhat elastic concept. In normal application it is likely to mean that, while technical approaches to all aspects of the plan are known, we will not always have had actual experience with them on the scale or to the accuracy desired. This, in turn, may call for an estimate of the real degree of technological risk in each aspect of the design, and provision for parallel or alternative courses of action where necessary. This initial planning stage is a much more difficult as well as a much more important activity than it was in earlier times, when technology took less bold steps. It seems impossible to overdo either the quality or the amount of the effort that ought, in principle, to be assigned to it.

This discussion has been at such length because the questions of technical planning and management it raises apply broadly to a whole class of ambitious technical projects. These are, moreover, projects that are especially likely to require Federal support; thus they are particularly relevant to this inquiry. Examples are found in the military and space fields, in "big science," and in various proposals for new types of metropolitan transportation systems, water management, and the like. In detail, projects in these various areas are likely to raise different sorts of questions. In the military area, for example, one is likely to be concerned with balancing technological risk against schedules and military performance; in "big science" one is, or should be, concerned with the trade-off between engineering costs and scientific results; in the other proposals one may be concerned with a whole host of social and economic considerations. However, the basic questions concerning the justification for undertaking a project at a given time, or at all (as illustrated by the supersonic transport)—the adequacy of the technical base and the technical plan erected on it (as illustrated by the communications satellite), and finally the skill of the organization to carry the project through to completion—arise in all fields and are often not easy to answer.

This leads to the final point of this paper. The advance of science and technology often makes the job of technical planning and management much more critical as well as more difficult than it has normally been thought to be in the past. There are two general reasons for this:

We are likely to be working in new and relatively unexplored technical areas, where one must make adequate judgments of probable performance and reliability, and of the best ways of realizing them, on the basis of skimpy or nonexistent experience. In addition, we may be dealing with complex and elaborate systems involving a maze of economic and technical considerations, all of which must be thought through in detail and brought into harmonious accord if the outcome is to be successful. Particularly in evaluating complex new projects, these planning and management factors, as well as the intrinsic desirability of the final result, need to be considered.

Conclusion

This discussion is intended primarily to be a background for the other papers of this collection rather than a direct response to the House committee's questions directly. To give the paper as much point as possible, however, it seems advisable to add a few words to emphasize points that appear to have at least broad relevance to the issues raised by the House committee.

The first question of the House committee has to do with the appropriate scale of support for basic research. I have not dealt with this question directly. Some incidental remarks about the contribution of scientific success to the international prestige of the United States provide an indirect measure, by inviting comparison with other expenditures to improve our prestige. My remarks on the general rise in research and development costs as a fraction of total production costs in certain areas may also be relevant. In my opinion, there is not much danger that first-class scientific work in the country will be oversupported. No doubt, one would always want to be a bit chary about second-rate work. However, a monetary ceiling on research support does not seem to be a very satisfactory approach to such a problem. It calls instead for internal policing by the scientific community. Any real consideration of such questions must, of course, allow for the fact that research is done in government and industrial laboratories, as well as at universities, and problems of motivation or mission definition, standards, financial support, and leadership may be very different in these various settings.

The Committee's second question, on the areas of science that most deserve support, is not answered very directly either. However, the section on the characteristics of modern science does have some indirect bearing on it. Obviously, the most inviting areas tend to be those in which science demonstrates that it can make the most progress. Thus, the remarks in that section on the gradual diffusion of science into more complex and less tractable fields, on the role of cross-fertilization between scientific fields, and the use of new methods and new instruments in individual fields, are all indicators of potentially rewarding areas for support.

It may be helpful also to recall some of the usual canons of quality in scientific research. Novelty is certainly one; the importance of discovering a new and unpredicted phenomenon requires no argument. Beyond this, the principal criterion can perhaps be described as a sort of intellectual efficiency in getting a great understanding of, and command over, nature for a small price. The generality of the result and the perspective it sheds on a wide range of situations, in other words, are important indices. The fascinating feature of Newton's law of gravitation was the fact that it applied to every particle of the universe. Such a formulation does not rule out the systematic experimental work that constitutes the backbone of science, but it is almost the opposite of defining research as the mere satisfaction of idle curiosity or the indiscriminate heaping up of disjointed facts, without pattern or purpose.

Such criteria as these are too general for routine administrative evaluation of individual projects. Scientific judgment is still required. They do, however, suggest some broad emphases. For example, the criterion of novelty tends to strengthen the role of the individual worker as against the role of "big science." The big scientific project usually exists in the first place only because we think we can foresee, at least dimly, what its results may be. Of course, it may yield something quite new that could not be found by any other means. However, a large fraction of the totally unpredictable discoveries, which furnish the starting point for later important fields, are still made by individuals. Thus, while modern physics advances through multi-million-dollar high-energy machines, it also advances through phonograph turntables in the hands of a Mossbauer. In a copious era, synthesizing and integrating concepts are of particular importance if generality of result is to be achieved. Thus, the same set of criteria suggests special support for areas of work (usually theoretical) that are promising here.

The bulk of my paper, however, is directed not to science but to technology, or rather to the interaction between science and technology. In summary, I have tried to show that, until comparatively recent years, science and technology pursued essentially independent courses. Advances in science affected technology only gradually, and, one might almost say, accidentally. There were only a few areas, such as the pioneer industrial laboratories, and public health or medical and agricultural laboratories or experimental stations, in which the applications of science were pursued in a systematic and determined way.

The deliberate application of science to technology on a broad scale is primarily a phenomenon of the war and post-war years. The change has come about partly because science now has more to offer than it ever had before. It is also due in large part to the fact that the public, principally because of wartime experience, now accepts the idea that science is applicable to technology, and looks to such applications as a mainspring for progress.

Since the deliberate application of science to technology in most areas of the country's life is so new, it may not be surprising that it presents a number of problems. The discussion in the section on the characteristics of modern technology, and in particular the later discussion of the problems of technical planning and management of complex projects, were intended to illustrate some of these. Perhaps it is also not surprising that the results in various areas of our society turn out to be uneven.

It is the major conclusion of this paper that these difficulties and unevennesses in the transition from science to technology represent the most critical aspect of the issues raised by the questions of the House committee—the aspect that most needs attention if the country is to maximize the yield from its investment in basic science. There are several ways in which we can think of bettering the situation. For example, since the application of science to technology does not come about all by itself, one way is through a consideration of the possible agencies—industrial laboratory, Government laboratory, or whatnot—that may do the job. The principles of operation that will make any one of these agencies actually successful in its appropriate setting, however, present an important problem in their own right, which goes well beyond the scope of this paper.

Another means of improvement relates to the people required. In my description of technology, I emphasized the need for applied research and for careful advance planning in new areas. The men one would like for such work are easy to describe. They should obviously be highly trained. To work well in interdisciplinary situations, of which they can expect a great many, they should be well enough and broadly enough acquainted with science to understand its structure and motivation. Since they will be close to the research frontier in many cases, they also need direct experience with research. In this respect they should be on the same footing as the typical pure scientist. At the same time, one should surely want men who also have enough engineering background and interest to operate effectively in essentially engineering situations.

Whether or not this ideal is often realized, the country is dependent on the universities for a sufficient supply of reasonable approximations to it, as well as for its supply of pure scientists (*5*). Federal support of research in universities is one of the principal means of assuring an adequate flow of new scientists, and, in gauging the level of support, it is important that needs of both sorts be understood.

A third approach is simply to recognize how much science and technology actually have in common. The descriptions of modern science and technology given in this paper were intended to stress some of these resemblances, including in particular the growing interdependence of the two areas in methods and techniques. In addition to methods, the two fields, when seen from a distance, seem to be growing together in everyday outlook. As we noted a few pages back, the objective of scientific research can be broadly described as understanding nature in an intellectually efficient manner. Similarly, the objective of technology can be described as control of the natural world to bring about prescribed results. But, in the context in which advanced technology now finds itself, understanding and control are inseparable. In very many cases the first and most crucial step is simply to establish a sufficiently thorough and fundamental understanding of a situation by procedures similar to those of pure science. Seen in this perspective, technology appears as a natural extension of science rather than as something essentially different.

References

(*1*) Listed are the numbers of patents issued by countries to their own nationals. In European countries, especially, the gross rate of patenting is frequently much greater because of the large number of patents issued to "foreigners" (frequently Americans) who wish to secure patent coverage in countries other than their own. Source: Publication No. 17 of the Subcommittee on Patents, Trademarks and Copyrights, U.S. Senate Committee on the Judiciary, December 1958. The U.S.S.R. is not included in table 6 for lack of comparable data.

(*2*) This is not intended to minimize the ultimate importance of pure research, since the effectiveness of the applied research effort may itself depend on the storehouse of information on which it can draw. It says, however, that in evaluating basic research we should lay stress on broad increases in understanding rather than on specific advances.

(*3*) "New Realities and Old Concepts" by Dr. Peter F. Drucker, *The General Electric Defense Quarterly,* vol. 2, No. 1, January–March, 1959.

(*4*) The number of planes that actually entered commercial service is no doubt very much smaller, but exact figures are not readily at hand. The number given does not include several thousand additional planes produced in other countries. Also omitted from the general discussion, for the sake of simplicity, are designs like the DC–4, which was the actual immediate successor of the DC–3, as well as designs by other makers, which should be included in any comprehensive account.

(*5*) In fact, of course, the two categories are not entirely separate, since many pure scientists eventually migrate into applied fields, thus automatically establishing a bridge between pure science and technology. This, however, does not affect the total number of people needed, although it may imply the desirability of giving more emphasis to breadth of training than is customarily given in modern scientific education.

FUTURE NEEDS FOR THE SUPPORT OF BASIC RESEARCH

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Introduction

The two questions posed by the House committee are exceedingly difficult to answer in any precise quantitative way. The general approach taken by this paper is that the answers can only be arrived at by successive approximations. We thus try to suggest some of the considerations and some of the mechanisms of choice that ought to be considered in determining levels of support for science.

I begin my paper discussing some of the problems involved in interpreting research and development statistics. Since current statistics must provide the basis for any future planning for science it is important that the limitations of these statistics be fully understood.

The second section deals with some of the reasons why the support of basic research is considered to be in the national interest, and why this support must be primarily a Federal responsibility. In this section we suggest some possible guidelines for future overall support of academic research.

In section III a conceptual scheme for considering the "science budget" is suggested. This involves an attempt to separate the requirements of big science from those of the individual investigator in the university. It suggests that the problem of relative allocation to fields is not one to be centrally determined, but rather a question of setting up suitable mechanisms for continuing decentralized choice. This section is concerned mainly with academic research.

The fourth section attempts to describe the difference between academic research and organized institutional research, and to explain the different mechanisms of choice and criteria that should apply to the latter as compared with the former.

I. *Some Remarks on Research and Development Statistics*

Since much current discussion of Federal spending on science is based on financial and manpower statistics, it is important that the meaning and limitations of these statistics be fully understood. A recent report of the Organization of Economic Cooperation and Development has remarked that most countries have better statistics on poultry production than they do on the activities of their scientists and engineers. To some extent this is inevitable since the product of scientific activity is an elusive entity that defies measurement. Especially in basic research we have nothing but historical analogy to go on in evaluating the worth of the product, and even in purely scientific terms the value of any given piece of work often does not become fully apparent until several years after it is published. In many cases an unsuccessful experiment may have more lasting value than a successful one. A classic example is the famous Michelson-Morley experiment, which failed to detect the absolute motion of the earth through space and led directly to Einstein's formulation of the theory of special relativity, but not until many years later.

As a result of these features research activity is not very amenable to the ordinary methods of economic analysis. We can measure the "inputs" in financial terms or in terms of "professional man-years" of effort, but we have no comparable currency in which to measure the "output." We can see the continuing growth of our economy as primarily a product of technical innovation, but until very recently little of this innovation was clearly connected with organized research and development. No striking acceleration of economic growth has accompanied the dramatic growth of organized research in recent years. This is not very surprising in view of the large average time lag between research discoveries and their application. On the other hand, the sectors of the economy showing the largest percentage growth rates are in many cases those most heavily dependent on modern research. All the advanced industrial countries devote about the same proportion of their national income to civilian research and development. Thus we have no "controls" by which we may judge what would have happened to economic growth if there had been no research and development, nor do we have a way of measuring the relative importance, economically speaking, of the research relative to the development. Indeed, there is no economic "payoff" from research until it is incorporated in some kind of product, service, or process, and this won't happen to current research results, for the most part, for many years. Thus research, and particularly basic research, is a speculative investment in the relatively long-term future; its economic payoff has a longer incubation time than any other form of investment, except possibly education.

On the other hand, there are certain things that can be said about the current economic benefits of technical proficiency to the United States. For one thing, this country has an exceedingly favorable balance of trade in "technical know-how," as measured by international payments for royalties, licensing agreements, and management fees. Such payments net nearly half a billion dollars a year, and payments to the United

States exceed, by a factor of nearly 5, payments from the United States to all other countries. As another example, analysis of our exports clearly reveals that the proportion of products from industries that may be classed as "research intensive" is very much higher in our export trade than it is in the gross national product as a whole, suggesting that it is the industries based on technical know-how that generally compete most effectively in world markets. Analysis of the exports of other advanced nations indicates a similar bias toward products and services based on research. On the other hand, one must interpret these figures with some caution, since they must relate primarily to technical advances that took place before the present high Federal investment in research and development, and since technical progress in Europe and Japan was heavily retarded by the effects of World War II and its aftermath.

Because of the absence of valid economic measures for the product or benefits of research and development we are forced to measure it essentially in terms of its economic inputs, with the implicit assumption that in some sense the output will be proportional to the input. In terms of inputs, one thing is clear: research and development probably constitute one of our fastest-expanding forms of economic activity. Nevertheless, one must regard statistics of the past with great caution. Even within a span of a few years, there has been a tendency to include more and more activities under the category of research and development that were formerly looked upon as part of production or design or, in the military field, procurement. A few years ago, as a result of a reorganization, the category of research and development in the Defense Department was changed to "research, development, test, and evalution." This placed the dividing line between development and procurement much further along in the weapons system cycle than had formerly been the case. Now sample production runs of weapons for evaluation, and the costs of expending them under simulated service conditions, are treated as part of research, development, test, and evaluation. Apart from this effect, which caused a discontinuous 20–30 percent jump in the apparent research and development budget of the Department of Defense, the general popularity of research and development probably resulted in a good deal of redefinition of many technical activities. Thus the growth of research and development in the last decade, while substantial, is probably not as rapid as indicated by the raw statistics.

On the other hand, there is much activity of a highly technical nature in the Federal Government which, while not classified as research and development, requires the participation and supervision of people with advanced technical training and experience. Many of the services performed by Government involve the collection of technical data on a more or less routine basis. Examples occur in weather forecasting, hydrographic and geological mapping, and collection of economic and population statistics. That the function of the Federal Government in our society is highly technical is indicated by the fact that nearly 50 percent of the professional civilian employees of the Federal Government are scientists, engineers, or health professionals, and the three highest grades of the civil service are even more heavily populated with people with technical backgrounds.

Similar problems arise when one talks about specific classes of activity, such as basic and applied research. In the first place, the motivations of the man who does research can, quite legitimately, be different from the man who supports it. In the second place, some basic research involves the design, construction, and operation of very large and complex equipment. The motivation for acquiring this equipment may be purely scientific, but much of the activity accompanying its design and use is indistinguishable from the more applied kinds of engineering or production. Thus, for example, in fiscal year 1964 the National Science Foundation reported a Federal investment of about $1.6 billion in basic research. It turns out that nearly half of this amount was spent by the National Aeronautics and Space Administration and that approximately 80 percent of the National Aeronautics and Space Administration expenditure was for the design and procurement of scientific space vehicles, the operation of tracking ranges, and payments to military missile ranges for putting the vehicles into orbit. A significant part of the oceanography budget goes into simply keeping research vessels at sea, without any science. The operation of a large particle accelerator requires annually something like 10 percent of its capital cost, or perhaps as much as 30 percent if one includes the cost of continued updating of the equipment. Similar figures can be quoted for large optical telescopes or arrays and "dishes" for radio astronomy. These are operating costs that are required simply to make a facility available, with no consideration of the additional costs of the actual science to be done.

Why is it necessary to stress these logistic costs of research? Since they are incurred for the purpose of achieving basic research results, they are legitimately chargeable to basic research. Nevertheless, the impression conveyed by statistics that include such supporting costs can be quite misleading. A basic research budget that rises annually by 15 percent may appear to be adequate or even generous, but if most of this cost increase is merely to ensure the availability of certain new facilities, then the increased budget could actually be supporting the activities of fewer scientists. The situation would be a little like building a new department store that was so expensive to keep open that it was necessary to fire all the salesmen. This is not an academic issue. Much of the planning for new research facilities that took place in fiscal years 1962 and 1963 was based on an implicit assumption of continuing expansion of research budgets. Now, in fiscal years 1964 and 1965, when these facilities are just coming into operation, the expenses of merely making them available—without any science—are confronting fixed or even declining operating budgets for basic research. The political embarrassment that would attend not using a facility already built makes it inevitable that the facilities are made available anyway, usually at the expense of the individual scientist who does not have large fixed costs. A recent calculation indicates that if the budget for oceanography continues to stay level, the cost of operating ships already planned but not yet completed will eventually consume almost the entire research budget. A similar situation appears to be developing in low-energy nuclear physics, and with respect to university computing facilities. In nuclear physics, for example, expenditures for facilities doubled between fiscal years 1962 and 1964, while operating expenditures increased only slightly and actually decreased in the university sector between 1963 and 1964. The point I am making is that simply to look at total budgets for basic research, or even their annual increments, can be highly misleading unless one knows something about the fixed availability costs that have been built into the program by past commitments for capital facilities. Where large availability costs are involved, the relation between research output and dollar input can be highly nonlinear, and hence measurements of basic research activity by dollar inputs can give a misleadingly reassuring impression as to the adequacy of support. Unfortunately, our present methods for collecting and classifying statistics on research expenditures are not sufficiently refined to reveal problems of this sort, or to draw clear-cut conclusions about the current situation. Subjective opinions of many individual scientists and research groups indicate that support for the individual investigator is becoming increasingly inadequate relative to his needs, but it is hard to prove this quantitatively, and even harder to establish that it is due to past commitments for facilities.

Classifications of research into basic and applied can also be misleading as to the type of manpower required. In the space example, a single experiment may involve the services of hundreds of technicians and skilled workmen, whereas only four or five scientists may be involved in the actual design of the instrumentation package and the analysis and interpretation of the data. The same amount of money in another field of research might finance the activities of 50 highly trained scientists. This issue is an important one because it is sometimes claimed that there is more money for basic research than the really competent people available to do it can spend wisely. This could be true, but I submit that it is a judgment that cannot be made in terms of total available funds, but only on a project-by-project basis. Two or three competent scientists can in some areas of research wisely command or direct the activities of a large number of less highly skilled people. In fact, one effect of increased research funds is that many scientists are able to buy from industry equipment that they would otherwise have to design and build themselves. The dollar input to their research is much larger than it would otherwise have been, but this does not necessarily mean that the research is more "expensive" if measured in terms of the research results obtained. The capital investment may not only enable the scientist to obtain more results for the same effort, but also may permit him to choose a much more significant problem or to obtain a much more conclusive answer. Just as capital investment embodying new technology improves the productivity of ordinary labor, so does it improve the productivity of scientific effort. Unfortunately, since it is the only thing that is quantifiable, there is a tendency to measure research in terms of man-years of effort or in terms of output of publishable papers. While the latter is certainly much more significant than the former, there is still too wide a variation in the information content and quality of scientific papers for paper publication to provide an accurate measure of research output.

Another statistic that is often quoted has to do with academic research. For a long time it was common practice to report only the total research and development support going into universities. However, in the postwar period many universities undertook the management of large applied laboratories or basic research institutes. Some of these, like Los Alamos, were remote from the campus and had no visible intellectual connection with the parent university. However, such clearcut cases were the exception; usually the relationship to the university was closer, as in the case of the Radiation Laboratory at Berkeley, the Cambridge electron accelerator, or even the Lincoln Laboratory at MIT. It has now become customary, however, to classify such organizations as federally financed research centers and exclude them in reporting the support of research in "universities proper." Nevertheless, there are many such organizations that employ faculty members part time and participate in the training of graduate students. Other organizations, such as the Brookhaven National Laboratory, the National Radio Astronomy Observatory, or the Kitt Peak National Astronomy Observatory, are not classified with universities at all, but nevertheless provide important facilities for university "user groups," including significant numbers of graduate students and faculty on temporary assignment. Conversely, there are some research activities within "universities proper" that are little more than research institutes with rather minimal intellectual connection with the rest of the university. The point here is that the line between "academic" and "nonacademic" research in universities—between universities proper and research centers—is not a sharp one if measured by involvement in the educational process. Yet, with respect to Federal research and development investment, the research centers account for something like 40 percent of all university research activity. With current emphasis on the connection between basic research and graduate education, there is a danger in completely eliminating the research center statistics from the overall picture, with the implication that the elimination or downgrading of such activities would have no effect on the educational function of the universities. In some cases this might well be so, but in others it would not be. There is equal hazard in the converse assumption that all the research funds going to universities proper are in support of graduate education and therefore required to maintain the quality of graduate training; unfortunately, we have discovered no quantitative way to measure the educational relevance of research funds.

Another statistic that may be misleading is the separation of Federal funds into contributions to "research" and "education." Thus, on the one hand, in reporting Federal research and development funds in universities proper, fellowship funds, research training grants, and certain types of institutional support are usually omitted, despite the fact that many of the individuals who receive stipends under such programs are actually engaged at least part time in research or in the supervision of student research. It is clear that a significant proportion of such funds contributes to the progress of research in universities. In the National Institutes of Health they amount to about 30 percent of all the funds contributed to universities, although they are less significant for other agencies. On the other hand, a very large proportion of the funds designated as "research" actually provide stipends for graduate students and postdoctoral research associates who, while engaged in research, are also receiving training. Indeed, since research experience is believed to be the most important and valuable part of advanced training in science, the separation between research and education funds is bound to be rather arbitrary and artificial.

Even the classification of research funds into Federal and non-Federal may be highly misleading. For example, procurement contracts in defense, space, and atomic energy permit business organizations to charge a small fraction of their independent research activity to procurement overhead and also allow technical work in connection with the preparation of development proposals, including unsuccessful proposals, as an overhead item. It has been estimated that the total funds channeled to industry in this way amount to close to $1 billion, about the same amount of money as flows from the Federal Government into universities proper for research, basic and applied. Yet this money is classified in the statistics as being financed by the private, not the public, sector. A good deal of private research is also financed out of the profits of military and space procurement. The proper classification of these activities is hard to decide. In the sense that the basic resource-allocation decisions are made in the private sector, regardless of the source of funds, the activity is correctly classified as private. On the other hand, the Government does exercise some surveillance over the expenditure of part of these funds.

Furthermore, the extent and scope of the activity is strongly conditioned by decisions in the public sector.

In considering research in the university sector it is often forgotten that, in practice, the salaries of faculty members engaged in research are paid largely by the university out of its own sources of funds, and are not a charge against Federal research and development budgets. This is in contrast to federally financed research centers and to research in private industry, where the Federal Government is routinely expected to bear the full costs. In addition, the universities make a major contribution in the form of unreimbursed indirect costs, estimated to exceed $60 million annually. In a sense that does not apply to any other sector to the same degree, the Federal contribution to university research is a contribution to a shared activity rather than procurement of a service at cost. Any increase in the Federal contribution to university research thus generally reflects an increased contribution from other sources as well.

In considering the totality of Federal research and development activities, there appears to be no unique way of breaking down expenditures into their significant components. Except possibly in the area of specific hardware development, most Federal research expenditures serve several purposes simultaneously, and most scientific activities relate to more than one traditional disciplinary categorization. The network of communications and organization in the technical community is so dynamic and complex that it is difficult to capture in a statistical snapshot at any one point in time, and even harder to characterize by fixed statistical categories over a period of time. In my personal view the most reliable and useful statistical categories are those that relate to institutional arrangements, such as universities, Federal research centers, and scientific departments or schools, rather than to such categories as basic and applied or to the various traditional scientific disciplines.

II. *Why Should the Federal Government Support Basic Research?*

The House committee has asked at what level basic research should be supported in order to maintain our present position of leadership. As background for answering this question it is necessary to inquire why the Federal Government should support basic research in the first place, and what functions basic research serves in our society.

One can recognize four distinct functions of basic research, some of which also pertain to certain types of applied research. They are: cultural, economic, social, and educational.

Cultural

Basic scientific research is recognized as one of the characteristic expressions of the highest aspirations of modern man. It bears much the same relation to contemporary civilization that the great artistic and philosophical creations of the Greeks did to theirs, or the great cathedrals did to medieval Europe. In a certain sense it not only serves the purposes of our society but *is* one of the purposes of our society. Science and technology together constitute the distinctive aspect of American culture that is most admired and imitated in the rest of the world, and I believe this admiration is connected with more than the economic and military power that derive from technology.

The attitude of the general public toward the space program suggests that this cultural aspect does enjoy a degree of public acceptance. While it is true that much of the public supports the space effort because it feels in a somewhat vague way that it is connected with military power, nevertheless there is a genuine sense of identification with the adventure of exploration into the unknown. To the scientists it may seem naïve that the public should identify the space program, especially the man-in-space program, with science. To many, but by no means all, scientists the relative emphasis on the lunar-landing program appears as a distortion of scientific priorities and of intellectual values. Is manned exploration of the near solar system really worth a thousand times as much as probing the secrets of distant galaxies or the dramatic and intriguing quasi-stellar energy sources? Nevertheless, public acceptance of the space program must be regarded as in some sense a vote of confidence in intellectual exploration as such and a recognition of the desirability of public support for such exploration. This recognition is, by itself, a new political phenomenon, and may represent only the first step toward a wider and more informed public recognition of the desirability of social support of intellectual exploration for its own sake.

Any statement of a cultural motivation for the support of basic research raises, of course, much more serious issues of political philosophy than the other motivations listed. Why basic science but not art, music, and literature? Why not research in the humanities? If we support science for cultural reasons, how can we tell how much is enough? I think the only definite answer that can be given to these questions lies in the nature of science as a system of acquiring and validating knowledge. Science—especially natural science—has a public character that is still lacking in other forms of knowledge. The results of scientific research have to stand the scrutiny of a large and critical scientific community, and after a time those that stand the test tend to be accepted by all literate mankind. Outside the scientific community itself this acceptance tends to be validated by the practical results of science. If it works it must be true. There is no question that the successful achievement of an atom bomb provided a certain intellectual validation for nuclear physics, quite apart from its practical value. Part of the public character of science results from the fact that it is always in principle subject to independent validation or verification. It is like paper money that can always be exchanged for gold or silver on demand. Just because everybody believes that he can get gold for paper, nobody tries; so the public seldom questions the findings of science, just because it believes that they can always be questioned and revalidated on demand. This is much less true of other forms of knowledge and culture, which may be of equal social importance but are more subjective and more dependent on the vagaries of private tastes and value systems. It is just because science is a cultural activity generally believed to transcend private value systems that it becomes eligible for government support where other forms of cultural activities are not. The system of indirect public support through tax exemption has been used in the United States successfully to support cultural activities in areas where there is no consensus of values or tastes. This is possible because, although public funds are used, actual decisions as to what will be supported are left in private hands. It may well be that this situation should be regarded as temporary. Direct Government support of other forms of cultural expression is generally accepted in advanced countries other than the United States.

The basic difficulty with the cultural motivation for Federal support of basic research is that it does not provide any basis for quantifying the amount of support required. The amount of basic research that should be supported for purely cultural reasons is certainly a fraction of what should be supported for other reasons. It is currently believed that the talent for really creative basic work in science is exceedingly rare. I believe there is a most creative minority, possibly not more than 5 percent of all the active basic research scientists, who should receive support for their work for no other reasons than their demonstrated capacity for original and creative work. This highly selected group of people might be provided with some minimum level of research support with no strings attached. They would simply be backed up to some level, say $20,000 to $30,000 per year, to work on anything they thought worth doing. If they needed more than this, then their requirements would have to be justified in competition with others in terms of their specific proposed work and for other than purely cultural reasons. I believe the Government could reasonably commit something of the order of $100 million (*1*) a year to this type of completely freewheeling research expenditure.

It must be remembered, however, that the work of this most creative group cannot be regarded as independent of the more run-of-the-mill kinds of research, as is sometimes implied. Important discoveries have sometimes been made by individuals who never did anything else of significance in their careers. The brilliant generalizations of giants often rest on the painstaking accumulation of data by less gifted individuals. The relative importance of brilliant and intuitive insight as compared with the more pedestrian hard work will vary from time to time with the circumstances of particular fields. For example, the progress of mathematics and theoretical physics is probably much more dependent on the insights of a few leaders of extraordinary ability than is the progress of experimental physics or chemistry. One cannot support only the geniuses and expect that science will continue to progress as though the workers in the vineyard were superfluous. However, it is certainly true that more than a merely cultural motivation should be required to justify the support of other than the few most highly gifted.

For the sake of its position of leadership it is essential that the Nation be prepared to invest heavily in equipment and facilities which place a few of its most talented groups at the "cutting edges" of modern scientific advance. No matter how talented the people, facilities that are second best are likely to leave them in the position of verifying exciting discoveries made by somebody else. The preeminence of the United States in nuclear physics owes much to the brilliance of its workers in this field, including many imported from other countries, but it owes even more to the superior equipment that generous Federal support, good planning, and high-class engineering have made possible. United States preeminence in many fields of science reflects not only the intellectual vigor of its scientists but also the excellence of its industrial base.

The United States has led the world in discoveries in optical astronomy almost since the turn of the century, and this is largely attributable to the foresight of some of the great private foundations that supported American astronomers in the construction of better instruments than existed anywhere else in the world. By contrast, in radio astronomy, despite a large investment, American instruments are inferior to some in Britain and Australia, with the result that the United States does not enjoy the clear lead in this field that it does in optical astronomy, despite the fact that the detection of radio waves from space was originally an American discovery.

Supporting basic science for purely cultural reasons, of course, pays dividends in other areas such as national prestige and the intellectual respect of the most influential groups in the rest of the world. Thus the purely cultural motivation supports the power and influence of the United States in the world and adds to the self-confidence of its own people. Nevertheless, paradoxically, supporting science solely for reasons of national prestige usually tends to corrupt it by distorting its scientific objectives and priorities, and thus ultimately to defeat the prestige objectives as well. This is generally an area where virtue is its own reward.

Economic

There is now general acceptance among economists of the importance of technological innovation in economic growth. To an increasing extent such innovation depends upon the results of basic science, although the degree to which this is true is difficult to quantify. To an increasing degree also there is a disposition to regard organized research and development as an investment in new knowledge equivalent in some sense to the investment in fixed capital. Indeed, most capital investments incorporate some measure of technological innovation. According to some economists the rise in capital-to-labor ratio accounts for only a small part of increases in productivity; about 50 percent is ascribed to other factors lumped under the general heading of "technical progress," which probably incorporates about equal parts of research and education as well as such factors as managerial and marketing innovations. There is also general agreement that in a market economy the allocation of resources to the advance and spread of knowledge will tend to be less than the optimum required for maximum efficient long-term growth of the economic system as a whole. Moreover, the further removed research is from ultimate practical application the less likely it is to be supported in a market economy without either direct public subvention or private support induced by special tax incentives, which is also a form of public support. Thus, there appear to be strong economic reasons for Federal support of research, and especially basic research.

In comparing the United States with other advanced industrial countries one finds that, if one sets aside military research and development expenditures, our investment in research is about the same in terms of percentage of national income as that of other countries, including Japan, the Netherlands, the United Kingdom, Sweden, West Germany, and France. It is noteworthy that Federal support of basic research in universities is a smaller fraction of total university basic research than in any other advanced country. This is, of course, because the United States has no Federal university system, and also because it relies much more heavily than other nations on indirect public support via tax deductions for private contributions. It is also noteworthy that the Federal share of university basic research has remained almost constant at about 57 percent over the last 10 years, despite the very large absolute increase. The fact that the Federal share of total research support in universities has increased is thus attributable solely to the increase in applied research, largely in the medical and engineering areas. Thus, in relation to our national investment in higher education, it does not appear that the Federal contribution to basic research in universities is in any way exceptionally large.

Since World War II there has been increasing recognition of the potential economic benefits of supporting science on its own terms without any commitment to specific applications. Politically, however, this commitment has always been made with some reserve. The National Science Foundation, the only agency with a clear mandate to support basic research as such, had a long struggle to come into existence, and an even longer struggle to attain a significant budget for research. Even today it accounts for only a little more than 10 percent of the support of research in universities proper—nearer 20 percent of the truly basic research. It also accounts for about 10 percent of all federally supported basic research. On the other hand, the Congress has been quite liberal in permitting the mission-oriented agencies to support basic research related to their missions, and the interpretation of mission-relatedness has been reasonably broad. If it had not been for this fact, U.S. science would not have attained the reputation for world leadership that it enjoys today.

In several fields Federal support for mission-related basic research has been of decisive importance for U.S. technological leadership, even in the field of civilian applications. Although the transistor was invented in private enterprise, Federal support for university solid-state research played an important role in creating an environment in which the transistor could be rapidly exploited and developed. Federally supported research also greatly accelerated the development of high-speed computers, and much of the pioneering work on computers was done in universities. Federal support of aeronautical research, largely in inhouse laboratories, was important for U.S. leadership in the development of modern civilian aircraft. Undoubtedly, Federal support for basic research in the medical sciences and biochemistry has accelerated the development of new drugs by industry. Support by the Atomic Energy Commission of basic nuclear research that was not obviously relevant to weapons or nuclear power has been largely responsible for the maintenance of U.S. leadership in this field.

On the other hand, in only three fields—agriculture, mineral resources, and civilian nuclear power—has the Federal Government explicitly supported applied research aimed at development of the civilian economy.

Social

In many areas, including public health and national defense, there is a recognized Federal responsibility. In these areas the Federal Government has generally been quick to utilize research in support of its missions, including a substantial amount of basic research. In fact, for the most part, basic research support has tended to derive from these special missions rather than from any overt policy concerning the desirability of social support for research. More recently, beginning with the National Advisory Committee for Aeronautics in 1920 and extending through the Atomic Energy Commission and the National Aeronautics and Space Administration, the Government has recognized a special responsibility for exploiting certain advanced technologies in the national interest. In these cases it was recognized that the technologies were sufficiently new and unappreciated so that they would not be adopted and adequately supported as part of the missions of existing Federal agencies or private institutions. They needed hothouse cultivation, as it were, before they could grow and mature on their own. In each example of such an agency, however, there was a strong military overtone to the justification; it is doubtful whether the National Advisory Committee for Aeronautics, the Atomic Energy Commission, or the reincarnation of the National Advisory Committee for Aeronautics in the National Aeronautics and Space Administration would ever have been justified without a quasi-military incentive. However, once there, their additional roles in economic growth gradually came to be appreciated.

It is clear that with increased urbanization and industrialization, our country is developing a number or problems that can only be faced on a national basis—for example, education, air pollution, water resources, weather forecasting and control, pesticides, radioactive wastes, public recreation, natural resources, air traffic control, highway safety, and urban transportation. The degree of Federal responsibility in these areas will always tend to be a matter for political debate, but there is greater consensus that the Federal Government has a responsibility for seeing that the foundations of knowledge are laid in these areas than there is that it has an operational responsibility. Research related to these social goals tends to be recognized as a Federal responsibility even when operation or regulation is delegated to the State or local level or to private enterprise. If applied research for these purposes is a Federal responsibility, it is clear that the basic research that underlies it must also be recognized as a Federal responsibility. Except in the areas of health and national security, however, there is still little appreciation of the contribution that uncommitted basic research can and should make to these goals. What is called basic research in many areas of Federal civil responsibility is still rather narrowly oriented in terms of obvious relevance to the immediate goal. Such oriented basic research is vital, but not sufficient. The rather rigid interpretation of relevance to mission that exists in the research in the older civilian agencies is in sharp contrast to the broader interpretation that is followed in national defense and health.

The difficulty with this motivation for Federal basic research is that criteria for the amount and character of basic research that should be supported in connection with social goals is difficult to establish. Clearly it is proper that research as a whole in these areas should compete on an equal basis with alternative means of achieving the same goals. Perhaps the only reasonable criterion is to relate the basic research effort of an agency to its total applied or development effort, possibly in terms of some percentage of the applied effort. Any such criterion, however, should involve some smoothing of fluctuations to take into account the larger time frame of basic research. The fractional effort on basic research will inevitably be strongly dependent on the breadth of the mission of an agency and on the magnitude of the total effort and its degree of dependence on relatively new or recently discovered scientific knowledge. I would suggest that in many instances 10 to 15 percent of the applied effort might be a good rule of thumb for the basic research effort. However, it is difficult to mount a viable basic research effort when the applied research is too fragmented into small units, as it is, for example, in the Department of the Interior, the Department of Agriculture, and the Department of Commerce (except for the National Bureau of Standards). In such cases it might make more sense for these departments to "task" the National Science Foundation with basic research in certain broad areas of relevance to the total mission of the department. It also seems rather important that not all the research, either basic or applied, be inhouse. Exclusively inhouse research often appears to be more efficient in the short run, since the people involved can be more closely channeled into research areas that meet the short-range requirements of the mission, but in the long run a purely inhouse research effort tends to cut the agency off from the scientific community. Not only is the scientific and educational community unaware of its problems, but its own people lose awareness of the opportunities that new developments in basic science present in the applied research it is doing. It always tends to define its own subject matter too narrowly.

Education

The intimate connection between basic research and graduate education has been repeatedly stressed in recent years. In engineering, medicine, agriculture, and several other areas, applied research is equally as important as advanced training, and there is danger that this fact may be forgotten in identifying the universities too exclusively with basic research. In particular, there is a tendency in the universities to regard the application of science as a lower order of intellectual activity than pure science, an attitude that tends to impede the healthy flow of talent between basic and applied science, which has been one of the characteristic features of American science contributing to its vitality. On the other hand, it is true that even in applied research the universities ought to focus on the longer-range goals, the things that are likely to become economically viable several years away, and that have the greatest generality in application. Research apprenticeship is the most essential part of graduate education beyond the master's level, whether it be in pure or applied science.

There is a broader sense in which research activity contributes to education. Research itself is defined as "learning work"—the production of new knowledge. While much of this knowledge is made explicit and public by publication in the technical literature, the individuals engaged in advancing knowledge acquire skills and perspectives that greatly transcend the sum of the information appearing in their publications. The contribution of a Fermi or a Von Neumann to our society is far greater than that of the bound volumes of their collected works or even than their influence on their students. A great scientist becomes a teacher of his whole culture. The people who devote most of their lives to research become a national human resource, available in emergencies to turn their attention to many problems outside their own immediate fields of interest. The rapid application of microwave radar during the early years of World War II was largely the work of nuclear physicists, even though the basic invention had been conceived several years earlier by engineers in Government laboratories. What was needed for the exploitation, however, was not just the invention itself but a whole complex of experience with advanced electronic techniques and with the integration of these techniques into an operable system. The nuclear physicists who had been working with accelerators possessed this kind of experience, and were able in an emergency to turn it to military applications. Through the decade of the 1930's, they had been unknowingly educating themselves, in a sense, for just this moment. It is doubtful whether any explicit or conscious form of education would have been as effective as their own continuing involvement in basic science. What applied to radar was even more evident in the case of nuclear weapons, since only those previously engaged in nuclear research, chemical kinetics, radiochemistry, and other fundamental fields had the accumulated skills necessary to proceed with projects in this field. The contribution of the engineering management skills of American industry—especially of the chemical engineering industry—was also indispensable, but without the intellectual leadership and vision of the basic scientists the project would neither have been undertaken nor carried to a successful conclusion. The development of the electronic computer in the early postwar years owed much to the high-speed electronic-circuit techniques in which nuclear physicists had trained themselves in order to sharpen the tools of their own basic research.

Not all individuals who receive advanced training in basic research remain in basic sciences. Some enter basic research in industry or Government but then move on to applied science or technology in the course of their careers, often following a basic-research development or technique through into its applications. Many techniques now common in industry, such as high-vacuum technology, low temperatures, X-ray diffraction, spectroscopy, nuclear-reactor physics and neutron instrumentation, radioisotopes, electron microscopy, had their origin as techniques of basic research. Hence, there is a demand in industry for people trained as basic scientists in such fields who then find their careers in applications. The staffing of major new technological or scientific programs such as nuclear power and nuclear weapons, space research, oceanography, or atmospheric sciences has come from people with original training in basic research in physics, chemistry, mathematics, or biology. This transfer of people forms one of the major vehicles for the translation of basic science into applied science and technology, as well as for the creation of new disciplines. Thus, basic science tends to be a net exporter of people into other more applied fields of science or into technology. Too little is known, actually, about the transfer of people between fields and the influence of people receiving basic research training in one field on the development and success of other fields. It seems clear, however, that the training of people in the most advanced techniques and concepts of basic science is not only beneficial to the development of basic research itself and of graduate education, but also has an important influence on the development of technology and of new industry.

Other individuals trained in basic science may choose basic research as a career but make important contributions on a part-time basis to technology and applied science. Von Neumann, a pure mathematician, formulated one of the key concepts of computer organization. Fermi, a pure physicist, conceived the idea of the nuclear chain reaction and played a leading role in its practical exploitation.

Many key ideas of military technology in the 1950's benefited from important contributions from basic scientists acting as amateur weaponeers. These people brought fresh viewpoints, new combinations of skills and techniques, and a broad vision of the potentialities of science to the weapons business. This contribution was often traceable to their basic research background. These contributions are an incidental benefit deriving from the vigorous support of basic research by the Federal Government, but they have played a significant role in the maintenance of United States preeminence in military technology.

Between the graduate student working as a research apprentice and the professor or laboratory scientist working at the frontiers of knowledge there has grown up a new group of post-doctoral research staff who also participate in the educational process, both as students and teachers. Such people have no formal part in the educational process; nominally, they are just research workers. They do not earn degrees and they do not teach classes. But they both help in the detailed guidance of graduate students and deepen their own knowledge in their chosen fields. Many university departments now have as many post-doctoral fellows as graduate students, largely supported out of federal research grants and contracts. Most of them stay only a few years and then move on to more permanent academic posts as full-fledged teachers. Because of their lack of formal academic status, we know very little about this group, although their support constitutes a very significant fraction of the total research money going into universities. In some other countries—notably Sweden, the United Kingdom, Japan, and the U.S.S.R.—there exists more formal recognition of the status of the post-doctoral student in the form of the D. Sc. degree, a sort of super-degree awarded on the basis of a body of significant contributions to the scientific literature.

The advantage of discussing the educational purposes of basic research is that this is the criterion for research support that is easiest to quantify. To an increasing degree U.S. policy has been evolving toward a consensus that, at least in science, society as a whole should be prepared to underwrite the opportunity for every individual to carry his education as far as he is willing and able to go.

Thus, by extrapolating long-term cultural trends, we are able to estimate fairly well how many people will be seeking graduate education in science and engineering during the next decade. The people who will do so are already in high school and college today, so there is not too much guesswork involved. The estimates of annual growth in the number of graduate students vary between 5 and 10 percent. The number has been about 8 percent for the last 2 years, but for the most part these students have not yet entered the research phase of their graduate study, so the full load on university research budgets has not yet been felt.

One can use the above figures to set a floor to the university research support required in the next 10 years if one makes certain plausible assumptions, as follows:

(1) The percentage of college graduates seeking graduate education in science will remain relatively constant or grow slightly. (Actually the Gilliland Panel assumed a slight growth in engineering and a slight decline in mathematics relative to the number of undergraduate majors in these fields.)

(2) The student-professor ratio will remain about the same as at present.

(3) The ratio of post-doctoral students to graduate students will not grow beyond its present value.

(4) The percentage of the total budget going into the support of large facilities, either construction or operation, or the support of research institutes relatively divorced from teaching but still in "universities proper" will remain about as at present.

(5) Because of the increased sophistication of research—including such items as more automatic data taking and data processing, greater use of computers, and greater availability of sophisticated instrumentation for purchase rather than local construction—the cost of research per man-year of research effort will increase at an annual rate of 5 percent in constant dollars.

(6) The contribution to research in universities from State, local, and private sources will increase at the same rate as the Federal contribution, so that the Federal share will not change.

With these assumptions one arrives at a university research requirement that rises at the rate of 13 to 15 percent annually. It is interesting to note that this figure agrees rather closely with projections of requirements for the optimum scientific development of selected fields of science made by certain committees of the National Academy of Sciences, which will report later.

It is important to note that almost all the assumptions in the above projection are conservative. For example, during the past 10 years, with relatively little growth in the number of graduate students, the research investment per Ph. D. granted 1 year later has increased by a factor of 2.5. This represents an increase of 10 percent a year on a per-man-year cost basis, nearly twice what is assumed above for the next decade. We are not sure of all the reasons for this growth. We suspect it is due mainly to a change in the character of universities that has been going on for the last 30 or 40 years, and that was probably accelerated by the availability of Federal research funds. Research has become an increasingly important part of the purpose of more and more American universities, as it has been of European and British universities for many years. Although university faculties have probably increased by less than 30 percent during this period, Ph. D. faculty has more than doubled. Furthermore, the population of post-doctoral research associates and to some extent the growth of research institutes with permanent research staff or research professors have caused research costs on a per-Ph. D. basis to rise rather rapidly. However, it is to be noted that, because of the upward trend of salaries in the last 10 years, the normal annual increase in cost per man-year of scientific effort has been more like 7 than 5 percent. The difference between 10 and 7 percent, or 3 percent, thus represents the cost of the general change in the character of the research economy of universities, and is not really dramatic. In the above projection we are assuming essentially that this long-range cultural trend will stabilize, a somewhat doubtful assumption. On the other hand, it is also true that the last decade was a period of rapid inflation of academic salaries, which had fallen seriously behind the cost of living during the postwar inflation. Academic salaries, at least for scientists, have now reached approximately their prewar position, and it is doubtful whether the inflation of the past decade will continue. Easing off of defense development expenditures may also take some of the inflationary supply-demand pressure off scientific salaries generally, especially in view of the projected increase in the supply of Ph. D.'s. The assumptions regarding the post-doctoral population are also probably conservative. On the other hand, this is the part of the academic research budget with the greatest flexibility; its size tends to be adjustable to the total funds available. A disproportionately large fraction of post-doctoral staff is probably of foreign origin, although many of them ultimately remain in the United States and take academic or industrial posts. With respect to the increased research orientation of university and college faculties, the assumptions are almost certainly conservative. As older professors oriented primarily to classroom teaching retire, they are likely to be replaced by younger men who expect to combine teaching and research. To an increasing degree it is expected that undergraduates may participate in research. Many formerly purely undergraduate institutions are talking about expanding into graduate work, if only to attract faculty of the requisite competence to maintain the quality of their undergraduate programs. Several areas of the country, especially in the South, are just at the beginning of recognizing the importance of research in the functions of a university. These expectations are not really taken into account in the estimate of 15 percent a year given above. They will not be satisfied unless one of several things happens:

(1) Research funds for universities are increased faster than 15 percent a year.

(2) There is a substantial cutback in support of new major research facilities at universities, and support going to post-doctoral associates and career research staff.

(3) Other sources of financial support for research become available, possibly as a result of tax incentives to induce greater contributions to university research from industry, or special Federal programs to encourage matching research funds from States.

(4) The declining post-education job market for scientists and engineers induces college graduates to seek other careers outside of the technical fields, so that present estimates of the demand for graduate education are grossly inflated.

In my opinion (3) and (4) appear highly unlikely. It is remarkable, in fact, that the non-Federal contribution to academic science has been able to keep pace as well as it has in the recent past. Most experts on fiscal and tax policy doubt that tax incentives could be designed to result in substantially increased allocation of resources to graduate education and research. In fact, the present tax system already provides many built-in mechanisms for transferring resources from the profit to the non-profit sectors of the private economy.

Cutbacks in defense spending may produce temporary effects along the lines of (4). On the other hand, historical experience does not suggest that the demand for graduate education is very sensitive to the short-term job market. In fact, it is entirely possible that lack of post-education opportunities may induce the opposite effect. The decline in the short-term financial advantage of going to work immediately after the baccalaureate might induce more people to continue their training, as tended to occur during the depression. In the past the massive Federal investment in research and development has scarcely influenced the fraction of college students choosing science; its effect has been mainly on the quality of the training available.

In my opinion it would be very unfortunate for U.S. science if any drastic change along the lines of (2) took place. The U.S. position of world leadership in science is highly dependent on the possession of research tools with greater capability than any in the world, and on the existence of a few outstandingly creative groups built up over a long period of time, which often set the pattern and stimulate the efforts of smaller groups throughout the country and train a disproportionate fraction of the people who become leaders and innovators in basic research in other institutions. The research-associate group in major centers often serves as the source of faculty for new centers.

Furthermore, an attempt to create new centers of excellence or achieve a wider geographical distribution of research funds primarily at the expense of existing centers of excellence would be of no service either to science or to graduate education. The inhibition of the best groups could not be compensated in leadership terms by better support of numerous other groups of a high but lesser level of competence. Inevitably it is the graduate schools of the leading institutions that set the standards to which the newer graduate schools aspire and by which they can be measured, and which must provide many of the leaders required to establish new centers of excellence. The wider diffusion of research support is an important and desirable goal, but we should not attempt to achieve it so fast that we destroy or degrade the excellence we have already achieved.

Possibility (1) may be worthier of more serious consideration. It could be achieved without as rapid an overall increase in research funds if support for nonuniversity basic research were held back, e.g., in research centers. On the other hand, even here the jeopardy to our leadership position would have to be carefully considered. The principal difficulty in this area is that it is much harder to judge quality in the research centers than in the individual university research projects. Large research institutions tend to have a different social ecology than smaller university research groups. They are less individualistic, and the whole tends to be greater than the sum of the parts. At their best they provide an environment that may exploit the talents of people of average ability much more effectively than if they were entirely on their own on a university faculty. On the other hand, great laboratories tend to be evaluated by the best work that they produce, and, when support is given on an institutional basis, a few excellent groups or individuals can often "front" for the whole organization, even though the total product may not be too impressive in relation to the numbers of scientists involved and the resources used. We have not yet learned how to apply the same rigorous standards to large research organizations that we do to individual research projects in universities. On the other hand, many of these organizations have a purpose other than mere excellence in basic research. It may be necessary for them to do some basic research, even if only of average quality, in order to keep a staff of the requisite level of competence to fulfill their applied mission. On the other hand, with a rising supply-demand ratio for technical people, it should no longer be necessary for such organizations to offer complete freedom of research to rather average people in order to attract them to the organization in the first place. A general tightening up in quality standards of the larger research enterprises both inside and outside universities seems both feasible and desirable in the coming decade. However, it is not clear that the real savings that might be effected in this way would be sufficient to cover the expansion required for education without substantial annual increases in the allocation of funds to basic research. In any event, very close scrutiny of major projects in space, geophysics, and other areas seems called for, not only to evaluate their intrinsic scientific merits but also to consider their impact on the rest of science. In the past, such ventures have been enthusiastically supported by the scientific community on the tacit assumption that there was no competition between these projects and the general support of "little science." This assumption is valid to only a limited extent, and tends to become less valid as research and development becomes a larger fraction of the national budget and of the budgets of individual agencies. This is because research budgets become more and more competitive with other activities within predetermined agency ceilings. As mentioned previously, such projects also imply commitments for operating funds merely to keep the facilities available without supporting any science.

To summarize, on the basis of educational requirements alone, it appears that a minimum annual rate of increase for university research support of 13–15 percent will be required for the next decade if the United States is to meet its announced goals for graduate education. This implies that by 1970 the Federal money being channeled into "universities proper" should be of the order of at least $2.3 billion, of which about $1.2 billion will be for basic research. It is to be emphasized that this projection is based on very conservative assumptions regarding the development of universities in the next decade. If these assumptions do not apply, the requirements are likely to be substantially larger, and can be met only by increased research budgets or by reprograming substantial funds from the Federal support of non-education-related research. Alternatively, it is possible that the educational goals are unrealistic and should be revised downward, but this is so contrary to past cultural trends that I find it difficult to accept. One would have to demonstrate that there is some other intellectual activity that would be much more socially productive, and that would require a radically different kind of educational preparation. It should also be noted that if these goals for research support are to be met, either the budget of the National Science Foundation will have to be increased much faster than is currently envisioned (probably of the order of 30 percent a year or more) or the responsibility of the mission-oriented agencies for graduate research training as such will have to be more explicitly recognized in national budgeting.

III. *Criteria for the Support of Various Fields of Basic Research in Universities*

A great deal has been written recently about criteria for support of various fields of basic science. I have already indicated that the small percentage of scientists representing the most talented and creative people should essentially be supported to do whatever they think best, within the financial limits indicated previously, since their own self-directed efforts are likely to be more useful to society than anything anybody of lesser talent could think of asking them to do. However, the people I am talking about probably represent only a small fraction (of the order of 5 percent) of those capable of doing competent and significant basic and applied research. The question of criteria, then, applies only to the activities of these less-than-top people. Even in this area it is my belief that the criteria are considerably less important than who applies them, that the fundamental problem of resource allocation within basic research is who makes the important decisions and how they are made. For example, to what extent should the cutting up of the pie among fields be left exclusively to the scientific community? At what level of detail should the financial decisions be made by the people not actually doing the work? Should resources be allocated to institutions and then divided within the institutions, or should they be allocated to broad fields and then divided within the field with the aid of representative groups of experts entirely from within the field regardless of institutional affiliation? To what degree should the system of choice be mixed, that is, with all allocation partly by institution and partly by field? If mixed, what are the proper proportions? What kind of guidelines should expert advisory committees be given? What kind of criteria, if any, apart from intrinsic scientific merit should be used? Should the definition of intrinsic scientific merit be left implicit rather than explicit—as something that every competent scientist knows intuitively but cannot express? To what extent should judgments in special fields be left entirely to the specialists in those fields, and to what extent should the judgment of fellow scientists from neighboring fields be brought to bear?

In trying to answer these questions, I should like to try to describe an idealized resource-allocation system for basic research. In doing this I am concerned primarily with university basic research, which for purposes of this discussion, however, should include major installations outside of universities, such as Brookhaven, Green Bank, or Kitt Peak, insofar as they exist primarly to serve the university community.

For the purposes of this discussion I feel that research funds should be placed in the following general classifications, which are quite separate conceptually if not organizationally:

(1) The capital costs of major equipment, including in general the cost of properly housing it. By major equipment I mean the kind of equipment that would not ordinarily be provided on a research grant. The amount of money involved might vary from field to field, but I am thinking of something at least of the order of several hundred thousand dollars. In general, I have in mind really major facilities like oceanographic ships, the Mohole platform, space tracking stations, or particle accelerators. This category would include the costs of any major refurbishing or updating of such equipment.

(2) That part of the operating costs of major facilities or equipment needed to make them available to the scientific community, exclusive of the cost of specific scientific work. This would include such items as ship-operating costs in oceanography, the costs of power, expendable supplies, maintenance personnel, and resident operating staff for big accelerators, the costs of computers, the logistic costs of scientific space vehicles including the cost of procurement, launching, and tracking of a given vehicle, but exclusive of the cost of the instrument package and data analysis and interpretation.

(3) The strictly scientific costs, including small permanent and expendable equipment, salaries of technical personnel, computer charges where the computer is shared by many users, publication costs, general administrative overhead, etc.

In my opinion the budgets for items (1) and (2) should be rather carefully segregated from (3). Together, they constitute what Professor Kistiakowsky has referred to in his paper as "big science." The decisions regarding allocations under (1) are the only decisions regarding allocations between fields of science that should be made at the highest levels of government, e.g., by the Bureau of the Budget and the Congress or by the agency head. They should be made with the advice of the scientific community, but it should be recognized that they are inevitably quasi-political decisions. They are the basic investment decisions of the Federal Government, and they are the decisions that determine the scientific priorities for many years ahead. They are also the decisions in which the price of error is highest. In general, science-allocation decisions are less crucial because there are many investigators working independently in the same general area, and so mistakes in the decisions of one investigator tend to be compensated for by the successes of others, and the proposal-evaluation system gradually eliminates the unsuccessful ideas and investigators by a sort of free market of ideas. For the big projects involving many investigators, however, choices are much more irretrievable, and there is often no way of telling whether an alternative choice would have been better until a substantial investment has been made. For example, if the recommendations of the various panels on high-energy physics with respect to what machines should be built prove wrong, the consequence could be the loss of U.S. leadership in this field for a generation. Even if the Government were prepared to retrieve the mistake by writing off the original investment and building a new machine at greatly increased expense, the time lost might be a serious setback to U.S. leadership. With respect to science these are the same sorts of crucial decisions as the choice of an intercontinental ballistic missile system is for the preservation of national security. They are the fundamental strategic decisions of basic science, and for them criteria something like those proposed by Weinberg seem appropriate. In this regard I second the views expressed by Kistiakowsky in his paper. They should be widely debated in the scientific community and elsewhere from every angle; they should ultimately be made in a highly visible and public way.

When the decisions in category (1) are made, their consequences in terms of category (2) should be clearly spelled out and understood, and should form part of the basis of the decision as to whether to go ahead. In projecting research budgets into the future, category (2) funds should be separately identified as such. In many cases, it would be wisest if they were not included in the ordinary individual research proposal, although this is an administrative question that may have to be decided in each individual case. It is my feeling, however, that the inclusion of fixed availability charges in individual research proposals tends greatly to confuse and complicate the proposal-evaluation process. In many cases it may be desirable to divide the availability charges between two budgets, with only a nominal charge to the individual research proposal.

Category (1) decisions also have implications for category (2) funds. It would make little sense to build facilities if support were not available for scientists to use them. On the other hand, I feel that scientific work with large facilities should not receive a specious priority just because of the political embarrassment entailed by lack of full utilization of a facility. Actually, once the commitment for the capital cost of facilities and their basic operating costs has been made, individual scientific experiments done with such facilities should compete on an equal basis in terms of scientific merit with other work that does not employ large facilities. Conversely, once the commitment to build and operate a facility has been made, I do not believe meritorious scientific work should be penalized before evaluation panels by having to bear the full category (2) costs related to the facility.

The above discussion takes care of category (1) and category (2) costs. The budgeting process should attempt to arrive at an overall Government-wide level for category (3) costs in universities. This will, of course, be a sum of agency budgets, and each agency will be expected to project its category (3) costs as a budget line item. In the National Science Foundation budget, for example, this would be approximately the basic research support category, although certain of the category (2) costs of particle accelerators and oceanographic ships might be excluded and budgeted under another category, and, as detailed below, certain other program costs might be included. The category (3) part of the total Federal budgets—the part for university research, that is—should then be evaluated against the 15-percent-a-year growth standard mentioned earlier. I am not saying that we must have 15-percent growth every year, or that we should limit ourselves to 15-percent growth in each year. Obviously, no part of the Federal budget can be sacred, and the amount of each category can be determined only in the light of the state of the economy, fiscal policy, tax revenues, and other global considerations. I am saying only that the 15-percent growth of category (3) Government-wide should provide a more adequate index than we now have of how we are doing in research support. Because of the confusion of "science" with category (1) and (2) expenses, which merely build the store and keep it open but don't sell any goods, our present system of budgeting doesn't tell us how much science we are buying.

At this point one must decide how to allocate the money in category (3) between disciplines and institutions. There appear to be several bases for this. Since the level of support in category (3) is being compared against a standard derived from the requirements of graduate and post-doctoral education, it ought to include not only basic research support funds, but also fellowship funds, general research support funds, and some proportion of science development funds, institutional base grants, and research training grants. In other words, it ought to include the total funds being channeled into higher education by the Federal Government that are related primarily to research and research training, as opposed to capital investment, and to graduate and post-doctoral research training or undergraduate research activities as opposed to formal teaching activities or curriculum development.

Taking the total of category (3) we now have the question of how it should be divided among the following categories of support:

(*a*) Project grants to individual professors or small groups of professors;

(*b*) Programmatic or coherent area grants to large groups or whole departments;

(*c*) Institutional grants, either on the basis of a formula or on the basis of specific selection criteria;

(*d*) Direct support of personnel, including graduate and postdoctoral fellowships, faculty fellowships, or career research awards, awarded on the basis of national competition;

(*e*) Direct support of personnel, but at the decision of the institution rather than on the basis of national competition between individuals in a discipline.

