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Assessing the Economic Value of Research

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Abstract:

For almost half a century World War II and the Cold War provided the political and fiscal context for public investment in science and technology. The report prepared by Vannevar Bush, *Science: the Endless Frontier* (1945), advanced an investment rationale for federal support of scientific research. In spite of pressure from Congress and the Office of the President the scientific community has resisted the development and application of operational economic criteria for the allocation of research resources.

Key Words:
Science, technology, economic value, resource allocation.

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ASSESSING THE ECONOMIC VALUE OF RESEARCH

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For almost half a century World War II and the Cold War provided the political and fiscal context for public investment in science and technology in the United States. As World War II was winding down Vannevar Bush, at the request of President Roosevelt, prepared a report, *Science: the Endless Frontier* (1945) that became the charter for postwar science policy.[1]

For most of U.S. history science policy had been derivative of technology policy. The Bush report insisted, however, that basic research not only contributes to national security but also generates new processes, new products, new industries, and new jobs. Investment in basic research should become an instrument of U.S. development policy. Bush resisted, however, the implications of the investment rationale and insisted that "basic research is performed without thought of practical ends" (Bush, 1945: 18). This contradiction between the rationale for investment in basic research and the allocation of research resources has remained unresolved.

Underinvestment in Research

It was not until the late 1950s and early 1960s that a clear economic rationale for public support of scientific research was articulated. In seminal articles Richard Nelson (1959: 297-306) and Kenneth Arrow (1962: 609-626) argued that the social returns to investment in scientific research exceeded the private returns that could be realized by a private firm. Thus research designed to advance scientific knowledge posses a "public goods" dimension. The benefits from advances in scientific knowledge "spill over" to other firms. A conclusion was that the private sector could be expected to under-invest and that public investment would be necessary to achieve a socially optimal level of scientific research.

The market failure argument was initially presumed to apply to basic, but not to applied, research in the private sector where relatively secure intellectual property rights combined with market incentives were assumed to induce an optimal level of research. However empirical research, initially by agricultural economists, demonstrated that publicly supported agricultural research, across a broad range of commodities and at the sector level, were several multiples the average rate of return on conventional private sector investments (Griliches, 1958; Huffman and Evenson, 1993; Alston, et al., 2000). It was subsequently demonstrated in a wide range of manufacturing industries that social rates of return were significantly higher than private rates of return (Mansfield, et al. 1977: 144-166; Griliches, 1992). High social rates of return have also been demonstrated on publicly supported academic research (Mansfield, 1991: 11-12).[2] An important exception has been the ambiguous results of attempts to measure the private rates of return to research by firms, largely in the defense sector, that conduct publicly funded R&D (Hall 1996: 148-155).

These microeconomic studies, combined with the more macroeconomic research on sources of productivity growth, led to a view that underinvestment in commercial R&D represented a serious constraint on economic growth (Jones and Williams, 1988; Jorgenson, 2001; Ruttan, 2001: 15-47). Two policy conclusions have been drawn from these studies. One is that underinvestment by the private sector should be augmented by public sector research. A second is that intellectual property rights should be expanded to enable private firms to capture a higher share of the benefits from research. The appropriate choice between these two alternatives rests on the importance of the attribution problem—on the extent to which research induced benefits can be correctly attributed to specific research performers. This in turn depends on the size of the locational and temporal spillovers (Alston and Pardey 2001).

By the early 1990s the underinvestment rationale for public sector support was being supplemented by several new analytical developments. One line of argument suggested that the economies of scale realized by firms that are first to introduce new technology may result in a ”lock in” of the initial technological trajectory, even though an alternative path of technological development might be more efficient (David, 1985; Arthur, 1989). A second line of argument, based on strategic trade theory, is that in an industry in which the optimum scale limits the number of low cost producers, wide bodied aircraft for example, subsidization of R&D may determine which firms will remain economically viable (Feller, 1992:119-131). Both the economies of scale argument and the strategic trade arguments suggest that in some industries government support may be necessary to stimulate the optimal level of private sector R&D. Critics have argued, however, that the information necessary to fine tune subsidies to meet the implicit criteria of the two arguments is rarely available to the government (Cohen and Noll, 1991).

