PASTEUR'S QUADRANT

Basic Science and Technological Innovation

Donald E. Stokes

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FOREWORD

MORE THAN FIFTY YEARS ago, Vannevar Bush released his enormously influential report, *Science, the Endless Frontier*, which asserted a dichotomy between basic and applied science. This view was at the core of the compact between government and science that led to the golden age of scientific research after World War II—a compact that is currently under severe stress. In this book, Donald E. Stokes challenges Bush's view and maintains that we can only rebuild the relationship between government and the scientific community when we understand what is wrong with that view.

Stokes begins with an analysis of the goals of understanding and use in scientific research. He recasts the widely accepted view of the tension between understanding and use, citing as a model case the fundamental yet use-inspired studies by which Louis Pasteur laid the foundations of microbiology a century ago. Pasteur worked in the era of the "second industrial revolution," when the relationship between basic science and technological change assumed its modern form. During subsequent decades, technology has been increasingly science based—with the choice of problems and the conduct of research often inspired by societal needs.

On this revised, interactive view of science and technology, Stokes builds a convincing case that by recognizing the importance of use-inspired basic research we can frame a new compact between science and government. His conclusions have major implications for both the scientific and policy communities and will be of great interest to those in the broader public who are troubled by the current role of basic science in American democracy.

Having put the final touches on his manuscript, Donald E. Stokes died of acute leukemia on January 26, 1997. At the time of his death, he was professor of politics and public affairs in the Woodrow Wilson School of Public and International Affairs at Princeton University. Stokes served as dean of the school between 1974 and 1992.

At Brookings, Theresa Walker edited the manuscript, Matthew Atlas and Tara Adams Ragone verified it, Inge Lockwood proofread it, and Julia Petrakis prepared the index.

The views expressed in this book are solely those of the author and should not be ascribed to the trustees, officers, or other staff members of the Brookings Institution.

Michael H. Armacost
President

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Washington, D.C.

PREFACE

THE PROBLEM I EXPLORE in this book first caught my eye when I was dean of the graduate school at the University of Michigan, a role that was, among other things, a walking subscription to *Scientific American*.

As I made my rounds of a number of scientific fields I was struck by how often a gifted scientist would talk about the goals of research—especially the relationship between the quest of fundamental understanding on the one hand and considerations of use on the other—in a way that seemed to me odd. Odd and unhelpful, since my preceptors' view of this relationship and of the relationship between the categories of basic and applied research derived from these goals kept them from seeing things I felt they needed to see.

This reaction was strongly reinforced when I served for several years on a council advising the director of the National Science Foundation and heard this same formulation on a number of occasions. One morning, as an eminent scientist again voiced these beliefs, I so startled the council with an alternative view that my ideas were projected as an overhead slide at the beginning of the afternoon session. An updated version of this slide appears in some of the figures in chapter 3. The Foundation widened its complicity by publishing a statement of the argument I sent the director.[1] I had a chance to explore other parts of the problem when I chaired a National Research Council panel that studied the federal government's support of research on social problems.[2]

My interest in the problem was kept alive by serving for a number of years as dean of the Woodrow Wilson School at Princeton. The research efforts of this school so clearly involved the interplay of understanding and use in the social sciences that no one could lead such a unit without thinking very deeply about this relationship, and I draw liberally here on the experience of the school's Office of Population Research and Research Program in Development Studies. Eventually I came to believe that these issues deserved to be explored in a book-length work.

It took somewhat longer to be convinced that I should write it, since the early chapters deal with elements of the history of science and of intellectual history in which I began with no particular advantage. But the issues I raise have implications for three things in which I have been directly involved—the building of research agendas, the creation of institutional settings for research, and the channeling of research support. The latter chapters of this book trace the implications of a revised view of the relationship between basic science and technological innovation for each of these areas of science policy.

No one should write such a book, least of all a book that cuts across a number of fields, without a clear idea of who might read it. The argument I set out is of natural interest to those who deal with science and technology policy in and outside of government and to members of the scientific community within the universities, the government, and the free-standing research institutes and firms. Since I draw on the experience of several of the countries of the industrial world, my argument may be of interest to the science and policy communities in these countries as well. And since I pass familiar light through new prisms, my argument may also be of

1. Donald E. Stokes, "Making Sense of the Basic/Applied Distinction: Lessons for Public Policy Programs," in *Categories of Scientific Research,* papers presented at 1979 National Science Foundation seminar, Washington.

2. The principal report of this study is National Research Council, *The Federal Investment in Knowledge of Social Problems* (National Academy of Sciences, 1978).

interest to historians of science and historians of ideas, however synthetic my scholarship in these fields may be.

Social scientists will recognize this as a work of social science. Indeed, my political science colleagues will have no difficulty seeing it as a work of political and institutional analysis. But my argument extends to research in all scientific fields—including the physical sciences and engineering, the biological and biomedical sciences, and the social sciences—since there is a unity to science in the respects that are critical to my argument. But this carries no implication that the sciences are in all respects the same; and certainly none that social science is as close to natural science as biology, say, is to physics.

I could not have sharpened my argument without the help of many friends and colleagues. Too many have lent their wisdom and encouragement for me to acknowledge them all. My special appreciation goes to a number of my Princeton colleagues, including Clinton Andrews, Peter Eisenberger, Harold Feiveson, Charles Gillispie, Frank von Hippel, Daniel Kammen, Walter Kauzmann, Michael Mahoney, Harold Shapiro, Robert Socolow, Thomas Spiro, Thomas Stix, and Norton Wise; if nothing else, this book is a tribute to the intellectual commerce within this university. Of the many members of the "invisible college" who have offered insight and encouragement from a distance, a special debt is owed to the remarkable Harvey Brooks, who read the manuscript with care and deep insight. I would also especially mention Max Kaase, Richard Nelson, Stephen Nelson, Albert Teich, and John Servos. I have benefited from the help of a number of people in government, including Jennifer Sue Bond, Patricia Garfinkel, and Carlos Krytbosch.

Carolyn North, my research assistant for two periods, gathered the materials for this work with intelligence, insight, and care. Mary Huber prepared the ground for this effort, and Betsy Shalley-Jensen, Robert Sprinkle, Frank Hoke, Chris Thompson, Michael McGovern, and Esra Diker skillfully grasped the baton as it was passed to them. I am greatly indebted to each of them.

I want to acknowledge my debts to four research organizations that have lent me invaluable assistance. In the autumn and winter of 1992–93 the Research Institute of International Trade and Industry in Tokyo helped to open a window to Japan's experience

with science and technology policy. In the spring of 1993 the Royal Society of London and the Science Policy Research Unit of the University of Sussex deepened my insight into the experience of Britain and Europe. I am very grateful indeed to Peter Collins and Mike Ringe at the Royal Society and to Christopher Freeman, Michael Gibbons, Diana Hicks, Ben Martin, Keith Pavitt, Margaret Sharp, and their colleagues at Sussex.

Finally, Bruce MacLaury, the president of the Brookings Institution, and Thomas Mann, the director of its Governmental Studies Program, offered unfailing support as I pursued a project that has ranged freely over the fields of science and technology, the several millennia of the Western experience of science and scientific philosophy, and the contemporary approaches to science and technology policy taken by the major countries of the industrial world. I am grateful to them and to Paul Peterson, Thomas Mann's predecessor, and my other interim Brookings colleagues. Because the Brookings Institution's own mission so clearly involves the goals both of understanding and use, it proved to be an ideal location in which to reduce my analysis to a written text.

Donald E. Stokes
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the links between basic science and technological innovation. Chapter 4 shows how this revised view could help renew the compact between science and government. Chapter 5 seeks a process by which American democracy can build agendas of use-inspired basic research by bringing together judgments of research promise and societal need.

The analysis begins with the nature of basic and applied research, since the relationship of research inspired by the quest for understanding and research inspired by considerations of use helps to define our essential problem. As the analysis unfolds, we will see where the prevailing paradigm is faithful to, and where it distorts, the real interplay of the goals of science and the links between basic science and technological innovation.

The Concepts of Basic and Applied Research

Research proceeds by making choices. Although the activities by which scientific research develops new information or knowledge are exceedingly varied, they always entail a sequence of decisions or choices. Some of these have to do with the choice of problem area or particular line of inquiry, some with the construction of theories or models, some with the derivation of predictions, deductions, or hypotheses, some with the development of instruments or measures, some with the design of experiments and the observation of data, some with the use of analytic techniques, some with the selection of follow-on inquiries, some with the communication of the results to other scientists. Harvey Brooks caught this universal aspect of research when he said that "any research process can be thought of as a sequential, branched decisionmaking process. At each successive branch there are many different alternatives for the next step."[8] The distinction between basic and applied research turns on the criteria that govern the choice among these alternatives.

Three observations set our argument in motion. The initial observation is this:

> The differing goals of basic and applied research make these types of research conceptually distinct.

On any reasonable view of the goals of basic and applied research, one cannot doubt that these categories of research are conceptually different. The defining quality of basic research is that it seeks to widen the understanding of the phenomena of a scientific field. Although basic research has been defined in many ways and involves the extraordinarily varied steps just suggested, its essential, defining property is the contribution it seeks to make to the general, explanatory body of knowledge within an area of science. In keeping with this conception, the Organization for Economic Cooperation and Development defines basic research as "experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundation of phenomena and observable facts," although the OECD definition adds a disclaimer as to practical use to which we will return.[9] Sometimes basic research is defined in terms of certain correlates on which it differs from applied research, such as originality, the freedom of researchers, peer evaluation of published results, and length of time between discovery and practical use. But these corollary properties ought not to be taken for the characterizing quality of basic research—its thrust toward a wider understanding of the phenomena of a field.

This quality can be found in any number of examples from the annals of research. One that is useful for the further discussion is supplied by the study that launched the scientific career of Louis Pasteur when the enigma of racemic acid caught his eye as a student at the *École Normale Supérieure* in Paris. The Berlin chemist Mitscherlich had found that two remarkably similar acids, tartaric and paratartaric (or racemic) acid, had very different actions on light, since tartaric acid rotated a plane of polarized light through a characteristic angle whereas racemic acid did not—despite the fact that the two appeared to be identical in chemical composition, crystalline form, specific weight, and other properties.