In this listing, the operative question is whether selection is on the basis of national competition within a discipline, or is primarily cross-disciplinary with award to institutions on either a formula or a competitive basis. (*a*), (*b*), and (*d*) are regarded as falling in the first category, (*c*) and (*e*) in the second. My own present belief is that the country has a bit overdone the matter of project support, to the point where many institutions have abnegated their responsibility for and influence over their own research activities and institutional development. Therefore, I would be inclined to recommend a gradual transition to a situation in which about 25 percent of category (3) is direct support of personnel, category (*d*); about 25 percent is institutional support, categories (*e*) and (*c*); and about 50 percent project support [including both (*a*) and (*b*)]. It seems to me the exact division between (*a*) and (*b*) is a matter for individual agency decision and negotiation with grantee institutions. It may well vary from agency to agency. As nearly as one can determine, the fiscal year 1963 figures corresponding to the recommendation above are as follows:

	Percent
Direct research and development support (including project and coherent area or program grants and contracts)	68
Institutional program (NSF institutional base grants and NIH general research support)	10
Training (including fellowships, training grants, career awards, and postdoctoral fellowships)	16
Construction	6

This still leaves open the question of allocation to disciplines. This presents no problem with regard to category (*c*) above, since the allocation is largely up to the institution. With respect to categories (*d*) and (*e*) I tend to be opposed, in principle, to too closely defined categorical fellowships such as those offered by the National Aeronautics and Space Administration and the Atomic Energy Commission. My observation is that students are cannier in choosing the right fields than any government administrator, and that, by and large, it is best to support the brightest people and let them choose the most promising and exciting fields, relying on the competitive salesmanship of different disciplines and the external scientific labor market to determine the actual allocation indirectly. In practice, the flexibility with which the National Aeronautics and Space Administration traineeships have been administered has apparently so far avoided what might ultimately prove to be a difficult and embarrassing problem.

If our policy is essentially to support the brightest people irrespective of field, then both government and the universities must give more attention to systematic presentation of the opportunities and promise of various fields, not only in terms of intellectual excitement but also with respect to occupational demand and social utility. I suggest that this method is superior to providing categorical fellowship support for rather narrowly defined fields. Obviously, the method of allocation on the basis of merit without reference to field is an ideal that can only be approached because of the limitations under which the mission-oriented agencies work. It might well be that some government-wide pooling of fellowship applications would be worth considering in this connection.

With respect to categories (*a*) and (*b*) there will obviously be variations from agency to agency. My feeling is that, to the degree it is consistent with the agency's mission, each agency should allocate support in accordance with its estimate of the requirements of the academic community, as judged by proposal pressure and the informal advice of its program officers and consultants. Intrinsic scientific merit should be the most heavily weighted but by no means the only criterion of selection, with each agency supporting projects having a distribution of topics centered about those most closely related to its mission, but by no means confined to these. Application of this principle may actually force some gradual reallocation of resources. Of all the Federal agencies, the National Science Foundation is the one that has the clearest obligation to respond primarily to the estimated needs of the academic community. There is, of course, a good deal of positive feedback between known availability of funds and proposal pressure. It is necessary to invent mechanisms to discount such effects. In this connection, widely representative advisory panels extending over several different disciplines, such as the divisional committees of the National Science Foundation or the institute advisory councils of the National Institutes of Health, must play a key role. These groups should be made more aware of the total resource-allocation problem, so that they become less inclined to promote only their own fields. Committees of the National Academy of Sciences appointed to analyze the needs of broad scientific fields and coordinated by an overall committee such as the Committee on Science and Public Policy should also play a key role in this connection. Federal agencies concerned primarily with civilian applied research should take more initiative in requesting that appropriate kinds of research be encouraged by the National Science Foundation.

There is a general problem with respect to research support and research priorities that deserves mention at this point. One aspect of it is discussed in the paper by Dr. Teller. One of the unfortunate side-effects of the generous support of university research in the last 10 years has been a tendency to denigrate the intellectual respectability of applied research. Perhaps this has always been present in the basic research community, but the size and influence of this community has reached the point where its viewpoints affect the self-image of applied scientists, engineers, and doctors, and especially the attitudes of young people toward their future careers. The generous support for academic basic research recommended in this and other papers in this series is predicated on the assumption that the healthy development of applied science and technology requires the continual infusion of people trained in basic research. Thus, to an increasing degree, many people trained in universities will be expected to move gradually into more applied areas as their careers mature. If the effect of their university training is to inculcate attitudes that make it too difficult for students to move into applied work, much of the benefit of their training will be lost to society and the justification for public support of basic research in connection with graduate education may ultimately be called into question. It is doubtful whether the long-term influence of university viewpoints on the attitudes and careers of students is as serious or as permanent as is sometimes represented. Basic research support outside of universities has been increasing rapidly at a time when the supply of new Ph. D.'s was relatively constant from year to year. As a result the opportunities for students trained in basic research to stay in basic research have been greater than ever before. This appears to be especially true in physics and biology. In chemistry, where the supply of Ph. D.'s is much larger in relation to the demand, a career in applied work is generally more acceptable. Looking toward the next decade, it appears that the situation in physics will tend to become much more like that of chemistry. I believe that the changing job market will tend to moderate the attitude of students. Still, I am in agreement with Professor Teller that there is a serious need to improve the intellectual status of applied work. This is most likely to occur when first-rate people go into applied work and provide the heroes or models that inspire youngsters. Experience shows that it is very difficult to make any intellectual activity respectable by definition, as opposed to example.

With regard to selection criteria for basic research proposals, I should like to suggest the following in approximate order of priority. Obviously, the relative importance of these criteria will vary between the National Science Foundation and the mission-oriented agencies:

(1) Quality of the people proposing the research, evaluated on the basis of their past performance as judged by their professional peers and by people in adjacent disciplines. In this instance, one must be careful to avoid development of a "closed system," since those who are supported will tend to acquire a reputation that will facilitate acquisition of more support. For this reason it is particu-

larly important that the support system provide adequately for the support of new investigators.

(2) Novelty, prospects for new generalizations or important changes in outlook, and degree of penetration into important and previously unexplored territory. In this connection, emphasis must be placed on the importance of new tools. Almost every new research tool has opened up unexpected richness of phenomena. No matter how tight research budgets become, it would be dangerous to forego the construction of really new research tools. Emphasis in research support should be on achieving new understanding or generalizations, and not merely the assembling of new data for their own sake. Measurements should be informed by hypotheses or expectations.

(3) Relevance to recognized practical problems, assuming there is a reasonable prospect of progress. This criterion must be applied with caution and good judgment. Applied too narrowly and unimaginatively it can result in the support of rather trivial and pedestrian research. There is always a tendency to support applied research projects that are really basic research, but whose intrinsic scientific merit does not make them competitive with other basic research proposals. To the degree that relevance to practical problems is claimed as a basis of support, certain hard questions should be asked. What is a solution to the problem worth? How critical is a particular piece of information to the solution? What is the probability of success? What is the probability of unanticipated development or byproducts? In answering these questions, the advice of people with experience in the practical problems involved should be sought, as well as the advice of people concerned only with the intrinsic scientific merit of the work.

(4) Educational value, in both the strict sense and in the broader sense of extending the capabilities of bright people or groups of people. Will the research tend to stretch the limits of an existing technology that is likely to have other applications? Will it exploit a new technology not previously available as a basic research tool? Will it help maintain a standby capability in terms of people whose activities may become nationally critical in the future, as in the nuclear weapons laboratories? Will it help train graduate students, and enhance our resources for graduate education?

The preceding discussion has been concerned primarily with the criteria that should be used in allocating resources to basic and applied research in universities as well as to fellowships and other forms of support that indirectly subsidize research. The criteria suggested apply not only to what Dr. Kistiakowsky discussed as "little science," but also to "big science" insofar as it is primarily connected to universities and graduate education. In the area of academic research the emphasis is on the autonomy of science and on primarily scientific criteria of choice, although certainly other considerations such as potential relevance to the mission of the supporting agency must be given significant weight. However, it must be recalled that less than 50 percent of all the basic research supported by the Federal Government is conducted in universities proper. We must now discuss the criteria for support of basic research that is not connected with graduate education.

IV. *Institutional Research*

The term "institutional research" is designed to cover a broad spectrum of activities ranging from university-based research institutes to industrial laboratories. Basically it is characterized by the fact that the great majority of the scientists are full-time career research workers not engaged in classroom teaching. As pointed out above, the social ecology of these institutions differs from that of universities proper, and judgments concerning their support should be based on different criteria. Institutes of this sort can be further subdivided into two types:

(1) Those primarily concerned with basic research, having the aim of advancing some generally defined broad area of scientific knowledge, or perhaps a group of such areas, but usually connected by some common theme or object of study.

(2) Those primarily concerned with an applied objective usually related to the mission or missions of some Federal agency.

Sometimes a single laboratory may combine both functions in some degree; for our purposes it should then be considered as two separate institutions. The great national and Government laboratories usually fall in category (2), as do industrial laboratories. The only exceptions are laboratories like Brookhaven, the Green Bank Radio Astronomy Observatory, or the Kitt Peak Observatory. These are really extensions of university research. They have a service function in relation to universities, but their career research staffs are independent scientists in their own right. In a sense, however, they still serve an instructional function in that they help train graduate students and faculty members and post-doctoral associates in the newest techniques of their science. They can also undertake research problems demanding greater continuity and cooperative effort than is possible in a university department with other responsibilities. The basic research laboratories in category (1) should not be judged by the same criteria as those used in connection with universities. In the first place, as Dr. Kistiakowsky has suggested in his paper, greater scientific productivity should be expected of such groups, since they do not have other responsibilities. In the second place, they should truly serve their function of supplementing and assisting the universities; the resident staffs should not be so large as to preempt the facilities for their own experiments. It seems to me that in periods of limited research funds the expansion of such institutes should have lower priority than the expansion of university-based research, which is more closely related to teaching. The local management should be given great freedom and should be promised continuity of support but not necessarily continually expanding support. The creation and support of such institutes ought to be based on general criteria for the support of various fields of "big" science along the lines suggested by Dr. Weinberg. In terms of quality, such institutions ought to be subjected to standards similar to those applied to university groups. Insofar as they carry out independent basic research, such institutions ought to concentrate on types of research requiring special facilities, an unusually programmatic or long-term type of approach involving the closely coordinated activities of many senior scientists, or other basic research activities that are unsuitable for the individualistic style of university research. Conversely, unversities should concentrate on types of research that lend themselves to the individualistic approach.

Most of the great national laboratories fall in category (2), i.e., they have an applied mission. It is entirely right and proper that such laboratories should do a substantial amount of basic research, since experience shows that participation in basic research enables them to attract better people, to keep their staffs alert to new scientific developments of potential importance to their missions, and generally to perform better. However, the total support for such establishments should be based on the national importance of their applied missions and on their long-run success in performance. The fraction of support that goes into basic research should be largely a local management decision. On the other hand, such a laboratory should not receive increased support for basic research purely on the basis of the excellence of its scientific work or the number of papers published by its staff in reputable scientific journals. These may be indications of the general quality of the laboratory, but are not enough by themselves to justify its support. If this policy is followed, increased support for freewheeling research activities should be provided essentially as a reward for success in the performance of the applied mission, thus serving to give the whole staff a stake in the applied goals of the organization rather than setting up a status system in the laboratory that isolates the basic research from the rest of the laboratory. The so-called "independent research" supported by several agencies as part of the overhead on procurement contracts with profit organizations contains such a built-in incentive for success in its applied objectives, and a somewhat similar incentive system might be encouraged with respect to non-profit institutions doing applied work.

A special problem has arisen in connection with support of basic research by the Federal Government in industrial laboratories. Not only do many agencies support project contracts with industrial laboratories on a somewhat similar basis as that applied to project grants to university groups, but whole laboratories exist primarily by performing research services for Federal agencies. Some of this project activity represents excellent scientific work. On the other hand, there is a real question in my mind whether the basic research project contract is the proper mechanism for supporting industrial groups. This is especially true when research proposals from these groups are evaluated primarily on the basis of intrinsic scientific interest or merit rather than on the basis of their potential contribution to a specific applied objective. It is hard to lay down hard-and-fast rules in this matter, but, in general, it is my opinion that institutional-type support is preferable for industrial groups. In this type of support the basic research is supported by the local management as part of a general program aimed at an applied objective. Government laboratories and federally supported research centers also occasionally attempt to supplement the support from their parent agencies by seeking basic research contracts with other Government agencies in competition with academic research groups. In principle, this is undesirable; I would be strongly opposed, however, to blanket rules or regulations concerning it, and it would be unwise to alter abruptly the system of support that has grown up over the years. Such a sudden change would be unnecessarily disruptive. I feel, however, that this is a general area that the Congress may wish to examine, and that agencies now supporting industrial and Federal laboratories under small project grants and contracts should be encouraged to devise new support mechanisms more consonant with the institutional character of these organizations. The extent of this type of project support is not known at present, but it has an open-ended character that could make it a potential drain on tight basic research budgets if it were not carefully watched.

Occasionally, it is advantageous for agencies to make contracts with industrial organizations with a view to exploiting unique industrial skills in getting rather specific jobs done, usually in relation to some broader applied program or to provide needed tools or materials for university basic research. Examples might be the growing of crystals for experimental purposes or the development of new research equipment for which the potential market may be insufficient to justify private financing of the development costs. I have no criticism of contracts of this type.

V. *Concluding Comments*

The basic thesis of this paper adds up to the conclusion that the concept of a total science budget, which is implied by the questions asked by the House committee, is probably not a very meaningful or significant one. Only in the restricted area of academic basic research does the concept of a government-wide "science budget" make a certain amount of sense. Even here it is essential to separate out the costs of major equipment—both the capital costs and the cost of keeping it available for the use of the scientific community. The rest of the "science budget" ought to be considered in a different context, in which the value of research and development is judged in competition with alternative means of achieving the same objectives. In these areas I think that the Congress and the administration ought to consider primarily the total resources that it is worth while to devote to a general objective, and then regard as tactical rather than strategic the decision as to what fraction of these resources should go into research and development. Inevitably, such decisions are quasi-political and must be settled by debate among the various groups concerned; the voice of the scientists should be heard but should not be conclusive in this part of the debate. Basic research outside of universities—more than 50 percent of the total—should be judged in terms of its potential contribution to the missions of specific agencies.

References

(*1*) Some scientists may derive their support through working in close association with the outstanding 5 percent; thus it is not legitimate to extrapolate the $100 million for the 5 percent to $2 billion for the total pool.

FEDERAL SUPPORT OF BIOMEDICAL SCIENCES

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Summary

The biomedical sciences are concerned with man. Their objectives are elimination of abnormalities, prevention or cure of diseases, and prolongation of lives. Among the major biomedical problems are mental illnesses, cardiovascular diseases, cancers, viral diseases, environmental pollution, and congenital abnormalities.

Mental diseases cause enormous amounts of disability and ill health. Mental illness in some form develops in about 5 percent of the population. Major psychoses occur in 1 to 2 percent. Cardiovascular diseases lead to a tremendous volume of disability and are the leading cause of death. About 54 percent of all deaths were attributable to these diseases in 1963. Cancers lead to a vast amount of disability and are second as causes of death. About 16 percent of all deaths were attributable to cancers in 1963. Viral diseases cause a large amount of ill health. About 60 percent of all illnesses are attributable to viral infections. Environmental pollution constitutes a large potential hazard and an increasing threat to health. Congenital abnormalities are present in about 5 percent of all children. Approximately 250,000 children with such defects are born each year.

These are not all the problems in the biomedical field. Others are arthritis, blindness, neurological diseases, muscular diseases, respiratory diseases, and genitourinary diseases. The Federal Government is supporting biomedical research in an amount equal to about $5.50 per person per year, a total of about $1.06 billion. The cost of medical care amounts to about $187 per person per year, a total of about $36 billion. Additional Federal support for biomedical research is recommended.

More students of high intelligence and promise are needed in both Ph. D. and M.D. educational programs. Additional Federal support for doctoral education and post-doctoral training is recommended. The facilities, including libraries, available for biomedical research are not as

up-to-date or extensive as they should be. Federal support for the construction or modernization of such facilities is small relative to the need.

In view of the problems, the advances already achieved, the certainty that more information can be obtained, and the burden of disease, Federal support of research in the biomedical sciences should be increased. The major advances made in the last 20 years have been dependent upon high-level public support for science, and the need for such support will grow until solutions are found for these critical problems of human health.

Introduction

The biomedical sciences are largely and directly concerned with the well-being of man and have as their ultimate objectives the elimination of abnormalities in development and growth, the prevention or cure of physical and mental diseases, and the prolongation of useful and satisfying lives. Although none of these objectives has been fully achieved, there are reasons to believe that some of them are not as improbable of accomplishment as they seemed only a few decades ago. The advances that have been made in the treatment and prevention of many infectious diseases, including bacterial, rickettsial, viral, and protozoal; the prevention of various deficiency diseases due to defects in nutrition; and the control of diabetes, pernicious anemia, and certain endocrine abnormalities serve as examples of what can be accomplished when work in the field of the biomedical sciences is vigorously pursued and generously supported.

The biomedical sciences constitute only a part of the life sciences, which also include other biological sciences, agriculture, psychology, and some of the social sciences. Biological sciences other than those in the biomedical field are considered in the report of Dr. Lawrence Blinks, and social sciences are discussed in the report of Dr. Carl Pfaffman.

The objectives of many investigations in the biomedical sciences may be described as useful and practical, applicable ultimately to the solution of human problems that stem from ill health and disease. They may be categorized, therefore, as applied research, but, as has been emphasized in the reports of other members of this committee, their achievement often depends heavily on unanticipated advances made through basic research. The relevance of work in basic research to significant accomplishment in applied research was recently underscored in the report of the National Academy of Sciences, entitled *Federal Support of Basic Research in Institutions of Higher Learning*.

Until recently, the biomedical sciences that bear on the problems of human beings extended in scope only from intact living organisms, i.e., man himself and other animals, to the smallest unit of life, i.e., the individual living cell. Between the living cells of the biological world and the molecules and energy of the world of the physical sciences was a gap in knowledge of about the same magnitude as that of the difference between the size of cells and the dimensions of molecules. Within the last few years this gap, which for centuries had sharply separated the life sciences from the physical sciences, has been effectively bridged. Large and unanticipated advances in basic research, especially significant in genetics and virology, came from the combined efforts of physical scientists and biologists. Especially in these fields it has become evident that certain chemical and biological dimensions may be similar. These dimensions are much smaller than those of the living cell and can be determined with the aid of the ultracentrifuge or the electron microscope, which can make visible certain macromolecules as well as the extremely small organelles that make up the fine structure of cells.

The discovery that certain nucleic-acid molecules guide and control biological heredity; that viruses contain infective molecules of this kind which direct their own replication; that the genetic code is written in structural chemical terms that appear to be identical for all living things, demonstrates the advantages that are gained when scientific inquiry is pursued broadly without regard to the boundaries of formal disciplines. Increasingly the recognition has grown that there should not be any separation between the biological and the physical sciences, and the recent development of disciplines such as biochemistry, biophysics, chemical genetics, and molecular biology indicate the extent to which unification of the physical and the biological sciences has progressed.

This development has already had important effects on the biomedical sciences, and there appear to be good reasons to hope—even to expect—that it will forward the pursuit of the stated objectives of investigations in this field and bring nearer the time when they can be realized. That the sophisticated techniques and powerful tools of chemistry, mathematics, and physics are being utilized increasingly for the solution of biomedical problems is encouraging, and strengthens the anticipation of further significant advances in the future.

Major Health Problems

A number of unsolved practical problems confront biomedical research, and probably will require long and intensive efforts before useful and effective means are developed for their management. As examples, some of the largest, most difficult, and most pressing problems that affect the well-being of man are mental illnesses, cardiovascular diseases, cancers, viral diseases, environmental pollution, and congenital abnormalities.

Mental diseases, because of their common occurrence and protracted duration, account for an enormous amount of human disability and ill health, which seriously affect the usefulness and satisfaction of the lives of large numbers of people. Some form of mental illness is believed to develop in as many as 5 or more percent of the people in the United States. Major psychoses, which include schizophrenia, may occur in from 1 to 2 percent of the population. It has been reported that a similar incidence of major psychoses occurs in other countries, including those that are designated as underdeveloped. This would suggest that geography, standards of living, and ethnic origin may not be important contributing factors to these conditions, and raises doubt as to the proposed connection between the stresses and strains of life in advanced industrialized societies and the incidence of severe forms of mental illness.

Patients with mental illnesses presently require more institutional facilities, both public and private, for their care than are required for the care of all other patients combined (American Hospital Association, 1963). It has been reported that about 51 percent of the some 1,400,000 public and private hospital beds used in 1962 were occupied by patients with mental illnesses. Despite the considerable improvement in the treatment of some of the less severe forms of mental illness that has resulted from the recent discovery and wide use of a number of new drugs, the nature of the underlying abnormalities associated with many forms of mental illness has not been fully clarified. The organic factors that may contribute to the development of the major psychoses remain to be discovered, and it is widely acknowledged that treatment, other than palliative, for these severe conditions is unsatisfactory. This huge field is of great importance and continues to challenge the efforts of many workers in the biomedical field. Undoubtedly it will require intensive investigation, including studies at the level of basic research, before effective control and management can be achieved.

Cardiovascular diseases, which include a large number of different conditions, account for a tremendous volume of human disability and are, by long odds, the leading cause of death in the United States. About 54 percent of all deaths, or some 970,000, were attributable to these diseases in 1963 (National Vital Statistics Division, U.S. Department of Health, Education, and Welfare). Although treatment with modern drugs, new surgical procedures—including heart, brain, and blood vessel surgery—and anticoagulants represent important advances that aid in the management and control of certain severe forms of cardiovascular disease, the various factors that lead to the frequent occurrence of these serious conditions are not fully understood. Major advances in the treatment of some of the most disabling conditions may be anticipated on the basis of the dramatic results that have already been obtained by intracardiac surgery, correction of valvular abnormalities, introduction of artificial blood vessels, removal of blood clots, artificial external kidneys, and even transplantation of kidneys from other persons, though much remains to be learned in this new field.

Arteriosclerosis is thought to be a factor in the large majority of cardiovascular diseases and may be associated with them in as many as 80 percent of patients. Although many factors have been implicated as bearing on the development of arteriosclerosis, it seems evident that much more work will be needed before the mechanisms responsible for this abnormality of the blood vessels are satisfactorily clarified. Additional basic research, especially in biochemistry and enzymology, as well as further applied research in the fields of nutrition, epidemiology, and clinical investigation are much needed. Since both effective treatment and useful prevention of disease are frequently dependent upon precise knowledge of causal factors and the mechanisms that lead to abnormalities, the importance of learning more about them in these common conditions can hardly be overemphasized.

Cancers, which include a wide variety of malignant neoplasms, lead to a vast amount of suffering and disability, and are now second in the list of causes of death in the United States. About 16 percent of all deaths, or some 280,000, were attributable to cancer in 1963 (National Vital Statistics Division, U.S. Department of Health, Education, and Welfare). It is estimated that only a few decades ago, treatment was effective and successful in no more than about 10 percent of patients with cancer. At the present time, earlier diagnosis and modern treatments by new and extensive surgical procedures, high-intensity radiations, and special chemical compounds have resulted in considerable improvement in therapy, and as many as about 30 percent of patients, on the average, can be treated effectively and successfully. Despite this considerable improvement in therapy, a great deal more needs to be done before these disastrous diseases can be brought under effective control and regularly successful management. At the present time there is only a small number of institutions in the United States that have sufficient professional personnel and facilities to carry on both comprehensive clinical studies and extensive laboratory research on the problems of cancer. In view of the importance and the dimensions of these problems, additional large centers capable of undertaking full-scale investigations might well be established.

Increased knowledge of the causal factors and the mechanisms that lead to the development of cancers is clearly required if efforts to prevent the occurrence of malignant tumors are to be successful. Although there is abundant evidence that environmental factors such as ionizing radiation, chemical cancerigens, and a number of different viruses can act as primary incitants of the cancerous change in cells and may induce cancers in animals, the contributions of radiation and chemical compounds to the occurrence of cancer in man have not been sufficiently clarified and there is as yet no direct or unequivocal evidence that viruses are contributing factors in human cancer. In addition, the role of intrinsic secondary factors such as genetic constitution, hormonal status, and immune reactions of affected persons has not been fully established and will need extensive further investigation. The results of much research in this field tend to support the concept that cancers are not to be attributed to a single causal factor. Basic research in chemical genetics, molecular biology, and biophysics, as well as applied research in cytology, immunology, and virology may be expected to contribute to a fuller understanding of the nature of the cancerous change in cells and ultimately perhaps to its definition in chemical terms.

Diseases induced by viruses, because of their variety and frequency, constitute a large problem and cause a very large amount of ill health and disability. It has been estimated that about 60 percent of all episodes of illness are attributable to viral infections. Acute respiratory infections alone, most of which are induced by viruses, are believed to account for the loss of about 1 billion man-days of work each year in the United States. Some 50 different viral diseases that affect man have been identified, and several hundred different types of viruses that can infect human beings are known.

Although effective vaccines have been developed for a few viral diseases, such as smallpox, yellow fever, influenza, poliomyelitis, and measles, useful preventive measures are not yet available for the great majority. Specific and effective treatment for these infectious diseases is almost nonexistent, although there are recent indications that certain new chemical compounds may be useful in the treatment of one or two of them. It is well established that antibacterial drugs, including chemical agents and antibiotics, are not useful in the treatment of viral diseases. Basic research has been particularly rewarding in relation to viruses and viral infections and a great deal of important new information has been obtained in recent years, but there remain large opportunities for additional applied research in this field, particularly for more effective and widely useful preventive and treatment procedures.

Environmental alterations such as air and water pollution, smog, pesticides, and new chemical compounds constitute potential hazards of as yet undetermined dimensions that are believed to represent steadily increasing threats to health and well-being. Many of these alterations in the environment are already obvious in large cities and near extensive industrial installations. The available evidence suggests that deleterious environmental alterations may serve to inflict double jeopardy upon persons whose resistance has been diminished by chronic cardiovascular or respiratory diseases. It has been reported that large amounts of smog have been associated with a considerable excess over expected numbers of deaths in affected communities in several countries, and occasional disasters have already occurred. There is much still to be learned about the precise nature of the mechanisms that produce the harmful effects, particularly those relating to air pollution.

Some form of congenital abnormality is thought to be present in about 5 percent of the children born in the United States. It is estimated that some 250,000 children with such defects are born each year. In only a small fraction of cases are the underlying causes of the abnormalities, which are of many kinds, fully understood. Genetic abnormalities, inborn errors of metabolism, defects in prenatal development, and even certain viral infections are known to account for some cases, but the causal basis for most cases remains to be learned. In many instances, treatment is not as effective as desired and, in a number of instances, such abnormalities are associated with mental retardation, which greatly increases the seriousness of the problem.

It should be emphasized that the problems summarized above were selected as representing only some of the major practical problems in the biomedical field. The list is not intended to be complete; it could well have included arthritis, blindness, neurological diseases, mental retardation, muscular diseases, chronic respiratory diseases, genitourinary diseases, and many more that are all too common.

The effectiveness of research in the biomedical sciences during the last few decades provides good reasons for thinking that these major human problems need not go unsolved indefinitely. In support of this thesis, it may be pointed out that some severe diseases of human beings that were among the leading causes of death in the United States about 40 years ago are now uncommon causes of death. Pneumonia plus influenza and tuberculosis were more frequent causes of death than cancer in 1920 (cf. table 1), but are much less frequent at the present time. The striking change in their ranks between 1940 and 1961 can be attributed in large part to the discovery and wide use of antibacterial compounds, including modern drugs and materials of natural origin such as antibiotics, for the treatment of these infectious diseases. As a result, they no longer constitute the serious threat to human health and survival that they did in the past. As additional examples of significant accomplishment, certain of the major threats to the health and survival of children may be cited. Measles, intestinal infections, and poliomyelitis, some 20 years ago, were responsible for the death or paralysis of very large numbers of children in this country. At the present time, as a result of the discovery and development of effective viral vaccines, poliomyelitis and measles can be prevented, and intestinal infections can be largely controlled by antibiotics or other antibacterial drugs.

TABLE 1.—*Leading causes of death—Continental United States*

Cause of death	1920			1940			1961		
	Death rate [1]	Percent of total	Order	Death rate [1]	Percent of total	Order	Death rate [1]	Percent of total	Order
All causes	1,298	100	......	1,074	100	......	930	100	......
Cardiovascular diseases [2]	314	26	1	464	43	1	468	50	1
Pneumonia and influenza	207	16	2	70	6	3	30	3	4
Tuberculosis	113	9	3	46	5	5	(3)	......	......
Cancer	83	6	4	120	11	2	149	16	2

[1] Gross death rate per 100,000 population per year (not age-adjusted).
[2] Including renal disease.
[3] Not listed as a leading cause of death.

Sources: National Office of Vital Statistics, Vital Statistics—Special Report; U.S Department of Health, Education, and Welfare—National Vital Statistics Division—Advanced Reports.

It should be noted that the death-rate data given in table 1 are gross rates that are not age-adjusted. Age-specific rates for each of the causes of death during the period covered appear not to be available. In a population in which the span of life is increasing, as it has in the United States during this century, gross rates tend to stress those causes of death that increase in frequency with advancing years. In the case of cancer, for example, in contrast to the apparent increase in frequency suggested by the gross rates, the age-adjusted rates have changed little, if at all, in recent decades, and have a value of about 125 per 100,000 population per year. (Source: American Cancer Society.)

Largely because of the discovery and development of useful measures for the maintenance of health and the prevention of death from disease at early ages, the span of life in the United States has been markedly increased during this century. At the end of the 19th century, life expectancy was only about 50 years; now it somewhat exceeds 70 years. This prolongation of life and the improved health of the people that has led to it can be attributed in large measure to advances in knowledge and their useful applications, many of which have been achieved as a result of research in the biomedical sciences.

Federal Support

According to reports prepared by the National Science Foundation (NSF–64–11), Federal support for all research in the biological, medical, and psychological sciences was estimated to amount to about $1.01 billion in 1963, and it was anticipated that this might be increased to approximately $1.11 billion in 1964 (cf. table 2). These sums represent somewhat less than 8 percent of all Federal expenditures for research and development, and somewhat more than 20 percent of anticipated Federal expenditures for research in all fields of science during these years. Relative to the life sciences, it was estimated that the total research obligations of Federal agencies would amount to about $200 million for biological sciences, $740 million for medical sciences, and $70 million for psychological sciences in 1963. It is not feasible to draw an acceptable line between the biological sciences and the biomedical sciences with the data that are available. In consequence, for the purposes of this report it will be necessary to consider that the data presented for the medical sciences represent a fair approximation of those that may be applicable to the biomedical sciences. The data given for the biological sciences are appropriately applicable to the report of Dr. Blinks.

TABLE 2.—*Federal support—Obligations for research*

Field of research	Estimates [1]		
	1962	1963	1964
Total research, all fields	3.2	4.2	5.8
Applied	2.1	2.9	4.1
Basic	1.1	1.3	1.7
Biological, medical, and psychological sciences	.84	1.01	1.11
Applied	.51	.60	.60
Basic	.33	.41	.51

[1] Billions of dollars.

Basic research in the biological, medical, and psychological sciences was expected to receive Federal support in 1963 in a total amount of about $410 million, and it was anticipated that this might be increased to approximately $510 million in 1964. These sums represent slightly more than 30 percent of anticipated Federal expenditures for basic research in all fields of science during these years. For the life sciences, it was estimated that the basic research obligations of Federal agencies would amount to about $140 million for biological sciences, $240 million for medical sciences, and $30 million for psychological sciences in 1963.

Federal obligations for basic research are made largely to educational institutions, and during 1963 and 1964 some 55 percent of Federal support for basic research in all fields of science is expected to go to such institutions. This serves to emphasize the prominent role that colleges, universities, and other institutions of higher learning play in the performance of basic research. However, it should be emphasized that only about 34 percent, or about $510 million, of these research obligations were to educational institutions as such, and that about 21 percent, or approximately $320 million, is expected to go to Federal contract research centers administered by such institutions.

Applied research in the biological, medical, and psychological sciences was estimated to receive Federal support totaling about $600 million in 1963, and it was anticipated that this might be continued at about the same level in 1964. These sums represent a little more than 17 percent of anticipated Federal expenditures for applied research in all fields of science during these years. Relative to the life sciences, it was estimated that the applied research obligations of Federal agencies would amount to about $70 million for biological sciences, $500 million for medical sciences, and $40 million for psychological sciences in 1963.

A large proportion of all research in the biological, medical, and psychological sciences is performed by colleges, universities, and research institutes. It is estimated that there are some 650 nonprofit and nongovernmental institutions that are engaged in research in these fields. The major agencies providing Federal support for these activities are the Department of Health, Education, and Welfare and the National Science Foundation.

Federal support for research in all fields of science, both basic and applied, during the period from 1957 to 1964 increased in an approximately linear manner from a total of about $3.3 billion to an anticipated $5.8 billion. This represents an average annual increase of about $310 million, or approximately 7 percent per year. When considered in relation to the increase in the cost of performing research during this period, particularly that for equipment, scientific instruments, and salaries, this average annual increase has not been sufficient to permit much growth in the total volume of the scientific research effort.

During the same period, Federal support for both basic and applied research in the biological, medical, and psychological sciences increased in similar manner from a total of only $130 million in 1957 to an estimated $1.11 billion for 1964. This represents an average annual increase of about $120 million, or approximately 30 percent per year. It should be pointed out, however, that this rate of increase declined to about 18 percent from 1962 to 1963, and is estimated to decline further to approximately 12 percent from 1963 to 1964.

In the light of the achievements of both basic and applied research in the biological and medical sciences, and the wide applications that have been made of these advances, the proportion of Federal support for research in all fields of science, i.e., slightly more than 20 percent, that is expected to be assigned to these fields during 1963 and 1964 could hardly be considered as too large relative to the importance of the human problems that need solution. It is estimated that the entire field of health involves total expenditures of about $36 billion per year in the United States, and employs approximately 4 million persons, or about 5 percent of the labor force. (Source: President's Commission on Heart Disease, Cancer, and Stroke.)

The Federal Government is currently supporting all categories of research in the biological, medical, and psychological sciences in an amount of about $1.06 billion, or about $5.50 per person per year. When this figure is considered in relation to the average per capita cost of the field of health including medical care, which now amounts to about $187 per person per year, for a total expenditure of about $36 billion, it seems obvious that it may be useful to consider whether Federal expenditures for the advancement of biological and medical knowledge, improvement of medical care, and reduction in the incidence and impact of disease are commensurate with the needs, hopes, and expectations of the public.

Manpower

The biomedical sciences include a considerable number of scientific disciplines that require and are best forwarded by persons who have received extensive professional education and training. In the great majority of instances, investigators who are concerned with the biomedical sciences have earned doctoral degrees, either the Ph. D. or the M.D., and in addition have had extensive post-doctoral training with established investigators before they are qualified and competent to undertake research as independent investigators.

The formal education and professional training that are needed for the development of competent and effective investigators in the several disciplines of the biomedical sciences are so extensive and lengthy that it is uncommon for them to be completed much before the age of 26 to 28. Usually an investigator requires 3 to 5 or more years of post-doctoral research experience to develop the skill and competence to qualify as a responsible scientist. In consequence, most contributors to knowledge in the biomedical sciences do not enter upon independent research activities until they are 29 to 33 years of age.

In most other major fields of scientific endeavor, the time, effort, and cost of formal education and post-doctoral training are less than for skilled and competent investigators in the biomedical sciences. This is due in large part to the diversity and complexity of biology and medicine, the innumerable interrelations between the tissues and organs that make up intact living organisms, including man, and the many variables that affect the results of studies with biological materials, including living cells. However, the amount of Federal support available for the predoctoral education of professional investigators in the medical disciplines that are required in much biomedical research is not large, and the amount that is provided for the development of scientists is heavily weighted toward those in Ph. D. programs.

Among Federal agencies, the National Institutes of Health is the largest single supporter of education and training for the biomedical sciences; obligations for 1964 are estimated to amount to about $189 million. The National Science Foundation provided approximately $3 million for the support of fellowships in the biological sciences in 1963. In addition, there are several loan funds, such as those provided under the Health Professions Educational Assistance Act of 1963, that provide needed aid to students of medicine and others.

In most institutions that are extensively engaged in work in the biomedical sciences, the professional staff concerned tends to be made up of about equal numbers of Ph. D.'s and M.D.'s. Such a ratio is commonly found in the departments of universities, medical schools, or research institutions that are largely concerned with biomedical research. This serves to emphasize the importance of efforts to correct the imbalance that presently exists in the amount of support provided by the Federal Government for the education and training of those in M.D. programs who aspire to and are capable of undertaking careers in biomedical research. The competent and effective professional scientist, regardless of the discipline in which he was trained or the field of science in which he works, represents a national resource of great value. Support for his education and development should not be wholly or even largely dependent on the uncertainties of personal or family resources. This is now commonly the situation for those who choose the route of the M.D. program to a career in biomedical research.

In 1964 the 87 medical schools in the United States awarded M.D. degrees to some 7,700 students and also sponsored approximately 400 students who received graduate training in medical school departments and were awarded Ph. D. degrees. In addition, it is estimated that, on the average, the medical schools provided research opportunities and special scientific training for only about 5 percent of medical students in M.D. programs. These two groups of students in either Ph. D. or M.D. programs represent only about 10 percent of the students associated with medical schools, but constitute a large and important fraction of the total number of investigators who ultimately carry on biomedical research.

It should be emphasized that clinical investigation, the study of disease as it occurs in man, requires specially trained physicians who constitute a considerable proportion of the investigators in the field of the medical sciences. In general the most competent and effective clinical investigators need extended experience in laboratory research in addition to their clinical training. Such persons must devote much more time and effort, preferably on a full-time basis, to the development of their professional competence than is required of other physicians. Despite their essential role in biomedical research, they are presently confronted with the need to find for themselves most of the funds required for the bulk of their years of medical education and post-doctoral clinical training. With relatively few exceptions, Federal support is provided only for training in research.

Numerous studies of the national needs for professionally trained scientists have emphasized the importance of attracting more young people of high intelligence and promise to both Ph. D. and M.D. educational programs. One of the most effective means of increasing the number of adequately trained and effective investigators would be additional Federal support for doctoral education and post-doctoral training. If the national effort in the biomedical sciences is to achieve its full potential, not only are more full-cost predoctoral and post-doctoral fellowships needed, especially for the M.D. and post-M.D. programs that lead directly to biomedical research, but also considerably more support is required for teaching and teachers as well as for the facilities that are essential for advanced education in the medical sciences.

A number of medical schools have already established special programs for selected medical students, which have as their objective the training of biomedical investigators. These developing programs should be fostered and extended to increase the supply of competent physicians qualified to undertake research in this field. Clearly the problem of selection of those students in medical schools who would merit fellowship support from the Federal Government for this purpose will require careful study. It seems probable, however, that, as in the case of predoctoral fellowships for graduate students in Ph. D. programs, such selections can be made satisfactorily by the faculty who are responsible for the special training programs in appropriate medical schools.

Facilities

There are problems as to the adequacy of facilities for biomedical research and in the fiscal affairs of many institutions that perform a large proportion of the biomedical research in the United States. The problems of staff, space, and support are intimately interrelated, and insufficiency in any one of these three resources affects the other two.

In general, the facilities that are available for biomedical research in colleges, universities, and research institutes that are in part privately supported are not as up-to-date or extensive as those that are available in institutions operated by the Federal Government or by industry. Federal support for the construction of new facilities or for the modernization of existing facilities is small relative to the need and in comparison with direct research support. In the case of most colleges and universities, such support is difficult to obtain because of the common requirement that matching funds for construction be provided by the institution. This situation is especially difficult in the university-operated medical schools, which are seriously deficient in up-to-date research facilities. This deficiency is widely recognized and accounts for the numerous efforts presently being made to obtain large private support for construction of new research facilities and improvement of existing ones. Not a few such institutions have considerably less than half the amount of laboratory space per person engaged in research that is generally accepted as optimal or efficient. Moreover, the research facilities that are available in institutions of comparable quality are frequently not comparable in quantity or effectiveness, and this imbalance appears to depend to a considerable extent upon the amount of private support that can be obtained.

Among the more important research facilities are the specialized libraries in which the records of past and present inquiries are available to investigators. Libraries maintain the organization of scientific and medical knowledge and insure ready access to that knowledge. As a means of effective research, the value of scientific libraries is at least as great as are laboratory facilities, instruments, and equipment. Yet library costs are not considered as appropriate direct expenses of research under Federal support and usually can be defrayed only in small part through "overhead" provisions. The Department of Health, Education, and Welfare obligations for scientific and technical information activities for 1964 are estimated to amount to about $36 million, or approximately 3.6 percent of its total obligations. However, less than $1 million is to go to the support of medical libraries.

It is estimated that there are some 990 specialized health-related libraries associated with universities, medical schools, and research institutions in the United States. Because of inadequate funds, the majority of these libraries are not in a position to satisfy the needs of investigators who require their facilities. This reduces the effectiveness of the nation's biomedical research program. Problems of adequate library support are aggravated by the restrictions on "overhead" that presently apply to grants provided by Federal agencies. As an indication of the importance that many investigators assign to this common problem, the several medical schools and major research institutes in New York City have joined together and recently established a medical library center designed to solve the mounting problem of library facilities, a problem that none of the participating institutions could readily solve by itself. It seems doubtful that such a solution can be widely applied, especially in smaller communities.

In view of the large needs of the public, the great advances that have already been accomplished, the certainty that still more valuable information and understanding can be acquired, and the enormous burden of disease, it appears evident that Federal support of research in the biomedical sciences should be continued and increased in amount. There are many competent scientists who think that the recent revolution in biology, which was initiated through basic research in genetics and has been advanced by research in the fields of virology and cell biology, has opened a new era in the biomedical sciences. There are good reasons to expect that this advancement in knowledge will have striking and prompt effects on solutions to the many human problems that result from ill health and disease.

In conclusion, it appears obvious that there are now tremendous opportunities for useful and widely applicable advances in the biomedical sciences, especially since the traditional separation between the physical and the life sciences is rapidly disappearing and science is coming to be recognized as a unifying whole. The immediate and the ultimate objectives of the biomedical sciences that are chiefly practical and humanitarian seem clearly to be within nearer reach than at any time in the past. Although it would be both presumptuous and irresponsible to attempt any predictions of things to come, it would not be in accord with the evidence to hold any other than an optimistic view of the probable future advances in this broad field. When or how effectively the major health problems of human beings may be solved no one can say, but it is certain that if a large effort is not made solutions will not appear. The practical problems of mental illnesses, cardiovascular diseases, cancers, viral diseases, congenital abnormalities, and numerous other ailments are old enigmas which, despite their antiquity, already show signs of yielding to penetrating scientific inquiry. To continue such inquiry should be considered an important obligation, both for those who are qualified and competent to carry them forward and for those who provide support for science. Already there are clear signs of a shortage of trained manpower for work in the biomedical sciences, and substantial public support for the education and training of a considerably increased number of investigators seems essential. The major advances that have been made in the last 20 years have been heavily dependent upon high-level public support for science; the need for such support will continue and grow until solutions are found for the remaining problems of human health.

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FEDERAL SUPPORT OF BASIC RESEARCH: SOME ECONOMIC ISSUES

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Summary

There is no necessary connection between leadership in basic science and leadership in the applications of science, because scientific progress is a cooperative endeavor and not a competitive game; indeed, there may be a conflict between basic research and applied science. The notion of "a position of leadership" in science raises questions of what leadership consists in and what its value is to the Nation. The two main arguments for government support of science are cultural-social, and economic. The cultural-social argument stresses scientific activity as a form of social consumption of wealth and raises the question of whether other uses of the resources employed would contribute more to the greatness of the society. The economic argument stresses basic scientific research as a form of investment for the future, and raises the questions of what the rate of return on such investment is and whether and to what extent government support is called for. Relevant economic research results bearing on this problem are scarce. Economic theory suggests that competition in the market will supply less than the optimal amount of basic research; but our society substantially supplements the market through private contributions to and government support of science, and the question is whether this supplementation is deficient or excessive. Here difficult problems arise, on which more economic research is needed; one of these concerns the relative merits of government and private support of science. The rule-of-thumb procedures generally recommended for determining the volume and allocation of Government support to basic scientific research generally ignore these problems, and amount to endorsing the present level of Government support or recommending that it be increased. Allocation of Federal support of science should take account of the possibility of stimulating the economic development of poor regions of the country by locating scientific research facilities in them.

Introduction

The questions put by the House Committee on Science and Astronautics to the ad hoc committee relate to the level of Federal support of basic research necessary to maintain U.S. leadership, and the allocation of that support among the various fields of scientific endeavor. These are questions of the allocation of scarce human and material resources among alternative uses, falling squarely within the scientific specialty of the economist, and it might be expected that an economist could provide definite quantitative answers to them. Unfortunately, the issues involved are so complex, and serious investigations of them by economists of such recent origin, that an attempt to provide quantitative answers at this stage would represent no more than an exercise of personal judgment. This paper makes no such attempt. Instead, it seeks to discuss certain questions of a kind that naturally occur to an economist confronted by the fundamental problems set by the House committee.

In discussing these questions, it is assumed that a meaningful distinction—albeit an imprecise one—can be drawn between "basic" and "applied" research. Basic research is conceived of here as research devoted to the acquisition of knowledge for its own sake, as distinct from research devoted to the elaboration and application of knowledge in the solution of practical problems or for the attainment of tangible results. Conceptually, basic research is concerned with adding to the stock of knowledge, and applied research with turning the stock of knowledge to practical use. The distinguishing characteristics of basic research, and the main source of difficulty in formulating public policy with respect to it, is the extreme uncertainty of both the contribution to knowledge that will result from a particular line of research, and the ultimate practical usefulness of that contribution.

Is Leadership in Basic Research Essential to Leadership in the Applications of Science?

It is a truism that the results of basic research must be available before they can be applied, so that basic research must come temporally before (and frequently has come long before) the practical application of its results. This does not, however, imply either that national leadership in applications of science requires leadership in basic scientific research, or that national leadership in basic scientific research will necessarily guarantee leadership in the advancement of technology or in the various applications of science and technology.

Given the international character of science, which makes scientific discoveries in one country available to scientists in all, and the fact that the value of a scientific discovery may not be apparent for a long time and is frequently contingent on other discoveries of an unpredictable nature, there is no reason to assume that leadership in basic scientific research and leadership in technology and applications of science are necessarily connected. A nation could achieve leadership in applications by drawing on knowledge provided by the basic research conducted by the scientists of other countries, confining its own participation in basic science to the minimum required to keep in touch with developments elsewhere and "fill in the holes" where necessary. Conversely, a position of leadership in basic research might benefit a nation almost exclusively in terms of the intangible prestige of scientific accomplishment, the concrete benefits of the application of scientific findings being reaped mainly by other nations. In that case, the expenditure of public money on the support of basic scientific research would serve mainly to save other countries the cost of basic research and enable them to concentrate on development and application; and the "leadership" so obtained might be largely leadership in the eyes of a relatively small group of scientists in other countries, plus those members of the general public who are interested in scientific achievement in the same sort of way as others are interested in international sporting competitions.

To put the same point another way, leadership in basic science is fundamentally different from other kinds of leadership. Scientific progress is not a conflict (like a game or a war) that a nation can win or lose. It is a cooperative endeavor in which the exertions of each benefit the rest, and in which leadership is measured by contribution to the general advance. Thus the nation that spends a comparatively large amount of public funds (relative to other countries) on establishing scientific leadership is spending its money to a significant extent for the benefit of other countries. This raises the obvious question of how far it is worthwhile to tax the citizens of one country in order to establish a position of leadership in an activity the benefits of which accrue to an important extent to other countries.

The cooperative nature of scientific progress, together with the increasing international mobility of scientists, also imposes limits on the extent to which a nation can establish leadership in basic science, and suggests that it would be courting disappointment to define the objective of scientific leadership in across-the-board terms, as some are inclined to do. It is inevitable that scientists of other countries will assume leadership in certain branches of science, and also that U.S. support of scientific research and training will have side-effects in strengthening other countries' scientific progress. Further, U.S. adoption of the objective of maintaining scientific leadership through governmental support of basic science is bound to provoke—and has already provoked—emulation by other countries desirous of establishing their own claims to world leadership.

The preceding paragraphs have argued that there is no necessary connection between leadership in basic science and leadership in technology and the applications of science. It is even possible that there is some conflict between them. This possibility is suggested by the broad historical generalization that, at least until modern times, human societies have been distinguished by superior attainment either in pure science or in technology, but not in both together (*1*). Even now it is frequently argued (for example, in recent discussions of British science policy) that concentration on basic research detracts from technological progress and the development of applications, by depriving applied science of the prestige necessary to attract able and ambitious minds. Others have argued the contrary, that concentration on applied research diverts manpower from basic research to the detriment of the latter in the short run and the former in the long run. This argument is forcefully developed in Professor Fritz Machlup's *Production and Distribution of Knowledge in the United States* (*2*), though it is questionable how far Professor Machlup's identification of basic research with university teaching and the production of graduate students is a valid approximation.

What Is Meant by "A Position of Leadership" in Science?

In view of the looseness of the connection between leadership in basic scientific research and leadership in technology and applications of science just discussed, it is necessary to define clearly what is meant by "a position of leadership" before it becomes possible to discuss Federal support of basic research as a means of achieving it.

In the writer's personal judgment, it would seem that in the past American leadership has rested primarily on two elements: the capacity to put science and technology to work in the service of raising the standard of living of the masses, and the capacity to mobilize science and technology on a massive scale in the development of superior armament. Both entail the capacity to produce to demand, and both reflect the wealth and efficiency of the U.S. economy. It has been the capacity to pay for and organize scientific and technical progress, in other words, that has established the leadership, rather than the scientific leadership that has established the capacity to perform the projects. The glory of the achievements reflects back on the scientists enlisted in them, of course, and on science in general; and it is natural enough for scientists to seek to claim the credit for the achievements, as a means of raising their status in the society and the amount of support they can claim, both from the Government and from private sources. The question, however, is whether scientific leadership would count for much if it were not allied with the economic and political power of the United States in the world, and what the United States acquires by expenditure on "scientific leadership" as such. It is true that the prestige of U.S. pure science has been growing rapidly—as measured for example by

Nobel prizes won and memberships in foreign scientific societies conferred—but one suspects that the prestige gained thereby is limited to a small and select audience—scientists themselves, and those members of the public who make a hobby of following scientific developments—and that the largest part of the audience is resident in the United States, so that most of the accomplishment of leadership is self-congratulation. It is often argued by scientists, nevertheless, that scientific leadership of this kind is of great importance in fostering international good will and cooperation through the personal contacts of top-level scientists of the various nations. This argument, however, when used in recommendation of Government support of basic science, raises the question of the value of this kind of good will in relation to the cost of the support of science necessary to generate the requisite number of top-level scientists.

To the extent that the position of leadership of the United States has been a reflection of its wealth and economic dynamism, there would seem to be no special argument for Government support of basic scientific research; rather there would be an argument for the pursuit of governmental policies designed to foster the growth of the American economy, the maintenance of a superior economic performance being sufficient, in this case, to maintain the U.S. position of leadership. To provide a case for Governmental support of basic research, and some standard for assessing how extensively such support should be provided, it is necessary to adduce some cogent reason why expenditure on basic scientific research produces a net benefit for the Nation that would not be enjoyed unless the Government assumed responsibility for such expenditures. Such a reason is necessary, because a free-enterprise economy normally trusts the processes of competition in the market to produce what is socially beneficial.

One alleged reason has already been mentioned—the contribution of contacts between top-level scientists to international amity and understanding; in the writer's opinion this is scarcely a cogent reason for governmental support of basic scientific research on any substantial scale. Two other reasons commonly advanced in current discussions are worth more serious consideration: one is cultural and social, the other economic.

The "Scientific Culture"

Much has been made in some quarters, especially among scientists, of the proposition that contemporary society is evolving a "scientific culture," in which the United States is the pioneer and the model for others to emulate. According to this view, it is the obligation or the privilege of the United States to support basic scientific research as a means of exercising leadership in the progress to a higher form of civilization.

The concept of "scientific culture" raises a number of questions, among which the most fundamental is the question whether basic scientific research is—in the economist's terms—to be regarded primarily as a consumption or an investment activity.

Every society devotes a portion of its current output to activities that increase its future capacity to produce (investment); but the bulk of its current output goes to supporting itself (consumption). Depending on its capacity to produce a surplus above the minimum needs of subsistence, it can devote more or less of the income not used for investment to activities that have no economic function—sport and recreation, leisure, meditation and scholarship, religious activities—or to the support of institutions and individuals specialized in the performance of those activities—in Thorstein Veblen's terminology, the leisure class (*3*).

Much of the contemporary "scientific culture" argument for Government support of basic scientific research is such as to put it—intentionally or not—in the class of economically functionless activity. The argument that individuals with a talent for such research should be supported by society, for example, differs little from arguments formerly advanced in support of the rights of the owners of landed property to a leisured existence, and is accompanied by a similar assumption of superior social worth of the privileged individuals over common men. Again, insistence on the obligation of society to support the pursuit of scientific knowledge for its own sake differs little from the historically earlier insistence on the obligation of society to support the pursuit of religious truth, an obligation recompensed by a similarly unspecified and problematical payoff in the distant future (*4*). At the more popular level, the interest in scientific accomplishment represents a leisure-time activity, more elevated than following professional sport and less culturally demanding than the appreciation of artistic endeavor, and hence peculiarly appropriate in the affluent mass society.

These comments are directed at clarifying the nature of the "scientific culture" argument. They do not necessarily imply that if basic scientific research is of the character of a leisure activity, it should not be supported by Government funds, or should be supported only to the extent that it promises to be instrumental in the achievement of nonscientific objectives in such areas as defense, public health, and so forth. Clearly, if the public is convinced that a scientific culture is desirable, it is perfectly appropriate for the taxpayers' money to be used to support scientists and scientific research. But to the extent that scientific activity is of the character of a consumption good (in the broad sense outlined above) its claims for public support need to be weighed against other pressing claims on the social surplus, such as the relief of poverty, the mitigation of social problems, the needs of the less-developed countries, or the claims of the average taxpayer to enjoy the personal disposition of the income he earns.

And the weighing obviously cannot be entrusted to scientists, or to any other beneficiary group: it must be the responsibility of Congress.

Insofar as cultural leadership in the world is concerned, there are several obvious areas—such as race relations, poverty, public health, and social security—where the expenditure of public money might well do more to establish America's image as the emerging society of the future than would equal expenditures on the support of science. Moreover, in a number of these areas there is no reason to believe that effective action requires a major preliminary program of basic scientific research: money applied with existing knowledge would suffice, because it is the nature of our political and social attitudes and institutions, not the backwardness of our social scientific knowledge, that is primarily responsible for the problems. With respect to poverty, for example, a major obstacle to more effective policies is not lack of knowledge of what causes poverty, but the belief that poverty is the poor person's own fault and that giving him money will sap his initiative.

Basic Science and Economic Growth

A great deal of stress is laid, in current arguments for Federal support of basic scientific research, on the importance of scientific progress to the improvement of productivity and the standard of living. Since the findings of economists on this question, such as they are, are easily subject to misinterpretation, it seems useful to provide a brief outline of their nature before turning to the economic argument for Government support of basic science (*5*).

Broadly speaking, economists concerned with economic growth conceive of the total output of the economy as being the resultant of various inputs of productive services into the production process, and seek to explain the measured growth of output by reference to changes in the quantities of inputs over time. The term "measured growth of output" embodies a limitation important in the present connection, since the methods of measurement of output largely fail to catch improvements in the quality of the goods and services produced, and such improvements are an important part of the contribution of progress in knowledge to human welfare. The procedure involves specifying both the inputs and the value of their contribution to output; any residual growth of output not explained by changes in input quantities is a measure of the contribution of factors not taken into account in the formulation of the relationships assumed to determine output. In the early stages of this type of research there was an unfortunate tendency to describe the residual as the increase in productivity of the inputs, and to identify it positively as the contribution of the advance of knowledge to increases in output—

particularly unfortunate as the early studies worked with very simple models of the production process and an extremely crude measure of labor input in terms of labor-hours without reference to skill, and for this reason among others produced residuals that were extremely high in relation to the total growth of input. Subsequently, the residual has come to be regarded as simply "a measure of our ignorance" and to be described as "the residual" rather than as "increase in productivity." Correspondingly, research on economic growth has aimed at improving the model of production and the specification and measurement of the inputs so as to increase the proportion of measured growth explained and reduce the residual.

The most comprehensive study of this kind is Edward F. Denison's *The Sources of Economic Growth in the United States and the Alternatives Before Us* (*6*), which attempts in particular to estimate the effects of changes in the quality of labor inputs associated with increased education and other changes, and to apportion the residual increase in output per unit of input among various contributing factors. Since it is the most comprehensive, it arrives at one of the lowest figures for the residual that economists have produced. Denison's figures ascribe approximately 20 percent of the growth of real national income from 1909 to 1929, and approximately 32 percent of the growth from 1929–57, to the increase in output per unit of input. For the latter period, somewhat over half of the increase in output per unit of input (just under 30 percent of measured growth) is ascribed to Denison's residual category labeled "Advance of Knowledge." This label is, of course, misleading, since the category is a residual that incorporates both any errors in the estimates of the influence on the growth of real income of changes in the factors explicity taken into account in Denison's analysis and the influence of all the factors not so taken into account. It is not a direct estimate of the contribution of "advance of knowledge," in any concrete sense of the phrase, to measured economic growth. Moreover, as previously mentioned, the measured growth of output fails to catch improvements in the quality of output, to which advances in knowledge make an important contribution. Nevertheless, imprecise in meaning and unreliable in magnitude as it is, the residual figure is the most careful estimate available of the portion of past growth that might be attributable to the growth of knowledge.

The growth of knowledge in question is the growth of all knowledge relevant to efficient production, managerial and organizational as well as technological and scientific. Denison further estimates that about one-fifth of the contribution of "advance of knowledge" to growth in the period 1929–57 can be attributed to organized research and development; and he calculates that the social rate of return on organized research and development is about the same as on investment in nonresidential capital. This in turn implies that the contribution of increased expenditure on research and development to measured economic growth would be small, and, more important, that there is no social benefit to be obtained from governmental measures to increase research and development activity. Denison's calculations are, however, no more than educated guesses; they do not include improvements in product quality, to which much of research and development is directed; the calculated rate of return on research and development could be much higher if research and development yielded its contribution only with a substantial lag (7); and there are reasons, elaborated below, for believing that resources are not allocated to research and development as efficiently as they could be.

Though the importance of the advance of knowledge to improved living standards is difficult to quantify, and the magnitude of the contribution of basic scientific research to the advance of productivity still more obscure, and though both may easily be exaggerated in carelessly formulated argument, there is no disputing that basic research has played a significant part in the growth of the U.S. economy. This fact by itself, however, does not constitute a case for Government support of basic scientific research, though scientists frequently write as if it did; the argument that it does is equivalent to arguing that, because part of the growth of output is attributable to population growth, the Government should subsidize births and immigration. In order to establish a case for Government support, it must be shown that basic research yields a social return over its cost that exceeds the return on alternative types of investment of resources. Alternatively, it must be shown that the amount of basic research that would be carried on in the absence of Government support would be less than what would be economically optimal. It is, incidentally, important to recognize that even without Government support some basic research would be carried on, as in the past—and probably on a much larger scale than in the past, owing both to the growth of interest in science and to the growth of wealth and the capacity to support scientific research through the universities and through privately supported research organizations. It is also important to recognize that Government cannot create additional resources for the economy, with which to support basic research; it can only take resources away from private individuals, who might prefer to use them for some other purpose, but would probably contribute some of them to the support of science if science is deemed socially beneficial. Government support means the difference between more and less, not between all or nothing at all.

