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Economic Approaches to Measuring the Performance and Benefits of Fundamental Science

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PREFACE

This Project Memorandum surveys the different approaches to quantifying the benefits to society in general which derive from the activities and outputs of fundamental science. This paper has been prepared as part of an inquiry into metrics for science. This paper was originally presented in draft form at the "Workshop on the Metrics of Fundamental Science" held on November 18, 1994. The workshop provided an opportunity for experts and evaluation practitioners to critique the prepared papers, to hear other speakers, and to discuss problems of and opportunities for measuring the returns to investment in science. The paper was subsequently revised based on comments received at the workshop.

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ABSTRACT

The contribution to society of investment in fundamental science has generally been assumed to be significant. For the purposes of resource allocation, however, the returns to that investment and the nature of the relationship between inputs, outcomes, and social benefits would be useful information. Analysts have applied several methods to the question of measuring the performance and benefits of fundamental science, with some success. Methods generally used for assessing the contribution of fundamental science are 1) output-oriented approaches that revolve around some form of bibliometrics, and 2) economic or results-oriented approaches attempting to measure the performance and benefits of research.

Output-oriented measures involve the study and analysis of scientific output on the basis of publication-based data. These methods include publication counts, citation analysis, and patent analysis. At its best, bibliometrics attempts to measure the linkages from one article to another to measure the intellectual connections between organizations and the linkages between subject areas. Citation analysis can identify a group of articles that are cited together. These data can help construct a "cognitive map" of scientific research activities, identifying where science is adding value to society. These methods have individual benefits and drawbacks, but as a method of analysis, they cannot by themselves make the connection between the outputs they measure and the influence of these outputs on the creation of social goods, except in the broadest possible sense. Moreover, they have not generally been considered rigorous methods when developing measures of the benefits of research to larger social goals.

Economic or results-oriented approaches to assessment are the more highly developed methodology for searching for quantifiable affects of scientific research. The economic measures examined for this report often were developed to answer other questions and often do not fully reflect the intricacies of the conduct of science. Even so, despite their limitations, economic measures are used to describe the contributions of fundamental science. The methodologies most often employed include production function analysis and social rates of return analysis. Production function analysis is a mathematical mapping of the inputs to the outputs. While acknowledged to have significant limitations, the Cobb-Douglas function is a commonly used form of production function analysis. Social rates of return analysis departs from the narrow specifications of a production function

and seeks to determine the sum of benefits accrued from changes in the knowledge base and compares these benefits with the cost of investment. These benefits are most often measured as changes in consumer surplus. Like the bibliometrics approach to analysis, economic assessment methods have benefits and drawbacks in themselves. Not all benefits of scientific research are fully enumerated, for example, and many times the research process is translated into a workable methodology, though rigorous and quantitative, which may not represent the richness and complexity of science.

Literature suggests that the most detailed method for assessing the influence of basic research on social and economic goals would be to scrutinize the chain of causality emanating from specific research outcomes and to note the downstream effects. This approach has not been fully explored, and has the drawback of being inherently retrospective. Other alternative approaches have been suggested which attempt to bridge the gap between the output-oriented and the results -oriented approaches:

- Susan Cozzens has suggested an approach that would combine quality of life outcomes with the evaluation of basic research activities.
- James Adams has suggested augmenting the Cobb-Douglas form of analysis with information on a representative firm's own stock of knowledge and spillovers or knowledge borrowed from others.
- Adam Jaffee has examined the relationship between patterns of industry research and development, university research and development, and corporate patent activity, by state, asking what are the spillovers to the commercial sector having academic research activities.

Because of a lack of an adequate model for these approaches, however, inferences about the marginal effect of raising or allocating research spending cannot be made on the basis of these experimental methodologies. Nevertheless, the larger implications conveyed by these recent studies is that there is a prospect of employing modifications of the standard production function approach to measure more accurately the specific performance of fundamental research with respect to desired outcomes.