Mitscherlich's report of this anomaly plunged Pasteur into the search for an explanation. When he turned his microscope on crystals made from racemic acid he found that they were of two forms, one identical to crystals of tartaric acid, the other their mirror image. Separating the two he found that a solution of the crystals identical to the tartrates rotated the plane of polarized light exactly as tartaric acid did, whereas a solution of the mirror-image crystals rotated the plane by the identical angle in the *opposite* direction. A solution with equal proportions of the two was optically neutral, deflecting the plane of polarized light not at all. Pasteur's excited *"tout est trouvé"* took its place in the litany of scientific discovery. He had indeed solved the problem by showing that racemic acid is composed of two isomeric forms whose equal and opposite actions on light canceled each other when the two were combined. His research, guided at each stage by the quest of understanding, had extended the frontiers of crystallography.

If basic research seeks to extend the area of fundamental understanding, applied research is directed toward some individual or group or societal need or use. This quality is illustrated well by an applied problem from Pasteur's subsequent career, his effort to cope with the persistent difficulties experienced by those who made alcohol from beets. These difficulties led an industrialist in the Lille region to seek his help. As the dean of the local Faculty of Science Pasteur had encouraged his students to do practical work in industry before pursuing industrial careers. He visited a factory and took samples of the fermenting beet juice to his laboratory for microscopic examination.

Threading his way through a maze of scientific misconceptions, Pasteur identified the microorganisms responsible for fermentation and showed that they could survive without free oxygen—indeed, that they produced the alcohol resulting from fermentation by wresting oxygen from the sugar molecules in the fermenting juice. This insight gave his industrial clients an efficient means of controlling fermentation and limiting spoilage. James Bryant Conant, in his case study of this work by Pasteur, notes that one of the most valuable properties of applied research is "reducing the degree of empiricism in a practical art."[10] Pasteur's study dramatically reduced the degree of empiricism in the industries using fermentation.

If the goal of basic research is, in a word, understanding, and of applied research, use, it cannot be doubted that these types of research are conceptually or analytically different. But the prevailing view of scientific research often includes a further element,

one that leads to the second observation that sets our argument in motion:

> An inherent tension between the goals of general understanding and applied use is thought to keep the categories of basic and applied research empirically separate.

A particular piece of research will, on this view, belong to one or the other of these categories but not both. This was Bush's view in *Science, the Endless Frontier* when he spoke of "a perverse law governing research," under which *"applied research invariably drives out pure."*[11] An inherent conflict between the goals of basic and applied research is thought to preserve an empirical boundary between the two kinds of inquiry.

This view did not spring Athena-like from Bush's brow after the war; in chapter 2 the idea of pure inquiry is traced through two millennia. But the perceived conflict between the goals of basic and applied research has rarely been so clearly spelled out as it was in Bush's report. The separateness of basic and applied research implied by this presumed conflict is an idea that is woven into the dominant paradigm of science and technology policy and perceptions of science held in government, the research community, and the communications media.[12] It is impossible to go through the commentaries on science of recent decades without sensing how deeply this idea pervades our outlook on scientific research. The belief that basic and applied research are separate categories also has a considerable history, and chapter 2 shows how it has been reinforced by the institutional development of science in Europe and America in the nineteenth and twentieth centuries.

Static and Dynamic Forms of the Paradigm

The belief that understanding and use are conflicting goals—and that basic and applied research are separate categories—is captured by the graphic that is often used to represent the "static" form of the prevailing paradigm, the idea of a spectrum of research extending from basic to applied:

Basic | Applied

This imagery in Euclidean one-space retains the idea of an inherent tension between the goals of understanding and use, in keeping with Bush's first great aphorism, since scientific activity cannot be closer to one of these poles without being farther away from the other.

The distinctness of basic from applied research is also incorporated in the dynamic form of the postwar paradigm. Indeed, the static basic-applied spectrum associated with the first of Bush's canons is the initial segment of a dynamic figure associated with Bush's second canon, the endlessly popular "linear model," a sequence extending from basic research to new technology:

Basic research → Applied research → Development → Production and operations

The belief that scientific advances are converted to practical use by a dynamic flow from science to technology has been a staple of research and development (R&D) managers everywhere. Bush endorsed this belief in a strong form—that basic advances are the *principal* source of technological innovation, and this was absorbed into the prevailing vision of the relationship of science to technology. Thus an early report of the National Science Foundation commented in these terms on this "technological sequence" from basic science to technology, which later came to be known as "technology transfer":

> —The technological sequence consists of basic research, applied research, and development. . . .
>
> —Basic research charts the course for practical application, eliminates dead ends, and enables the applied scientist and engineer to reach their goal with maximum speed, directness, and economy. Basic research, directed simply toward more complete understanding of nature and its laws, embarks upon the unknown, [enlarging] the realm of the possible.
>
> —Applied research concerns itself with the elaboration and application of the known. Its aim is to convert the possible into the actual, to demonstrate the feasibility of scientific or engineering development, to explore alternative routes and methods for achieving practical ends.
>
> —Development, the final stage in the technological sequence, is the systematic adaptation of research findings into useful materials, devices, systems, methods, and processes. . . .
>
> *From these definitions it is clear that each of the successive stages depends upon the preceding [one].*[13]

If production and operations, the final stage of converting basic science into new products or processes, is added, the linear model is produced. This sort of dynamic linear-model thinking gave rise to the Department of Defense's categories for R&D, which soon accounted for the major share of postwar federal spending on research. Together with its equally linear static corollary, the basic-applied spectrum, this dynamic linear image provided a general paradigm for interpreting the nature of research, one that is remarkably widespread in the scientific and policy communities and in popular understanding even today.[14]

The diffusion of this paradigm in the postwar world is suggested by another voice in another place. Keith A. H. Murray, longtime rector of Lincoln College, Oxford, and chairman of Britain's University Grants Committee, instructed Australia's prime minister, Robert Menzies, and the government colleagues of Menzies on the needs of Australia's universities in the second decade after the war. The 1957 report of the Murray Committee said in part:

> It is obvious that most of the basic secrets of nature have been unravelled by men who were moved simply by intellectual curiosity, who wanted to discover new knowledge for its own sake. The application of the new knowledge usually comes later, often a good deal later; it is also usually achieved by other men, with different gifts and different interests.[15]

This declaration expresses both the belief that basic and applied research are separate ventures, pursued by different people "with different gifts and different interests," and the belief in the priority in time of the discoveries of basic science.

As the validity of these beliefs is examined, one must remember that the goals defining the categories of basic and applied research by no means exhaust the motives driving the scientific enterprise. Those who have offered general or particular accounts of the motives of research scientists paint an extraordinarily diverse portrait of the actual incentives for research. Some of these are strongly joined to the normative structure of science, as Robert K. Merton's classic study of the race for priority in scientific publication shows.[16] But the presence of other motives for research does not diminish the importance of deeply probing the relationship between the goals of understanding and use, since the postwar paradigm is characterized by the belief that these goals are necessarily in tension and the categories of basic and applied research necessarily separate as well as by the belief that innovations in technology have their source in advances in basic science.

The Experience of Science

It is possible to form a very different view of these relationships from the annals of research, and the third observation completes the statement of the problem:

> The belief that the goals of understanding and use are inherently in conflict, and that the categories of basic and applied research are necessarily separate, is itself in tension with the actual experience of science.

Although a great deal of research is wholly guided by one or the other of the goals of understanding and use, some studies of great importance show that the successive choices of research are influenced by *both* these goals.

This possibility is strikingly illustrated by the rise of microbiology in the nineteenth century; the examples from Pasteur's work were deliberately chosen. No one can doubt that Pasteur sought a fundamental understanding of the process of disease, and of the other microbiological processes he discovered, as he moved through the later studies of his remarkable career. But there is also no doubt that he sought this understanding to reach the applied goals of preventing spoilage in vinegar, beer, wine, and milk and of conquering *flacherie* in silkworms, anthrax in sheep and cattle, cholera in chickens, and rabies in animals and humans.

This mix of goals was not visible in the young Pasteur. The 22-year-old chemist who immersed himself in the enigma of racemic acid was engaged in a pure quest of understanding. Yet as Pasteur went to work on this enigma, he caught sight of a further puzzle, the question of why racemic acid mysteriously appeared in some places and not in others. He strongly suspected that microscopic agents were at work, and this conjecture greatly enhanced his interest in the microorganisms he found responsible for fermenting beet juice into alcohol in his studies at Lille. As he pursued this research, he began to fashion a framework for understanding a whole new class of natural phenomena, and he obtained the strikingly original result that certain microorganisms were capable of living without free oxygen. This work launched his assault on the medieval doctrine of the spontaneous generation of life and led to the brilliant later studies in which he developed the germ theory of disease. Hence, as Pasteur's scientific studies became progressively more fundamental, the problems he chose and the lines of inquiry he pursued became progressively more applied.

The problem of deriving alcohol from beet juice makes this point well. Pasteur's work on this problem is, as Conant noted, a distinguished example of applied research, a highly successful effort to improve the technology of fermentation. But the study that Conant called a prime example of applied research was at the same time a distinguished example of basic research. This blend characterized virtually the whole of Pasteur's later career. He probed ever more deeply into the processes of microbiology by accepting applied problems from a Lille industrialist, from the minister of agriculture, even from the Emperor Napoleon III—and, in a case that did much to build the Pasteur legend, from the distraught mother of a child bitten by a rabid dog. Many of his detailed lines of inquiry, such as the experiments by which he developed the process of the "pasteurization" of milk or his experiments in growing attenuated bacterial strains to immunize patients from disease, are unintelligible apart from his applied goals. The mature Pasteur never did a study that was not applied, as he laid out a whole new branch of science.[17]

Pasteur's example was by no means unique. Across the English Channel, Kelvin's physics was inspired by a deeply industrial view and the needs of Empire.[18] Across the Rhine, the German organic esting one because this problem focus became, if anything, sharper as the quest for understanding moved to deeper levels. In demography's early years its research agendas came under heavy pressure from those who wanted to support quick action programs. At this stage a small core of research demographers pulled back and pursued a far more fundamental research agenda, partly by developing highly sophisticated mathematical models of population replacement. The worth of this strategy of pursuing applied goals through fundamental understanding was borne out when these models were refined after World War II for the limited fertility and mortality data of third world countries—and revealed for the first time, fewer decades ago than we now remember, the staggering force of the population explosion that lay in store.[24]

Science and Technology

The examples from the history of science that contradict the static form of the postwar paradigm call into question the dynamic form as well. If applied goals can directly influence fundamental research, basic science can no longer be seen only as a remote, curiosity-powered generator of scientific discoveries that are then converted into new products and processes by applied research and development in the subsequent stages of technology transfer. This observation, however, only sets the stage for a more realistic account of the relationship between basic science and technological innovation.