From the point of view of economic analysis, research is conceived of as one form of investment of resources, the investment involving the use of human and material resources to acquire knowledge and the return resulting from the application of that knowledge to increase human welfare in one way or another. Normally a free-enterprise economy depends on the exercise of private decisions operating in the marketplace to decide on the total investment of all kinds and its allocation among alternative forms of investment. The market will arrive at a socially efficient allocation of resources provided that the risks undertaken by and the prospective returns open to the private decision-taker coincide with the risks and returns to society as a whole. These conditions are not fulfilled for private investment in research, and particularly for private investment in basic scientific research. The risk to the private investor in the creation of scientific and technological knowledge is greater than the risk to society, because the knowledge that results from the research may be useful to someone else but not useful for him, and the return to the private investor is likely to be less than the return to society as a whole, because the benefits to society cannot be fully appropriated by charging for the use of the knowledge. These divergences of private and social risks and benefits are by definition greater for basic scientific research than for applied scientific research; they are also smaller for the large diversified research organization or industrial corporation than for the small specialized research organization or company.

In consequence, there is good theoretical reason for expecting that, left to itself, the market would not only tend to allocate too few resources to research in general, but would also tend to bias the allocation against basic scientific research as contrasted with applied scientific research, and toward research in scientific areas related to the technology of industries dominated by large multiproduct corporations. This expectation seems to be substantially confirmed by the facts, especially those on the industrial distribution of research and development expenditure. A further relevant point is that, insofar as private appropriation of the benefits of successful research requires concealing the new knowledge from other potential users, the social gain from research is reduced correspondingly (*8*).

These defects of the market mechanism with respect to the allocation of resources toward and among investments in research imply that the market needs to be supplemented, and perhaps, with respect to basic scientific research, entirely replaced by social provision and allocation of resources for the support of scientific research. Our society does not, however, in fact depend exclusively on the market mechanism for decision on the amount and allocation of resources to be invested in. Instead, large amounts of money are channeled into basic research through the universities, through local and State governments, through private contributions, and through the Federal budget. The question then becomes, not whether the market system needs supplementation, but whether the degree of supplementation provided through existing nonmarket channels is adequate, too large, or too small, in relation to the economically optimum, and whether the resulting allocation of resources among rival fields of scientific inquiry is reasonably efficient.

To provide satisfactory answers to these questions, and therefore some firmer basis for answering the House committee's questions, would require calculations of an extremely difficult sort, probably impossible to effect with any reliable degree of accuracy—calculations that have so far not been attempted on any substantial scale (*9*). The difficulties are suggested by some of the questions that need to be asked: What have been the social rates of return on past investments in basic scientific research, for particular research projects, and on the average? How likely are particular proposed lines of research to produce new contributions to knowledge, and how valuable to society are these contributions likely to be in relation to their cost? (The assessment of the returns on specific projects requires an estimate of the likelihood of success, as well as of the value of success and the prospective cost.) How likely is it that if a particular project is not undertaken in the United States it will be undertaken somewhere else, and what net loss, if any, would there be to the United States from relying on scientists to carry it out?

These and similar questions relate primarily to the allocation of resources among research fields. With respect to the total allocation of resources to basic scientific research, questions of a different nature arise. One concerns the extent to which increased allocations of scientific personnel and supporting resources to basic scientific research would reduce the quality of the average research product; in other words, how rapidly do returns to research diminish?

The second concerns the relative extent to which increased expenditures on scientific research is reflected on the one hand in increased research effort and on the other hand in higher money costs; in other words, how far does increased expenditure on scientific research increase the quantity of research results produced, and how far does it merely bid up the salaries and raise the operating expenses of research personnel? Economists who have considered these questions (such as Machlup and Denison) seem to believe that the returns from increased expenditure on scientific research diminish fairly sharply for both reasons; more concrete evidence on these economic questions would be extremely useful.

A final question, of considerable relevance to policy-making, concerns the extent to which Government support and private support of basic scientific research are substitutes for one another, in the sense that larger-scale Government support for science tends to reduce the private support forthcoming, and conversely a reduction in Government support would elicit larger-scale private support. It is quite conceivable that the interest of the public (including business firms) in science, and its faith in the ultimate usefulness of contributions to scientific knowledge, together with the competition for excellence among the universities, would furnish the resources required (or a large part of them) on the alternative basis of private donations and fees. (Raising the funds for basic research in this way would, of course, involve substantial institutional changes.) Moreover, private support of science might have certain advantages over Governmental support, in that it might tend to produce a more flexible adjustment of support to the changing frontiers of scientific advance. That is, in science as in the production of commodities, a decentralized decision-taking process might produce a closer adjustment of supply to changing needs or opportunities.

The foregoing questions illustrate the kind of information that is necessary to judge the adequacy of Federal support of basic scientific research, from the economic point of view. In the absence of hard information or reasonably reliable estimates, any such assessment has to be an exercise in informed judgment and inference from scrappy evidence. In this connection, Richard Nelson has advanced an argument to the effect that the United States is probably not spending as much as it profitably could on basic scientific research (*10*). He reasons as follows:

> * * * if basic research can be considered as a homogeneous commodity, like potato chips, and hence the public can be assumed to be indifferent between the research results produced in government or in industry laboratories; if the marginal cost of research output is assumed to be no greater in nonprofit laboratories than in profit-oriented laboratories, and if industry laboratories are assumed to operate where marginal revenue equals marginal cost, then the fact that industry laboratories do basic research at all is itself evidence that we should increase our expenditure on basic research.

The key to the argument is the assumption discussed earlier, that the social benefit from industry research exceeds the benefit to the firm conducting it; the assumptions stated imply that the social benefit exceeds the cost in industry research, and that the same situation is true of nonprofit research. Nelson admits that the factual assumptions are extremely shaky, particularly with respect to the comparability of the research output of nonprofit and profit-oriented laboratories; and some doubt is cast on the argument by carrying it to its logical conclusion, which is that Government support of basic research should be extended to the point where no profit-oriented laboratories have any incentive to conduct basic research.

How Much Federal Support Should Be Provided for Basic Scientific Research, and How Should It Be Allocated among Fields of Scientific Endeavor?

The discussion of the preceding section has illustrated the difficulty of providing any firm guidance on these questions by drawing on economic analysis. In principle, the "scientific culture" type of argument for Federal support of basic scientific research requires that public opinion, as expressed through Congress, must decide at what point to strike a balance between supporting the scientific culture and using its resources for other desirable forms of expenditure. The economic argument, on the other hand, would require allocating resources among scientific fields so as to equalize the prospective social rates of return from marginal expenditure on each field, and fixing the total of resources allocated to basic research at the level yielding a marginal rate of return on all investment in basic research comparable to what is earned on other forms of investment, or else equal to the rate of interest at which the community is willing to forego the alternative of consuming the requisite resources (*11*). But since the information required to perform these exercises is absent, the principles can serve at best as a way of formulating decisions on the questions.

In the absence of any firm knowledge about the relation between the level and allocation of Federal support for basic science research and the magnitude of the social benefits obtained therefrom, there is a strong temptation to attempt to evade the issue by resorting to rule-of-thumb procedures based on the situation of the present or recent past. One such is the attempt to establish normative percentages tying expenditure on basic research to gross national product or to Government expenditure on major applied-science projects. The difficulties with this procedure are, first, that the percentages are usually derived from some base period, and there is no reason to expect the level in the base period to have been the right level; and second, that there is no reason to expect the correct relationship to be a constant.

An alternative is to ignore the question of benefits, and to approach the question from the science side: This is exemplified by the recommendation that adequate support should be provided for all qualified talent in the category of "little science" research, while political decision on priorities should be taken in the light of prospective cost in the category of "big science" research. This recommendation essentially amounts to taking the consumption view of scientific research: In little science, support everyone who demonstrates talent according to the scientific standards of his fellow scientists, and, in big science, decide how much society can afford to spend.

As regards little science, the approach just outlined evidently trusts the scientific community and the process of educational selection to produce a total and an allocation of expenditure not wildly out of line with governmental capacity to pay and the rough requirements of efficient distribution of scientific effort (efficient, that is, in scientific but not necessarily economic terms). Ultimately, it relies on the self-equilibrating processes of the intellectual market in ideas and the commercial market in scientifically trained labor to prevent serious misallocations. It should also be noted that the approach depends on a particular assumption about the supply of scientific talent: that there is a limited and fairly readily identifiable group in the population that is capable of acceptable scientific performance, and a sharp difference in ability between this group and the rest. This assumption does not make economic sense in any long-run perspective: One would expect the supply of potential scientists, like the supply of any other kind of skilled labor, to vary in response to the income and career opportunities offered. (Even if scientifically talented people were a fixed proportion of population, it would still be possible to increase the number available in the United States through immigration.) Consequently, any attempt to fix the total level of support on this basis implies a judgment that the present level of scientific activity (or something near it) is the correct one. Alternatively, if the principle is to offer support to everyone of competence who offers himself for a scientific career, the level of scientific activity will be left to be determined by the attractiveness of alternative occupations.

The Geographical Distribution of Support of Basic Science

In conclusion, it seems desirable to draw attention to a facet of policy toward basic science that is important but tends to be overlooked by scientists. This is the implication of the geographical distribution of science support for the pattern of growth of the U.S. economy. The location of scientific research activity in a particular city or region generally constitutes a focal point for the development of science-intensive industries in the surrounding area, and this should be taken into account in deciding on the location of such scientific activity. There is a natural tendency for scientific activity to agglomerate around established centers of scientific accomplishment; and this is probably the most efficient way of conducting scientific research from the point of view of science itself. From the economic and social point of view, however, and perhaps even from the longer run scientific point of view, there is a strong case for encouraging the development of scientific research centers in the more depressed and lower income sections of the country, as a means of raising the economic and social level of the population in those sections. Much of the poverty problem is associated with geographical concentration of high-income industries in certain areas and their absence from others, which makes migration the only feasible route to economic improvement. A deliberate policy of locating scientific research in the backward areas of the country to encourage their industrial development could in the long run provide a socially and economically more attractive attack on the poverty problem than many of the policies now being applied or considered.

This point, it should be emphasized, is independent of whether the Nation is spending too little or too much on the support of basic research, that is, of whether the beneficial effects described are worth their cost.

So long as public funds are allocated to the support of basic research, the geographical allocation of the funds should take account of the social effects of their expenditure.

References

(*1*) On this point see the comment by Thomas S. Kuhn on Irving H. Siegel's "Scientific Discovery and the Rate of Invention," in Richard R. Nelson (ed.), *The Rate and Direction of Inventive Activity: Economic and Social Factors* (Princeton, N.J.: Princeton University Press, 1962), pp. 450–57.

(*2*) Princeton, N.J.: Princeton University Press, 1962.

(*3*) Thorstein Veblen, *The Theory of the Leisure Class* (London: Macmillan Co., 1899). It should be emphasized that in this context "leisure" does not mean idleness or frivolous activity, but merely time free from the arduous and uninteresting tasks of producing a subsistence and available for the pursuit of nonmaterial interests.

(*4*) To an important extent, indeed, scientific research has become the secular religion of materialistic society; and it is somewhat paradoxical that a country whose constitution enforces the strict separation of church and state should have contributed so much public money to the establishment and propagation of scientific messianism.

(*5*) The following summary is only a broad sketch; for a more extensive survey, see Moses Abramovitz, "Economic Growth in the United States," *American Economic Review*, LII, No. 4 (Sept. 1962), pp. 762–82, a review article on the Denison volume referred to below.

(*6*) New York: Committee for Economic Development, 1962.

(*7*) Denison relates the contribution to growth to current research and development expenditures; since research and development expenditure has been growing rapidly, the rate of return would be higher if calculated on the basis of earlier (smaller) expenditures. On this point see Abramovitz, *op. cit.*, p. 780, n. 16.

(*8*) For more elaborate theoretical analyses of the economics of research, see Richard R. Nelson, "The Simple Economics of Base Scientific Research," *Journal of Political Economy*, vol. LXVII, No. 3 (June 1959), pp. 297–309; Kenneth J. Arrow, "Economic Welfare and the Allocation of Resources to Invention," pp. 609–26 in R. R. Nelson (ed.), *op cit.*; Dan Usher, "The Welfare Economics of Invention," *Economica*, N.S. vol. XXXI, No. 123 (August 1964), pp. 279–87.

(*9*) An outstanding example of the type of calculation required is Zvi Griliches' estimate of the realized social rate of return on public and private funds invested in hybrid corn research. (Zvi Griliches, "Research Costs and Social Returns: Hybrid Corn and Related Innovations," *Journal of Political Economy*, LXVI, No. 5 (October 1958), pp. 419–43.) Griliches estimates "that at least 700 percent per year was being earned, as of 1955, on the average dollar invested in hybrid corn research" (p. 419). He is, however, careful to point out that this was a successful research venture, and that the finding does not mean that any amount of expenditure on research is bound to be worthwhile. One of the limitations of economic research in this area is that it has tended to focus on cases of successful scientific research.

(*10*) *Op. cit.*, pp. 304–5.

(*11*) These two standards differ substantially, and there is an outstanding debate among economists regarding which is the more appropriate to use in assessing public investment.

LEADERSHIP IN APPLIED PHYSICAL SCIENCE

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Edward Teller ("The Role of Applied Science") makes the point that a prime problem in the maintenance of leadership in science and technology is leadership in applied physical science. He is certainly correct in pointing out that deficiencies in applied physical sciences result in the wasteful expenditure of huge sums of money. Perhaps a more important result of the lack of sufficient creative talent in applied physical science is to be seen in our difficulties in the formulation of imaginative defense and space programs. There can be no question of the accuracy of Dr. Teller's association of these deficiencies with our educational system. As he has pointed out, most of the superior Ph. D.'s coming from our universities have a primary interest in basic rather than applied science. I would like to discuss this problem and to propose remedial action.

Attracting superior students to applied science demands clear exhibition of its opportunities for leadership. The opportunities for leadership inherent in basic science are most dramatically illustrated by the profound consequences of great discoveries. Similarly, great inventions exhibit most clearly the opportunities for creative leadership in applied science. Leadership in science and technology is shared by invention and discovery, but the academic history of the two functions is very different.

Even when an important invention is made on campus, the center of interest quickly moves to industrial and/or Government laboratories charged with its exploitation in depth. Thereafter, although the university may continue to do (applied) research in the field, it cannot compete for leadership. On the other hand, the consequences of a discovery not having immediate practical implications will be elaborated on campuses, where the motivation to achieve practical results is not important. It is, thus, clear that there will be a vast difference between the stature of basic science and the stature of applied science at universities. While there can be little doubt that our universities maintain world leadership in basic science, there is similarly little doubt that leadership in applied sciences is primarily to be found in nonacademic institutions. In such an environment one cannot expect anything other than the prevalent attitude that applied science may be all right for those who cannot meet the standards set for pure scientists. This attitude presents an intolerable obstacle to the achievement of excellent university education in applied science.

The centers of progress in applied science are not integrated into our formal educational process. Therefore, although Ph. D.-level education in pure science centers around involvement in the great university adventures in pure science, this is not possible in applied science. Only by early experience in a climate of thinking in which applied science is the central interest and not a pale imitation of pure science, can a young person acquire the enthusiasm essential to great achievement in applied science. This climate can be found only in our great industrial and Government laboratories. It is only here that the tradition of the great inventors is maintained. In order to attract superior students to applied science, it is essential that during all portions of their higher education they have intimate contact with industrial and Government laboratories. For undergraduate and beginning graduate students, this can be accomplished by their spending summers and/or other periods of time as apprentices in outside research laboratories. A critical problem arises in connection with thesis research, which is, of course, the most important part of Ph. D.-level education. Traditionally, universities have depended upon Ph. D. students to provide the working force for university research and, thus, there is considerable reluctance to allow thesis research to be done off campus. However, this portion of the educational process is most important for people aspiring to applied research. It is here that value systems are most frequently set and it is in this period that enthusiasm for the creative aspects of applied research must be ignited.

Two possibilities for providing this inspiring atmosphere for students beginning their research careers present themselves. Either the great projects in applied science must be moved onto the university campuses, so that students can become an intimate part of them and so that university-applied science can enjoy an academic prestige more appropriate to its importance in the nonacademic world or, alternatively, students must be brought into the applied physical science programs conducted in Government and industrial laboratories. Attempts at the first alternative have resulted in friction with the university community, as could be expected with the introduction of the competitive system of values accompanying a great project in applied science. I believe, therefore, that the most likely opportunity available to us is to set up educational adjuncts as part of all our great laboratories of applied physical science. This type of educational arrangement is common in medicine and in other applied sciences, and involves cooperation between universities and institutions in which the applied responsibility is paramount (e.g., hospitals). The university continues to be responsible for maintenance of educational standards and the awarding of degrees.

Ample precedent has been set for the widespread implementation of this plan. Several industrial and Government laboratories have accommodated a few doctoral students engaged in thesis research; to meet our pressing needs, however, this program must be greatly expanded. Excellent industrial and Government laboratories must be authorized and financed to perform this educational function. This financing for educational purposes should be proportional to the number of students in training. It should operate with the freedom of choice of research area characteristic of grants to universities, maintaining the stipulation, of course, that the work be restricted to those fields in which the laboratory has already achieved excellence. This funding should be arranged to ensure freedom from deep involvement in large-scale programs that could place undesirable restrictions on thesis research. Typically, the thesis topics I have in mind would involve side investigations perhaps inspired by large-scale programs, or exploratory investigations that must precede large-scale programs. These theses would provide opportunity for especially talented students to relate themselves significantly to applied research projects, and thus, could provide a challenging and stimulating atmosphere for their thesis work. The thesis work should be conducted in the applied laboratories and under the supervision of senior people who engaged in major projects.

The machinery for awarding this type of grant already exists in the agencies of the Department of Defense that are engaged in supporting research, and in the National Aeronautics and Space Administration, the National Institutes of Health, and other Federal agencies. I would recommend legislation enabling these agencies to fund educational efforts in applied science to be conducted outside the universities with university cooperation.

It is essential, of course, that high standards of performance be set for this "external" education. First, contracts or grants should be awarded only to laboratories maintaining world leadership in areas of applied science and student activities should be restricted to areas of science in which that leadership prevails. Second, the thesis should be under the immediate supervision of senior laboratory personnel who have established reputations for achievement in applied science. Third, the awarding of degrees should remain under the control of the university faculties. Thus, examinations to qualify students for thesis work should be identical with those given to "internal" students, and the lecture course work in preparation for these examinations preferably should be presented on university campuses by either university or outside personnel. The final examination in defense of the thesis should be conducted before a committee representing both the university and the outside laboratory.

I believe that difficulties in the maintenance of superior standards in this "external" education will not be greater than in "internal" education. Certainly opportunities for the education of great numbers of excellent students in applied science can be created in this way. I am convinced that a plan of this sort is required for the maintenance of U.S. leadership in applied physical sciences.

FEDERAL SUPPORT OF BASIC RESEARCH

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In discharging its responsibilities for the security and welfare of the Nation, how much should the Federal Government spend on the support of basic scientific research? How should it divide these expenditures among the various fields of science? Answers to these questions, on which the advice of this ad hoc group has been sought, can take various forms, ranging from a discussion of first principles to a detailed and critical review of present programs with specific recommendations for increases and decreases, deletions, and additions. What is presented here is much nearer the first than the second of these extremes, but—I hope—with enough suggestions on how the principles could be applied to make it useful to legislators and administrators charged with responsibilities for practical decision in these matters.

To begin with, we define basic research as scientific effort directed toward an increase of our knowledge and understanding of the world, within the framework of science. This definition may be difficult to apply in particular cases, and often goes more to the attitude and purpose of the research worker than to objectively definable characteristics of the work. Experiments designed to measure the same characteristics might in one context be viewed as basic research, in another, as applied research or even as part of a development effort. A rough correspondence between purpose and institutional location is also useful as a guide in this respect, and basic research can for some purposes be largely, although not entirely, identified with what is done in universities and certain types of research centers, many of which are closely identified with universities.

The National Science Foundation defines basic research as that type of study "which is directed toward increase of knowledge in science. It is research in which the primary aim of the investigation is a fuller knowledge or understanding of the subject under study, rather than * * * with a practical application thereof." (1) On this definition total expenditures for basic research have risen from $432 million to $1,488 million in the short period between 1953–54 and 1961–62. Federal support has paid for about 55 percent of this total in recent years, while the rest has been financed by private industry (25 percent), colleges and universities (13 percent), and other nonprofit institutions (7 percent). Over the whole of this period, expenditures on basic research have averaged about 9 percent of total expenditures on research and development as reported by the National Science Foundation, and the Federal share of support of basic research has been substantially smaller than the Federal share of support for the whole research and development program.

In terms of performance, the basic research effort has been divided among institutions of different types as follows:

TABLE 1.—*Distribution of funds for performance of basic research*

	Average proportion 1953–56	Average proportion 1959–62
	Percent	*Percent*
Federal Government	11	15
Industry	35	30
Colleges and universities	47	47
Other nonprofit institutions	7	8
Total	100	100

Source: National Science Foundation, *Reviews of Data on Research and Development*, No. 41, September 1963, p. 5.

The fundamental justification for expending large sums from the Federal budget to support basic research is that these expenditures are capital investments in the stock of knowledge which pay off in increased outputs of goods and services that our society strongly desires. However, the nature of the payoff is such that we can appropriately view these investments as social capital, to be provided in substantial part through the Government budget, rather than private capital to be provided through the mechanism of the market and business institutions. Broadly, the payoff of basic research in the aggregate to the whole of society is clear, as we shall argue in some detail below. However, the fruits of any particular piece of research are so uncertain in their character, magnitude, and timing as to make reliance on the market mechanism to provide an adequate flow inappropriate. The market mechanism operates on the principle that he who pays the costs gets the benefits, and vice versa, and relies on an anticipation of benefits that is certain enough to justify the outlays required to realize them. The benefits of the kind of knowledge that basic research seeks are usually difficult or impossible to keep for a particular firm or individual. Indeed, the knowledge is often useful as it can be added to the general stock of scientific knowledge that is held in common by the community of those technically proficient in the relevant discipline. Thus a business firm which paid for a particular piece of basic research work could not, in general, prevent its result from being used by others. Further, the uncertainty as to just what would result, and when, and as to whether the useful purpose to which it could be applied would in fact be one that was relevant to the activities of the firm, would in general make expenditure in support of this work an unattractive investment. Finally, several of the kinds of payoffs from basic research relate to outputs that are already the product of Government activity, rather than of business operating through the market mechanism.

We can distinguish at least four different kinds of benefits to the community that flow from basic research. First, it is a major input to the advance of applied science and technology, from which there flows continuing growth in our military capability, our health, and our productive capacity. This point is obvious and needs little elaboration. But it is worth reminding ourselves that the relation between input and output is an elastic one. The relationship of the whole revolution in military technology, which began in World War II and is still continuing, to advances in basic science of the preceding generation has been discussed at length and frequently. In medicine, we can mention the practical therapeutic fruits of research on vitamins and hormones carried on by physicians, biochemists and physiologists. In industry, we can compare the history of transistors, on the one hand, with that of neon and fluorescent lighting on the other. In the first case, the passage from basic research to wide industrial application was unusually rapid; in the second, more than 50 years passed between the first systematic scientific examination of the phenomena of electric discharge tubes and fluorescence, and their practical applications in lighting. An even longer gap, and a much less predictable set of applications, is exemplified by the period that lay between Cayley's development of matrix algebra, and its use in such diverse fields as aerodynamics and the analysis of communication networks.

Second, there is the intimate relation between the conduct of research and the provision of higher education in science and technology. Trained scientists, engineers, and doctors are needed in increasing numbers to operate the apparatus of society in defense, industry, and health, as well as to continue the stream of improvements in that apparatus that we have experienced in the past and expect in the future. The training of these specialists is increasingly carried on in close connection with the conduct of both basic and applied research. There is wide agreement among both the consumers and producers of specialized scientific and technical training that an intimate relationship between research and teaching in these areas is necessary, and that the best centers for training are those that provide this connection. This is a requirement for the support of research that would exist even in the absence of a useful application of the knowledge that the research produced (*2*).

Third, experience shows that an applied research and development effort, undertaken with the purpose of solving specific practical problems, benefits from a close relation with basic research. This is true both in general and in the individual research laboratory. The whole body of scientists and engineers in applied research establishments—whether in defense or industry or medicine, private business or government—do their job of problem-solving more effectively when they are in contact with scientists undertaking basic research in areas that underlie their particular problems. Many industrial laboratories have found this to be true by experience, and either incorporate basic research groups or try to achieve the same effect by visiting and consulting arrangements with university scientists. In overall terms, there appear to be no exceptions to the proposition that nations with strong capabilities in applied science and technology have strong capability in basic research; though the association does not necessarily hold in the reverse direction.

Finally, the corps of scientists working on basic research represent an important reserve of capability in applied research and development that can be drawn upon when national needs dictate. Our experience in World War II showed the tremendous reliance placed on so-called scientists in military research and development. This was true not only in nuclear weapons, but also in radar, sonar, proximity fuses, and other critical fields of military research. The talent of the superior scientist lies to a large extent in his ability to make conceptual inventions and in nuclear weapons, but also in radar, sonar, proximity fuses, and other functioning devices. These are precisely the talents required to make large forward strides in technology in a short time. Indeed, but for the stimulus to American science created directly and indirectly by the inflow of refugees from Europe in the 1930's, it would not have been possible for us to do all that we did do during the war. If we allowed basic research to sink to the level represented by what might be paid for by business and educational institutions out of their own funds, we would be deprived of much of this reserve. In the future, we could envision circumstances in which we might wish to draw on this reserve capability for other purposes than military needs; indeed, in the space field, and to some extent in connection with problems of civilian technology and assistance to developing countries, we can see some examples of this kind already.

The foregoing classification of the kinds of benefits that basic research can be expected to provide makes clearer why this activity qualifies for support from the Government budget. Of the benefits listed above, those relating to military capability fall directly within the sphere of Federal responsibility, and only the Federal Government can and will pay for them. This applies both to military requirements for applied research and development, and to the insurance value of the scientific reserve corps. Those relating to health are increasingly an area of social concern, in which governmental responsibilities are recognized. The same can be said of those relating to higher education. It can be argued that beneficiaries of services should pay their full costs in both higher education and health. However, this is not the direction that public policy appears to be currently taking.

Thus only two classes of benefits are potentially the basis for support through the market system: The value of research outputs as inputs for technical developments of direct value to business firms, and the value of basic scientists as stimuli to the better functioning of scientists and engineers working directly on applied research and development projects in the same laboratory. (So far as the latter are involved in defense and related enterprises, this too is a matter of Government finance.) On the second count, we may say that, by and large, the market system will work so as to provide for the support of a level of basic research activity appropriate to that purpose taken in isolation. On the first, as we have seen already (p. 2 above), there are good reasons for expecting that business firms, acting individually, will systematically underinvest in basic research to a substantial degree. These reasons—the difficulty of appropriating the benefits of basic research to any single firm, and the uncertainty in the character, magnitude, and timing of the payoff in new technology of the fruits of any particular piece of basic research—are not absolutes; they are rather a matter of degree. The longer the time horizon over which a particular business can look ahead, the broader the scientific basis of the technology underlying its processes and products, the more its activities cover the whole range of that technology, the less its position in the markets in which it operates is subject to competitive inroads, the more likely it is to invest in basic research. Thus the relatively few firms that make large investments in basic science—outside those financed through defense contracts in any event—are those like Bell Telephone, General Electric, Du Pont, Standard Oil of New Jersey, and the like. Indeed, to a significant extent, the competitive positions and prospects of these firms are such that the question of whether it pays to make these expenditures is not one which they need face too sharply. But for the generality of firms, the extent to which such expenditure appears wise is limited.

It might be possible that private support for basic research could be given through tax-exempt foundations and the like, rather than in business motives. However, in recent years the level of foundation support

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for basic research has been small: less than $50 million per year, with medicine and biology getting almost half the total, and the social sciences another quarter (*3*). Even with a large cut in Federal taxes, it seems unlikely that the scale of such spending could rise to levels at which it could displace substantial Federal spending. Cuts in tax rates would in general reduce the incentives of wealthy taxpayers to make large gifts to foundations. Further, foundation spending is unlikely to have the continuity and flexibility required to support programs that involve both large expenditures for facilities and large continued operating costs—for example, the giant installations for high-energy physics. To the extent, of course, that these anticipations proved incorrect, there would be an argument for a lower level of Federal spending.

Of course, these separate categories of benefits are not independent products of separate research activities designed to serve each particular end. Rather they are joint products of the level and distribution of activity in basic research as a whole. Thus, while the categories are analytically helpful in thinking about the why of the support of basic research, they do not provide a set of individual measures of necessary levels of research effort of various types to be added together to arrive at a total.

It is sometimes argued that the point made above on the difficulty of appropriating the knowledge that basic science produces, and indeed the great dependence of the usefulness of the knowledge on its being incorporated into the common pool of "science," is applicable as between nations just as it is as between business firms. Therefore any individual nation may not gain by trying to put "too much" into the pool, since it can draw on the whole pool, not only its own output. The international character of the pool is clear. A small nation, with limited resources both in wealth and trained personnel, might well be mindful of this in planning its own science policy. For the United States, however, which is relatively large in terms of both economic and scientific activity, this possibility is not a very fruitful one. Our own contribution to the stock of basic knowledge, especially in the period since World War II, has been so great that we cannot simply act as if the total were given independently of our own actions (*4*). Further, this argument is relevant only to the value of basic science as an input to applied science and development. As far as the other values discussed above go, the extent to which they can be realized is more or less proportional to the extent to which we ourselves are carrying on the research activities from which they flow. Further, even though the individual scientist in basic research works in a relatively small group, the broader intellectual community within which he lives affects the quality of his own work. A large country with a great effort in applied science and technology that tried to rely on borrowing in respect to basic science would find that the quality of its own work deteriorated. Such facts as are available suggest that the U.S. research and development investment in nonmilitary fields is about the same proportion to national income as that of the other major industrial countries. The much larger expenditure on defense research of the United States makes the total figure for research and development higher in relation to national income than for the other countries. Only the U.K. figures show a breakdown that gives basic research separately; their proportion of basic research to total is somewhat higher than that of the United States: 11 percent versus 9 percent (*5*). These figures suggest, but certainly do not show, that we are neither heavy borrowers not heavy lenders in the international scientific balance.

The argument so far has been couched entirely in instrumental terms. The value of basic research has been assessed in terms of other goods, for which it is a necessary input: military strength, health, economic growth. This is a narrow view: scientific research can be viewed as itself a desired end-product in at least two different ways. First, it may be a significant separate component of national power in our nationalistic, competitive, less-than-orderly world of many nations. Second, it is an esthetically and morally desirable form of human activity, and the increase in this activity is itself a proper measure of social and national health. I myself—as might be expected of an academic—share the second view. I am skeptical of the first, since I believe that the politically significant element of prestige which rests on excellence in science is related to the military and economic significance imputed to scientific leadership. Nonetheless, I think it is unnecessary to debate the merits of either of these views, since the investment or instrumental aspects of basic research are in my judgment of sufficient importance to provide a basis for policy judgment independently.

None of the arguments above that justify Federal support for basic scientific research provide in themselves a measure of what level of expenditure is necessary or desirable. Indeed the nature of the arguments themselves is such as to make it impossible for any precise payoff calculation to be made. In sum, they say expenditure on basic science is investment in a special kind of social overhead—knowledge and understanding—that contributes directly and indirectly to a wide variety of vital social purposes. It is in the very nature of an overhead that a nice calculation of the "right" amount to expend on it is difficult. While we could conceive a level of research activity so small that education and applied research began visibly to suffer, and equally, we can conceive a flow of funds so generous that they would obviously be wastefully employed, the limits between the two are very wide.

In the absence of more specific bases of calculation, the usual method of budgeting for an overhead item is to allocate to it some share of the total to which it seems most relevant. In this case, we might assign to basic research some fraction of the total expenditure on applied research and development, on the ground that this is the major item to which basic research is an overhead. In fact, the past growth of basic research figures as reported by the National Science Foundation has been closely parallel to the growth in total expenditures for research and development. The share of basic research in the total has fluctuated between 8 and 10 percent. Thus one method for solving the problem is simply to continue to allocate about 9 percent of the total expenditure of research and development to basic research, and continue to provide the same proportion, or about 55 percent of this amount, from Federal sources.

An alternate method is to look to the fact that it is ultimately brains, and not money, that is the limiting factor on the size of the useful national research effort. The budget for basic research could then be set in terms of the level required to support the research activity of all those with proper training and an appropriate level of ability, with the share provided out of Federal resources again to be determined on customary grounds. Let us put aside for the moment the thorny question of what is the appropriate level of ability, and the equally thorny one of who determines its presence or absence in particular cases. It is worth noting that the intimate tie between research and advanced training means that the results to which this method would lead would not be unrelated to those arising from the previous one. Any given rate of growth in the total applied research effort requires a corresponding rate of increase in the pool of scientists and engineers who perform it. If these are trained at the institutions that do a major part of all basic research, as they are now, and in such a way that training and research are complementary, then the size of the basic research effort required to finance the activity of the teacher-researchers is related to the rate of flow of the scientists and engineers they are called upon to train. But, as a practical matter, this approach does not provide an easier way to calculate the proper level of Federal support for basic research than the "overhead" method suggested above. The problems of defining the levels of competence and training that qualify a man for support are great. Teaching and research are not strictly complementary, and there is no fixed ideal ratio between them that can be applied to every institution and every branch of science. Finally, future demand for graduates with scientific training is necessarily uncertain, and basing present basic research budgets on projections of these demands may tend to introduce an element of inflation in the estimating process.

For these reasons the overhead approach appears more useful. In applying it, two kinds of problems must be borne in mind, which will make it necessary to review the appropriateness of the particular overhead percentage from time to time. First is the fact that at any moment, the composition of total expenditures on basic research in terms of investment in large new facilities, their operating and maintenance expenditures, and expenditures on personnel and current support may be such as not to permit maintenance in the future of both the particular total and the particular composition. The creation of large new facilities may build in a requirement for operating and maintaining them of such magnitude as to require either more expenditures or a cutback in actual research effort. Second, the overhead ratio appropriate to a rapidly rising level of total expenditures may not be the one appropriate to a more slowly growing one. This caution is especially relevant to the situation we may face in the middle future, as the rate of growth of expenditures in applied research and development slackens off. At that time, a part of the adjustment process to such a change may well be a temporary increase in the overhead ratio for the support of basic research. Since it is my aim to sketch general principles and indicate how they might be applied, rather than to formulate detailed programs, I leave further discussion of these points, both of which are important, and potentially complex in detail.

From the considerations and procedures that should govern total expenditures on basic research, we turn now to those that should guide its allocation among the various fields of science. Here again, one is tempted—and no one more than an economist—to think in terms of payoffs. Surely the relative amounts to be spent on mathematics and biology should bear some relation to the relative values of the results to be expected from these different kinds of work in terms of the goods we have already identified: military power, health, economic welfare. But the same problems arise that we have come up against in attempting to associate basic research in aggregate with payoffs in aggregate. Indeed, there is a sense in which it is both harder and easier to try to identify what field of scientific activity will produce results in what practical field. It appears obvious in some sense that research in biology will produce more results, ultimately, in the field of health than research in, say, mathematics or physics. Yet it can just be the case—and sometimes is—that a new statistical concept, through its application to genetics, or a newly observed and explained physical phenomenon, through its translation into a new kind of observational instrument, may advance our understanding of the mechanism of a particular disease more than current biological efforts which appear more directly related to it. Nor is the connection just a one-way relation between more and less "basic" sciences; problems in a less "basic" science may give rise to ideas or methods that in turn illuminate more "basic" sciences. While, as we shall say a little later in the argument, there is reason for maintaining some kind of connection between area of research and area of application in the allocation process, a direct approach through an attempt to use estimates of relative payoffs as the basic allocative criterion is unpromising. It appears wiser to proceed by indirection.

In our consideration of the allocation of research effort among different fields of endeavor, it is clear that there is an important question of the degree of detail in which we examine this question. At one extreme, it is perfectly clear that the most sensible decision on how a particular scientist divides his own effort among various pieces of research will in general be the one that he makes himself. It is also clear that if we wish to decide a little broader question, say the relative importance of two kinds of experiments in high-energy physics, we will do it best by seeking to get a consensus of opinion among the competent workers in the field. At the other end of the scale, it is probably unlikely that we can determine how much effort should be given to the study of physiological mechanisms of drug action, on the one hand, relative to, say, the distribution of X-ray-emitting loci in the galaxy, on the other, by getting a consensus among pharmacologists and physiologists and astronomers. Conceptually, there is some "natural" dividing line between the allocations of effort within a field, which ought to be settled by some mechanism drawing chiefly on the judgment of competent workers in that field, and allocations among fields, which clearly require a different process, and cannot simply be left to the judgments of scientists, which in this case will hardly be scientific judgments. A field can be defined by the ability of the scientists within it to communicate in terms of a common language, assisted by their having undergone similar educational experience, etc. The present organization of the National Science Foundation is now based on one division of basic science into fields, and, pragmatically, this appears the appropriate division with which to begin for our purposes. This is the level of detail for which we wish to formulate allocational principles.

We have one other point to consider before we can formulate these principles, and that is the distinction between what have been called "big science" and "little science." "Little science" can be defined as the current support of ongoing work of individual scientists and small teams of scientists, working within existing institutions, and involving expenditures on the salaries of people, laboratory supplies, laboratory equipment of a "small" sort, travel, etc. "Big science" involves the construction of large new pieces of scientific equipment and the auxiliary facilities that go with them, such as accelerators and radio-telescopes; or the creation of new institutions for research, sometimes in connection with such facilities, sometimes for other reasons. "Big Science" expenditures also include the annual maintenance and operating costs of the specialized facilities, which in some cases—oceanographic vessels, for example—are of the same order of magnitude as the capital cost. Another way of making the division is to say that "little science" expenditures come in packages of \$100,000 or less, while "big science" expenditures come in packages of \$1 million or more, and may run into the tens or hundreds of millions, as is the case with the Mohole and the Stanford linear accelerator (*6*). Two different allocation mechanisms are offered below, one for "little science" and one for "big science." There is also the problem of the proportional division of the basic research total between the two, and some observations directed to this question.

In attacking the "little science" problem, we return to our earlier observation that it is the supply of brains, not money, which is the true constraint on the amount of useful work that can be done in any field. Accordingly we propose that total funds for "little science" be allocated among fields in proportion to a set of numbers which, for each field, are a function of the stock of scientists in the field, and the rate of change of the stock:

$$P_i = f[N_i|N,\ d(N_i|N)dt]$$

P_i is the proportion of total funds spent on basic research in the i-th field, N_i is the number of scientists in the i-th field, and N is the total number of scientists doing basic research.

It is worth asking how different this new scheme would be from what we have been doing without any explicit formula. To answer this question, an attempt was made to fit a function of this type to data for 1960 and 1962, the only years for which both funds for basic research divided by field, and estimates of the stock and flow of scientists by field, were available. The form of the function used was:

$$P_i = b_i N_i^a M_i^{1-a}$$

where P_i is defined as above, N_i is the proportion of scientists doing basic research in the i-th field, and M_i is the average proportion of new Ph. D.'s in the i-th field. The results, which show neither that, whatever its inner mechanism, the current system of allocation produced the results of our proposed system, nor that it produces something radically different, are given in the Appendix below.

The parameter b_i can be interpreted as the average cost of keeping a scientist employed for a year in the i-th field of basic research, exclusive of capital expenditures. The parameter a can be viewed as a policy variable, chosen to reflect the relative weight to be attached to the size of the current intellectual resources in a field on the one hand, and the attractiveness of the field to new men on the other. The latter is a useful measure of the intellectual liveliness of a field, and, by giving the parameter a lower value, more emphasis can be placed on current vitality than on past history. As the function has been defined, a is the same for all fields. This is not necessary. Separate parameters for each field could be treated as policy variables, and more emphasis given to research in particular fields, to reflect broad judgments about payoffs. However, the opposite tack, of holding the parameter constant as between fields, has the virtue of greater objectivity, and relies simply on evidence of intellectual vitality given by the volume of production of new Ph. D.'s as the determinant of proportionate emphasis among fields.

Application of a formula of this type requires definitions of eligibility for inclusion in the count of the total stock of researchers and the flow of new Ph. D.'s. These are problems with which we already deal, and for which the existing machinery of scientific advice is suitable.

It is worth emphasizing that the formula device is proposed solely as a means of guiding the gross allocation of expenditures among fields. Within that total, something much like the present mechanism for determining the support of individual research projects and programs can operate. The National Science Foundation, the National Institutes of Health, the Department of Defense and the constituent military services, the Atomic Energy Commission, and the other agencies of Government supporting basic research would continue to rely on the advisory-committee mechanism to review and support individual projects. It would, of course, require that there be some coordinating mechanisms that kept the actions of the individual agencies consistent with the total allocation. The present machinery of the Office of Science and Technology, the President's Science Advisory Committee, and the Bureau of the Budget appears to be capable of performing this task.

There is a positive justification for keeping the funding of basic research divided among a number of mission-oriented agencies, in addition to the National Science Foundation, in addition to the wisdom of recognizing the inertia of existing arrangements. While the whole burden of our argument has been to urge the impossibility of allocating resources to basic research on the basis of calculations of payoff, the funding of projects and programs by agencies responsible for particular practical activities does inject an element of project selection from the point of view of the interested agencies. While the Office of Naval Research has supported a variety of research projects in mathematics and physics, it is likely that its selection of particular projects for support might differ from that of the National Aeronautics and Space Administration or the Atomic Energy Commission in ways that reflect the ideas of scientists and administrators conscious of the Navy's problems about what research might be fruitful. This is not to say, however, that the present set of Government agencies with the power to make independent decisions to support basic research is just the correct one. While military support for research is subject to some centralized review, and is divided among four or five major sources, the agencies representing the concerns of the civilian economy are much less unified, and might conceivably benefit by being more so with respect to this problem.

In this scheme, the National Science Foundation would play a balancing role in two different and equally important senses. Its support of particular fields might be the most flexible, to compensate for the narrower view that each particular operating agency supporting basic science might be inclined to take about what kind of basic science it should support. It will also be providing balance in a more fundamental sense, since it is not selecting activities to support in terms of the perspective of any particular operating mission, but solely in terms of the perspective of science itself. Whether the Foundation and the mission-oriented operating agencies are now providing the right proportions of total Federal support for basic research is a question worth asking, but difficult to answer. To the extent that the Foundation can be said to support the investigation of problems that arise from the side of science, while the other agencies are supporting investigations of problems that originate from the side of application, an argument might be made that the present Foundation share of the total—about 10 percent—is too small. It is certainly too small to permit the Foundation to act as an effective balancer. Further, no matter how arbitrary the distinction is at the margin, there is an element of real importance in it. Thus an increase in the National Science Foundation proportion to as much as a third might well have favorable effects, and at worst would make no difference.

Because expenditures in "big science" come in much larger lumps, and fluctuate in their concentration in particular fields at various times, the kind of formulation adopted for "little science" would not be applicable to the problem of dividing these expenditures among fields. Such expenditures can be divided into two quite different kinds, as we indicated above. The first is the purchase of large new research instruments and the costs of maintaining and operating them, whether the first of their kind or not, which are necessary to the performance of particular kinds of research. The instrument may be an accelerator, an oceanographic vessel, a radio telescope, or a large-scale computer. The second kind of expenditure relates to the creation of a new center for research, which involves the construction and equipment of laboratories. Here again, maintenance and operating expenditures must be included. In some cases, the new center is closely related to the new instrument; in others, it might contain no particularly large instruments. An example of the second kind might be the finance of a laboratory for molecular biology in a medical school that hitherto had none, or even the finance of a series of laboratories, to constitute a research center at a university that was hitherto weak in the whole area of research (7).

We propose an administrative mechanism rather than an allocation formula for dealing with the allocational problem in "big science." The mechanism has three elements. First is the proportion of total basic research expenditures to be available for "big science" in all fields.

In principle the chosen proportion should be an upper limit, which should not be exceeded, although it need not be spent in every year. It may, however, be desirable to permit "borrowings" against the future in particular years, so that a year in which the limit was exceeded would be followed by a year or several years in which it was not reached. Such a ceiling would control the tendency to build more large facilities than there are people to use them effectively, a tendency likely to develop in the absence of a direct economic check on the "productivity" of the facility, which we do not have.

Good figures on what this proportion has been in the past are not available. Some guesses by knowledgeable scientists suggest that, properly measured, it has been in the neighborhood of one-half.

The second element of the mechanism is a committee of representatives of the various sciences and the funding agencies, meeting each year to scrutinize the whole list of proposed expenditures and evaluate their competitive merits, in relation to available funds. The evaluation process in practice could be expected to involve a fusion of two elements. The first is an element of professional judgment as to the "ripeness" of particular projects, in terms of their ability at that time to make a substantial contribution to the forward progress of the particular science in which they were to be used. The second is an element of political compromise, so that neither any single science nor any single group of closely related problems is the recipient of the lion's share of the funds over any period of years. This political element is necessary. In the absence of an objective standard for judging whether, say, particle physics ought to be developed faster than radio astronomy, and in the presence of a budgetary constraint, the allocational decision must inevitably represent somebody's preferences or prejudices. The present proposal incorporates an explicit mechanism for registering the preferences and prejudices of those who are both affected and knowledgeable. The mechanism is broadly a representative one, though informally rather than formally such, and similar in character to consultative organizations in other areas, on which the Government relies for advice with respect to decisions affecting the interests of particular small groups in the society. The result of this process would be an approved list of projects, which typically would involve funding commitments over a number of years, and which in total would fall within the limits of the formula, with the exceptions set forth above.

The user representatives on the allocating committee might include people drawn from the President's Science Advisory Committee, the

National Science Board, and the outside scientific advisory groups to the various operating agencies; while the funding agencies might be represented through the Office of Science and Technology. Behind this top committee might well be a structure of appropriately chosen subcommittees screening the requests from each field of science. Special ad hoc arrangements might be needed from time to time to deal with proposals cutting across traditional fields. Here again, there is experience to build on. Consideration of the support of new research centers, as opposed to new instruments, might also make it appropriate for the U.S. Commissioner of Education to be represented on the final allocating committee and in the structure of subcommittees as well.

The third element in the mechanism would be a "tax" placed on the "little science" expenditures in each field to support part of the costs of any facility or center built in the field. If the "tax" were set at 20 percent, for example, the overall "big science" budget would be set at a level expected to cover 80 percent of the approved expenditures. The other 20 percent would then be provided by the "tax" on the "little science" budget of each relevant field in respect to 20 percent of the cost of each approved facility. This cost-sharing arrangement would appear as another useful administrative control device, directed toward making those representatives of any field not themselves too directly concerned with using large facilities sensitive to their costs in terms of their own interests. Without some such device a solid-state physicist, for example, might feel that, although he himself is not concerned with a particular accelerator, the money might as well be spent on it rather than on, say, a large facility for biological research. The figure of 20 percent suggested here is obviously arbitrary, and either further analysis of past action or experience might suggest a different figure.

There are arguments to be made against the "tax" device. Scientists in a part of a field that does not rely on large instruments, e.g., solid-state physics, may view reducing the funds available to them in order to benefit the high-energy physicists as unjust. Further, the interests of the one group may be so distant from those of the other that the "tax" may not be an effective constraint on the decisions it is intended to influence. On the other side might be set the corporate sense of responsibility that continuing advisory committees tend to develop, even though their membership changes. On balance, the proposal appears to the writer to add something useful to the allocation mechanism. The mechanism could work without it, however, relying simply on the ceiling on new commitments to "big science" as a control.

It is equally true that determining the total budget for "big science" on the basis of experience is arbitrary. But there appears to be no other basis, and the quota would obviously need revision in the light of both experience and changing techniques in science. It is clear, however, that the question of how fast we should try to make progress in particle physics—assuming for the moment that this is the same as the question of the pace at which we should construct new and larger particle accelerators—is not susceptible of any single answer. On the other hand, the question of how useful it is to try to make progress in this field in comparison with some other can at least receive a reasoned discussion. The proposed procedure focuses attention on questions of the second type rather than the first.

So far, we have been concerned with the allocation of Federal funds, and have assumed that non-Federal funds will continue to flow in the same proportion as at present. The major outside source of support is business, which currently finances about 25 percent of all basic research. In the near future, two opposite trends may be expected to affect the size of business activities. More firms will find that their research and development activities are growing to the size where some support of basic research appears desirable or possible; and thus, for this reason, some growth of basic research in relation to total research and development activities financed by business can be anticipated. On the other hand, any decline in military procurement will induce a decline in basic research financed by defense contractors out of their general revenues, as well as in that research directly financed under contracts with Federal agencies. It is difficult, of course, to say whether those two effects will balance each other. As far as the other two sources of funding—universities and colleges, and private foundations—are concerned, it appears a safer guess that these are likely to decline in the future, in relative terms, than that they are likely to grow, and that, if they do grow, their growth will not be large. Further, the whole of our initial argument on the reasons for Federal support buttresses this conclusion further: Private business will not find it worthwhile to increase their own basic research activities substantially, while nonbusiness private sources in general will not have the funds to do so.

To be sure, it is possible to envision alternate future patterns which, though they now appear unlikely, cannot be ruled out. In particular, changes in tax laws designed to stimulate gifts to tax-exempt institutions, or designed to encourage universities to charge much higher tuitions, combined with such programs of loans, scholarships, and the like as would be needed to make acceptable the total effects of all these changes on the distribution of income, might allow universities to support a much higher proportion of their research activities out of their own resources. Modifications in tax laws and anti-trust policies, designed to encourage the creation and expansion of industrial research institutes financed on an industry-wide basis to do the kinds of research individual firms do not find profitable to support, conceivably could lead to a significant increase in industrial support of basic research of many kinds. But neither of these developments appear likely, and both would raise large questions on other grounds. Thus we can conclude that the level and allocation of Federal funds is likely to continue to be a critical problem in the support of basic research.

The application of the principles of allocation sketched in the foregoing pages to a situation in which total expenditures in research and development activities are either rising at a steady rate, or are constant, is a straightforward process. However, the next few years may show a situation in which the growth rate of total research and development expenditures slackens, and perhaps the total level of expenditures begins to fall. The great growth in research and development activities directly or indirectly connected with military goals has paced the past expansion. The possibility of a decline in the level of the total military budget, and especially that part of it spent on the procurement of complex weapons, is strong. This in turn could lead to a decline in expenditures on military research and development, and it is not likely that industrial research and development expenditures would expand at a rate great enough to maintain the growth of the aggregate in the absence of a deliberate policy to bring this about, as, for example, a step-up in the space program. In such a situation, discrepancies between the current flow of finance for basic research which our allocation formulae would produce and the requirements created by the past buildup of new facilities might make it necessary to make special provision for transitional situations. Thus, in a period of slackening growth or even decline of expenditures on applied research and development, the share allocated to basic research in total might be increased above its "normal" level for a period, and then gradually returned to that level, so as to make easier and smoother the adjustment of current research activities, flows of new students, and creation of new research facilities.

The same argument about the relation between research and development in general, and production, might be made. In a period in which expenditures on military hardware are expected to decline, a deliberate effort to increase temporarily the effort in applied military research, and even development of components of various kinds, might form a useful element in a process of adjustment to the decline. Further elements in such an adjustment policy could include an increase in the national effort in space and a more organized Federal program of applied research and development in civilian technology. These would cushion the effect of the decrease in military hardware purchases on the aggregate level of research and development, but it is doubtful that either of these programs should or could be expanded sufficiently to eliminate the effect entirely. Thus some adjustments of the kind mentioned above will probably be necessary.

Appendix

A version of the model discussed above was fitted to the relative proportions for expenditures, number of scientists, and number of Ph. D.'s for the years 1960 and 1962. The model was:

$$P_i = b_i n_i^{a_1} m_i^{a_2}$$

where P_i represents the proportion of Federal support for basic research within the i-th field of science; n_i is the proportion of persons with Ph. D.'s doing basic research in the field; and m_i is the proportion of Ph. D. degrees granted in the field by American universities. The b_i's, on the other hand, are designed to represent the relative capital intensities of research within the particular fields, while a_1 and a_2 denote the relative importance of the two explanatory variables.

The equation was fitted to the logs of the variables under the constraint that $a_1 + a_2 = 1$. This constraint was added because our original model implied that allocations should be proportional to some weighted average of the number of scientists and new Ph. D.'s, and strict proportionality required the addition of this constraint. The following results were obtained (*8*):

$$\log P_i = \log b_i + \underset{(0.483)}{0.111} \log n_i + \underset{(0.483)}{0.889} \log m_i$$

$$R^2 = 0.95$$

$$\text{Corrected } R^2 = 0.89$$

$$N = 16.$$

The figures in parentheses are the standard errors of the estimated coefficients, and it may be observed that although a_1 is not significantly different from zero, a_2 is statistically significant at the 90-percent level. Given the nature of the constraint, the former result implies that a_2 is not significantly different from 1. In addition, all but one of the estimated b_i's are statistically significant at the 90-percent level, and the logarithms of the coefficients are presented in the following table. We can also note that, despite the very small number of observations, the fit is sufficiently good that the multiple correlation coefficient (R^2) is significant at the 99-percent level.

TABLE 2

	log b_i	Standard error
Agricultural science	−0.72	0.44
Astronomy	2.36	.51
Biology	−.69	.28
Chemistry	−.77	.30
Geology	1.64	.35
Mathematics	−.46	.35
Physics	.87	.28
Psychology	−1.21	.63

The model was also tested in two additional formats, and both of these relaxed the constraint resulting from the assumption of proportionality. In the first, the same variables were utilized, while in the second, data were used that measured the absolute levels of Federal expenditures, number of scientists, and number of new Ph. D.'s (*9*). When fitted to the logs of the relevant variables, the following equations were obtained:

$$\log P_i = \log b_i + \underset{(1.69)}{2.34} \log n_i + \underset{(0.48)}{1.09} \log m_i$$

$$\begin{aligned} R^2 &= 0.95 \\ \text{Corrected}\quad R^2 &= 0.86 \\ N &= 16 \end{aligned}$$

$$\log Y_i = \log b_i + \underset{(1.70)}{3.04} \log N_i + \underset{(0.43)}{1.81} \log M_i$$

$$\begin{aligned} R^2 &= 0.95 \\ \text{Corrected}\quad R^2 &= 0.85 \\ N &= 16. \end{aligned}$$

When the absolute levels of the variables are used, the estimate of a_2 is statistically significant at the 99-percent level, while that of a_1 is statistically significant at the 90-percent level. When, on the other hand, the relative proportions are used, a_2 remains significant but at the 95-percent level, while a_1, although greater than its standard error, is no longer statistically significant (*10*). In both cases, however, the equations are homogeneous of degree greater than 1. From this result, it would appear that the Government has acted in such a manner as to allocate funds to the larger sciences more than in proportion to the number of scientists and the number of new Ph. D.'s in the field (*11*).

While we have not "proved" that our model explains the division of Federal support among the various sciences, still it does appear that it is not inconsistent with past behavior, and that the criteria proposed above would not be likely to lead to a sharp break with present procedures.

References

(*1*) National Science Foundation, *Federal Funds for Research, Development, and Other Scientific Activities,* vol. XII, p. 50.

(*2*) One aspect of the complementarity that exists between higher education and Federal support of basic research lies in the substantial portion of research expenditures within universities that goes to support graduate students who assume roles as research assistants. It has been estimated that for 1959, Federal research funds provided about $60 million of direct financial support in this manner. Charles V. Kidd, *American Universities and Federal Research,* p. 79.

(*3*) See NSF 62—Aug. 28, 1962, *Scientific Research and Other Programs of Private Foundations,* 1960.

(*4*) The change in America's position since World War II is pointed up by the figures on Nobel prizes awarded in the fields of physics, chemistry, medicine, and physiology. Between 1901 and 1939, 15 out of 128 prizes were won by American scientists, or about 12 percent. From 1943 through 1956, on the other hand, Americans were awarded 34 out of 67, or slightly more than half. These figures are given in Roger Burlingame, *Scientists Behind the Inventors,* p. 16.

(*5*) See OECS, *Science, Economic Growth, and Government Policy* (Paris, October 1963), charts A, B, and table 13.

(6) The total cost of this accelerator, which is presently under construction, is estimated at $115.6 million. U.S. Atomic Energy Commission, *Annual Report to Congress,* 1963, p. 672.

(7) Some data on past outlays for "big science" appear in the following table.

Construction Costs of Facilities Associated with High-Energy Accelerators—as of December 1960

[Millions of dollars]

1. Cosmotron, Brookhaven National Laboratories, 1952:	
Basic machine	$9.3
Target area	3.5
Total	12.8
2. Bevatron, Lawrence Radiation Laboratory, 1953:	
Basic machine	9.7
Major improvements	9.6
Miscellaneous additions	4.2
Physics building	2.0
Total	25.4
3. Alternating Gradient Synchrotron, Brookhaven National Laboratories, 1961:	
Basic machine	31.0
Miscellaneous additions	2.4
Bubble chamber house	1.7
Total	35.1
4. Zero Gradient Synchrotron, Argonne National Laboratories, 1963: basic machine	42.0
5. Cambridge Electron Accelerator, 1962:	
Basic machine	10.2
Major additions	1.4
Total	11.6
6. Princeton—Pennsylvania Accelerator, 1962:	
Basic machine	11.2
Major additions	10.8
Miscellaneous additions	.7
Total	22.7

The dates shown indicate the initial fiscal year of operation. Miscellaneous additions are listed through fiscal year 1961.

Source: U.S. Congress, *Background Information on the High Energy Physics Program and the Proposed Stanford Linear Electron Accelerator Project,* report to the Joint Committee on Atomic Energy, 87th Cong., 1st sess., 1961, pp. 85–89.

(8) The data used to determine P_i were obtained from National Science Foundation tables of Federal Obligations for Basic Research divided by field of science for fiscal years 1960 through 1963. The average value of fiscal year 1960 and 1961 was considered an appropriate value for 1960, while the average of fiscal year 1961 and 1962 was considered appropriate for 1962. For the variable n_i data were gathered from NSF reports of *American Sci-*

ence Manpower, 1960 and 1962. These figures covered the number of scientists listed in the National Registers of Scientific and Technical Personnel with Ph. D.'s whose primary activity was considered to be basic research. Adjustments were required in a few cases because of the limited coverage of the National Register. The data for m_i for 1960 and 1962 were obtained from National Academy of Sciences—National Research Council, *Doctorate Production in United States Universities, 1920–1962*. From these sources, data on eight scientific fields were obtained for 1960 and 1962. The fields included agricultural science, astronomy, biology, chemistry, geology, mathematics, physics, and psychology.

(9) Y_i denotes the absolute level of Federal expenditures in the i-th field, while N_i and M_i are, respectively, the number of scientists and the number of new Ph. D.'s within the field.

(10) In these two equations, a_1 and a_2 are conceptually the same coefficients and these estimates would be the same in an exact relationship. But the b_i's are conceptually quite different in the two cases. Since, however, the estimates of a_1 and a_2 are not distributed independently of the estimates of the b_i's, we obtain two sets of estimates of the same underlying coefficients. We might note, moreover, that the different estimates of the standard errors are quite similar, and also that the estimates of the coefficients differ in both cases by less than the value of two standard errors.

(11) A dummy variable used to denote differences between 1960 and 1962 was also introduced into the models, but the estimated coefficient was not statistically significant.

In preparing this paper, the author has benefited greatly from the assistance of Dr. William Comanor. He did the statistical analysis of past expenditures in the appendix, but his contribution went beyond that to a general discussion of the whole of the paper which sharpened many of the arguments presented.

ON FEDERAL SUPPORT OF BASIC RESEARCH

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General Considerations

The questions posed by the House Committee on Science and Astronautics require us to look into the future and recommend certain congressional actions with respect to science that will advance the welfare and insure progress of the United States sufficient for maintenance of leadership. This cannot be done with precision; all one can do is study the recent past, and the present, and draw inferences for the future. As to the recent past, we have overwhelming evidence that scientific research, translated into technological innovations through the media of organized applied research and engineering development, has had a dominant and beneficial effect on the welfare of advanced nations, thus adding health, military, and economic values to its intrinsic cultural worth. Our entire civilization is based on technology, and one may with some confidence predict that further progress will be as dependent upon technological innovations as upon educational and other social factors. Indeed, social adaptation to technological change requires continuing technological innovation. The impact of such innovation on the lives of individuals and on the fate of whole nations is far too broad to be measured by economic indexes alone, important as these are. What is the economic measure of a radical change in the balance of international power resulting from the utilization of scientific discovery of nuclear fission? Or what is the economic value of research to nearly a million people in the United States who have been saved from cancer and now lead useful lives? Or to people who, though living far from our cultural centers, are enabled to share in their activities by advances in communications and transportation?

