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:

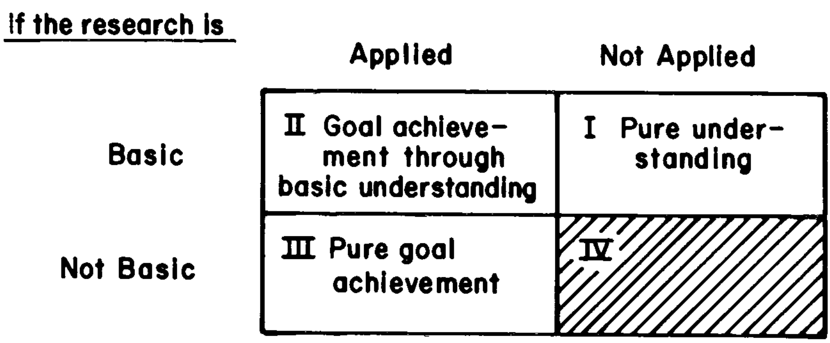

FIGURE 1. The motives of scientific research

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, they envisage the movements of research only across a single threshold. In particular, such commentaries emphasize how the ideas developed through basic research may later be turned to significant use through applied research and development. They also describe the opposite movements across this threshold, as when empirical anomalies identified by applied research inspire more fundamental scientific inquiries.

But some of the most interesting and important cases in the annals of research are the ones in which a line of "pure" basic research acquires a significant applied purpose without losing its fundamental scientific character; i.e., cases of a shift between Quadrants I and II. A vivid example is the demographers' discovery, soon after the Second World War, that they could use the models of population replacement developed between the First and Second World Wars to study population change in the less developed countries, for which only fragmentary data were available. This case is an interesting one because the earlier work had almost literally been forgotten (7). Soon after the First World War, Lotka did the fundamental work in modelling the processes by which biological populations reproduce themselves, spelling out the relationships between fertility, mortality, growth, and age. As the early use of these models led to empirical contradictions, they were neglected until the end of the Second World War, when it was realized that they held the key to studying population growth in many of the nations where only limited demographic data were available. The models provided a far better understanding of the scale of the problem of population growth and also allowed a number of governments and international agencies to prepare more effectively to deal with the population explosion.

This transition would be distorted if it were viewed simply as a shift from basic research to applied research. It is far more accurate to say that the results of an earlier line of "pure" basic inquiry were absorbed into a new line of basic research dealing with one of the most critically important problem areas of the postwar world—in short, a shift of research activity from Quadrant I to Quadrant II.

Redefining the basic and applied distinction also allows us to recognize movements of research from Quadrant III to Quadrant II; that is, shifts in which a line of purely applied research moves toward underlying structures or processes of much wider significance—without losing its problem focus. An example is again furnished by demography. At a time when a number of organizations with responsibilities in the population field wanted research that was specifically targeted on alternative action programs, some investigators recognized that action would be, in the end, far more effective if a fundamental base of knowledge existed about the sources of differential fertility. This belief led part of the demographic research community to pull away from narrowly applied research (Quadrant III) in order to mount more basic studies that were still seen as strongly relevant to the problem of containing population growth (Quadrant II).

A further example from the annals of social research is furnished by highway safety studies. As concern grew over highway deaths, extensive descriptive statistics were amassed on the frequency, severity, and circumstances of traffic accidents. Most of this work plainly fell into Quadrant III. But causal or explanatory questions came more and more to the fore as it was seen that the choice of effective policy measures required a deeper knowledge of cause. This prompted a series of studies of the etiology of accidents, studies that raised such fundamental questions as the reasons for the greater resistance to social control among young people. But these studies were strongly motivated by the applied purpose of finding points for effective intervention to counter the trend of mounting highway deaths (9). In other words, they fell into Quadrant II.

V. ORGANIZING THE RESEARCH COMMUNITY

The American research community provides a number of institutional settings in which the motives of basic understanding and applied use can merge. If the separation of the physical sciences from the engineering departments within research universities reflects a traditional view, a good deal of fundamental scientific work—which is also seen as generating critically useful knowledge—is carried on by engineering faculties. Bell Laboratories is only one of the most striking examples of an institutionalized effort by an industry to link basic and applied motives. Medical schools are, as stated previously, an arena for the fusion of basic and applied objectives in biological research. The merger of these goals in the internal and external research supported by the National Institutes of Health is emphasized by the practice of naming particular units within NIH for major problem areas such as heart and cancer. Although social science research is less institutionalized in the research universities, some of the most influential research organizations, such as Columbia's pioneering Bureau of Applied Social Research and Michigan's Institute for Social Research, were loci for programs of research that combined the thrusts toward scientific discovery and applied use.

Yet the research community has paid a price for incorporating too simple a view of the relationship between basic and applied science in its organizational arrangements. The complementary misperceptions, that what is basic is *not* applied and what is applied is *not* basic, have taken a toll. No one with first-hand research university experience can miss the ease with which many faculty who are engaged in basic research accept the corollary idea that they are therefore not engaged in applied science. Those who accept this view may pay lip service to the possibility that fundamental advances of science will lead later to worth-while applications. But pursuing these is often seen as a task for others, who may be thought to have a somewhat lower scientific status. The relationship of the physics and engineering faculties might be a good deal easier if their research missions were explicitly understood to overlap in Quadrant II. A number of other university units would have a clearer view of their appropriate research terrain if the reality of research that is at once basic and applied were more widely recognized.

