SCIENCE POLICY PERSPECTIVES: USA-JAPAN

Edited by

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PREFACE

This book is based on the Second Seminar on Science Policy under the U.S.-Japan Cooperative Science Program. It was jointly sponsored by the Division of International Programs of the National Science Foundation (NSF) and the Japan Society for the Promotion of Science (JSPS). The conference was held in Honolulu, Hawaii, in August 1981 and was preceded by an exchange of papers from the participants of each country.

The philosophy behind this volume is that there is much to be learned between Japan and the United States by exchanging ideas on each other's science policies. This exchange built on the previous conference. Individual papers were not limited to government policies but rather to different aspects of each country's systems for issues affecting scientific and technological progress.

Donald E. Stokes points out that deeply rooted in the American scientific community is the belief that basic and applied scientific research are fundamentally distinct. However, the viewpoint presented in this chapter is that the tendency to think of these types of research as mutually exclusive has created difficulties. Abe and Tezuka explain that in Japan basic research is carried on in universities, applied research is carried on in national research institutes, and research and development is carried on in the private sector. Langenberg's chapter emphasizes the point that to categorize research on two dimensions (basic or applied) can be misleading.

Inose *et al.* explain the pattern of research support in Japan and the allocation by sectors. Radnor shows the relationship between national goals and R&D programs in the U.S. government agencies. This chapter particularly examines the question as to how closely the R&D portfolios reflect the goals for which the agencies are responsible.

Bartocha and Cziesla focus on the U.S. government role in supporting research. This is contrasted with Japanese government support systems as explained by Abe *et al.* Schlie examines the fundamental basis for governments supporting R&D activities.

In the chapter by Senich and Kaatz, the role of American college and university faculty in the industrial innovation process is described, pointing particularly to the changing relationships of U.S. colleges and universities to the nation's business environment. The issues of university-industry cooperation are addressed by Gerstenfeld and Colton.

The issues of the U.S. patent policy for inventions made under government funding are reviewed by Ganz. This is followed by Toyama and Hasegawa's chapter explaining the current patent system in Japan. Bremer focuses on inventions performed under U.S. government contracts. The final chapter written by The Science Council discusses the handling of patents in Japan for the inventions of university professors.

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PERCEPTIONS OF THE NATURE
OF BASIC AND APPLIED SCIENCE IN THE UNITED STATES

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I. INTRODUCTION

Few ideas are as deeply rooted in the American scientific community as the belief that basic and applied scientific research are fundamentally distinct. And few ideas have as firm a hold on the American governmental community as it formulates science policy. It is therefore paradoxical that efforts to give this distinction a firm conceptual underpinning should have met with so little success. The relationship between basic and applied research has proved remarkably elusive both for commentators from the scientific community and for those who make and implement science policy.

The viewpoint taken here is that basic and applied science are indeed conceptually distinct but that the tendency to think of these types of research as mutually exclusive in a logical or empirical sense has created unnecessary difficulties for the

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research community and for the development of science policy. The discussion begins with an analysis of the prevailing conceptions of basic and applied research, then examines the misconceptions of the relationship between these two kinds of scientific work and suggests ways in which these misconceptions have distorted the organization of the research community, the development of science policy, and the efforts to understand the course of scientific research. The analysis draws on an earlier paper in which this argument is outlined (1), as well as on papers of a symposium sponsored by the National Science Foundation (NSF) on categories of scientific research (2). Although the argument applies quite generally across scientific fields, many of the examples are taken from the social sciences.

II. PREVAILING CONCEPTIONS OF BASIC AND APPLIED RESEARCH

Perceptions of basic and applied research have been influenced, at least since the Second World War, by political and administrative considerations. Indeed, the currency of the term "basic" research reflects the influence of the report, *Science the Endless Frontier,* by which Vannevar Bush, director of the wartime Office of Scientific Research and Development, launched the brilliantly successful campaign to create the NSF and commit the national government to the support of fundamental scientific research in peacetime (3).