Despite the numerous well-known examples of technology built upon scientific discoveries, resulting from seemingly unrelated research not motivated originally by practical aims or planned for specific purposes, the public tends to forget this crucial feature of technological progress.

To illustrate, there is a great deal of public pressure, quite correctly, to direct large sums of money toward the solution of the problem of cancer. Great strides have indeed been made in preventing and curing cancer in its early stages, and it is interesting to examine the history of the successful methods that are now available. Surgery and chemotherapy (themselves results of research) are very important, of course. In addition, as is well known, X-rays and rays from radioactive substances play a major role. If, at a time before these two phenomena were known, a large-scale planned effort had been made to discover a cure for cancer, it is perfectly obvious that no money whatsoever could have been directed specifically toward the discovery of X-rays or radioactivity, since no one had the slightest inkling or suspicion of their existence. They were discovered because there were social mechanisms for supporting basic research over the whole domain of scientific subjects, in a manner that permitted individual investigators to follow up interesting ideas or interesting observations on their own initiative.

A serious danger of the present situation is the increasing spread, among those not fully familiar with the way in which basic science must operate, of the idea that basic science can be planned in detail and that money need be allocated only to specific topics to provide the necessary scientific knowledge for the advance of technology. Unfortunately, it just does not work this way. For instance, no committee of skilled administrators, or of highly competent scientists, could have made a plan for the discovery of catalysis, which by now has found hundreds of key industrial applications, including high-octane gasoline, synthetic rubber, many plastics, and a great variety of other essential chemicals. Discovery of catalysis was essentially accidental. Thus Sabatier was experimenting on metal-carbon compounds (carbides) and, in studying reactions of acetylene with various metals, discovered hydrogenation catalysis. To take a very different example, Hopkins believed that life could be maintained in higher organisms on a diet consisting exclusively of proteins, carbohydrates, and fats. His experiments, designed to prove this thesis, convinced him that he was wrong, and out of this seemingly disappointing result grew the realization of the indispensable health function of vitamins.

Such examples abound in the past; moreover, we do not have to go very far into the past to find them. A very recent one is the discovery of the laser and maser. Many millions of dollars are now being expended annually on their development and use. The maser (as a part of a complex system) is making possible trans-Atlantic satellite communications, for which a stock issue of $200 million has been eagerly bought by the public. And yet, the maser and the laser are descended from a long line of totally impractical scientific investigations beginning with Einstein's work on the quantum theory of radiation in the early

1900's and extending through many basic investigations on the response of solids and gases to optical and microwave radiations. All these investigations provided the background that made the maser invention possible. Its immediate occasion was a study of the microwave spectroscopy of the ammonia molecule. The study was a small part of a broad program of basic research supported by several science agencies in the Department of Defense. It would have been quite impossible to demonstrate in advance that these investigations would lead to satellite communication systems, and yet they have.

The point that requires repeated emphasis is that closely defined mission-oriented research has value but, by itself, is insufficient and incapable of developing really new ideas and new principles on which each particular practical mission will ultimately find itself based. If the social climate and the support mechanisms are not such as to encourage the free exploration of new ideas rapidly and effectively, our technology will die on the vine because, in the absence of the results of new, undirected basic research, applied work tends to become more and more confined to increasingly expensive refinements and elaborations of old ideas. Perhaps one reason the United States didn't develop the turbojet engine prior to World War II, although it was spending a great deal of public money on aircraft powerplants, is that the powerplant work in the National Advisory Committee for Aeronautics was organized in accordance with the design criteria of piston engines. Administratively, there was no provision for work on really new ideas.

Being a chemist I would like to mention two more examples illustrating specifically the connection of chemical research and the welfare and the position of leadership of the American Nation. Past development of genetics, which provided new hybrid and mutant varieties of plants and animals, had an extraordinary impact on the broad field of agriculture. In recent years, however, this classical field has taken on new vigor by merging with chemistry in what is known as molecular biology. Thus far, no practical applications are in sight or being planned. But those familiar with the field are confident that revolutionary developments will follow. For instance, some day it will be possible to create new variants of living organisms by a controlled chemical process, instead of waiting, as now, for random mutations.

In my next example the practical results are already largely in hand. It is only some 40 years ago that basic scientific research began to attack the problem of what are now called polymeric molecules. Some chemists explored their structures and showed them to be giant chainlike molecules. Others investigated the kinetic mechanisms that lead small molecules to join into these almost infinite chains, and in what orientation. Still others, including pure theoreticians using methods of statistical mechanics, explored the connection between the molecular configuration, the length and bridging of the molecular chains, and the physical properties of the resulting substances. Out of these researches, partly quite recent, grew the present tremendous industry of plastics, artificial fibers, and synthetic elastomers (rubber). The impact of the availability of these comparatively inexpensive materials upon the standard of living of society has been immeasurable. The impact is so great because, in addition to creating new devices and materials for our civilization, we are also reducing the cost, in terms of human labor, of a great many already-existing things, thus making them available to multitudes of people when once they were accessible only to a wealthy few.

It may well be asked whether the rate of increase in scientific activities and hence in their financial support must continue at a faster pace than, for instance, the growth of our gross national product. It cannot continue at such a pace indefinitely, of course. On the other hand, however, it is in the nature of technology not only to solve human problems, but also to generate new ones. Thus it is hardly likely that sometime all our problems will be solved and we can sit back in a static world and enjoy the fruits of our efforts. We live in a dynamic situation of growing population and depleting natural resources in which problems are attacked and solved by scientific and technological means, and inevitably the changes that are thereby brought about introduce new problems that must be attacked and solved in turn. Furthermore, the solutions of many human problems generate the need for faster solution of others. These problems are in part social and in part technological. For example, the fact that the Western democracies have achieved a very high standard of living for most of their citizens creates much stronger pressures for the solution of the problems of internal poverty and for the uplifting of the economic well-being of the rest of the world. As long as nearly everyone on earth was poor, people regarded poverty as inevitable; but when many have abolished poverty, the others will no longer wait for indefinitely delayed alleviation of their distress. Millions of underprivileged who once lived in isolation and accepted poverty because they knew of nothing better now learn of the true possibilities of life by means of modern communications and transportation. Thus it is the technology of advanced nations that has created in other nations the "revolution of rising expectations," and this in turn places more and more demands upon technology.

The same is true for questions of health. On the one hand, spectacular improvements in public health over wide areas of the earth have so reduced mortality that we are experiencing a world population explosion; thus means must be found for control of population growth. On the other hand, having seen the conquest of many diseases, the people are dissatisfied with delays in the solution of other health problems. A careful study, endorsed by a Government agency, has demonstrated that cigarette smoking is dangerous to health, and yet a large part of our population continues to smoke. A scientifically developed elimination of this hazard, rather than prohibition of smoking, seems called for. Technology resulting from science has made possible modern industry and the concentration of population in great urban complexes, but these in turn have created problems of environmental pollution and deterioration for which science and technology must furnish solutions. These are only a few of a vast number of examples of the modern world's need for more and more science and technology, even as its older problems are solved. In many cases, the solutions will turn out to be the result not of patient, planned investigations along well-charted lines, but of new ideas that arise from the efforts of scientists to understand Nature better and to solve abstract scientific problems that suddenly appear soluble in the light of the existing state of knowledge.

It is quite certain that the solution of the major problems of humanity by technological innovations alone is impossible. Educational and other social progress is probably more important, but I am not qualified to judge what specifically needs to be done in these areas. However, observing how widely different are the opinions on what should be done in these areas, I conclude that we need better understanding of relevant social and behavioral problems. This will require more and better research in social and behavioral sciences. Modern technology, by means of computers, new techniques of information-processing and analysis, and so forth, is involved increasingly in such research, and the research methods approximate more and more closely the experimental methods of the natural sciences. The results of such research will enable us in turn to utilize and modify existing technology (for instance, to improve education by audiovisual aids) to achieve further social progress. The main point is very clear: we must not commit the error of those in England in the early 19th century who resisted the Industrial Revolution. Progress of human society cannot benefit from the cessation of technological innovation; indeed, it must be based on social change keeping pace with technological progress.

Some Comments on Research and Development Statistics

There is a great deal of discussion about the rate of growth of research and development in the United States. The total is now estimated at nearly $20 billion, including both federally financed activities and those financed by State, local, and private sources. This figure is quite irrelevant, however, to the discussion of pure science, since about 90 percent of the total is being spent on efforts to achieve clearly specified practical objectives, so many of which are related entirely to the military and space programs. Basic research, even after counting in many activities that are hardly relevant from the point of view of pure science, accounts for only a few tenths of 1 percent of our gross national product. The number of scientists engaged in basic research (calculated on a full-time basis) is less than 100,000, compared to the total technical community of some 2 million engineers and scientists.

In appendixes A and B of this volume, and in the paper of Dr. Harvey Brooks, are discussed in some detail the significance and the inadequacies of research and development statistics. It is also noted there that, relative to their gross national products, all advanced nations invest about the same fraction on research and development, and that in all such nations the investment of private resources in these activities, especially in basic research, is being complemented by investment of public funds, because returns on such investment are too long-range and too diffuse to attract sufficient private capital. Dr. Brooks' paper also contains a thoughtful discussion of the cultural, economic, social, and educational impacts of basic research. I subscribe to his analysis and conclusions and will not repeat them here, but refer the reader to his paper.

The House Committee on Science and Astronautics has posed two distinct questions, one dealing with the requirements for overall support of basic research, the other with proper balance of support among the several sciences. In the following I shall attempt to give a partial answer to both questions, but shall use a modified approach, breaking down basic research not into several sciences, but into organizational forms of research and organizational environments in which it is done. I shall thus divide basic research into four categories. The first category has been called by others "little science," somewhat inappropriately, because it is not small either in total investment or in the results achieved. It is, however, characterized by the small size of the autonomous research units that comprise it. In contrast, "big science," the second category, involves large coordinated effort and usually involves very costly research facilities. Both of these categories are mainly identified with academic institutions. The third category is basic research in mission-oriented Government establishments, and the fourth is basic research in industrial laboratories.

The reason for choosing this approach is that the available information on the current investment in several sciences, and on the scientific opportunities and material requirements of these sciences, is so inadequate as to make projections for a breakdown by separate sciences well-nigh impossible. A start toward analyzing opportunities and requirements of particular scientific areas has been made by groups of experts in planning reports on oceanography, high-energy physics, atmospheric sciences, and space sciences. These reports have been used by Federal agencies as a partial basis for allocation of resources, but this experience indicates that the earlier planning reports need extensive revision. Hopefully, the expert groups now working on such reports (ground-based astronomy, computer sciences, physics, chemistry, and others), which are financed mainly by the National Science Foundation and supported by the Committee on Science and Public Policy of the National Academy of Sciences, will benefit from past experience, and that their reports, when available, will make further valuable contributions to decisions affecting broad allocation of national resources to various sciences. In all cases, one must be careful to regard these planning reports not as exact blueprints, but as carefully reasoned guesses regarding future scientific trends and requirements. Their value is unquestionable, as indications of what science may bring and what it requires; yet one must always bear in mind that scientific progress is intrinsically unpredictable and that too detailed centralized planning can be harmful to that progress. The purpose of planning in science is not to "mastermind" its detailed development, but rather to chart in advance the evolution of a scientific environment and climate in which innovation and creativity can flourish.

Criteria for Fiscal Support of Scientific Research in Four Categories

The first category, "little science," is in the main an integral part of our universities, "educational institutions proper" in the language of Federal statisticians. How have such revolutionary discoveries as radioactivity, X-rays, vitamins, antibiotics, masers, and the mechanism of genetic inheritance been made in the past? What were the system and organization of science that were favorable to such discoveries? Overwhelmingly it was the pattern of the university professor doing the teaching and working with his graduate students and postdoctorate assistants in fields chosen by him exclusively for their scientific interest and because they presented problems that were capable of solution. In the past, these men were supported by their universities, or sometimes by private foundations; they were free agents able to shift their attack from one day to the next as the state of knowledge developed and it became apparent that a given problem could be hopefully attacked, or as ideas suddenly came to individuals working in a given field. This category of scientific research has been considered at length in a recent report of the National Academy of Sciences, entitled *Federal Support of Basic Research in Institutions of Higher Learning.*

Today this research is largely (approximately 57 percent) supported by research grants and contracts from the Federal Government to universities. The rest comes from State, local, and private sources. The total amount of Federal money being expended on "little science" annually is comparable with the capital being put into the single enterprise of constructing a world-wide satellite communication system, an enterprise that would have been impossible without the discoveries of "little science" in the past. Reckoned on a full-time basis, there are substantially fewer than 100,000 people involved in this kind of work. I believe that it is a very important component of our scientific effort; it is responsible for great contributions to scientific knowledge and so to our scientific world leadership. Characteristically, these contributions cover the entire spectrum of scientific fields because individuals are free to choose the subjects on which they want to work, and hence select the most promising and most exciting fields. Frequently in the course of their lifetimes they move from one field to another as new opportunities and new challenges offer themselves.

This segment of the scientific community is responsible also for the training of most of the new scientists—the some 7,000 science Ph. D.'s now being produced annually, a number that is growing at the rate of 6 or 7 percent per annum. The new Ph. D.'s constitute a group that is completely indispensable for the future technical progress of the United States; the responsibility of "little science" for the training of most of them sets it apart from other categories of science, as being especially vital to the Nation. Only a fraction of these research-trained Ph. D.'s stays in the universities to teach our youth and to engage in basic research. Others staff industrial laboratories in which they translate basic research results into technological innovations; still others staff government laboratories and management organizations that are responsible for the many technical and military programs of the Government. On the basis of extensive personal experience and observations, I believe that education and research in basic science form the best base from which young scientists can develop their skills in applied work. I might also note that many of the senior people have themselves established active contacts with industry and/or government and have thus added to the insight, inspiration, and guidance in activities that are applied in character. Perhaps the most striking example of this contribution was the impact of American scientists on the evolution of military technology during World War II.

As noted earlier, the Federal support funds are largely in the form of research project grants and lump-sum contracts, which are discussed in the National Academy of Sciences report referred to earlier. A not insignificant fraction of the investigators working in "little science" do research under group contracts and grants in which several tasks are specified, each being the responsibility of one senior scientist. Whether or not a given project is given support by a Federal agency is frequently influenced by referees chosen from among the active scientists to evaluate the scientific merit of a proposal. In other words, the scientist is judged by a jury of his peers. This system is not only democratic but also scientifically sound; even though it demands a good deal of the time of scientists, it is hard to think of a better one, although some refinements would be desirable.

It has been estimated by the National Science Foundation that the average cost to the Federal Government of a senior researcher, including his part-time salary, technicians' help, scientific supplies, travel, and other pertinent expense items, is $20,000 per annum; that of a post-Ph. D. research associate is $15,000, and that of a graduate research student $3,500. Allowing for indirect costs, purchase of special equipment, and depreciation of general laboratory facilities, I estimate that the total annual cost of research averaged over all these people is under $20,000 per annum per man. Of course, the annual value of some project grants and contracts is much larger, because each is supporting the work of a whole group of graduate students and Ph. D. research associates under the leadership of a senior man.

Contiguous to "little science" is a very wide variety of research activities more or less intimately connected with the universities. Not really distinguishable from "little science" are various research laboratories and institutes, which bear such names largely because, for local reasons, the old accepted form of departmental organization was unsuitable. Many of these establishments perform essentially the same functions as those performed by the traditional departments. One must recognize, however, that some of the best scientists are not interested in, and some not capable of, effective teaching and training of young scientific personnel. Such scientists tend to segregate themselves in research institutes, which sometimes are unrelated to universities. To deny them research support (and I am thinking here still in terms of support on the scale indicated above and applicable to "little science") because they are not engaged in the production of scientific manpower would be exceedingly unwise. But (although this cannot be put in precise quantitative terms) more should be expected from them in scientific output if they are not contributing to the training of new manpower.

In some cases, financing of the work of institutes takes the form of single large contracts or block grants covering the whole establishment. In some respects this form of government financing is extraordinarily attractive, since it reduces bookkeeping chores and other administrative problems for individual investigators and makes money more readily available for exciting and unforeseen research than do project grant funds. On the other hand, these large contracts and grants, because of the way they are awarded, have the serious weakness that distinctly second-rate and poor research can be more easily hidden and protected by the good work of a few leaders. For budgetary purposes some of these establishments should be included in "little science." Some others, even though they lack a unique costly research facility characteristic of "big science," which I shall consider next, should be included in that category. The dividing line is far from sharp and could not be drawn solely on the basis of the magnitude of the supporting grant or contract. The nature of the activities involved is more important and should be considered in each case—those being similar to "little science" being included in that category, and those involving highly coordinated group effort, i.e., acting as a single scientific task force, included in the category of "big science."

On what basis can one decide what the total amount of money expended on the "little science" category of basic science should be? There appear to be several bases for this decision. In the first place, it has been recognized on the basis of past experience that rapid fluctuations in support cause long-lasting damage to scientific progress. Hence continuity is important and what was effectively expended in the past year should be a major factor in deciding what should be expended in the next year. This historical base can be improved by determining the growth rate from recent history and extending it into the immediate future. The second very vital consideration is assurance that students of tomorrow who have scientific aptitudes will be given the opportunity to learn productive scientific research. Scientific talent is scarce now, and to do less than educate all those who want to and are capable of being trained for creative scientific work, regardless of geographic origin or economic status, would be most unwise. The third consideration is the level of support in the rest of the world, since I believe that our country should strive to maintain its status as scientifically the most advanced. When calculating the expenditures of other countries, some adjustment factors must be introduced because many undertakings are more expensive in this country than in others. Nevertheless, relationship to national income or gross national product is probably as good a criterion as we have, and certainly the United States, with the highest per capita income in the world, is justified in spending a larger fraction of its income on research than do other industrial nations. The amount of money that can be spent efficiently in a given area of science should provide another reasonable limit; estimation of this should be greatly improved with the availability of the planning reports on opportunities and requirements of several sciences, which I have referred to previously.

The costs of scientific research are steadily increasing because of ordinary inflation, rising scientific salaries, and most important, because the problems of science become more difficult every year, so that the time and equipment required to solve them becomes more expensive. With the efficient instruments we now have, problems that appeared very formidable many years ago can be solved in a matter of days instead of years, and thus much more cheaply. Thus, a century ago Adams spent approximately 5 years of work on making orbital calculations which led to the discovery of planet Neptune. Recently, this calculation, including programing, was redone in a matter of weeks on a computer at the Smithsonian Astrophysical Laboratory. Moreover, we are not concerned today with problems of the same nature as those that concerned us many years ago, but with much more difficult problems. These require the full efforts of our investigators aided by the most modern instrumentation. It is the solution of the easy problems and the necessity for facing more difficult ones that makes research more expensive each year. Science planning reports will provide the relevant data on this aspect of research for budgetary considerations.

The ideal way to decide on the proper size of the budget for pure science would be in terms of material requirements of the Nation, but this is extraordinarily difficult without factual knowledge of the relation between scientific research and material progress. We do know that we are not making sufficient advances in many scientific areas as a potential basis for technological application. For example, we are not solving the problems of cancer and heart disease sufficiently rapidly; the control of insects has gotten us into very serious difficulties with no clear way out; the economical extraction of pure water from salt or brackish water, though making good progress, is still in the future; the discovery of methods for the control of excessive population increase acceptable to all religions has become a very urgent problem; the problem of employing usefully the fraction of the population that seems to be incapable of procuring employment in our highly automated society is not yet solved; the psychological problems of delinquency and crime have no obvious solutions. It is highly likely that the ultimate solution of each of these problems, even the social ones, will stem, in part, from some scientific advance in a totally unexpected area.

It is clear, therefore, that we need more basic research across the whole range of scientific fields, including the behavioral sciences. I doubt very much that the availability of qualified scientific personnel is the limiting factor in the present rate of progress. An indication of this can be seen in the numbers of contract and grant proposals that various Federal agencies have been unable to support. Substantial percentages of these have been rated as worthy of support and yet the funds are not available. There is some duplication in proposals, and this must be allowed for, but studies indicate that this probably pertains to less than 5 percent of the total of all applications and hence does not alter the main conclusion.

Making allowances for the growing number of graduate students, for the need to give them more extended training (often beyond the Ph. D.)

because of the increasing complexity of science, and for rising costs of research due to more sophisticated instrumentation, I arrive at a very tentative figure of 15 percent for the overall annual growth of "little science" for the next 5 to 10 years to meet the objectives spelled out in the questions posed to us. The National Science Foundation calculates that in 1963 the Federal support of basic research at educational institutions proper amounted to approximately $450 million. A large percentage of this and comparatively little from other categories of Federal support should be classed as basic "little science." To arrive at the total, one must also add a rather small, but currently not known, relevant fraction of the $160 million in Federal obligations for the research and development plant at educational institutions proper. Also, the value of fellowships to students, pre- and post-doctoral, in "little science"; that is, a fraction of the $330 million obligated by the Federal Government for fellowships, traineeships, summer schools for high school teachers, and similar training allocations. The total for "little science," I estimate, was not far from $600 million in 1963 and, at 15 percent per annum, it would double in 5 years.

The dollar figures given above refer to basic research. The totals going to educational institutions proper are at least twice as large, the difference being defined as applied research and related expenditures. Much of this applied research (e.g., in biomedical sciences) is not substantially different from basic research in "little science," and should perhaps be included in it for budgetary purposes. On the other hand, the objectives in supporting applied research—while certainly as valid as those relating to basic research—are different. Moreover, applied research is mainly concentrated in professional schools—medical, engineering, public health, for example—while basic research is typical of faculties of arts and sciences. On the whole I believe that separate budgetary planning for basic and applied research has some advantages in view of the differences noted above.

The practical budgetary problems of maintaining a steady growth of "little science" are far from being simple. For instance, as is pointed out in Dr. Brooks' paper, the long-range commitments of several Federal agencies to "big science" projects have currently led to an actual contraction of "little science" budgets in several scientific areas, because the total scientific budgets did not increase as fast as expected by agency planners when the commitments were made. Another difficult problem arises because much of "little science" is supported by agencies with practical missions (e.g., the Department of Defense, the Atomic Energy Commission, and the National Institutes of Health) and is therefore, in principle, "mission-oriented" research. The breadth of definition of what basic research is relevant to the practical mission varies greatly from one agency to another, and also changes with time, and this creates difficulties.

For instance, the Department of Agriculture does not support broad-based research in plant sciences in the way that the National Institutes of Health supports research in high-energy physics, but the National Aeronautics and Space Administration appears to be relatively indifferent to astronomy beyond the solar system; and so on. These difficulties are made more serious by changes in policies defining what is mission-oriented, reflected, for instance, in a recent decrease of support of "little science" in universities by some research offices of the Department of Defense. To cope with these situations and trends, which may be wholly justifiable from the point of view of mission-oriented agencies but which can be very harmful from the national point of view, I see but one procedure. It is to set the research budgets of the National Science Foundation so as to allow for budgetary trends in other agencies and thus to ensure the overall growth of "little science" that has been decided upon. At the same time, the mission-oriented agencies should be discouraged from abandoning basic research, since otherwise the federal system will become too centralized.

So much for the problem of gross allocation. There remains the problem of detailed allocation to individual problems. Here, I strongly believe that every effort should be made to avoid overly detailed centralized planning and control. No man was wise enough in 1900 to foresee that the Curies would make a major contribution to the cure of cancer. Similarly, no man today is wise enough to know from what field will come a critical discovery that, directly or indirectly, will solve the problem of the control of insects. Will it be new chemical discoveries, the use of radioactivity, as in the case of the screwworm fly, or sex attractants, or some idea that does not now exist in any man's mind and therefore cannot even be guessed, much less incorporated into a planned program of research?

How, practically, can we be sure that freedom of scientific enquiry will always be carefully considered and protected in the allocation of funds? As far as "little science" is concerned, this can be assured if allocations are guided by the requests for funds from individual investigators. Does chemistry have a large number of worthy grant or contract proposals? Are the funds for chemistry sufficient to cover these? Naturally any such device can be distorted, by padding proposals and other obvious means. Federal agencies are and must be constantly on the watch for distortions. The reports on opportunities and requirements of particular sciences should be of major help in determining broadly what is important and possible. But in specific cases it is only the investigator working closely in his field who knows what is possible, and he is constantly on the alert for problems that are both important and soluble. Similarly, research students, free to study what they wish, tend to move into scientific fields with the greatest intellectual opportunities.

The detailed allocation of support should therefore be based, democratically, on the multitude of the individual choices made by individual investigators. This is the basis of the flowering of science, and hence technology, in the 20th century.

Prior to World War II, basic science seldom made use of costly research equipment, almost the only exception being astronomy. Since World War II, because of Federal support, the use of costly research facilities has been growing very rapidly and has led to the establishment of a new category of basic science, which I will call, following others, "big science." The organized teamwork that is usual with the use of these costly research facilities has opened completely new fields to scientific research and greatly advanced our knowledge of the world we live in. This "big science" is as indispensable to world leadership and the continuing progress of the United States as is "little science," but from a fiscal point of view it presents different problems. In appreciating these problems one needs to bear in mind that the transition from "little science" to "big science" is continuous and far from sharp. However, a somewhat arbitrary boundary might be defined in fiscal terms. I have estimated above that 1 man-year of research in "little science" costs under $20,000. This included the annual cost, per researcher, of general laboratory facilities, which are usable, of course, for many years. The boundary of "little science" could be drawn by defining "little science" as limited to annual costs of this order of magnitude. Where special research equipment or facilities are costing much more or a major research establishment involves a centralized task-force effort, one might speak of "big science." For instance, a research facility initially costing $12 million, whose annual operating cost is $3 million and which becomes obsolete or requires major reconstruction after 6 years, would cost about $170,000 per scientist-year if 30 scientists, including post-Ph. D. fellows and graduate students, were using it. The costs of individual projects in this category have been rising very rapidly since World War II (perhaps by a factor of 100 in 18 years), and there is no natural upper limit to such costs. "Big science" is fiscally open-ended because the commitment of scientific personnel per project is rising comparatively slowly and the costs are concentrated in the engineering effort of constructing the special facilities and their maintenance, operation, and improvement. To illustrate, it has been reported in the newspapers that Project Ranger has already cost $200 million. It has yielded very significant detailed pictures of the moon surface which are being studied by astronomers and will advance our understanding of the moon. The overwhelming fraction of the money has gone, however, into the engineering of the space probes, the manufacture of the rocket boosters, the construction and operation of the launch facilities, and similar items. Our engineering resources are so great that it has become technically feasible to engage in individual projects costing in the range of hundreds of millions of dollars (e.g., the proposed nuclear-particle accelerators) or even many billions of dollars if Project Apollo is regarded as scientific. The essential point is that technically such projects are not limited by the supply of scientific talent in the way "little science" is, but only by the (much greater) supply of skilled engineering manpower.

Currently this "big science" is composed mainly of a rather large group of establishments for basic research that are built around expensive special facilities and are normally fairly closely connected to universities. As examples, I might mention the nuclear-particle accelerators, large radio and optical telescopes, oceanographic research vessels, the National Primate Center, and special ventures like the Mohole, Project Ranger, and scientific satellites. Some of these activities are not managed by individual universities, but by groups of universities, as the Brookhaven National Laboratory, the Kitt Peak Astronomical Observatory, and the National Center for Atmospheric Research. Still others, like Projects Ranger and Apollo and the Mohole, are managed directly by Federal agencies. The financial characteristic of these operations is that the cost to the Federal Government per active scientist and per unit of scientific knowledge, as measured by scientific papers, is very much higher than that in "little science." Where a scientific paper may have cost the Federal Government $20,000 in the domain of "little science," a similar scientific paper based on research with a large nuclear-particle accelerator may cost half a million dollars, because of underlying engineering and logistic costs. About the same ratio of costs probably applies to the training of new scientific personnel in these establishments, and it is questionable whether individual training costs of such large magnitude can be used to justify the operation of these establishments. In fact, they usually depend for most of their personnel on people who were scientifically trained in "little science." The student training they do should be seen as a secondary aspect of their activities, and their support should be justified mainly on the ground that they provide scientific results that cannot be obtained by any other means and are important enough in at least one of the ways defined below to justify the cost.

Realistically, we must think in terms of a limited total national scientific budget, and then, because of the absence of natural limitations to the costs of individual "big science" projects, the question of financing them acquires a special perspective. Although an accurate figure is difficult to arrive at, I estimate that in the United States today considerably more money is going into these establishments and projects than into "little science." Thus the space sciences budget of the National Aeronautics and Space Administration, of which a large fraction is in the category of "big science" as defined above, alone amounts to nearly half of the $1.6 billion classified as total Federal basic research in fiscal 1964.

It is my belief that in order to maintain our world leadership we should continue to support "big science," and support it on a broad front, but the determination of the proper growth rate requires different considerations than those advanced earlier for "little science." In seeking to determine what factors should be considered in determining the financing of the projects and establishments of "big science," I found the considerations developed by Dr. Weinberg in his article in the magazine *Minerva* (winter 1963) very helpful.

Before discussing them, however, I wish to emphasize another factor. Before weighing seriously the comparative value of a new project in "big science," it is essential to ascertain two factors: (*a*) it must be feasible technically in the judgment of engineering experts; (*b*) it must be backed by a qualified group of scientists who consider it important enough to make firm personal commitments to it if undertaken. The latter point is essential to insure scientific success of the project.

One factor considered by Dr. Weinberg is the impact of a given type of research on related and significant branches of science. With respect to this factor, he has given a low rating to high-energy physics research on elementary particles. The contribution of this factor can be determined in detail by consultations with people who are not themselves involved in the research in question, but who are active in adjacent scientific fields and hence can sense the impact on their sciences of the special programs and projects considered.

A second factor, useful because many of these projects are of somewhat programmatic nature (and hence of somewhat predictable outcome), is the impact of the results of scientific work on practical applications. This is related to Dr. Weinberg's social-value criterion. For instance, the drilling of the Mohole will undoubtedly advance oceanic drilling to shallow depths, which may be of importance for oil and other natural resources. Much oceanographic research has clear, practical implications of a number of types, including military. Atmospheric research has obvious practical importance for weather prediction, and possibly ultimately for weather modification. These considerations must be developed for each establishment or project, and weighed.

The third factor is one of national prestige and international influence, the significance of which, of course, is strongly dependent on the general world political situation. There is no doubt that space explorations by means of satellites and deep space probes have a substantial international impact, improving the apparent stature of a nation, even though their scientific results are of interest only to a very limited audience. High-energy physics research seems also to be a matter of some national prestige. Oceanographic research, by showing the flag, as, for instance, in the case of the current expedition to study the Indian Ocean, certainly has a major international impact.

The last factor, which is probably least susceptible to quantitative evaluation, is the broad cultural impact of scientific research, satisfying the age-old urge of humans to understand the world and themselves. Three scientific areas impress me as now probably having the greatest human appeal and grandeur in this respect. They are galactic astronomy, probing the structure and the origin of the entire universe; molecular genetics (now part of "little science"), advancing the understanding of the origin and evolution of life; and high-energy physics, exploring the elementary particle structure of all matter.

To sum it up, when engineering feasibility and adequate commitment of scientific talent are assured, there remain four main considerations that have to be carefully weighed and put together, and on that basis it should be possible to arrange the existing and the newly proposed projects in an order of preference. If one restricts consideration to projects whose initial costs are in the millions and operating costs (including scientific activities) are correspondingly high, the total number (excluding establishments whose main function is applied but including the previously mentioned "single-task" research institutes not attached to a unique facility) would be only in the hundreds, so that an ordering of priorities on the national level is wholly feasible. Since these ordering judgments are fallible, I would not advocate dropping a particular type of activity—for instance, deep earth drilling—as totally worthless. The judgment is not between "black" and "white," but in terms of various degrees of value. Here also some "planning reports" on the opportunities and requirements of particular sciences will be valuable, because their authors, who are experts in the field, identify the most promising new research facilities in their scientific fields and assign relative priorities to them. After a decision on a total Federal budget for all "big science" ventures in the light of available fiscal resources and needs of the Government, it should be possible to allocate the total available funds among the different enterprises—existing and proposed—according to the judgments developed above. Because of inaccessibility to me of all relevant information, I am unable to conclude whether the total budget for "big science" should grow more or less rapidly than that of "little science," but it certainly should not grow at the expense of "little science," for the reasons already developed. A poor nation that would still profit greatly by nurturing "little science" may not be able to afford "big science." (Note the recent openly acknowledged decision of the Soviet Academy of Sciences not to "compete" except in selected areas of "big science" because of limitations of their national resources.) On the other hand, "big science" generates effects of international import and provides some scientific knowledge that "little science" cannot.

The third category of basic scientific research to which I turn my attention is some of the work done in Government laboratories, other Government technical establishments, and contractor-operated Federal research centers. Included in this category are all those that were established to achieve practical goals within the broad mission of the Government agencies financing the establishments. They are numbered in the hundreds. Careful studies of the optimum conditions for effectiveness of such establishments have led to a consistent conclusion (see, for instance: *Strengthening American Science,* Washington, 1958, a report of the President's Science Advisory Committee) that they should incorporate in their activities basic research related to their practical missions, i.e., mission-oriented research. Such in-house activity provides a bridge between the contemporary science and the applied effort and assures a continuing forward-looking posture of the establishment. This general requirement, I believe, is absolutely essential to insure the viability and worth of such establishments, but its details—that is, the definition of the scientific scope of mission-oriented basic research and the division of funds between such basic research and the applied work—should be very largely the administrative function of the sponsoring agency and the management of the establishment, subject, of course, to the approval of Congress (*1*). The primary concern of Congress should be to determine the total funds to be allocated to the establishment on the basis of the importance and promise of the practical mission, and on the basis of the past record of the accomplishment of the establishment in the performance of its practical mission.

While thus endorsing firmly the need and value of mission-oriented research, I have reservations about the introduction of general basic research, not related to their missions, into such establishments. This creates distinct classes of personnel and is not conducive to an effective prosecution of the main objectives of the establishment. In this connection I must also note that the original practical missions of some—but certainly not the majority—of such establishments have been accomplished or lost, and no new one of equal validity has been established. The substitution of basic research for the practical mission as a justification for the growth of an establishment appears to me to be not very sound. Valid questions must be raised concerning such factors as lack of firm scientific tradition in management, comparative isolation, high costs and inability to educate new scientific personnel, as well as scientific productivity. If the answers are unfavorable, clearly the establishments should receive low priority in the allocation of funds for scientific research. While abrupt discontinuance of support of these establishments would be harmful and disturbing, if their scientific productivity were recognized as satisfactory, essentially fixed budgets seem to be the most that can be recommended, except where research is of outstanding quality. In effect, the decision to apply fixed or decreasing budgets would be equivalent to "mothballing" an establishment in order to have it available for an unanticipated new practical need.

The fourth category is basic research financed by the Federal Government in the research laboratories of profit-motivated (and some rather similar "nonprofit") private corporations as distinguished from wholly Government-financed Federal contract research centers included in category 3. This fourth category includes basic research done under contracts aimed at practical objectives, some of the so-called independent research financed partly from the overhead of procurement funds (ASPR 15, amounting to about $1 billion in 1964) and separate contracts and grants for basic research.

The principles developed above for category 3—the Government laboratories—are, I believe, applicable to category 4 also. When the profit-motivated organization is engaged in an applied practical task under contract to the Government, the Government agency and the contractor must determine whether related basic research by the contractor will expedite the accomplishment of the mission. If so, they must also determine what constitutes mission-oriented basic research. It should also be mainly an administrative function of the agency, in consultation with the contractor, to decide on the division of funds between the various activities of the contractor. The Congress, on the basis of the importance of practical missions and records of progress achieved, should, of course, decide on the total funds to be allocated.

Most of the funds under ASPR 15 are given in support of applied research. Where these funds are used to support basic research, the principle now in force, I believe, is that the work is to increase the contractor's value to the Government, and is therefore to be related to the contractor's existing practical capabilities. This principle appears sound to me and requires no modification, but perhaps a more consistent application is called for.

The contracts and grants for basic research by profit-motivated corporations that is unrelated to their development and production activities for the Government appear to me to have little justification. Of course, there are exceptions where an outstanding scientific capability exists and where continuing creative activity is in the national interest. Where there is no solid basis for assuming such capability, the same arguments should apply, as stated earlier regarding Government establishments without applied missions. The high costs of such research (see testimony of Dr. D. F. Hornig before the Subcommittee on Science and Research, May 21, 1964) militate against its expansion or even continuation.

In conclusion, I want to comment on two subjects that have been referred to in this paper only very briefly. While I believe that basic research should be supported over the whole spectrum of sciences because of the difficulties in predicting its practical value in individual cases, I am convinced that the main justification for Federal support on the present scale lies in its practical applications of benefit to the Nation. Hence the process of translating basic scientific findings into practice is highly important, and this involves applied research prior to engineering. The strengthening of such research in Government laboratories and in profit-motivated corporations is as vital to the Nation as that of basic research, mainly in universities. The former and the latter types of research involve different considerations, however, and I have not discussed the former because I consider it to be somewhat outside the specific problem posed to us by the House committee.

Finally, I want to reaffirm my deep conviction that better education of our youth, based on equal opportunity for all who want to avail themselves of it, is indispensable to a good future for our Nation. Education and research training of young scientists is but a part of this large task.

References

(*1*) Needless to say, the use of competent "outside" scientists in assessing the scientific worth of proposed basic research would be as valuable here as it is in categories 1 and 2.

I wish to acknowledge valuable contributions to this paper by Dr. E. B. Wilson, Jr., and the vigorous criticisms by the entire ad hoc group, which helped to clarify my thinking.

LEADERSHIP AND QUALITY IN SCIENCE

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Introduction

The questions put to us, in brief, are:

(1) What level of Federal support is needed to maintain U.S. leadership in science and its applications?

(2) How shall one adjust the balance of Federal support given to various fields of science?

These questions are tough and perhaps unanswerable. In this paper, only a small part of an answer is attempted.

To (1): A vital component in scientific leadership is the presence of top-quality scientists. They are rare. They may not appear where we think we want them; they crop up in fields of their own choice, and not in those planned by Federal panels. They may not appear when we want them; the number of top-quality scientists may not increase to match the current explosive growth of science. But the top-quality scientists are vital if this growth of science is to be truly fruitful; hence they must be supported whenever and wherever they appear. In sum, to the questions at issue.

($1a$) The level of Federal support should always be adequate to provide research opportunities for all those men who give promise of true originality.

($1b$) The level of support should never be so lush as to drown these men of quality in a sea of mediocrity.

To (2): Balancing Federal support is an allocation problem; it involves the usual difficulties typical of other economic adjustments of supply to demand, plus special difficulties arising from the fact that the supply of scientists—notably the supply of scientists of top quality—is limited and inelastic. As a result, the limitations to scientific growth are not merely fiscal ones.

Most of the examples quoted in this paper will be drawn from the field of mathematics, pure and applied. Now mathematics (computers aside) is all "little" science and no "big" science; all theoretical, with no experimental aspects. Hence our examples will bear most directly on the problems typical of scientific work that is theoretical and "little." On the other hand, mathematics provides especially clear examples of some important points of general science policy: Quality is important in all the sciences, and quality work can be recognized with exceptional accuracy in mathematics. Again, mathematics is currently growing more explosively than any other science, so that it illustrates most clearly some of the allocation problems consequent upon scientific growth.

Why Leadership in Science?

A position of international leadership in science is possible today for the United States—in considerable part because of past decisions by Congress that have strengthened science: decisions such as the development of the National Institutes of Health, the establishment of the National Science Foundation, and the sharp increase of the level of Federal support of science made in 1957 with the appearance of Sputnik. Our scientific leadership in the past has led to improvements in our economic well-being and in our military security, and has also given us other but less tangible cultural and intellectual benefits. This paper starts from the assumption that continued leadership in science is worth substantial investment (Federal, State, and private) because it is highly probable that such leadership will continue to yield such advantages.

This paper is concerned with the problems of scientific leadership and not with the problems of the mass education of scientific technicians, though the latter problems also bear on the economic benefits of science.

The Components of Leadership

Scientific leadership in a nation can mean a number of different things:

(1) *Spectacular accomplishments:* Examples: big rockets, sensational new drugs, big accelerators, deep Moholes, supertelescopes, and large-scale military achievements.

(2) *Rapid practical applications of scientific discoveries:* Example: The transistor, soon after its discovery, was applied to give pocket radios, better components for computers, and miniature electronic devices for rockets.

Many other types of scientific leadership appear as leadership in a particular scientific specialty, say, in one of the following forms:

(3) *Outstanding centers:* The University of X is one of the two or three best places in the world to study such and such. Thus it is Brookhaven or CERN for big accelerators, and it is Australia or

Jodrell Bank for radio astronomy. Within mathematics, it is Paris or Cambridge (Mass.) for algebraic geometry, Princeton or Berkeley or Oxford for topology, and Chicago or Tubingen or Moscow for group theory. This type of specific scientific leadership matters—and can be measured.

(4) *Predominance:* Sometimes the experts in one country will dominate a given field of science. Thus, much of the current exciting development in molecular biology is sparked by activity in the United States. Again, it is acknowledged that we have a predominant position in optical astronomy; we have the biggest telescopes with the best equipment on top of the tallest mountains with the clearest weather, and many of the most important conceptual advances in astronomy started in the United States.

(5) *Origination of new ideas:* Leadership in this sense comes not with massive investigation, but from *one* crucial experiment or from *one* idea. At the time of discovery, the scope of this idea may not even be recognized. Thus radio astronomy started from the backyard observations of an engineer (and it took the Australians to really develop this idea). Current work on the breaking of the genetic code has been vitally influenced by Marshall Nirenberg's pioneering *in vitro* experiments. The development of high-speed computers owes much to Von Neumann's contribution of the stored-program idea. Mathematical work on partial differential equations has been decisively strengthened by the discovery of "distributions" by Laurent Schwartz in Paris. Current exciting progress in the foundations of mathematics derives directly from 25-year-old work by Godel on the consistency of the continuum hypothesis. And so it goes with germinal ideas in many fields of science.

(6) *The best young men:* In certain of the sciences, notably in physics and mathematics, the most vital new accomplishments are those of young scientists. In this regard, leadership lies with the countries where circumstances most effectively stimulate the ablest young scientists and permit them to attain intellectual independence at the peak of their productive potential.

What Support Is Needed for Leadership?

Consider next the extent of Federal support appropriate to each of the six listed types of scientific leadership.

(1) "Spectaculars" are usually costly, both in money and in scientific manpower. At any one time, each of several "spectaculars" may be technically feasible, although at most one or two are fiscally attainable. The decision as to whether to launch a "spec-

tacular," and if so, which one, involves political considerations and questions of the allocation of scientific talent. The political considerations may be overriding in some cases. For example, the decision to land a man on the moon may be justified in part as a spur to the economy or in part as a moral equivalent to the competition of war.

(2) Practical applications of known scientific techniques lie on the development end of the research-and-development spectrum. Here again the choice of applications and the decision as to the speed with which they are to be pressed involves both political and scientific considerations, as well as careful cost comparisons between alternative applications.

(3) Outstanding centers for individual branches of science cannot stay outstanding, under current conditions of rapid scientific growth, unless they have liberal support. Experience indicates the merits of a pattern of combined private (or State) and Federal support. For the latter type of support the "project system" of grants and contracts—with a variety of Government granting agencies—is well adapted to the purpose.

(4) While our country has the sort of leadership represented by essential predominance of some fields of science, it is probably neither desirable nor possible that this exclusive leadership be continued indefinitely; some other country may soon be able and willing to match our best. To reap the economic benefits of scientific leadership we do not need the sort of position represented by continued dominance (even though the specialists in the field might sometimes wish it so). In military respects, domination (if possible) has evident advantages, but even here a greater dispersion of our scientific effort may give us a better gamble on the unforeseen profits of future scientific breakthroughs.

(5) Origination of new ideas is, we hold, the vital element necessary to keep alive the other components of scientific leadership. To this end, we need ample support at all likely sources of originality.

(6) What brings the best young men now into this field, now into that? The excitement of a field of science where there are current great discoveries? The award of Nobel Prizes to scientists in that field? The presence in a country of especially stimulating teachers? The promise of high monetary rewards? Or is it simply the fashion of the moment or the lack of other alternatives? The interaction of these forces is not well understood. Sample cases in mathematics: In the decade 1945–55, France was outstanding in producing the very best young mathematicians. (There were stimulating and revolutionary teachers in France, and there was little competition from the neighboring field of physics.) In the decade

1955–64, the United States (and perhaps Russia) leads in this regard. Here it is highly likely that extensive Federal support of the (previously relatively neglected) science of mathematics played an important role. In Japan (where the motivations are different) there have been many able young mathematicians with very little government support.

The best conclusion may be that the active development of the best young scientists requires both monetary rewards and intellectual stimulus.

The Judgment of Quality

Originality and real scientific novelty are rare, individual, and often unpredictable. Because originality is usually that of an individual man or a couple of close collaborators, the support of such originality means primarily the support of an individual scientist or of small groups of two or three coworkers. Usually the support must be requested before the original ideas are all at hand, so the support of originality involves some guesswork. This guesswork can be educated, though one needs to support many really able scientists in order to be sure that one has included all the profoundly original ones. Fortunately, there is a dependable method for judging the able scientists: a judgment of the scientist and his project by means of a panel of experts in his field. Care must be exercised that the offbeat but original scientist is not refused because he arouses conservative doubts. With this precaution, the panel system is probably better in locating possible originality than alternative systems based on institutional grants or on regional development projects.

Such questions as to the choice of individual projects by no means cover all significant decisions of governmental science policy. There are many other plans, and other types of projects. This paper holds that in every such case the elements of quality and originality are vital. We propose no automatic or guaranteed test for the presence of these elements. At best we can suggest that one ask of each project: Does it pay attention to quality and to originality? If it is big, will the administration be of high quality? If it requires many scientists, will their abilities best be used here? If it calls for "more research on X," is it just combining the evident importance of X with the popular slogan, "more research"—or does the proposal really present actual ideas as to research to be done? Quality in science is upheld only when hard questions are asked of each project. Research is possible only if it starts with an idea.

The Mathematical Sciences

Among the sciences, mathematics displays many special traits. It is wholly theoretical: no mathematical theorem is ever disproved by an experiment. Mathematical research resembles artistic creation, and some may hold that mathematics is an art; yet it displays other characteristics of a science: There is absolute agreement upon conclusions, and the results of one mathematician can be used by many others. Mathematics (computers excepted) is highly individualistic; ability at teamwork is no virtue; there are often two or three (coequal!) research partners, but there are no research teams. Mathematics is recondite: even the most fundamental problems of the science are hard to explain to nonspecialists. Mathematics is an independent and self-sustaining science; were mathematics cut off from all contact with other sciences, its internal problems would keep it vigorously alive for at least 200 years. But there is not and should not be such isolation; mathematics does receive many stimuli from other sciences, and many of these sciences make extensive use of mathematics. However, until recently there has been little contact in this country between mathematicians and other scientists; even today practically none of the leaders in mathematical research have any role whatever in the formulation of national scientific policy.

At the same time, mathematics provides one of the best examples of some current problems of scientific policy. For instance, it illustrates forcibly the problems consequent upon rapid scientific growth. There is a shortage of talent in many academic disciplines; the shortage is at present most severe in mathematics. Before 1945 there was only a handful of industrial mathematicians; today they are nearly as numerous as the academic ones. Elsewhere, the lines between different fields of science blur and mixed fields arise; here, separate disciplines (statistics, computer science) appear to split off from mathematics. Mathematics is highly theoretical, but the very rapid development of science generally is likely to make other sciences more theoretical, and perhaps in this respect more like mathematics. This has already begun to happen in fields as diverse as aeronautics, automatic control, and psychology.

Finally, mathematics best exhibits the effects of quality upon scientific progress, because here quality is highly visible. Mathematics abounds in famous unsolved problems; the man who finally solves one usually does so not by dint of new apparatus, but by harder or more effective and courageous thought. At the other end of the scale, routine mathematical research is likely to be essentially worthless. No piling up of low-grade effort solves a hard problem. In this science, quality really matters.

In these regards, mathematics in the United States has recently been strikingly successful. In the last half-dozen years, young mathematicians in this country have solved at least five famous problems: the "Hauptvermuting" of combinatorial topology, the continuum hypothesis, the Burnside conjecture on finite simple groups, the Poincaré conjecture for higher dimensions, and the resolution of singularities of an algebraic variety. Still another big problem, the "index theorem" for partial differential equations, has yielded to an Anglo-American attack. Many other important though perhaps less spectacular advances have been made, and many promising young mathematicians have developed.

Mathematics thus represents a clear case in which scientific leadership did develop in this country; it would be good to know exactly what steps brought this development about. It is reasonably clear that extensive financial support (Federal *and* private) for individual research projects in mathematics played a vital role, both in encouraging able young men to take up mathematical careers and in giving them (most important) opportunities for postdoctoral study at active centers of mathematical research.

Mathematical research requires no laboratory; it does require extensive discussion and rapid publication. This is another point at which governmental financial support is vital, even though the costs are not particularly high. This is especially the case with review journals. In many sciences, summary of the current literature can be accomplished by an abstracting service or by a more or less mechanical indexing arrangement. In mathematics, more is necessary: a *review* service. Currently, the world mathematical literature is organized by three parallel review journals: *Referativnyi Zurnal Matematika* (Russian), *Zentralblatt fur Mathematik* (German), and *Mathematical Reviews* (American; published by the American Mathematical Society, with the aid of a substantial annual subvention from the National Science Foundation and from other Federal sources). *Mathematical Reviews* covers all the current research articles in mathematics, publishing for each one a review by a (usually!) competent specialist. This provides for all the Western countries a reasonably careful and current critical check on all mathematical research activity; it inevitably gives a first measure and emphasis on quality. Publication of *Mathematical Reviews* is now possible only through Federal subsidy. Its presence in the country is a vital element in American leadership in this field of science.

Applied Science

There are cogent reasons, bearing upon national policy, for developing more and better applied scientists. This certainly is the case in mathematics, where there is assuredly an especial shortage in this country of applied mathematicians. Relieving this shortage is a complex problem; we list some of the complexities:

(1) Recent progress in applied mathematics has not been as spectacular as that in pure mathematics (recall the famous problems noted above). This situation inevitably influences the choice of field made by young mathematicians.

(2) There is no sharp line between "pure" and "applied" mathematics. For example, 25 years ago symbolic logic was the "purest" branch of mathematics; today it is heavily applied, as in computers.

(3) Applied mathematics is not one field but many, ranging from classical physics through continuous mechanics and partial differential equations to game theory and mathematical models in the social sciences. Needed are *both* applied mathematicians *and* mathematically trained experts in the substantive field.

(4) Some of the problems of applied science are of derivative interest; worth doing because of the application and not because of the science itself. For instance, World War II problems of airborne fire control required mathematics—in the sense that they required intelligent use of nothing more than elementary calculus.

(5) Other problems of applied science are fascinating, and sometimes fascinatingly difficult, such as the unsolved qualitative problems of dynamics (as in the three-body problem). These questions are not only very difficult; they require background knowledge that is rare in this country.

(6) The training of good young applied mathematicians in universities requires good older experts in these fields. These are the same older experts who are in short supply; they are needed for Government and industrial laboratories. This is a problem of allocation in the face of shortage; to have enough applied mathematicians for industry in the future, one must somehow use a larger proportion of the men now available as teachers. This is a typical allocation problem of the sort that should be faced in planning additional big projects that will require, *inter alia,* many applied mathematicians. It involves a decision between current expenditure and investment.

This is but a partial list of the complexities involved—here and perhaps in other applied sciences. There is no simple solution. Reasonable partial suggestions might be to return more experienced applied scientists from the laboratories to the graduate schools and to encourage more budding scientists in related fields to get more extensive background knowledge of mathematics.

Problems of Growth

Sciences may suffer from substantial growing pains. This is the case for mathematics, which has exhibited especially rapid growth during the 6-year period 1957–63. In this period the number of members of the American Mathematical Society increased from 5,736 to 9,515, and the number of Ph. D.'s granted in mathematics and related fields from about

226 to about 588. (Figures from annual lists of Ph. D.'s published by the American Mathematical Society.) The number of undergraduate majors in mathematics has increased much faster. The growth in mathematics has been worldwide. *Mathematical Reviews,* which covers the world literature, had 982 pages in 1957 and 2,503 in 1963.

This rapid growth has brought about many strains and dislocations. Departments of mathematics in many universities have doubled in size, with a consequent increase in committee work and administrative work (and no corresponding increase in the number of older men normally responsible for such work). Individual professors have a heavier load of guiding Ph. D. theses. The increased volume of mathematical research has led to many overlapping results, where there was little duplication before. (Sample: At a conference in 1963, essentially the same result was reported by three different workers in differential topology.) In part, the means of communication haven't been developed to match the growth in numbers. Mathematicians move easily from job to job; in these 6 years, top salaries have increased by at least 60 percent. Perhaps the most important decision determining the quality of a faculty is the promotion to permanent tenure (usually at the associate professor level). In the period 1948–55, the ablest young men normally spent 3 years as instructors (or in postdoctoral studies) and then 5 or 6 years as assistant professors before being promoted to permanent tenure. Currently, most able young men expect to spend, at most, 2 years as instructors (or in postdoctoral studies) and then, at most, 3 years as assistant professors.

It is a fair conclusion that mathematics has been growing at about the maximum manageable rate.

New continued leadership in a science clearly requires steady but manageable rates of growth. Going beyond this, enthusiasts for the utility of science occasionally argue for big expansions in the number of young scientists being trained. This approach runs the danger that a mere emphasis on quantity can stifle the quality necessary to true originality and fruitful scientific growth.

A striking example is the so-called Gilliland report of the President's Science Advisory Committee (*Meeting Manpower Needs in Science and Technology,* The White House, December 12, 1962). This report argued for a vast increase in the number of Ph. D.'s granted annually in the physical sciences and engineering, and pressed for the increase without taking any account of the difficult problems of maintaining the quality of the Ph. D.'s. For example, in mathematics the report called for an increase of 300 percent in 10 years.

This recommendation for mathematics is utter nonsense: Ph. D.'s in mathematics, currently of good average quality, are turned out rather slowly and "by hand," in the sense that each Ph. D. thesis is different in style and requires individual direction. The number of professors of mathematics able to direct such theses is limited, and the work of directing too many theses can become a distraction. These distractions and related problems have already appeared in the recent 1957–63 increase in mathematical Ph. D.'s granted. This increase has already taken up most of the "slack" in the system, as one may see from the following more extensive figures on the annual numbers of Ph. D.'s in mathematics:

Year	1951	1952	1955	1957	1959	1961	1963
Total Ph. D.'s	217	237	261	226	299	370	588

Any serious attempt to accomplish the 300-percent increase recommended by the Gilliland report could have a number of serious side effects. The mature research projects of faculty personnel would be slowed down or stopped; the increased number of advanced students would often receive inadequate supervision. Moreover, such a recommendation encourages universities without adequate mathematics faculty to establish Ph. D. programs in mathematics. (Regrettably, this already happens too frequently.) The young students attracted to such schools would receive inadequate training, often in obsolete fields of research; it is a well-established observation that able students so trained are nearly always ruined for further serious scientific work. For these and related reasons, any substantial attempt to realize the recommended extrarapid increase in the number of Ph. D.'s in mathematics is likely to fall short of its goal and is certain to depress the quality of the Ph. D.'s produced.

This is but a sample of the difficulty with manpower studies. They are likely to deal with the wrong questions, such as superficial estimates of overall percentage increases and not with the more vital (and less quantitative) problems of quality or selective increases, say, in applied mathematics. What is required is a better balancing of supply against demand. Not all the industries claiming to need Ph. D.'s and not all the colleges wanting Ph. D. faculty can really make effective use of scientists at this high level of training. The occasional proposal that Government should support graduate training for all competent students through the Ph. D. degree is too vague. (Where is the cutoff point for "competence"?) Sound decisions as to the size of Government-sponsored graduate fellowship programs should balance the number of students at hand against the facilities for graduate training realistically available.

Scientific research finds its most natural home in universities. A university is not just a center of science, it is a center of learning; along with natural sciences and behavioral science, it must include literature and history, linguistics and philosophy—whether or not they are labeled "science." The great universities such as Cambridge, Oxford, Paris, and Harvard usually have a long tradition of excellence in many fields of learning. Science flourishes in such intellectually rich environments. The more reflective sciences, such as mathematics and theoretical physics, are particularly at home there. One may find scientific excellence in particular departments or individuals in less-favored environments, but in such cases stimulus from related fields, especially neighboring sciences, is usually vital.

There are marked differences in quality among the universities of the United States, ranging from very best, through very good and good, to ordinary and mediocre. This spread has met the variety of our educational needs. Though there is no precise way of classifying universities, there is a real gradation in quality, and there is a sharp difference between an ordinarily good university and a really great one (or, as we shall call it here, a *first-rate* one). In a first-rate university the intellectual activity is at a higher pitch and quality than elsewhere. Moreover, intellectual leadership is a lonely business: the first-rate universities are few in number. In France there is traditionally one, in Paris; all others tend to be provincial. This sort of cultural centralization is strongest in France. In England for generations there have been perhaps three first-rate universities (in Cambridge, Oxford, and London); there are now vigorous efforts to get more. In Russia there have been perhaps two; with considerable effort one more (in Novosibirsk) is now being promoted. In the United States 30 years ago there were by general consensus only three truly first-rate universities; in alphabetical order, they were California (Berkeley), Chicago, and Harvard. Today there may be a few more, but not as many as university presidents would have us believe. There is often complaint that too big a proportion of Federal support of science goes to a few centers, but this situation is not due primarily to any geographical favoritism, but to the hard facts that there are not too many first-rate men to go around and that a first-rate center usually develops slowly.

There is a similar situation in each individual science: a variety of departments, headed by a few really first-rate ones. The most original scientists are usually (not always) in a first-rate department, and it is these departments that provide the leadership in the science. The exact situation varies from science to science. Consider mathematics as an example. In the 1930's, for graduate study and research in mathe-

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matics, there were exactly two first-rate centers in the United States: Harvard and Princeton. Twenty years later there was one more: the University of Chicago. The number has increased steadily; today there are seven or eight first-rate centers. The first-rate centers are the ones where most of the leading scientists will be trained. (Sample statistic: 21 of the 47 members of the section of mathematics of the National Academy of Sciences received their doctoral training at just three universities—Princeton, Harvard, and Gottingen.)

Why are there no more centers? Because a first-rate center usually requires some tradition and certainly requires a sufficient concentration (a "critical mass") of first-rate scientists. One cannot have more centers than the population of scientists allows; there is some reason to believe that the "critical mass" necessary for a first-rate center has increased (because of the increasing complexity of science). In mathematics, a dozen years from now, there might well be as many as 15 first-rate centers. In each field of science there will be a similar limit, varying from field to field. Any attempt to get more centers than this limit will simply disperse the leaders of science so much that their vital interactions and their stimulus on groups of students are lost. A single first-class scientist, mixed with many second-rate ones, is likely to be buried and lost. Explicit examples show that one or two first-class students, grouped with large numbers of mediocre students, miss an essential stimulation. Quality depends on concentration.

In the light of this background, one can see some of the grave difficulties attending the current National Science Foundation program to support new "centers of excellence."

The first difficulty is one of terminology. What the Foundation program in fact appears to intend is to bring a number of newer centers up to the good or very good level. The label "excellent" is a misnomer. In any reasonable use of language, "excellence" in universities must refer to the best, the first rate, or the great. Pretense at excellence is a fatal flaw.

Second, the program is vague, because it has no objective criteria for choice (at least, none revealed in congressional hearings). Many institutions would like to become better; the National Science Foundation provides no well-defined or scientific way of choosing which ones to support.

Third, the program does not appear to make realistic estimates of the possibilities. How many very good centers can actually be achieved in each science in the next 5 years? In fields with extreme shortages of scientists, how is the National Science Foundation money to be used to build up new centers? Perhaps to let these centers offer larger salaries to leading scientists at old centers? A large part of the real effect of the program might be increases in the salaries of the limited supply of high-quality scientists, whether or not they move to new centers. This is clearly useful to the scientists concerned, but may not be the effect really intended.