Allocating Research Resources

It is somewhat ironic that the underinvestment argument, which was initially developed as a rationale for public support of fundamental or basic research, has found its primary application in the evaluation of the returns to public research devoted to technology development. In the meantime efforts by the scientific community to develop operational criteria for the allocation of resources to scientific research have foundered. Passage of the Government Performance and Results Act (GPRA) in 1992 reflected frustration on the part of Congress about the lack of quantitative information on the relationship between the resources allocated to research and the effectiveness of Federal research programs. The passage of GPRA represent an attempt (ineffective) by the Congress to establish a system to measure the contribution of research effort to the realization of national goals such as security, health, and productivity.

In 1995 the Senate Appropriations Committee asked the National Academy of Sciences of to address "the criteria that should be used in judging the appropriate allocation of funds to research and development activities, the appropriate balance among differing types of institutions that conduct such research, and the means of assuring objectivity in the allocation process (Committee on Criteria for Federal Support of Research and Development, 1995: v). The Academy responded by establishing a study committee under the chairmanship of Frank Press, a former president of the Academy.

Among the several recommendation of the Press Committee substantial attention has focused on Recommendation 4. "The President and Congress should ensure that the Federal Science and Technology (FSAT) budget is sufficient to allow the United States to achieve preeminence in a select number of fields and to perform at a world class level in other major fields." This recommendation has been criticized, correctly in my view, for viewing science as primarily a competitive activity in which relative international standing is more important than the contribution of the knowledge generated in realizing national goals. In effect it would delegate U.S. funding decisions to priorities established by other nations (Robinson, 1997).

The amount the U.S. spends on any area of S&T should be related, not to our preeminence or lack of it, but to the primary social and economic goals to which the S&T is related. Furthermore, preeminence in a field of science does not translate directly into effectiveness in achieving economic and social goals. For example the U.S. is preeminent in almost every aspect of biomedical science--in both the underlying basic science and in clinical applications. But many health indicators for the U. S., such as the infant mortality rate, rank well below similar health indicators for countries with much more limited biomedical research capacity.

The Press report has also been interpreted, in spite of numerous caveats, as embracing a “science for scientists” perspective. It characterized attempts to apply quantitative measures, such as those used to estimate rates of return to technology development, to the evaluation of scientific research as “mindless application (that) can undermine the functions that such measures are intended to improve” (Committee on Criteria for Federal Support of Research and Development, 1995: 27). However, when it is seeking budget increases the science community has been quite willing to embrace the findings of economic research that suggests that the social rates of return are significantly above the private rates of return (Malakoff, 2000).

Modeling the Benefits of Science

In modeling the benefits of scientific research it is necessary to specify more clearly what can be expected from basic research. Basic research can be viewed as an intermediate input that enhances the productivity of applied research and technology development. Applied research can be thought of as a process of sampling from a distribution of potential processes or products. The effect of basic research is to expand the distribution of attributes within which the sampling occurs. By expanding the distribution basic research increases the probability of discovering technically and economically viable research outcomes and of reducing the cost of the search process (Evenson and Kislev, 1975: 140-155: David, et al. 1992: 73-90).

This view leads to what might be termed a “double derived demand” model of the demand for scientific knowledge. The demand for technical change is derived primarily from the demand for commodities and services. The demand for advances in scientific knowledge is in turn derived from the demand for technical change. But how can this model be employed in the development of criteria for research resource allocation if, in fact, the process of search and discovery in basic science—the supply of fundamental knowledge—is governed by a stochastic process?

Four decades ago Alvin Weinberg, then Director of Oak Ridge National Laboratory, attempted to respond to this question in addressing the future of "big science" (Weinberg, 1961: 161-164; 1964: 42-48). Weinberg's first step was to insist on the legitimacy of both internal and external criteria in the allocation of research resources. He emphasized two internal criteria: "(1) Is the field ready for exploitation? (2) Are the scientists in the field really competent? (Weinberg, 1964: 44). External criteria are generated outside the scientific field. They attempt to determine why and how intensively a field of science should be pursued. Weinberg insisted that whether a field has scientific merit cannot be answered within the field itself: "That field has the most scientific merit which contributes most heavily and illuminates most brightly its neighboring scientific disciplines" (Weinberg, 1964: 45). Remarkably Weinberg, a physicist, suggested that molecular biology was the field that, in the early 1960's, most closely approximated his specification.[3]