VI. STRENGTHENING SCIENCE POLICY

A price has also been paid by government for accepting too simple a view of the relationship between basic and applied science. If the complementary misperceptions that basic is non-applied and applied is non-basic are widely held in the research community—which ought to have thought most deeply about these matters—it should not be surprising that they distort the vision of a number of those in government who frame or implement the nation's policies for the support of science. The hold of the belief in the basic-applied antithesis is remarkable. It is evident in countless hearings and debates in Congress. It is also evident in the thinking of many people in the agencies in the Executive Branch of the national government that play a role in making or administering science policy.

This antithesis has first of all distorted the research agendas of the so-called "mission" agencies of the federal government. The standard practice of the national government, at least since the Second World War, has been to couple the creation of new government programs with the authorization of research and development bearing on the problems to which the programs are directed. As a result, several hundred agencies which administer government programs are also charged with conducting research that bears on their missions.

Much of the resulting research has been purely applied. If it were widely understood that basic research can also have an applied purpose, a greater number of the mission agencies would have mounted significant programs of basic research that bore on their problem areas as they also supported narrowly targeted applied research. Some did in the first decades after the war, when the purse strings of government were less tightly held. But these programs of problem-centered basic research have been seriously eroded by recent budgetary pressures and by the passage of the "Mansfield Amendment," which required the international and defense agencies of government to justify their research investments in terms of the missions of their agencies. In view of the extent to which the basic-applied antithesis was accepted by those who administer or oversee federal research programs, the inevitable tendency of both these pressures has been to limit the research supported by the mission agencies to "pure" applied research. A number of researchers and federal R & D managers have testified to the baleful effect of these pressures in closing off lines of problem-centered basic research.

But too simplistic a view of the basic-applied antithesis has also constrained the free-standing federal R & D agencies with special responsibility for the support of basic research, including NSF. Although various members of the National Science Board, the Foundation's policy body, and of the Foundation's staff have held a more complex view of the relationship between the basic and applied objectives of scientific research, it is hardly surprising that an agency created with Vannevar Bush's mind-set should have been chary of being drawn away from the central mission by taking too deep an interest in the social utility of research knowledge. The simplest way of avoiding this was to lay primary stress on "pure" basic research. The tensions over the years between this approach and the Foundation's periodic efforts to promote research that is directed to certain of the country's unmet needs will not be traced here. In the current period the Foundation is moving toward a conception of basic research broad enough to encompass the support of fundamental science that is relevant to societal needs as well as the more traditional basic research defined by the disciplinary agenda. It need hardly be added that, if my argument is correct, such a shift implies no concession in rigor or scientific import.

A fresh formulation of the roles of NSF and the mission agencies in the support of basic research can be drawn from our four-fold classification. Too simplistic a view of the basic-applied antithesis has tended to focus NSF on the support of "pure" basic research: projects directed only to the development of a scientific field (Quadrant I). This view has also tended to focus the mission agencies on the support of "pure" applied research: projects narrowly targeted on applied uses (Quadrant III). The sponsorship of research that more properly belonged to Quadrant II has been vulnerable in both arenas, since it was (correctly) seen as applied by NSF and as basic by the mission agencies.

A more appropriate view can be offered. If my argument is correct, the terrain of the free-standing R & D agencies, including NSF, which have special responsibility for the support of basic research is the top row of Figure 2: Quadrants I and II together. The terrain of the mission agencies of government is the left column of the figure: Quadrants II and III together. Such a formulation by no means implies an unfortunate or wasteful overlap of efforts in mounting research that is basic *and* applied (Quadrant II). There would still be a clear division of labor between the two types of agencies, with the free-standing R & D agencies laying greater stress on the fundamental development of scientific fields and the mission agencies on coping with major societal needs. But neither would, under this revised conception of science policy, shrink from the support of Quadrant II research that might otherwise seem to be outside its area of responsibility.

