Basic Research—Description versus Definition

A definition of basic research in probability terms is useful, but statistics based thereon are not.

Charles V. Kidd

"An abstract term is like a box with a false bottom: you may put in it what ideas you please, and take them out again without being observed."—ALEXIS DE TOCQUEVILLE (*1*).

Descriptions and definitions of basic research have at least two kinds of potential uses. The first is to convey—generally to nonscientists—a sense of the nature of basic research, a feeling for its importance, and an appreciation of the motives and working conditions of scientists. One ultimate purpose served by such a description is to expand the scientific capacity of the country by creating understanding of, sympathy for, and support for, the full array of conditions that seem to be conducive to the production of basic findings.

To serve this function satisfactorily, basic research can be described in general, impressionistic terms, and logical precision is not required.

Vannevar Bush, among others, has written such an impressionistic description (*2*):

"Basic research results in general knowledge and an understanding of nature by its laws. This general knowledge provides the means of answering a large number of practical problems. The scientist doing basic research may not be at all interested in the practical applications of his work yet the further progress of industrial development would eventually stagnate if basic research were long neglected. New products and new processes do not appear full grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science. A nation which depends upon others for its new basic knowledge will be slow in industrial progress and weak in its cooperative position in world trade, regardless of its mechanical skill."

A second use of definitions of basic research is to provide rational, and adequately precise, criteria for decisions required in classifying research as basic for the purpose of compiling statistics.

The burden of this article is that basic research can be and has been described adequately for the first use, but that basic research has not yet been defined—and may never be defined—so as to permit an unambiguous, objective measurement of the dollars spent for basic research in this country.

Problem of Definition

The fact that the problem of securing an adequate definition of basic research has not been resolved is made clear in a recent report of the National Science Foundation (*3*):

"University officials estimate that, during the academic year 1953–54, academic departments of colleges and universities and agricultural experiment stations received about $85 million for basic research from the Federal government. But Federal officials estimate that they provided barely half that amount to universities for the same purpose and during the same period."

Somewhere between the offices in Washington which hand out research funds and answer questionnaires and the offices in universities which receive funds and answer questionnaires, the meaning of the definitions of basic research undergoes a metamorphosis that permits one set of observers to find the quantity to be twice as large as the other observers say it is. Such a discrepancy raises a number of questions, including the nature of the definitions that provide such a flexible yardstick.

Investigator-Centered Definitions

A useful point of departure is the definition of basic research given to both federal agencies and universities by the National Science Foundation as a guide to classification of research (*4*):

"Basic research is that type of research directed towards increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study, rather than a practical application thereof."

The salient characteristic of this definition, it seems to me, is that it is framed in terms of the "aim," or the intent, or the motive of the investigator and not in terms of the research finding itself. This thought led me to collect definitions of basic research and to try to group them in various ways.

Without pretending to have exhausted the subject, I have found that definitions of basic research seem to fall into two general categories. There are first those, such as the National Science Foundation definition cited above, which define research in terms of investigators' motives and intent and the conditions under which they work. The second group of definitions relates not to investigators but to the work itself.

Let us look first at some of the definitions that are investigator-centered. A historian of science, I. B. Cohen, has spelled out a definition in these terms (*5*):

"The difference between those who work at fundamental research and those who work at applied research is in the point of view with which they face the problem and the goals they have in mind. The man working at the 'pure science' end of the spectrum, whether in a university or in an industrial laboratory, pursues a problem because it is interesting or because it appears to have a certain relevance to fundamental knowledge. By contrast, the man working at the applied science end of the spectrum

The author is chief of the Office of Research Planning of the National Institutes of Health, Bethesda, Md. This article is taken from a forthcoming book, "Universities and Federal Research" (Harvard University Press).

pursues a problem because it has a relevance to a particular practical goal."

As another example, A. M. Brues has stated in the *Bulletin of the Atomic Scientists* (*6*):

"Basic research, now, is an attitude of curiosity about underlying relations between things, and about fundamentals; it can be pursued either by abstraction or through ability to follow up unexpected findings."

J. A. Stratton, former provost of Massachusetts Institute of Technology, has stated the motive or intent criterion in this way (*7*):

"Research in the natural sciences before the 17th and 18th centuries began largely as an avocation of amateurs, of gentlemen of leisure with a curiosity about the nature of the physical world. It was *fundamental* in that it was motivated wholly by a desire to know and understand."