Three questions of increasing importance arise about the dynamic form of the postwar paradigm. The least important is whether the neatly linear model gives too simple an account of the flows from science to technology. An irony of Bush's legacy is that this one-dimensional graphic image is one he himself almost certainly never entertained. An engineer with unparalleled experience in the applications of science, he was keenly aware of the complex and multiple pathways that lead from scientific discoveries to technological advances—and of the widely varied lags associated with these paths. The technological breakthroughs he helped foster during the war typically depended on knowledge from several, disparate branches of science. Nothing in Bush's report suggests that he endorsed the linear model as his own.[25]

The spokesmen of the scientific community who lent themselves to this oversimplification in the early postwar years may have felt that this was a small price to pay for being able to communicate these ideas to a policy community and broader public for whom science was always a remote and recondite world of affairs. This calculation may well have guided the draftsmen of the second annual report of the National Science Foundation as they stated the linear model in the simplistic language quoted earlier in this chapter. In any case, these spokesmen did their work well enough that the idea of an arrow running from basic to applied research and on to development and production or operations is still often thought to summarize the relationship of basic science to new technology. But it so evidently oversimplifies and distorts the underlying realities that it began to draw fire almost as soon as it was widely accepted.

Indeed, the linear model has been such an easy target that it has tended to draw fire away from two other, less simplistic misconceptions imbedded in the dynamic form of the postwar model. One of these was the assumption that most or all technological innovation is ultimately rooted in science. If Bush did not subscribe to a linear image of the relationship between science and technology, he *did* assert that scientific discoveries are the source of technological progress, however multiple and unevenly paced the pathways between the two may be. In his words,

> new products and new processes do not appear full-grown. They are founded on new principles and new conceptions, which in turn are painstakingly developed by research in the purest realms of science.[26]

Even if we allow for considerable time lags in the influence of "imbedded science" on technology, this view greatly overstates the role that science has played in technological change in any age. In every preceding century the idea that technology is science based would have been false. For most of human history, the practical arts have been perfected by " 'improvers' of technology," in Robert P. Multhauf's phrase, who knew no science and would not have been much helped by it if they had.[27] This situation changed only with the "second industrial revolution" at the end of the nineteenth century, as advances in physics led to electric power, advances in chemistry to the new chemical dyes, and advances in microbiology to dramatic improvements in public health. But a great deal of technological innovation, right down to the present day, has proceeded without the stimulus of advances in science. Chapter 2 reviews evidence that developments in military technology, an area in which America remained pre-eminent in the postwar decades, proceeded without much further input from basic science. And in recent decades, Japan has achieved its position in such markets as automobiles and consumer electronics less because of further applications of science than because of its thinking up better products and making good products better through small and rapid changes in the design and manufacturing process, which were guided by customer reaction and considerations of cost.[28]

But the deepest flaw in the dynamic form of the postwar paradigm is the premise that such flows as there may be between science and technology are uniformly one way, *from* scientific discovery *to* technological innovation; that is, that science is *exogenous* to technology, however multiple and indirect the connecting pathways may be. The annals of science suggest that this premise has always been false to the history of science and technology. There was indeed a notable *reverse* flow, from technology to science, from the time of Bacon to the second industrial revolution, with scientists modeling successful technology but doing little to improve it. Multhauf notes that the eighteenth-century physicists were "more often found endeavoring to explain the workings of some existing machine than suggesting improvements in it."[29] This other-way-round influence is called the oldest type of interaction of science and technology by Thomas S. Kuhn, who notes that Johannes Kepler helped invent the calculus of variations by studying the dimensions of wine casks without being able to tell their makers how to improve their already optimal design—and that Sadi Carnot took an important step toward thermodynamics by studying steam engines but found that engineering practice had anticipated the prescriptions from the theory he worked out.[30]

This situation was fundamentally altered from the time of the second industrial revolution, in two respects. One is that at least in selected areas, science was able to offer a good deal to technology, and this trend has accelerated in the twentieth century, with more and more technology that *is* science-based. But the other, complementary change, one that is much less widely recognized, is that developments in technology became a far more important source of the phenomena science undertook to explain. This was much more than a matter of instrumentation, which has loomed large in science at least since the time of Galileo. It was rather that many of the structures and processes that basic science explored were unveiled only by advances in technology; indeed, in some cases *existed only in* the technology. Hence, more and more *science* has become *technology* derived.

This development was illustrated by the research of Irving Langmuir on the surfaces of the devices being produced by GE and the other electronics firms of his day. It would not be right to say that the several-billion-year history of the universe had produced no analogs of the surfaces that so fascinated Langmuir. But neither humankind nor its scientific community had seen them until they were unveiled by the advancing technology of the electronics industry. By working out their physics, Langmuir earned a Nobel Prize in 1932 as he also cleared the way for significant further advances in the technology itself. In Leonard S. Reich's view, Langmuir felt that "understanding the principles of the physical world and making improvements to technology were part of the same venture" and that his "concern with applicability gave considerable direction to his research," influencing his choice of apparatus, analytical method, and conceptual outlook.[31] The developing technology of the electronics industry revealed the physical phenomena he probed, and his understanding of molecular interaction in crystals and surface films led to important advances in the technology.

A contemporary example of fundamental research that is technology based is provided by the work of the condensed-matter physicists who are seeking the fresh scientific knowledge that will allow semiconductors to be grown atomic layer by atomic layer. Although the knowledge laid down by the creators of solid-state physics between the wars was essential to understanding the transistor when it was discovered after World War II, what then transpired was more a triumph of technology than of science as the semiconductors moved through their successive generations, with astonishing reductions in scale and increases in speed. The miniaturization has now carried to the point where it may be possible to convey information by the location of individual electrons. But for this a fresh advance in fundamental knowledge will be needed—to see, for example, whether in circuits that consist of many quantum dots or wells an electron can behave simultaneously as a wave and particle, a finding that can be enormously important both for fundamental physics and for future technology.

The influence of technology on the course of basic science is clear in technological innovations in *processes* as well as products. This has characterized the role of medical practice in the advances of biological science. The evolving but incomplete technology of epidemic control in the nineteenth century influenced the use-inspired basic science of Pasteur. As Bruno Latour has shown, Pasteur lent a cutting edge to the broad public hygiene movement in France and Britain, whose calls to action had been frustratingly unconvincing before his discoveries on the sources of disease armed the movement with an adequate theory of the problem.[32] A further example described by Judith P. Swazey and Karen Reeds is the emergence of endocrinology from the work of clinical physicians concerned with the malfunction of particular glands.[33] In the latter part of the nineteenth century these physicians had observed a series of disorders such as diabetes, goiter, and cretinism that are now known to be glandular in origin. They connected their observation of these disorders with the anatomists' discovery of a series of ductless glands in the human body. Thomas Addison, the London physician who gave his name to Addison's disease, helped establish this link by recognizing that patients who had the symptoms of this disease also exhibited pathological changes in the adrenal glands. Another pioneer was the French physician Pierre Marie, who linked the appearance of the coarse and elongated features of acromegalic patients with pathological changes in their pituitary glands. In a similar way diabetes was linked to disorders of the pancreas—and myxedema and cretinism to disorders of the thyroid.

The research launched by these observations laid the foundations of the modern field of endocrinology, which has worked out the chemical regulation of physiological processes through the endocrine system. By the early twentieth century these studies had established that the ductless glands secreted directly into the bloodstream various hormones essential to the physiology of the body; the rival hypothesis that these organs detoxified the blood was decisively rejected. By the 1920s and 1930s this growing field had provided an understanding of the complex interactions of the several glands of the endocrine system; by the end of World War II, of the relationship between the endocrine and nervous systems. In recent decades attention has centered on the molecular processes by which cells and organs receive hormonal direction. Clinical observation of disturbances in the endocrine system and successful intervention in the process of disease have been as influential on research in the recent past as they were in the time of Addison and Marie. Pathologies have proved to be both a continuing source of insight into the system's normal functioning and a motive for extending basic knowledge.

Who Reaps the Technological Harvest from Science?

Experience also reveals as problematic the third element we have identified in Bush's conceptual system, the idea that a country can expect to capture the return in technology from its investment in basic science. A skeptic seated at Bush's elbow when he penned his claim that "a nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade" might have pointed out that elsewhere *Science, the Endless Frontier* noted that the United States reached the front rank in industrial technology when it was still far behind Europe in basic science:

> In the nineteenth century, Yankee mechanical ingenuity, building largely upon the basic discoveries of European scientists, could greatly advance the technical arts.[34]

The question of who reaps the technological rewards from advances in basic science was scarcely asked in the postwar world, with the United States so in the ascendancy in both science and technology.

But the world could scarcely miss this lesson now that the Jap-

3 TRANSFORMING THE PARADIGM

HALF A CENTURY has passed since Vannevar Bush articulated the paradigm view of basic science and its role in technological innovation that was absorbed into the thinking of the scientific and policy communities after World War II. This framework of understanding, partly inspired by the ideal of pure inquiry in Western scientific philosophy and reinforced by the institutional separation of pure from applied science and by the postwar interests of the scientific community, has influenced science and technology policy over much of the succeeding period.

Yet this framework has come under heavy pressure as the policies to which it led seem less adequate for the needs of a different era. Indeed, these doubts have appeared in each of the major industrial countries. It is no longer believed that a heavy investment in pure, curiosity-driven basic science will by itself guarantee the technology required to compete in the world economy and meet a full spectrum of other societal needs. Britain, for example, issued a May 1993 White Paper on science and technology policy which flatly stated, "The Government does not believe that it is good enough simply to trust to the automatic emergence of appli-

cable results [from basic research] which industry then uses."[1] In each of the industrial countries interest in harnessing science for the technological race is increasing, and this interest helps to create a climate that is receptive to a fundamental critique of the postwar framework for thinking about science and technology.

Early Dissents

Once the prevailing paradigm is challenged, it is not difficult to find early observers who tried to reshape its one-dimensional images, seeking like Michelangelo to release the conceptual angel from the surrounding marble. Such an early sculptor was James B. Conant, who as Harvard's president served as one of Bush's closest colleagues during the war. Conant declined to be named the founding director of the National Science Foundation when the new agency was created in 1950. But he agreed to join the National Science Board and was elected its chairman. His foreword to the Foundation's first annual report included this notably heterodox view:

> No one can draw a sharp line between basic and applied research and the Foundation will support many investigations that might be classed in one area or the other. Indeed, speaking for myself and not for the Board, I venture to suggest that we might do well to discard altogether the phrases "applied research" and "fundamental research." In their place I should put the words "programmatic research" and "uncommitted research," for there is a fairly clear distinction between a research program aimed at a specific goal and an uncommitted exploration of a wide area of man's ignorance. It would be safe to say that all so-called applied research is programmatic *but so, too, is much that is often labeled fundamental.*[2]

Conant made clear that this view was his own and not the board's. Well he might, since the Foundation's annual reports stressed the importance of the "technological sequence." Conant avoided a direct clash with Bush by substituting "fundamental" for "basic." But Conant understood these terms to refer, interchangeably, to all research that seeks to extend understanding within a scientific field—and therefore to include more than the curiosity-driven science that he called "uncommitted research" and Bush had called basic research.[3] Indeed, by refusing to equate "fundamental" with "uncommitted" Conant recognized a cross-cutting relationship between the goals of understanding and use, one that divides basic or fundamental research into *programmatic* work that is influenced by considerations of use and *uncommitted* work that is a pure voyage of discovery.