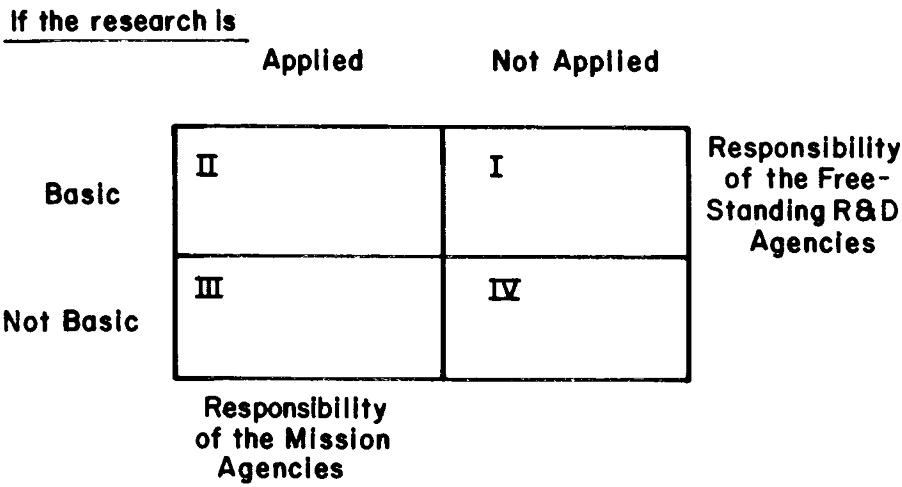

FIGURE 2. A Revised View of Science Policy

The view in Figure 2 is offered with a keen sense of simplifying a complex set of issues; a great deal is necessarily left out of such a sketch. .But the tenacity of the traditional view is partly due to the appeal of the simple spatial image that assigns basic and applied research distinct regions along a single continuum. It may therefore be useful to present a more realistic conception in terms of an equally spare image. I am convinced that those who keep the annals of scientific research, or shape the organization of the research community, or frame and implement the science policies of our national government would be well served by absorbing this alternative, two-dimensional view.

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PERCEPTIONS ON BASIC AND APPLIED SCIENCES IN JAPAN

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I. FROM APPLIED RESEARCH TO BASIC RESEARCH—A HISTORICAL OUTLINE

At the very beginning of Japan's modernization, under the Reformation Government in 1868, the government decided to introduce science and technology to the country in order to catch-up with the advanced industry and the economic prosperity of the West. They attempted to assess the nation's resources, to establish a telecommunication system, as well as to create a new system of administration, etc., all of which were required for a modern na-

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tion. As the quickest way to implement these plans required personnel who could be of immediate service, the government employed foreign engineers and specialists; this, of course, was very costly. In a few instances (e.g., in the construction of a navy shipyard at Yokosuka), the government employed not only foreign engineers but also foreign workers. Obviously these hiring practices could not go on indefinitely.

A training program was instituted where capable, young samurais, looking for new opportunities, were selected from among the unemployed and placed in apprenticeship with foreign engineers. This method of practical training soon led to the creation of technical institutions by several government ministries. In 1871 the Ministry of Engineering, employing British engineers, established a technical school for telecommunication workers. Some 60 students were given short-cycle training and, upon finishing the courses, were sent to work immediately. Thus young, jobless samurais were given new skills and new employment opportunities through practical learning. Also in 1871, the Ministry of Justice established a law college (Meihoryo) and the Hokkaido Development Agency founded the college of that agency under a similar arrangement.

These schools and colleges grew as time progressed. In 1874, for example, the Tokyo Kaiseigakko and Tokyo Igakko (both of which started as institutions for language training in 1868 by the Ministry of Education), were brought up to collegiate level and merged to form Tokyo Daigaku, or the Tokyo University. In the same year, the engineering college of the Ministry of Engineering was also given the name of university.

From 1870 to the early half of the 1880s, the institutional paths for promotion were multiple. Bachelor's degrees were conferred by the Tokyo University of the Ministry of Education, the Engineering University of the Ministry of Engineering, the Agricultural College of the Ministry of Agriculture and Commerce, and the Sapporo Agricultural College of the Hokkaido Development Agency.

At this point in time graduates from the Engineering University and from the Law College of the Ministry of Justice held a higher reputation than graduates of the law faculty of the Tokyo University.

The period from 1868 to 1885 was a period of economic transition. During this transition period, the Japanese intellectuals at institutions of higher learning were still unable to deal with basic research or pure science. The introduction and duplication of applied technology, as it appeared to be practiced in the West, was an adequate challenge in itself.

In 1885, control of the Ministry of Justice's Law College and the Ministry of Engineering's Engineering University were given to the Ministry of Education and subsequently were merged with the Tokyo University to form, in 1886, the Imperial University. This marked the beginning of Japan's economic modernization.

From the outset, the Imperial University had Faculties of Letters, Law, Science, Engineering, and Medicine. In 1890, when the Tokyo Agricultural College was transferred from the Ministry of the Interior and merged into the Imperial University, the Faculty of Agriculture was incorporated. The constitution of the faculties reflected the view of the science administration that applied science was as important as basic science. In the national universities that were established thereafter, faculties of applied science (i.e., engineering, agriculture, and medicine) were given an important place in their departments.

The national universities in Japan have much in common with the state universities in the United States in that they were established and operated for the public sector and had faculties of applied science as an important component from the beginning. It goes without saying that, at that time, emphasis on applied science met the needs of the nation.

It was only after the establishment of the Imperial University and the decision of various ministries to recruit career officers from among the graduates of the Imperial University, that the undisputable prestige of the Imperial University was confirmed. The transformation of the Tokyo University into the Imperial University in 1886, therefore, was a remarkable transformation in Japan's institutional history of higher education.

In the 19th century major centers of learning were England, France, Germany, and the United States. After careful observation the Japanese felt that the center of scientific activities had shifted from England, at the end of the 18th century, to France; then, during the latter half of the 19th century, to Germany. The United States was upcoming.