Conant has taken the same approach (*8*):

"One may consider science as an attempt to either lower the degree of empiricism or to extend the range of theory. . . . Almost all significant work of scientists today, I believe, comes under the heading of attempts to reduce the degree of empiricism; the distinction between one group and another is in the motivation. Those who are interested in the fabric of science as such are ready to follow any lead that gives promise of being fruitful in terms of extending theoretical knowledge."

A distinct subgroup of investigator-centered definitions distinguishes between basic and applied research in terms of conditions under which the investigator works, and particularly of the degree of freedom he enjoys. Thus, basic research is sometimes defined as research undertaken by the "uncommitted" investigator—that is, an investigator who is not bound by external forces to follow a predetermined line of study. Such a definition appears in *Basic Research, a National Resource,* a 1957 publication of the National Science Foundation (*3*):

"Basic research is systematic, but without direction save that which the investigator himself gives it to meet the challenge of the unknown. He is strictly on his own, guided primarily by his interest in learning more about the workings of nature."

There is a logical distinction between the motive and intent of investigators on the one hand and the degree of freedom with which they work on the other hand. However, the two are often considered together, as is the case in the quotation immediately above. In any event, both criteria are clearly centered around the scientist—his motives and intent and the degree of freedom with which he works.

Substance-Centered Definitions

Let us turn now to definitions of basic research which center around the substance of research.

One category within this group distinguishes between basic and applied research in terms of the prospective utility of findings in meeting some practical need in the near future. Here the criterion is that the work, as described in advance, does not appear to have any immediate practical application. Such a definition, intended to indicate the kind of basic research the National Science Foundation and other federal agencies should support, appears in Executive Order 10521 (21 March 1954): ". . . support by other Federal agencies of basic research in areas which are closely related to their missions is encouraged." Such research is designated as "special purpose" basic research, in contrast to "general purpose" basic research to be supported by the National Science Foundation. Unfortunately, this definition in practice does not provide a usable means of distinguishing between types of research because there is no inherent distinction, particularly before the work is initiated, between the substance of "special purpose" and that of "general purpose" basic research.

In the second category of substance-centered definitions, distinction is made between basic and applied research in terms of the scientific significance of findings.

There is a wide agreement among scientists that breadth of findings is the criterion for assessing the basic character of a discovery. For example, Poincaré has stated (*9*):

"There is a hierarchy of facts. Some are without any positive bearing, and teach us nothing but themselves. There are, on the other hand, facts that give a large return, each of which teaches us a new law."

Hardy, the mathematician, has noted that (*10*) "mathematicians value ideas by their generality and depth."

Conant wrote (*11*):

"Systemized or well ordered empirical inquiries are one element in the advancement of science; the other element is the use of new concepts, new conceptual schemes that serve as working hypotheses on a grand scale. Only by the use of new ideas of broad significance has science advanced. . . ."

This criterion of the generality, breadth, or significance of findings has been well summarized by Cohen (*5*):

"We thus naturally classify scientific work according to the degree whereby it affects scientific thought and procedures; according to the amount by which it changes the foundation or structure of science itself. We may call this the fundamental character of the research. Some work is of a more fundamental character than other work simply because it affects a broader area, or because within its narrow area of applicability it has a deep and penetrating effect."

Contradictions and Inadequacies

If each investigator-centered and substance-centered definition is taken literally as satisfactory and self-contained, as is often done, each of them is patently inadequate.

For example, the definition of the National Science Foundation—that basic research is research "where the primary aim of the investigator is full knowledge or understanding of the subject under study, rather than a practical application thereof"—invites the obvious rejoinder that persons aiming to solve a very practical problem have produced findings of general significance. This definition, drawn up for the foundation's statistical reports on the volume of basic research conducted in this country and fairly widely used for other purposes, suffers from the further deficiency that decisions as to who will or who will not receive a research grant cannot in practice be based on assessment of scientists' motives. Accordingly, as the deputy director of the National Science Foundation has stated in describing how the foundation selects research proposals for support, "There is really only one criterion, and that is the excellence of the particular research proposal which is made to the Foundation" (*12*). Implicit in this procedure, which is not literally followed in practice, is a definition of basic research which relies on the substance of proposed research rather than on an assessment of the man or of his motive, intent, or working conditions.

Those who define basic research solely

as research conducted by investigators free to follow wherever their findings and curiosity lead are open to the observation that some research performed under these conditions is worthless. Furthermore, if basic research is defined solely as work with no foreseeable application, some trivial and irrelevant research will be admitted to the category of basic research.