A similar argument has been made by Paul David and several colleagues (1992: 73-90). David and his colleagues reject cost-benefit or rate of return analysis as a basis for resource allocation to or within basic research. Like Weinberg, they regard basic research as an intermediate input that enhances the productivity of scientific effort in closely related fields, and of applied research and technology development. But "the channels through which basic research yields economic payoffs are so complex, and the assumptions necessary to develop estimates of the returns on an investment in basic research are so fragile and unrealistic, that this exercise is of little use in guiding actual policy decisions (David, et al., 1992: 87).

Assessing Science Research Priorities

Assessment can occur at several levels and stages. These include the peer review of investigator-initiated project proposals at the time they are submitted, bibliographic measures of individual or organizational research productivity, and performance reviews of the program of a research institute or the research portfolio of a science agency (Pacos, Pivic and Teich 1999; Roessner 1999)

As indicated above, there is general agreement among economists that attempts to conduct economic evaluation of science research at the individual project level would be wasteful of both economic and scientific resources. There is also almost universal agreement within the science community that use of the peer review mechanism in the evaluation in the evaluation of project or program project merit should be the primary method of determining the allocation of research resources to individual projects. This consensus is so strong that there is often an implicit judgment that unless research support is obtained through a peer review process "it can't be good science"! Empirical evidence that a peer review system generates better science than other methods, institutional support grants for example is lacking.[4] My own sense is that the commitment of the academic science community to the system of peer review as a basis for project funding is based at least as much on equity considerations as on considerations of research quality or productivity.

There is much less consensus both within the science community and in the broader research community on how to evaluate research programs, institutes, or agencies. It is at this higher level that the issue of relevance to broader social objectives becomes increasingly important—is the research worth doing no matter how well it is done? At this level my own experience suggests that reliance on peer-reviewed investigator initiated proposals or bibliometric measurement of research productivity becomes a less adequate basis for making decisions about research resource allocation. At this broader level external economic and social evaluation becomes both more feasible and more relevant.[5] No project level peer review process would be capable of dealing with Weinberg's stipulation of how to identify the potentially most productive areas of research.

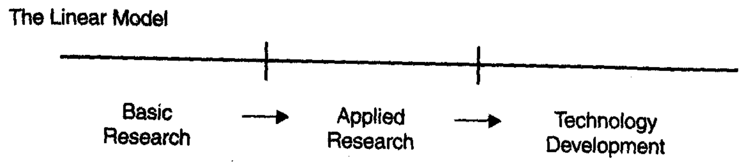
It is clear that we have entered a period when all Big Science R&D programs will be subject to increasingly critical review and reexamination. In decisions about their future technical and economic criteria will carry more weight than in the past. An illustration of this point is the contrasting fate of the Global Climate Change program and the Superconducting Supercollider project.

If we, as a science community, are to be able to successfully, respond to the concerns of society about research productivity it is important that each major mission oriented public research or research funding organization should have a small unit devoted to testing and refining performance criteria and the analytical methods that research managers can use in making research resource allocation decisions and in demonstrating the value of the contributions of the research program to agency administrators and congressional committees. A research organization that does not have such capacity will have great difficulty in responding to its critics or arming its defenders.

An Implicit Social Contract

The post-World War II relationship between the federal government, the science community, and the research universities, where most basic research is conducted, has been governed by what some have termed an implicit social contract. “Government promises to fund the basic research that peer reviewers find most worthy of support and scientists promise that the research will be performed well and honestly and will provide a steady stream of discoveries that can be translated into new products, medicines or weapons” (Guston and Keniston, 1994: 2).

The postwar social contract between the scientific community and society, reflected in the linear model of the relationship of the relationship between science and technology, has substantially eroded since the end of the Cold War (Fig. 1). We have entered a period when investments in basic science “performed without thought of practical ends” will have to be justified primarily in terms of advancing scientific culture rather than in terms of contributions to more specific social or economic objectives. My own sense is that those areas of science and technology for which identifiable benefits are not anticipated within half a century will have increasing difficulty in achieving the credibility needed to lay claim to substantial scientific and technical resources.