Interesting to note is the selection of standards in the introduction of learning from the West. Following is an excerpt from "A Regulation on Sending Students Abroad" written by Tomotsuna Iwakura in 1870. This document identified, for the Japanese government, the country to which students should be sent and the subject they should study. The paper reflected the Japanese evaluation of the strong areas in each country:

France: law, international law, biology, botany, demography, astronomy, mathematics, physics, chemistry, and architecture;

England: mechanical engineering, commercial law, geology and mining, steel manufacturing, architecture, shipbuilding, animal science, and social welfare;

Germany: political science, economics, physics, astronomy, geology and mining, chemistry, animal and plant science, medicine, pharmacology, and systems of education;

The Netherlands: hydrology, architecture, shipbuilding, economics, and social welfare;

The United States: postal services, engineering, agricultural sciences, cattle breeding, commercial law, and mining.

In the 18th century universities in France and England were, in essence, places for the transmission of past learning, from one generation to another. Research was to be done, and new ideas to be developed at academies rather than at universities. The concept of Alexander von Humboldt—that the best university teachers ought to be the best researchers—was then revolutionary. The upcoming prosperity of the German universities of the Humboldt principle was promoted primarily by the Faculty of Philosophy under the leadership of Fichte, Schleiermacher, and Hegel, all of Berlin. The idealism of the German philosophers won international reputation and furthered the model of the modern German university. In addition, the development of natural science laboratories supported the practice of wiscenschaft in German universities. It was within such a context that the introduction of the "chair system" was implemented.

In Japanese universities, particularly in the research-oriented universities like the former Imperial universities, the basic units of research and teaching had been the professorial chairs. The "chair system" as such was established at the Imperial University in 1893 under the leadership of Kowashi Inoue, who had drafted the Meiji Constitution and who was, at the time, the Minister of Education. Inoue's objective in establishing the chair system was to create a system of academic specialization in specific disciplines. While Inoue attempted to establish a system of practical education quite different from the Imperial university, he intended to have the Imperial University pursue philosophical and theoretical research. The chairs were intended to be the core of academic disciplines at the Imperial University.

The process for implementing the system revealed the attitude of Inoue toward the basic and applied sciences. The original draft, presented by Imperial University President Arata Hamao, proposed the establishment of 152 chairs. Inoue, however, reduced this to 125 chairs. The biggest cut was made in the Faculty of Engineering: the proposed 33 chairs was reduced to only 21 chairs. Also, many courses which had been offered, such as industrial law, mineral science, applied physics, and industrial economics were eliminated. The Faculty of Law was also severely cut—from the proposed 29 chairs to 18 chairs.

The Faculty of Letters and the Faculty of Science, on the other hand, were treated favorably. In the case of the Faculty of Letters, Chinese History and Greek and Latin were the only courses whose chairs were not approved; the proposed 19 chairs were reduced to 17 chairs. The Faculty of Science was almost untouched except for the reduction of Physics chairs (from three chairs to two chairs); of the proposed 19 chairs for the Faculty of Science, 18 were approved. Thus the proposal of the university remained intact in the Faculties of Letters and Science. This reflected the attitude of the policy-maker that preferential weight should be given to basic sciences through the structural implementation at the Imperial University.

The slighting of applied learning is even more clearly visible in the salary scale fixed at that time. Salary for a chairholder was to consist of basic increments and job-based increments. With regard to the job-based increments: chairholders on the Faculties of Medicine, Law, Letters, and Science were to receive four levels of payment: 699 yen, 650 yen, 500 yen, and 400 yen, while the Faculty of Agriculture only received levels of payment of 500 yen, 450 yen, and 400 yen. As applied sciences need more money for research, the level of payments can be regarded as a reflection of the policy to favor academic learning.

Another expression of government support for basic research was the presence of the Japan Academy. Its predecessor was the Tokyo Association of Degree Holders, established in 1879 during the presidency of Yukichi Fukuzawa. Then, in 1906, legislation was passed to transform it to the Imperial Academy. Under the American Occupation, it was renamed the Japan Academy. The faculty distribution follows:

Department of Humanistic Sciences	70
Literature, History, Philosphy Section	30
Law and Political Science Section	24
Economics Section	16
Department of Natural Sciences	80
Science Section	31
Engineering Section	17
Agricultural Science Section	12
Medical, Pharmaceutical, Dental Section	20

The above distribution clearly was weighted toward the basic and the pure sciences. However, today the weighting is directed more toward the applied sciences.

The above historical sketch reveals that, in the preliminary phase of modernization, there was no distinction of basic and applied sciences: universities had to emphasize applied learning. After the transitional period, however, leadership gave weight to basic learning in the establishment of the structural foundation of the Imperial University. This was done with a sense of balance and systems for applied learning were promoted as separate lines of development. The development of a division of functions in the late 19th century can be observed. In the trends that continued until today, universities, in principle, were consciously aloof from practical and applied learning; the latter areas were mostly entrusted to the efforts of the private sectors and the public research institutions.

II. WORKING DEFINITION OF TERMS

Discussions in this chapter, and in the chapter entitled "Policies and Practices of Government Support for Basic and Applied Research," will deal with research manpower, research budgets, research institutions, etc. Data have been collected from many sources, but most were taken from the annual surveys conducted by the Statistics Bureau of the Prime Minister's Office. It is necessary, therefore, to understand the definitions of the terms used by this office, particularly with reference to private companies.

Basic research: Research undertaken for the advancement of scientific knowledge, where a specific practical application is not the direct goal.