Overall, the literature suggests that, in their present state, none of these approaches are sufficient in themselves to answer the question of what are the economic and social returns from investment in fundamental scientific research. This does not mean that these methods are useless; rather, that only a subset of research activities are subject to empirical testing. Moreover, the empirical methods of assessing returns to science are at best approximations of a technical relationship. They may or may not be accurate representations of the complex series of economic, social, behavioral, and technological interconnections which underlie them. Moreover, we do not know the extent to which these models and methods represent, or fail to represent, what is actually taking place. Despite the shortcomings, there is still value in conducting such studies for the insight they provide, and continuing to push the methodologies to improve their representation of the nature of scientific research.

I. THE SEARCH FOR GENERAL MEASURES OF EFFECT

This paper surveys the different approaches to quantifying the benefits to society which derive from the activities and outputs of fundamental science[1]. After an introduction, the discussion will identify two avenues to such assessment: 1) applying tools such as bibliometrics to *ex post* research project evaluation; and 2) using economic approaches to assessing the social and economic benefits derived from scientific research. The latter will receive the most attention. Intended primarily as an interpretive survey of these methods, the discussion will also consider the implications, strengths, and weaknesses of using such approaches for public policy purposes. While this paper is intended to be a bibliographic survey of assessment methods, a preliminary (if cursory) discussion is in order to outline the difficulties which undercut the potential effectiveness of many of the approaches described.

Certainly, there is good reason to pose the question, "what is the general contribution of fundamental science to society?" The public and their representatives must consider what benefit is provided by the use of public funds to support the fundamental scientific research enterprise. That there does exist a benefit is generally assumed. There is considerable anecdotal evidence providing instances of rich pay-off to particular research findings. Yet, demonstrating a general effect and measuring its magnitude has proven to be difficult both conceptually and as a practical exercise. A "wish list" of those considerations about which we would seek to develop better understanding as a means for measuring the contribution of the publicly funded fundamental research enterprise, would include:

- defining the types of influence basic research has on the attainment of public goals;
- gaining fuller understanding of the channels through which this influence is exerted;
- measuring the magnitude of this influence;

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1 Fundamental science might be considered to be that part of science that requires research to provide new knowledge and understanding of how nature works and of how its many facets are interconnected. The term "fundamental" implies that the knowledge embodied in the science provides the foundation on which all other activities related to science take place.

- assessing the relation to privately funded R&D, both in terms of (a) enumerating the synergies and connections and (b) as a comparative yardstick for evaluating performance;
- measuring the public benefit in relation to national goals that accrues from fundamental science funded through public means;
- a means and rationale for selecting research within a field;
- a means and rationale for allocating resources for research across fields.

This is a maximalist list: we need not seek nor necessarily require that all points be equally well addressed. There do exist techniques and methodologies which shed light on some of these desiderata. By and large, however, the results of this survey suggest it is still early days; for many of these criteria of choice there still remains considerable work to be done in developing metrics of merit and performance.

The difficulties encountered stem from two principal sources. One part of the problem in applying economic and social benefits analysis to determine the returns to investment in science is that we have rarely been explicit in stating *ex ante* what goals we hope to achieve and what, therefore, would be the appropriate axes along which to measure the performance and contribution of fundamental science. This is in some measure inherent in the nature of scientific research itself: if one knows in advance what the outcome will be, then one is not pursuing research. Accordingly, the benefits of scientific research—on both the project level and the global level—have been sought after the fact. The apparent difficulty of making these measurements may have contributed to insufficient attention into how we might phrase goals and milestones at the outset of a project in such a way as to improve the overall process of assessment.

The issue of performance has become a matter of public discussion because the implicit assumptions, including assumptions about appropriate goals, for the public R&D enterprise have come to be viewed as needing explicit validation in the post-Cold War era. National security and wealth and job creation remain crucial public goals, but they are now joined by such considerations as health and education of the citizenry, world leadership in scientific disciplines, environmental quality and sustainability, and ability to make fullest advantage of the revolutions in communications and information technologies.[2] To be sure, these goals are highly inter-related but are also sufficiently

[2] President William J. Clinton and Vice President Albert Gore, Jr., "Science in the National Interest", Executive Office of the President, Office of Science and Technology Policy, Washington, DC, August 1994.

distinct as to suggest that there may be several ways available, and perhaps even necessarily must be used to assess fundamental research returns. The suggestion, therefore, is that at least as important as the particular methodology employed, the practice of assessment must also include explicit attention to the process by which goals are identified and defined.