The idea of dividing basic research according to whether or not it also is inspired by considerations of use has appealed to a number of observers who wanted to provide for a more complex relationship between these goals. The historian of science, Gerald Holton, in his remarkable essay on Thomas Jefferson's vision of the Lewis and Clark expedition, articulates the need for a category of research that combines Newton's tradition of understanding the natural world with Bacon's tradition of using this understanding to achieve purposive ends. Such a category would encompass "research in an area of basic scientific ignorance that lies at the heart of a social problem."[4] Lillian Hoddeson, in a series of articles on basic research in Bell Laboratories, offered this modification of the framework:

> "Fundamental" and "pure research" refer to the attempt by experimental and theoretical means to understand the physical underpinnings of phenomena. The special term "basic research" refers here to fundamental studies carried out in the context of industry, which may lead to, but do not aim primarily at, application. Applied research, on the other hand, which encompasses engineering and technology, does aim primarily at practical application.[5]

Hoddeson's specialized use of "basic research" is close to the category of research offered by Deborah Shapley and Rustum Roy in their dispirited survey of contemporary science and science policy:

> What was lost, in a word, was the importance of applied science and engineering, and something else we shall call *purposive basic research,* i.e., research of a fundamental nature that is done with a general application in mind, like Charles H. Townes' discovery of the maser while working on microwave transmission for Bell Laboratories, or most biomedical research.[6]

Frustration with the prevailing framework is indeed endemic among those who have tried to fit its categories to research in biomedical science. A number of biomedical scientists have argued that applied research includes studies that also seek a more basic understanding of a field. Thus Julius Comroe and Robert Dripps, in their seminal study of work leading to major clinical advances, define a category of research that is related to a clinical problem but is also "concerned with basic biological, chemical, or physical mechanisms."[7]

The insulation of basic research from thought of practical ends has been defended against such challenges partly by conceding the legitimacy of the concern for applied goals among those who *support* research but not among those who *perform* it. In an era of institutionalized science, research is typically set in an organizational framework where influence on goals may be shared with those who establish priorities and control funds at various levels. Alan T. Waterman, NSF's first director, wove this difference between sponsor and investigator into a defense of Bush's belief that scientists must be free to pursue basic research wherever it leads. In his 1964 address as retiring president of the American Association for the Advancement of Science, Waterman noted:

> There has been a steady increase in the support of basic research which may be termed "mission-oriented"—that is, which is aimed at helping to solve some practical problem. Such research is distinguished from applied research in that the investigator is not asked or expected to look for a finding of practical importance; he is still exploring the unknown by any route he may choose. But it differs from "free" basic research in that the supporting agency does have the motive of utility, in the hope that the results will further the agency's practical mission . . . Thus, basic research activity may be subdivided into "free" research undertaken solely for its scientific promise, and "mission-related" basic research supported primarily because

its results are expected to have immediate and foreseen practical usefulness.[8]

It is noteworthy how deftly Waterman introduced the category of "mission-oriented" basic research without giving an inch on Bush's insistence that basic research must be done by scientists who have no thought of practical ends. In Waterman's formulation, only the funding agency need have such thoughts, as it supported "mission-oriented" basic research. The individual investigator would, in effect, share with the sponsoring agency only the choice of the research problem, and thereafter be free to pursue the research without thought of practical ends.

Harvey Brooks offered a more sophisticated version of Waterman's view in a 1967 report to the House Committee on Science and Astronautics on how to enlist science for advances in technology as Congress took up the "Daddario amendments" to NSF's charter.[9] Brooks's introduction sets out an interesting analysis of the distinctness of basic and applied research, one that echoes Waterman by noting that

> there can be a perfectly viable difference in viewpoint between the research worker and his sponsor. Research that may be viewed as quite fundamental by the performing scientist may be seen as definitely applied and may fit into a coherent pattern of related work from the standpoint of the sponsoring organization or agency.[10]

This observation led Brooks, as it had Waterman, to subdivide basic research according to this interplay of institutional influences on problem selection. Shortening Waterman's "mission-oriented basic research" to "oriented basic research," he observed that

> the general field in which a scientist chooses or is assigned to work may be influenced by possible or probable applicability, even though the detailed choices of direction may be governed wholly by internal scientific criteria. Research of this type is sometimes referred to as "oriented basic research."[11]

Brooks also noted that research may be differently perceived

according to where it is done. For example, certain types of research on semiconducting materials, carried out in a university laboratory, "might be regarded as fairly 'pure,' while in Bell Laboratories they would be regarded as 'applied' simply because potential customers for the research results existed in the immediate environment,"[12] a factor that influences the view held by the bench scientist and not only the view held by the scientist's sponsors:

> Once the transistor was discovered, and germanium became technologically important, almost any research on the properties of group IV semiconducting materials could be considered to be potentially applicable . . . and research into the theory of zone-refining single crystals was of such obvious immediate application to the control of transistor materials that it could legitimately be called applied rather than merely applicable," whereas "prior to the discovery of the transistor, both of these types of research would have been of equal interest and importance from the scientific viewpoint, but they would have been classified as quite fundamental or 'pure'.[13]

But in a remarkable aside, Brooks allowed himself a far more radical view by noting that

> the terms *basic* and *applied* are, in another sense, not opposites. Work directed toward applied goals can be highly fundamental in character in that it has an important impact on the conceptual structure or outlook of a field. Moreover, the fact that research is of such a nature that it can be applied does not mean that it is not also basic.[14]

He supported this observation with the example of Louis Pasteur, whose later work was, as we have seen, an impressive synthesis of the goals of understanding and use. This aside represented a much more radical break with the idea of a one-dimensional spectrum of basic and applied research and helps to prepare the way for a different framework for thinking about the goals of understanding and use.

Official Reporting Categories

With the interests served by Bush's framework so firmly entrenched, the United States did little at an official level to respond to the logic of these early dissents. But the countries with a different postwar experience sought to recognize a more complex relationship between understanding and use. With the exception of Britain, none of the other industrial countries shared the distinctive circumstances that led the postwar paradigm to become so deeply ingrained in America.[15] The economic and social dislocations of the war kept the scientists in these countries from making claims on government equivalent to those asserted in the United States by the campaign that followed publication of *Science, the Endless Frontier,* although the postwar stature of American science made Bush's framework highly visible in all of the industrial countries.

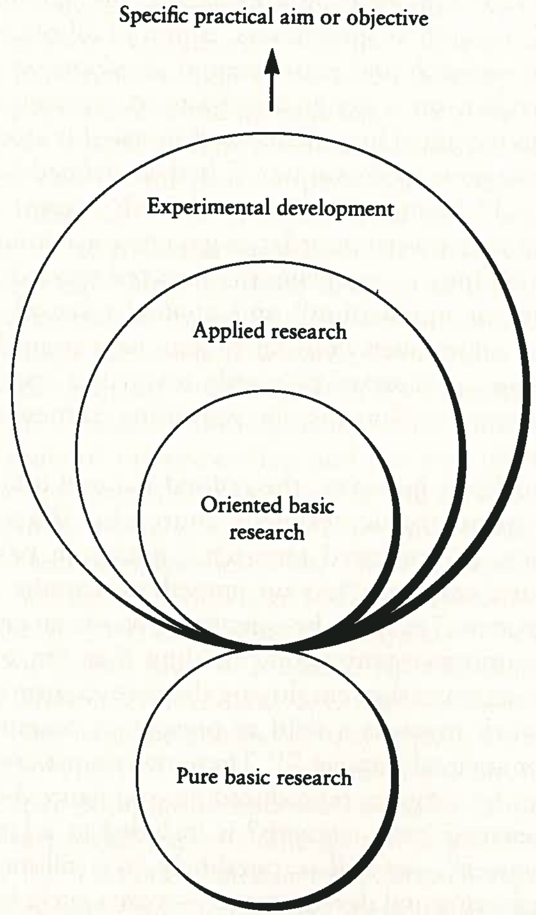
A natural, if ultimately limited, focus for the conceptual efforts to mix the goals of understanding and use was the work of the Organization for Economic Cooperation and Development to refine the categories within which OECD's member nations reported scientific and technological activities. These efforts can be traced through the successive versions of OECD's *Frascati Manual,* so called because the 1963 conference that agreed on the first manual was held in the Italian town of Frascati. The first manual, drafted largely by Christopher Freeman, a British specialist on science policy who later was cofounder of the Science Policy Research Unit at the University of Sussex, drew on the definitions the U.S. National Science Foundation had been using for about a decade. Hence, his draft presented no challenge to the Bush categories, to the relief of the National Science Foundation's representatives. *Fundamental research* was defined as "work undertaken primarily for the advancement of scientific knowledge, without a specific practical application in view;" *applied research,* as work that did have "an application in view." Moreover, in keeping with the linear model of technology transfer, *experimental development* was defined as "the use of the results of fundamental and applied research directed to the introduction of useful materials, devices, products, systems, and processes, or to the improvement of existing ones;"[16] Bush's second canon, that technological innovation is ultimately rooted in scientific discovery, was alive and well at the Frascati conference.

These categories were modified when the *Frascati Manual* was revised in 1970. This revision approached the definition of basic and applied research at three levels. It first of all offered a generic definition of *research and experimental development* as "creative work undertaken on a systematic basis to increase the stock of scientific and technical knowledge and to use this stock of knowledge to devise new applications."[17] It then defined *basic research* ("fundamental" having given way to Bush's term) as "original investigation undertaken in order to gain new scientific knowledge and understanding . . . not primarily directed towards any specific practical aim or application" and *applied research* as "original investigation undertaken in order to gain new scientific or technical knowledge . . . directed primarily towards a specific practical aim or objective."[18] Thus far, the prevailing framework remained unchallenged.