Apart from the inadequacies of each definition, the group of investigator-centered and the group of substance-centered definitions of basic research are, if accepted literally as adequate self-contained definitions, manually exclusive. Thus, if the breadth of findings is to be the criterion for defining basic research, such things as the investigator's motive and the freedom with which he works are irrelevant. Conversely, if the attitude and approach of the investigator and the degree of freedom which he enjoys are to be the exclusive criteria defining basic research, the nature of the findings has no bearing on whether research is basic or not.

Each of the criteria, when considered singly and literally, does lead to contradictions which have been unwisely invoked to ridicule the concept of basic research. Yet if the idea of basic research is critically important, as it certainly is, it is also important that a generally acceptable definition be worked out.

Reconciliation

Inadequate and mutually exclusive definitions of basic research are currently used without apparent concern by intelligent and experienced people. Such a situation leads to the suspicion that there may be some underlying problem forestalling a clear resolution of the contradictions pointed out above. I think that there is such a problem, arising from an unapparent confusion of ends and means.

To reconcile the various definitions of basic research in a way that is not only logical but operationally useful, it seems to me imperative to begin by drawing a distinction, pointed out by others, which may appear to be overly nice. "Basic findings" are fundamentally different from "basic research" because findings are an end product and research is a process. This distinction, seen clearly by Brues (*6*), is not always drawn, even though it is of central importance to a clear resolution of the problem of definition. Failure to draw the distinction generally leads to confusion. This is illustrated by Cohen's statement quoted above. The system of classification implied by his definition is ambiguous because one cannot tell whether the *work* referred to is the process of research or the findings. Literally, this *work* is a process, but the sense of the word *work* in the context of Cohen's description is "findings."

For the administrator, definitions in terms of the end product—basic science or basic findings—are not usable because decisions must be made before the research is completed and the findings are known. Administrators are forced to be prophets. They must support basic research before the returns are in. This can actually be done with an adequate degree of precision by observing the kinds of people, the kinds of motives, and the kinds of working conditions that have as a matter of probability tended to produce basic findings.

With this view of the problem of definition, the array of criteria used to describe the research process must be viewed not as literal descriptive definitions but as statements of the probability of producing a basic finding. Thus, a "definition" of basic research in terms of the investigator's freedom is simply the statement of an assumption that those whose thought is not restricted and narrowly channeled are more likely to come forth with scientific ideas of great breadth or depth than are those working with less freedom. Definitions in terms of the motive and intent of the investigator are essentially statements of a belief that those with wide-ranging native curiosity are more likely than others to produce basic findings.

When the criteria of basic research are viewed as statements of the probability that basic findings will be produced under certain conditions, rather than as a literal description of the process of basic research itself, apparent contradictions disappear. For example, the "no practical application" criterion would mean not that no one working on an applied problem can produce a basic finding but simply that the probability of producing a fundamental finding is greater among those whose thinking is not restricted by a search for application. Definition in terms of the freedom with which the investigator works would not mean that basic findings are produced by all of those who are free to do whatever they wish but that the probability of producing new ideas of broad significance is greater among investigators who are free.

Probability Definition for Collection of Statistics

A definition of basic research in terms of the circumstances that appear as a matter of probability to lead to basic findings is inherently unsuitable for the purpose of collecting statistics. One reason for this is that a sound and usable definition of the conditions under which basic findings are, as a matter of probability, most likely to be produced must encompass all of the predisposing factors. Motives, intent, working conditions, and prospective applicability of findings must all be included. Other circumstances that appear to increase the probability of producing basic findings may be added to those already generally accepted. Just what circumstances should be included in a definition, and the weight to be given to each, are matters decided in large part by the exercise of subjective judgment.

Second, motives, attitudes, and working conditions cannot be measured precisely. How curious must a scientist be about fundamental phenomena before his work is viewed as basic research? How free must he be? And free from what, or for what? How remote from application must his findings be before his research is considered basic? These questions suggest that even if there were a firm consensus as to the criteria that are properly a part of a definition of basic research, it would be impossible to measure such factors quantitatively and comparably.

The criteria to be used in defining basic research and the weight to be given to each are both affected by such things as institutional goals, traditions, and personal experiences and predilections. This explains why, as shown in the first part of this article, people in universities have looked at a given universe of research and have decided that the proportion of this research belonging in the basic category is twice as large as the proportion placed in the basic category by federal administrators. As another example of the nature of this problem, if engineers and physicists had to classify each others' work as basic or applied, less engineering and more physics would be called basic than would be the case if each discipline classified the work that is done in its own fields.

It may be that these difficult problems of definition can be overcome in time. Magnitudes that were in earlier years the source of sharp debates are now measured by generally accepted

techniques. For example, such things as the national income accounts—the gross national product, private investment, savings, and so forth—have evolved into standard statistical series only after years of sustained effort and critical discussion by a large group of economists.