Even in the arena of assessing benefits *ex post,* there is difficulty in applying economic and social benefits analysis to science. The second of the two major problems revolves around the difficulty of establishing linkage. When applied R&D is pursued, it is usually directed from the outset to a specific end. Fundamental scientific research by its nature is only incidentally related to applied purposes. Its primary intent is to increase the store of basic knowledge. It is not clear *ex ante* when or how such knowledge may be used or how success might be judged.[3] Fundamental scientific research provides a result which may prove to be an intermediate input to the applied R&D process—or it might have value in itself in other than narrowly defined economic or social benefits terms. It is also important to recall that one of the principal arguments in favor of public support of fundamental scientific research is that it generates knowledge which is not easily or exclusively appropriable by private agents and therefore creates a social benefit greater than the private benefit that may be captured by the performer of scientific research. Thus by their very nature, the findings of fundamental scientific research are usually difficult to link to specific applications.

Despite the problems inherent in measuring the returns to investment in science, policymakers continue to seek improved ways of assessing the pay-back from public investment. Where science was once justified largely for its own sake, policymakers increasingly seek to judge science by the extent to which it contributes to both traditional and new public policy concerns. Economic and social benefits analysis may shed some light on the historical returns to a specific project, or to the global benefits derived from investment in science, but these methods have few tools to tie research activities and performance to the service of social goals.

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[3] For example, the 19th century English schoolmaster and logician who engaged in the purely theoretical, if not esoteric, exercise of considering what laws would govern an algebra for a system consisting only of two integers, 0 and 1, could not have known that in Boolean algebra he was inventing the crucial tool upon which the operating languages of all computers would be based.

There are two central threads running through the literature seeking to measure the results of scientific research. One focuses upon assessing the output of the research endeavor while the other seeks more to ascertain the broader effect of these outputs. In the first school of studies ("output oriented"), the intended purpose is to measure research outputs directly; the focus is on the *ex post* evaluation of the research itself. Such studies utilize several methodologies: bibliometric analyses, citation frequency checks, patent surveys, compilations of specific accomplishments of a program or work performed in some area of research, and so forth.

These studies are designed for specific purposes of limited scope. They typically concern measuring the influence of research results within the research community. Because of their intent, they are not designed nor do they lend themselves well to providing a basis for assessing the connection between basic research and larger societal goals nor to the problem of providing performance measures to on-going projects. Bibliometric methods, in particular, cannot by themselves make the connection between the outputs they measure and the influence on social goals except by an implicit syllogism: the greater the research output, the presumed greater welfare. Recently, some researchers have begun to use bibliometric tools to provide data which may then be utilized within the economic approaches to assessment which do speak more directly to the question of benefits stemming from basic research then do bibliometric methods. Some of these studies and approaches will be detailed below.

The second group of studies ("results-oriented") also intends to develop *ex post* measures of output of scientific research in general, but in this case the focus of the measurement is in assessing the presumed effect of research on the larger goals of interest for public policy. These approaches tend to be largely based upon economic theory and econometric methodology. There are several alternative approaches, but such studies almost always use one of the following as the metric of performance: productivity growth, increase in national income, or cost versus benefit—particularly improvement in social welfare as measured by changes in consumer and producer surpluses.[4]

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[4]There are, of course, alternative ways to categorize studies in the aggregate assessment literature. Hertzfeld (1990) offers a useful classification scheme for studies on the contributions by fundamental research. Rather than utilize methodology as an organizing principle as we have done in the present survey, it employs five categories to group studies by issue: (1) research output at the sectoral level, (2) measurement of social welfare, (3) evaluation of emerging technologies, (4) research infrastructure development, and (5) research structure efficiency. Henry R. Hertzfeld, 1990. "An Assessment of Studies of the Contribution of Fundamental Research," draft submitted to

The economic approaches impose a limitation on the applicability of this type of measure in that broader issues of welfare beyond those of directly measurable economic value receive insufficient attention. Only a portion of the benefits ensuing from scientific research are amenable to this avenue of assessment. This highlights the fact that the results ensuing from fundamental research are often not fully enumerated. To be sure, fundamental research leads to a formal "result", usually transmitted in the form of a scientific paper, but other less formal products are also created:

- training of new scientific and technical workers;
- enriched expertise and experience of current researchers;
- development of "schools" of researchers who represent an informal community focused on a particular system;
- exploration of new experimental designs, instrumentation, and research protocols;
- increased numbers of "guesses" about the nature of the system being explored;
- lowering the notional price of an "option" on a prospective technological application which may or may not have some value in the future and for which the basic research finding provides an input;
- negative results which are important in more effectively directing later efforts which may then lead to formal results; and
- the intangible benefits conferred on society and the public imagination by the search itself and knowledge for its own sake.