At a third level, however, the revised manual added some observations about basic research that echo Waterman's and Brooks's view of "oriented research," noting in particular that, although basic research "has no immediate specific practical applications in view," it "may be oriented towards an area of interest to the performing organization," adding that "in *oriented basic research* the organization employing the investigator will normally direct his work towards a field of present or potential scientific, economic or societal interest."[19] These revisionist comments were accompanied by a figure, reproduced here as figure 3-1, in which a circle of "oriented basic research" is included in a larger circle for "applied research"—as well as, puzzlingly, in a still more embracing circle for "experimental development"—with a circle for "pure basic research" tangent to, but not intersecting, these nested circles.

Although this *fleur-de-lis*-like figure did signal some relaxation in the presumption that understanding and use are opposed, it did little to clarify the conceptual relationship between these goals and has not been widely reproduced. It also suffered the disabilities of providing only for a dichotomous split of basic research between "pure" and "oriented" research and of presuming that the mix of goals in the latter category results only from the organizational

Figure 3-1. *Diagrammatic Presentation of the Concepts of Basic and Applied Research and Experimental Development Research in the 1970* Frascati Manual



SOURCE: OECD Directorate for Scientific Affairs, *The Measurement of Scientific and Technical Activities: Proposed Standard Practice for Surveys of Research and Experimental Development (Frascati Manual)* (Paris: Organization for Economic Cooperation and Development, 1970), p. 14.

sponsorship of research and not from a meld of goals held by the research scientist. This organizational gloss is missing from a number of subsequent definitions of "strategic research," the term that supplanted "oriented research" in the 1980 revision of the *Frascati Manual*.[20]

Two British scholars, John Irvine and Ben R. Martin, have addressed the issue of strategic research in the course of two illuminating surveys of research foresight in a number of countries.[21] Their 1989 book has this to say:

> Here, the traditional three-fold distinction between "basic research," "applied research," and "experimental development" is now recognized as inadequate. The "basic" category is especially problematic in that it covers a disparate variety of activities ranging from curiosity-oriented, proposal-driven research through long-term targeted programmes supported by sectoral government agencies, to speculative work in industry where no specific application is yet in mind. It is therefore useful to subdivide "basic research" into "curiosity-oriented research" and "strategic research."[22]

It has been inherently difficult for governments to resolve the conceptual issue surrounding the goals of research by adumbrating a set of statistical reporting categories. Almost any useful statistical series becomes the prisoner of its existing definitions, and the difficulty of establishing the motives of scientific research has strengthened the hand of those who have wanted to preserve the empirical separateness of basic and applied research. Hence, the conceptual issue of strategic research has been taken hostage by problems of measurement and has remained unresolved.

This was decidedly so when the U.S. National Science Foundation considered the possibility of revising the Bush framework. The backdrop to this episode was the willingness of Congress and the Reagan administration to establish new programs of Engineering Research Centers and Science and Technology Centers. These centers were typically located in universities but with the participation of industry and the state governments and were designed to bring the resources of several scientific and engineering disciplines to bear on problem areas of evident importance for the country's needs.

It is hardly surprising that as the centers took root, NSF's director, Erich Bloch, should wonder whether the categories for reporting government-funded R&D adequately provided for strategic research of the kind the centers were intended to mount.

Bloch therefore created a task force to consider this issue and requested that the group also examine the mixed taxonomies proposed by the British government and U.S. General Accounting Office. The task force's report clearly signaled the importance of the newly funded centers in launching this review:

> In recent years a number of new "research centers" have been formed, often as a partnership of Federal government, state government, industrial and academic interests. The research performed at these centers tends to combine many traditional disciplines and is oriented toward generating knowledge in fields that may lead to discoveries that will enhance the strategic position of the U.S. in the world economy. . . . The existing taxonomy of research does not address this type of research very well.[23]

The task force did not address the conceptual issue head-on but shifted the basis of a taxonomy from the goals of research to the intended *users* of research.[24] It proposed a threefold categorization of research—with *fundamental research* leading to "results intended at the time the research is funded for dissemination to other researchers and educators"; *strategic research* to results "of evident interest to a broad class of users, external to the research community, that can be identified at the time the research is funded," although "the intended users of the results may also be within the research community"; and *directed research* to results bearing on "the specific needs of the sponsoring organization."[25]

The report and appended evaluation of other taxonomies made clear that the task force had chosen to focus on users in the belief that it would be difficult to match accurate data to a taxonomy based on goals and that it should find a taxonomy that would be a "nonthreatening" change for other federal agencies that fund substantial R&D; the conceptual issue was again taken hostage by problems of measurement. In any event, not much came of the task force's proposals. NSF still adheres to definitions of basic and applied research that are firmly in the Bush tradition. Only in its annual survey of *industrial* R&D does it attach to the definition of basic research the limited observation that basic research "may

be in the fields of present or potential interest to the reporting company."[26]

Hence, the twenty-year effort of the OECD countries to modify their reporting categories has done less than we might expect to clarify the relationship of understanding and use as goals of research. The extensive discussions of a new (fifth) edition of the *Frascati Manual* that were held in the early 1990s found only two governments pressing to modify the traditional distinction between basic and applied by including a category for strategic research. These two governments did not agree on how such a category should be defined, and the reservations among the other members were strong enough to limit the headway that could be made toward resolving the conceptual issue surrounding this distinction. Indeed, language that went beyond the prior *Frascati Manuals* was considerably watered down during the stage of consultation with member countries on the text of the new edition.

The reservations were of several kinds. To begin with, there again was a desire to preserve the historical distinction between basic and applied and the statistical series associated with it. As a result, revisionist proposals were directed toward how strategic research might be accommodated by drawing distinctions *within* the basic and applied categories, rather than by cutting across these categories. There was also the semantic concern that "strategic research" might be confused with national or international security studies, or with research on strategic materials or technologies. But there was at least a faint new concern—that by reporting commercially relevant strategic research an OECD country might be seen by other governments as indirectly subsidizing goods exported by firms that benefited from the results of such research. Some of OECD's members were reluctant to seed a new set of trade disputes by creating a category for reporting strategic research.

With this last concern, the wheel came full circle. The belief that science could be enlisted in the drive toward economic competitiveness had fueled much of the interest in strategic research in OECD's member countries in the first place—and therefore had also fueled much of the interest in defining categories for reporting such research. But the very awareness that strategic research might improve a country's trading position, and therefore be regarded as an export subsidy, ultimately helped to close off the effort to define one or more categories for reporting strategic research. After defining language had been excised from the draft, all that remained in the new Frascati edition was the observation that distinguishing oriented from pure basic research "may provide some assistance towards the identification of strategic research"[27] and the observation that

> while it is recognized that an element of applied research can be described as strategic research, the lack of an agreed approach to its separate identification in Member countries prevents a recommendation at this stage.[28]

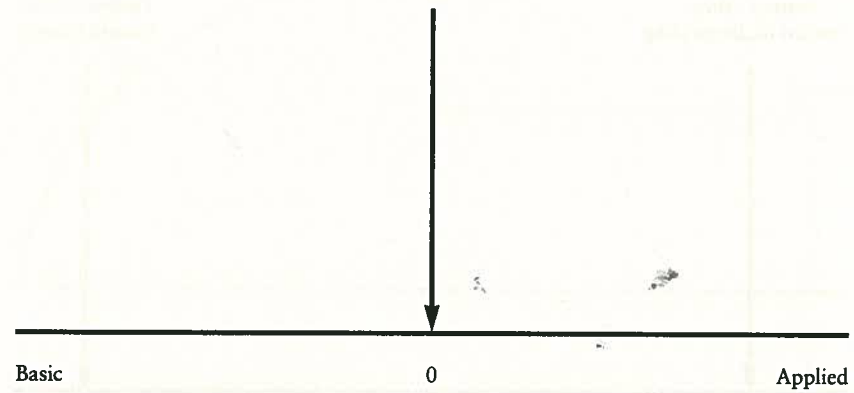
If OECD is to play a significant role in clarifying the conceptual issue of the relationship of understanding and use as goals of research, it awaits a new *Frascati Manual* in 2000 before some fresh sculpting of categories can free this conceptual angel from the statistical marble. What is needed is a way of cutting through the inherently ambiguous choice of assimilating strategic research *either* with basic *or* with applied research. Let us see how this problem can be resolved by a framework that is clear and conceptually spare.

Expanding the Dimensional Image

So strong is the hold of the one-dimensional basic-applied spectrum that many observers who find it difficult to fit this framework to the realities of research think the problem must be because of the uncertainty of classification near the middle of such a spectrum, as if they were measurement psychologists seeking to discriminate two latent classes of subjects on the basis of unreliable measurements on a single scale. In this vein, a former director of the Division of Science Resource Studies of the National Science Foundation has said of the basic-applied spectrum that

> any process that divides a continuum into discretely demarcable regions is generally plagued by fuzziness and overlaps at the boundaries of the subdomains.[29]

Figure 3-2. *One Hypothetical Placement of Pasteur on the One-Dimensional Basic-Applied Spectrum*



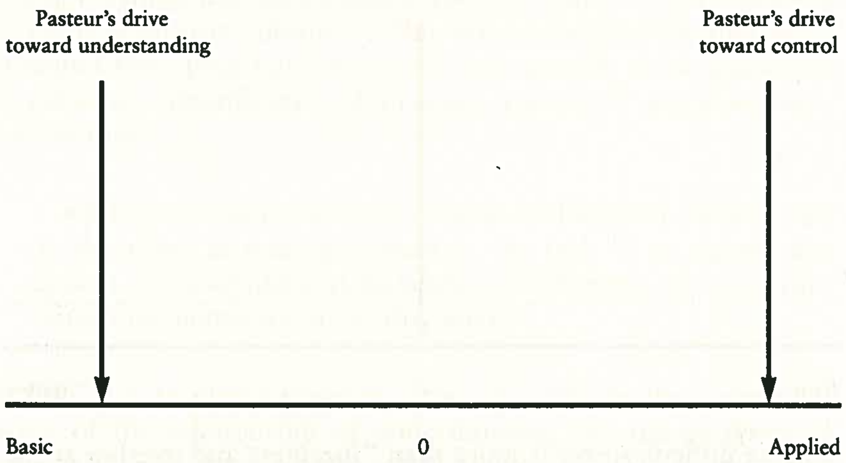
But the difficulty here is more than "fuzziness and overlap at the boundaries." It lies rather in the attempt to force into a one-dimensional framework a conceptual problem that is inherently of higher dimension.