It would, however, be a mistake to suppose that this category grew simply out of efforts to make or implement science policy. The term "basic" may be relatively new, but the concept it denotes goes back at least to Francis Bacon and is deeply woven into the thinking of the scientific community. The term "fundamental" research is practically synonymous with basic research. The term "pure" research, which is also frequently thought to be synonymous with basic research, tends to define such research in terms of what it is not, rather than in terms of what it is. This point will be discussed later.

Several closely related characteristics supply the core idea of basic research. One is the breadth of the domain of the knowledge it seeks to develop. It is widely accepted that such research is directed toward underlying structures or processes of broad explanatory or predictive significance. The broader this significance, the more basic the scientific work is thought to be. Since advances of this kind have a lasting impact on the intellectual structure of a scientific field, it is also widely understood that the most basic research will modify the conceptual structure or organization of knowledge—achieving at the limit a scientific "revolution" (4).

The uncertainty of research outcomes complicates the effort to translate these ideas into policy terms. The allocation of support among competing uses requires judgments at the time research problems are chosen and projects funded. But we cannot be certain in advance which problems or projects will yield basic advances of scientific knowledge. The *ex ante* effort to identify basic research must therefore consult the intentions of the scientific investigator and the criteria that shape the choice of research problems and designs. These have seemed excessively subjective to a number of commentators who argue that, since we cannot have a fully objective or "operational" way of defining basic research in advance, we ought to reserve this category for the *ex post* judgments that can be made when the actual development of a scientific field is known.

These difficulties are easily exaggerated. It is, to be sure, difficult to foretell with certainty which work will lead to the most basic advances of scientific knowledge. But *a priori* judgments are made in every scientific field as to the research problems and the techniques which will have the best prospect of widening knowledge. The "peer review" system is designed to render advance judgments so that scarce resources, including the support of basic research by the national government, can be channeled to the investigators and projects most likely to enhance scientific knowledge.

Certainly it is clear that advance classification of basic research need not rest only on the intentions of the investigator. In the words of one observer:

> *Any research process can be thought of as a sequential, branched, decision-making process. At each successive branch, there are many different alternatives for the next step, and one may use the criteria that govern the choice among these alternatives as the measure of whether the research process is basic or applied. If the criteria are primarily related to the internal logic of the subject, i.e., to some larger conceptual framework of laws and principles, then the research is basic.* (5)

The peer review process routinely assesses the quality of this sort of decision making in alternative research proposals.

Applied research is, on the other hand, directed toward some end or use apart from the extension of knowledge for its own sake. This concept, too, is a very old one in the thinking of the research community. Whereas the knowledge developed by basic research is meant to shape the intellectual structure of a scientific field, the knowledge developed by applied research is meant to deal with some unmet individual, group, or societal need.

It is important to note that this view of applied research is also *ex ante*. Many of the advances of basic research have been later put to a variety of applied uses, and it is an article of faith in the scientific community that most fundamental advances will have subsequent applications of this sort. But this does not mean that all basic research is seen as applied. The distinguishing characteristic of applied research is that its potential social utility will, *at the time*, help to guide the choice of research problems and the development of research designs. This *ex ante* viewpoint is essential to advance policy judgments, especially the allocation of research support among alternative uses.

Skeptics also have challenged this definition as being excessively reliant on subjective intentions as to the use of the knowledge to be gained from research. The rejoinder to those who are doubtful about the advance classification of basic research is no less appropriate here. Applied research also involves a sequential, branched decision-making process. If the choice of research problems and techniques is influenced by the potential use of the knowledge gained, then the research can be called applied. As is the case with basic research, a number of processes have been developed for making these decisions.