We should recognize from the outset that some of these outputs may not be amenable to empirical testing given the tools we have at our disposal. Even so, as the discussion below will suggest, only a smaller subset of those which might yield to quantification have in fact been routinely addressed by the literature on assessing the performance of fundamental science in general.

The fact that this asymmetry in the analytical literature exists should not be too surprising. Many of the large issues which have currency at the highest levels of policy (e.g., competitiveness, health, educational attainment, sustainability, quality employment) are themselves poorly defined or are not easily measured. Here again, we recall the earlier point that one of the first steps toward an assessment mechanism must

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the National Science Foundation, Directorate for Science, Technology, and International Affairs, Division of Policy Research and Analysis, October. [photocopy dated May 11, 1992] be a clearer definition of the intended goals against which performance will be judged. As an expedient, analysts rely on existing measures, such as productivity, which do have a clear definition and seem to capture much of what we mean by terms like competitiveness. The approaches tend to be derived from economics not least because of the discipline's richness in quantitative measurement tools, and naturally channel effort in the direction of variables of interest to economists. After all, as a practical matter, the conceptual and methodological challenges of analyzing the benefits from research are already sufficiently complicated without adding the complication of developing new measures. The tendency is to rely upon the tried and true where there already exist a set of defined relationships which possess the virtue of being in common use.

This narrowness of choice in the selection of analytical tools need not necessarily always be the case. One explanation for the relative scarcity of work using non-economic aggregate measures is the one given above, that the task is inherently difficult. But another explanation is that there has not previously been sufficient demand which would have generated studies that approached the question in a rigorous manner. Yet the need should be clear: given the multiple goals we can identify as being among those we would wish to affect positively by engaging in fundamental research, there is danger that too narrow a selection of observational tools will provide a misleading portrait of the full effects of research. A wider range of instruments should be explored.

As an example, consider the amount of money spent over the years on health-related research in the U.S.[5] This has been substantial, reflecting a much larger-share commitment than has been the average in most of the developed world. The goal of this research in part has been to develop knowledge which may then be used to affect such factors as longevity, late-age health, and so forth. Yet, if one were to apply solely economic measures to the problem of assessing effect, one might well conclude that such research is a net decrement to economic growth. As a *reductio ad absurdum*, if one were to measure human existence only within the most narrow definition of economic functionality, the ideal would be for individuals to cease consuming once they were no longer capable of contributing to production, as measured.[6] The clear absurdity of this focus as well as its repugnance to our moral precepts, illustrates the paradox we face in

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[5] I am indebted to Henry Ergas for calling attention to the illustrative power of this example.

[6] This ignores, for the purpose of the simple-minded argument, the role individuals outside the formal work force play in contributing to aggregate demand.

relying solely on economic measures of productivity and growth as a means of assessing the contribution of fundamental research.

There is not much work pointing to alternative means for valuation. NIH, for example, undertakes an internal assessment exercise as means for preparing its briefing for the annual appropriations cycle. It employs a procedure for measuring performance that entails tallying health care costs saved or health benefits gained as the results of research programs and comparing them to the program costs.[7] This requires careful research to develop accurate estimates, but the fundamental methodology is economic cost/benefit analysis—and subject to all that methods shortcomings, as detailed below. The present point, however, is that once again the objects of interested are translated into terms which make the methodology tractable, not necessarily those which speak to the fundamental goals we seek to measure.

One recent exception is contained in a paper by Cozzens (1994).[8] The paper begins to explore a framework for combining quality-of-life outcomes into the evaluation of basic research activities. The conclusion is that considerable work remains to be done if we are to seriously consider incorporating such a methodology into a structure for evaluation. In particular, there is a need to understand more about the links and transmission mechanisms between research outcomes and quality-of-life—or, by extension, other non-economic—desiderata. Secondly, there needs to be an active mechanism to identify and draw into the process evaluators outside the research system whose experience and expertise could provide insights into both what phenomena to be looking for and how to measure them. These themes will be returned to below.