To trace the implications of this we may note that Conant and other physical scientists who have wanted to divide basic research according to whether it also is guided by applied ends have implicitly seen a cross-cutting relationship between the goals of understanding and use. And Comroe and Dripps and many of the other life scientists who have wanted to divide applied research according to whether it also seeks a more fundamental understanding have likewise seen a cross-cutting relationship between these research goals.

To see how this reformulation would go, return to the familiar idea of a spectrum of research that extends from basic to applied and ask *where* on this spectrum should one place the mature Pasteur? The first instinct might be to place him at the mid- or zero-point of the spectrum in view of his commitment to both understanding and use (figure 3-2).

But a moment's reflection is enough to see that this is quite wrong and that the mature Pasteur deserves to be placed not at one point but at two: he belongs far to the left of the spectrum in terms of the strength of his commitment to *understand* the microbiological processes he discovered, but he equally belongs far to

Figure 3-3. *A Second Hypothetical Placement of Pasteur on the One-Dimensional Basic-Applied Spectrum*

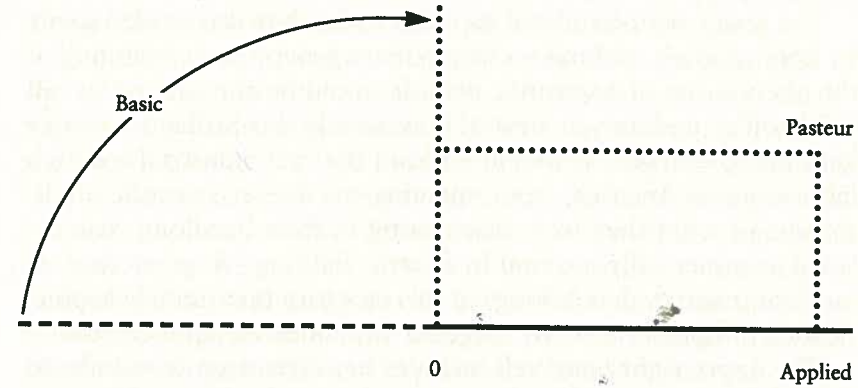


the right of the spectrum in terms of the strength of his commitment to *control* the effects of these processes on various products and on animals and humans (figure 3-3).

We have therefore the anomaly of Pasteur's being represented by two Cartesian points in this Euclidean one-space, an anomaly that should lead us to wonder whether such a one-dimensional figure can adequately characterize research in terms of its basic and applied goals. We may remove this anomaly while still retaining the ease of interpreting a space of spare dimension if we grasp the spectrum at its zero point, rotate the left-hand half through an arc of 90 degrees, and restore Pasteur to the status of a single Cartesian point in what is now a two-dimensional conceptual *plane* (figure 3-4). The vertical axis represents the degree to which a given body of research seeks to extend the frontiers of fundamental understanding, the horizontal axis the degree to which the research is guided by considerations of use.

There is not the slightest reason to think of these dimensions only in dichotomous terms, since there can be many degrees of commitment to these two goals. But if we do so for heuristic reasons, it is clear that we now have not one dichotomy but two. This dual dichotomy can be exhibited as a fourfold table with cells or quadrants (figure 3-5).[30]

Figure 3-4. *Pasteur's Placement in a Two-Dimensional Conceptual Plane*



It will help to fix the meaning of this array if we characterize its quadrants. The upper left-hand cell includes basic research that is guided solely by the quest for understanding without thought of practical use. It might be called Bohr's quadrant in view of how clearly Niels Bohr's quest of a model atomic structure was a pure voyage of discovery, however much his ideas later remade the world. This category represents the research ideal of the natural philosophers, institutionalized in the pure science of the Germans

Figure 3-5. *Quadrant Model of Scientific Research*

Research is inspired by:

Quest for fundamental understanding?		Considerations of use? No	Considerations of use? Yes
	Yes	Pure basic research (Bohr)	Use-inspired basic research (Pasteur)
	No		Pure applied research (Edison)

in the nineteenth century and of the Americans in the twentieth, and includes Bush's concept of "basic research."

The lower right-hand cell includes research that is guided solely by applied goals without seeking a more general understanding of the phenomena of a scientific field. It would be appropriate to call it Edison's quadrant, in view of how strictly this brilliant inventor kept his co-workers at Menlo Park, in the first industrial research laboratory in America, from pursuing the deeper scientific implications of what they were discovering in their headlong rush toward commercially profitable electric lighting. A great deal of modern research that belongs in this category is extremely sophisticated, although narrowly targeted on immediate applied goals.

The upper right-hand cell includes basic research that seeks to extend the frontiers of understanding but is also inspired by considerations of use. It deserves to be known as Pasteur's quadrant in view of how clearly Pasteur's drive toward understanding and use illustrates this combination of goals. Wholly outside the conceptual framework of the Bush report, this category includes the major work of John Maynard Keynes, the fundamental research of the Manhattan Project, and Irving Langmuir's surface physics. It plainly also includes the "strategic research" that has waited for such a framework to provide it with a conceptual home, a case of orphanhood noted above.

The lower left-hand quadrant, which includes research that is inspired neither by the goal of understanding nor by the goal of use, is *not* empty, and the fact that it is not helps make the point that we do have two conceptual dimensions and not simply a more elegant version of the traditional basic-applied spectrum. Indeed, the "prediction" of such a category further validates the framework as a whole. This quadrant includes research that systematically explores *particular* phenomena without having in view either general explanatory objectives or any applied use to which the results will be put, a conception more at home with the broader German idea of *Wissenschaft* than it is with French and Anglo-American ideas of science. Research of this type may be driven by the curiosity of the investigator about particular things, just as research in Bohr's quadrant is driven by the curiosity of the scientist about more general things. The bird watchers who are grateful for the highly systematic research on the markings and incidence of species that went into *Peterson's Guide to the Birds of North America* might want to call this Peterson's quadrant, although this is too limited an example to warrant the name.

In the dynamic pathways that link research in the four cells of the table, it is clear that studies in the fourth quadrant can be important precursors of research in Bohr's quadrant, as it was in the case of Charles Darwin's masterpiece *The Origin of Species*, as well as of research in Edison's quadrant. Other motives inspire research in this quadrant. There are cases in which the prime goal of research is to enhance the skills of the researchers. Arnon gives examples of agricultural research projects in which the investigators start to work in a new area not for the findings they will obtain but to gain skill and experience they may later use "when problems arise in the area" or when breakthroughs achieved by other researchers make the field hot.[31] Those familiar with the role of research in the policy process will have no difficulty identifying cases where studies are launched not for what they learn but to block the start of an operating program, a goal to which the investigators may be willing parties.[32]

Probing the Framework

The sense of abstractness is lessened and the greater realism of such a conceptual plane is demonstrated if this framework is applied to an illustrative body of research. A chapter from the annals of research that admirably lends itself to this purpose is the analysis by Comroe and Dripps of the developments in physical and biological science that led to the most significant recent advances in diagnosing, preventing, and curing cardiovascular or pulmonary disease.[33] These investigators mounted their uniquely detailed inquiry into the scientific backdrop of new technology in the 1970s, provoked by the shift toward purely applied biomedical research that had been signaled by the Johnson and Nixon administrations.

The findings from their meticulous study are, to begin with, a striking illustration of how multiple, unevenly paced, and nonlinear are the paths between scientific discovery and new technology. From this standpoint, their account of the developments leading to cardiac surgery is especially interesting:

When general anesthesia was first put to use in 1846, the practice of surgery exploded in many directions, except for thoracic surgery. Cardiac surgery did not take off until almost 100 years later, and John Gibbons did not perform the first successful operation on an open heart with complete cardiopulmonary bypass apparatus until 108 years after the first use of ether anesthesia. What held back cardiac surgery? What had to be known before a surgeon could predictably and successfully repair cardiac defects? First of all, the surgeon required precise preoperative diagnosis in every patient whose heart needed repair. That required selective angiocardiography, which, in turn, required the earlier discovery of cardiac catheterization, which required the still earlier discovery of X-rays. But the surgeon also needed an artificial heart-lung apparatus (pump-oxygenator) to take over the function of the patient's heart and lungs while he stopped the patient's heart in order to open and repair it. For pumps, this required a design that would not damage blood; for oxygenators, this required basic knowledge of the exchange of O_2 and CO_2 between gas and blood. However, even a perfect pump-oxygenator would be useless if the blood is clotted. Thus the cardiac surgeon had to await the discovery and purification of a potent, nontoxic anticoagulant—heparin.[34]

The aspect of their analysis that directly bears on our framework is their painstaking assessment of the goals moving those responsible for the scientific advances that prepared the way for these breakthroughs in medical technology. Comroe and Dripps first of all elicited from physicans and specialists in the field the ten most important clinical advances since the early 1940s for "diagnosing, preventing, or curing cardiovascular or pulmonary disease; stopping its progression, decreasing suffering, or prolonging useful life." In addition to open heart surgery, the resulting list included blood vessel surgery, treatment of hypertension, management of coronary artery disease, prevention of poliomyelitis, chemotherapy of tuberculosis and acute rheumatic fever, cardiac resuscitation and cardiac pacemakers, oral diuretics (for treatment of high

blood pressure or of congestive heart failure), intensive care units, and new diagnostic methods.

They then analyzed the work that led to each of these advances. With the help of 140 consultants they identified the knowledge essential to each advance and more than 500 "key articles," going back in some cases more than two centuries, reporting the work that developed this knowledge. They made these articles (or, equivalently, the reported research) the central focus of their analysis, classifying them in two ways. The first was whether the authors of these reports gave any sign of the work's having been "clinically oriented" by indicating "an interest in diagnosis, treatment, or prevention of a clinical disorder or in explaining the basic mechanisms of a sign or symptom of the disease itself." In our framework, this amounts to asking whether a given piece of work should be placed in the left- or right-hand *column* of figure 3-5.

Comroe and Dripps crossed this with a second classification, according to whether the reported research was basic in the sense that the investigator sought to understand the mechanisms responsible for observed effects; that is, in our terms, whether the research should be placed in the upper or lower *row* of our fourfold table. The three resulting categories—basic research unrelated to the solution of a clinical problem, basic research related to the solution of a clinical problem, and research not concerned with basic biological, chemical, or physical mechanisms—correspond with Bohr's, Pasteur's, and Edison's quadrants. They found that these categories included, respectively, 37 percent, 25 percent, and 21 percent of the key articles. The remaining 17 percent were classified as development (15 percent) or as "review and synthesis" (2 percent). The 25 percent classified as basic research related to the solution of a clinical problem (that is, work lying in Pasteur's quadrant) is impressive further evidence of the intermingling of understanding and use as goals of research, although the 37 percent classified as basic research unrelated to the solution of a clinical problem (that is, work lying in Bohr's quadrant) is a fresh tribute to the role of pure research in new technology.[35]

What is entailed by this way of thinking about basic and applied science may be further clarified by addressing four conceptual issues. Each is important in its own right, and a discussion of these points may also lessen any sense that invoking the idea of a conceptual plane is a purely formal device.

