Technology and the pursuit of economic growth

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A new framework for research and development: Analysis and policy implications

Technology's contribution to economic growth and competitiveness has been the subject of a large and growing literature in recent years. This debate has been spurred in part by a recognition of the importance of innovation in an economy that is increasingly open to the products of foreign firms that have proven to be the technological and economic equals of U.S. firms in a number of industries. Many contributors to this literature and a growing number of policymakers are concerned with the design of public policies that can improve the innovative performance of U.S. firms and with the training of U.S. managers to better manage technological change.

Economists have contributed a great deal to the identification of the importance and (imprecisely) the measurement of the contribution of technological change to economic growth. It is therefore surprising that economists have not played a more prominent role in recent debates of science and technology policies. In our view, the potential contributions of economics to the development of improved public and private policies for innovation have been hampered by the limitations of the theoretical framework employed by most economists for the analysis of innovation. This book is intended to aid the development of an alternative framework for the analysis of innovation and the design of policies for its support and management.

The chapters in this book do not develop a comprehensive alternative theory of innovation; indeed, the analytic insights offered here complement those of the neoclassical economic framework. Nevertheless, our analysis is based on several premises that depart in varying degrees from those of the neoclassical framework and its descendants. Rather than focusing exclusively on the conditions affecting the supply of research and development, we are concerned with the utilization of research findings and their translation into commercial innovation. It is only through this conversion of research into innovation that the economic payoffs from scientific research are realized, and recent history suggests that strength in research alone may be insufficient to guarantee that the economic benefits of research investment will be realized by the nation making the investment. Our concern with utilization also motivates an extensive discussion of the processes linking scientific research and commercial innovation.

Because the structure and organization of the institutions of research heavily influence the utilization of research results and their translation into commercial innovations, our analysis examines the development of the U.S. R&D "system" in some detail and compares this system with those of other industrial nations. The neoclassical economic framework for the analysis of R&D and innovation says very little if anything about the institutional structure of the research systems of advanced industrial economies, yet this structure heavily influences the ability of nations to realize the economic payoffs from research. Not only does the structure of institutions matter, but their historical development affects innovative performance.

The performance of a given institutional structure for R&D clearly depends on the technological and economic environment of that research system. As the environment changes, the research system must adapt, if its performance is to remain strong. The U.S. research system has undergone at least one upheaval thus far this century (the rise of federal funding of research during and after World War II) in response to a significant change in its environment. This system may be in the midst of a second transformation, as new structures for the performance of research, including domestic and international cooperative research, now are emerging in response to change in the environment. It is impossible to understand and evaluate the causes and implications of these structural shifts without an understanding of their historical origins.

The first section of this chapter discusses the neoclassical economic framework for the analysis of R&D, focusing on its limitations for policy and for the analysis of the organization and management of research. The next section presents an alternative framework for the analysis of innovation, one that emphasizes the interactions among basic research, applied research, and commercial development. The final section discusses the role of basic research within this schema, arguing that current analytic distinctions (distinctions that are reflected in current data collected by the National Science Foundation) between basic and other forms of research lack a substantive basis.

Appropriability and policy

The vast economics literature on technological innovation, market structure, and government policy is reviewed in Scherer (1980), Cohen and Levin (1988), and Kamien and Schwartz (1982). This section sketches the conceptual framework for the organization of R&D and for public policies toward innovation that has developed out of this literature.

The economist's theory of the firm is actually a theory of markets; theoretical understanding of the basis for the existence of the firm, to say nothing of the factors that determine its boundaries, is not well advanced, despite important work by Williamson (1975, 1985), Chandler (1962, 1975, 1988), and Teece (1982, 1986). This deficiency is particularly significant in the analysis of the organization of research and development. Neoclassical theory's emphasis on the appropriability of the results of research must be complemented with an analysis of the conditions supporting the utilization of these results. In the absence of an analysis of the utilization of the fruits of R&D, economic theory is hard pressed to provide answers to a broad array of increasingly urgent policy questions.

The key elements of the neoclassical economic theory of R&D are contained in articles by Nelson (1959) and Arrow (1962) that considered the economics of knowledge production through investment in industrial research (Mowery, 1983b, contains a more detailed discussion of this work). Research was portrayed in these papers as an activity resulting from an investment decision made by the profit-maximizing firm. The critical element in the research investment decision of the firm was the returns to this investment. Consistent with the tenets of microeconomic theory, the firm was treated as a "black box" whose internal workings and structure could be ignored. The critical element in the firm's decision on R&D investment was the returns to the investment. Nelson and Arrow argued that the social returns to research investment exceeded the private returns faced by the individual firm, a condition leading to underinvestment by the firm (from the societal point of view) in research.