Deeper conceptual issues are raised by the question of whose motives to consult when the decisions that shape research are taken in complex institutional settings. For example, the investigators within a company's research laboratories may see themselves as pursuing scientific knowledge for its own sake. But the laboratories' directors, and the corporate managers for whom they work, may build research agendas on the basis of the applied use of knowledge. These uses often depend on a broad array of related projects rather than the single projects pursued by individual researchers. Similarly, one view of the objectives of research may be taken by a performing institution, while a quite different view is taken by the sponsoring institution (5).

These are more than quibbles. It is difficult to pursue these points very far without wondering whether the motives for basic and applied research must be mutually exclusive. If the decisions shaping particular research projects taken by people playing different institutional roles can be influenced both by the quest for scientific understanding and by the practical utility of the results, surely both motives can influence the decisions of individual scientists. We ought indeed to be skeptical of the prevailing conception of basic and applied research as necessarily opposed.

III. THE RELATIONSHIP BETWEEN BASIC AND APPLIED RESEARCH

The tendency to see "basic" and "applied" as excluding each other is a conspicuous element of the prevailing conception of these research types. Vannevar Bush subscribed to the view that "basic research is performed without thought of practical ends," and this same view underlies the use of the term "pure research" as a synonym for basic research (3). Likewise, the traditional view is that research undertaken for practical ends is an inherently limited instrument for advancing our understanding of structures or processes of broad scientific significance.

These ideas are frequently translated into a single-dimensional continuum or spectrum that places basic research at one end, or pole, and applied research at the other pole. Indeed, the metaphor of Euclidean one-space is a standard element of efforts to expound the basic-applied distinction. In accord with this spatial view, the more closely research is thought to conform to the pure type represented by one end of the scale, the less well it will conform to the pure type represented by the other end of the scale. Classification may be more difficult near the middle of the spectrum because the scale is continuous. Such a view is typified by the remarks of a highly knowledgeable observer who explains this difficulty by noting that "...any process that divides a continuum into discretely demarcable regions is generally plagued by fuzziness and overlaps at the boundaries of the subdomains" (6).

This one-dimensional view seriously impairs understanding. The search for a satisfactory single distinction between basic and applied research is bound to fail because the difference between these research types involves not one distinction but two. Each of the paired concepts of "basic" and "applied" research is a type in its own right, and neither is the polar opposite of the other.

There is no reason to see the distinction between basic and non-basic research as necessarily dichotomous, since there are degrees to which research seeks fundamental knowledge. Similarly, there is no reason to think of the distinction between applied and non-applied research as necessarily dichotomous, since the instrumental goal of research is also a matter of degree. If, nonetheless, these root concepts are treated as dichotomous, it is clear that there will not be one dichotomy but two. The categories produced by crossing these dichotomies in a two-dimensional array would include one for research that is basic *and* applied in the sense of seeking to extend scientific knowledge *and* to meet a social need. They would also include a category for basic research that is unprompted by a problem or need. And they would include a category for applied research that is not basic, although such research may utilize the scientific understanding generated by prior studies.

It may help to visualize this double dichotomy if we represent these jointly defined types by the cells of a four-fold figure, as follows:

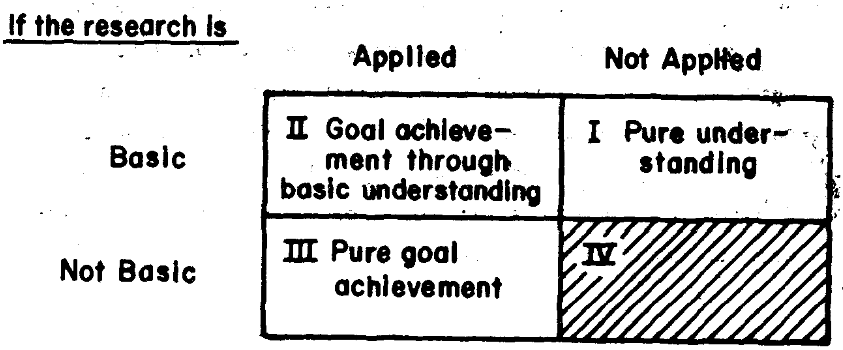

FIGURE 1. The motives of scientific research

Quadrant I includes research that seeks pure understanding without a particular problem or social need in view; projects for which the term "pure research" would be appropriate.