Characterizing research ex ante or ex post. The first of these issues is whether the classification of research as basic and applied should rest on advance judgments as to the intended goals of research or on retrospective judgments as to what research has achieved. It is sometimes objected that classifying research on the basis of intended goals involves unscientific speculation about the motives of researchers that is quite unlike the assured and objective judgments historians of science can later make. One resisting scientist has said that distinguishing basic from applied research on the basis of such ex ante judgments is like putting scientists on the couch.

The logic of classifying research on the basis of intended goals rather than known achievements rests on the fact that policy has to do with choice—the choices facing individual scientists, the choices facing those who match resources to alternative research uses at the retail or wholesale level. All of these require ex ante judgments under the uncertainty that is an inherent part of research yet to be done. Although the historian of science will in due course be able to give far more assured judgments as to which research proved in fact to advance the general understanding of a field and which in fact led to significant use, only a framework that deals ex ante with the goals of research can serve the needs of science and technology policy.

Such an approach reaches beyond purely private motives. Although there must always be some uncertainty as to whether the goals of research will be achieved, these purposes have to do with "objective" future conditions, about which considered judgments can be made. Indeed, the integrity of the peer review process rests on the fact that it is possible to reach considered, institutionalized judgments on the likelihood of achieving the goals specified for particular projects of research.

Whose goals are to be consulted? Sometimes the objection is made that it is impossible to distinguish types of research on the basis of goals because those who play different roles in the modern system of research may have different goals for a given project. In

an era of organized science, research is, as already noted, typically done in an institutional framework where influence on goals may be shared with those who set priorities and control funds at various organizational levels. The sharper focus of working scientists on understanding and of their sponsors on use is a conspicuous element of a system that involves heavy government support. A university-based biomedical scientist seeking support from the National Institutes of Health may see the proposed work as extending fundamental knowledge, and so may the investigator's department head and the NIH study section that recommends support. Yet the project may be approved by the university's vice president for medical affairs and funded by NIH, and ultimately by Congress, for the contribution it makes to the control of disease. Some years ago Charles V. Kidd wryly noted that the nation's universities reported accepting $85 million in federal grants for basic research in a year in which the government thought its outlays for basic research were half that large.[36] Vannevar Bush, after all, recognized the difference of view between scientist and sponsor on the grand scale when he called on the nation to advance its social and economic goals by supporting research that would, in an immediate sense, be driven only by the scientist's quest of added understanding. As noted earlier, Alan T. Waterman, NSF's first director, wove this difference of view between investigator and sponsor into a defense of Bush's belief that scientists must be free to pursue basic research wherever it leads. In one sort of limiting case, a sponsor might put together a portfolio of basic studies involving multiple researchers without letting the researchers in on the applied objective.

Yet the point should be forcefully made that the mix of goals in use-inspired basic research is not *only* the result of differing goals being held by those at different levels of the institutionalized system of modern science. Despite the rearguard action by Waterman and others to defend the purity of the quest of understanding by the individual scientist, the annals of research are replete with examples of work by investigators who were directly influenced both by the quest of general understanding and by considerations of use. Pasteur wanted to understand *and to control* the microbiological processes he discovered. Keynes wanted to understand *and to improve* the workings of modern economies. The physicists of the Manhattan Project wanted to understand *and to harness* nuclear fission. Langmuir wanted to understand *and to exploit* the surface physics of electronic components. The molecular biologists have wanted to understand *and to alter* the genetic codes in DNA material.

Moreover, the sharing of influence on research choices between working scientist and sponsor need not entail so sharp a dissonance on goals as to make it unreasonable to classify research within our two-dimensional framework. In the major scientific countries the independence of university-based scientists is well enough established that they largely set their own goals within inevitable resource constraints and the perspectives of their scientific disciplines, which typically dominate the peer-review mechanisms for allocating grants. Yet this independence has not precluded a lively interest in applied goals in academic fields as diverse as chemistry, computer science, economics, molecular biology, pharmacology, statistics, and atomic, molecular, and optical science. If academic scientists have a deserved reputation for pursuing interests of their own, they too are generally faithful to added objectives when they become involved in basic, use-inspired sponsored research. Likewise, scientists who work in government or industrial laboratories generally accept the mission of these units, even if many retain a taste for basic science—one that is encouraged by the leadership of the strongest of these laboratories as a means of recruiting, developing, and retaining excellent staff.

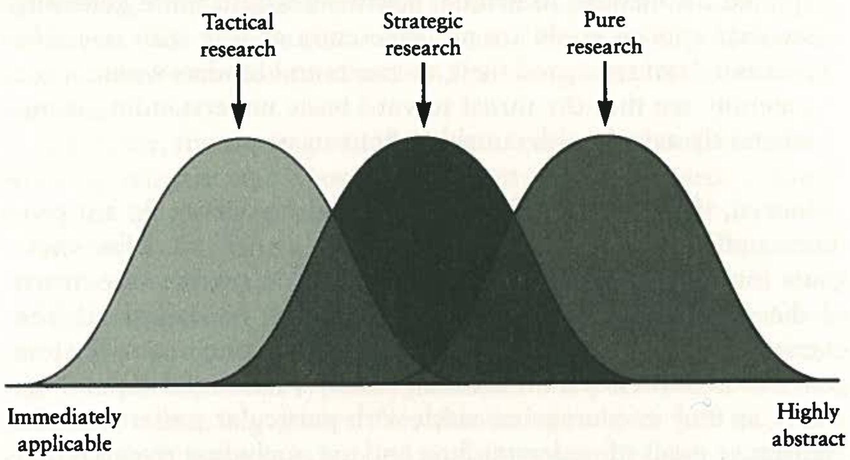
The substantial volume of basic academic research that is use inspired helps to explain the ironic inequality noted by Kidd—that the universities reported accepting a total of federal grants for basic research twice as large as the government thought it made in a particular year; since the accounting both by the universities and by the government used an either-or coding of basic and applied research, it should not be surprising that the universities considered much of their federally funded Pasteur's quadrant research to be basic, while the government considered much of it to be applied.[37] The dissonance as to goals between working scientists and their overseers or funders would be diminished if it were generally perceived that research can be simultaneously influenced by the quest of scientific understanding and considerations of use, a point that deserves special emphasis:

Freed from the false, "either-or" logic of the traditional basic/applied distinction, individual scientists would more generally see that applied goals are not inherently at war with scientific creativity and rigor, and their overseers and funders would more generally see that the thrust toward basic understanding is not inherently at war with considerations of use.

Indeed, the institutional settings of modern science do not produce conflict over research goals so much as help to define these goals for their scientific staff. This conclusion reverses the thrust of the reservation as to whose goals should be consulted. The organizational settings of research do not so much complicate a goal-based framework for thinking about science and technology policy as they encourage research with particular patterns of the alternative goals of understanding and use, including research that is both basic and use-inspired. For example, a number of research units, some within industry (Bell Laboratories), some free-standing (the Rand Corporation), some within the universities, have used a matrix plan of organization to engage first-class scientists in research of impressive scientific rigor that is also deeply influenced by considerations of use.

Can the two dimensions be reduced to one? The graphic image of a one-dimensional basic-applied spectrum naturally gives way to the two-dimensional plane once it is clear that these goals are not inherently opposed. But the power of one-dimensional thinking is such that there have been other attempts to array research on a single scale. An instructive effort of this kind was included in a 1981 report of the Australian Science and Technology Council (ASTEC).[38] The report reproduces the categories of research proposed by the *Frascati Manual*, with modifications by Australia's Bureau of Statistics. These definitions, as already noted, move toward a cross-cutting vision of basic and applied research. But the report fails to pursue this logic and instead proposes a single-dimensional research spectrum that extends from "immediately applicable" to "highly abstract." The graphic representation of this spectrum is reproduced here as figure 3-6. The relative locations of pure, strategic, and tactical research are suggested by three Gaussian (bell) distributions that march across this Euclidian one-

Figure 3-6. *Australian Modification of Linear Model*



SOURCE: Australian Science and Technology Council, *Basic Research and National Objectives* (Canberra: Australian Government Publishing Service, 1981), p. 6. Commonwealth of Australia copyright, reproduced by permission.

space from the "immediately applicable" to the "highly abstract" poles.

This spatial imagery owes more to the appeal of one-dimensional thinking than to the characteristics of research that the ASTEC authors sought to bring out. By labeling one of their poles "immediately applicable," the drafters show their desire to contrast the two goals of research described by their modified Frascati definitions. But it simply clouds the issue to make "highly abstract" the pole opposite from "immediately applicable" and to produce another one-dimensional array. Abstract thinking is no doubt most conspicuous in research that lies in Bohr's quadrant and least prominent in work that lies in Edison's quadrant. But this is only a statement about an empirical correlate of the goal patterns envisaged by the Frascati definitions. The ASTEC authors would have been closer to the mark if they had abandoned their one-dimensional framework and used their graphic skills to illuminate the conceptual basis of the distinctions among "pure," "strategic," and "tactical" research.

Time to application. The most important factor that is sometimes believed to array research on a single continuum is the idea of "time to application." Indeed, this factor is often thought to define the difference between basic and applied research. No one can doubt that there is a vast difference in the time that is likely to elapse between the production of new knowledge and the utilization of this knowledge for an applied purpose as one moves from Bohr's to Edison's quadrants. It could hardly be otherwise, since pure basic research seeks only to probe unknown fundamentals, while purely applied research seeks only to meet some clearly defined need. But time to application is far more problematic in the use-inspired basic research of Pasteur's quadrant, which seeks both to probe unknown fundamentals and to meet a societal need. The knowledge gained by Pasteur's own fundamental work in microbiology was quickly applied to industrial and public health problems, as much of the fundamental work in molecular biology is quickly applied in biotechnology today—indeed, so quickly that some observers speak playfully of negative time to application. However, the plasma scientists will in the end require more than half a century to gain the basic understanding that will yield commercially profitable power from nuclear fusion. Moreover, there is a good deal of variation not only in time-to-application but in our ability to estimate the time horizon of application. It may indeed make sense to regard time-to-application and the predictability of this time as separate dimensions. The reasonable view is therefore that time to application is not a one-dimensional substitute for our conceptual plane but an important empirical correlate of the pattern of goals that defines this two-dimensional framework.