The reasons adduced for this market failure illustrate the deficiencies of the appropriability framework. Arrow in particular argued that although firms incurred costs in producing scientific or technical knowledge, the costs of transferring this knowledge once discovered were effectively zero. That the consumption by another firm of the knowledge produced by a firm did not diminish or degrade it made the information a public good. From the social point of view, the widest possible diffusion of this knowledge is optimal. The price necessary to achieve this goal, however, one equal to the costs of transfer, was so low as to bankrupt the discoverer. The supply of socially beneficial research in civilian technologies, and basic research in particular, therefore was insufficient because of the disjunction between the privately and socially optimal prices for the results:

> Thus basic research, the output of which is only used as an informational input into other inventive activities, is especially unlikely to be rewarded. In fact, it is likely to be of commercial value to the firm undertaking it only if other firms are prevented

> from using the information. But such restriction reduces the efficiency of inventive activity in general, and will therefore reduce its quantity also. (Arrow, 1962, p. 618)

Although this theoretical analysis identified an important source of market failure in the generation of new knowledge through private investment, it is deficient. The fruits of research do not consist solely of information that can be utilized by others at minimal cost for innovation. As Pavitt (1987) and others (Rosenberg, 1982; Mowery, 1983b; Cohen and Levinthal, 1989) have noted, transferring and exploiting the technical and scientific information that is necessary for innovation constitute a costly process that itself is knowledge intensive. The neoclassical analysis of innovation focuses largely on the conditions of appropriability of the returns to innovation in concluding that market failure results in too little R&D investment. The critical factor for commercially successful innovation, however, may well be the utilization of the results of R&D. The market-failure analysis must be supplemented by an analysis of the conditions affecting the utilization of the results of R&D. Utilization of the results of research is heavily influenced by the structure and organization of the research system within an economy, a topic on which the neoclassical theory is either silent or incorrect.[1]

For example, the appropriability analysis of R&D investment provides no basis for explaining a topic discussed at some length below, namely, why is most U.S. industrial research conducted in laboratories that are a part of manufacturing firms? Why did industrial research develop in this fashion rather than being provided through contract by independent firms? The growth of industrial research within U.S. manufacturing was part of a broader process through which manufacturing firms absorbed functions previously carried out and organized through the market and developed entirely new activities within the firm (Chandler, 1977). In-house research allowed for the development and exploitation of firm-specific knowledge, reflecting the superiority of the quality and quantity of intrafirm communications to those between contractual partners, as well as the fact that production and the acquisition of detailed technical knowledge frequently are joint activities. Among other things, these factors meant that the activities of independent, contract research organizations and the relationship of contract research to in-house research differed substantially from the predictions of such distinguished proponents of neoclassical theory as George Stigler (see Chapter 4).

Moreover, for policy purposes, the distribution and utilization of the

[1] An important and interesting exception to this characterization of the economics literature is the recent paper by Clark, Chew, and Fujimoto (1987), which compares the performance of European, Japanese, and U.S. auto firms in developing new designs and emphasizes the roles of internal communication and organization in explaining differences in performance.

results of R&D, rather than solely the sufficiency or insufficiency of its supply, are critical. Although it pinpoints a critical source of underinvestment in R&D, the appropriability analysis does not yield useful policy guidance of a more specific sort. Its failure to address issues of utilization means that the appropriability framework ultimately can provide very little analysis or prediction about the distribution of the benefits of cooperative R&D among industries and firms – but such distributional issues are crucial in this context. Moreover, this analytic framework ignores many of the key characteristics of the process of technological change.

Rather than a page from a book of blueprints, a new technology is a complex mix of codified data and poorly defined "know-how."[2] A richer analysis of the economics and organization of R&D must stress the costs to the individual firm of finding and adopting new techniques. This perspective also allows for different levels of technical performance among firms in the same industry and differences in the technical performance of the same industry in different nations. It places greater weight on internal firm structure and issues of utilization in a more explicit analysis of the implementation and distribution of R&D results than does the appropriability paradigm.

