Federal Support of Science

What is the role of science in achieving national goals? On what basis is its federal support justified?

Alan T. Waterman

Concern regarding federal support of science is evident from many sources—the scientific community, advocates of special programs, individual agencies, the executive and legislative branches of government, and spokesmen for the general public. Among the latter three groups, much of this concern centers around the current magnitude of the national research and development budget, the predominance of federal funding, and especially the increasingly large proportion of uncommitted federal appropriations assigned to this item.

The question is: How may the growing magnitude of this effort be justified and reconciled with other increasingly critical needs?

Of primary interest to the scientific community is the support of basic research and its justification. On the one hand it is claimed that, during the remarkable boom that science has enjoyed since World War II, in which scientists have been practically given *carte blanche*, there has been inadequate justification in terms of defined objectives, coordinated planning, and identified achievements.

On the other hand, members of the scientific community are practically unanimous about the need for increased support of basic research. They point out the urgency for favorable competition in world science, the need for national investment in scientific knowledge as the basis for advancing technology, and the requirement of basic research for the advanced training of scientists and engineers.

For applied research and development, the basis for justification is clear and understandable—to accomplish some identified, practically useful purpose. Here decision-making, while often complex and difficult, is in general understood. It requires the attention of many kinds of experts as well as of scientists and engineers. In the area of basic research, however, where the general objective is progress on the frontiers of science itself, planning and justification should originate among research scientists alone. By the same token, it is the responsibility of the scientific community to present these plans and their justification to budget and administrative authorities and to the general public in cogent and constructive terms.

Key Questions and Points of View

For basic research, the key questions are: (i) What is the optimum level of support? (ii) What should be the basis for its distribution? (iii) On what grounds should this support be justified?

Progress toward answering these questions may be aided by the following oversimplified formulation of competing viewpoints involved in the argument:

1) Competent scientists should have opportunity, from private or public funds, to pursue the research which they choose as individuals or as constituted groups.

This states the time-honored academic point of view. It has served science well; even now few scientists would deny its validity, in the abstract at least.

To the layman, however, this principle seems to be a case of special pleading, and also to be highly uncoordinated. Actually it is coordinated in the following way. Since a scientist's standing depends upon the originality of his work, he must keep in close touch with on-going research in his field and avoid undesirable duplication. Since all scientists are in the same situation, they must communicate with one another; thus a built-in coordination and selectivity are achieved.

But to be realistic, under this rubric the degree of competence required to qualify for support must be considered and appraised. In addition, as an abstract principle, how is it to be defended for support with public funds?

2) Support should be confined to that research which has promise of contributing to the solution of practical problems.

This principle is understandable and widely applied. It constitutes the primary justification for the support of basic research by industry, by agencies with practical missions, and in appropriate contexts by others. It lends itself readily to establishment of priorities.

Occasionally the question is asked: How can basic research, which by definition has no practical objective, contribute to the solution of a practical problem? The answer is simple —it does so by furnishing a clearer understanding of the phenomena underlying the practical goal in view.

But this principle limits support to areas of science that have obvious or foreseen practical application. How can it provide for such epoch-making discoveries as x-rays, radioactivity, antibiotics, or quasars if one does not know of their existence? By itself, is this not too restricted and too shortsighted a justification?

3) Support should be given to basic research in the interest of progress in science, but it should be allocated according to a predetermined system of subject-matter priorities.

This is a logical and inviting thesis for providing a reasonable answer to those who grant the importance of science but demand an intelligible rationale by way of justification.

However, attempts to formulate a comprehensive plan for research attention and distribution of support along this line have invariably encountered a deep-seated reluctance on the part of the participating scientists toward making qualitative judgments on the relative scientific importance of the various subject-matter areas. The most that may be expected is an exposition of the current scope and promise of research in these areas and a summary statement of the comparative needs of each in order to make

The author is special consultant to the president of the National Academy of Sciences. He was formerly director of the National Science Foundation and president of the AAAS.

constructive progress. But few scientists would have the temerity to suggest that one branch of science is intrinsically more important than another. This attitude is strengthened by the knowledge that outstanding discoveries may occur in any branch of science, that the relative progress and activity in different areas are continually changing, and that no one is wise enough to foresee these effects with any degree of confidence. Consequently, from the standpoint of science alone the conclusions drawn cannot be expected to constitute a blueprint for the allocation of basic research funds on any basis other than current needs for optimum effort on the part of all, together with identification and appraisal of areas where marked progress is currently taking place.