It will be important to have a clear view of the relationship between time and use to understand the policy implications of this framework, which are discussed in chapters 4 and 5. It will be especially important to see that some advances of fundamental scientific importance have near-term applications—and not to think that all research of a basic character must play only a distant role in advances in technology. This point is far more easily grasped if one appreciates the reality of use-inspired basic research, a reality expressed by Pasteur's quadrant. If we are aware of how often considerations of use, including the needs of evolving technology, do influence fundamental research, it will be easier to understand that this research can have applications in a relatively near future.

But it will also be important to see that considerations of use may influence basic research that is unlikely to bring an early return in technology—and not to suppose that all research with a distant horizon of use must be curiosity-driven science that lies in Bohr's quadrant. To believe that all research with such a time horizon is a pure venture in understanding, whose applications are impossible to foresee, is again to miss the essential point that, where the applications of fundamental science are concerned, "everything good does not have to start with a twinkle in a basic researcher's unfocused eye."[39]

Rethinking the Dynamic Paradigm

To recognize the possibility of use-inspired basic science is to see the role of science in new technology from a perspective quite different from the postwar paradigm's view of basic research as a remote dynamo of technological innovation. Although a degree of metaphorical license will always be needed to organize our thinking about these complex relationships, it is clear that the license extended to the "linear model" running from basic to applied research and on to development and production and operations has long since expired. In the words of Nathan Rosenberg, "everyone knows that the linear model of innovation is dead,"[40] even if it still lives on in parts of the science and policy communities and broader public. It has been dealt mortal wounds by the spreading realization of how multiple and complex and unequally paced are the pathways from scientific to technological advance; of how often technology is the inspiration of science rather than the other way round; and of how many improvements in technology do not wait upon science at all.

Indeed, the last of these criticisms has led a number of observers to shift their focus away from the links between science and technology as such to *all* of the sources of technological innovation, with only a secondary interest in how many of these are ultimately traceable to science. The rapidly expanding literature of innovation has offered a number of alternative images of these sources that are vastly less simplistic than the linear model. Ryo Hirasawa, for example, has proposed a "concurrent system" model of the overlapping rather than sequential management of the phases of research, development, production, and sales by innovative Japanese firms.[41] Hirotaka Takeuchi and Ikujiro Nonaka use a sports idiom to develop this distinction, contrasting a relay race, in which a baton is passed from one runner to the next at the end of each lap, with a rugby game, in which the outcome depends on a team that "tries to go the distance as a unit, passing the ball back and forth."[42] Stephen J. Kline and Rosenberg offer an iterative "chain-linked" model of innovation that distinguishes chain, feedback, and initiation elements, a model which, if nothing else, conveys the potential complexity of the innovation process.[43]

Inevitably, the more general canvass of the sources of innovation involved in these models has somewhat diverted attention from the relationship between basic science and technological innovation. This relationship was what the linear model was all about, however flawed its account. Attention has also been diverted from this relationship by the concern with economic competitiveness, which has led a number of commentators to shift their focus to the link between new technology and its commercial application. Erich Bloch, former director of the National Science Foundation, and David Cheney, a colleague at the Council on Competitiveness, express this concern well:

> Technology that remains in the lab provides almost no economic benefits. Technology that is applied only to government markets, such as defense, provides much smaller economic benefits than technologies that contribute to success in the much larger commercial markets, and especially in the ever more important global markets.[44]

In the view of these authors, the United States leads the world in basic science and probably also in technological innovation; it is falling down, however, in converting new technology into products and services that meet the test of the market. It is almost a commonplace of commentaries on America's lagging competitiveness how often technologies first developed in the United States have been commercially exploited elsewhere in the world, especially Japan. The authors of a comprehensive report on European policy toward innovation and technology diffusion also distinguish between new technology and its use in products or services that meet the test of the market.[45] The British government has incorporated this distinction into the vocabulary of technology policy, using "innovation" for the development of new technology, "exploitation" for its commercial application.

The case for drawing such a distinction would seem to be strengthened by the notable examples from the annals of technology, detailed by Rosenberg and others, in which it took many years for a new technology to find its most important commercial uses. The steam engine was initially seen as a device for pumping water from mines and only later as a power plant for movable ships or carriages. The railroad was initially seen as a feeder of goods for canal transport and only later as a fully articulated system of transportation in its own right. The radio was initially seen as a "wireless" substitute for the electric telegraph for communicating between two points that could not be connected by wire, such as ship to shore, and only later as a means of "broadcasting" communication to a mass audience.[46] Indeed, this is an almost universal phenomenon in the evolution of technology. New technological paradigms seldom spring full-blown from the minds of their inventors, and when they do, as in the case of Arthur Clarke's vision of communications satellites, the visionary is unlikely to be the person who makes the technological dream come true.

Yet there are pitfalls in distinguishing a technology from its applications. A valid distinction is to be drawn between a general technology and its application to particular products or processes. Moreover, particular goods or services may combine several technologies, and some aspects of marketing and finance that may be critically important for economic success are quite distinct from the technology that is being exploited. But it is simply a holdover of linear-model thinking to suppose that technology is shaped only by technical or engineering considerations, free of market influence. Technology itself can be deeply influenced by consumer demand in emerging markets, as it was in each of the cases of the steam engine, railroad, and radio; the technology of the steam

locomotive had moved considerably beyond the steam technology that pumped water from the mines. It makes perfectly good sense to speak of a "trajectory" of technology that is guided by technical and by market considerations, as we might speak of the trajectory followed by a branch of science that is guided by several influences—including, at times, the opportunity to create a commercially successful technology.[47]

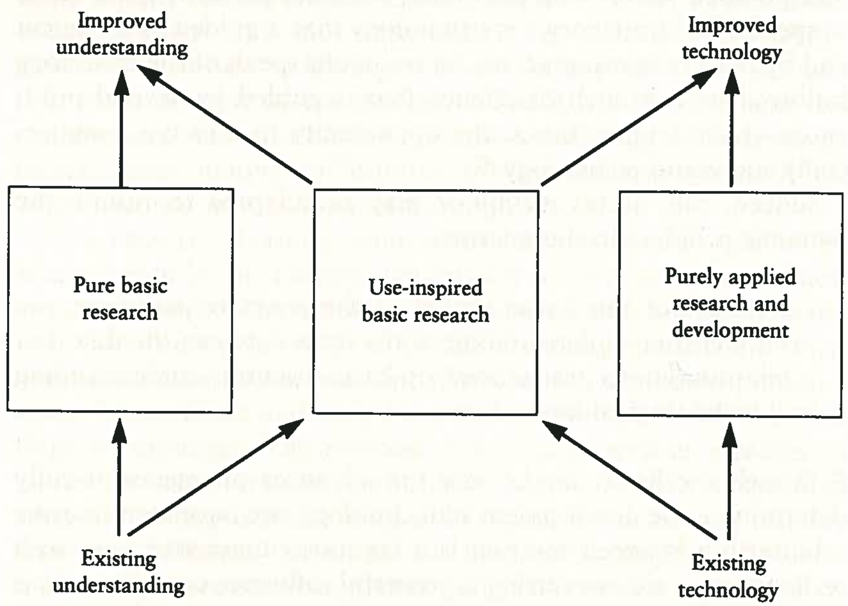
Indeed, this useful metaphor may be adapted to restate the dynamic problem in these terms:

> To replace the linear model of the postwar paradigm, we need a clearer understanding of the links between the dual but semiautonomous trajectories of basic scientific understanding and technological know-how.

Although the linear model saw the advances of science as fully determining the development of technology, we have seen that the relationship between the two is a far more interactive one, with technology at times exerting a powerful influence on science. It is here that the problems of transforming the static and dynamic paradigms come together: a deeper understanding of this relationship is possible if the dynamic importance of research in Pasteur's quadrant is noted.

Although it would be playful to see a double helix in the intertwined, upward course of scientific understanding and of technological capacity, the one-dimensional, one-way model of the link between basic science and technological innovation clearly needs to be displaced by an image that conceives of their dual, upward trajectories as interactive but semiautonomous (figure 3-7). These trajectories are only loosely coupled. Science often moves from an existing to a higher level of understanding by pure research in which technological advances play little role. Similarly, technology often moves from an existing to an improved capacity by narrowly targeted research, or by engineering or design changes, or by simple tinkering at the bench, in which fresh advances in science play little role. But each of these trajectories is at times strongly influenced by the other, and this influence can move in either direction, with use-inspired basic research often cast in the linking role. In a similar vein, Brooks has observed that "the relation between sci-

Figure 3-7. *A Revised Dynamic Model*



ence and technology is better thought of in terms of two parallel streams of cumulative knowledge, which have many interdependencies and cross-relations, but whose internal connections are much stronger than their cross connections."[48]

This image of dual trajectories of knowledge leaves a good deal out of account. The interaction of science and technology includes the role that new research technologies at times play in the creation of operational technologies and the importance that the availability of commercialized measurement methods may have in supporting new fundamental science. Nonetheless, a loosely interactive relationship has characterized the trajectories of scientific understanding and technological capacity since the period in the nineteenth century when the concept of "technology" first took root in positivist thought. In this period the marriage of science with the practical arts proposed by Francis Bacon more than two centuries before was at last consummated by the influence of technology on the development of science and by the influence of the

emerging disciplines of physics, chemistry, and biology on the development of new products and processes.

Implications for Policy

More is involved in these revised images of the links between basic science and technological innovation than their greater faithfulness to the annals of research. These revisions in the postwar paradigm are also of broad importance for science and technology policy. Indeed, the following five observations may carry us across the threshold between analysis and policy:

—The paradigm view of science and technology that emerged from World War II gave a notably incomplete account of the actual relationship between basic research and technological innovation.

—The incompleteness of the postwar paradigm is impairing the dialogue between the scientific and policy communities and impeding the search for a fresh compact between science and government.

—A more realistic view of the relationship of science and technology must allow for the critically important role of use-inspired basic research in linking the semiautonomous trajectories of scientific understanding and technological know-how.

—A clearer understanding by the scientific and policy communities of the role of use-inspired basic research can help renew the compact between science and government, a compact that must also provide support for pure basic research.

—Agendas of use-inspired basic research can be built only by bringing together informed judgments of research promise and societal need.

The policy implications of these observations are discussed in chapters 4 and 5. Chapter 4 explains how a more realistic view of the relationship of basic research to technological innovation can help restore the compact between science and government. Chapter 5 explores how to link judgments of scientific promise with judgments of social value in the funding of basic research that is inspired by considerations of use.