Applied research: Research undertaken primarily for the advancement of scientific knowledge, with a specific practical application in view.

Development: The use of the results available from basic and applied research, and actual experiences, directed toward the improvement of, or introduction to, new materials, equipment, systems, processes, etc.

Basic research is conducted mainly in the universities which, administratively, are under the jurisdiction of the Ministry of Education, Science, and Culture (MESC). Policy recommendations from the scientific community are voiced through the Science Council of Japan, comprised of 210 members elected from registered full-time researchers. Some recommendations have influenced MESC in the forming of its science policy, while some recommendations formed the basis for the establishment of the research institutes.

Applied research is conducted at many institutions but is most predominant at the public research institutions under various ministries. Most important are the institutions and programs of the Science and Technology Agency (STA) and the Ministry of International Trade and Industry (MITI). Other ministries operate research institutes in the areas related to their respective mandates, i.e., the Ministry of Agriculture, the Ministry of Welfare, etc.

The major impetus for development are private companies. During the past two decades, the increase in the number of researchers in the private sector has been much faster than the increase in any of the public sectors. Indeed, as will be discussed in other chapters, the number of researchers in the private sector is comparable to the total number of researchers in *all* areas of the public sector.

The following statistics are from a survey conducted in 1980:

	Research Manpower	*Research Budget (billion yen)*	*Research Institutions*
Universities	*161,910*	*128,245*	*1,889*
Public Research Institutions	*31,844*	*66,039*	*1,118*
Private Companies	*174,244*	*266,491*	*18,058*
Totals	*366,998*	*460,775*	*21,065*

Source: Sorifu Tokei Kyoku (Statistics Bureau, Prime Minister's Office, Showa 55 Nen Gakujutsu Kenkyu Chosa Hokoku). Report on the Survey of Research and Development, 1980. Tokyo:1980, pp. 3639.

The words "science" and "research" must be used carefully. Neither basic science and basic research nor applied science and applied research are coterminous. There are cases where researchers in applied science are concerned primarily and directly with the principles and the elucidation of fundamental facts. It is appropriate to classify such research as basic research, even if this research is conducted within the framework of applied science. Furthermore, there are also cases where basic researchers promote, on their own initiative, the application of their research findings. It is also appropriate to regard this as basic research in order to distinguish it from pure basic research.

On the other hand, sometimes the demand in applied science makes it difficult to accommodate the principles of basic sciences, even if it is at the level of basic research. In the basic fields of medical science, such as physiology and biology, there is a growing tendency to recruit people with scientific backgrounds. Often, it is argued, the scope of these scientists differs from the expectations of the medical people. For example, a chair on a medical faculty, in an area such as molecular biology, has frequent difficulty with the scientific faculty; therefore, a need is felt for the establishment of an independent chair, separate from both the medical and the science faculties. The difficulty of industry-university cooperation may be attributed, at least in part, to this fundamental difference in attitude and approach between the basic and the applied sciences.

It is, then, difficult or impossible to establish a distinction between basic science and applied science in terms of the nature of the research. Some standard of distinction is necessary, however, for the purpose of handling statistics as standardized figures to help show the general picture of the nationwide trend of research investment. A satisfactory solution is to use the faculties as a measure, i.e., faculties will be categorized as relating to either basic or applied science. (In the chapter entitled "Policies and Practices of Government Support for Basic and Applied Research," faculties of science and humanities are identified with basic science while the faculties of engineering, agriculture, and medicine are identified with applied science. Those faculties that do not lend themselves to the aforementioned categories are classified on an individual basis.)

When the above standard is employed, many researchers engaged in applied research may be counted under basic science, and many researchers engaged in basic research may be counted under applied science. As the faculties are the key units of university operations and as reliable figures are readily available, this method should enable identification of trends in basic and applied research.

III. DISTINCT FUNCTIONS OF UNIVERSITY FACULTY MEMBERS AND PRIVATE COMPANY RESEARCHERS

In general, it may be said that basic research is carried on in the universities, applied research is carried on in the national research institutes, and research and development is carried on in the private sector. The tradition to give weight to basic research was initiated during the last quarter of the 19th century with the establishment of the chair system at the Imperial University. The elite students who received their higher education at the Imperial University became leaders in private companies, in the civil service, or in the universities as professors. The Japanese formulated their system in accordance with the university system of Germany, then the world center of learning.

Therefore, in Japanese universities, up until·today, there was a strong tendency to respect basic science, while in Japan's private companies, where the focus of research is geared toward development, the top leaders were conscious of the importance of basic learning. The sectors of research in Japan today are: applied research, as well as research and development (all needing basic aspects). The further the research continues, the more the basic aspects are needed. In particular, when high levels of technology are implemented, very often basic research is pursued.

Laboratories in the private sector do not pursue basic research as a primary goal. They do produce some basic research achievements as by-products of their research goals. Occasional changes within research groups, and frequent changes of research goals, force company researchers to adapt themselves to a variety of situations.

The system of lifelong tenure works positively for research workers in company laboratories because the rotation in service provides workers with the mobility and with the opportunity for diversification. In the universities, the lifelong tenure system seems to have a negative effect, because faculty are assigned to the narrow, unchangeable duties which are prescribed to their chairs. Theoretically, the mandate attached to the chair binds the chairholder to the same research specialization for life. Although the chair system serves higher education very well, it deprives faculty of the opportunity for mobility and for diversification; it does not encourage the personal development of the chairholder as a researcher.