We agree with the argument of the neoclassical analysis that research services and technical knowledge have unique characteristics that distinguish them from such commodities as wheat. Our critique of market mechanisms is in some ways a more fundamental one than the market-failure analysis of the appropriability paradigm. Even if the specific market failure identified by the appropriability analysis could be remedied, the distributional consequences would still be unacceptable (i.e., certain firms and industries would not be served). Further, the distributional consequences of such markets for R&D may not be remediable through public subsidization of the supply of contract research.

The processes of innovation

The focus on appropriability also leads to a distorted view of the process of innovation. The neoclassical analysis focuses largely on the incentives

[2] Research on government demonstration projects carried out by a team at the Rand Corporation emphasized the costs and difficulties of knowledge transfer; see Baer, Johnson, and Merrow (1977) for a summary. Surprisingly, the Rand analysis says rather little about the role of client firms' in-house expertise as an influence on the success or failure of such demonstrations. The authors of the study cite as important for demonstration projects' success "the existence of a strong industrial system for commercialization" and the "inclusion of all elements for commercialization" (p. 955), both of which criteria presumably include in-house technical expertise among client firms. However, these categories as they are employed by the authors are nearly vacuous. In the wake of a failed demonstration project, it may be obvious that "all elements needed for commercialization" were not present. However, the policy problem is one of prediction; for this task, more precise criteria are needed.

of firms to invest in R&D and views internal structure and process as unimportant. As a result, it devotes little if any attention to the process through which research is converted into commercial innovation. According to this view, new technologies represent the application of previously acquired scientific knowledge – usually meaning "recently acquired" scientific knowledge. Thus, technological innovation is regarded as essentially the application of "upstream" scientific knowledge to the "downstream" activities of new product design and the development of new manufacturing processes.

In reality, however, many of the primary sources of innovation are located "downstream," without any initial dependence on or stimulus from frontier scientific research. These sources involve the perception of new possibilities or options for efficiency improvements that originate with working participants of all sorts at, or adjacent to, the factory level. The participants include professional staff such as engineers and those who have responsibilities for new product design or product improvement, and may include customers as well, as von Hippel (1978) has noted.

The acquisition of knowledge for innovation is not a once-and-for-all matter. Rather than a unidirectional, one-time occurrence of transfer of basic scientific knowledge to application, the processes of innovation and knowledge transfer are complex and interactive ones, in which a sustained two-way flow of information is critical. The ability to adopt a new technology, to evaluate a new technique, or even to pose a feasible research problem to an external research group may require substantial technical expertise within the firm.[3]

The process of technical innovation has to be conceived of as an ongoing search activity that is shaped and structured not only by economic forces that reflect cost considerations and resource endowments but also by the present state of technological knowledge, and by consumer demand for different categories of products and services. Successful technological innovation is a process of simultaneous coupling at the technological and economic levels – of drawing on the present state of technological knowledge and projecting it in a direction that brings about a coupling with some substantial category of consumer needs and desires.

[3] Arrow's famous 1969 set of "Classificatory Notes on the Production and Transmission of Technical Knowledge" noted, "When the British in World War II supplied us with the plans for the jet engine, it took ten months to redraw them to conform to American usage" (1969, p. 34). Concerning Japanese technology imports from the industrialized West, Caves and Uekusa (1976) stated: "The level and pattern of research and development within Japan are closely related to the import of technology from abroad. Firms must maintain some research capacity in order to know what technology is available for purchase or copy and they must generally modify and adopt foreign technology in putting it to use. A 1963 survey of Japanese manufacturers showed that on average one-third of the respondents' expenditures on R&D went for this purpose. The moderate level, wide diffusion, and applied character of Japan's research effort are consistent with a facility for securing new knowledge from abroad" (p. 126).

The choice among technological alternatives and the decision as to how much performance improvement is worth acquiring involve commercial and economic judgments, not only technological criteria. There are typically many ways of strengthening a bridge or reducing the weight of a commercial aircraft or improving the conductivity of an electrical transmission system. The exchange of information among specialists in establishing the optimal trade-off is therefore a central part of the firm's decision-making process.

Work by Nelson and Winter (1982) contains an excellent statement of these characteristics. A central component of their view of innovation is the portrayal of the discovery of alternatives, as in research, and the choice among alternatives, as in the decision to pursue development of a discovery, as a single process, rather than separate activities.[4] Research and development is portrayed as a particular type of search activity, consisting of repeated "draws" from a distribution of possibilities that may be more or less "distant" from a firm's existing endowment of skills and technological capabilities. This view of innovation, and of knowledge more generally, has two central elements. A great deal of the knowledge that is important to the operation and improvement of a given process or product technology is "tacit," that is, not easily embodied in a blueprint or operating manual. A closely related characteristic of technical knowledge is that much of it is highly firm specific and results from the interaction of R&D and other functions within the firm.