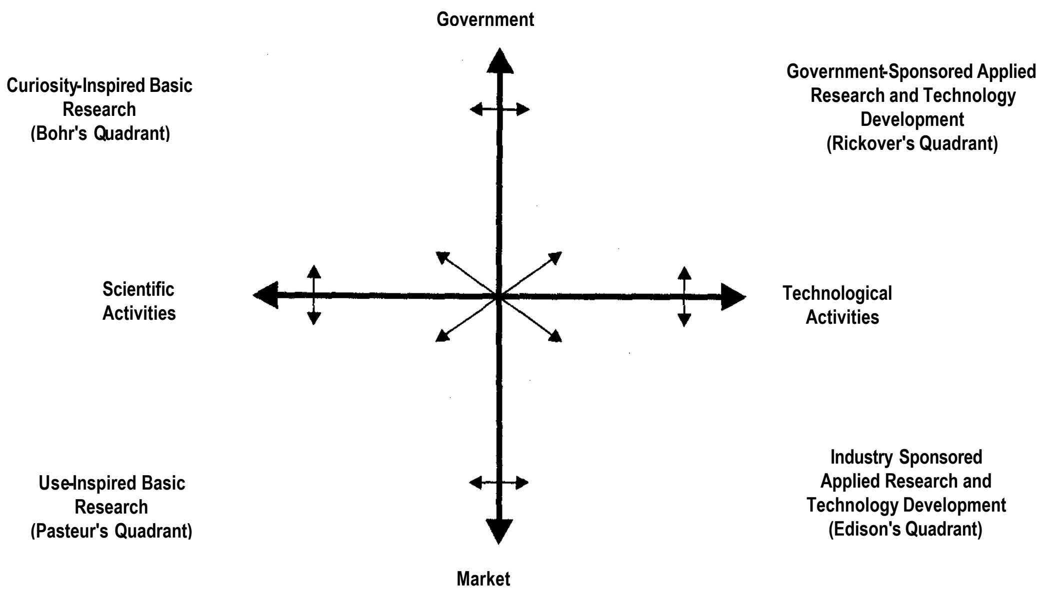


Figure 1. Quadrant model of organization of scientific research and technology development. *(Sources:* Adapted from Donald E. Stokes, *Pasteur's Quadrant: Basic Science and Technological Innovation,* Washington, DC: Brookings Institution, 1997; Maureen McKelvey, "Emerging Environments in Biotechnology," in *Universities and the Global Knowledge Economy*, Henry Etzkowitz and Loet Leydesdorff, eds., London: Pinter, 1997:63.)

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Endnotes

[1] In this paper I draw substantially on my earlier work in the area of science and technology policy. See particularly Evenson, Waggoner and Ruttan (1979); Ruttan (1982), and Ruttan (2001: 534-599). I am indebted to the Minnesota Agricultural Experiment Station, The Rockefeller Foundation and the Sloan Foundation for support of the work on which this paper is based.

[2] The social rate of return reflects the benefits to society as a whole, relative to the costs to society as a whole, expressed as a rate of return to account for the differences in the timing of flows of benefits and costs. The private rate of return correspondingly, refers to the benefits relative to the costs born by a particular group or organization" (Alston and Pardey 1996)

[3] It is of interest that "molecular biology was made possible in part by the application of the tools and experimental techniques of physics and was partly created by converted physicists. The surge of interest in the earth sciences—solid earth geophysics, atmospheric physics and of physical oceanography—has been partly created by the application of physical techniques and concepts in these fields (Brooks 1968: 398).

[4]I have discussed the controversies about peer review in greater detail in Ruttan (2001: 567-570). See also Enserink (2001: 2187-88).

[5] Nathan Rosenberg has argued that opportunities for both ex-ante and ex-post economic evaluation are greatest in areas where technological advances open up new demands and opportunities for advancement of scientific knowledge. "I have in mind, for example, the impact of the work on the transistor by Shockley, Bardeen and Bratton. … Within a few years after the invention of the transistor … solid state physics became the largest subdiscipline in Physics (Rosenberg 1982: 155).