Consequently, researchers in company laboratories can adjust more easily to changing, current research needs than researchers in university laboratories. Another consequence is difficulty for cooperative research between university and company laboratories. Cooperation is maintained mostly on the basis of personal contacts and through the provision of young researchers from universities to companies. Questions of patents, different levels of research, and different systems of funding have made complete cooperation between private companies and universities impracticable.

It can be said that what the companies need from universities is not a high level achievement in basic research but a thorough education of students who, after graduation, may be remolded to suit research and development needs. There is a great demand in company laboratories for university graduates with bachelors and masters of science degrees. On the other hand, private companies feel, however, that those students who achieved a Ph.D. in science are not flexible enough to engage in target-oriented research that companies must pursue. Yet, due to the system of earning a doctoral degree by submission of a dissertation, a large number of company researchers earn doctoral degrees by their achievement in research conducted in company laboratories. This indicates that company laboratories do promote some basic research, the results of which are worthy of an earned, scientific doctoral degree.

Faculty members are given the double function of being a researcher and an educator. In terms of legislative definitions and budget allocations, the role of educator outweighs the role of researcher. But faculty members identify themselves as researchers and, in fact, their professional reputations and their promotions are usually based on their achievements in research. However, this double image adds to the difficulty of organizing extensive research groups with universities and private companies.

Secondly, it should be noted that the amount of research manpower in the universities is much smaller than it appears. This is due to: (1) the relatively small minority of faculty who are seriously engaged in research (i.e., those applying for grants-in-aid for scientific research is only about one-third of those who are qualified to apply); and (2) the fact that the bulk of research, in the universities, is performed by graduate students. This is reflected by the fact that the number of citations in the Chemical Abstracts closely parallels the number of graduate students in each university. Graduate students, then, are the major source of research manpower in the universities. Their level of research, obviously, is not likely to be more advanced than the level of research done at company laboratories where employees are given continued training, are engaged in full-time research work, and are paid higher salaries.

Thirdly, the real number of full-time research professors is very small. However, research institutions for joint use by universities are being constructed in strategic areas. Sabbatical leaves, to spend full time on research for a year, have not been institutionalized. Research opportunities for university personnel are still very limited in Japan.

Lastly, and perhaps most important to all researchers in the universities, is the distinction made between discipline-oriented researchers for fundamental knowledge of educational importance, and research that is aimed at solving specific problems of broad national and societal concern, or for opening new horizons in the foremost problems of science. Such distinction has an important advantage in enabling structural presentation of the status of nationwide research investment. It will make the public more aware of the need for increasing basic research support.

DISTINCTIONS BETWEEN BASIC AND APPLIED RESEARCH

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I. INTRODUCTION

The word "research" represents a range of human activity that has as its objective the acquisition of new knowledge and understanding. To some, including most scientists and scholars, "new" means new to all humanity, not previously understood by anyone. To others, new means new only to the inquirer; for such persons, research may simply mean consulting a reference library. In this paper I assume the former definition of research.

A standard tool of human thought is subdivision, classification, and labeling. This is an essential tool, because the ability of the human mind to comprehend complex things is limited. Big problems must usually be attacked one bite-sized chunk at a time. But, like any tool, this one must be used with care and due regard for its limitations. For example, it is common to classify research by field or discipline, e.g., mathematics, physics, biochemistry, oceanography, economics, etc. Such a classification is reflected in the very organization of many in-

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stitutions concerned with research, such as universities and government agencies. Yet, in practice, we are often forced to recognize the importance of research that does not fit neatly into a disciplinary classification. We then call it interdisciplinary research and seek ad hoc ways to support and perform it, frequently against great impediments created by the disciplinary classification system itself.

Another common way to classify research is as "basic" or "applied." The severe limitations of such an either-or classification, applied to so complex an enterprise as research, are apparent to almost everyone associated with the enterprise. Nevertheless, it has its uses, and is used. It is therefore essential that its users understand its capabilities and disabilities as thoroughly as possible.

Following is a collection of thoughts and comments about several ways to classify and label research activities. These are not the fruit of any extensive research into the matter, but are simply some personal views of a university researcher, sometime university research administrator, and amateur of science policy questions.

II. HOW (NOT) TO CATEGORIZE RESEARCH

In considering how (and whether) research can be distinguished as either basic or applied, it is helpful to think about other ways in which research might be classified and to ask whether these alternative classifications might supplement or even replace the basic-applied classification. Let us consider, then, some characteristics of research that might provide a basis for alternative classifications.

1. Freedom and Motivation of Researchers.

Researchers can have or can perceive themselves to have varying degrees of freedom to choose and pursue research problems. At one extreme, researchers may feel completely free to explore any question of interest. At another extreme, they may be or may feel constrained to address only questions that are clearly related to some rather specific objective defined by others. Of course, there are many types of possible constraints on researchers. Availability of necessary resources may impose limitations on what a researcher can attempt. A pure mathematician, for example, is arguably freer in this sense than an oceanographer or an experimental particle physicist, whose work depends on the availability of large and expensive facilities. A biomedical researcher may frequently be constrained by the ethical and legal imperatives inherent in research using human or animal subjects.