The process of R&D has often been equated with innovation. If innovation consisted solely of R&D, understanding innovation would be far simpler and the real problems would be far less interesting. Successful innovation requires the coupling of the technical and the economic, rather than being solely a matter of "technology push" or "market pull" (see Mowery and Rosenberg, 1979), in ways that can be accommodated by the organization while also meeting market needs, and this implies close cooperation among many activities in the marketing, R&D, and production functions. The research that goes on within this internal cooperation and communication process is not usually considered as science, but it is essential to successful innovation. The importance of these types of research has been underestimated in the recent past, probably in part because of the use of an oversimplified linear model of innovation that entirely omits them as categories of research.

[4] "Orthodox theory treats 'knowing how to do' and 'knowing how to choose' as very different things; we treat them as very similar. Orthodoxy assumes that somehow 'knowledge of how to do' forms a clear set of possibilities, bounded by sharp constraints, and that 'knowledge of how to choose' somehow is sufficient so that choosing is done optimally; our position is that the range of things a firm can do at any time is always somewhat uncertain prior to the effort to exercise that capability, and that capabilities to make good choices in a particular situation may also be of uncertain effectiveness" (Nelson and Winter, 1982, p. 52).

The role of basic research

Basic research occupies a central place within both the appropriability framework for the analysis of R&D and the science and technology policy structure that has developed under the influence of this intellectual schema. The appropriability analysis of R&D investment essentially equates research activity with basic research: The payoff is uncertain and distant, and the knowledge quickly moves into the public domain. Historically, federal support of nonmilitary research and technology development has focused on the support of basic research, since this activity was assumed to display the greatest divergence between a modest private and high social payoff.

Our analysis of innovation questions many of these assumptions and policy prescriptions and undercuts the basis for the definition of basic research. In this section, we examine the role of basic research within the innovation process. If the returns from this activity are largely nonappropriable by the performer, why do private firms invest in this activity? Our discussion stresses the hazy distinction between basic research and other innovation activities that underpins a portrayal of the innovation process as a linear sequence of phases stretching from scientific discovery to application. In fact, basic research has a complex relationship with most of the other phases of the innovation process.

In 1985, industry performed nearly 20 percent of all basic research within the United States (universities and colleges performed the largest share of basic research, 48 percent). Industry performed nearly 38 percent of all "nonacademic basic research" (a category including basic research by nonprofit institutions, federally funded research and development centers (FFRDCs), and the federal government). A slightly smaller share, roughly one-third (32.9 percent) of nonacademic basic research (total nonacademic basic research was $5.7 billion in 1985), was financed by industry.[5] Incomplete data compiled by the National Science Foundation (NSF) for 1984 indicate that 61 percent of the industry-financed basic research was concentrated in four sectors – chemicals, electrical equipment, aircraft and missiles, and nonelectrical machinery (an industry classification that includes computers) (NSF, 1986).

In understanding why some private firms do basic research, it is necessary to recognize that businesses do not live in a neat, orderly world where causal relationships are clearly defined and where causality works in one direction. The business environment is much more interactive, full of "feedbacks" where some "downstream" development reacts back on, and alters, behavior "upstream." Perhaps most important, it is full of

[5] All data are from National Science Foundation (1987).

unplanned or accidental developments that then turn out to have important consequences of their own.

It is essential to emphasize the unexpected and unplanned, even if – or especially if – it renders serious quantification impossible. In fact, the difficulties in precisely identifying and measuring the benefits of basic research are hard to exaggerate. The point has been expressed succinctly: "Project selection methodologies of a formal, quantitative nature reduce the tendency to perform basic research" (Nason, 1981, p. 24). Difficulties in applying quantitative techniques to basic research planning stem in part from the distinction between risk, where probabilities can be assigned to each of a number of possible outcomes, and uncertainty, where the very nature of outcomes is unknown. As one moves from applied research and development toward basic research, risk declines and uncertainty increases. Most quantitative techniques for investment analysis and planning that are relevant to basic research resource allocation decisions are designed to manage and minimize risk, and cannot deal effectively with uncertainty. Indeed, corporations that support basic research programs rarely subject these activities to the same type of quantitative analyses that are employed for their product development or manufacturing investments.[6]

A second source of difficulty with the quantitative evaluation of basic research investment stems from the fact that the output of basic research is rarely if ever a final product to which the marketplace can attach a price tag. Rather, the output is some form of new knowledge that has no clear dimensionality. The output is a peculiar kind of intermediate good that may be used, not to *produce* a final good, but to play some further role in the *invention* of a new final good. These connections are extraordinarily difficult to trace with any confidence, even ex post. But even if these difficulties could be overcome, the problems of evaluating the knowledge and of providing an appropriate incentive system to reward the knowledge producers appear to be insuperable.