Whether a comparable effort could produce a generally accepted set of statistics dealing with basic research is a matter of judgment. For the reasons set forth above, it seems to me that the problem is inherently unsolvable, and that efforts to secure adequately precise and comparable statistics by undertaking to improve the definition of basic research are therefore futile. But I could be wrong.

Even if statistics on basic research are inherently affected by subjective judgments, it may be better to collect and publish what can be collected than to make no effort to do so. The case for making the effort rests essentially upon our strong national predilection to rely upon statistics in reaching judgments. If one accepts the idea that the nation would be better off if greater attention were paid to basic research, and if statistics help to convince people of the validity of the idea, it may be worth while collecting and publishing the information even though the statistics are inherently allegorical.

Probability Definition for Administrative Decisions

Definitions of basic research in terms of such factors as the degree of freedom with which the investigator works and the prospective applicability of his findings are useful in making administrative decisions on the support of research. In practice, administrators do not decide to support work because it is basic or not basic. Indeed, the term *basic research* is used much less frequently in the day-to-day business of research administration than it is in communicating with the nonscientific world.

Administrators consider the man—his past performance as judged by his peers—even though the merit of the research project is ostensibly the basis for judgment. They consider the facilities available to him. They take into account the support available in his field—whether it is a "gap area" or one well financed. What those who make decisions cannot do and do not attempt to do is to judge the intentions and the motives of investigators. Definitions of basic research in terms of motive and intent are, in practice, used by administrators—those who participate in decisions on the distribution of research funds—neither in administering research nor in collecting statistics on research.

The criterion of freedom of the investigator as a condition conducive to the production of basic findings is also usable and used as a guide to research administration. More broadly, this criterion encompasses the total array of factors conducive to scientific research of high quality. H. A. Shepard, in an article in the *Journal of the Philosophy of Science* (*13*), came to this conclusion:

"Efforts to define basic research operationally are misleading and bring about neglect of the forces that produce it—the training, discipline, values, way of life and system of social control that motivate men to advance knowledge for its own sake. . . . Support for basic research means support of a social system which so motivates men."

I am suggesting, first, that it is not possible to define basic research operationally. Second, I think that basic research can be effectively promoted by concentrating on provision of funds under terms and conditions designed to strengthen the forces, values, and social system which appear as a matter of probability well designed to promote basic findings.

Administrators—federal, university, or industrial—do have it within their power either to give scientists a large degree of freedom or to hem them in with a wide array of well-know requirements and restrictions. Freedom means, here, broad definition of areas of research; easy, informal changes in the direction of the research; assured stability of support; and freedom from onerous and essentially unproductive reporting requirements. In administrative terms, the terms and conditions under which all federal funds are provided affect basic research as significantly as does provision of funds for work labeled as basic. To the extent that scientific freedom affects the character of findings, a plea that the Federal Government "support more basic research" is a plea for administration of a larger proportion of federal research funds in a manner which places few restrictions upon investigators.

References and Notes

1. A. de Tocqueville, *Democracy in America*, vol. II, book I, chap. XVI.
2. V. Bush, *Science, the Endless Frontier. A Report to the President* (Washington, D.C., 1945), p. 13.
3. National Science Foundation, *Basic Research, A National Resource* (Washington, D.C., 1957), p. 25.
4. ———, "Federal Funds for Science, Fiscal Years 1956, 1957 and 1958" (Washington, D.C., 1958), p. 20.
5. I. B. Cohen, *Science, Servant of Man* (Little, Brown, Boston, Mass., 1948), pp. 303, 355.
6. A. M. Brues, *Bull. Atomic Scientists* 11, 344 (1955).
7. J. A. Stratton, *Chem. Eng. News* 31, 2581 (1953).
8. J. B. Conant, *Science and Common Sense* (Yale Univ. Press, New Haven, Conn., 1951), p. 58.
9. H. Poincaré, *Science and Method* (Dover Publications, Dover, Del., 1952), p. 284.
10. A. H. Hardy, *A Mathematician's Apology* (Cambridge Univ. Press, Cambridge, Mass., 1941), p. 17.
11. J. B. Conant, *Modern Science and Modern Man* (Doubleday, New York, 1953), p. 46.
12. Society of Sigma Xi at the Rensselaer Polytechnic Institute, *Changing Patterns of Academic Research* (Troy, N.Y., 1957), p. 55.
13. H. A. Shepard, *J. Phil. Sci.* 23, 57 (1956).