Members of Congress have shown an active interest in the possible development of new centers of science. If this interest is to be effectively realized, it would seem appropriate to get a better start with a more serious study of some of the tough problems involved: How large must a science department in a university be if it is to be a viable center? In what ways does science benefit from the interaction of several university departments close to each other? How does this compare with the effects of dispersion? Does the promising young scientist need the stimulus of a first-rate center? If so, how soon can and should he "go out on his own"? Under current conditions, how many good and how many excellent centers are possible in this and that science? How many good departments does it take to make a viable center? How does one choose between the various competing would-be centers in one underprivileged region? Is it more effective to set up such a center or to pay traveling expenses to an established center? How can the merits of the project system be protected from regionalism? Finally (and hardest), what mysterious process turns a very good university into an excellent one?

Quality and the Future

This paper holds that Federal support of the highest quality work in science is vital to the maintenance of U.S. leadership in the advancement of science and technology. In this context, supporting the highest quality work means selective support and means some defense of such work from the pressures of mass projects and "big science." In consequence then, this paper holds that the rate of growth of science should be positive but moderate. Arguments for rapid growth can be based on impressive population statistics; such figures inevitably miss the crucial point of quality and the fact that too rapid growth must increase the proportion of mediocre work.

And what of the future? Difficult decisions on questions of national scientific policy—these questions or others—will become more pressing. There will be a need for more scientific statesmen and administrators. Experience indicates that the best men of this type are found among scientists who have themselves done work of quality. In this regard, as in others, support of the best builds for the future, and support of the best means asking hard and critical questions.

In closing, I quote from the annual report (1962–63) of Caryl Haskins, president of the Carnegie Institution of Washington:

* * * And so we may be in particular danger of forgetting that the accumulation of facts, however important, is only a secondary business of science. It still represents

at best the "in-grade" phase in the evolution of our insights about the world. The real greatness of scientific scholarship inheres in the thin, tenuous, ephemeral thread of reason and vision and insight and crucial experiment, in the acts of true understanding that from time to time and from place to place over the scientific front lead from one conceptual level to another. In the last analysis, it is by the consistency and the effectiveness of this second, germinal process, through the years and over the whole broad span of the effort, that our scientific progress and our scientific stature must be measured. In publicly misreading the principal business of science, in imagining its basic task to be primarily the accumulating of facts about the natural and the social worlds rather than the winning of significant new insights into their essence, there is a real danger that we could misunderstand its deepest requirements and so compromise its greatness, and its long-term vitality, at exactly the times and places where a superficial view might suggest that we were most actively promoting it. * * *

BEHAVIORAL SCIENCES

by Carl Pfaffmann
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Summary

This paper provides background relevant to the question of balance of support among the various fields of science. Behavioral science refers to those disciplines that study the many aspects of man's behavior as an individual and as a social being. The term behavioral sciences is of relatively recent origin and emphasizes those parts of social science that attempt to solve their problems by empirical and scientific methods. It includes most of contemporary psychology, sociology, anthropology, and certain aspects of political science and economics.

The primary motivation for basic research is the desire to know; at the same time there are many social problems calling for practical solution. These are often symptomatic of deeper questions that may extend beyond the immediate aim of specific programs of applied research or programs of action. Basic research with its deeper and broader study is essential and some examples are given from anthropology, psychology, economics, political science, and sociology. As in other areas of science, increasing sophistication of methods and instrumentation, both in the laboratory and field study as in survey research is becoming increasingly expensive.

There are great manpower needs, especially for manpower trained in rigorous quantitative methods. Accordingly, there is increasing need for support of training as well as for research. It is probably the shortage of manpower to do research that sets the present limits.

The psychological and social sciences currently receive about 5 percent of the Federal funds in support of research. A modulated increase in support is recommended for the already established behavioral science programs of the National Institutes of Health and the National Science Foundation. Mission-oriented agencies are urged to include adequate budgets for basic research that is relevant to their applied research and operations. The continued support of behavioral science is urged in order to ensure its balanced growth. The potential benefit to man from a mature behavioral science is great.

Introduction

This paper will attempt to provide background relevant to the second of two questions posed by the House of Representatives Committee on Science and Astronautics, as follows:

> What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered either within existing levels of overall support or under conditions of increased or decreased overall support?

In particular, this paper will discuss the behavioral sciences. Following several introductory general sections, there will be five sections in which each of the major behavioral science disciplines will be discussed individually. They are treated separately for, although there is much interdisciplinary overlay among the fields, each can be identified by its own professional society and membership, specialized journals, and departmental organizations in universities and colleges. Finally, some overall comparisons of the disciplines and their training and research needs will be discussed.

What Are Behavioral Sciences?

The new term—behavioral sciences—was invented to emphasize the empirical aspects of the social sciences, those parts that attempt to get data on and to analyze actual behavior of human beings. Thus the core of the behavioral sciences includes most of contemporary psychology, sociology, and anthropology, especially social anthropology. It also includes certain aspects of political science and economics, and may even touch upon history and law where those disciplines are concerned with human behavior. The term behavioral sciences is sometimes used as if it were brand new and revolutionary but, in fact, it merely refers to a growing and increasing trend in social science—a trend toward a greater degree of empiricism. Actually, the term is preferred by some because it is more explicit as to the data and even the methods of observation. Thus, it is possible to include among the behavioral sciences those parts of psychology and anthropology where the concern is with the biological foundations of behavior and the object of study may be the individual organism in situations that are not immediately social. These two disciplines provide a bridge with the zoological sciences and the substantial segment thereof that is concerned with behavior. Ethology, a field of zoology, has uncovered findings in the evolution of behavior of great importance for psychological understanding and theory. The International Brain Research Organization associated with UNESCO has a section on behavioral sciences, where the emphasis is on the relation between the brain and behavior. It is yet too early to tell whether this vast global sweep from the physiological to the social determinants of behavior can be encompassed in a unitary section of science. The recent action of the National Research Council in changing its former Division of Anthropology and Psychology to a Division of Behavioral Science, and adding sociology, political science, and economics to its purview, is an attempt to include such a broad coverage among the advisory functions of the National Academy of Sciences.

At one time, social sciences and theories of social science were often formulated in the abstract, as if social systems had their own laws independent of other aspects of behavior. The emphasis on behavioral science brings in the basic concept that economic institutions, social customs, or other organized systems of government are all devices developed through the long range of history by man to control and guide his behavior, satisfy his needs, and maintain stability of sorts in interpersonal relations.

We may note two trends in the current development of behavioral science. Along with the empirical character of the study of behavior and social institutions is the trend toward quantification, the application of mathematical techniques in the analysis of data and the construction of theoretical models. The attempt at mathematical formulation demands greater precision with regard to the concepts, the definition of variables, and the formulation of theorems and hypotheses. The development of computer science has aided this move toward quantification, for it provides the means by which exceedingly complex clusters of data may be analyzed and systems with many variables handled mathematically.

A second major trend is the increasing interdisciplinary nature of behavioral science. Thus, whereas economists had a long-standing interest in decision processes in relation to pricing mechanisms in the marketplace, traditionally economic units were treated as if they were single, rational organisms operating in fully known and simple environments. Behavior-oriented critics of such models have stressed the limited rationality of human decisionmaking, the uncertainties and unknowns in the real marketplace, and the fact that business firms could not be properly treated exclusively as individuals. Thus there has developed a large amount of psychological-economic research on decision processes in general and within the business firm in particular.

Another example might be drawn from the relations between psychology and political science. Political science is concerned with the institutions by which societies are brought under legal order or regulation by law and the way these institutions make and enforce their policies. Thus, understanding of individually acquired beliefs and habits relating to political life and learning theory, as developed by psychologists, is being applied by political scientists in studies of the acquisition of such beliefs and habits.

Looked at in another way, the term "behavioral sciences" emphasizes the development of a methodology that begins to come closer to that familiar to us in the biological and physical sciences. Research in this area should lead to the discovery of the principles at work; the probabilities involved in any situation, and a clearer perception of the factors determining behavior. This field should make contributions to human welfare and the solution of problems of both national and international scope. There are both skeptics of and enthusiasts for behavioral science, and there is concern that there may be overacceptance by the latter of what behavioral sciences can achieve, especially in the immediate solution of practical problems. Nevertheless, the decision of the National Research Council and some Government agencies to include all aspects of behavioral science in their research and advisory functions attests to the growing appreciation of the contribution that this body of knowledge may make to the overall scientific enterprise and the national welfare.

Social Problems and Research in the Behavioral Sciences

When thinking of critical social problems, we tend to think first of juvenile delinquency, illegitimacy, alcoholism, drug addiction, homosexuality, or other extremes of behavior deviation and social disorganization. But virtually all human problems involve social and psychological aspects, and their solution may be facilitated by behavioral science knowledge. In some cases it may seem as if the problem is solely one for the natural sciences; but this may be only because we tend to concentrate our attention on those aspects we know how to solve or have solved before. Thus, the decline in death rates since 1750 in Western countries has been hailed as a triumph of medicine. It was, in fact, mainly a matter of economic development and crude environmental sanitation up to about 1875. After that, the sciences underlying medical practice and public health played an increasing role, but it was precisely at this time that a lack of social science and its application began to turn the achievement of better health into the nightmare of overpopulation. In other words, the adoption of a purely technological or natural science solution to a human problem often results in the creation of additional problems. The automobile, for instance, is an excellent vehicle for improving transportation; its mass use in cities, however, is tending to prove self-defeating. The advances in biochemistry and genetics that enable people with grave genetic defects to reproduce may well pose problems for future generations because the social side of the matter is ignored.

The distinction between basic and applied research, difficult enough to make in the natural sciences, is even harder to make in the behavioral sciences. Yet, for two reasons, the distinction is probably more important in the latter disciplines.

First, there is the danger that purely applied social research to support some action program will be so hedged in by popular prejudices and assumptions that it fails to get to the root of the problem and, hence, becomes trivial. For instance, there is considerable research at present in underdeveloped countries designed to get villagers to accept innovations in agricultural practices. A tacit assumption behind much of this research is that the obstacle to acceptance of innovations is simply the wrong attitude, and that the problem is to find the proper educational and propaganda techniques to alter the traditional way of looking at agriculture. The question of whether the innovation is economically profitable and socially rewarding to the villager in economic and social terms is assumed to be answered affirmatively, but that is precisely the question that takes a great deal of systematic research to answer. If the answer is affirmative, very little propaganda, if any, may be required to gain acceptance of the innovation. To assume that the problem is solely a matter of the wrong attitude is an easy way out, because then the knotty problems of the socioeconomic system, with its rewards and costs for the villager, can be ignored.

The solution to the world's population problem is commonly thought to involve the invention of a foolproof and utterly convenient contraceptive device and the "diffusion" of it to people in backward countries. Millions of dollars are currently being spent in this effort. Yet there is no population that has reduced its birth rate solely in this way, nor is any population likely to do so in the future. As long as the social and economic system is one that rewards people for reproduction and punishes them for nonreproduction, they will continue to bear sizable numbers of children. As yet, no social system has emerged that, in the long run, discourages reproduction to the point of simply replacing the population. Reductions of the birth rate occurring in industrial societies have been brought about, not by contraception alone, but by social changes leading to marital postponement, celibacy, and abortion, as well as a variety of simple but reasonably effective contraceptive practices. Even so, the reductions have not been adequate. The continued debate over the morality of "birth control" confounds the solution to the population problem. In the meantime, the applied research designed to induce people to accept contraception is not only trivial but also harmful, insofar as it turns attention and money from deeper research that would have a likelihood of greater effectiveness.

One example of research carried out with due regard for basic underlying processes and not constrained by a specific point of view is the

Vicos Project in Peru. Cornell University, with funds from the Carnegie Corporation of New York, has for about 10 years been conducting a highly successful experiment in a Peruvian Indian community under the direction of Professor Allan Holmberg of the Department of Anthropology at Cornell. A large hacienda was bought from an absentee landlord. It had been run inefficiently by a hired resident manager. The Indian inhabitants were little more than peons or serfs—ignorant, exploited, and wholly lacking in incentives.

The Cornell anthropologists proceeded to initiate major economic improvements—fertilization and crop rotation—as well as new and improved crops. Capitalizing always on individual incentive, they improved living and housing conditions, education, and public health. A marketing cooperative was established, resulting in much-improved prices for the local commodities. In 10 years the per capita income was multiplied many times. A good school and a clinic with three nurses were established—all out of the decreased income. The depressed and dispirited population became politically aware and now, for the first time, dealt directly with the agencies of the Peruvian state rather than through their absentee landlord. Noting the great improvement in economic, educational, medical, and sociopolitical respects, neighboring communities have begun to imitate the people of Vicos, and the Peruvian Government has begun to organize comparable projects in the vicinity of Lake Titicaca. The conscious and intelligent application of behavioral science knowledge is perhaps the only way to make benevolence and good will genuinely effective in many aspects of international relations.

The second reason for distinguishing between basic and applied work is that the normal aversion to basic research is greater in regard to the social sciences than it is in regard to natural science. One can see the relevance of basic principles in physics and chemistry to achievements in making weapons, television sets, and medicines; but one cannot see so clearly the relevance of special "abstractions" and "jargon" concerning things we know about already, such as taxes, schools, race relations, and the family. The skepticism is increased by the fact that the layman has his own common-sense views about social matters. He objects when these are placed in question by empirical evidence supporting contrary and usually less sweeping generalizations. This is particularly true if the matter is one to which people attach strong positive or negative values.

Behavioral science often deals with human behavior in the context of daily affairs and everday life, both with regard to individual behavior and the institutions in which it is embedded. It is, thus, tied to many practical situations and has future potential application for human and national welfare. Problems requiring behavioral study and solution may be considered to be of two types:

(1) General questions of human behavior stemming from the nature of "human nature," dependent in part on man's biological

and genetic character and the interplay between these and the various social factors and social institutions that condition human behavior individually and collectively.

(2) Man-made problems stemming from the impact of society upon man. One can cite modern technology—in terms of automation and its effect on unemployment, redistribution of jobs and effort, and the individual's leisure time—for example. Advances in medicine and public health have had great impact on the growth of world populations which, in turn, give rise to many social, economic, and political problems.

But practical problems often require action, whereas scientific study requires some degree of isolation from the demands for immediate solutions. The scientist must look at and analyze the situation with some objective detachment in the attempt to develop generalizations applicable beyond the immediate ad hoc situation. Basic research directed toward increasing our understanding of a phenomenon in depth may lead to greater practical effects in the long run than more applied research aimed at implementing some specific plan of action. Often practical problems are symptoms of deeper problems that require more basic study and research.

Status of Behavioral Science Disciplines

Anthropology

Anthropology is concerned with every aspect of the study of man—biological, technological, economic, social, and cultural. It maintains especially close relationships with other specialized disciplines that deal with man, notably biology, economics, geography, medicine, psychology, sociology, and political science. It unites four major subdisciplines that are often pursued independently in other countries: (1) archaeology, or prehistory; (2) ethnology, or cultural anthropology; (3) linguistics, or the study of language; and (4) physical anthropology.

Support of research and future development.—Research in anthropology was very inadequately supported prior to World War II. Since then the situation has changed markedly for the better, and the level of support today is, in general, reasonably satisfactory. One small foundation, the Wenner-Gren Foundation for Anthropological Research, devotes its resources almost exclusively to the support of anthropology. A variety of other private foundations promote research at particular institutions. The Social Science Research Council fellowships for foreign-area research are invaluable, though there are not enough of them. The National Science Foundation and the National Institutes of Health offer very generous support to anthropology, and much important research is supported by the Office of Naval Research, the Special Operations Research Office, and other Federal agencies.

Several important fields of research are still seriously lacking in support. Among them are human genetics, primate behavior, and comparative linguistics (the latter perhaps because of the mistaken assumption that it is a humanity rather than a science). Departments of anthropology are handicapped by a paucity of funds for "small grants"—from a few hundred to a thousand dollars or so—to meet emergencies in ongoing research, such as are currently made available to deans or chairmen in most leading schools of medicine and public health.

One major enterprise for data collection and retrieval, which serves geography, human biology, psychology, and sociology as well as anthropology, is especially deserving of stable support. This is the Human Relations Area Files (HRAF), a collaborative operation of the Smithsonian Institution and 20 American universities. The support of HRAF by Federal agencies, though generous, has fluctuated rather violently in the past as a result of technicalities and shifting interests. Its support urgently needs to be placed on a stable basis.

By far the most serious need of anthropology—and of several other behavioral sciences as well—is for a greatly expanded program of field research in foreign areas, notably Oceania, southern and southeastern Asia, Africa, and South America. Here the scientific objectives of anthropology coincide remarkably closely with the objectives of the United States in international relations. Detailed knowledge of the economy, technology, population characteristics, social and political organization, and cultural values of other peoples is crucial to the successful administration of foreign aid in underdeveloped countries, to the successful prosecution of present and future military operations in such regions as southeast Asia, and to the successful countering of disruptive forces in friendly nations. The ignorance or ignoring of social and cultural realities in many of the programs of the Agency for International Development and the Organization of American States, and in the relations of the American military with the local populations in Laos and South Vietnam, has had the gravest of consequences. Corrective steps can be taken only on the basis of obtaining fuller behavioral science knowledge and utilizing it effectively.

The potentialities in this direction are illustrated by the highly successful cooperation between anthropologists and the U.S. Navy in the Trust Territory of the Pacific (the former Japanese mandated territory) after World War II. When the Navy assumed responsibility for the administration of the trust territory, it established a training program for naval officers at Stanford University under the direction of Prof. Felix Keesing, then chairman of the Department of Anthropology there.

In 1946, the administrative personnel were provided with additional information by a survey of the area made by a group of anthropologists, economists, and geographers, and in 1947 the Office of Naval Research granted $100,000 for a coordinated Investigation of Micronesian Anthropology (CIMA) conducted by the Pacific Science Board of the National Research Council. More than 40 anthropologists from some 20 institutions were sent into the area for intensive ethnographic research over periods ranging from 5 months to a year. These studies provided the Navy with detailed information on most of the island peoples, on the basis of which it was enabled to adapt its administration highly effectively to the varying local cultures and conditions. In addition, the scientists, who lived with the natives and learned their languages, were able quickly to detect sources of friction at the local level and to advise on means of correcting them.

After the completion of the CIMA program, in 1948, the naval administration of the trust territory appointed an advisory anthropologist at the central headquarters and also at those of each of the regional administrative districts. In consequence, the Navy achieved what is probably the outstanding example of an enlightened and humane colonial administration in modern times. Many, though not all, of its administrative practices were adopted by the subsequent administration under the Department of the Interior. The success of this effort has lessons for the application of behavioral science knowledge in other parts of the world.

Economics

Economics is eminently a practical and applied science rather than a pure one, though in recent decades the attempt to gain a basic understanding of its subject matter has led to more and more abstract investigations. The focus of interest has shifted from time to time, in response to the pressing practical problems of the day. For example, in the early part of the century monetary and tariff problems were the liveliest areas of economics. During the great depression of the 1930's the problems of the level of employment and the determination of the national income came to the fore. Since World War II economists have become increasingly concerned with the problems of economic growth, both in this country and in the less-developed countries of the world.

These concerns are pursued in many ways, including day-to-day advice to governments and business firms, polemical writings on controversial issues, and scholarly studies of the fundamental mechanisms believed to have a bearing on practical economic affairs.

The methods used by economists have been changing rapidly over the last 20 years or so. Whereas much empirical work had been done prior to World War II, there is now more tendency to tie empirical research and theory together. Theorizing and mathematical-statistical methods have become more sophisticated. The advent of the electronic computer is causing a revolution in the methods of economic research, which is still in its beginning stages.

The continued development of economics depends to an important extent on the continued development of four new research tools: (1) improved compilations of data through systematic national income accounting, (2) more extensive and skillful use of sampling methods, (3) more powerful methods for the statistical analysis-of-time series, (4) simulation methods, which are a partial offset to the nonavailability of laboratory experiments.

One stimulus for the recent increase in interest in quantitative, empirical economics is its affinity with a field variously called managerial economics, management science, and operations research. This field attempts to apply in practical affairs of business and Government some of the doctrines of economics that previously had been regarded as being doubtless true but too profound and abstract to be of practical interest. When the possibilities of applying these doctrines appeared, interest in elaborating them—that is, basic research into the theory of economizing—flared up. Inventory theory, mathematical programing (priority contested by the Russians, with considerable justification), game theory, and input-output analysis are some of the important elements of this movement. These have been unexpectedly fruitful in expanding the theoretical basis of economics. They have also enjoyed widespread practical application, and have been useful in strengthening the empirical basis of economics as well. It is an understatement, however, to say that there are unsolved problems in all these areas.

Support of research and future development.—As in many fields, there is a shortage of personnel. Economics is widely taught and the country is fortunate in having at least a dozen first-class centers of economic training and research. Yet the supply and output of economists is far from adequate to meet the needs of college and university departments, Government and international agencies, business and consulting firms. Entrance into the field is now encouraged by a variety of fellowship programs, but more are needed.

Many practicing economists in midcareer have not had an opportunity to keep up with progress in this rapidly evolving field. This is particularly true of economists on the staffs of Government agencies and business firms. Fellowship programs that would enable such economists to modernize their skills would be especially productive in the short-run in overcoming the deficiency of economists with modern statistical and analytic techniques at their disposal. The long-run need, however, can be met only by increasing the intake of fresh blood.

Economic research is becoming increasingly expensive. It is not expensive for a scholar to think or to pore over a volume of census reports.

But it is very expensive to gather empirical data specially designed to test a specific hypothesis and to carry out the elaborate computations such a test may require. And it is expensive to use electronic computers in simulation studies, which are frequently necessary to deduce the consequences of hypotheses about such complicated structures as an economy or a business firm operating under the influence of an uncontrollable and unpredictable environment. Projects requiring extensive field research or heavy computations frequently demand budgets of $250,000 or more. The Ford Foundation, the Rockefeller Foundation, the Carnegie Foundation, and a number of smaller foundations have helped meet this need. The Federal Government has long contributed substantially to economic research in a variety of ways, and the relationship has been reciprocal. The statistical and fact-gathering activities of the Government have been the foundation stone of economic research in this country. Not only do agencies gather essential data in the course of normal administrative and reporting activities—the monthly Current Population Survey is an important example—but significant analyses of these data are made by the Bureau of the Census, the Bureau of Labor Statistics, and almost every other agency that is charged with responsibility for economic affairs. The continuation of this fruitful collaboration is of first importance both to well-informed Government economic policy and to the further progress of economic science.

Since World War II, the Federal Government has helped to defray the cost of many studies through the National Science Foundation, the Office of Naval Research, and a number of other agencies. Financial support of this sort is indispensable to the continuation of empirical and quantitative research in economics. Nevertheless, many worthwhile undertakings have been impeded or abandoned for lack of funds. Elsewhere in this report we remark that the principles and practices for the allocation of Government research funds are themselves significant topics for economic research. This is a particularly important program that the National Science Foundation is beginning to develop. More should be undertaken in this area. Special mention should be made of the important problems of economic development and growth. Some of these are factual: We need more hard knowledge about actual conditions in the less-developed countries, the existing technologies, the levels of consumption, the kinds and quantities of capital available, and so on. Many of the problems, however, are theoretical.

Since World War II, systematic attempts to deal with these problems have been begun, including study of such topics as the economics of research and education, the diffusion of improvements in productive technique, and the laws of growth of economies under the impact of population increase and technical advance. These are very difficult problems, but of great significance in the technological world of today.

Technological and economic leadership requires that we understand them.

Political Science

An examination of what political scientists actually do—what they write about and what they deal with in their academic courses—shows that their main concern is Government, politics, and public affairs. Since World War II, the view has been increasingly held that the political scientist's primary concern is with the manifestation of power and influence wherever found, and therefore that religious structures, the business firm, the labor union are as properly objects of his attention as are governments, which make and enforce law. At the same time, there has been a marked increase in emphasis on scientific method in political science study. This is most apparent among American political scientists, but the trend is notable in all Western countries. This trend has had three main consequences for political science in the United States so far:

(1) Political scientists now seek to contribute to a corpus of scientific knowledge about man in his social relationships. Thus, learning theory developed by psychologists is being applied by political scientists in studies of how the individual acquires his beliefs and habits relating to political life. The theory and empirical findings of psychologists and sociologists that relate to "role" are being augmented by political scientists who examine the behavior of man in political parties, legislative assemblies, and other governmental institutions.

(2) Descriptive accounts and evaluative studies are now planned and executed with more rigorous attention to the canons of scientific method. There is increased concern to rest findings on empirical data, increased care in the collection of empirical data, and increased caution in drawing inferences from empirical data. The studies of recruitment of political leaders, electoral behavior, and legislator-constituency relationships, in which political scientists associated with the University of Michigan Survey Research Center are prominent, are illustrations of this trend.

Studies of voting behavior, of what occurs in campaigns and elections, for example, have benefited from increased understanding of the validity of sampling, increased competence in conducting interviews that induce responses in accord with actual beliefs and actions, and increased skill in mathematical treatment of data obtained from interviews and other sources. As a result there is new knowledge about the relationship of various socioeconomic characteristics of populations to, for example, such political behavior as willingness to vote and in other ways to participate in the political process.

Studies of the metropolis, large city, town, and rural community attempt to determine how authority to decide and ability to influence those who make decisions is distributed in a community. A successful study differentiates the roles of the public official, leader of a political party, spokesman for an interest group, man behind the scenes, etc.; what people in each category actually do, how they express themselves and mobilize support for their recommendations; and the measure of influence they exert in making public policies and determining the course of public events. For example, studies show that, in urban communities, the concern for public affairs is widely dispersed rather than being concentrated in a single power structure. This does not mean, necessarily, that there is wide-spread participation in all matters because different groups are concerned with one aspect or another according to their interests in the variety of public issues that arise from time to time. The point is that there seems to be no one group that dominates all political decisions.

These studies are first steps in the development of a scientific literature of politics. But, beyond the collection of empirical data, an increasing number of inquiries are aimed at discovering a deeper theoretical framework for the description of political behavior. Thus, game theory is being applied to decisionmaking of such deliberative bodies as city councils and legislative committees. Efforts to proceed directly to tested propositions worthy of being called scientific, however, are, at best, tentative and exploratory.

(3) There is less inclination than previously to develop doctrine supporting political reform. Political scientists actively participate in party organizations and political campaigns, serve as consultants to governmental organizations, and move in and out of administrative posts. But writing designed to induce change in governmental organization and procedures has been in relatively low repute since World War II.

Support of research and future development.—In spite of the substantial advances in recent years, training for scientific inquiry is not well developed in most American universities that give the doctorate in political science. Graduate courses that examine comprehensively the various theoretical approaches and data-collection methods appear to be available in most graduate political science departments. Integrated study programs that develop high proficiency in particular styles of inquiry are rare, however. And it is unlikely that more than a half-dozen political science departments offer a battery of courses in statistical methods and quantitative analysis; other social science departments (or the mathematics department) typically provide training of this sort for the political scientists.

Without question, the more elaborate descriptive studies are the efforts to go forward in the scientific study of politics will benefit from increased financial resources. Whether special attention by Congress should be recommended depends on the consequences of the recent recognition of political science by the National Science Foundation. If political science receives support from the Foundation comparable to that for economics and sociology during the past few years (and assuming support from foundations in keeping with past experience), it is probable that political scientists will have available as much financial support as they can fruitfully absorb in the decade ahead.

Two closely related sets of questions illustrate the Nation's need for fuller understanding and identify challenges that, hopefully, may respond to types of inquiry that political scientists are now prepared to pursue. These questions relate to loyalties and disaffections in the American population; and to readiness and reluctance to respond to authority. For instance, we know almost nothing about the distribution and political effects of an individual's attachment to his many associations, private and public; about the appeals each makes for his support or how he decides where to place his support when the appeals are in conflict. Response to authority—the authority of parents, of a moral code, of government—will be understood in large part, no doubt, when we understand the spread and intensity of loyalties.

These questions can be fully illuminated only by the collaborative efforts of all the social sciences. But they are questions in which political scientists have a special interest and for the study of which they have special preparation because of their longstanding attention to organization of political authority, and to law, which is one expression of political authority. The dispatch and sureness with which political scientists penetrate these and other areas of critical social significance, whether as leaders in study or as collaborators only, will depend on the number of workers and their quality of training.

Psychology

Psychology is concerned with the scientific study of learned and innate behavior of man and lower organisms as determined by biological endowment and the influence of the physical and social environment. It deals with performance and skill, perception, learning, thinking, motivation, emotion, personality and social interactions, including deviations or abnormalities therein.

The following paragraphs will illustrate the wide range of topics investigated by psychologists. One of the most fascinating is the recent discovery by a physiological psychologist of the so-called "pleasure centers" of the brain. Animals with electrodes permanently inserted in certain areas of the brain will rapidly learn to press a key or other device to turn on brief, mild, electric shocks to particular brain areas associated with pleasure sensations. They may work for many hours, sometimes to exhaustion, for this reward, and brain self-stimulation is often preferred over other natural incentives like food, water, or sex. Further study of the same effect by neurosurgeons in human psychotic or terminal cancer patients indicates that brain stimulation may activate pleasurable or other emotional effects strong enough to alleviate pain and distress or lead to a change of mood. From such studies is emerging a clearer conception of the relation of brain processes to emotion and motivation. The study of how drugs, especially psychoactive drugs, affect these systems and, thus, behavior is now a particularly active field that promises to be of value when its principles are applied to the treatment of mental illness. Research of this character is obviously interdisciplinary, often involving collaboration between biomedical and behavioral scientists.

Another important research area is that of conditioning and learning. Such studies have demonstrated, for example, the importance of reward, success, or "reinforcement" in the learning process, and of the timing of reinforcement. Laboratory studies of animal and human learning were basic to the development of teaching machines and programed instruction. This is an example of how basic research can lead to application. We are just at the beginning of the applications of this technology to education. Programed instruction with teaching machines is clearly important as a mechanical aid that could help alleviate teacher shortages; moreover, the attention to preparation and planning of material for machine use forces better organization of material to be taught generally. It is conceivable that combining teaching machines with computers would make both devices even more adaptable to instructional uses. Practice in problem solving, i.e., "playing against the machine" in solving problems of logic and mathematics or in other reasoning tasks, might add another dimension to teaching beyond rote memory. There is already some evidence that quite young children given an "intelligent typewriter" can learn language skills at a remarkably early age. It is quite likely that we have not stretched the intellectual capacities of our young people and that, under appropriate stimulation, genius might be less of a rarity. The converse, namely that failure to provide stimulating environments can lead to a "deprivation syndrome" with attendant emotional as well as intellectual deficits has been well documented. Indeed, recent studies seem to indicate that a stimulating and novel environment leads to increases in brain tissue as compared with the effects of a monotonous one.

Computer science has other significances for psychology. Thinking and problem-solving processes have been simulated on computers. Specific computer programs for alternate ways of solving problems can be compared with the methods used by human beings in solving these problems. By this means, various hypotheses or assumed steps in reasoning must be made more explicit so that they can be converted into "computer language." This very requirement improves the precision of stated hypotheses, with the result that research on mental processes is more penetrating.

Although psychological tests are the backbone of applied psychology, many fundamental problems on individual differences in intelligence, the interrelation of abilities, and the "structure of the mind" still remain to be answered by basic research. The further theoretical study of psychological tests is probably the best hope for improving their efficacy as practical devices for selection and placement in education, industry, the military, and other situations where special skills are required.

In the measurement of personality traits and characateristics, psychology attempts to deal with more complex aspects of human behavior. Such traits as extroversion-introversion or dominance-submission were early subjected to analysis, but other personality factors are now being given further study and assessment. Personality tests should be used with due caution and qualifications, but some of the recent criticism of personality tests in business and industry has been overdone. Clinical psychologists use many testing procedures for personality assessment. Here, primary reliance is often placed upon diagnostic tests administered individually, face to face.

Social psychology, dealing as it does with human society, brings the psychologist close to other behavioral sciences, sociology, anthropology, political science, and economics. Once heavily identified with the study of attitudes and opinion polling, social psychology is now moving in the direction of more precise experimental methods, as in small group research. For example, miniature bargaining or conflict-resolution situations can be studied with groups that use different methods to gain their ends. But many of these experimental situations utilize relatively homogeneous white, U.S., middle-class groups as subjects, and thus fail to take sufficient account of variations in cultural and socioeconomic background. One contemporary research trend is to correct this limitation. Of particular note is current worldwide investigation on the semantics of words and phrases in different languages and cultures, as perceived and reacted to by members of those societies and cultures. Such work should help clarify some of the psychological factors underlying international misunderstanding.

Support of research and some future developments.—Until the National Science Foundation's Division of Social Sciences was established in 1961, it could be said that the balance of support for psychology did not give equal weight to all aspects. Experimental and physiological areas were early included in the biological sciences program in the National Science Foundation under psychobiology, in the Office of Naval Research under psychophysiology, and in various military services on projects in support of human-factor research and engineering psychology. Clinical psychology also had been well supported, first under the program of the Veterans' Administration and then under the National Institutes of Health in connection with training for applied work in mental health and rehabilitation. Basic psychological research in the clinical field, as well as clinical evaluation and development of methodology for mental health care, is an intrinsic component in the program of the National Institute of Mental Health. Other relevant studies find support in other National Institutes, as, for example, the new Institute of Child Health and Human Development, with its broad charter for all aspects of normal growth and development. Basic research aimed at fundamental understanding of human behavior generally is an essential foundation for progress in attacking the psychological aspects of mental retardation, psychosomatic disorders and other behavior deviations. Even where diseases seem to be largely organic in origin, psychological processes may aggravate or influence the condition. Indeed, one of the currently most active fields of research in physiological psychology is on the neural and chemical determinants of normal as well as deviant behavior. Psychologists and other behavioral scientists work with biomedical scientists on many of these problems, and great advances here can be expected in the years immediately ahead.

I have already pointed to the potentialities of applying some of our basic knowledge of learning and the concept of reinforcement in advancing and extending our educational goals. Another major development just on the horizon is the extension of learning theory to deal with complex situations of two kinds: (1) behavior change in clinic, therapy, and retraining settings, and (2) developmental psychology. Advances at this level would contribute to the furtherance of knowledge and skills in dealing with the psychological aspects of mental health and behavior disorders.

Other possible applications of psychological science can be indicated by new trends in social psychology in its relation to other behavioral sciences. Thus, some economic theorists have called attention to the importance of national attitudes and social values that seem necessary for modernization and economic growth. But often these are assumptions about human nature, and the relation of such concepts to economic success requires further study. One psychologist has indeed begun to obtain evidence in the United States and from cross-cultural studies on the "achievement motive." Advancing knowledge along this line should provide a better understanding of the psychological climate underlying success in modernization and economic development, and thus will not only advance our own national interest but facilitate our efforts in the increasingly crucial task of nation-building where we have such a national commitment.

As in other fields, there is the never-ending need for research investigators and research facilities coupled with the increasing need for university and college teachers of psychology. With the increasing utilization of psychologists in many different capacities in basic research, clinical practice, counseling, applied research and development, and other fields, it is essential that an adequate supply of manpower be ensured through the support of training. Research and graduate education are interdependent in the behavioral sciences, as they are in all science. The National Science Foundation and the National Institutes of Health, in addition to certain aspects of the Department of Defense, National Aeronautics and Space Administration, and National Defense Education Act programs, have provided important support of graduate training for research in psychology. These should be continued or strengthened.

Sociology

In its broadest sense, the field deals with the structure and behavior of human societies. It is concerned with the institutions and groups that compose a social system, and how these interact, not excluding an interest in how the individual develops in society. It embraces the study of principles of social behavior and the development of systematic methods for such study. As one of several social sciences, it tends to specialize on those aspects of society that are not specifically dealt with by political science and economics.

Although there is still some confusion in the public mind between sociology and social work, the development of the field has been in the direction of a basic social science. It supplies principles and research techniques for the investigation of social problems, but generally leaves action programs to social workers, administrators, and other practitioners.

To obtain objective information about aspects of society that are often emotionally charged, sociologists rely heavily upon statistical methods and a habit of methodological criticism. They have pioneered in the application of quantitative methods to the study of attitudes, interpersonal behavior, residential segregation, labor-force participation, social mobility.

Among noteworthy recent tendencies is a push for more rigorous training in mathematical and statistical skills. In addition, there is a clear trend toward greater specialization among sociologists. New fields, such as political sociology (the application of sociological techniques and theories to the study of political behavior), are emerging. A third trend is the widening employment of sociologists in full-time research positions and the tendency, within universities, for an ever-greater number to be employed outside of sociology departments—e.g., in schools of business, schools of public health, medical schools, and industrial-relations bureaus. Such a diffusion of sociological work is due to the gradual recognition that most human problems have a sociological aspect and require sociological techniques for their investigation.

Sociologists have played a major role in founding and administering survey research centers throughout the country—for example, the Bureau of Applied Social Research at Columbia, the Survey Research Center at the University of California, the Institute for Social Research at the University of North Carolina, the Social Relations Laboratory at Harvard University, and the Detroit Area Survey at Michigan. Their interest is increasingly extending to foreign countries, where American sociologists are conducting numerous field studies and where, regardless of initial sponsorship, data from all studies are being analyzed and compared in American centers. At the University of California, for example, there is an International Data Center for the collection and secondary analysis of survey data from all over the world.

Future promise and needs.—As a subject of graduate instruction, sociology has more than doubled its output of Ph. D.'s since 1957. In 1962, the number of doctorates in sociology was about equal to the number in botany and almost half the number in mathematics. Yet the demand for trained sociologists currently far exceeds the supply. The reason for this is primarily the expansion of sociological research and the rising use of sociologists in professional schools and in governmental and private agencies.

There is no foreseeable end to the rising demand for sociologists. More funds are constantly being made available by Government and by foundations for research on the pressing problems of our own and other societies; also, undergraduate enrollment in sociology courses is expanding, at least as fast as general college enrollment, and probably faster, thus requiring more qualified teachers.

At the present time, the greatest need is for support of graduate training. This lack is seen partly in regard to fellowships and scholarships for graduate students, but it is much more crucial with respect to the number of professors available to give graduate instructions and the facilities for apprentice (laboratory) training in social research. The shortage of professional positions is due to the somewhat late and rapid emergence of the field in American universities (Princeton, for example, did not begin sociological instruction until 1944, and the University of California at Berkeley did not do so until after that). Funds are therefore needed to help more universities qualify for graduate training in sociology and to help those that do qualify employ more professors in ratio to the number of graduate students. In addition, a sizable effort needs to be made to provide sociology departments with facilities for research training. This is especially important for graduate instruction. Since competence in research cannot be acquired solely in the classroom but must be learned by practice under detailed supervision, each department needs funds for a research facility in which the student participates in organized investigations, utilizes the appropriate techniques of interviewing people and handling data, has access to calculating and computing equipment, is surrounded by sources of basic data and handbooks of methods, and above all, has personal contact with one or more instructors engaged in research of the same character and whose duties include supervision of student research activity. The need for laboratories was fought out in university organization decades ago with respect to the natural sciences; it is being fought out now with respect to the social sciences, and although the battle is being won, the level of support for laboratory work is far below what it properly should and could be.

An example of the use of sociology is to be found in the work of the research branch of the Army during World War II. Under the direction of Samuel Stouffer, a sociologist on leave from Harvard, a research team composed of social scientists systematically made approximately 300 surveys among our soldiers for purposes of solving problems of morale, group friction, combat performance, and training.

Demography

Demography is the science dealing with population, including not only the methods of enumerating people, but also the causes and consequences of changes in the number of people. It is concerned with rates of mortality, reproduction, marriage, and migration, and with closely related characteristics of the population such as the age-sex composition, marital status and family organization, geographical (e.g., rural-urban) distribution, and occupational structure.

As such, it is one of the oldest sciences of man, although accurate information began to be available only around 1800, when census-taking was started and registration data were utilized in a few countries. Gradually, censuses and vital statistics were improved and their coverage was extended to more and more nations. By now virtually all the world's countries have had a modern-type census, in most cases including a census within the last 20 years. The scientific character of demography has seldom been questioned. It is a field in which mathematical and statistical methods form the core, and in which an empirical approach is taken for granted.

Of particular interest in the present, is the observation that countries that have become industrialized have gone through a "logistic" pattern of population growth. The demographic cause of this is known to be the decline of mortality with economic improvement, followed after a lag by eventual fall in the birth rate. The important question is whether currently underdeveloped countries are going through the same transition. The answer is clearly that they are not doing so, at least not in a comparable manner, for their rates of population growth far exceed those experienced by the older industrial countries in their heyday of human multiplication, and the pattern and causes of mortality decline are far different.

A great portion of demographic research in the United States is carried out by Government agencies such as the Census Bureau, the National Center for Health Statistics, the Bureau of Labor Statistics, and the Department of Agriculture. An increasing amount of research is being done by State and local governments and by university centers of population research.

Promise and needs.—In recent years, the supply of people trained in demography has been critically small in relation to the Nation's requirements. One graduate department where training is offered has regularly had about 20 requests for every candidate it has turned out. One Government agency—the National Center for Health Statistics—has developed a costly and admittedly improvised in-service training program because of the lack of candidates for numerous unfilled jobs. A similar situation prevails abroad. In the absence of suitable training facilities, the United Nations has set up two training centers (one in Santiago, Chile, the other in India) and is contemplating two more, one in Southeast Asia and another in Central America.

The critical shortage of trained demographers arises from both the supply and the demand side. On the demand side, there has been an enormous rise in the need for demographers in the governments of the world and in international agencies. Economic planning inevitably means that systematic account must be taken of future population changes, whether at the national or the local level. In addition, the unprecedented rise in the rate of population growth in the world as a whole and in the underdeveloped countries in particular has given rise not only to great popular concern over population problems but to governmental concern and the formulation of population policies. About 15 underdeveloped countries now have population policies designed to lower the rate of population growth. Demographers are in demand to conduct population research bearing on economic development, population policies, and city planning; to teach in universities in a growing number of courses dealing with population problems; and to make population projections for State and local as well as for national units.

On the supply side, however, the field suffers from the peculiar fact that it straddles a chasm in the organization of universities. It is in part a biological science and in part a social science. Furthermore, it is heavily statistical and mathematical but also has close connections with medicine, economics, and sociology. It is not big enough to constitute a major division of a university and yet cuts across the traditional divisions in such a way that it cannot be assigned to any one of them without serious loss. As a consequence, the subject usually gets placed in some department where it is subordinate to the other interests of a wider field and cut off from vital parts of its own interest. There is no department of demography in any American university (a center for population studies has just been formed at the Harvard School of Public Health, but an oceanographer has been asked to head it, making this an exception that proves the rule). Ph. D.'s in demography are not offered at any American university. The subject is taught variously, if it is taught at all, at the graduate level, in departments of sociology, economics, statistics, and biology. As a result, virtually no students are adequately trained in demography and only a few are given even a passable training.

The shortage of trained demographers is a serious situation in the scientific development of the country. We believe, furthermore, that the situation can be rapidly remedied with governmental support in collaboration with professional demographers and the major universities. One university, with aid from the National Institutes of Health, is currently working on an interdisciplinary curriculum for the granting of M.A. and Ph. D. degrees in demography. Grants to universities willing to follow suit (as many of them probably will be) would measurably speed up an evolution that is doubtless likely to occur eventually anyway. It is our opinion that the current number of people getting adequate graduate training in the field (approximately 20 per year) could and should be quickly increased by about five times within the next 5 years, rising speedily after that. When it is realized that the highly industrial nations have numerous population problems of their own, and that the population problems peculiar to underdeveloped countries will probably get worse, it seems highly likely that the demand for professional demographers will continue to rise at a rate that can be met only by extraordinary measures.

Summary of Present Financial Support for Research in the Behavioral Sciences

According to the National Science Foundation Survey of Science Series, *Federal Funds for Research, Development, and Other Scientific Activities,* NSF 64–11, the estimated Federal research support, both basic and applied, for all behavioral sciences (psychology plus social science) amounted to \$139 million for 1963 and \$203 million for 1964. These totals are consistent with the study of overall Federal research support in the behavioral sciences reported by the American Behavioral Scientist in vol. VII, 1964, No. 9. This magazine included in its survey much applied research related to operational programs such as the Census Bureau, Department of Agriculture (Agricultural Marketing Service, Agricultural Economics), Bureau of Labor Statistics, and others, both intramural and extramural.

NSF 64–11 gives the breakdown of obligations for basic research and total research by all fields as estimated for fiscal year 1963. This is reproduced in part in table 1.

Another National Science Foundation Report, NSF 64–14, shows that in 1960, 272 private foundations dispersed $437.4 million for a variety of purposes, but those supporting research numbered 177. Research support including endowments and capital expenditures totaled $89.4 million, of which $76.1 million was allocated to the direct operating cost of research. (See table 2.)

TABLE 1

[Millions of dollars]

Fields	Basic research	Total research	Obligations basic research as percentage obligations for research
Life sciences	$403	$1, 021	39
Physical sciences	934	2, 930	32
Psychological sciences [1]	33	67	50
Social sciences [1]	23	72	32
Others:			
Operations research	2	155	1
Field conflict techniques, etc	....	....	....
	1, 395	4, 245	33

[1] The National Science Foundation in its analysis of employment of social scientists and psychologists in the Federal Government uses the following definitions:

"*Psychological sciences* are those dealing with behavior, mental processes, and individual and group characteristics and abilities. This category includes research on animal behavior, sensory, perceptual and physiological psychology, learning, motivation, higher mental processes, clinical psychology, personality, educational psychology, engineering psychology, personnel psychology, and social psychology including group processes, interpersonal relations, opinion and attitude change, and developmental psychology.

"*Social sciences* are directed toward an understanding of the behavior of individuals as members of a group. These include such sciences as cultural anthropology, economics, history, political science, sociology, etc. In addition to work done in disciplines or subjects traditionally considered as being social sciences, this should also include work done in other disciplines or subjects where the work is undertaken primarily for the purpose of understanding group behavior."

TABLE 2 [1]

[Millions of dollars]

Field	Basic research	Applied research	Total
Life sciences	$21.7	$13.5	$35.2
Physical sciences	6.0	1.0	7.0
Psychological sciences	1.9	1.5	3.4
Social sciences	9.5	14.3	23.8
Education, others	2.8	4.0	6.8
	41.9	34.3	76.3

[1] Based on NSF 64–14

In the same period (1960), the total Federal funds for basic research were $747 million (National Science Foundation 11th Annual Report, 1961) as compared with $42 million from foundations. Behavioral sciences, especially social sciences research, have fared better percentage-wise in support from private foundations; about 30 percent of foundation research support going to behavioral sciences. In fact, the pattern of support by private foundations seems to be the converse of that by the Government. Even within the behavioral sciences field, psychology, which is better supported than other behavioral sciences, receives less foundation support than the social sciences. Although the pattern of foundation support may rectify in part an imbalance in Federal behavioral sciences support, it may be questioned whether this will be adequate in the future to insure adequate growth and development of a mature behavorial science that increasingly utilizes more sophisticated and more complex research techniques.

The growing costliness of behavioral science research can be illustrated by one example from survey research. A properly conducted national, regional, or metropolitan survey based on 2,500 approximately 1-hour interviews can cost anywhere between $50,000 and $125,000 (1). The staff, skill, and facilities needed include (1) adequate sampling for purposes of the study, (2) questionnaire construction, (3) interviewing, (4) data processing, and (5) statistical analysis. These requirements would overtax the normal resources of the university, and we have witnessed the establishment of a number of semi-autonomous survey and opinion research centers or frankly independent commercial firms. Government agencies, however, seem to prefer university centers to commercial firms because of the former's emphasis on scientific and technical purity.

The increasing cost of surveys reflect only in part the general rise in general level of costs. Surveys have become increasingly expensive as their technology becomes more complex and as greater precision from the data is demanded by researchers. For example, a study by the National Opinion Research Center of the prestige of occupation in 1947 cost little more than $9,000. An improved restudy of the same topic currently underway will cost more than $150,000. Because few of the Government or even private sources that support social science research give grants of $100,000 to $200,000 without being convinced of the practical importance of the research, large-scale survey research is generally "applied" social research, i.e., research whose results will have some immediate bearing on policy formation. Not all the work of large-scale survey centers is applied, but purely basic projects with no particular applied interest are infrequent or usually supported on a lesser scale. In recent years the establishment of the Social Science Division of the National Science Foundation and behavioral research study sections within the National Institute of Mental Health has considerably increased the funds available for basic research. It is important that such a trend be continued and expanded.

In terms of total funds allocated for research by the Federal Government, the whole behavioral sciences field, including psychology, is below the life sciences and physical sciences. This is due in part to the less well-developed character of the field and the less costly nature of the installations it requires, and in part to the more recent establishment of support in the social sciences by Federal agencies. In the National Science Foundation itself, support for basic research was divided as follows:

TABLE 3 [1]

Area	Amount	Percent
Mathematics, physical and engineering sciences	$59, 895, 475	56
Biological and medical science (includes some neurophysiology, experimental psychology under psychobiology)	38, 394, 851	36
Social sciences, including social psychology and personality research	8, 956, 172	8
Total	107, 246, 498	100

[1] NSF 1963 Annual Report.

The survey NSF 64–11, *Federal Funds for Research, Development, and other Scientific Activities,* page 28, gives the following breakdown for the support of research.

TABLE 4 [1]

Field of Science	Actual 1962	Estimates 1963	Estimates 1964
Total, all fields (millions of dollars)	$2, 977	$4, 245	$5, 785
	Percent distribution		
Physical sciences	63	69	74
Life sciences	28	24	21
Psychological sciences	2	2	2
Social sciences	2	2	2
Other sciences	5	4	2

[1] NSF Table X.

NOTE.—Rounding of figures might throw off some of the totals.

For the support of basic research, table from page 38, NSF 64–11, is reproduced.

TABLE 5 [1]

Field of Science	Actual 1962	Estimates 1963	Estimates 1964
Total, all fields (millions of dollars)	$1, 085	$1, 395	$1, 782
	Percent distribution		
Physical sciences	66	67	68
Life sciences	29	29	27
Psychological sciences	3	2	3
Social sciences	2	2	2
Other sciences	(2)	(2)	(2)

[1] NSF table XIX.

[2] Less than 0.5 percent.

Thus, behavioral sciences research support is a considerably smaller percentage of the total than is available for other sciences.

Estimate of Manpower Situation

Professional Societies

Some estimate of the professional scientific pool in the behavioral sciences might be given by the size of the national associations as follows:

American Psychological Association	23,000
American Anthropological Association	1,200
American Political Science Association	7,152
American Sociological Association	7,836
American Economic Association	11,285
Total	50,473

Psychologists outnumber other behavioral scientists, but considerably more than half are practitioners or in applied fields. Anthropology is by far the smallest group. For comparative purposes, membership in other selected associations are given as follows:

American Medical Association	191,239
American Chemical Society	99,475
American Institute of Physics	35,165
American Mathematical Society	9,515
Federation of American Societies for Experimental Biology	7,893
American Institute of Biological Sciences	[1] 70,000

[1] Approximate.

The report, *American Science Manpower of 1962*, NSF 64–16, surveyed more than 200,000 scientists reporting to the National Register of Scientific and Technical Personnel:

		Percent
Physical sciences	122,148	57
Biological sciences	37,943	18
Psychologists	16,791	8
Sanitary engineering	4,923	2
Other fields	33,135	15
Total	214,940	100

The other fields included engineering, social sciences, humanities, and other specialties that were not analyzed.

Ph. D. Production

The studies of U.S. doctorate production by the National Academy of Sciences—National Research Council (Publication 1142) is the source of the following statistics.

The trend of overall production of Ph. D.'s in all fields shows a 7 percent annual increase. At the present moment, the current rate of growth is 10 percent, but because of an earlier falling off, the production is somewhat below the long-term 7 percent figure. This growth rate exceeds that of the population, which is 2 to 2.5 percent per annum. Ultimately, the two curves will meet, but in the next two to three decades this does not put a limit on potential growth.

There is as yet no indication that we have reached a ceiling in ability or aptitude for graduate training in science. Many factors other than ability, of course, influence choice of advanced academic training. Further, it is not implied that all who are capable should become candidates for the Ph. D. or even the Ph. D. in science. But it is assumed that each individual who has the ability and the interest should have no impediments or constraints placed in his way if he desires to go on to advanced work in the sciences.

In recent years doctorates in the behavioral sciences have averaged about 18 percent of all doctorates and 27 percent of all science doctorates. In 1962, behavioral science doctorates fell off to 25 percent of science doctorates from a high of 29 percent in 1958.

TABLE 6.—*Numbers of doctoral degrees awarded in behavioral sciences (not including history) and physical plus biological sciences. Percentage of doctoral degrees in behavioral sciences of all science doctoral degrees*

Year	Behavioral sciences	Physical and biological sciences	Total	Percentage of behavioral sciences
1955	1,615	4,239	5,854	28
1956	1,505	3,926	5,431	28
1957	1,483	4,169	5,652	26
1958	1,671	4,143	5,814	29
1959	1,712	4,473	6,185	28
1960	1,723	4,820	6,543	26
1961	1,908	5,169	7,077	27
1962	1,936	5,941	7,877	25
	13,553	36,880	50,433	27

Table 7 gives some idea of the relative growth to the various behavioral sciences from 1955, a year roughly during the plateau period. The index for biological and physical sciences is included for comparison.

In the analysis of this period, the NAS–NRC report notes that the physical sciences, which were on a plateau throughout the early and middle 1950's, had a new growth spurt beginning in 1958, probably attributable in large part to the cumulative effects of the governmental fellowship programs. Such a plateau was less extensive for the biosciences and still shorter for the social sciences. The growth curve for social science should be watched because since 1960 it does not seem to have kept pace with that for the other science fields or for the arts and professions.

Conclusions

The preceding account has attempted to show how basic research is fundamental to the growth and vitality of the behavioral sciences as science. It also cites certain examples of how the behavioral sciences have relevance for the national welfare and international leadership of the United States. This report could have been considerably lengthened by adding specific examples of direct military and defense applications of behavioral science in personnel selection and training, human factors applications to engineering practice, design of weapons systems, manpower allocation and management, oversea operations and nation building, persuasions and motivation, strategic planning, civil-military relations, analysis of alliances, and international relations generally. These are over and above the important problems already mentioned: overpopulation, economic development, race and social conflict, and psychological aspects of mental ill health.

Simply to enumerate these as problems of human behavior is not sufficient justification for labeling the disciplines that study them as scientific. Rather, we have tried to show how the behavioral sciences are increasingly applying scientific method to their solution. Frequent reference throughout the preceding paper has been made to the increasing use of precise and quantitative methods. Although this can be taken as a sign of increasing maturity, it should be remembered that the ability to quantify and to make precise measurements is less important than discovering the right thing to quantify—finding the significant questions to ask of experiment and observation. The questions for behavioral sciences are often at the prequantitative first stage; in other cases, the phenomena being studied may not be suited to quantification.

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TABLE 7.—*Numbers of doctoral degrees awarded per year in behavioral sciences (not including history). "Miscellaneous behavioral sciences" includes geography, area studies, and other and general* [1]

Year	Psychology		Anthropology		Sociology		Political science and public administration		Economics		Miscellaneous behavioral sciences	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
1955	733	100	55	100	188	100	214	100	374	100	51	100
1956	627	86	57	104	170	90	248	116	314	84	89	174
1957	723	99	65	118	117	62	202	94	304	81	74	145
1958	780	106	73	133	174	93	226	106	322	86	96	188
1959	809	110	67	122	183	97	230	108	338	90	85	167
1960	762	104	75	137	156	83	251	117	375	100	104	204
1961	870	119	59	107	183	97	264	123	434	116	98	192
1962	871	119	86	156	195	104	266	124	407	109	111	217

Year	All behavioral sciences		Physical and biological sciences	
	Number	Percent	Number	Percent
1955	1615	100	4239	100
1956	1505	93	3926	93
1957	1483	92	4169	98
1958	1671	103	4143	98
1959	1712	106	4473	105
1960	1723	106	4820	114
1961	1908	108	5169	122
1962	1936	120	5941	140

[1] NAS Pub. 1142, pp. 10–13.

The practical solution of social problems will require action programs that are the province of legislators and administrators. However, the steps to be taken or the alternatives to be chosen can be guided, and their likelihood of success can be assessed with the help of behavioral science and behavioral scientists. The boundary line between applied and basic research in this domain is at best difficult to draw. Pure or basic research is usually directed toward increasing our understanding of human behavior without any immediate concern for social action. Here is the paradox, for many of the phenomena the behavioral scientist studies demand that he be close to the scene of action. But the pressures for answers and applications are often too great in the "applied places" (i.e., the scientists spend too much time giving briefings and too little in thought and experimentation). A lack of relevance may be the danger in the "pure places." Yet much that is already being done just because we want to find out more about human behavior is directly relevant to crucial issues of our time with a minor shift in materials, subjects, or emphasis.

Some who are concerned with the role of behavioral science in the national scene have urged that greater use be made of behavioral scientists in action programs, not simply as advisors but in "on-the-spot" assignments. Not only would they use their special skills here, but also they could "feed back to the discipline" problems that need investigation. Here, there is a double responsibility; to the governmental body or administrative officer, to employ behavioral scientists in appropriate spots, and secondly, to the profession to provide the needed manpower. This latter may be much the most difficult, for the academic orientation is still a tradition and the manpower is scarce. Such arrangements as the Congressional Fellowship Program should bring the young behavioral scientist closer to an understanding of public policy formulation and encourage utilization of behavioral science knowledge and techniques in appropriate areas of governmental operation.

The relation between Government and science—indeed, the very questions that initiated this series of reports—is a topic with behavioral science ramifications. It seems taken for granted that science, especially natural science, is a major stimulus to economic development. Science and technology are, by definition, devoted to improving the means for doing whatever human beings want to do; and it is science that furnishes the fudamentals on which technological progress is built. However, there is a paucity of scientific investigations of science itself. Some of the questions that might be asked are economic; for example, can one assess the returns in scientific achievement per million dollars expended? If so, how can such returns be maximized? Other questions are psychological and demographic: What are the limits to the pool of human resources and intelligence for scientific work? How does training effect scientific creativity and productivity? Some are sociological: What conditions in the family or school contribute to the motivation to go into science? Others are organizational: How do the conditions of work, pay, or social prestige influence productivity? How do large-scale organized laboratories compare with more individualized small operations with regard to scientific creativity? Is there a critical size for the productivity of a laboratory group? What is the proper balance between basic and applied work or among the different fields of science? Although work on some of these questions has begun and there have been pioneer studies of the psychology and sociology of science, a much more systematic analysis of science itself utilizing the methods of behavioral science is called for.

The economic, sociological, legal, and political aspects of science are of major significance in the further development of science and in the wise expenditure of funds and development of policies aimed at that goal. They are also of great importance in assessing the effects of science, not only on the economy but also on the society in which it comes to play such an essential role, as it does in our society. The support of studies in this area by the National Science Foundation, as outlined in *Current Projects in Economic and Social Implications of Science and Technology, 1963*, is aimed at correcting this gap in our knowledge. These studies include highly applied agricultural economics, econometric studies of research and development, the sociology of science and scientists, the administration, organization, and management of science and international and foreign studies, e.g., of technology change and balance of power.

It might be worthwhile for the appropriate congressional committee to consider asking the Division of Behavioral Sciences of the National Academy of Sciences—National Research Council to establish an advisory panel, project, or study group specifically focused on questions of the economic and social implications of science and technology oriented towards congressional concerns in this area.

Finally, we come to the question of overall distribution of support in the national budget for behavioral science. It is clear from the tables showing relative support of the different sciences that the behavioral sciences (listed therein as "Psychology plus Social Sciences") receive considerably less support than do the physical and life sciences. Even allowing for the fact that some of the behavioral fields may have been classified under the "Biological" heading, the support is still relatively small. The numbers of people involved are considerably fewer than in natural sciences; thus, the imbalance in research support is in some measure a reflection of the smaller numbers of professionals engaged in research. Then, too, behavioral science is largely "little science," although in some of the preceding sections, the cost of adequately large-scale surveys, or documentary centers of wide scope, and so forth, were cited. In some fields, support was rated as good. In certain other fields it was lacking, and in sociology, demography, and political science, special mention was made of training needs, although this could be said for all of them.