Support of "Free" versus "Directed" Basic Research

At this point, one realizes that in presenting their case scientists must also take into account the necessity for justifying support from public funds, and include some reference to the expected significance of the basic research. However, in giving a higher priority to basic research because of its presumed practical application, one runs two risks: (i) the practical justification may be exaggerated or turn out to be unwarranted—the idea may prove not to be practically feasible; (ii) the effect of a shortage of funds is to retain support for such "directed" basic research at the expense of the "free" variety. Thus, too much emphasis upon practical justification for individual projects runs the danger of a cumulative trend toward support of basic research projects solely for practical reasons in each case, and the disappearance of support for basic research in fields whose practical significance appears more remote or completely lacking. Moreover, to limit support only to that basic research which is judged in advance to have relevance to some practical problem is to ignore the potential discoveries and the steady advances that lie completely in the unknown, for which one knows neither their nature nor their possible utility. The inherent power to cultivate this potential is one of the assets of a free society. Incidentally, it possesses great social and cultural advantages as well, and this is abundantly clear from a study of history.

Basic Research as an Investment

The apparent dilemma may be resolved by approaching the justification of basic research from a somewhat different standpoint. This assumes that, in addition to providing useful background for identifiable practical objectives, basic research is important as an investment, comprehensive in scope and varied in detail. It stresses quality of performance, originality of ideas, and degree of promise rather than subject-matter priorities. Like other investments, this one should encompass a broad range all the way from conservative items with small but certain yield to radical items with uncertain but possibly outstanding yield, and in this way should endeavor to cover the entire field. Again, as in any well-planned investment, a sound, comprehensive basic research program is statistically certain of important, constructive results even though the region of breakthrough and marked progress are not known in advance. In fact, as history amply proves, such results far more than repay the cost of the entire program in raising the general level of technology and in opening up impressive opportunities for outstanding developments. At the same time it advances the frontiers of knowledge and provides for the up-to-date training, in all fields of science and engineering, of the needed men and women for future research, teaching, and administration. Accordingly, to a high degree this approach provides justification from all three of the standpoints mentioned. It covers the comprehensive support of basic research for practical reasons, while at the same time it justifies the use of public funds for "free" research and thereby fosters progress in science as such. By no means the least of such a policy is its intrinsic preparation for change—change in requirements for research knowledge and in demand for specialized training of scientists and engineers. For surely one of the most important attributes of a society for the future will be its adaptability.

If one includes in this rationale the basic research support components contributed by agencies with practical missions, that is, for basic research closely mission-related, then one forms a picture of the federal support programs as presently conceived. In practice it is strongly modified by budget limitations. Thus, of the basic research supported by the federal government at academic institutions, only about 20 percent is provided by the National Science Foundation, the only agency which by its charter frankly and simply supports basic research in science (except the Smithsonian Institution which has a modest program along its own lines of interest). Most of the federal support for the life sciences comes from the National Institutes of Health, and for the physical sciences from the Department of Defense, the Atomic Energy Commission, and the National Aeronautics and Space Administration.

Thus, in the federal support of basic research, the element of relevancy toward specific practical goals strongly predominates. Furthermore, among agencies with practical missions, the mission-relation criterion is more strictly and narrowly interpreted in times of severe budgetary restrictions. As already mentioned, such moves tend to retain support of applied research at the expense of mission-related basic research, the latter being regarded as desirable but not essential. Accordingly, over the years federal agencies may be expected collectively to concentrate on applied research and development and on basic research only in fields most central to their primary missions. The outcome is a general federal research effort that is largely channeled into fields of unquestioned practical importance, but which overemphasizes some fields where actual progress may be slow and which hold little promise by this direct approach. In the latter case, progress is best sought by recourse to basic research of the most fundamental variety in all fields which may contribute to an understanding of the phenomenon in question. This history of research on the prevention of cancer is a case in point.

From the point of view of science this overconcentration upon specific fields not only warps the national effort in science; it may fail to encourage basic research into some of the most promising scientific areas, it also fails to recognize the possibility of important research findings as yet unforeseen, and in others may result in overfunding special areas or wastefully financing unproductive research.