Quadrant II includes research that seeks to address a need by extending basic scientific understanding to discover how that need can be met.

Quadrant III includes research that seeks a clearly defined goal by developing knowledge that may be of little scientific importance, but will achieve the goal set forth.

Quadrant IV emphasizes the fact that "basic" and "applied" research are logically distinct concepts and that the categories represented by Quadrants I—III are more than an elaborated version of the familiar dichotomy of basic and applied research. This cell, reserved for research that is undertaken neither to advance basic scientific understanding nor to develop knowledge for an applied purpose, is by no means empirically empty. There are cases, for example, where the real motive for launching a research project on a social problem was the sponsors' desire to reduce pressure for creating a government program to deal with the problem (7).

The view that basic and applied research are radically separate first developed in the physical sciences and has been most influential there. This view owed something to the aristocratic origins of those who created the scientific achievements of the Enlightenment, since many of these pioneers were free to think in terms only of the intellectual return on the effort they invested in their scientific investigations. Also, this view fit comfortably with the fact that many early engineering advances of the Industrial Revolution were the work of practical men who had little need of deeper scientific understanding. Indeed, the distinction between basic and applied took on the added meaning that fundamental science dealt with the discovery of the general laws of *nature* and applied science and engineering dealt with devices created by man or with the built environment (8). In this century the distinction in the physical sciences is well institutionalized within the academic research community by the separation of the physical science disciplines from the applied science and engineering disciplines. The creation of the National Science Foundation clearly reflected the belief that a specialized agency should channel federal support of basic research to the physical sciences.

By contrast, the root distinction between basic and applied research was much less clear to the pioneers of biological science. The revolution carried through by Pasteur and his colleagues supplied an especially sharp example of the interplay of basic and applied motives in biological research. Their early studies in microbiology probed phenomena of far-reaching scientific importance. But the drive toward a new theory of disease was also fueled by the desire to lessen the ravages of disease in animals and man. There is a much less sharp institutionalization of the basic and applied distinction in the organization of the biological research community. The reform of the medical schools by Abraham Flexner and his followers created biomedical departments that house a great deal of scientific research that is both basic and applied. The fusion of these motives of research has been a far more natural idea to the National Institutes of Health, the principal channel by which federal support has flowed to biological and biomedical research, than it was to NSF in the first decades of its life (8).

The fusion of the basic and applied motives for research is still clearer in the social sciences. Indeed, some of the most famous social science advances in this century have been driven by both types of motives. This is true, for example, in the rise of modern demography. Those who pioneered the study of human population have regarded demographic change both as a basic process that challenges understanding at the most fundamental level and as a basic problem with explosive implications for most of the nations of the world. Similarly, the unfolding of macroeconomic theory in the hands of Keynes and his successors has involved research that is at once basic and applied. These researchers sought to understand the working of the economy at the most fundamental level in order to lift the misery of recurring depression and to better the human condition through sustained economic growth. Much of the fundamental work in each of these social disciplines would be classified in Quadrant II as research that is prompted by both basic and applied objectives.

Indeed, the economics discipline shares with certain engineering disciplines the characteristic of having an intellectual structure that is inherently directed toward problem-solving, or design, or policy choices. Much of microeconomics is expressed as a positive or normative theory of the enterprise or the firm, while much of macroeconomics is expressed in terms of the need of nations, or governments, or trans-national agencies to deal with choices of macroeconomic policy. As a result, a great deal of the intellectual structure of modern economics has been built from research that deserves to be called both basic and applied.

IV. REFINING THE ANNALS OF RESEARCH

Treating "basic" and "applied" research as logically independent concepts—and keeping both axes of the four-fold figure in mind—gives a more revealing account of the interplay of the two types of research objectives over time. Since traditional accounts force basic and applied research onto a single continuum,