The consensus seems to be that support for basic research in the behavioral sciences could be improved, but that any increase substantial enough to match the physical sciences is not called for at this stage. Sufficient funds should be provided to ensure that all worthwhile research projects are supported. Evidence on this could be forthcoming from the staffs of the National Science Foundation and the National Institutes of Health with regard to the number of worthwhile projects in this field that could not be supported because of lack of funds. Another paper in this series, by Dr. John Willard, cites data on the percentage (based on dollar value) of proposal receipts that were supported by the National Science Foundation of those submitted in 1963, 1964, 1965 (est.). In the social sciences, the percentage of support ranged from 27 to 30 percent; in the biological and medical sciences, from 26 to 30 percent, and in mathematics, physical, and engineering sciences, from 15 to 22 percent. Although dollar value is not a criterion of scientific merit, it was noted that many of the projects that could not be supported, or could only be partially supported, were of outstanding merit. Other agencies are encountering a similar situation. It would seem wise to let the demand set the stage, provided always that the criterion of excellence is adhered to. At the same time, there should be an increasing trend of support to ensure the proper growth and encouragement of these disciplines. Support of the biomedically related and the more social science aspects of these fields through such existing agencies as the National Institutes of Health and the National Science Foundation has been administered in an effective and highly satisfactory manner. The programs of these agencies in support of behavioral science should be strengthened, but no new agency or mechanism for support seems indicated at this time.

Certain aspects of these fields, as in the past, will and should be supported by operational agencies, since the work is pertinent to their missions. Indeed, mission-oriented agencies should be encouraged and urged to include adequate budgets for basic research that is relevant to their applied research and operations. In this way, basic and applied research can be brought closer together, to the advantage of each.

The continued vitality and growth of the behavioral sciences will depend heavily on the scope and quality of training. The success of the Veterans' Administration in affecting clinical psychology, first by its extensive training program and then by opening up employment opportunities, provides an illustration of how great an impact well-conceived Government support can have.

It is probably the shortage of manpower, competent and eager to do the research, more than the shortage of money that sets present limits. The training needs are great—greater in some fields than others—as noted in the preceding sections. Increasing financial support for both training and research in these disciplines should be the strategy in the next decade to insure a balance in the overall scientific enterprise of the nation. This will represent only a small part of the total cost in the support of science, yet the returns will be great, for "the proper study of mankind is man."

References

(*1*) Rossi, Peter H., "Researchers, Scholars and Policy Makers: The Politics of Large Scale Research" *Daedalus,* fall 1964, pp. 1142–1161.

This paper was prepared with assistance of the members of the executive committee of the Division of Behavioral Sciences, National Academy of Sciences-National Research Council: Drs. G. P. Murdock, R. Dorfman, K. Davis, G. Finch, F. P. Thieme, C. S. Hyneman, and consultation with Dr. H. Odbert.

THE EARTH SCIENCES AND THE FEDERAL GOVERNMENT

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Summary

Many of the economic benefits from research in the earth sciences—improvements in weather forecasting, protection from catastrophes, better ocean and air transportation, conservation and economic use of water supplies, better weapons for national defense, and reliable communications—affect many kinds of enterprises and very large numbers of individuals; only the Federal Government has broad-enough responsibilities to encompass these diverse interests. In other areas, for example, the conservation of natural resources, the Government needs to be involved because the anticipated benefits lie in the future beyond the time horizons of individual corporations or local governments.

The greatest need in the earth sciences is to maintain and develop centers of excellence in our universities. This can best be accomplished through university centers of teaching and basic research, where earth scientists and students can work in close contact with each other and with the fundamental disciplines of physics, chemistry, mathematics, and biology, and where the interplay of teaching and research can stimulate both. By underwriting the vigor and dynamism of many such centers, we will increase the likelihood that new ideas and fresh viewpoints can arise.

If we take as a national objective the production of 600 Ph. D.'s per year in the earth sciences by 1970, between 1,000 and 2,000 university faculty members and a full-time enrollment of about 6,000 graduate students—about double the present numbers will be required. The total annual cost would be around $70 million. Because of the Federal Government's preeminent interest, it would need to assume a very large share of this total, which would be a minimum figure for Federal support of basic research in the earth sciences.

Introduction

The study of the earth, from its fiery core to the invisible veils of the Van Allen belts, is a unity. We deal with a single object in space and time, extraordinarily complex and unimaginably old, the seat of a form of matter so highly organized and so complex that it can understand the nature of matter itself. That form of matter is, of course, ourselves.

Like all the objects in our observable universe, the earth is not static but continually changing. The changes are controlled by unchanging relationships, the immutable laws of physics and chemistry. A principal objective of the earth sciences is to study these changes, to discover the sequences of events in past times, and to find ways of estimating what may happen in times to come.

The earth contains an archive of its own history. Past events are recorded in its present state. Hence the earth sciences rest upon a clear description of the earth's composition and structure, and a good understanding of the processes occurring today on the earth and within it.

Our planet can be thought of as a sphere unsupported in space, isolated and complete in itself. But in trying to understand her, we cannot leave out of account her parent sun or her sister planets. Radiation from the sun drives the winds of the air and the currents of the sea. It provides all the energy used by living things, and it determines many of the processes that have shaped the earth's surface.

The earth and her sister planets were made from the same store of materials; they share a common history, yet there are profound differences between them. An understanding of the reasons for these differences would give great insights into the nature and early history of the earth itself. At the same time, a deeper knowledge of the earth is essential to understanding the other members of the solar system.

The space sciences and the earth sciences cannot be separated intellectually. They can be separated only partially on the basis of the instruments used. Orbiting space vehicles are a powerful means of studying the earth. They have already added to our knowledge of the distribution of matter in the earth's interior, the composition and physical processes of the outer atmosphere, and the patterns of world weather. Photographs of planetary spectra through groundbased optical telescopes; physical and chemical studies of meteorites compared with terrestrial rocks; laboratory models of the behavior of highly rarefied, highly charged ions and molecules; measurements of cosmic rays; and observations of signals from the planets by radio telescopes on earth are still the most powerful tools for studying the solar system.

According to modern ideas of stellar history, our sun is a second-generation star among the billions of stars in the Milky Way. The earth and the sun could not exist as we know them, if the heavy elements they contain had not been formed in a star that died before the sun was born, and in dying spewed its newly formed elements into interstellar space. The solar system is probably about a third as old as the galaxy. Thus there are no real boundaries between the sciences of the stars and the sciences of the earth. Both deal with particular objects in space and time, rather than with universals. Both are concerned with the ways in which continuous change has occurred under the action of unchanging laws. But these "field" sciences differ in important ways from the laboratory sciences.

The physicist and the applied mathematician can make their own worlds in the laboratory. The astronomer with his telescope, the geologist with his hand lens, and the oceanographer on his ship must face the world as it actually exists, with its tangled knot of interacting processes and its long and difficultly decipherable history.

Because the astronomical and earth sciences do not deal with universals, but only with physical laws acting in particular situations, the physicist tends to think of them as applied rather than fundamental sciences. He believes they give no new insights into the nature of matter, but only descriptions of its arrangement.

The field sciences have something else in common; the tools required to pursue them are expensive in terms of the scientific results achieved. A large optical telescope, an orbiting satellite, or an oceanographic ship cost a good deal more per man-hour of creative scientific efforts than much of the "little science" conducted in university laboratories. This is not to say that laboratory experiments and theoretical work are less essential in astronomy and the earth sciences than in physics or chemistry. In recent years remarkable discoveries have come from the use of powerful new instruments to study rocks and meteorites in the laboratory, from the application of physical theories of plasmas and the nature of the solid state, and from the use of mathematical tools such as large computers and new methods of statistical analysis. But theory and laboratory experiment must go hand in hand with field observations and measurements if our understanding is to continue to increase.

The study of the stars fills man's deep need to understand his place in the universe; the space sciences ride the wings of high adventure. Neither deep emotion nor brave deeds can be easily invoked to justify the study of the earth. It is appropriate to ask how far this study can be justified on economic and social grounds.

In general terms, the answer is obvious. Men are children of the earth. Their heredity was forged on the anvil of its surface, and they depend for their lives on its resources. To insure their welfare and their survival, men need to know a great deal about their planetary home. But such generalizations cannot be used to appraise a desirable level of effort in the earth sciences. We want something more specific.

Nature of the Earth

The earth contains only about one five-hundredth of the initial mass of matter from which it was formed. Nearly all the hydrogen and helium and other inert gases, and most of the carbon, nitrogen, oxygen, and other volatile substances have been lost.

Some time after its initial formation, the components of the earth began separating into a central core and surrounding shells. The core itself is in two parts: an outer liquid core, probably consisting largely of molten iron and nickel with some silicon, surrounding an inner solid core of the same matter. The diameter of the core is about half the diameter of the earth and it contains nearly a third of the total mass.

Surrounding the core is a solid mass called the mantle, nearly 3,000 kilometers thick, which is believed to consist principally of iron and magnesium silicates. Over this is a thin crust containing a higher percentage of silicon and oxygen than the mantle, together with relatively high concentrations of aluminum, sodium, potassium, calcium, and other nonvolatile elements. The mantle contains slightly more than two-thirds of the earth's total mass, and the crust, with an average thickness of 35 kilometers under the continents and 7 kilometers under the oceans, about four-tenths of 1 percent. The oceans, covering 71 percent of the crust, consist of oxygen (85 percent), hydrogen (11 percent), and a very thin broth of all the other elements (3.5 percent). The entire mass of water and dissolved substances is about 0.025 percent of the earth's mass.

The solid and liquid parts of the earth are bathed in a thick layer of gas that becomes rapidly attentuated with height above the surface. In its lower layers this gas consists largely of molecules of nitrogen, oxygen, and carbon dioxide, and of argon atoms. At greater heights, it is a plasma of ionized and highly energetic particles, extending out for several earth diameters. The entire gaseous mass is only about a millionth of the mass of the earth.

Like the stars, the earth is "alive" in its interior, in the sense that heat is continually being generated, but at a very much lower metabolic rate. Whereas the sun emits 2 ergs per gram per second, the heat flowing out from the interior of the earth is only 4×10^{-8} ergs per gram per second. For both the stars and the earth, nuclear processes are the source of energy.

One of the remarkable facts about the earth is the irregularity of its solid surface. Because of this irregularity, the ocean waters do not constitute a continuous film over the globe, but are gathered together in deep basins surrounding the great islands called continents.

Applications of the Earth Sciences

Knowledge gained from the scientific study of the earth can be useful in many ways:

(1) In locating, appraising, and conserving natural resources.

(2) In making forecasts of weather and climate, perhaps eventually in learning how to make weather and to change climate.

(3) In reducing damage from violent convulsions of the earth—hurricanes, tsunamis, earthquakes, and volcanic explosions—that are murderously destructive to human beings and their structures.

(4) In preventing or overcoming pollution of the environment caused by man's activities.

(5) In designing, testing, and using military weapons, and in predicting weapons effects.

(6) In providing the knowledge to improve long-distance communications.

(7) In increasing the economy and efficiency of ocean and air transportation.

(8) In developing optimum patterns of land use.

(9) In designing engineering works that modify the environment; for example, artificial harbors, bars, breakwaters, and other coastal structures.

Natural Resources

Natural resources include many things, but among them are minerals and fossil fuels buried in the earth or under the sea; water on the surface and underground; and the useful fishes, invertebrates, and plants of oceans, lakes, and streams. In the past, the earth sciences have contributed most to finding underground deposits of oil and natural gas, and in evaluating the reserves of these fossil fuels. Almost everything that has been learned about the formation of sedimentary rocks, their distribution over the earth, and the structural deformations they have undergone, has been useful in finding oil; as oil and gas deposits become harder to find, we will need to know more and more about sedimentary rocks.

Clearheaded geological thinking, based on knowledge of how the permeability of sedimentary strata can vary horizontally, led young A. I. Levorsen, working alone in the 1930's on maps of east Texas spread out on his kitchen table, to recognize places where large oil accumulations were trapped against impermeable materials.

Delicate and complex geophysical instruments have been invaluable tools in finding oil. For example, measurements of variations in the force of gravity in Louisiana and parts of Texas have proved to be the best means of locating buried salt domes. Exploitable oil accumulations commonly occur on the edges of these domes.

In the mineral industry, many of the older deposits were discovered as surface outcrops by prospectors and exploited by engineers without much benefit of science, but geology and geophysics have come into their own in recent years. The discovery during World War II of enormous deposits of bauxite (aluminum ore) in Jamaica and Haiti is a spectacular example of the use of geologic knowledge in finding materials. In 1942, our supplies of South American bauxite were threatened by German submarines, which were sinking large numbers of ships laden with ore. To supply the aluminum for our vastly enlarged airplane production, it was necessary to find a source of bauxite closer to the United States. One small deposit was known in Jamaica. This had been discovered accidentally when a farmer sent a sample of his soil to London for analysis; it had been mined for a while by a Dutch company, and then abandoned because the ore was thought to contain too much iron. Examination of this deposit showed that the bauxite had formed from the weathering of limestone, and that it had accumulated in one of the large sinkholes, or karsts, that are characteristic of limestone country. The bauxite produced a light permeable soil on which grass grew sparsely, and mangoes and sweetpotato vines flourished, while bamboo and sugarcane could not be grown. Aerial reconnaissance, combined with geologic mapping, revealed many areas covered with grass or with mangoes and sweetpotatoes, indicating the presence of bauxite, and a good many of these were thick accumulations in sinkholes in the limestone. The total reserves of bauxite thus far found in Jamaica amount to about a billion tons, worth several billions of dollars and many times larger than any deposits previously discovered. Exploration for buried mineral deposits now involves the use of many geophysical tools, such as airborne magnetometers, gravity meters, and seismic and geochemical techniques. At the same time, the technology of mineral extraction has improved, and lower and lower grade ores are being used. Mineral technology is becoming more and more a problem of large-scale earthmoving and sophisticated metallurgical chemistry.

We can anticipate a time to come when relatively ordinary igneous rocks will be the source for such metals as aluminum, iron, and copper. Small differences in the metal content of these rocks will be economically critical; recognition of these differences and estimates of their areal extent will demand understanding of the processes deep within the earth by which the rocks were formed. For other minerals, including tin, uranium, diamonds, and manganese, the buried concentrates called ore bodies will still be sought, and scientific methods of exploration will be more and more essential.

Water is the most abundant substance on the part of the earth accessible to man, and he uses more of it than any other material, but the natural distribution of water in space and time accords poorly with human needs. In the semiarid lands that are otherwise highly suitable for human life, water is the limiting factor.

For the surface waters of rivers, streams, and lakes, we need scientific knowledge about their variability with time and how this can be regulated by such human actions as the building of dams for storage and aqueducts for transport. For underground waters we need to know the location, extent, and availability of the resource, the rates of replenishment, and the speed and direction of movement in aquifers. We want to estimate the potentialities for storage of surface waters underground, and the problems that will be encountered in accomplishing this because of the water-transporting characteristics of the overlying soils.

Increasing the ocean fish catch would be of the greatest benefit to the two-thirds of mankind that suffers from a deficiency of animal proteins, but it could also raise the annual production of the U.S. fisheries industry, which is increasingly operating on a worldwide basis, by nearly a billion dollars within the next 15 years. To maintain present fish catches and to increase the ocean harvest, we need to know why certain regions of the sea are fertile pastures while others are sterile deserts. What are the relationships between changes in ocean currents and the migration, behavior, and population size of different species? We need to know how many fish there are in the sea and how fast they can reproduce themselves.

Long-Range Weather Forecasting

The present accuracy of long-range weather forecasting is low, but if it could be improved, great economic benefits would follow in planting and harvesting crops, in planning seasonal fuel transportation and storage, in the timing of building and road construction, and in flood and drought protection.

Over the 15-year period from 1946 through 1960, the estimated damage from floods in the United States was $4.2 billion, or an average of $280 million a year. Better long-range weather forecasting might reduce this by 25 to 50 percent, or $70 to $140 million a year. Such forecasts, for example, would give the engineers a better basis for judging when to release water from reservoirs before times of heavy runoff.

Annual expenditures on new construction in 1962 were $59 billion, and about 3 million people were employed. Labor costs amounted to roughly one-third of the total costs. If the efficiency of utilization of labor and equipment could be improved by 5 percent through better scheduling based on reliable long-range weather forecasts, a billion dollars would be saved.

Fuels and electric power cost U.S. consumers around $40 billion a year. About one-fourth of this represents the costs of space heating and air conditioning. Reliable forecasts of temperatures and humidities during the coming winter or summer would allow savings through better scheduling of coal, oil, and natural-gas production; oil-refining operations; transportation by pipelines, rail, and ships; and storage. Even a 5-percent saving would be worth $500 million.

The total value to the farmers of commercial vegetable production in 1962 was $1.2 billion. The value of potato production was roughly $500 million, and of fruits, including grapes, perhaps $2 billion. The added value from processing and marketing was about twice these figures. Forecasts of growing conditions and optimum planting and harvest times in different parts of the country would help farmers to avoid market gluts and shortages, and would enable processors to plan and schedule their operations more effectively. A 5-percent gain would represent around $500 million.

The farm value of cattle and hog production in 1962 was $9 billion. Weather-produced variations in the size of the crops of corn, oats, and hay have serious economic effects for livestock producers, as to change from year to year in the productivity of permanent pastures and range lands, caused by variations in seasonal rainfall. Significant savings would be obtained if the farmers could plan how to feed and dispose of their stock on the basis of reliable long-range weather forecasts. A 5-percent saving would amount to $450 million.

It must be admitted that these estimates are not based on a careful economic analysis, but rather on a feeling that savings of 5 percent would be "reasonable." A thoroughgoing analysis, if it could be made, might show considerably larger potential savings. Adding the various figures, we arrive at a minimum of around $2.5 billion that could be saved by farmers, fuel producers, public utilities, builders, and water managers if they were equipped with better forecasts. This figure does not take into account possible economic benefits in the various industries associated with tourism and recreation, or the intangible savings to individual families, from being able to plan their travel and household activities more satisfactorily.

The ocean of air in which we live and the ocean of water beneath us are interlocked components of a great heat engine. The engine works to transport heat energy from low latitudes to high latitudes, where it is radiated into space. Much of the energy of the air—about one-third—enters it through the condensation of water vapor evaporated from the sea surface. A large part of the remainder is transferred as sensible heat or as infrared radiation from the warm sea to cooler air. Evaporation, heating, and back radiation do not take place uniformly over the ocean, nor are they uniform at any given latitude. They are high where the cloud cover is small and in regions where the difference in temperature between the surface ocean waters and the air are greatest.

The maximum temperature differences between the sea and the air shift in location and vary in intensity. Similarly, the regions where storms are born and the paths of storm travel appear to change with variations in water temperature near the sea surface. But because of its high heat capacity and massive inertia, the ocean can change only slowly with time. The persistence of weather patterns over periods of weeks to years may result, in part, from this sluggishness of the ocean.

The hope of improved long-range weather forecasting depends largely on our learning how to predict changes in persistent weather patterns. Insofar as these patterns depend on patterns in the sea, it is clear that in order to gain greater understanding of the mechanisms of change we need to understand the large-scale interactions between the sea and the air, and the large-scale movements of ocean water masses.

Recent work has shown that anomalies in atmospheric circulation result in anomalies of ocean-surface temperature. For example, with increasing winds of cold origin there is an increased transfer of sensible and latent heat from the ocean to the atmosphere and an increased stirring of the upper layers of the stratified sea. Both these processes result in a lowering of the sea-surface temperatures. The restoring processes by which the sea temperatures return to their "average" value come from a slow strengthening of the poleward-moving ocean currents near the sea surface. The character and rate of the changes in the ocean density distribution that cause this strengthening of the poleward currents is still unknown.

To attain the practical objective of improved long-range weather forecasts will require cooperative study of large-scale interactions between the sea and the air by oceanographers and meteorologists. Time series of measurements at many points in the upper water layers need to be combined with continuous maps of cloud cover, winds, and atmospheric temperature distributions over the oceans. Many of these atmospheric measurements will come from weather satellites, but the measurement of the ocean waters will probably require establishment of a network of anchored buoys.

Reducing Damage From Catastrophes

Destructive earthquakes and volcanic eruptions result from the violent release of energy deep within the earth. When earthquakes occur under the sea floor they are often accompanied by tsunamis, waves several hundred miles long in the deep sea and only a few feet high, which travel across the ocean at speeds of about 700 miles an hour until they reach shallow water. Here they pile up in high, steep waves that can do enormous damage.

Because of our inadequate understanding of how earthquakes and volcanic explosions occur, they cannot be predicted at the present time. But there is a reasonable hope that this will be possible in the future as a result of advances in seismology and the other sciences of the solid earth. If so, many thousands of human lives could be saved in each decade. Because it takes several hours of a tsunami to travel from its point of origin to some of the places where it may do extensive damage, radio and telegraphic warning systems are being established. These are already saving lives and property, but they would be much more effective if more were known about the modes of origin of these giant waves.

Murderously destructive tropical storms, called hurricanes in the Atlantic and typhoons in the western Pacific, are born and have their embryonic growth over the ocean. Over the period from 1940 to 1957, the average damage caused by hurricanes in the eastern and southern parts of the United States was $140 million a year. Nearly 1,000 people were killed during these 17 years. It is not impossible that these storms could be aborted in their early stages if a means could be found to prevent anomalously large transfers of heat energy and water vapor from the sea to the air in the regions of hurricane formation.

The possibility that we may be able to eliminate hurricanes is very uncertain. But we shall certainly not be able to do so unless we can learn more about them. The meteorologists have already learned a good deal, chiefly since World War II, and this has led to marked improvement in forecasting travel paths and intensities, and to development of protective measures. In 1926, many lives were lost and numerous structures were demolished when a hurricane passed over the Miami-Fort Lauderdale area of Florida. Hurricane Cleo, in August 1964, violently struck the same area without taking a single American life or destroying any buildings.

Pollution

The enormous growth of cities during the past few decades, both in population and area, is producing unprecedented problems of pollution of water, air, and soil. These problems are aggravated by the use of chemical pesticides, nondegradable detergents, and automobile fuels laced with lead tetraethyl. In the future, low-level radioactive contamination from nuclear powerplants will also have to be reckoned with. There are many aspects to the pollution problem, including regulation of the production of the contaminating substances, but one important aspect is the rate of dispersal of pollutants, determined by their rates of diffusion and mixing with different air and water masses in the environment. Knowledge of large-scale diffusion and mixing processes in the air, and in rivers, estuaries, and the open sea will become more important with the growth of megalopolises, such as the continuous city from Boston to Richmond that is rapidly accreting on the eastern seaboard. Pollution-control measures will have to be carried out over regions determined by meteorological and hydrological realities, rather than by political boundaries, and even to delimit such regions will require much more knowledge of air and water than we now possess.

Designing, Testing, and Using Military Weapons

The Department of Defense is concerned with the earth sciences, both to improve our military capability and to reduce its cost. It can use the results of these sciences in many ways, ranging from the provision of information for training exercises to considerations affecting the choice of a major weapons system. In testing a new antisubmarine sonar gear, for example, it is essential to know the depth of the uppermost wind-stirred ocean layer and the sound-reflecting behavior of the sea surface and the sea floor. Otherwise, we cannot use the test to estimate the performance of the gear under the variety of conditions that will be encountered in actual operations.

In trying to assess the value of the earth sciences to the national defense, we face the problem that it is extremely difficult to assign a dollar value to military effectiveness. Ideally, an answer to this question would involve quantitative estimates of—

(1) The amount by which other defense expenditures could be reduced as a result of research in meteorology, oceanography, seismology, and upper atmosphere studies, and the length of time before these savings could be made;

(2) The increase in effectiveness of present and future weapons systems that could result from likely increases in knowledge about the earth at different future times;

(3) The level of effectiveness of weapons systems required to defend the United States from a foreseeable enemy, and the knowledge about the earth needed to reach this level of effectiveness;

(4) The level of research capability required to meet unforeseen military contingencies in which new knowledge of the earth will be needed;

(5) Alternative uses of funds and people that might be substituted for present or future earth sciences expenditures, to provide the required level of effectiveness of weapons systems more quickly or less expensively; and

(6) The extent to which present and planned defense expenditures are meeting military needs for knowledge of the environment. Finally, if these needs are not being met, what changes in direction, scope, or level of effort are needed in the Defense Department's research program in the earth sciences?

Unfortunately, neither our knowledge of the earth nor our ability to forecast the potentialities of future weapons systems is adequate for quantitative answers to these questions. More fundamentally, national defense is not a relative but an absolute necessity. We must spend whatever is needed to assure our national survival. We cannot take the chance that an enemy will gain knowledge we do not possess that will enable him to destroy or mortally weaken the weapons systems on which our survival depends. For example, we must make every effort to be first in learning about any possible mechanisms of energy propagation in the ocean that could be utilized to detect our own Polaris submarines.

The history of the attempts by the Soviet Union, the United Kingdom, and the United States to limit by international agreement their testing of nuclear weapons illustrates the potentially grave consequences of neglecting an apparently impractical field of science. In this case, the science was seismology, one of the branches of the earth sciences. An agreement might have been reached several years earlier if there had been more certain knowledge of the energies of natural earthquakes compared with the energy levels of the seismic waves from explosions, and of other differences between seismic waves produced by explosives and those produced by earthquakes.

Long-Distance Communication

Sunlight, the visible radiation from the sun that we can see and feel, is remarkably constant. But the sun radiates many other kinds of energy, including X-rays and a broad spectrum of radio waves, and it occasionally shoots out clouds of hydrogen nuclei at speeds of hundreds of miles a second. These particles and rays are highly variable; they interact in complex ways with the earth's magnetic field and with the tenuous plasma of the earth's outer atmosphere.

When a fast-moving cloud of solar particles envelopes the earth, long-distance radio communications are blacked out; the vital radio links between transport aircraft and the group may be broken; transoceanic telephone cables may cease to function, and even electric power stations can be forced to shut down. At other times variations in solar activity cause ionized layers in the atmosphere to vary in reflectivity and to rise or fall in altitude, so that radio communicators must shift wavelengths and relay points. Monitoring of these solar-terrestrial processes is essential to maintaining the effectiveness of present radio communications. Research on the electromagnetic behavior of the upper atmosphere, on terrestrial and solar radio noise, and on atmospheric phenomena that may be used to channel radio signals—for example, the trails of shooting stars—is important in developing means for carrying the ever-increasing quantity of information that must be transmitted by radio waves.

Ocean and Air Transportation

Among major raw materials, the United States is now self-sufficient only in coal, molybdenum, phosphate, and magnesium. Wood and petroleum have shifted from net exports to net imports, and we depend largely on oversea supplies for asbestos, tin, manganese, iron ore, bauxite, cobalt, nickel, chromite, quartz crystal, and industrial diamonds. Our imports of other essential raw materials and food products are steadily rising.

The sea is the major highway for the international transportation of heavy or bulky materials; it will undoubtedly remain so for many generations to come. To carry the estimated 400 million tons of ocean cargo entering or leaving U.S. ports in 1970 will require full-time use of between 20 and 30 million deadweight tons of cargo ships, partly under the U.S. flag and partly under foreign flags. Assuming a 20-year life for these vessels, the world rate of ship construction for replacement of existing tonnage carrying our trade will be between 1.0 and 1.5 million deadweight tons annually. By 1970, the amount of cargo to and from the United States should be increasing at a rate of about 15 million tons a year, and the rate of construction of new ships for this increased trade will be between 0.75 and 1.1 million tons a year. Thus the total world ship construction required for U.S. ocean trade in 1970 will be between 1.75 and 2.6 million tons a year. Perhaps as much as half of this new tonnage will be in bulk carriers, with a construction cost of around $150 per deadweight ton, and the other half in decked cargo ships costing around $250 per deadweight ton. Assuming that our ocean trade will continue to increase during the 1970's, the annual cost of new construction will be close to $500 million by the middle of the decade. All this cost will be a burden on American importers and exporters, and on their oversea customers and suppliers.

Freight costs for ocean cargoes vary widely with the type of cargo and the distance it is carried. With present technology, the total freight cost for U.S. ocean trade could be $5 billion per year by 1975. About half of these costs would be charged against the time the ships are at sea, and the other half against the "turnaround" time required for loading and unloading and other operations in port.

A reduction in the cost of ocean shipping would serve the interests of the United States and of the countries with which we trade. In particular, lower shipping costs would help the less-developed countries, because they are so largely dependent for their economic development on the oversea sale of raw materials and agricultural products, and on the importation of heavy machinery for industrialization. Meteorological and oceanographic research can make significant contributions to a reduction in ocean-shipping costs. Many aspects of knowledge about winds, waves, and currents in the ocean areas have a direct bearing on the use of the oceans as the major intercontinental highway. For example, better statistics on sea-surface waves should make it possible to improve the design and lower the cost of new ships. Through improvements in the forecasting of waves, winds, and currents, ships could be better routed along minimum time paths; both fuel consumption and time at sea would thereby be reduced. Improved routing should also lower storm losses. Stranding and collision losses could be lowered through improvements in navigation, based on more detailed knowledge of sea-bottom topography. Greater knowledge of near-shore wave and current conditions and sea-floor characteristics is needed for improvement of existing harbors and construction of new ones, and for the development of new methods of loading and unloading. Increased knowledge of the life histories, behavior, and physiology of fouling and boring organisms could help to lower the losses caused by these pests.

The effects of weather on air transportation are obvious to any traveler who has experienced fog in an airport or turbulence aloft, or whose arrival has been delayed by adverse winds. Atmospheric conditions have additional consequences that are less evident to passengers, but of great concern to pilots. For example, magnetic storms and other disturbances to radio transmission seriously affect aerial navigation and ground-to-air communications.

High-flying jet aircraft may themselves perturb the atmosphere. Their vapor trails may cause a chain reaction of ice-crystal formation over large areas, and thereby alter the ratio of reflected to transmitted sunlight. The results of these changes in the reflectivity of the upper air are as yet unknown.

Optimizing Land Use

The physical properties of soils—permeability and water-holding capacity, friability and cohesiveness, acidity or alkalinity, salt content, thickness and variability—determine the kinds of crops for which they are best suited, or, indeed, whether they should be used for agriculture at all. These properties in turn are determined by interactions between land, water, and climate, by the geologic history, and by man's actions. The agricultural revolution of the past quarter century depended in large part on greatly increased understanding of these relationships. The future improvements of agricultural productivity that are needed for expanding population will demand even further understanding.

The dust bowls of the 1930's; widespread river flood destruction in flood plains; catastrophic washing out of dams due to failure of the surrounding rock walls; proposals to build nuclear powerplants near major active earthquake faults; losses of beach property due to shoreline erosion; all are examples of improper land use because of failure to understand or to apply scientific knowledge of the earth.

The United States has had so much area compared to the number of its people that we have been able to squander our heritage of land. But with our population rising above the half-billion mark within the next 75 years, one of our basic national goals must be the best use of the land. We have very little time left to decide which areas are best suited for farming, for forestry, for recreation, for wildlife refuges, or for cities and their airports, roads, and reservoirs.

Engineering Works to Modify the Environment

Attempts to control fresh-water runoff from the land may have profound effects on the seashore. For example, on the beaches of southern California, the sand supplied by rivers is transported along the beach by wave-generated currents, and is lost to the deep sea at submarine canyons. Runoff control has cut the supply of sand to nearly zero, and without some intervention, the beaches may seriously deteriorate within the next two decades. Even normal processes of wind, waves, tides, and currents can thwart man's attempts to improve a coastal area for his benefit, unless the influence of these processes is understood and allowed for in the design of the improvement works.

The development of new beach areas, the protection of beaches against erosional or depositional damage, the extension of coastlines, the development of harbors, improved sport fishing through the building of artificial reefs—all these are affected by the physical, chemical, biological, and geological processes of the sea. Improved oceanographic knowledge of these processes can materially influence the effectiveness with which plans for such developments are brought to fruition, and hence can materially reduce the costs of successful development. The efficiency and safety of management and use of marine recreational facilities can be improved through adequate forecasts of wind, wave, and surf conditions, and of storm tides. These are also important areas of oceanographic research.

A fourth of the entire area of the continents is under water. The submerged regions are called the continental shelves, and their area is about equal to that of Africa. Some very recent work by Jacques Cousteau and his colleagues in France, and by engineers and medical scientists of our own navy, suggests that human beings may soon be able to live and travel at will on the shelf floor, and, in general, to treat it as a part of the continent accessible to man. A large fraction of our remaining oil reserves, estimated by some experts to be as high as 40 percent, must live under the shelves. The recent large-scale mining of diamonds from the continental shelf off southwest Africa emphasizes that placer deposits of valuable minerals may be widespread. A great deal of research and development in oceanographic engineering will be necessary to open up the continental shelves for many different human uses.

Federal Support of the Earth Sciences

To gain the understanding we need, we must study the earth as a whole. This is most evident in meteorology and oceanography. A storm over New England may have originated off Japan, and an intensification of the trade winds of the tropical Atlantic can result in a quickening of the Gulf Stream off the Grand Banks. But it is equally true for other earth phenomena. Turbulent motion in the molten core of the earth disturbs the magnetic field on the surface and bends the magnetic lines of force that extend out to distances of thousands of miles. The strength and position of these magnetic lines in turn determine the paths of protons and electrons projected to the earth by solar flares. These floods of particles produce radio blackouts and other headaches to radio engineers. Slow, giant-scale convection in the mantle apparently results in folded mountains, island arcs, and great deeps around the Pacific, and in the broken mountain ranges that characterize the two sides of the Atlantic.

Detailed studies of small geological structures, such as the shapes of oilfields and the distribution of ore bodies, can safely be left to the oil and mining companies. But the fundamental phenomena that determine these details must be investigated on a very large scale. The global nature of the earth sciences requires that they be supported on a national basis and carried out with a full measure of cooperation between nations. The effectiveness of such cooperation was well illustrated during the International Geophysical Year, the greatest scientific enterprise ever undertaken.

Many of the economic benefits from research on the earth—improvements in weather forecasting, protection from catastrophes, better ocean and air transportation, conservation and economic use of water supplies, better weapons for national defense, and reliable communications—affect many kinds of enterprises and very large numbers of individuals; only the Federal Government has broad enough responsibilities to encompass these diverse interests. In other areas, for example, the conservation of natural resources, the anticipated benefits lie in the future, beyond the time horizons of individual corporations or local governments.

Some of these reasons for Federal support of the earth sciences have long been recognized by the United States Government. In 1962, one-third of the earth scientists were employed by some governmental agency, and all but a few percent of these by the Federal Government. Another 17 percent were employed by educational and nonprofit institutions; their research support came largely from Federal funds. Total Federal expenditures in the earth sciences were around $540 million in fiscal 1963 and $600 million in fiscal 1964.

Large sums of money are spent on obtaining and disseminating information on the environment in such agencies as the Weather Bureau, the Coast and Geodetic Survey, the Geological Survey, the Navy's Oceanographic Office, the Air Weather Service of the Air Force, the Army Signal Corps, the Naval Aerological Service, and the Army Map Service. This information is published in maps, charts, weather forecasts, statistical tables, and similar documents. Although much of it is useful for research, the collection, compilation, and publication of this information is carried out on a systematic or routine basis, and is not, in itself, research as scientists would define the term, nor is it classified as such in Federal research and development statistics. At the same time, these agencies also conduct studies and investigations that add to our understanding of earth processes, history, and structure, lead to the development of new instruments and methods for obtaining information, or are sufficiently exploratory in character to be included under the rubric of research.

Many agencies, including the National Science Foundation, the Office of Naval Research, the Advanced Research Projects Agency, other military research organizations, the Weather Bureau, the Bureau of Commercial Fisheries, and the Public Health Service, support research in aeronomy, meteorology, oceanography, marine biology, geology, water resources, terrestrial ecology, seismology, terrestrial magnetism, and tectono-physics, in universities, other nonprofit institutions, and industry. Some of the National Aeronautics and Space Administration's expenditures can also be counted here. Other agencies, such as the National Bureau of Standards (particularly its Central Radio Propagation Laboratory), the Geological Survey, the research divisions of the Department of Agriculture, and some of the navy laboratories, conduct considerable inhouse earth sciences research.

Future Needs in Earth Science

The greatest need in the earth sciences is to maintain and develop centers of excellence in our universities. There are two reasons for this: (1) In order to upgrade the scientific effectiveness of the Federal Government's expenditures in these fields, the number of hydrologists, meteorologists, oceanographers, geologists, and geophysicists with doctor's degrees, that is, with advanced training and demonstrated competence in research, should be increased. (2) To advance the economic and social objectives of the earth sciences, as well as to satisfy men's desires to understand the world in which they live, new levels of understanding of earth processes must be attained. This can best be accomplished through university centers of teaching and basic research, where earth scientists and students can work in close contact with each other and with the fundamental disciplines of physics, chemistry, mathematics, and biology, and where the interplay of teaching and research can stimulate both. By underwriting the vigor and dynamism of many such centers, we will increase the likelihood that new ideas and fresh viewpoints can arise.

In 1964, about 2,000 of the earth scientists whose research was supported by the Federal Government had doctor's degrees. This number was approximately half of all United States Ph. D.'s in the earth sciences. Federal expenditures were about $330,000 per Ph. D. worker. Compared to most other fields, this is a high ratio, made necessary at least in part by the high cost of the vehicles and instruments used in these sciences. But it can be convincingly argued that the Federal Government's earth sciences program would be strengthened substantially if the number of federally supported Ph. D.'s were raised to at least 4,000—double the present number.

If this were to be accomplished in 10 years, and if we also take into account the need for replacement of personnel, the entire present output of our universities, about 300 doctorates a year, would be required. An increase in the number of Ph. D.'s per year can be attained only by increasing, in a carefully planned way, the support of basic earth sciences research in the universities.

If we take as a national objective the production of 600 Ph. D.'s per year in the earth sciences by 1970, between 1,000 and 2,000 university faculty members and a full-time enrollment of about 6,000 graduate students—about double the present numbers—would be required. The total annual cost would be around $7 million. Because of the Federal Government's preeminent interest, it would need to assume a very large share of this total, and this would be a minimum figure for Federal support of basic research in the earth sciences.

Although support for research is an essential ingredient in building up and strengthening centers of excellence in our universities, it is by no means sufficient. What is basic to attract and hold first-rate minds is the intellectual excitement of interesting, scientifically important, and solvable problems, particularly problems relevant to other fields of science. The earth contains many such problems. One of the most dramatic is the origin of life. What were the conditions and the sequence of events on the primitive earth by which inorganic materials became combined into more and more complicated organic molecules, which first learned to transform energy and ultimately to reproduce themselves?

As remarkable as the origin of life is its evolution into organisms of ever greater complexity and diversity. The nature and rates of evolution can be understood by studying the earth together with genetic mechanisms.

Some properties of matter under extreme conditions can best be investigated through the earth sciences. The inner mantle and core of the earth are the only (more or less) observable large masses of matter at relatively low temperatures that are subjected to pressures higher than a hundred million pounds per square inch for more than a few microseconds. Only within the mantle can the deformation of solid material be observed at extremely low rates of strain.

The earth is an ideal laboratory for the study of fluids subjected to both electromagnetic and mechanical processes. The earth's magnetic field, originating in its liquid core, extends outward several earth diameters. There it interacts with charged particles produced in the high atmosphere by photochemical and collision processes, and with particles arriving from the sun and from outer space.

Whether the magnitude of the fundamental physical constants has changed with time is one of the most profound and difficult questions of physics. Because the earth contains records of past events over several billion years, it may be possible to find evidence for the constancy, or alternatively the variability, of such quantities as the acceleration of gravity and the rates of radioactive decay.

Finally, it is important to emphasize the dependence of the earth sciences on tools and concepts derived from physics, chemistry, mathematics, and biology. Continuing growth in the vigor and effectiveness of scientific studies of the earth must depend, therefore, not only on imaginative and generous Federal support of the earth sciences themselves, but also on the continuing advance of the whole scientific enterprise.

THE ROLE OF APPLIED SCIENCE

by EDWARD TELLER
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Summary

The Committee on Science and Astronautics of the U.S. House of Representatives has raised the question of how to maintain our position of leadership in science. In my judgment we have actually lost this leadership in applied science. Unfortunately such a loss may have a deep influence during the years ahead of us.

It is my belief that to regain this leadership we should put the greatest possible emphasis on higher education in applied science. Direct support of programs in applied science is apt to be spent ineffectually because of the insufficient number of prominent applied scientists who could effectively direct our efforts. It is, therefore, most important that we remedy our deficiency in first-class applied scientists, and this can be done most effectively by an educational effort.

We possess the means of establishing a good educational program by means of cooperation between our applied science laboratories (both governmental and private) and our universities. Such cooperation is in need of appropriate encouragement by the Government. A further means of improving applied science is establishment of fellowships for the special purpose of encouraging applied scientists to complete their education by obtaining Ph. D. degrees.

Introduction

The questions concerning the proper level of Federal support and the balance of support given to various fields are of great and obvious importance. There are, however, other closely related and specific questions. These are concerned with weaknesses in our present effort and with the general problem of how to make the overall scientific and technological program more efficient. In the following I shall limit myself to research, development, and education in the physical sciences, for the simple reason that my competence lies in this field.

It is my opinion that the U.S. effort is not sufficiently strong in the broad field of applied science. In particular, we are not placing sufficent emphasis on the education of applied scientists. I believe that a reasonably planned effort in the graduate education of applied scientists would have most beneficial consequences for our whole program.

I do not feel that I can criticize the high level of Federal support directed toward the advancement of science and technology. When the amounts spent on the various fields are compared, everyone will have a different view concerning comparative merits and concerning desirable shifts in the program. I feel, however, that a particularly strong point can be made about the lack of applied scientists. Most of our Federal expenditure is used to support applied science and the engineering developments based upon applied science. At the same time, most of our educational effort on the relevant graduate level goes into the support of pure science (*1*). As a result, the most massive expenditures of our Government suffer from inadequate technical leadership.

The following statement proposes to describe this situation more fully and to discuss some methods of possible improvement. For purposes of clarification I shall start by defining "pure science," "applied science," and "engineering development."

Pure Science

Pure science is concerned with the discovery of new facts and with the understanding of nature. This endeavor is not affected by any expectation of practical applications. It is guided by value judgments concerning the interest in various fields. In general, these value judgments are rendered by the scientific community as a whole. One might expect that from time to time strong differences of opinion would arise concerning these value judgments. The fact is, however, that as a general rule a remarkable agreement exists within the scientific community concerning these judgments.

For instance, it is generally believed today that high-energy physics is of great interest. There are differences of opinion as to the amount of support that high-energy physics should receive, but these are differences of degree. That reasonably strong support should be given to high-energy physics is denied by no one. At the same time, one must remember that no practical applications of high-energy physics happen to be in sight.

The value of pure science may be explained in two different ways. On the one hand, pure science has an intrinsic intellectual value. This value is readily recognized by the expert. Often it is difficult to explain this intellectual value to the layman. It is, in my opinion, highly important to try to do so.

The other way to appreciate pure science is to point out the fact that most important practical applications have resulted from scientific discoveries that were originally "pure," i.e., were not motivated by any foreseeable application. Thus, Lord Rutherford was deeply convinced that nuclear energy would never be applied. Nevertheless he and his like-minded collaborators created the basis of a technology that has deeply affected the power balance between nations and is gaining more influence on our peacetime economy. When Faraday explored the laws of electricity and magnetism, his main motivation was the conviction that these laws had a basic importance in explaining the structure of matter. This intuitive expectation proved to be correct. However, most important practical applications emerged even before electromagnetism was applied to atomic structure. Faraday himself, when questioned by Gladstone about the practical uses of his discoveries, could only say about electricity: "Some day, sir, you may tax it."

Pure science is currently receiving most generous support. As a result, the United States enjoys an unquestionable lead in pure science. This could result in practical applications that, in some cases, may be only 10 or 20 years away but, in many other cases, may emerge in 100 years or more.

Engineering Development

If pure science may be considered as the first phase in the structure of science and technology, then engineering development may be placed in the position of the last phase. In this phase, feasibility of a project is assured. The only question remaining is that of effective, economical, and safe execution of the project.

The United States has a long and firmly formed tradition in engineering development. Our engineering schools turn out a reasonable number of young people with bachelor's degrees who can participate in this effort. There have been some complaints recently that the number of our engineering graduates is declining. The effect of this is offset, at least in part, by the fact that the quality of engineering education is improving. A further increase in the number of engineers possessing master's degrees and doctor's degrees would appear to be of advantage in view of the increasingly complex and numerous problems with which engineers are faced. While such an increase will have beneficial effects, it does not satisfy the specific need in applied science, which is to be discussed in the next section.

The importance of engineering development has never been doubted. Its financial support is very respectable, although, considering the great needs of our time, it may not always appear to be ample. It is my belief that this field certainly should not be considered a weak link in our scientific and technological effort.

Applied Science

Applied science occupies an intermediate position between pure science and engineering development. In applied science we have a definite, practical aim in mind. We are, however, uncertain whether or not this aim can be accomplished. Even if feasibility should be proved, it is by no means clear whether or not the proposed effort will lead to rewarding results in an economic and practical sense.

To find the new fruitful applications of pure science is in itself a creative process. In a very great number of cases the application does not fill any existing need. Instead it generates a novel need and answers a question that never has been asked before. Atomic energy, space exploration, and electronic computers are good illustrations. Applied science often postulates a completely new situation and then attempts to realize it. For this reason applied science is an eminently creative field. It is not the amount of creativity but its direction that distinguishes it from pure science.

Many of the methods employed in applied science are the same as those practiced in pure science, but there are differences. A project in applied science very often requires the cooperation of experts from many different fields. This imposes a different style of work. It is also quite frequently true that applied science projects are tied more closely to a time scale than are undertakings in pure science. However, imagination and invention play similar roles in the fields of pure and applied science.

In our educational institutions applied science may almost be described as "no man's land." Recently I interviewed 24 most promising students from the various departments of the Massachusetts Institute of Technology. These departments included mathematics, physics, chemistry, and many branches of engineering. The purpose of the interview was to select students for fellowships in applied science. The interviews revealed that 22 out of the 24 showed a marked preference for pure science. In noting this ratio, one should consider that the Massachusetts Institute of Technology is supposed to have a particularly close connection with technology.

Our deficiency in applied science might be illustrated by referring to our space program. This program, on which we are spending more than $5 billion a year, has connections with engineering development, applied science, and pure science. Most of the money is spent in engineering development, but applied science is an important component, and this component determines to a great extent how effectively the money is spent. Pure science is hardly needed for the planning of our space program but will probably gain from the results obtained.

Problems like improved space propulsion or developing the right instrumentation for our space vehicles are typical problems of applied science. It is difficult to estimate how many fully educated "applied scientists" are to be found in our space program. My suspicion is that this number may not be sufficiently great. In a new field this may be unavoidable but the result is that the money of the National Aeronautics and Space Administration is not being spent in an optimal manner. With more emphasis on education in applied science, the time required to correct this situation may be shortened.

I believe that the major part of our scientific-technological effort could be greatly improved if more applied scientists could be educated and if some of our most talented and energetic young men could be attracted to this field of endeavor.

Examples of Applied Science

The field of applied science is so broad that a complete listing of its various branches seems hardly feasible. In the following I shall attempt to give a number of examples in order to obtain a more vivid picture of the requirements that face us.

In chemistry, a close and healthy contact has been maintained between pure science and applied science. This has resulted in a healthy and vigorous development of our chemical industries.

In the first decades of this century aeronautics was a very popular branch of applied science. This was due to the great and understandable excitement caused by the development of the art of flying. The rising importance of aviation in wartime applications was a contributing cause. At the California Institute of Technology there exists a most outstanding school of aeronautics. Thus, aeronautics is one field in which applied scientific work has flourished. It is certain that work invested in applied science in this field has paid ample dividends.

Another success story of applied science is that of electronics. Here the Massachusetts Institute of Technology has played a leading role, stimulated to a considerable extent by the wartime development of radar. Electronics has been further strengthened by the most remarkable work done in the Bell Telephone Laboratories. Even these few examples clearly illustrate that a strong position in applied science is often obtained in connection with one or a few outstanding centers of research.

One application of electronics has assumed gigantic proportions, and it certainly deserves a few separate words. This is the development of electronic computers. In the earlier stages of this development the Massachusetts Institute of Technology and the excellent laboratories of the International Business Machines Corp. have played outstanding roles. While this particular area of applied science is based on electronics, it has in turn stimulated applied mathematics, the introduction of precise methods into new fields, and the increasing replacement of human labor by reliable and effective mechanized equipment. In my opinion it seems probable that automation will continue to be one of the most rapidly growing branches of applied science.

In contrast to the examples cited above, there are several fields of applied science in which efforts in the United States have been insufficient. One of these is meteorology, which some consider to lie on the borderline between pure science and applied science. During the war meteorology was emphasized, but this effort ended in 1945. There have been recent attempts to step up our progress in meteorology by establishing a national center of atmospheric research at Boulder, Colo. It is too early to evaluate this particular effort, but it seems to me that it constitutes the very beginning of an important development. Parts of this development are connected with weather observations from satellites and with numerical weather predictions with the help of electronic computers. It is probable that this will lead to improved forecasting. It is also probable that eventually weather modifications will become possible, although the extent to which we can influence weather is as yet quite unclear. At any rate, the possible benefits from the study of meteorology appear very great in comparison with the limited efforts that have been made in this field by U.S. scientists. It should be stated that past and present methods by weather modification have been criticized as amateurish. This criticism is probably well founded.

Oceanography is in a position similar to that of meteorology. Woods Hole and the Scripps Institution are examples of excellent but small oceanographic laboratories. In recent years a greater effort has been made. One incentive relates to the fact that the oceans constitute our greatest reservoir of organic material and therefore of food. Thus, oceanography requires cooperation between the physical and biological sciences.

During World War II, very great strides were made in nuclear engineering. After that period, progress has been spotty and, in some areas, insufficient. Some nuclear reactors have successfully entered the phase of engineering development. More advanced nuclear reactors do not always receive sufficient attention. The use of isotopes has been pursued in a widespread and altogether effective manner. Nuclear explosives have been developed with some success for military applications, but the challenging and hopeful field of peaceful application of nuclear explosives was neglected up to the year 1957. Since that time some progress has been made, but we are not moving at a sufficient speed. This has been due to the erroneous conviction, originally shared by military and civilian authorities, that the proper application of explosives lies in the military field.

The exploration of controlled thermonuclear reactions has been strongly supported for the past dozen years. Success is not yet in sight.

But, as is often the case in applied science, important results have been produced in directions a little different from those originally contemplated. Thus, the study of plasmas is leading to new applications in electrical engineering. On the other hand, a better understanding of plasma physics has become more helpful in astrophysical discussions. What started as a development in applied nuclear science is paying dividends in nonnuclear engineering and is also exerting a stimulating influence on certain fields of pure science.

Space exploration occupies a unique position in our discussion. This is due to the fact that since Sputnik our deficiency in this field has often been discussed. Furthermore, this field receives by far the most massive support. Finally, astronautics enjoys an unparalleled popular appreciation. It is possibly true that funds not expended on space exploration could not be readily transferred to support of other scientific or technological endeavors. As a result, our program in space exploration has outpaced considerably our ability to perform in a really effective manner. Apart from a more rational distribution of funds, the solution, in my opinion, must lie in an increased effort in education. While such improved education will certainly benefit our space effort, it will also have important beneficial consequences in all other fields of applied science, both those listed above and those that I have omitted.

Education in Applied Science

Education in applied science should proceed at the laboratories and centers at which applied science has actually been developed, where the pioneers in applied science work, and where the best equipment is available. Here the students who should obtain Ph. D. degrees in applied science can acquire the best tradition while working in surroundings in which the primary motivation is to mold our future technology along the lines made possible by the latest advances in science.

To give proper stature to these programs and to exercise the required controls, it is suggested that these programs be worked out in conjunction with universities. The degrees should be given by the universities and the men who are charged with the duties of teaching and supervising the Ph. D. theses should become professors or part-time professors at the universities. The geographical separation as well as the novelty of the program will undoubtedly cause difficulties. Faculties of universities may not easily accept applied work as similar in value to the old academic disciplines. But one should realize that in the program proposed here, there will be a requirement for the greatest possible amount of new initiative and imagination. For this particular reason such a program, once it is accepted, should fit excellently into the framework of our universities.

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It would be natural to limit the programs herein described to graduate studies. The undergraduate work could be properly completed at our universities. It would seem appropriate that students from engineering, physics, chemistry, and mathematics be acceptable in this graduate program. In fact, an applied scientist must possess a general background in all physical sciences and in engineering.

The modern applied science effort requires the cooperation of many people with diverse skills. If the general education indicated in the above paragraph is not available, smooth operation in our applied science laboratories will not be possible. Our university departments tend increasingly to emphasize specialization. While this trend helps in many branches of pure science and also is useful in engineering developments, it has a decidedly harmful effect in applied science.

The needed general background may be made available in undergraduate studies at the university. Alternatively, it should be possible to impart the needed knowledge in the first year or in the first 2 years of graduate work. It may be expected that many of the most talented students will turn toward applied science after having completed their undergraduate work at a university in engineering, mathematics, or one of the physical sciences. It should be made possible for these students to enter the curriculum and to pick up the needed general information at the graduate level.

Subjects that should be required in all cases include mathematical techniques like the handling of complex variables, matrices, partial differential equations and boundary value problems. Students should be given a thorough familiarity with the handling of modern computing equipment. In the sciences, an applied scientist must certainly have thorough familiarity with physical chemistry, thermodynamics and statistical mechanics, electromagnetic phenomena, and most particularly with the broad field of the structure of matter. The latter must include a basic understanding of atomic structure and also the general properties of materials in all their states of aggregation. The more this broad knowledge can be imparted in close relation with engineering applications, the better it will be for the success of the program.

In addition to this background a second skill must be acquired by an applied scientist. He should become an expert in at least one specialty by completing an original piece of work as his Ph. D. thesis. This is important not only in itself but also because of the psychological fact that in many ways one specialist can better understand another specialist as long as both can fall back on a common language.

Recommendations

To stimulate the needed cooperation on education between appropriate Government laboratories and universities, several steps may be suggested. The simplest measure would be to permit the use of Government laboratories for educational purposes. In some cases there are regulations that limit such use. Any such limitations should be carefully reviewed and, wherever possible, removed. The Government laboratories, such as the laboratories of the National Aeronautics and Space Administration, the Department of Defense, and the Atomic Energy Commission, should be instructed to investigate from case to case whether cooperation with a university would be feasible and in what way the best mechanism for such cooperation could be provided. To finance such collaboration a small fraction of the budget of each laboratory (from a fraction of 1 percent to a few percent) should be earmarked by Congress for the support of such a cooperative effort. Enabling legislation might be required for this.

Further support for such a cooperative enterprise might come from the National Science Foundation. This could take the form of grants to round out the educational programs of laboratories with activities that lie outside their usual programmatic goals. Such programs might be established by laboratories where existing facilities make the pursuit of an important technical development particularly appropriate. At the same time, the National Science Foundation might negotiate contracts with universities for the purpose of facilitating collaboration with laboratories.

The cooperative effort need not be limited to Government laboratories. Collaboration with laboratories of private industries might be considered. This could be done, for instance, by offering matching funds to any university, whether a private university or a State university, for the purpose of establishing a joint educational venture with an applied laboratory.

It may be argued that in this way certain industrial laboratories would be favored. On the other hand, it should be emphasized that work of such industrial laboratories is in the national interest and is at present encouraged by Government contracts directed toward research. One might therefore consider making Federal aid dependent essentially on only one criterion: the quality of the educational work that can be expected. Continuation of these funds should depend on satisfactory performance.

A further measure that Congress might consider is establishment of tax benefits designed to stimulate capital expenditures for educational facilities. Within limits the tax benefits might be extended to the operational costs as far as they could be charged to identifiable educational activities.

The last but not least important measure should be the financial support of graduate students in applied science. Fellowships are already available for such students and, in principle, no new measures are necessary. It would be important, however, to keep track of the fraction of graduate fellowships that support applied science, and to seek continually the best ways to recruit and support talented students in applied science.

It must be pointed out that young scientists inclined toward applications are frequently eager to join practical projects before they have completed their education. Since good men in this field are in short supply, they usually can earn relatively high salaries. In the long run, the resulting incomplete education is not desirable. To enable these young people to complete their education, generous stipends will have to be made available.

The United States possesses the fundamental skills from which a vigorous development of applied science can proceed. These skills, however, are separated from our educational process and they are not so organized as to satisfy the growing demands for young applied scientists. We have to tackle this special problem to make sure that we maintain, and in some areas regain, a position of leadership in pioneering the developments of future technology.

References

(*1*) There is, of course, a very massive educational effort in engineering, most of which, however, is on the undergraduate level. The deficiency that I am discussing in the present paper lies specifically in graduate education. The relation of engineering to the topic of this paper is discussed separately.

FEDERAL SUPPORT OF BASIC RESEARCH

by John Verhoogen
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Summary

There is no simple formula to relate the level of Federal support of basic research to economic or military needs; because basic research is, by definition, concerned with the unknown, its returns per dollar spent cannot be predicted on grounds other than the observation that, so far, they have been, culturally, socially, and economically, very great indeed.

A guideline to a policy for science may perhaps be found in the proposition that our rate of progress in science will depend mostly on the quality and quantity of available brainpower. The first step must then be to insure that scientists and potential scientists are provided with adequate opportunities to develop, demonstrate, and exercise their creative powers. Ideally, every scientist who is capable of raising a valid scientific question, and of contribution significantly to its answer, should be given the means to do so. Research in universities supported in large part by Federal grants should remain the backbone of Federal policy. The largest share of funds for basic research should go to general-purpose agencies (e.g., the National Science Foundation) rather than to mission-oriented ones (e.g., National Aeronautics and Space Administration); the National Science Foundation, in particular, needs considerable budgetary strengthening. A general increase in research funds of 10 to 15 percent per year for the next 10 years may be appropriate.

Introduction

We ask an answer to the question: "What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?"

A few preliminary remarks are in order:

The question, as formulated, assumes that the United States now has a position of leadership in the economic, cultural, and military applications of science. The assumption is important, because it might seem to follow that the level of support of basic research that has established such leadership should also be sufficient to maintain it, at least for the next few years. We note, however, that our military supremacy, or whatever part of it science is responsible for, is still, and we hope will remain, untested. The United States has without doubt mastered the technology of many fields; but brilliant engineering achievements are not to be seen exclusively in the United States, and our technological supremacy does not extend to all fields. Where we do lead, the margin is not invariably very wide, and it is conceivable that at the present rate of development other countries might surpass us in many respects in a decade or less. Nor is it clear that we lead in social or cultural aspects of science. Our rate of progress in the social aspects of medicine is notoriously slow; the philosophical and cultural implications of science seem to receive more attention abroad. The number of Nobel laureates compared to total population is not highest in the United States. Complacency and self-admiration are never helpful. What is needed is not just to maintain our position, but to improve it.

A second remark pertains to the word "basic," as applied to science. Basic research, as we understand it in this report, is that part of scientific inquiry that is in essence nonprogramatic. It is motivated primarily and simply by curiosity, by the desire to know and understand without regard to any practical applications that may arise from it. But the dividing line between basic and applied science is at best an elusive one, if it exists at all. Whatever the differences in motivation may be, their requirements and procedures are often identical, and although basic science is usually thought to precede applied science, the feedback from the latter to the former is by no means negligible. The two are inherently difficult to dissociate. For the purpose of the present report, it may be convenient to draw the dividing line where the ratio of cost to direct applicability becomes large; therefore we do not consider research of the type that can be supported by the industry that will derive immediate returns from it, but will include much of what some might consider to be "applied" research.

Two Features of Modern Science

There are two characteristics of modern science that must be kept in mind. The first is its all-pervasiveness. There is no need to repeat here what has been said many times about the scientific revolution we are now living. It is a fact that science now molds our daily lives to a degree unimaginable 50 years ago. And it is also a fact that if we were to shut down our laboratories and burn our scientific libraries, our society would not survive very long under its present form. The trend will continue; science, which has been felt so far mainly in technology and medicine, is now spreading to all human affairs. The second characteristic is that as science progresses the cost of each new bit of information increases as it becomes more difficult to obtain. Exploring the surface of the earth, for example, is relatively cheap compared to the exploration of its interior. These two trends explain why governments are increasingly called upon to support science: both its scope and cost now vastly exceed the resources of a private institution or group. The 18th century was the age of the single investigator working in his own private laboratory. The 19th century saw the development of universities well capable of serving with their own resources the scientific needs of the time. The 20th century must look at the matter in quite a different way.