The agency uniquely qualified to plan and support a comprehensive research program in the cause of science to compensate for such trends is the National Science Foundation. Therefore, if the nation is to maintain a leading competitive position in science, a serious responsibility devolves upon that Foundation as the agency uniquely authorized to sustain this effort. By the same token, the scientific community should be prepared to defend this kind of support in ways that are understandable to the administration, to the Congress, and to the general public. The future of the United States in science and technology will, in a large measure, depend upon the extent to which this situation is understood and results in constructive action.

Formulation of Policy for Federal Support of Science

In a concerted attempt to solve this general problem, during the past 2 years a comprehensive study has been proceeding under the auspices of the nation's highest scientific authority, the National Academy of Sciences, under the supervision of its Committee on Science and Public Policy (COSPUP), with the backing of budget and administrative authorities in the executive and legislative branches of government. A preliminary report has been published and some of the disciplinary components in this study have already appeared.

While the importance of this effort is unquestioned and its results are awaited with great interest, one should bear in mind certain inherent limitations in this approach, such as:

1) The outcome is admittedly an extrapolation, the value of which diminishes rapidly with time, often in a most unpredictable manner. Moreover, in order for the report to be effective in federal programs, one must allow a period of at least 4 years—a minimum of 2 years for the completion of the study, and two more years for the planning, adoption, and assignment of appropriations in the federal budget cycle.

2) It is a lengthy and time-consuming process, and necessarily involves outstanding research specialists from all disciplines, from whom it claims valuable time and attention from their personal research. Hence a full-scale study should not be attempted too frequently, certainly not at less than 5-year intervals.

3) It has most validity in particular disciplines, but even in these the result must be regarded as a compromise between the estimated relative merits of sub-disciplines.

Such a study inevitably leads to consideration of the three principles mentioned earlier and is valuable for mutual criticism among supporters of these and other criteria. But even a study under such distinguished auspices should not be expected to provide a complete answer to national research problems nor do its participants and sponsors expect it to do so. In its component studies it can, at most, strive for authoritative, critical judgments on current progress and promise in the respective disciplines of science, together with an estimate of their dollar, manpower, and other requirements, to maintain optimum progress. It can provide a comprehensive summary of the current general situation in scientific research. To some extent, it can take into account probable contributions to technology and other useful services to society. But it should not be expected to do more than this.

Nevertheless, it can provide an input of the greatest importance, namely, an authentic exposition of the current status of the various fields of science, their rates of progress, and present promise. For, in addition to identifying the needs of science, this is the logical starting point for consideration of the feasibility and priority of research and development programs, the identification and appraisal of technological goals, and the optimum degree of effort toward national objectives of many kinds.

A primary reason for this importance is that pressing scientific and technical problems can then be approached with the best available evidence of feasibility and promise for their solution. It is one thing to determine what we need to know or what would be helpful to know. But whether this can be accomplished immediately is quite another matter, and obviously an essential point. In other words, as a general principle for maximum progress and efficiency in the solution of technical problems, we should look for a match between (i) what we know or have good prospect of learning and (ii) what we need or desire to know. Such a study as that by COSPUP constitutes a determined attempt to answer the category (i). In decision-making it should be utilized together with the desired objectives from (ii) so as to maximize progress and efficiency of effort.

Proper attention to this sequence should go far toward improving the effectiveness of our national programs and avoiding the waste and inefficiency that accompany infeasible or unproductive effort. This principle, well known to scientists, was succinctly stated by Robert Oppenheimer as follows: ". . . in the end you will be guided not by what it would be practically helpful to learn, but by what it is possible to learn." This is not to say that the apparently impossible should never be attempted. But, generally speaking, such a prospect should be pursued by means of basic research and only in the most experienced and talented hands.

Thus a major contribution of the COSPUP study lies in the fact that it represents a thorough and concerted effort on the part of leading scientists to analyze the national scientific effort, to determine the degrees of activity and rates of progress among the various scientific disciplines, and to recommend the optimum proportions of support for them in the light of current scope and promise. As such, the study will provide a fundamental background for decision-making; it should have unique and powerful significance for federal research planning and budgeting.