The application of quantitative techniques to the analysis of basic research is further complicated by problems in defining basic research. The distinction between basic research and applied research is highly artificial and arbitrary. The distinction usually turns on the motives, or goals, of the person performing the research. But this is not a very useful distinc-

[6] See the study by the Office of Technology Assessment, *Research as an Investment: Can We Measure the Returns?* (1986), and the testimony in the 1986 hearings of the Science Policy Task Force of the Science, Space, and Technology Committee of the U.S. House of Representatives (1986). One of the most ambitious efforts to trace the payoffs to defense research, Project Hindsight, was unable to compute any measure of the returns on the investment in defense research, ultimately rejecting "the possibility that any simple or linear relationship exists between cost of research and value received." (Office of the Director of Defense Research and Engineering, 1969, p. xxii).

tion. Pasteur in 1870 was trying to solve some practical problems connected with fermentation and putrefaction in the French wine industry. He solved those practical problems – but along the way he invented the modern science of bacteriology. Sadi Carnot, fifty years earlier, was trying to improve the efficiency of steam engines.[7] As a by-product of that particular interest, he created the modern science of thermodynamics.

Other examples of the workaday nature of the motives for and influences on the basic research agenda can be drawn from the history of Bell Telephone Laboratories, a major industrial performer of basic research.[8] Jansky's pioneering research on the sources of static in the new transatlantic radiotelephone service was motivated by the need to reduce or eliminate the background noise. Based on work with a rotatable antenna, Jansky concluded in 1932 that there were three sources of noise: local thunderstorms, more distant thunderstorms, and a third source, which he identified as "a steady hiss static, the origin of which is not known." It was the discovery of this "star noise," as he labeled it, which marked the birth of radio astronomy.

Jansky's experience underlines one of the reasons why it is so difficult to distinguish between basic research and applied research. Researchers often make fundamental breakthroughs when dealing with applied or practical concerns.[9] Attempting to draw the line between basic and applied research on the basis of the motives of the person performing the research – whether there is a concern with acquiring useful knowledge (applied) as opposed to a purely disinterested search for new knowledge (basic) – is hopeless. Whatever the ex ante intention in undertaking re-

[7] Carnot made this utilitarian concern perfectly clear in the title of his short but immensely influential book, published in 1824: *Réflexions sur la Puissance Motrice du Feu et sur les Machines Propres a Développer cette Puissance.*

[8] Hoddeson (1981), pp. 512–44, argues that the decision to strengthen the in-house research capabilities of the Bell System in the early 1900s stemmed from the perception of senior management that the solutions to operating problems could be found only through fundamental research: "Out of Vail's nonscientific goal to create a universal system grew his decision to build a transcontinental line, from which came the technological problem of developing a nonmechanical repeater, and this problem in turn contributed crucially to the start of Bell's formal commitment to in-house basic research. 'Basic' industrial research was now [1910–11] recognized as intrinsically dual in nature, being fundamental from the point of view of the researchers while at the same time supported by the company for its possible applications" (pp. 534–5). Interestingly, the decision to expand fundamental research within the predecessor of Bell Labs followed the discharge in 1907 of two of the firm's senior scientists (both of whom held Ph.D.s), Hammond V. Hayes and William Jacques (Hoddeson, p. 529).

[9] Birr (1966) argues that one of the distinguishing characteristics of industrial research was the use of scientific research methodologies to address technological problems and concerns. As the in-text argument suggests, the merger of the technological and scientific research agenda within the industrial research laboratory makes it possible to exploit an interactive relationship between basic research and technological concerns: Not only can the methods of research be applied to technological problems, but technological issues can also structure and inform the scientific research agenda.

search, the kind of knowledge actually acquired is highly unpredictable. Fundamental scientific breakthroughs have come from people like Jansky or Pasteur or Carnot, who thought they were doing very applied research, and who would undoubtedly have said so if they had been asked.