Researchers may also be motivated by a variety of objectives. They may be driven by intellectual curiosity, by a desire for a secure position, for professional recognition, or for financial reward. Most researchers are driven by all of these factors, but attribute different degrees of importance to them.

If researchers are generally free to seek new knowledge wherever their curiosity may lead, and may reasonably expect success to be rewarded by position, recognition, and freedom from financial need, but not by wealth from some commercial consequences of the research, then I believe many observers would describe such researchers as performing basic research. If, instead, researchers are constrained to explore questions relevant to rather specific objectives, defined by others, or are substantially motivated by an expectation of large financial gain from the research, then I believe many observers would describe such researchers as performing applied research.

Unfortunately, research freedom and motivation are not good characteristics on which to base a system of research classification, however important they may be in principle. Most, if not all, present classification systems use data and judgments provided by researchers' institutions or sponsors, not by the researchers themselves. One could imagine a system that depends on researchers' own assessments of their freedom and motivation, but it would surely be cumbersome and expensive.

There is a way to characterize research which indicates something about the researchers' motivations, supplemented by peer assessment of the quality and character of the research. That is to examine where the results of the research are published. A researcher must choose a specific journal to which to submit research results for publication. In doing so, he or she makes some judgments about the nature and purposes of the work and about the type of reader for which the report is intended. In addition to judging the quality of the work, the journal editor and/or reviewers usually either affirm or reject the author's assessment of the nature of the research by judging its appropriateness for publication in the journal in question. Therefore, if it were possible to classify journals as either basic or applied (as some now so classify themselves), then the place of publication would provide an automatic (although after-the-fact) classification for research on the basis of the combined judgments of researcher, editor, and knowledgeable peer reviewers.

In the foregoing I have assumed that research results are published. What if they are not? A little thought about this question leads to another possible means of classifying research in which the freedom and motivation of the individual researcher are one factor, but which also depends strongly on institutional characteristics that are relatively easy to determine.

2. Institutional Intent.

"To publish or not to publish" is a question—if not the question—on which a workable classification scheme might be based.

Almost all researchers are associated with an institution, e.g., a university, an industrial laboratory, a national laboratory, or a government laboratory. The most objectively quantifiable rewards a researcher can receive (salary incresases, promotions, and the like) are provided by the institution and based, at least partly, on assessments of merit, performance, and value to the institution. For some institutions and some researchers, the principal measure of performance is the quantity and quality of the researcher's publications in the open literature of their fields. For others, publications are not viewed as being of primary importance, and emphasis is placed on the value of the researcher's results as contributions to the internal activities of the institution. In the first case, the most important assessments of the researcher's work are made by persons outside the institution; the peers who review and judge proposals for funding and reports of research results submitted for publication, and who provide critical reviews of the researcher's work to institutional managers for use in making reward decisions. In the second case, the most important assessments of a researcher are made by persons inside the institution, the managers themselves.

It seems to me that such differences could be used in a reasonably objective way to classify research. Although they are certainly related to the motivation of the researcher, the crucial criterion is an institutional one. If an institution expects or requires researchers to publish in the open literature and judges their publications, or if, to put it bluntly, the researcher is expected to "publish or perish," then the researcher's research could be assigned to one category. If publication is of lesser importance than, say, patents or contributions to the development of particular products, then the research would go into another category. It should be relatively easy to make such assignments; all institutions have criteria, policies, and practices, either written or unwritten, for assessing their researchers. These could serve as a basis for a classification system of this sort.

Although the categories defined in this way would not be identical to basic and applied research, as they are usually defined, there would be a high degree of similarity. For example, all, or almost all, university research would fall in the first category as would some research performed in certain government and industrial laboratories. Most of the research in this category could reasonably be thought of as basic, although the category would probably be somewhat broader than the conventional basic research category.

3. Sponsor Motivation.

Most of the arguments for and against the use of researcher motivation as a means of categorizing research also apply to the use of sponsor motivation or intent. There are, of course, some important differences between the two cases. A sponsor (e.g., a federal funding agency) usually has a much greater need for categorization than a researcher and, at the same time, is probably less well equipped to make the necessary distinctions. Where a sponsor has a clear expectation regarding the disposition of research results, i.e., open publication or not, some of the considerations discussed in the previous section on institutional intent would apply.

In passing, one may note the possible relevance of the Grants and Cooperative Agreements Act (P.L. 95-224). This piece of legislation, as I understand it, is intended as a first step toward rationalization and simplification of the whole federal system for funding non-federal entities. It defines two modes of funding—assistance and procurement—and three types of funding instruments—grants, cooperative agreements, and contracts. The former two types would generally be appropriate for assistance, and the latter for procurement. The act mandated a massive study by the Office of Management and Budget (OMB) of the implications of the act and how it might be implemented in various circumstances. This study is now in its final stages. Applications of the act to research will require distinguishing different kinds of research and matching them to appropriate funding modes and mechanisms. In a simple world one might hope to achieve a consistent categorization of research in terms of such modes and mechanisms, but it is my impression that the Grants and Cooperative Agreement Act is not likely to reduce the difficulty of categorizing research, and may add to it.