Science and the Military

We now turn to our central theme, which is to assess the proper level of Federal spending in basic research, and consider first the effect of science on national security. World War II and the years thereafter offer a stunning picture of the speed with which scientific discoveries move from the realm of "pure" science to become instruments of survival. From this picture two lessons must be learned. The first is that this Nation must not find itself, as it did in 1941, in a position where, for lack of previous adequate support of basic science at home, its fate depended to a large extent on knowledge obtained abroad. The second lesson is that as no one knows what the next weapon will be, basic research must be carried out simultaneously on many fronts, including many seemingly implausible ones. Few people would have thought, 10 years ago, that national security could in any way depend on, or be related to, for instance, fundamental research in seismology.

Thus, although military considerations unequivocally demand basic research on a broad front, the long-range unpredictability of military needs precludes any precise evaluation of what would be an adequate level of expenditure. Clearly, we need science; but just how much is enough? Only history may tell.

Science and Economic Growth

Very much the same situation exists with respect to the level of support of basic research needed for economic growth. The relation between the two is still most tenuous and obscure. Although the most prosperous countries are also those where inquiry flourishes best, it has been pointed out that the two industrialized nations that devote the highest proportion of their gross national product to research and development—the United States and Great Britain—are not the nations that have had in late years the fastest rate of economic expansion; the extraordinary recovery of Germany after World War II was not based on science, at least not on German science. The discrepancy may be accounted for in a number of ways. It may perhaps suffice to point out in the first place that basic science does not necessarily and invariably precede technology; historically the Industrial Revolution preceded the scientific one, and James Watt invented the steam engine long before the laws of thermodynamics on which it operates had been formulated; our knowledge of aerodynamics was still very meager when the first airplanes appeared in the sky. Secondly, some industries apparently grow very well without having to spend much money on research. Finally, the "leadtime" between scientific discovery and its technological application is still large, on the order perhaps of 20 years or more in some instances. If it is true, as stated recently by Lord Todd, that the "seeds of any large technological developments to be made in the next 20 years are already with us," it follows that money spent today on basic research may not bear its economic fruits much before 1980. This timelag could in all probability be reduced if needed, for it seems to depend much on the effort put into development; yet on the whole it might increase rather than decrease, for as science becomes more sophisticated, more esoteric, more abstract, and as its ideas become increasingly remote from "common sense," it will presumably take them longer to reach the design engineer. The industrial utilization of gravitational energy that keeps "quasars" going will certainly require much more time than that of nuclear power.

What all this adds up to is, again, that although science does in a general way lead to economic growth, the proper level of support of basic research cannot be determined by a simple formula from economic considerations only; there is as yet no practical way of estimating possible economic returns of money spent on general basic research.

Science and Culture

Even in our pragmatic culture, usefulness is not the sole criterion of merit. Basic research has a much broader justification in that the quest for knowledge is one of man's most characteristic and vital urges; the desire to know is perhaps what most sharply separates him from beast. Most of human history can be read as an incessant query, the search for answers to unceasing questions: What is the stuff of the universe, and why; what is life, and how did it start? It is properly mankind's heritage that knowledge is an essential aspiration—to give insight into the circumstances of our existence, and to give us freedom from fear of natural forces.

To put it simply: Human beings want bread, and they want freedom, and some of them want to know.

At this point it is not inappropriate to consider the close relationship of science to a free society. Is it accidental that the 18th century produced at the same time the first great burst of basic science and the first great step toward free democratic societies? Is it mere coincidence that the American Constitution and the French Declaration of Human Rights are contemporaneous with the great mathematicians, physicists, chemists, geologists, on whose work all of our modern science still rests? Many have considered the relationship between a free society and the scientific spirit to be fundamental. A democratic society, it is said, is one that is uniquely favorable to the scientific spirit; conversely, a society is more likely to prosper and remain free if it fosters in all its citizens the spirit of free inquiry, the desire to know, the search for new and better ideas, and the curiosity that are basic ingredients of science. Even though science has occasionally been misused, and scientists have supported undemocratic philosophies, it remains true that allowing the scientific mind free play is a means of strengthening the individual freedom of mind without which a democracy may find it hard to survive.

It should be pointed out, in fairness to other aspects of culture, that science is not unique in promoting democratic welfare: Philosophy and the arts are just as indispensable as science. The study of history is surely a better guide to political wisdom than is quantum mechanics. It has been said again and again that science cannot flourish when divorced from the humanities, and to that view we subscribe. Support of science must entail support of the liberal arts. A full discussion of this matter should properly find its place in a report on governmental support of education, which is not the subject of this paper; let it suffice at the moment to remind the reader that good science requires good education, in the broadest acceptation of that term.

Federal Role in Science

Convinced as we are that the pursuit of science is an essential function of our present society, we have nevertheless reached the conclusion that the proper level of spending on basic research cannot be determined from economic or military considerations only. The point is that basic research being, by definition, concerned with exploration of the unknown, we simply cannot predict what the returns per dollar spent will be.

We have, of course, no historical precedents to guide us. We do not know of any society that collapsed from overspending on basic research, nor is it clear which societies, if any, have collapsed from underspending. The idea of governmental support of research is, forgetting the alchemists, not much more than 30 years old. Prior to that, peace-time research was conducted almost exclusively in universities, in industrial laboratories, themselves not much more than 60 years old, and in a few governmental specialized agencies (e.g., U.S. Geological Survey). Governmental support of research on a broad scale began to appear in Europe in the 1920's; large peacetime expenditures on research in this country really date from World War II. The form of governmental support has changed rapidly in the past, and presumably will continue to do so in the future. How can we then, in such changing and largely unpredictable conditions define the Federal responsibility?

A Tentative Guide Line

A tentative guide to a policy for science may perhaps be found in the observation that the most important ingredient of scientific progress is, of course, the scientist himself; the size and quality of our scientific enterprise will be determined by the quality and quantity of the available brainpower. Science consists of much hard work and a few brilliant ideas; we need many well-trained, intellectually alert, and thoroughly competent scientists to assemble the facts, and we also need the extraordinarily gifted—and therefore very rare—individuals who, starting from the facts, create significant ideas. The ratio of the two varies much from field to field; in mathematics, for instance, only the very few most creative brains really matter, whereas in other fields we still need very much to assemble the verifiable facts on which new ideas may grow. It follows that our rate of progress in science will depend mostly on the quantity and quality of available brainpower. As the judgment of quality can be made only by judging achievements, the first requirement in a policy for science should be to provide opportunities for scientists and potential scientists to develop, demonstrate, and exercise their creative powers.

Research in Universities

Opportunities to develop and exercise creative power exist, of course, in universities; it is, in fact, the prime responsibility of the universities to provide them. Such opportunities consist, first, of exposure to excellent teaching. But teaching, however good it may be, is not enough. One does not become a good scientist just by listening to lectures, or even by performing a routine piece of experimental research. What counts most is the intellectual atmosphere, the stimulation and excitement of seeing research done and results achieved, the development of inquisitiveness, the spirit of adventure, the willingness to follow one's own mind wherever it may lead. All this thrives best where research itself is thriving; good research in the universities is a prerequisite to good research anywhere.

The matter of Federal support of basic research in institutions of higher learning has been considered in detail in a recent Academy report. We need only repeat briefly some of its conclusions: Research project grants should remain the backbone of Federal policy; what is most needed is a liberal system of individual grants ("little science") of the type now administered, for instance, by the National Science Foundation; we also need (1) institutional and general research grants; (2) small and largely "uncommitted" grants to young scientists on the basis of a very general outline of their interests and the endorsement of their seniors; (3) a selective program of research grants to weaker institutions that could be raised to a higher level of excellence. Suggestion No. 2 seems particularly pertinent, for it is probably from the group of young scientists who have not yet made a name for themselves that the most vigorous and original ideas will come.

"Little Science"

The essence of this proposal is that, ideally, every scientist who is capable of raising a valid scientific question and of contributing significantly to its solution, and who is not already supported by industry or by private or state funds, should be given an opportunity to do so by means of a research grant. A competent scientist probably profitably spends about $20,000 per year on the average. The number of competent scientists with good ideas is hard to estimate; it is certainly less than 100,000 and possibly in the neighborhood of 50,000. The total expenditure nationally would amount to perhaps $1 billion per year.

The main problem that obviously faces us here is the judgment of quality: What is a good proposal? That judgment could perhaps best be made by panels of experts, as is currently done, for instance, in the evaluation of National Science Foundation proposals. In fact, the method of evaluation could lead to a practical method of determining the proper annual appropriation for "little science;" it should be commensurate with the total sum requested during the previous year for proposals rated "good" by scientific advisory panels, the rating being made independently of available funding.

"Big Science"

"Little science" grants, by definition, would go mostly to single scientists or small groups of scientists, working in small laboratories with equipment that can be made or bought for, say, less than $100,000. There are, of course, scientists whose work centers around large "machines" and to whom "little science" grants are not very useful, as the main cost of their research lies in the capital investment of big tools: Telescopes, oceanographic vessels, high-energy accelerators, for example. Clearly also, a greater number of scientists will find themselves in need of large computers which, like telescopes, cannot be bought on ordinary research grants. The "little science" program must therefore be supplemented by a "big science" program of capital investment. A number of planning reports outlining the needs of the various branches of science are now being prepared, so that it is premature to guess what the total will be—perhaps of the order of $500 or $1,000 a year.

General Support of Graduate Schools

Grants to university investigators are unlikely to be productive if the investigator himself has no laboratory to work in; he also needs a library, shops, and free time. All this is presently beyond the means of many smaller universities, so that the Federal Government must be prepared to provide increasing support to graduate schools, and may well have to provide most of it to institutions of emerging excellence. National Science Foundation appropriations in this field are still very modest and should increase rapidly. In addition, and because the advancement of science is inseparable from progress in the arts and humanities, as noted above, a form of support broader than can presently be provided by the National Science Foundation may have to be devised to secure well-rounded growth of the undergraduate colleges that feed the new graduate schools.

Science in Federal Agencies

Not all basic research is done in universities or private institutions: Federal agencies such as the Weather Bureau, the Bureau of Standards, the Geological Survey—to mention just a few—are concerned with research as basic as that pursued anywhere. It is of the highest importance that such programs be strengthened, and that the agencies be maintained at a high level of efficiency and productivity. In the first place, their research, e.g., meteorology and weather prediction, is commonly of a kind and scope that cannot be carried out in universities. Second, competition in science is good, and Government agencies may set standards of excellence in research that private institutions should equal or surpass. Finally, it is difficult in a free society such as ours to insure that science will move forward with the necessary vigor on all fronts; at times there are likely to be "fashionable" research topics that attract general attention while other equally important fields lie neglected and fallow. By a judicious choice of its own research program, the Federal Government can to some extent correct the imbalance.

Allocation of Funds

The difficult point arises here as to how funds should be allocated among the various branches of science. Of a total of approximately $1.4 billion obligated for basic research in fiscal year 1963, about 32 percent went to the National Aeronautics and Space Administration and only 11 percent to the National Science Foundation. Admittedly, research is more expensive in some fields than in others; nevertheless, a ratio of 3 to 1 of space-oriented research as compared to all other fields supported by the National Science Foundation must raise some questions, for we submit that no single branch of science can in the long run be more important than any other, or than all others. Because it is inherently impossible to predict where the most significant discoveries will be made, adequate support should be available at all times in all fields, and the allocations of research funds should be determined primarily by the scientific merits of the proposed investigations. Research in mission-oriented agencies comes under a different category, for the decision to carry out such missions (e.g., man on the moon) is not primarily motivated by its scientific importance; whether such expenditures are justified or not is not entirely a matter for scientific judgment. The large items for mission-oriented research in the Federal science budget tend to obscure the fact that funds for general-purpose research are still very low. The need to strengthen general-purpose agencies, and particularly the National Science Foundation, cannot be overemphasized.

The Need for Continuity

A further argument for gradually shifting the support of basic research from mission-oriented to general-purpose agencies is that the support of science must be continuous. Unlike public works, science cannot be turned on and off again. Industrial plants can be set up and properly staffed in a relatively short time; laboratories and universities cannot. It takes years to train a scientist, and it may require many more years for his work to mature and bear fruit; likewise, any disruption of scientific activities can have lasting effects. Science in Germany, for instance, still shows today the adverse effects of political events of the 1930's. A policy for science must of necessity be a long-range policy, as money spent today may not show its beneficial effects for many years to come. Responsibility for carrying out a policy for science should rest primarily in permanent agencies set up for that purpose.

Conclusions

The general theme of this paper may be summarized as follows: Science and technology have become so closely woven into the fabric of our society that we simply cannot do without them. Yet the relation of basic research to economic growth remains elusive; because basic research is, by definition, concerned with the exploration of the unknown, there is no way of calculating in any serious way the probable economic returns to the Nation of investments in science, and there is no formula for relating the level of support of basic research to short-range military or economic needs. But even though it may be difficult, if not impossible, to disentangle the respective contributions of science and technology to economic developments and material progress, it seems evident that enormous returns have indeed accrued from the very small sums spent on basic research up to and including the first half of this century. There is no reason to suspect that things might be different in the future.

For want of a better criterion, I suggest that our scientific policy be based on the premise that the best chances for scientific progress lie in giving competent scientists who have valid scientific questions to ask an opportunity to work out the answers. Research-project grants should remain the backbone of Federal policy, as discussed in a recent National Academy of Sciences report (*Federal Support of Basic Research in Institutions of Higher Learning,* National Academy of Sciences, 1964).

In practical terms, this recommendation means that the level of spending on basic research should be such as to allow funding of all meritorious research proposals, the judgment of merit being made on purely scientific grounds by appropriate panels drawn from the scientific community.

The total rate of spending implied by this policy is very hard to estimate: Perhaps about $4 to $5 billion per year 10 years hence. This figure is, admittedly, a very rough guess. It was arrived at by the simple expedient of multiplying the Federal obligations for basic research in 1963 by a factor of 3. The factor 3 was chosen because the level of spending on science depends roughly on the number of scientists, and that number is not likely to more than double in the next 10 years; an additional factor of 1.5 was included to allow for increasing costs (per scientist) of work performed in some fields. Large as this figure may be, it should be remembered that an increase by a factor of 3 in 10 years means a rate increase of only 11 percent per year. Expenditure of $4 billion for science as a whole 10 years hence does not seem excessive if the exploration of space alone is now worth $5 billion per year.

What the Federal budget for science should ultimately be is, of course, not a proper matter for scientists to decide; all I wish to do is point out what I think scientists could usefully spend in the near future. But whatever the figure will be, it is important that the largest share of the money for basic research should go to general-purpose agencies rather than to mission-oriented ones—e.g., the National Science Foundation rather than the National Aeronautics and Space Administration. The reasons for this are, first, that as no one knows in what field the most significant discoveries will be made, research must be pursued on a broad front; and second, support of science must have more continuity than can be provided by agencies that could conceivably be terminated when their mission, or part of it, has been accomplished.

SCIENTIFIC CHOICE, BASIC SCIENCE, AND APPLIED MISSIONS

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Introduction

My purpose in this essay will not be to answer directly the two questions concerning scientific choice put by the House Committee on Science and Astronautics. Rather, I shall try to outline a way of looking at the question of scientific choice which I believe both clarifies the issue and diminishes the magnitude of the problem. What I hope to contribute is a point of view that will help our Government allocate resources to science more rationally, rather than any specific suggestions as to what such allocations should be.

The questions posed by the House committee assume implicitly that "Support of Science as a Whole" is a useful subclassification within our Federal budget, and that we must be prepared to make allocations within an overall science budget for all scientific and engineering activities. I, along with many others, believe that lumping all science and technology together is misleading. The same considerations that militate against a single Department of Science militate equally against putting support of all science in one compartment of the budget. I shall therefore propose a different and, I believe, more rational way of dealing with questions of choice and magnitude of support of science; and I shall indicate how this could influence our governmental organization for science (*1*).

One overriding practical problem emerges from my analysis. This has to do with the predicament of our expanding basic research enterprise in the physical sciences. Until now U.S. basic research in the physical sciences has been supported largely as an assessment or "overhead" charged against mission-oriented agencies, such as the Atomic Energy Commission and the Department of Defense. These agencies, whose primary missions lie outside science, have fairly stationary budgets; they can hardly be expected to underwrite the expansion in basic research that most of our physical scientists consider desirable. This expansion, if it takes place, will therefore have to be charged to the Government agency whose primary mission is the support of science—that is, the National

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Science Foundation. Whether or not basic physical science continues to flourish will therefore depend largely on whether or not Congress encourages the growth and vigor of the Foundation. Expansion of the National Science Foundation is perhaps our country's central political problem related to the support of science.

I. *Political and Administrative Choices*

As a prelude to addressing the problem of scientific choice, I digress into political philosophy to distinguish between what I call political choices and administrative choices. Political choices define the national interest; administrative choices implement political choices. Congress, with the strong concurrence of the President, determined in 1941 that it was in the national interest to declare war; this was a political choice. The President and the Armed Forces, with the concurrence of Congress, decided how to fight the war; this was an administrative choice. Congress decided in 1961 that it was in the national interest to send a man to the moon; the National Aeronautics and Space Administration determined that the lunar orbital rendezvous with a Saturn-boosted launch was the way to achieve this goal.

In practice, political and administrative choices are not so sharply separated. Congress greatly affects administrative choices through its control of the purse strings. The Executive obviously influences political decisions, although Congress has the last word in drafting legislation.

The problem of choice is not so much who makes political choices and who makes administrative choices: It is rather that choices should be made among political alternatives or among administrative alternatives, not among political and administrative alternatives. We should not choose between an end and a means to achieve an end: Whoever makes choices should choose between different ends or between different means to achieve the same end.

Allocation of some of our science budget involves administrative choices; allocation of other parts involves political choices. Therefore choosing among alternatives from "all of science" confuses political choices with administrative choices.

II. *Basic Science and Applied Science*

This digression into political philosophy is relevant to the issue of allocation of scientific effort, the root problem the Miller committee wishes us to consider. For, insofar as applied science is merely a means to a nonscientific end, the allocation of our applied research and development budget involves administrative decisions, whereas allocation of our budget for pure science—science which is done for its own sake and is therefore an end in itself—involves political decisions.

An example will clarify the matter. Suppose we decide, as a matter of national policy, to spend several million dollars to help India control its population explosion. This is a political decision. In carrying out such a decision many alternatives are available. We could spend all the money to get up an old-age insurance system to reduce the economic incentive for Indian women to have more children. Or we could spend most of the money on scientific research, some of it fairly basic, to enlarge our knowledge of the physiology of reproduction in the hope that such knowledge would eventually help India control its population. Here we must choose between two strategies—one involving science, the other not involving science—for achieving the same end. The choice of the better way to achieve the end—whether science or "operations"—is an administrative decision, not a political decision. It must therefore be made primarily by the agency responsible for helping India control its population. Moreover, money spent on biological research which, in the judgment of the responsible agency, is needed to cope with India's population problems should not be part of our science budget: Such funds should be part of our foreign aid budget. We do not argue about how much the Government spends for transportation as a whole, or accounting as a whole, or legal advice as a whole. These activities in every instance are carried out not for their own sake, but to further some other politically defined objective. The expense of science as a means to achieve a nonscientific end should logically be assessed against the budget for achievement of that end, not against some mysterious budget labeled "Science as a Whole."

Some science that is relevant to population control may be rather basic—for example, the study of how progesterone affects DNA synthesis. Nevertheless, if the science is motivated by a desire to achieve a certain end outside of science, then it should be judged against other, nonscientific, ways to achieve that end: It should not be judged against other science, such as ecology or group theory, that is done either to achieve a different nonscientific end or merely for its own sake.

Insofar as we can identify elements of the science budget that are pursued for some purpose outside of science, we have succeeded in reducing the size of the problem of choice with which Congress is concerned. The choice then is left to the agency responsible for achieving the political end: if the agency decides that more science rather than more transportation is a better way to achieve its mission, the decision may still be questioned by Congress, but not on the basis that one should spend more for a different kind of science having nothing to do with the mission of the agency.

There remains a residuum of science which really is pursued for its own sake. More accurately, there is a gradation of science from the heavily applied (which is so recognized by those conducting the research) to the very pure (again as viewed by its practitioners). I shall assume for the moment that we can decide what is applied and what is basic, though in some cases such a decision is the essence of the problem. Support for applied science would be assessed as an expense against the end we seek to achieve, as I have suggested above. The remaining basic sciences, pursued primarily for reasons that are intrinsic to science itself, would then be properly included in a budget which I call the "Intrinsic Basic Science Budget."

This activity of our society, intrinsic basic science, should properly be balanced against other activities of the society—for example, education and foreign aid. As I see it, the choice between intrinsic basic science as a whole and other, nonscientific, activities is the primary relevant political decision. However, since the basic science budget is relatively small—perhaps $1.5 billion—the problem before the country is considerably smaller than one might expect.

What I say suggests that our habit of putting together our expenditures on all science has obscured the real issue. Though I am not advising that we never look at our science budget as a whole, I suggest that this not be done with an idea of making adjustments between money for basic science and money for applied science.

I believe that by realistically identifying science that is done to accomplish a nonscientific—i.e., politically defined—mission, and separating it from science that is done just for its own sake, we can simplify many of the problems of scientific choice that the Congress is worried about. The real business at hand is to decide realistically and honestly what science is done to achieve a nonscientific mission, and what science is done largely because it seems intrinsically interesting and significant to its practitioners. It would be most illuminating to examine the scientific program of each of the agencies, and to tabulate how much of its scientific work really is mission-oriented and how much is basic. I suspect that the scientific work of at least some of the agencies tends to become relatively less mission-oriented as the years go by. This is certainly true of the Atomic Energy Commission. When the Commission took over from the Manhattan District, most of what it did scientifically was directly related to the development of nuclear energy. As the years have gone by, the fraction of research less directly relevant to the mission of the Atomic Energy Commission has expanded—or, perhaps better, the mission of the Commission has been reinterpreted to include support of basic research itself—even basic research that is fairly remote from nuclear energy and its byproducts.

The task of making such an inventory will be difficult, especially since many researchers, if pressed, can find a connection between what they do and the mission of whatever agency is paving their way. Nevertheless, I believe that the National Academy of Sciences and its Committee on Science and Public Policy have a fine opportunity to make this inventory through the panels that are now reviewing different fields of science and projecting their future needs and growth. Each panel reporting on basic science ought to identify, for each major proposed expenditure in basic science, the applied science or development that this basic research supports, and the fraction of the budget for the relevant agency mission represented by this field of basic research.

In judging how much basic science "overhead" an agency can afford, many complicated but rather obvious issues are raised—like degree of relevance to the agency's mission, promise of progress, and so on. The main point is that wherever a tie-in with an applied science can be made, one has a clue, derived from the original political decision defining the importance of the applied science, as to the importance of the related basic science, and the level at which the public ought to support it.

The view I express—that to bring order to our thinking about public support of research it is first necessary to separate research done to achieve a nonscientific end from research done to further science—is a very old one. I am only urging that we really do make this separation rather than just talk about it. Had Congress originally realized that the money devoted each year to science for its own sake was about $1 billion, and not $16 billion, I doubt that there would have been so much concern about expenditures for science.

III. *Relation between the National Science Foundation and Other "National Science Foundations"*

The tendency of mission-oriented agencies to do basic research that is relevant, though only remotely, to their missions has in a way defeated the original design for the National Science Foundation. For now we have eight or nine "National Science Foundations"—the National Institutes of Health, the Atomic Energy Commission, the Armed Services, and others. Almost every agency now supports extramural research that is only remotely relevant to its mission.

This mode of support can be justified on the grounds that basic research in a field is necessary for doing related applied research. The cost of the basic research is properly to be added to the cost of achieving the nonscientific end that justified the applied research in the first place. For example, our country in 1961 decided to send a man to the moon. How much our country can afford to spend on this venture is a political decision: it must compete with foreign aid, veterans' benefits, farm subsidies, and other major governmental activities. To send a man to the moon requires a vast scientific and technological effort; obviously most of the science is heavily applied. At least some of the scientific work falls in the field of astronomy—but mostly solar astronomy, not stellar astronomy. Nevertheless, I believe stellar astronomy is sufficiently related to solar astronomy, and progress in stellar astronomy has enough fallout in solar astronomy (both directly and indirectly, through students, techniques, etc.) that it can be considered a proper assessment against the cost of solar astronomy and therefore against the cost of the moon shot. Thus I can see a justification for making the National Aeronautics and Space Administration a little National Science Foundation for basic stellar astronomy; and, moreover, I can see that the National Aeronautics and Space Administration ought to support considerable stellar astronomy as a fraction of its budget for solar astronomy.

In the same way, I would argue that each mission-oriented agency should assume the responsibility for supporting basic science in fields generally relevant to the agency's applied science, *at a level which bears some relation to the size of its applied science budget.* Thus, basic botany might be supported by the Department of Agriculture, acting as a sort of National Science Foundation for botany, and its support for basic botany would depend on the money available for applied agricultural research; basic physiology would be supported by the National Institutes of Health in the same spirit, and so on. The point is that the level at which mission-oriented agencies support relevant basic research should be related to the level of the applied research that they feel they must do to accomplish their missions. This basic research is a tax, assessed against every agency using science as a means to accomplish its mission.

How big the basic research assessment should be will vary from agency to agency. The amount of such support probably should be geared to an estimate of just how much the agency's applied mission derives from past basic research—for example, the Atomic Energy Commission, the National Aeronautics and Space Administration, and the Department of Health, Education, and Welfare should spend a large fraction, like 10 percent, of their budget on basic research, and other agencies perhaps proportionately less. In practice, this judgment, though made primarily by the agency, would undoubtedly, and properly, be reviewed by the President's Science Advisory Committee and the Office of Science and Technology, as well as by Congress. This support for basic research would be regarded as repaying a debt—a replenishing of the well—so that other similar endeavors will have something to draw on; it should also be regarded, in part, as work expected to pay off in a rather tangible but unpredictable way.

In many cases, the fraction of its applied budget that an agency is willing to devote to relevant basic research will be less than the scientists in the fields involved think they require to keep their fields healthy (as evidenced, for example, by proposals that cannot be funded). Here is where the National Science Foundation must find its primary function: The Foundation should be the agency that supports basic research, even in the fields that are supported by the mission-oriented agencies, *at levels over and beyond what the mission-oriented agencies think is proper for the accomplishment of their missions.* This function of the National Science Foundation was recognized in Executive Order 10521 dated March 17, 1954—some 4 years after the National Science Foundation was established:

> As now or hereafter authorized or permitted by law, the Foundation shall be increasingly responsible for providing support by the Federal Government for general-purpose basic research through contracts and grants. The conduct and support by other Federal agencies of basic research in areas which are closely related to their missions is recognized as important and desirable especially in response to current national needs, and shall continue.

Thus, suppose the National Aeronautics and Space Administration agreed to support stellar astronomy, but at a level only one-third as great as the astronomers believed to be scientifically justified. This is understandable, since the National Aeronautics and Space Administration's interest in stellar astronomy is somewhat peripheral. The National Science Foundation could then step in with the money needed to support astronomy for its own sake, beyond the level needed to assure success of the missions of the National Aeronautics and Space Administration.

In some cases the National Science Foundation would decide not to support a field beyond the level supported by a mission-oriented agency. This is perfectly proper. The Foundation would, in effect, be making a choice between different fields of basic research, all of which have to be supported within a total budget for "science for its own sake," and the pursuit of each of which is an end in itself.

The primary political decision involving science for its own sake would then amount to determining how much the National Science Foundation, the custodian of science for its own sake, gets from Congress. The criteria of choice the Foundation would use in making judgments between different fields could be those in my article, "Criteria for Scientific Choice," (*2*) or other similar criteria; they would include both internal and external criteria, as described there, but the criterion of technological merit would probably be given relatively little weight. In this respect the National Science Foundation would weigh its criteria of choice differently than would the mission-oriented agencies, since the latter would use relevance to their missions as a primary criterion.

I realize that the position I espouse places the bulk of the motivation for support of basic research on some hope of eventually obtaining applied benefits. This is as opposed to support of basic research for its cultural value—the pleasure it gives people to read about and understand new discoveries. I now think the expectation of eventual technical benefits to society is the firmer basis for support and is entirely suitable, provided one does not expect the benefits to be too direct too soon.

IV. *The Coming Crisis in Support for Basic Physical Research*

The line of thinking I have described (for which I certainly claim no originality) provides a key to how we deal with part of the second question put by the Miller Committee: How do we allocate support within an overall increasing or decreasing science budget? My scheme does not directly answer this question; instead, it proposes a pattern of organization (in Congress as well as in the executive branch) for dealing with the question.

According to these views, the basic science budget that supports mission-related activities (either in the sense of "pay-off" or "repayment of debt") is part of the budget for accomplishing the mission. To some extent it goes up or down as Congress decides that the mission of the agency deserves more or less support; or as the management of an agency and its advisers decide that the science is more or less germane or otherwise worthy of its support. It can be argued that this places basic research at the mercy of mission-oriented people who may have little concern for basic research. But I do not see this as all bad. For this basic research is supported in the first place because of its relevance to the agency's mission, and it is inevitable that if the agency's mission is downgraded so will be the basic research performed to support that mission. Each activity aimed at accomplishing an agency's mission should have as much a priori chance to argue for its piece of the total agency "pie" as every other. Nevertheless, I see two justifications for basic research receiving better treatment from its supporting mission-oriented agency than, say, development, in a time of falling budgets. In the first place, basic research is usually a more specialized scientific activity than is development; basic researchers cannot move as easily from one task to another as can development engineers. For this reason basic research suffers relatively more from stop-and-go funding than does development. It is easier to remobilize quickly around a specific applied project than around a highly specialized basic research project. In the second place, basic research done by an agency is like "money in the bank": it can always be drawn upon to support expensive developments, but only if it has been deposited in advance. Thus, even when an agency can support fewer big developments, it ought to continue a vigorous basic research effort in anticipation of the time when findings from basic research will assure the success of some future development.

Still, a part of the basic research budget will follow the budget for the mission-oriented agencies. The remainder, which I have identified with the National Science Foundation, would then go up or down as the Foundation's budget goes up or down. From the point of view of the scientist, such a way of arriving at our total expenditure for all basic science has the disadvantage that the bulk of science for its own sake is too visibly identified with a single agency—the National Science Foundation. But I think scientists will have to learn to live with some such situation. When we talk about large increases in science over the next 10 or so years, we are talking mainly about biology (which is broadly relevant to the mission of the National Institutes of Health) and about basic sciences in fields (like high-energy physics) which are not so clearly necessary, as is biology for the National Institutes of Health, to help the agencies succeed in their missions. The National Institutes of Health seems to me to be the Government agency, the achievement of whose mission is most directly and obviously dependent on a great push in our understanding of an underlying basic science, and whose mission will continue to enjoy greatly expanding public support. Hence I see no reason for concern about the future of the basic biological sciences: they will surely prosper as the fortunes of the National Institutes of Health prosper.

On the other hand, the necessity of expanding basic physical science research in order to further the missions of the other major agencies using science—mainly the Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration—is not so obvious to me; nor can I visualize the budgets of these other agencies expanding as fast as that of the National Institutes of Health. Even with the best intentions on the part of these agencies to maintain support of their basic research, basic research in the physical sciences is faced with a crisis. Most of its support has come from the mission-oriented agencies, but these agencies will probably not expand their support of basic research as fast as our capacity to do basic research expands. We shall therefore be faced with two alternatives: either halt our expansion of basic physical research or find a source of support for it outside the mission-oriented agencies. I favor a continued, orderly expansion in the basic physical sciences; I therefore also favor the action that, above all, will make this expansion possible: namely, an accelerated expansion of the National Science Foundation. I would expect that the Foundation will have to become a billion-dollar-a-year agency by, say, 1970 if our country's preeminent position in the physical sciences is to be maintained. I can think of no political question concerning science more urgent than the question of expansion of the National Science Foundation.

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MAINTAINING LEADERSHIP IN BASIC RESEARCH

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Summary

The frontiers of research far exceed the capacity of currently available investigators to explore them. The type of trained mind required for the job is in short supply. Continued leadership by the United States in basic research poses two requirements: (1) Additional effort to motivate able students and train scientists, starting with the elementary schools, and (2) full support of scientific activities of able investigators whose training has been completed. It is with these requirements that this paper will deal.

Leadership in basic research is of importance to the well-being of our country because it yields an increased understanding of the principles that govern the operation of our physical, biological, and sociological worlds. This understanding increases our ability to design innovations that improve our health, comfort, and safety. Stimulus to the economy, cultural advantages for more of the population, and improved methods of military defense grow from these innovations.

The Lesson of History about Basic Research

The assumption that development of technological innovations is dependent on basic research is fundamental to this discussion. History has proven it to be valid, as illustrated by a few brief examples which we shall give here. Thus, when Dalton did experiments leading to the atomic theory he did not dream that these were essential stepping stones to the synthesis of wonder drugs or the release of energy from uranium. Volta, with his primitive battery that made frogs' legs twitch, and Faraday, in his experiments with electric currents, were motivated by scientific curiosity and could not have imagined the role electricity would play in present-day civilization. If in 1890 a government has said, "Let's support research to find a better method of treating fractured bones," it is inconceivable that the programmatic research that might have been initiated could have produced the discovery of X-rays. This discovery occurred as a byproduct of basic research on the behavior of electrical discharges in evacuated tubes in Roentgen's laboratory in 1895. In addition to contributing an invaluable aid in the treatment of fractures, it triggered a chain of basic discoveries, starting with Becquerel's experiments to determine whether X-rays accompany the fluorescence of uranium, which led him to the discovery of radioactivity. This in turn led to intensive investigations of atomic structure culminating in the concept of the nuclear atom. Then came the first artificial transmutation of one element into another, the development of the cyclotron, the discovery of the neutron, and the discovery of artificial radioactivity. Up to this point in the chain there were few "practical" byproducts. In the late 1930's, Rutherford still said that tapping the energy stored in the nucleus, known to be vast in terms of the Einstein formula, $E=mc^2$, seemed completely impossible. But to the basic research man the goal was an understanding of nature, and dramatic discoveries continued. They led to the recognition of the fissioning of uranium by Hahn and Strassman and the subsequent demonstrations that this could produce a self-perpetuating nuclear-energy release. And so uranium, which had been used only as a tint for ceramics for the two centuries since its discovery, became an element of prime economic and military importance, with far-reaching implications in international politics.

In a quite different area, the importance of basic research in paving the way for practical applications is illustrated by three discoveries made within a period of two decades in one university biochemistry department. Each of these was made in the course of fundamental investigations and depended in turn on the prior contributions of many scientists to a basic understanding of organic chemistry, and of blood chemistry or nutritional chemistry. The first, the discovery that nicotinic-acid deficiency is a contributory cause of pellagra, provided the means of minimizing this disease, which had caused great suffering among poorer groups of the population existing primarily on cereal diets. The second, the discovery that ultraviolet irradiation of ergosterol produces vitamin D, has made it possible for dairies throughout the country to fortify their milk with vitamin D concentrate, with the result that rickets is now only a medical curiosity in the United States. The third, the synthesis of a dicoumarol derivative that prevents blood clotting has saved hundreds of millions of dollars through use of the product as an effective rat poison, and has prolonged lives through its use in the treatment of coronary thrombosis.

Many examples of the fruits of basic research in agriculture could be cited. A recent example of the enhancement of the value of agricultural products by research is the commercial process, using interfacial polymerization, for making nonshrinkable wool textiles by giving each fiber a coating of nylon a millionth of an inch thick.

These examples are illustrative of how practical applications, unpredictable in advance, grow from basic research.

It is of interest to note that, following their discovery in basic research laboratories, the methods of production of vitamin D and of the anticoagulant for blood were developed commercially to a point of national usefulness under patent protection. It is important that Government patent policies with respect to basic research conducted with Federal funds be such as to encourage commercial development of discoveries so that they will become useful to the public and of value to the economy.

Capitalizing on Manpower Resources through Education

Our country's most important natural resource is the ability of its citizens. As with other natural resources, the "mindpower" of each generation can be used efficiently or it can be wasted. The efficiency with which we use this resource determines the extent to which we can reap the benefits to be had from basic research, as it also determines our stature in other areas. More funds are needed to make use of the full capacity of our present scientists, but even with these we will fall short of our potential because of those people who have not been trained or motivated to the full extent of their abilities. Success in conserving our mindpower resources depends on the quality of our educational system. Continued Federal involvement in improving this quality is needed. In the paragraphs immediately below some features of this system will be considered briefly.

The pipeline that feeds our manpower pool of able research scientists, as well as able citizens in all other areas, starts in the elementary school. In good elementary schools, students have their natural curiosity cultivated and channeled productively. They are given a taste of the rewards of disciplined mental activity that leads them to want more. They are encouraged to ask "why" and "how," and to seek ways of testing the answers objectively. The development of each child to his full potentiality requires highly skilled and motivated teachers. Such teachers increase the quantity and quality of the flow in the manpower pipeline. Poor teachers lead to loss of able students.

At present, nearly a fifth of the elementary schoolteachers of the country have had less than 4 years of college training (*1*), and many of those with the bachelor's degree do not have adequate training in the specific subject matter of their teaching. The supply of newly trained elementary schoolteachers for the fall of 1964 is estimated to be 130,000, far short of the 188,000 required to replace those leaving and to serve the predicted increased enrollment (*1*). Still more (estimated at 60,000) (*1*) are needed to relieve overcrowding, to eliminate half-day sessions, to replace unprepared teachers, and to provide important types of instruction not now provided. Great improvements have been made in the last 15 years in the average level of formal training of elementary teachers, but the figures show that we are far from having an adequate number of qualified teachers at this level. Added incentives in terms of job opportunity, prestige, and salary are needed to attract more able people to elementary teaching.

Potential scientists usually have their first contact with specialized chemistry, physics, and biology courses in high school. Many high school science teachers have had far too little specialized training to give courses that will challenge the abilities of today's able students and instill interest in continuing in science. A survey by the National Science Foundation (*2*) shows that 66 percent of the high school physics classes in the country are taught by teachers with less than the 18 semester-hours of college physics considered to be the minimum necessary preparation, and 25 percent are taught by teachers with less than 9 semester-hours of course work in their subjects. A recent analysis of the problem (*3*), entitled "Education and Manpower in Physics," based on a statistical survey by the American Institute of Physics, states: "Any attempt to increase substantially either the quantity or quality of physics instruction in the high schools must recognize that: many high school physics teachers have had little formal training in physics; most of them spend only a minor part of their time teaching physics; and the annual production of college graduates certified to teach physics is pitifully small."

At fault are those schools of education that require too high a ratio of courses in pedagogy to courses in subject matter fields, and school boards whose formulas for salary raises discourage teachers from taking courses that would increase their subject matter competence. Such formulas specify that courses taken must be graduate courses leading to the master's degree. Most teachers of high school science do not have the prerequisites to take graduate courses in science, and so must take additional courses in education to meet the requirements of the salary formula. The appropriate Federal agency could exert important influence in improving the subject matter competence of high school science teachers by providing school boards with tables of recommended undergraduate science courses appropriate for teachers with various levels of previous training. The school boards would then have a formal basis for giving promotion and salary advancement to teachers for taking the courses most vital to improvement of their teaching.

The difficulties resulting from lack of specialized training of high school teachers are often compounded by the schedules required of them. Typical schedules allow little or no time for study and lesson preparation or the development of special projects for outstanding students. Most of the periods in the day are filled with assigned classes, often in two or three different subjects, following which the teacher is expected to supervise student extracurricular activities in his "free time" and grade papers at night. A teacher with these burdens can rarely demonstrate to his students the challenge and excitement of a scientific career or any other kind of career. The prospect of such burdensome schedules discourages able people from entering the teaching profession. These problems may be summarized by a quotation from a high school mathematics teacher (*4*). "The one crying need that seems to become more apparent in teaching year by year is time. * * *. It seems to me that the powers-that-be (probably through no fault of their own) have given us teachers a job to do—a difficult job if done conscientiously—and then have taken away all the time necessary to do the job."

Also important among the reasons why more qualified people are not attracted to high school science teaching, and why many who are do not remain, is that there are much better salary opportunities for scientifically trained people in areas other than teaching. Average starting salaries for technically trained new bachelor's degree graduates going into industry in 1963–64 were \$7,368 per year (\$614 per month) (*5*) while those for teachers were about \$5,000 per academic year (\$530 per month). The nationwide expansion of research programs has multiplied the opportunities for other types of scientific employment for those who are qualified for secondary school science teaching. The availability of more money to hire research workers in both industry and academic institutions weakens the competitive situation of schools in hiring science teachers. The solution to this problem is to improve both teaching schedules and salaries in the schools, so that the competition will be better balanced. Further, methods should be worked out for use of Federal support to provide greatly increased incentives for those with appropriate training to enter and remain in the teaching profession.

What more can Federal agencies do to improve the curricula and the caliber of teaching in elementary and secondary schools? Much is already being done. The National Science Foundation is at the forefront of this effort in its support (*6*) of the development of new curricula and texts by groups of concerned scientists. This effort has already resulted in substantial upgrading of the level of science and mathematics courses. Good teachers in the best high schools now give courses similar to good college courses of 10 years ago. This influence needs to be spread more broadly and, in particular, further efforts are needed to ensure the availability of teachers qualified to teach the new subject matter. Of major importance to the continued development of national leadership in all areas are the institutions that teach teachers. Added support should be given to the many 4-year institutions of this type that now have inadequate facilities and staff for training in scientific fields, provided they have the potential and desire to improve their subject matter offerings. Continued support of the National Science Foundation's summer and academic-year institutes for high school teachers should be assured.

Our discussion thus far has dealt with factors affecting the motivation of students prior to the age at which most of them have made firm career decisions. These decisions are usually made during the college years, and it is here that intensive training of future scientists really begins. The present-day rapid increase in scientific information demands reorganization of methods of presentation in order to bring the potential scientist to the research frontiers without unduly prolonging his training period. This is difficult because established patterns are hard to alter, but some progress has been made. New textbooks and new curricula have correlated subject matter formerly presented only in graduate schools so that it can be presented in undergraduate courses. The best high schools and the best colleges are evolving accelerated programs that challenge students to use their abilities more fully. For example, calculus is now given in many high schools. The changes in educational programs necessary to achieve increased efficiency of learning require a great deal of thought and effort from individual scientists and groups of scientists. Federal support has encouraged (7) and must continue to encourage such improvements, and the integration of improvements, from the elementary school to the graduate school.

It is estimated that less than 7 percent of American college and university students come from families in the lowest third of the economic scale. Lack of both finances and motivation account for this low percentage. It represents a loss from the manpower pipeline. This is illustrated by statistics for one institution (Berea College, Berea, Ky.) where all 1,350 students come from this segment of the population. The college gives preference to young people from the Southern Appalachian area; the median family income is $3,200 per year. Yet the record shows that 26 percent of all the alumni since 1922 have gone on after the baccalaureate degree to earn advanced degrees. Berea has been listed as one of the leading institutions with respect to the percentage of its graduates that later obtains the doctor's degree in five major fields. One hundred and thirty-two graduates earned Ph. D.'s in the period 1950–61. Such a record bespeaks the need for more effective means of assuring that able students of poor economic background are given the encouragement and opportunity necessary to develop their abilities.

The increase in opportunities for scientists in industry and in Government and academic research has made it difficult for small colleges to staff their science departments adequately. To minimize this problem, granting agencies should maintain a special category of grants to assist qualified college staff personnel in carrying on research in conjunction with their undergraduate teaching and during summers. The special category is needed to avoid shutting out the able small college investigator by competition with investigators at larger institutions, where a smaller fraction of each faculty member's time is devoted to undergraduate teaching. A considerable number of qualified scientists are attracted by the closer personal relationship of teacher and student that prevails in the small college, when adequate equipment and support are also available.

Graduate Education and Basic Research

A recent survey (*8*) shows that of the current basic research papers published in the field of chemistry, approximately 59 percent come from universities, 29 percent from industrial laboratories, 9 percent from Government laboratories, and 3 percent from research institutes. In several other fields the percentage of basic research done in the universities would be even higher. The universities are the major centers of basic research because they provide the type of intellectual atmosphere most favorable for individual creative effort in pure science. It is an atmosphere of objective, critical appraisal of all knowledge, and of searching for new knowledge through the best efforts of the human mind, with the aid of the most sophisticated instruments. It includes continual review of fundamental concepts as they are taught to students and continual testing of ideas in informal debate. It is here that a student fresh from undergraduate school chooses a research problem and, working on it in collaboration with his major professor, grows to the maturity of an independent investigator and contributes to the world's knowledge while so doing. And it is here that still more advanced research experience in specialized fields is given to selected postdoctoral students.

Traditionally and logically, Federal support of the education of scientists has been most extensive at the graduate-school level. At this level potential scientists have committed themselves to careers in science, are devoting their full efforts to science, and are clearly a national resource. The responsibility for their training is peculiarly a national rather than a local or State responsibility, since the research that they do usually has no specialized importance to the State where they do it, and since they seek permanent job opportunities on a national rather than State basis.

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At the graduate level, support for the education of scientists is usually also support for the conduct of research, since the graduate student is a research apprentice. Training of graduate students in some important areas is now threatened by lack of fellowships and research assistantships and by lack of space and equipment in our educational institutions.

Federal Support for Basic Research

The Committee on Science and Public Policy of the National Academy of Sciences, in its recent report entitled *Federal Support of Basic Research in Institutions of Higher Learning* (*9*), has examined the history and present organization of Federal support of basic research and analyzed current levels of support. The conclusion is clear that such support has led to American preeminence in science and its applications. Continuation of this support, not only in dollars but also in administration of the support in a manner conducive to the most creative scientific productivity, is essential. As we write there is clear evidence that the dollar level of support should be increased. Illustrative is the fact that the number of qualified applications received by the National Science Foundation during the last year from scientists requesting support has been much greater than the number that could be approved with the funds available.

This is shown in table 1. For fiscal year 1965 support for basic research by the National Science Foundation has fallen short of the total proposals from scientists by nearly $550 million. Less than 20 percent of the dollar value of the proposals could be funded. Many of the projects that could not be supported or were supported only partially were of outstanding merit. Failure to activate them has lessened our potential research stature. To quote testimony of Dr. Robertson, Associate Director of the National Science Foundation, before the House Appropriations Committee, "I would like to make it perfectly plain that we are not only giving no support to many proposals that we feel should be supported, but we are cutting back heavily on the projects we do support. We go well beyond squeezing out any water that may be in these proposals. We give topnotch people less support for their research than they should have in order to do the kind of job that we think they should do * * *. We must use all of our creative talent in science just as effectively as we can. We are pretty far from that goal."

TABLE 1.—*Comparison of research proposals received and grants made by the National Science Foundation* [1]

	Actual, fiscal year 1963	Estimate, fiscal year 1964	Estimate, fiscal year 1965
	A. BIOLOGICAL AND MEDICAL SCIENCES		
Value of proposals received	$126, 104, 000	$145, 000, 000	$165, 000, 000
Value of proposals supported	38, 394, 851	40, 500, 000	43, 500, 000
Value of proposals not supported	87, 709, 149	104, 500, 000	121, 500, 000
Percentage of proposal receipts granted support (based on dollar value)	30. 4	27. 9	26. 4
	B. MATHEMATICAL, PHYSICAL, AND ENGINEERING SCIENCES		
Value of proposals received	$271, 313, 744	$366, 000, 000	$469, 000, 000
Value of proposals supported	59, 895, 475	64, 000, 000	70, 700, 000
Value of proposals not supported	211, 418, 269	302, 000, 000	398, 300, 000
Percentage of proposal receipts granted support (based on dollar value)	22. 1	17. 5	15. 1
	C. SOCIAL SCIENCES		
Value of proposals received	$30, 212, 000	$35, 000, 000	$40, 000, 000
Value of proposals supported	8, 956, 172	9, 700, 000	10, 800, 000
Value of proposals not supported	21, 255, 828	25, 300, 000	29, 200, 000
Percentage of proposal receipts granted support (based on dollar value)	29. 6	27. 7	27. 0

[1] The data of the table are taken from the House of Representatives Appropriation Committee hearings for fiscal year 1965.

As with the National Science Foundation, other agencies are also finding that they must turn down good basic research proposals because of lack of funds. An ad hoc panel established in 1964 to obtain an expert and independent scientific review and evaluation of the off-site research supported by contracts with the Office of Chemistry Programs of the Atomic Energy Commission has made the following statement (*10*):

> It is the opinion of the Panel that every effort must be made to counteract the erosion of the Off-Site Chemistry Research Program. The rejection rate for sound and worthwhile new research proposals related to the Atomic Energy Commission program is alarmingly high; increased funds to make use of this large reservoir of chemical talent would be very much in the long-range interests of the Atomic Energy Commission and of the country. In the fiscal year 1964, 20 proposals from new investigators had to be rejected for every one that was accepted. A check of the referee reports indicated that approximately half of the rejected proposals were not only appropriate for AEC support, but had sufficient scientific merit to deserve it if funds had been available.

At another point in its report, the panel states:

> The number of young scientists who can be supported is far too small to insure the continuing development of the field of atomic energy. Furthermore, the total number of researches that can be supported with a fixed amount of money decreases with time because of the increasing costs of doing research. It would be highly desirable to be able to accept 40 to 50 percent of meritorious new proposals made to the AEC, rather than the much smaller fraction which is now the case.

The Air Force reports (*8*) that it has been able to support only 15 percent of those proposals submitted to it that were considered as excellent and that most of those that were not supported did not get support elsewhere.

These facts indicate that the talents of many able scientists are used only inefficiently because of lack of proper instrumentation, lack of space, and lack of technical assistance. It means that the exploration of many promising research ideas must be postponed or abandoned. It means that the research training of many graduate students is slowed down.

How much should Federal expenditures for basic research be increased? If we accept the fact that basic research is the goose that lays the golden eggs from which technological innovations grow, if we take cognizance of the currently unsupported research potential (e.g., see table 1), if we note that total Federal expenditures for basic research are only about 0.3 percent of the gross national product, it is clear that greater support is in the national interest. An off-the-cuff evaluation based on a feeling for the number of good proposals now undersupported, and for growth of the population of scientists in training in the next decade, would indicate that at least a doubling of support is needed within the next 5 years and at least a tripling within 10 years. Such increments seem modest when one notes that the estimated budget for 1964 for the National Aeronautics and Space Administration alone, for aspects of the space program other than research, was over $4.4 billion, while that for all federally supported basic research in all fields in colleges and universities proper was only about \$0.5 million (*11*). The 9-percent increase for basic research called for in the 1965 budget will not do the job that needs to be done. An annual increase of the order of 17 percent per year for the next 5 to 10 years seems to be more nearly the rate needed. One analysis (*12*) of the scientific manpower available during the period 1965–75 and the dollar support needed to train and utilize this manpower estimates a cost of \$65 billion for the 10-year period, exclusive of costs due to the increasing sophistication of research, to "big science," and to inflation. Considering the fraction of support that may be expected from State and private sources, it is concluded that the Federal expenditures needed for education, buildings, equipment, and personnel for college and university science and engineering in the 10-year period will be about \$30 billion, or an average of \$3 billion per year.

To provide a further factual basis for judgment as to the magnitude and type of need for basic research support in different sciences at this time, the Committee on Science and Public Policy of the National Academy of Sciences has asked a number of ad hoc committees to review several major scientific fields. The reports of these committees will provide detailed analyses of the most important areas of research, including information on their interrelation, present sources of support, and projected requirements. For example, the Committee for the Survey of Chemistry is seeking detailed information on research potential, areas of critical need, and sources of funding in chemistry. When the facts are assembled from university chemistry departments, from industry, and from Government laboratories, they will be presented with critical evaluation in the committee's report.

"Little Science" and "Big Science"

One of the conclusions to which the survey of the Chemistry Committee has already led is that new techniques and new types of instrumentation herald exciting new breakthroughs in chemistry, in the biological and health-related sciences to which chemistry is basic, and in the interrelated sciences such as geochemistry and chemical physics. These new tools—molecular-beam equipment, pulsed linear accelerators, electron-spin-resonance spectrometers, nuclear-magnetic-resonance spectrometers, ultraviolet and infrared spectrophotometers, mass spectrometers, and vapor-phase-chromatography equipment—are often costly (\$1,000–\$100,000) relative to other laboratory equipment. However, even the sum total of such instruments required for the work of all the basic research chemists and chemistry graduate students undergoing research training in all the colleges and universities of the country is no greater than the cost of certain single projects in the "big science" category. This is reflected in an excerpt from a summary of preliminary findings of the Chemistry Survey Committee:

> The total present investment of $55 million for major chemical apparatus for all the universities of the United States may be compared with $64 million estimated by the Federal Government for oceanographic ships in 1963 and 1964, or with the $40 million estimated cost of a single high-resolution radio telescope, or with the $114 million estimated for the construction of the Stanford linear accelerator. Although chemistry is one of the major sciences, although chemists constitute by far the largest group of physical scientists, although the great American chemical industry depends heavily on university research and training * * * the total investment in equipment for chemistry is still relatively modest. It follows that scientific advances through chemistry can be made with added investments which are small when compared with the potential gains.

The term "little science" is sometimes used to distinguish the work of individual investigators and their groups of graduate students from the "big science" (i.e., big dollar) projects such as high-energy accelerators, radiotelescopes, and space ventures. The former are responsible for the major portion of the country's basic research, while the latter are necessary to extend investigations in certain special fields. New ideas in research are the products of individual minds, often catalyzed by contacts with other minds, but still the product of the individual. Thus "little science" is responsible for the basic science necessary to construct the machines of "big science." It is also often responsible for generating the ideas necessary for putting these machines to their best use. It is essential in the national interest that support of "little science" not be slighted because of preoccupation with the dramatic needs of "big science."

The current critical financial needs of "little science" in university centers of basic research are of three types: (1) space for research and the teaching of graduate students, (2) stipends for research assistants, (3) the purchase of specialized research instruments. Only by meeting these needs can the national potential in basic research be realized. The cost of meeting them, though substantial, is small compared to the costs of even single pieces of many types of military and "big science" hardware.

Getting the Most Return for the Federal Research Dollar

The Congress of the United States has shown great wisdom in development of support of basic research in the universities through a variety of agencies, including the National Science Foundation, National Institutes of Health, Atomic Energy Commission, Department of Defense and others. The Federal agencies that administer funds for basic research in the universities are, in general, to be commended for the wisdom of their policies in terms of getting the most return per dollar expended. They have recognized that since basic research is exploring the unknown, specific results cannot be predicted or contracted for in advance. They have recognized that the best results are obtained by backing able men to the fullest possible extent, trusting their judgment, and minimizing time-consuming administrative redtape and paperwork. To assist in insuring that only good projects will be supported, the agencies have developed sound systems of review by referees and panels of experts. Most university research workers, like Congressmen, work far more than 40 hours a week and do not punch a timeclock while doing it. Most university administrations are scrupulous custodians of public funds. It is, therefore, recognized by most of the agencies that it is "pennywise, pound foolish" to apply time-consuming, irritating timeclock-type regulations on all because of the very few not worthy of trust. The latter may be handled more effectively by discontinuation of funds. Creative thinking is not a timeclock-type operation. Those who can do it can't escape from it; it goes on around the clock.

Conclusion

Continued leadership in basic research depends upon the capacity of our educational system to motivate and train youth who have potential scientific ability, and on adequate support for the research programs of well-qualified investigators. Success in the educational process requires very able teachers with depth of training in subject matter. Increased incentives are needed to attract able teachers to the teaching profession and retain them. Graduate education and basic research are complementary functions in university graduate schools, where the major portion of the basic research of the Nation is done. Laboratory space, research assistant stipends, and instruments are the major current needs requiring financial support. Indications of the extent of underused research capability is given by the number of high-quality research proposals for which national agencies do not have funds. Current evaluations of basic research potential being made by committees of the National Academy of Sciences will help further in evaluating dollar needs. The methods of administering basic research support must be designed to stimulate rather than inhibit creative thinking.

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APPENDIXES

Appendix A

DIMENSIONS OF FEDERAL INVOLVEMENT IN RESEARCH AND DEVELOPMENT

Nature of Research and Development Statistics

To obviate repetition of similar background material in each committee member's paper, this section outlines the Federal Government's role in the total research and development effort of the United States. This account is based mainly on information drawn from publications of the National Science Foundation. While the data are familiar to many, their nature and the limitations affecting their use are less well known. For that reason a brief delineation of problems involved in the collection and utilization of research and development statistics prefaces the summary description of research and development activities in the United States. These comments are intended only to preview the problems for the reader until he has had an opportunity to go over these fuller expositions developed by Dr. Harvey Brooks and other committee members in their individual papers. The aim is to provide the reader with a guide to understanding why material has been presented in certain ways, cautions to be observed in interpretation of these data, and appropriate uses of the available research and development data.

All economic statistics suffer from some limitations that do not inhibit their use. Foreign trade, industrial production, national income, and every other form of economic measurement involve successive approximations and constant refinement of concepts and methods. Important conceptual and practical problems in the collection of research and development statistics remain to be resolved. Some statistical estimates are known to be subject to significant margins of error. However, the quality of research and development data has improved substantially during the last few years, and there is every reason to hope that it will be further enhanced.

The relative newness of this statistical field should be taken into account. Five years ago, a specialist on the subject wrote: "Today, data on research and development funds and personnel are perhaps at the stage of growth in which national income data could be found in the late 1920's (*1*). Moreover, the great complexity of the task of securing relevant data should be appreciated. In the United States there are intricate patterns of relationships among the organizations that finance and perform research and development. Some organizations simply provide research funds to others; some do research and development work financed almost entirely by outside sources; others combine both functions to differing degrees; and some serve primarily to collect and redistribute research moneys. The organizations involved are diverse, for they include public governmental agencies, private business firms, educational institutions and other nonprofit organizations, and hybrids like the Federal contract research centers.

One problem is generated by the fact that the various organizations use different accounting periods. Many nongovernmental organizations keep their records by calendar year or on some other basis than the fiscal year uniformly observed by Federal agencies. This poses difficulties in reconciling data received from them and is responsible for the use—here also adopted—of hyphenated years (e.g., 1961–62, with the first referring to the calendar year and the second to the overlapping Federal fiscal year from which the data are taken) in connection with figures on total national expenditures for research and development.

Some Federal time series are maintained in terms of obligations, but the others are in terms of expenditures because certain kinds of information are more abundant on the one basis than on the other. "Obligations" represent the amounts for orders placed, contracts awarded, services received, and similar transactions during a given period, regardless of when the funds were appropriated and when future payment will be required. "Expenditures" represent the amounts for checks issued and cash payments made during a given period, regardless of when the funds were appropriated. The time that elapses between obligation and expenditure ranges from a few minutes for an over-the-counter purchase to several years for an item with a long leadtime. Hence, obligation and expenditure totals differ for any given year, but they are closely enough related that either can be used for the examination of broad trends in Federal financing of research and development. Particular caution is required in distinguishing between obligations and expenditures in programs that have been rapidly growing, such as the National Aeronautics and Space Administration program in the past 3 years. In such cases obligations may lead expenditures in a given year by as much as 20 or 30 percent.

For several reasons it is desirable to gather statistics on research and development activities from both the performers and the supporting agencies. Data reported by the performers of research and development frequently differ from those provided by the suppliers of funds. A performer may spend part of the funds in a different reporting period than the one in which they were provided. Performers and funding agencies may classify aspects of research and development projects differently.

For example, performers often classify activities as basic research which the supporting agency may consider to be applied.

The gravest problem, however, arises from the application of variable survey definitions by the large number of heterogeneous organizations being covered. Even where there is acceptance of recommended definitions of "basic research," "applied research," and "development"—which is far from universal among the respondents—identification of funds for basic research is often very difficult, especially for agencies with complex programs. And in defense work, where a great telescoping of development and production often occurs, demarcating the scope even of developmental activities is not always easy. Moreover, some large reporting units have accounting systems with only very general budget entries for research and development. Even companies and agencies that maintain detailed records often base them on research and developmen concepts that differ from those used in the surveys. In such cases, respondents must employ estimating procedures to produce survey data, with a wide range in the accuracy of their figures. Thus, all research and development statistics are frequently estimates of varying accuracy.