But the distinction breaks down in another way as well. We have to distinguish between the motives of the individual scientists and the motives of the firm that employs them. Many scientists in private industry could honestly say that they are attempting to advance the frontiers of basic scientific knowledge, without any applied interest. At the same time, the managers who finance research in basic science may be strongly motivated by expectations of useful findings. Thus, Bell Labs decided to support basic research in astrophysics because of its relationship to the field of microwave transmission and to the use of communication satellites for such purposes. It turned out that at very high frequencies, rain and other atmospheric conditions interfered significantly with transmission. This source of signal loss was a continuing concern in the development of satellite communications. It was out of such practical concerns that Bell Labs scientists Arno Penzias and Robert Wilson conducted fundamental research in radio astronomy. Penzias and Wilson first observed the cosmic background radiation, which is now taken as confirmation of the "Big Bang" theory of the formation of the universe, while they were attempting to identify and measure the various sources of noise in their antenna and in the atmosphere. Penzias and Wilson justifiably shared a Nobel Prize for this finding. Their finding was about as basic as basic science can get, and it is in no way diminished by observing that the firm that employed them did so because it hoped to improve the quality of satellite transmission.

The fact that basic research may be financed for profit-seeking motives has other implications. When basic research is isolated from the rest of the firm, whether organizationally or geographically, it is likely to become sterile and unproductive. The history of basic research in industry suggests that it is most effective when it is highly interactive with the work of applied scientists and engineers. This is because the high-technology industries are always throwing up problems, difficulties, and anomalous observations that may occur only within these sectors. In order for scientists to exploit the potential of the industrial environment it is necessary to create opportunities and incentives for interaction with other components of the industrial world.

Basic research should be thought of as a ticket of admission to an information network. This network includes a variety of information flows that fit equally well into the basic or applied categories. There is a high degree of interactivity, embracing work that goes on within the realm of development as well as research.

The attempt to classify research into basic and applied categories is hard to take seriously in some areas and disciplines, for example, in the realms of health, medicine, and agriculture. A strict application of the most common criterion for basic research – research that is undertaken without a concern for practical applications – could easily lead to the conclusion that the National Institutes of Health are not deeply involved in basic research, which is absurd.[10]

There are a number of activities that are essential to the success of business firms that depend heavily on a basic research capability, even if that capability does not play a direct role in solving industrial problems. For one thing, firms need to do basic research to understand better how and where to conduct research of a more applied nature. For another thing, a basic research capability is essential for evaluating the outcome of much applied research and for perceiving its possible implications. In providing a deeper level of understanding of natural phenomena, basic research can provide valuable guidance to the directions in which there is a high probability of payoffs to applied research.

A basic research capability also is indispensable in order to monitor and to evaluate research being conducted elsewhere. As we noted earlier, most basic research in the United States is conducted within the university community (see Chapter 6), but in order to exploit the knowledge that is generated there, a firm must have an in-house capability. It may be difficult for a firm to benefit from university research unless it performs similar research. This observation has some important implications for the fate of cooperative research organizations. Since a larger in-house investment may be necessary to utilize basic research performed outside the firm's boundaries (Cohen and Levinthal, 1989), participants in cooperative research programs often shift the collaborative research agenda away from basic research. These pressures appear to have contributed to recent decisions by such well-known cooperative research institutions as the Electric Power Research Institute (EPRI) and the Microelectronics and Computer Technology Corporation (MCC) to reduce their basic research commitments.[11]

[10] In conducting its resource surveys, the NSF defines basic research as research that has as its objective "a fuller knowledge or understanding of the subject under study, rather than a practical application thereof." By contrast, applied research is directed toward gaining "knowledge or understanding necessary for determining the means by which a recognized and specific need may be met" (National Science Foundation, 1985, p. 221). These definitions appear to mean that if NIH directed a major research thrust into cellular biology to provide the knowledge necessary for the development of a vaccine against AIDS (or a cure for specific forms of cancer), none of the resulting research could be classified as basic. It is hard to see why the determination to deal with a particular disease cannot give rise to research that provides "a fuller knowledge or understanding of the subject under study," even when there is a "practical application" in mind. Here again the introduction of motives or goals is less than useful.

[11] The EPRI primarily serves electric utilities, which support modest in-house research staff and facilities. Historically, EPRI member firms have not been direct competitors, serving different geographic areas – a factor that contributed to the formation of EPRI in 1973.