4. Generality of Problems or Results.

One of the characteristics of major research results, those sometimes called "breakthroughs," is their generality. A mathematician may discover the key to dealing with a whole class of mathematical problems; a physicist may discover that many apparently unrelated elementary particles are composites of a few even more elementary particles; or a virologist may uncover a biochemical process common to all disease-causing viruses. Research results that are specific to a very narrow subject area and are not generalizable to a broader area are often considered to be less valuable. A research finding that is very general is quite likely to be fundamental. Unfortunately, it is hard to argue that a research finding which is not general is therefore applied. Furthermore, even if we were to assume that the generality of a research finding could be related to the basic-applied nature of research that produced it, we would at best have a rule for classifying research which could be applied only after the research was done and the results were evaluated. Then there is the fact that research that everyone would agree is applied sometimes produces very general, basic results. Some of these difficulties vanish if we are willing to accept as a measure of basicness the generality of the problem addressed by research rather than the results, but there remains the difficulty that non-general does not necessarily imply applied. It is obviously possible to classify research as general or specific, but such a classification seems not to be simply related to the basic-applied classification.

5. Relative Simplicity of Subject Systems.

There is a sense in which the relative simplicity of the systems investigated by a type of research is related to the degree to which the research can be characterized as basic. For example, many physicists would argue that elementary particle physics, which is concerned with understanding the ultimate forces and constituents in the universe and which usually studies interactions among very small numbers of fundamental particles, is more basic and less applied than condensed matter physics, which is concerned with the behavior of aggregations of very large numbers of fundamental particles. This kind of argument is sometimes used to order disciplines along a scale from applied to basic (sometimes, concurrently, from soft to hard, or from low-caste to high-caste), e.g., "Sociology is applied psychology is applied biology is applied chemistry is applied physics is applied mathematics is the queen of sciences." The flaws in this sort of characterization are many; it sheds little light on the basic-applied question.

6. Nature and Timing of Expected Benefits.

The case for public support of both basic and applied research is commonly made on the basis of expected benefits to society. Applied research is expected to lead to useful technology in the short term. Basic research is expected to do so over the long term and is sometimes also justified on grounds of cultural or aesthetic benefit. There is some merit in these associations between the nature of the expected benefits and the character of the research but, in my view, not enough to make them a basis for a consistent basic-applied classification. There are just too many exceptions to the rule. For example, the central thrusts of the United States fusion program can be viewed as applied research, yet a useful technology is almost surely several decades away. Basic research frequently leads to technology on a shorter time scale. Another example: Although most would consider the research which led from the early transistor to today's large-scale integrated circuits applied rather than basic, it is yielding cultural and aesthetic benefits (positive and negative) on a large scale.

There are many other characterizations of research which might be used as a basis for categorizing types of research. None of them is really congruent to the basic-applied classification in the several forms in which it is currently used. If one insists on having a single, two-valued classification, in which individual researchers or research projects can be labeled on the basis of reasonably well-defined, objective, and perhaps even documentable characteristics, then the scheme discussed above under the heading "Institutional Intent" appears to offer several advantages. It is based on readily verifiable institutional characteristics which can be functional only if concurred in by both the researcher and the external sponsor (if any) of a research project. Hence, it reflects to some degree the motivation and intent of all three interested parties. It offers high resolution in that it would, in principle, make possible the classification of individual research groups, researchers, or research projects associated with a single institution or sponsor (e.g., different groups within an industrial research laboratory). The scheme requires answers to two basic questions. "Does the institution (and, where applicable, the sponsor) expect that the principal result of the research will be information which will be made available to all, freely and expeditiously, through publication in the open literature?" "Is the performance of researchers assessed by the institution (and, where applicable, by the sponsor), primarily on the basis of the quantity and (peer-judged) quality of their publications?" In most cases it should be possible to answer these questions with a reasonable degree of objectivity. Finally, although this scheme would provide classifications somewhat different from present basic-applied classifications, I believe there would be a high degree of similarity.

Perhaps the most important lesson to come from consideration of the several ways one might categorize research is that any classification which yields a single, two-valued parameter (basic or applied) must be incomplete and may be dangerously misleading. There is a fundamental difficulty in attempting to characterize research in this way or even by a continuous but one-dimensional scale with, say, basic on one end and applied on the other. Research is a human activity, a rather personal and individual human activity, moreover, and therefore exhibits all the complexity and multiple varied characteristics one expects of a human activity. To attempt to characterize it simply and starkly as basic and applied is about as accurate as characterizing a human being using nothing but the words big or small. One is reminded (at least if one is a physicist) of the problem of characterizing a physical event. This at least requires specifying its time and place, and that requires four numbers any way you slice it. One number simply won't do. This line of thought leads one to suggest that some serious consideration be given to the question of how one might devise a multi-parameter scheme for characterizing research. As a management tool it would, depending on one's point of view, suffer from the defect of increased complexity or have the advantage of reduced oversimplification. But if modern managers can understand and practice matrix management, they can surely cope with a matrix classification of research. I'm less sanguine about researchers' abilities to do so, but nevertheless I think this is a question worth pursuing.