Particular difficulty results when an agency reclassifies certain research or development activities. A striking but uncommon example of this appears in the revised figures supplied by the National Aeronautics and Space Administration in successive annual reports to the National Science Foundation. For intramural basic research obligations the change for fiscal year 1962 was from $68.3 to $26.1 million and, for fiscal year 1963, from $105.0 to $41.9 million; the National Aeronautics and Space Administration also made changes of comparable magnitude in its classification of total research and basic research by fields of science. Such reinterpretations may lead to erroneous impressions of the extent to which research and development activities are actually changing in character.

Similarly, unless adjustments are made for fluctuations in the value of the dollar over the years, a time series of economic data may be misleading. The postwar period during which research and development expenditures grew so rapidly has experienced a sharp decline in the purchasing power of the dollar. Moreover, because of the rapid rise in salaries and increasingly expensive equipment involved, research and development costs have been subject to their own special kind of inflation. On the other hand, rising costs, particularly of equipment, are often more than offset by the greater research "productivity" per man-year effort. Reliable price "deflators" have yet to be devised.

Undoubtedly, many of these problems will be reduced in importance with time. However, the application of concepts as amorphous as basic and applied research to activities involving mixtures of all categories of research and development will always necessitate substantial reliance on subjective judgments. Research and development accounting and other control records maintained by respondents will continue to be shaped more by the requirements of tax codes, cost accounting, and organizational practices than by requests for survey data. And in any event, the dynamic nature of the research and development process would frustrate attempts to achieve complete comparability or consistency of data in statistical time series. At most, the hope is to make the guesses more "educated." An ineradicable element of imprecision will remain in research and development statistics, but this should not nullify their usefulness for policymaking purposes.

Research and development statistics do help illuminate the major patterns of relationships among the many performers and sponsors of research and development in the United States. Also, changes in these statistical relationships over a period of time can confirm, modify, or challenge impressions of trends derived from qualitative historical data. Thus, they can serve as useful tools for analysis and policy formulation.

The following paragraphs use research and development statistics to sketch in broad outline the dimensions of Federal involvement in the research and development activities of the whole country. Fiscal year 1962 is employed because that is the latest period for which completely unprocessed data on nongovernmental support of research and development are available. Unless there is an indication to the contrary, the figures cited are contemporaneous with those in the most recent issue of the National Science Foundation publication, *Federal Funds for Research, Development, and Other Scientific Activities*, vol. XII (NSF 64–11).

The National Scene

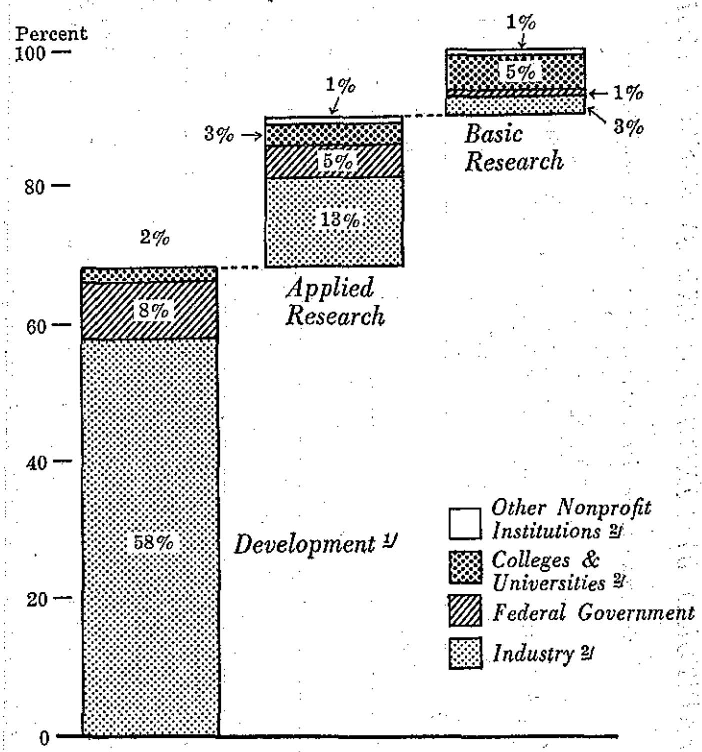
Two striking impressions emerge from examination of total research and development expenditures for the country as a whole. One is that the bulk of them is for development. The second is that a pattern of public finance and private performance has evolved in which the Federal Government supplies most of the money while private industry does most of the work.

Of the roughly $14.7 billion spent for research and development in the Nation during the year 1961–62, about 68 percent went for development, 22 percent for applied research, and only 10 percent for basic research. (See chart 1.) Approximately 65 percent of these funds came from the Federal Government; 32 percent from industry; and the remaining 3 percent from colleges and universities, which include State and local governments and other nonprofit institutions. Yet only

Chart 1

TOTAL R & D EXPENDITURES BY PERFORMER AND TYPE OF WORK, 1961-62

Total R & D Expenditures: $14.7 Billion



1/ *Other nonprofit institutions reported less than 1 percent.*

2/ *Includes funds for federal contract research centers.*

Source: National Science Foundation.

14 percent of this amount was spent by Federal agencies on doing research and development work themselves. Seventy-four percent for the performance of research and development by industry. Six percent was used by colleges and universities proper; 3 percent by Federal contract research centers operated by them, such as the Lincoln Laboratory at the Massachusetts Institute of Technology. And the remaining 3 percent was spent for research and development work done by other nonprofit institutions. (See chart 2.)

Chart 2

TOTAL R & D EXPENDITURES BY PERFORMER AND BY SOURCE, 1961-62

Research and Development Performers (Millions of Dollars)

Sources of Funds Used	Federal Government	Industry	Colleges & Universities: Proper 1/	Colleges & Universities: Fed'l Contr. Research Centers	Other Nonprofit Institutions	Total	Percent Distribution R & D Sources
Federal Government	$2,090	$6,310 2/	$600	$450	$200 2/	$9,650	65
Industry		4,560	55		90	4,705	32
Colleges and Universities 3/			230			230	2
Other Nonprofit Institutions 3/			65		90	155	1
Total	$2,090	$10,870 2/	$950	$450	$380 2/	$14,740	100
Percent Distribution, R & D Performance	14	74	6	3	3	100	

1/Includes agricultural experiment stations. 2/This amount includes funds from the federal government for research centers administered by organizations under contract with federal agencies. 3/Data include state and local government funds.

NOTE: All data are based on reports by the performers.

Source: National Science Foundation.

Breaking these figures down further by source of funds for each category of performer reveals that the Federal Government provided about 58 percent of the \$10.9 billion used by industry, and industry supplied the rest. Of the \$950 million spent by colleges and universities proper, 63 percent was from the Federal Government (or 75 percent of the \$1.4 billion total when affiliated Federal contract research centers are included), 24 percent from colleges and universities themselves, 6 percent from industry, and 7 percent from other sources And the Federal Government financed about 53 percent of the \$380 million expended by other nonprofit institutions, with the remainder coming in equal parts from industry and from other sources.

When basic research alone is considered, the picture is somewhat altered. Though the Federal Government remains the primary source of funds, colleges and universities displace industry as the principal performer. Of the approximately \$1.5 billion devoted to basic research in the year 1961–62, the Federal Government contributed about 57 percent; industry, 24 percent; colleges and universities, 12 percent; and other nonprofit institutions, 7 percent. Again, the Federal Government doing basic research intramurally used only 16 percent of this total. Colleges and universities proper spent 39 percent; Federal contract research centers operated by them spent 8 percent; industry spent 27 percent; and other nonprofit institutions spent 10 percent. (See chart 3.)

Of the \$583 million devoted to basic research by colleges and universities proper, about 57 percent came from the Federal Government (or 65 percent of the \$695 million total when affiliated Federal contract research centers are included), 31 percent from the colleges and universities themselves, 8 percent from other nonprofit institutions, and 4 percent from industry. The Federal Government contributed 22 percent of the \$403 million that industry used for basic research; and industry the remaining 78 percent. Other nonprofit institutions received about 53 percent of their funds for basic research from the Federal Government, 8 percent from industry, and the rest from other sources.

The above figures on total expenditures for research and development reflect trends that extend back many years. Although the growth curves of the three types of research and development have not proceeded exactly in parallel, the apportionment of funds among them has been remarkably stable, with development consistently getting about two-thirds and basic research around 10 percent of the total each year for

Chart 3

TOTAL EXPENDITURES FOR BASIC RESEARCH BY PERFORMER AND SOURCE, 1961-62

Research and Development Performers (Millions of Dollars)

			Colleges & Universities				
Sources of Funds Used	Federal Government	Industry	Proper 1/	Fed'l Contr. Research Centers	Other Nonprofit Institutions	Total	Percent Distribution Basic Research Sources
Federal Government	238	89 2/	330	112	80 2/	849	57
Industry		314	25		12	351	24
Colleges and Universities 3/			180			180	12
Other Nonprofit Institutions 3/			48		60	108	7
Total	238	403 2/	583	112	152 2/	1,488	100
Percent Distribution, Basic Research Performance	16	27	39	8	10	100	

1/Includes agricultural experiment stations. 2/This amount includes funds from the federal government for research centers administered by organizations under contract with federal agencies. 3/Data include state and local government funds.

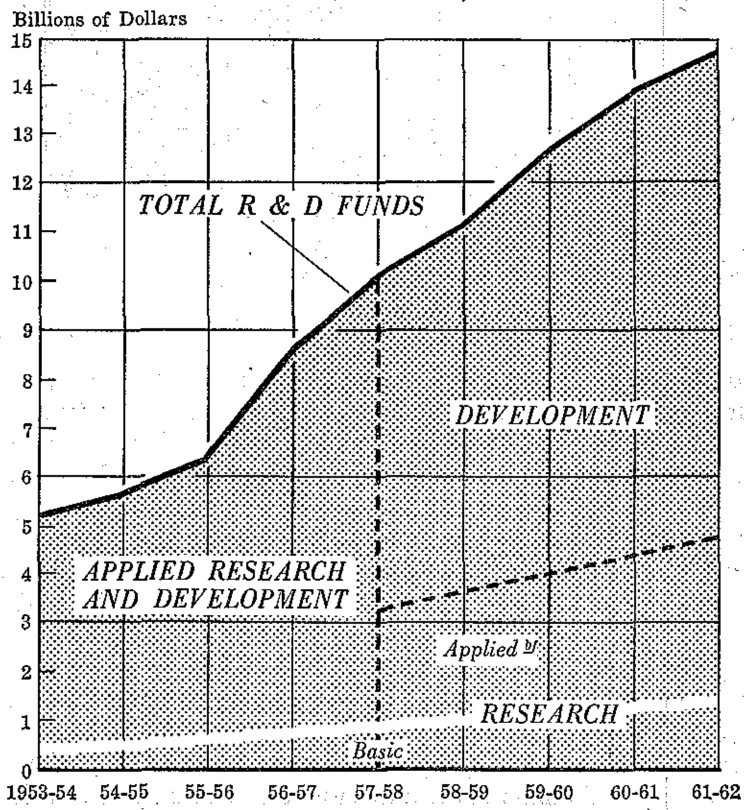
NOTE: All data are based on reports by the performers.

Source: National Science Foundation.

which detailed breakdowns are available. (See chart 4.) The distribution of funds among the different categories of performers has varied more, but even they have maintained the same rank order during the past decade or so. From 1953–54 to 1961–62, the funds used annually for research and development work by each class of performer increased as follows: for industry from $3.6 billion to $10.9 billion, for the Federal Government from $970 million to $2.1 billion, for colleges and universities (including the Federal contract research centers operated by them) from $450 million to $1.4 billion, and for other nonprofit institutions from $100 million to $380 million. Thus, industry has been, by a wide margin, the principal performer of research and development throughout this period. While the Federal Government has remained the second largest performer, its rate of growth has been the slowest. And the

Chart 4

TRENDS IN TOTAL EXPENDITURES FOR RESEARCH AND DEVELOPMENT, 1953-54—1961-62 [a]

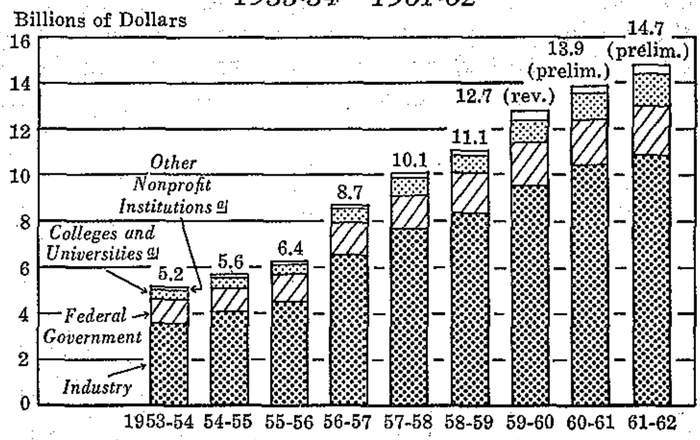


a/Data are based on reports by the performers.
b/Data separately identifiable for 1957-58, 1959-60, and 1961-62 only.
Source: National Science Foundation.

research and development expenditures by the smallest performer—other nonprofit institutions—have experienced the most rapid relative increase. During this same 10 years, the annual total of expenditures for research and development was approximately tripled, rising from $5.2 billion to $14.7 billion, and thus was approximately doubled in terms of constant value dollars. (See chart 5.)

Chart 5

TOTAL R & D EXPENDITURES BY PERFORMERS 1953-54—1961-62



a/ *Data include state and local government funds.*

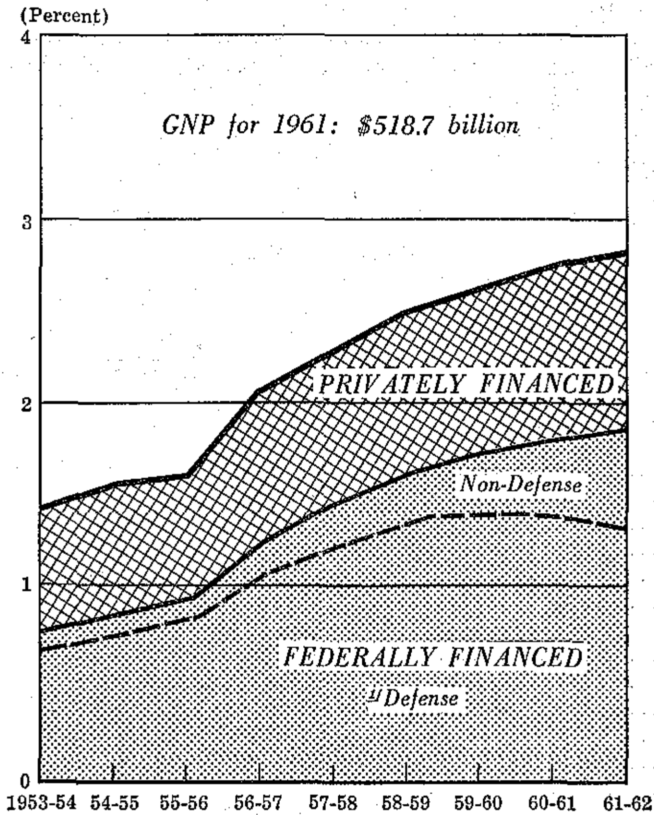
NOTE: Data are based on reports by performers. Amounts for federal contract research centers included in each category.

Source: National Science Foundation, 1963.

Another perspective for viewing this growth is obtained by expressing the sums as percentages of the gross national product. The fraction of the gross national product devoted to research and development has been increasing at a slower rate than the absolute expenditures. From 1953–54 to 1961–62, the amount the country as a whole was spending on research and development rose from approximately 1.4 percent to approximately 2.8 percent of the gross national product. The growth in this percentage has recently been leveling off. Moreover, in 1961–62, when an estimate of defense-oriented expenditures (defined as those of the Department of Defense and those of the Atomic Energy Commission for weapons and other military development) is subtracted, the total spent on all other research and development activities drops to around 1.5 percent of the gross national product. (See chart 6.) And the approximately $1.5 billion devoted to basic research that year constituted less than three-tenths of 1 percent of the gross national product, with the portion financed by the Federal Government less than two-tenths of 1 percent of the gross national product.

Chart 6

R & D EXPENDITURES AS A PERCENT OF THE GROSS NATIONAL PRODUCT, 1953-54 — 1961-62



1/Includes DOD and part of AEC.
NOTE: Data for the GNP refer to the earlier calendar year.
Source: R & D data, National Science Foundation. GNP data, U.S. Department of Commerce.

The Federal Focus

To develop the picture in greater detail requires concentrating on the Federal share of the national research and development bill. Expenditures for research and development by the Federal Government have grown apace with those of nongovernment institutions. From fiscal year 1953 to fiscal year 1962, the amounts the Federal Government spent yearly for research and development (including facilities, figures for which were not obtainable from private sources) rose from $3.1 to $10.4 billion, thus being somewhat more than tripled. Expressed as fractions of the Federal administrative budgets, this represents an increase from 4.2 to 11.8 percent of annual total expenditures by the Federal Government. According to estimates currently available, this percentage rises rapidly in fiscal year 1964 to 15.1 percent (largely due to the growing budget of the National Aeronautics and Space Administration) and levels off considerably in fiscal year 1965 at 14.6 percent of all the Federal expenditures then, or, in absolute terms, approximately $15.3 billion in fiscal 1965. (See table 1.) During this period, the bulk of these funds has been for defense and space programs, with the portion devoted to space activities growing the more rapidly of the two from fiscal year 1960 on. (See chart 7.)

TABLE 1.—*Federal expenditures, total and for R.&D. and R.&D. facilities, fiscal years 1940–65*

[Millions of dollars]

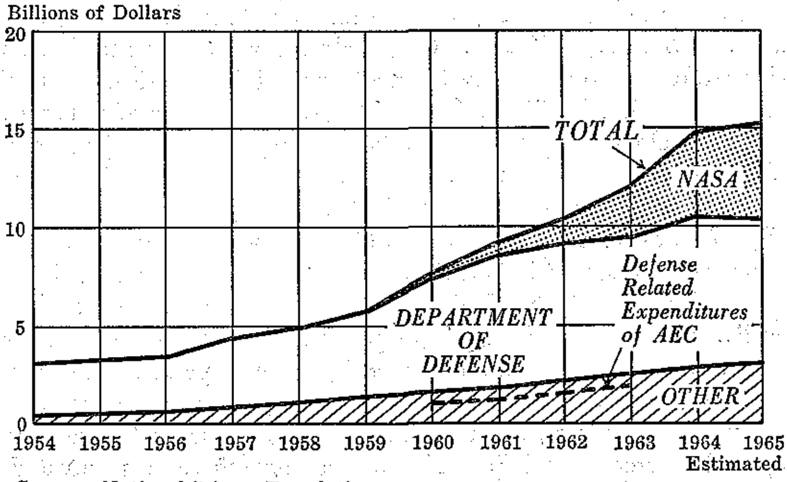
Fiscal year	(1) Total Federal expenditures [1]	(2) Total R.&D. and R.&D. facilities [2]	(3) Col. 2 as a percent of Col. 1
1940	9,055	74	0.8
1941	13,255	198	1.5
1942	34,037	280	.8
1943	79,368	602	.8
1944	94,986	1,377	1.4
1945	98,303	1,591	1.6
1946	60,326	918	1.5
1947	38,923	900	2.3
1948	32,955	855	2.6
1949	39,474	1,082	2.7
1950	39,544	1,083	2.7
1951	43,970	1,301	3.0
1952	65,303	1,816	2.8
1953	74,120	3,101	4.2
1954	67,537	3,148	4.7
1955	64,389	3,308	5.1
1956	66,224	3,446	5.2
1957	68,966	4,462	6.5
1958	71,369	4,990	7.0
1959	80,342	5,803	7.2
1960	76,539	7,738	10.1
1961	81,515	9,278	11.4
1962	87,787	10,373	11.8
1963	92,642	11,983	12.9
1964	98,405	14,883	15.1
1965	97,900	15,287	15.6

[1] Based on table 15, p. 454, the Budget of the U.S. Government, fiscal year ending June 30, 1965.

[2] Amounts for fiscal years 1940 through 1953 based on table XXXI, NSF 64–11, p. 52. Amounts for fiscal years 1954 through 1956 based on table H–1, p. 408, the Budget of the U.S. Government, fiscal year ending June 30, 1965.

Chart 7

FEDERAL ADMINISTRATIVE BUDGET EXPENDITURES FOR R & D, FISCAL YEARS 1954-65



Source: National Science Foundation.

As you might be expected then, a very few agencies dispense most of the research and development funds. Four of them—the Department of Defense, the National Aeronautics and Space Administration, the Atomic Energy Commission, and the Department of Health, Education, and Welfare—accounted for 95 percent of the $10.3 billion obligated by the whole Federal Government for research and development (excluding $778 million obligated for research and development facilities) in fiscal year 1962. Four others—the Department of Agriculture, the National Science Foundation, the Department of the Interior, and the Federal Aviation Agency—accounted for another 4 percent. And the remaining 1 percent was scattered among the other 21 agencies reporting obligations for research and development in fiscal year 1962.

The proportions in which these funds are obligated among the three types of research and development work vary widely from agency to agency. In fiscal year 1962, at one extreme was the Department of Defense with nearly 85 percent for development, 12 percent for applied research, and slightly less than 3 percent for basic research. At the other was the National Science Foundation with less than 1 percent for development and over 99 percent for basic research. Almost squarely in the middle was the National Aeronautics and Space Administration with 50 percent for development and 50 percent for research, and the research almost evenly divided between the two kinds (28 percent for applied and 22 percent for basic). The Atomic Energy Commission obligated 76 percent for development, 5 percent for applied research, and 19 percent for basic research. Both the Department of Health, Education, and

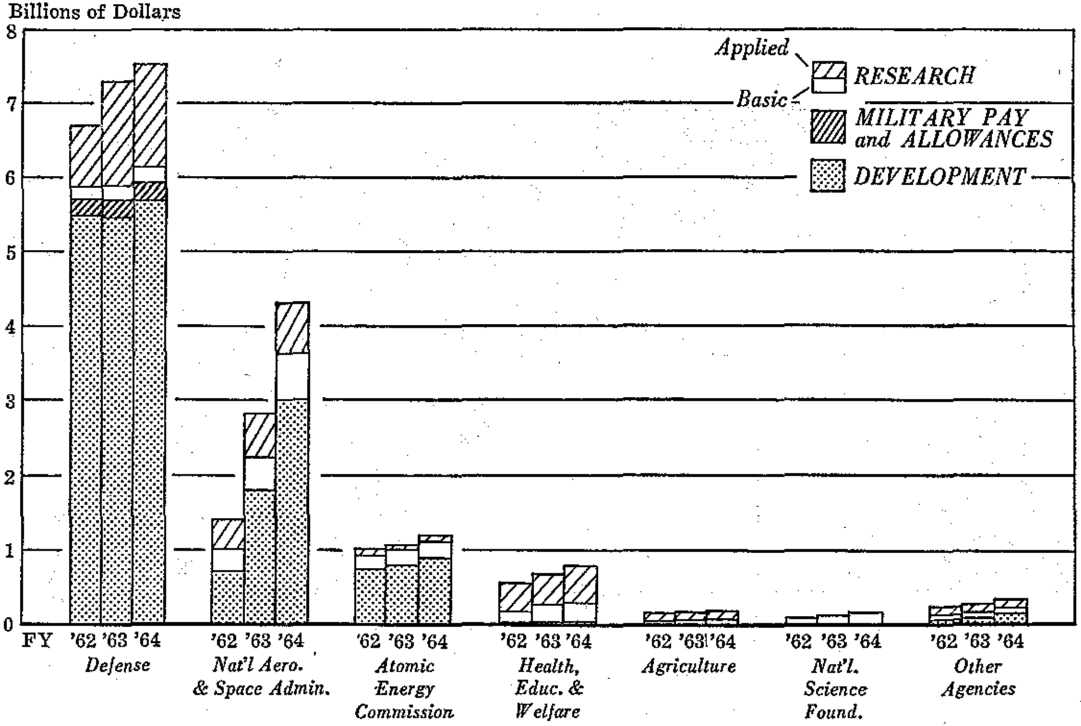
Welfare and the Department of Agriculture allotted over 95 percent to research, of which roughly two-thirds was applied and one-third basic, and only the very small residue was allotted to development (less than 5 percent for Agriculture and less than 1 percent for Health, Education, and Welfare.) (See chart 8.) However, when the total amount obligated for research and development by the Federal Government that year is analyzed, a familiar pattern emerges. Approximately 70 percent went for development, 19 percent for applied research, and 11 percent for basic research. (See chart 9.)

The distribution among performers is essentially the same for the Federal portion as for all the research and development funds spent in the Nation. Of the $10.3 billion they obligated for research and development in fiscal year 1962, Federal agencies received 22 percent intramurally. Profit organizations proper received 57 percent; Federal contract research centers administered by them, 4 percent; educational institutions proper, 8 percent; Federal contract research centers managed by them, 5 percent; all other performers (foreign as well as domestic) proper, 2.3 percent; and Federal contract research centers operated by other nonprofit institutions, 1.5 percent. (See chart 10.) For the $1.1 billion of these obligations devoted to basic research alone, Federal agencies used 18 percent intramurally; educational institutions proper, 35 percent; Federal contract research centers administered by them, 20 percent, organizations proper, 13 percent; Federal contract research centers managed by them, 3 percent; all other performers proper, 8 percent; and Federal contract research centers operated by other nonprofit institutions, 3 percent.

How Federal research and development funds are apportioned among different major fields of science can be indicated only for research, because such data are not available for development. Of the $3 billion obligated by Federal agencies for applied and basic research in fiscal year 1962, about 63 percent went for the physical sciences (engineering, mathematical, and physical sciences proper), 28 percent for life sciences (biological, medical, and agricultural), 2 percent for psychological sciences, 2 percent for social sciences, and 5 percent for other sciences (*2*). The $1.9 billion allotted to the physical sciences was divided into 52 percent for the physical sciences proper, including earth sciences, 44 percent for the engineering sciences, and 4 percent for the mathematical sciences. Of the $821 million obligated for the life sciences, 73 percent went to the medical sciences, 18.5 percent to the biological sciences, and 8.6 percent to the agricultural sciences. This pattern of distribution reflects trends that extend back several years and which available estimates indicate will continue in the immediate future. Since fiscal year 1959, approximately three-fifths or more of the Federal obligations for research each year has been for the physical sciences, the

Chart 8

FEDERAL OBLIGATIONS FOR R & D, BY SELECTED AGENCY AND TYPE OF WORK, FISCAL YEARS 1962, 1963 AND 1964 [1/]

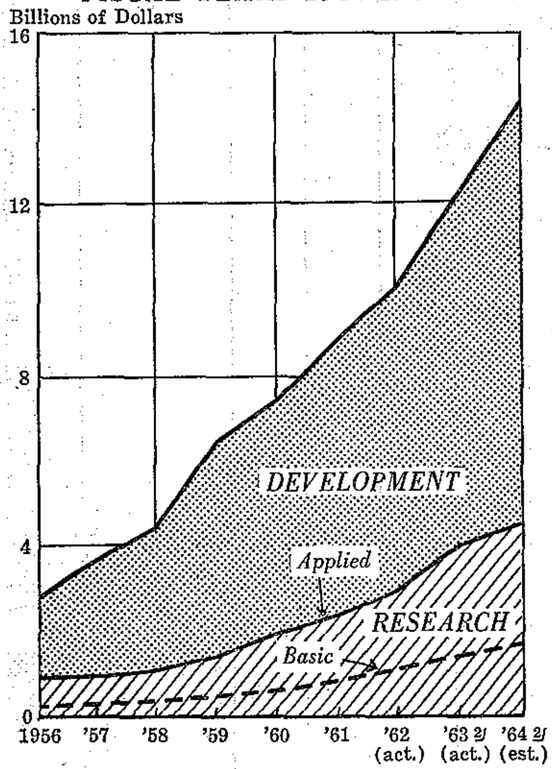


1/Figures for fiscal years 1963 and 1964 taken from unpublished data for Federal Funds for Research, Development, and other Scientific Activities, Vol. XIII.

Source: National Science Foundation.

Chart 9

FEDERAL OBLIGATIONS FOR RESEARCH AND DEVELOPMENT, FISCAL YEARS 1956-1964 [1]



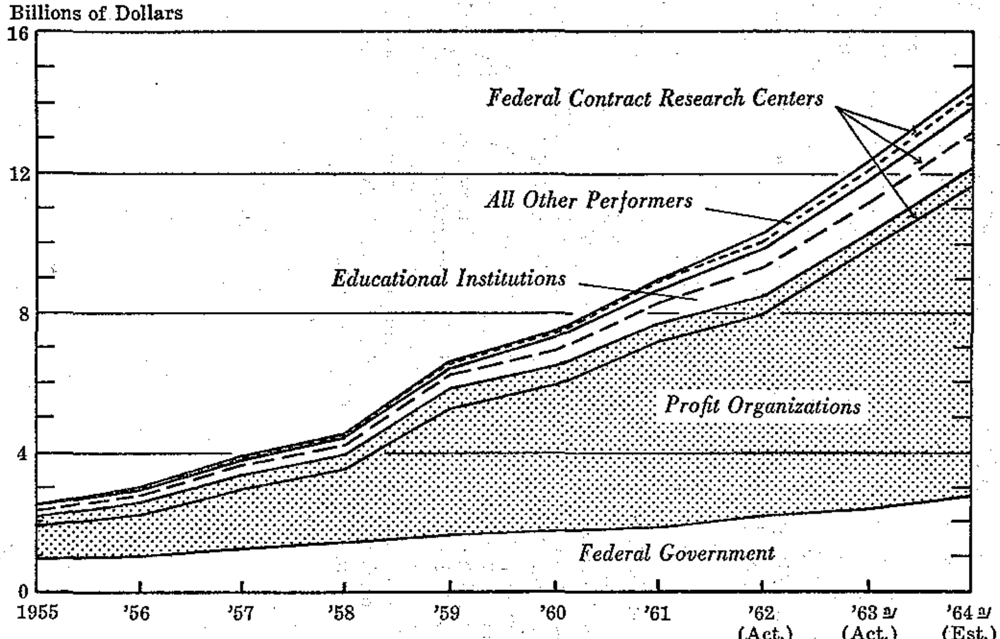
1/ *Excludes pay and allowances of military personnel in research and development*

2/ *Figures for fiscal years 1963 and 1964 taken from unpublished data for Federal Funds for Research, Development, and other Scientific Activities, Vol. XIII.*

Source: National Science Foundation.

Chart 10

FEDERAL OBLIGATIONS FOR R & D BY PERFORMER, FISCAL YEARS 1955-1964



a/ *Figures for fiscal years 1963 and 1964 taken from unpublished data for Federal Funds for Research, Development, and other Scientific Activities, Vol. XIII.*

Source: National Science Foundation.

largest portion of that being for the engineering sciences, and the next largest for earth (including space) sciences; and the remainder has been mostly for the life sciences, principally medical. (See chart 11.)

When basic research is considered alone, substantially the same situation obtains with respect to major fields of science. Of the $1.1 billion obligated by Federal agencies for basic research in fiscal year 1962, about 66 percent went for the physical sciences, 29 percent for the life sciences, 3 percent for psychological sciences, 2 percent for social sciences, and less than half of 1 percent for other sciences. However, there are differences in the distribution of support among disciplines within major fields. For instance, of the $721 million allocated to the physical sciences that year, 86 percent went to physical sciences proper, including earth and space, only 11 percent to engineering sciences, and 3 percent to mathematical sciences. (See chart 12.)

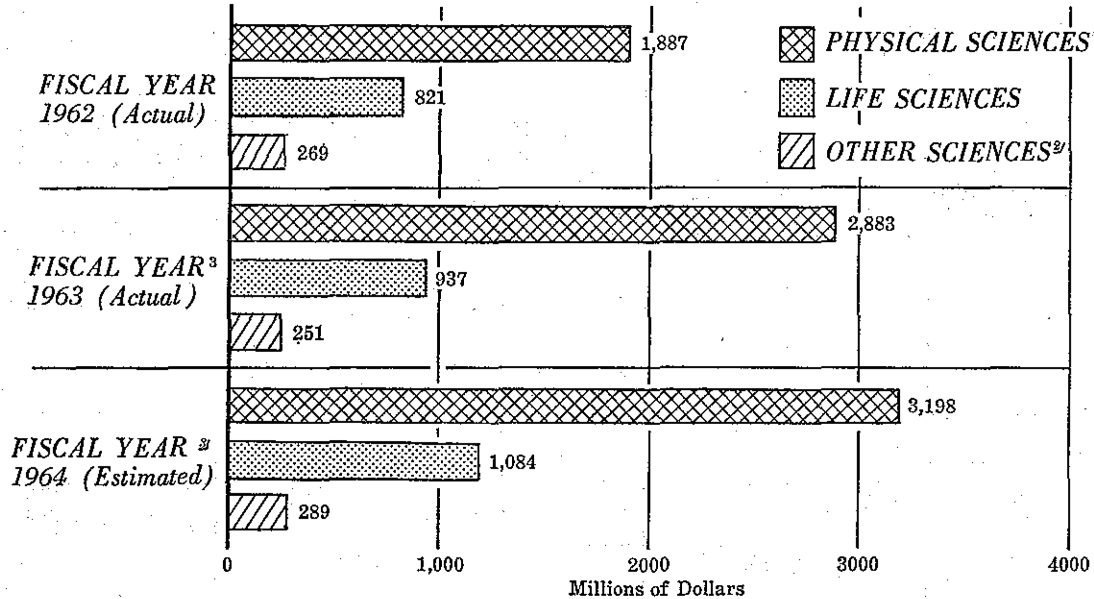
References

(*1*) Kathryn S. Arnow, "National Accounts on Research and Development—The National Science Foundation Experience," *Methodology of Statistics on Research and Development* (NSF 59–36), p. 61.

(2) These are the categories of major fields used by the NSF in *Federal Funds for Research, Development, and Other Scientific Activities,* vol. XII. "Physical Sciences Proper" comprehends astronomy, chemistry, earth sciences, physics, and other physical sciences proper. Discussion of this subject is greatly complicated by the fact that other analyses of these same data—including some presented by Government officials at congressional committee hearing—list several of these disciplines and all the constituent sciences in the "Physical Sciences" and "Life Sciences" categories as separate major fields. Obviously, how disciplines are aggregated into major fields affects generalizations about which is receiving the most Federal funds.

Chart 11

FEDERAL OBLIGATIONS FOR TOTAL RESEARCH BY MAJOR FIELDS OF SCIENCE [1] FISCAL YEARS 1962, 1963, AND 1964



1/Excludes pay and allowances of military personnel in research and development for fiscal 1962 but not 1963, 1964.

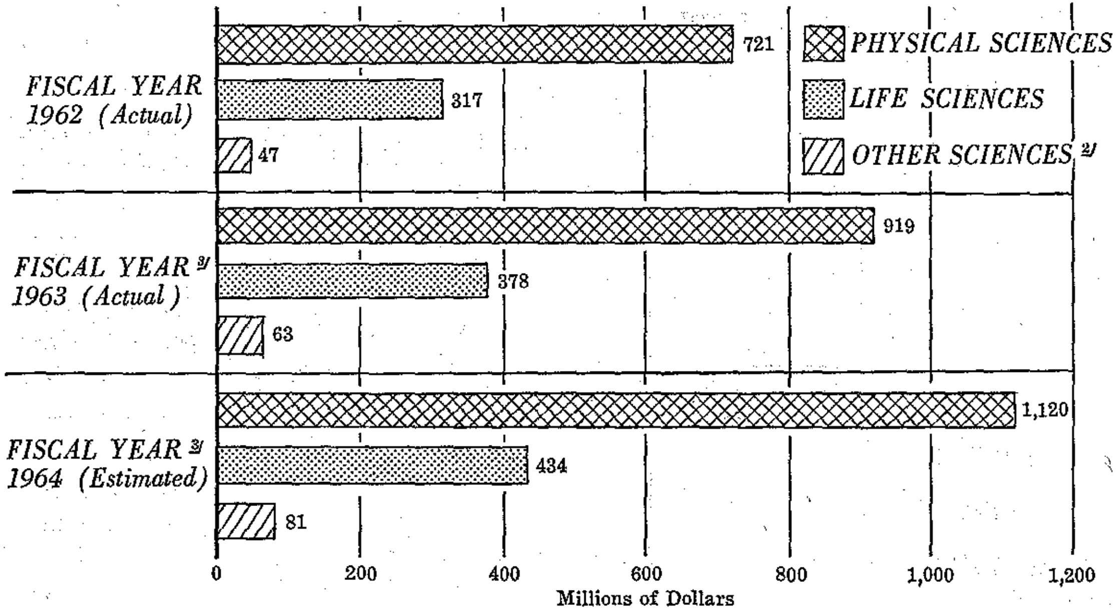
2/Includes psychological, social, and all other sciences.

3/Figures for fiscal years 1963 and 1964 taken from unpublished data for Federal Funds for Research, Development, and other Scientific Activities, Vol. XIII.

Source: National Science Foundation.

Chart 12

FEDERAL OBLIGATIONS FOR BASIC RESEARCH BY MAJOR FIELDS OF SCIENCE [1] FISCAL YEARS 1962, 1963, AND 1964



1/Excludes pay and allowances of military personnel in research and development.

2/Includes psychological, social, and all other sciences.

3/Figures for fiscal years 1963 and 1964 taken from unpublished data for Federal Funds for Research, Development, and other Scientific Activities, Vol. XIII.

Appendix B

RESEARCH AND DEVELOPMENT IN OTHER COUNTRIES

What is impressive about research and development in other countries of the western world—especially the more industrialized—is less the absolute level of effort by comparison with the United States than the determination to catch up and to organize government to deal effectively with science and technology.

That direct comparisons are not too significant is not surprising when one considers (1) that Europe lost much of its scientific and technical personnel to the United States during and after the war, (2) that her academic and political traditions have been, and often still are, essentially out of tune with modern needs for massive applications of science and technology, and (3) that Europe and Japan spent the first decade after the war in immediate tasks of economic recovery, while the United States was able to concentrate effort and resources on such things as nuclear weapons, rocket vehicles, and development of a brand new electronics industry.

What rather surprises the contemporary observer, therefore—and should interest responsible public quarters in the United States—is the deliberate, concerted effort to overcome these obstacles to rapid scientific and technological progress, in Europe generally and also, especially, in Japan, which has had the advantage of freedom from institutionalized tradition. Evidence of this effort is found (1) in the level of research and development in Britain, which approaches that of the United States, (2) in the determination of France to achieve nuclear independence, (3) in recent moves by the advanced European countries to pool activities in both space and industrial research and development, and (4) even in such a less-developed country as Greece, which is currently on the verge of creating a new technical institution—a sort of small Massachusetts Institute of Technology—as a means of modernizing its educational system and of improving its competitive position vis-à-vis the countries of the European Economic Community, with which it has recently become associated.

Scope of Efforts

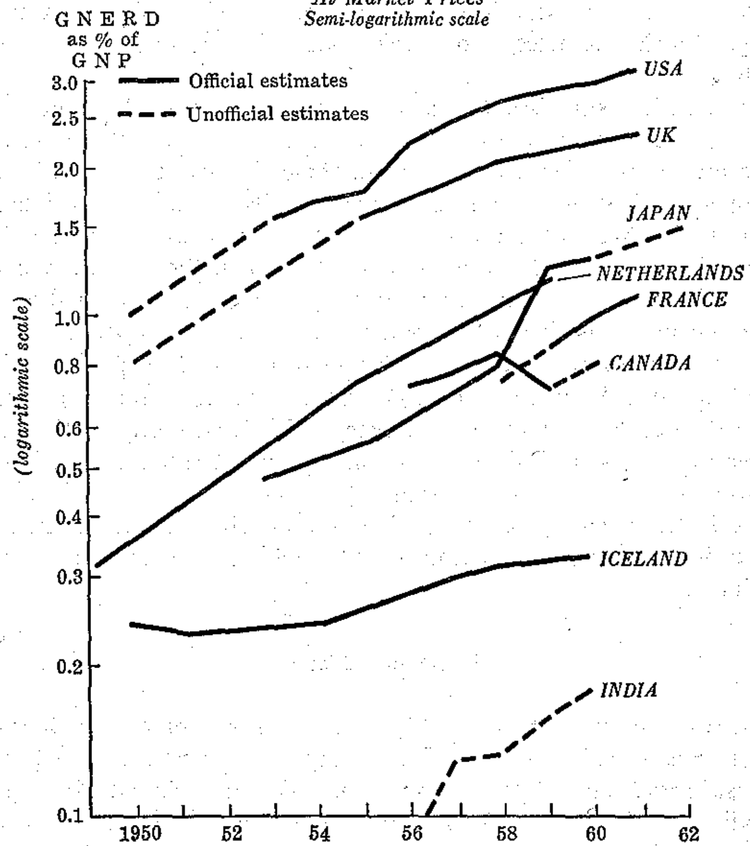
The relevant numbers, too, testify to the vigor of research and development activities in all countries. It is apparent that there has been a rapid increase in research and development expenditures worldwide since 1950. For all countries for which official or unofficial estimates have been obtained, this rise has been much more rapid than that of the gross national product over the same period. (See chart 13.) Although we have deliberately refrained from dealing specifically with the Soviet Union in this report because of the total lack of meaningful statistics, what evidence there is, and the testimony of all observers, shows that the Russian economy and society are very heavily based on science and technology in all their dimensions. And since the Soviet Union is a Communist structure, it is clear that all science and technology in that country are directly financed, controlled, and managed by the government. The only exception to the sharp upward trend of research and development activity is Canada, where the normal trend was temporarily interrupted by abrupt cancellation of military development work.

Chart 13

GROSS NATIONAL EXPENDITURE ON RESEARCH AND DEVELOPMENT AS A PERCENTAGE OF GROSS NATIONAL PRODUCT

At Market Prices

Semi-logarithmic scale



Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

This is the clear evidence of the available statistics, although it should be emphatically noted that research and development statistics, however inadequate they may still be deemed to be for the United States, are far more unreliable still for other countries (possibly with the exception of the United Kingdom), and virtually nonexistent for most of the less-industrialized ones. Moreover, even where foreign statistics do exist, they differ significantly in definition, scope, and methods of collection and analysis from American statistics, so that comparisons are hazardous.

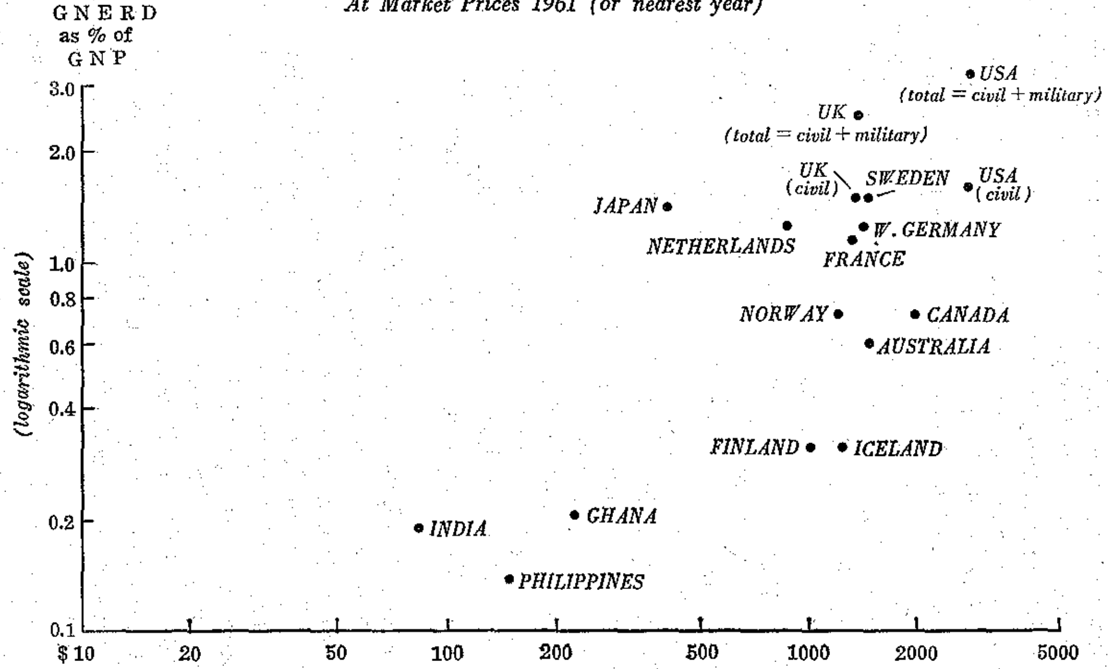
Some further general cautions should be observed in embarking on international comparisons of research and development effort. The ratio of research and development expenditures to gross national product ("research ratio"), for example, appears related to per capita gross national product. Countries with high per capita gross national products tend to have substantially higher research ratios than countries with low per capita gross national products. Advanced industrial countries typically spend more than 1 percent of their gross national products on research and development, while less-developed countries may spend less than 0.25 percent. (See chart 14.) But the research ratio is influenced further by the economic structure and military needs of a country. Australia, Canada, Norway, Finland, and Iceland have research ratios that are low in relation to their per capita gross national products because agriculture, forestry, mining, and fisheries—all of which have comparatively low research inputs—still make relatively large contributions to gross national product. In Canada and Australia, moreover, much of industry is foreign-owned, and relies heavily on research "imported" from parent firms in other countries, which is not included in the domestic statistics.

On the other hand, the research ratio of some industrialized countries is very high in relation to per capita gross national product. Japan, whose economy is growing at a remarkable pace, is making a great effort to develop the most modern possible science-based industries. The once great importance of "imported" research and development to the country's postwar industrial growth is declining, and Japan is now planning to increase its per capita gross national product by over 50 percent between 1960 and 1970, and its research ratio from just over 1 to 2 percent. In the United States and the United Kingdom, high research ratios clearly stem to a large degree from high levels of military research and development expenditure. Based on civilian research and development alone, these two countries do not have exceptionally high research ratios. (See chart 14 for rough estimates of their civilian as well as their total research ratios.) This observation must, however, be tempered by recalling that the research ratios of such countries as Canada and Sweden also include significant expenditures for military research and develop-

Chart 14

RESEARCH AND DEVELOPMENT EXPENDITURE IN RELATION TO PER CAPITA GNP

At Market Prices 1961 (or nearest year)



Per capita G.N.P. in US Dollars at year of R & D estimate.
(logarithmic scale)

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

ment, and that the figure for France has not been adjusted for the part of military research and development that seems to be included in it.

The reader should be reminded, finally, that research ratio is not an entirely reliable yardstick for international comparisons, because of difference in internal research costs among countries. "A systematic and detailed comparison of research activity in different countries would have to use much more refined techniques."

The quotation is from, *Science, Economic Growth, and Government Policy,* a report of the Organization for Economic Cooperation and Development published in 1963 (*1*). On the initiative of the OECD, its members have recently adopted a manual containing standard definitions and conventions of measurement as a basis for national surveys of research and development activities, and a projected Statistical Year on Research and Development, under the ægis of the same organization, should help further to increase both the quality and comparability of international research and development statistics (*2*). Full fruition of such efforts will of course take several years, and even then additional refinements will be necessary to secure reliable time series on research and development expenditures. Techniques will have to be devised, for example, to deal with national differences in research costs and the proportions in which they are combined. As in all economic statistics—foreign trade, industrial production, national income—the process of improvement in collection and analysis is never-ending. Yet the very prevalence of these efforts at improvement is additional evidence of widespread determination of all the countries of the Western world to put their research and development houses in order.

In the meantime, the reader should be warned that unofficial estimates have occasionally been used in this discussion, as noted in the accompanying charts and tables. Also, although a rough adjustment has been made for such major items as capital expenditures where necessary, it has not been possible to take full account of different definitions and survey practices. The statistics, finally, are in terms of current prices, because no reliable "deflators" have yet been devised for research costs, which almost certainly have risen more than general price levels.

Yet the rapidly rising trend of research and development activities, in all countries noted above, shows clearly even through the inadequate statistics. In all these countries, moreover, Government provides a large part of the funds for research and development. The share of the total national research and development effort financed by Government in 1961 ranged from over three-quarters in France to slightly under a third in the Netherlands, the average being close to three-fifths. (See table 2.) The relatively large sums provided by the Governments of France, the United Kingdom, and the United States are necessitated mainly by their military, space, and nuclear research programs. The almost

TABLE 2.—*Funds for research and development by source and by performer in various countries*

[In percentages of total]

Country	Year	General Government sector	Business enterprise sector (manufacturing industry shown in parentheses)	Nonprofit sector including higher education	Total
A. Performers of R. & D.:					
Japan	1959	14	63 (56)	22	100
United States	1961	15	75 [5] (73)	10	100
Netherlands	1959	[1] 20	64 (62)	15	100
United Kingdom	1961	28	63 (59)	9	100
France	1961	[2] 32	57 (51)	11	100
Canada	1959	48	39 (35)	13	100
Philippines	1959	65	35 (27)	nil	100
Australia	1960	68	20 (..)	12	100
B. Sources of finance for R. & D.:					
France	1961	78	22	[3] nil	100
United States	1961	66	32	2	100
Finland	1956	62	38	[3] nil	100
United Kingdom	1961	61	37	2	100
Canada	1959	61	31	8	100
Norway	1960	[4] 51	42	7	100
Japan	1959	36	64	[3] nil	100
Netherlands	1959	30	63	7	100

1. Assuming that "other research institutes" are 70 percent in the Government sector and 30 percent in the nonprofit sector.
2. Assuming that 40 percent of C.N.R.S. grants are to higher education.
3. Excluding small contributions from the nonprofit sector.
4. This includes 41 percent direct Government grants, and 10 percent collected through the Norwegian system of using part of the profits from football pools to finance research.
5. A part of communications research is included in that of manufacturing.

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

NOTE.—The sector definitions are based on standard national income definitions, that is, publicly owned enterprises or industries are included in the business enterprise sector.

equally heavy contributions, proportionally, by the Governments of Finland and Canada can be attributed to the fact that these two countries have less manufacturing industry than the first three, and to their need to develop mining and other natural resources. In Japan and the Netherlands, where there are no large-scale defense, nuclear, or space research programs, and where manufacturing industry is well developed, the Govenment still contributes about one-third of total national research and development expenditures.

In France and the United Kingdom, as in the United States, the bulk of governmental research and development funds are distributed by a very few agencies, mainly those concerned with defense, nuclear, and space research, although significantly also by such organizations as the Department of Scientific and Industrial Research (D.S.I.R.) in the United Kingdom and the Centre National de la Recherche Scientifique (C.N.R.S.) in France—agencies that are vaguely analogous to the United States National Science Foundation. In all the European countries, moreover, often more than 90 percent of basic research in the universities is more or less directly Government-financed.

Although defense and nuclear programs account for almost half of the Federal research and development expenditures in Canada, a relatively large share is handled by the ministries responsible for agriculture, fisheries, mining, and other natural resources. (See tables 3–7.) In the Netherlands and in many of the smaller European countries, well over half of the governmental research and development funds are disbursed by ministries responsible for education and university research.

The proportion of research and development funds spent intramurally by Government agencies, i.e., within Government research establishments, varies widely from country to country. (See tables 2–7.) In France and the United Kingdom, as in the United States, the Government contracts out much research and development work to private industry. Government contracts—mostly for defense, nuclear, and space research—account for between 45 and 60 percent of the total funds used for research and development by industry in these three countries. The Governments of Canada, the Netherlands, and Japan contribute much less to industry in the form of research and development contracts. And in Canada, the Government spends nearly half of the total research and development funds (public and private) intramurally.

As already indicated, the proportion of funds for basic research supplied by the Government is much higher in most European countries than in the United States, although overall national investment in basic research in most industrially advanced countries represents a comparable proportion of gross national product.

TABLE 3.—*Expenditure on research by some government agencies in various countries (current prices)*

United States, including capital expenditure (dollars in millions)	1947–48	1956–57	1959–60	1960–61	1961–62	1962–63 estimates
All agencies	900	3,446	5,803	7,738	9,278	10,195
Department of Defense	551	1,639	4,183	5,654	6,618	6,504
National Aeronautics and Space Administration [1]	35	71	146	401	742	1,287
Atomic Energy Commission	38	474	877	986	1,111	1,315
Department of Health, Education, and Welfare	10	86	253	324	374	558
National Science Foundation	......	15	51	58	77	116
Department of Agriculture	39	88	125	131	148	160
Department of Interior	20	36	72	65	75	88
Federal Aviation Agency	......	......	28	41	53	57
Department of Commerce	5	20	30	33	36	72
All other agencies	[2] 20	77	39	44	46	63

U.K., including capital expenditure (pounds in millions)	1947–48	1956–57	1959–60	1960–61	1961–62	1962–63
					Estimates	
Atomic Energy Authority (civil)	......	......	......	......	49.0	50.0
National Institute for Research in Nuclear Science	......	......	......	5.2	5.1	6.7
Department of Scientific and Industrial Research	3.1	......	13.6	16.0	18.1	22.1
Medical Research Council	.8	......	3.1	4.5	5.5	5.8
University Grants Committee	......	......	18.0	19.2	21.2	23.7
Ministry of Agriculture—Scottish Department of Agriculture	......	......	3.3	3.7	[3] 4.8	[3] 4.6
Agricultural Research Council	.9	......	4.6	5.6	6.1	6.5
Ministry of Aviation (civil)	......	......	......	16.8	19.9	25.5

France (new francs in millions)	1947–48	1956–57	1959–60	1960–61	1961–62	1962–63
CNRS	......	57.5	105.6	142.3	176.6	......
University laboratories	......	7.7	16.4	29.5	44.4	......
National Institute for Public Health	......	2.8	5.2	9.4	11.8	......
National Institute for Agricultural Research	......	9.0	22.3	29.0	37.6	......

[1] National Advisory Committee for Aeronautics before 1958.
[2] Including Manhattan District Project (Atomic Energy).
[3] Including Agricultural Advisory Service.

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

TABLE 4.—*Canada. Total Federal Government funds for research and development, 1959–60*

[Thousands of Canadian dollars]

Field of competence of department or agency	Total expenditure	Percentage of total Government funds for R. & D.	Percentage of funds spent intramurally
A. Agriculture and fisheries	37, 833	17. 9	99. 9
C. Defense	67, 420	31. 7	70. 0
D. Health and welfare	4, 310	2. 0	38. 4
E. National research council	32, 824	15. 5	76. 8
F. Northern affairs and natural resources	6, 928	3. 3	84. 4
F. Mines and technical surveys	27, 684	13. 0	100. 0
G. Nuclear science	32, 780	15. 4	92. 0
K. Other	2, 537	1. 2	55. 4
All agencies	212, 316	100. 0	83. 2

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

TABLE 5.—*France. Total Government funds for research and development, 1961*

[Millions of French francs]

Field of competence of ministry or agency	Total expenditure	Percentage of total funds for R. & D.	Percentage of funds spent intramurally
A. Agriculture	51	1. 9	....
B. Civil Aviation	88	3. 3	....
C. Defense	1, 477	55. 4	....
D. Health	13	. 5	....
E. Higher education and CNRS	512	19. 3	....
F. Telecommunications	40	1. 5	....
G. Nuclear Energy	381	14. 3	....
H. Overseas research	39	1. 5	....
K. Other	60	2. 2	....
All agencies	2, 676	100. 0	55. 4

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

TABLE 6.—*United Kingdom. Total Government funds for research and development, 1961–62*
[Millions of pounds]

Field of competence of ministry or agency	Total expenditure	Percentage of total Government funds for R. & D.	Percentage of funds spent intramurally
A. Agriculture, forestry, fisheries, and food	12. 7	3. 3	....
B. Civil Aviation	19. 9	5. 2	....
C. Defense	245. 7	63. 8	35. 0
D. Health and medical	6. 3	1. 6	....
E. Higher education	23. 3	6. 1	0
F. Industry and Communications	19. 2	5. 0	....
G. Nuclear science	54. 1	14. 0	....
H. Overseas research	1. 5	. 4	....
K. Other	1. 8	. 5	....
All agencies	384. 5	100. 0	42. 4

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

TABLE 7.—*United States. Federal Government funds for research and development, 1961–62*
[Millions of dollars]

Field of competence of ministry or agency	Total expenditure	Percentage of total Government funds for R. & D.	Percentage of funds spent intramurally
A. Agriculture	148	1. 6	71
B. Civil aviation	53	. 6	35
C. Defense	6, 618	71. 4	21
D. Health and welfare	451	4. 8	18
E. Education and NSF	....	....	....
F. Commerce and resources	111	1. 2	82
G. Nuclear energy	1, 111	12. 0	2
J. Space	742	8. 0	19
K. Other	46	. 5	72
All agencies	9, 278	100. 0	20

Source: Science, Economic Growth and Government Policy, OECD, Paris, 1963.

Science Policy Mechanisms

More significant even than what comes through essentially primitive statistics is the recent rapidly increasing concern in Europe and Japan with "science policy"—i.e., with problems of Government organization and procedures aimed at (1) insuring the health and growth of science and technology, and (2) securing for Government activities and policies in every field the advantages inherent in the knowledge and power that progress in science and technology imply.

Until a very few years ago, Government financing of research and development was part and parcel of the ordinary budgetary process. Of late, special institutional arrangements are taking shape in many countries to formulate national science policies and to provide guidelines for the allocation of scientific resources in both money and trained manpower. Potentially an even more important objective of such mechanisms is to insure that adequate attention is given to the contributions science can make to foreign policy, and to dealing with national problems in the military, economic, social, and other spheres of Government concern. This development has resulted from realization (1) that there will henceforth be a continuing need for a high level of public support for science, (2) that the extent to which certain programs can be carried out will depend as much or more on availability of trained manpower as on adequate funds, and (3) that science affects, directly or indirectly, every area of national policymaking. Dramatically underscoring this last point in particular, Sputnik I gave added impetus to efforts along these lines, not only in the United States, but in the Western world generally.

As might be expected, the specifics of the science-policy organs in Belgium, France, the United Kingdom, and the United States differ rather widely with differences in institutional and cultural environment, particular needs, scientific traditions, policy objectives, and resources in each country. The Belgian and French structures are more centralized in organization and systematic in some procedures; e.g., in segregating the scientific components of agency budgets into a separate science "package" for special consideration. However, all approaches reveal essentially the same elements in one form or another: an official responsible for science policy high in the administrative structure supported by a small full-time staff and by a variety of governmental and scientific advisory groups. And despite the apparently more theoretical approach followed by some, all represent essentially pragmatic adaptations to a succession of practical situations.

One major difference should be noted between the United States and many other governments in dealing with matters relating to research and development. In parliamentary systems of government, legislatures vote on the national budget as a whole, with allocations to and within science and technology decided entirely by the executive departments. In the United States, of course, the Congress makes a line-by-line examination of the budget, which gives it a much more direct say on the details of science policymaking.

A more detailed description at this point of the formal structures and procedures in different countries would not contribute much to meaningful comparisons between the United States and other countries. Considerable explanation of the total institutional setting in which each functions would be required to appreciate the significance of their differences. Moreover, description is static, and these arrangements are peculiarly dynamic because sensitive to the constantly changing character of science and technology. A good case in point is the major reorganization currently underway in British civil science in response to recommendations of the recent Robinson and Trend reports. Most importantly, there is generally a gap between intentions and achievements, so that information about how these institutions actually work must be continuing. An interim committee of high-level science-policy officials set up as an aftermath of the OECD's October 1963 ministerial meeting has accordingly been charged with a continuing investigation of the development of science-policy machinery and activities in the 21 member countries of that organization.

Virtually all Western countries are now asking explicit questions about science with increasing insistence. These questions reveal an awareness of the great and growing impact of science and technology on all the policies of modern nations that must surprise the observer who has heretofore believed that the United States has been alone in pushing science for the national welfare. Given such fertile ground, it is unlikely that other nations will long lag behind this country in exploiting their research and development resources to the full.

References

(*1*) The Organization for Economic Cooperation and Development (OECD), which came into being on September 30, 1961, is the successor organization to the Organization for European Economic Cooperation (OEEC). The legal personality possessed by the OEEC continues in the OECD, but the membership has been broadened to include all the major industrialized countries of the world except Russia. The present members are: Austria, Belgium, Canada, Denmark, France, the Federal Republic of Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, Turkey, the United Kingdom, and the United States. Yugoslavia is an associate member.

(*2*) See OECD, *Proposed Standard Practice for Surveys of Research and Development*, Report DAS/PD/62.47 (3d Revision), Paris, 1964.

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