The Cozzens paper is very much the exception. Almost all evaluation mechanisms accord to one of the two main types outlined above. The current state of methodology for studies of the first type, those with of a bibliometric cast, does not usually allow the analyst to address issues of performance in the larger sense that the second set does. Bibliometric approaches will, therefore, be outlined cursorily below but will not receive extensive treatment. On the other hand, it must be recognized that while the second, economic, approach to measurement of research performance explicitly seeks to

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[7] Personal communication, Office of the Director, NIH.
[8] Cozzens, Susan E. (1994). "Quality of Life Returns from Basic Research", photocopy, 3 October, to appear in *The Contributions of Research to Society and the Economy*, Bruce Smith and Claude Barfield, eds., forthcoming. (Washington: Brookings/American Enterprise Institute).

demonstrate the relationship between research outputs and economic performance, the means through which the proposed effects occur and the lines of causality between knowledge input and economic outcome cannot be clearly specified given our present understanding. Neither of these two schools has been given the keys to the kingdom nor demonstrated clear superiority. At this stage, it is probably true that any specific application designed as a tool for policy planning or analysis would need to be purposefully designed by drawing upon both.

The methods described clearly have some benefit for addressing the questions of measuring the contributions of science, particularly when conducting *ex post* assessment on the project or global level. However, these approaches tell us little about how one might adapt these approaches to phrasing goals and milestones for science, or to assessing the performance of science on a continuing basis. This should not be viewed as a drawback inherent in the methodologies themselves. It is a caution against either injudiciously applying these methods over too wide a domain or a myopic view of their sufficiency in telling us all we would wish to know about the performance and affect of fundamental scientific research. The sections below provide details about the application of specific methods to assessing scientific research and the strengths and weaknesses of these approaches.

II. BIBLIOMETRIC APPROACHES

Bibliometrics, a term that covers a wide variety of methods, involves the study and analysis of scientific output on the basis of publication-based data.[9] In its simplest form of publication counts, the publications resulting from a research project, a group of researchers, a department, etc., are tallied with appropriate modifications to account for collaborations or co-authorships. This is a quantifiable, if fairly crude, indicator of the quantity of knowledge produced. What is difficult, however, is establishing a link between this output and attainment of economic or social goals.

Bibliometric methods, in particular, cannot by themselves make the connection between the outputs they measure and benefits derived therefrom except by an implicit syllogism: the greater the research output, the presumed greater welfare. Recently, some researchers have begun to use bibliometric tools to provide data which may then be utilized within the economic approaches to assessment which do speak more directly to the question of benefits stemming from basic research then do bibliometric methods. Some of these studies and approaches will be detailed below.

There are many problems with the use of publications counts. The unit of measurement— the publication—is not a standard value. Even within a field, one publication may not be equivalent to another as a research output. Chemists report their results in numerous short papers; mathematicians publish infrequently but in depth; geologists produce occasional massive accounts of their field work.[10] There is also the problem of how to include within one numerical scale the differing weights that books, articles, co-authored articles, and other forms of publication should have. The use of

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[9] For detailed explanations of bibliometrics, see: van Raan, A. F. J., 1993. "Advanced Bibliometric Methods to Assess Research Performance and Scientific Development: Basic Principles and Recent Practical Applications," review report based on invited paper, University of Leiden Report CWTS-93-05, August; Cozzens, Susan E., 1989. "Literature-Based Data in Research Evaluation: A Manager's Guide to Bibliometrics," report to the National Science Foundation; Narin, Francis, Dominic Olivastro, and Kimberly Stevens, 1994. "Bibliometrics/Theory, Practice and Problems." *Evaluation Review*, Vol. 18, No. 1, February, pp. 65-76; van Raan, A. F. J., 1993. "Advanced Bibliometric Methods to Assess Research Performance and Scientific Development: Basic Principles and Recent Practical Applications," review report based on invited paper, University of Leiden Report CWTS-93-05, August. For individual bibliometric methods, see notes on following pages.
[10] Ziman, John, 1994. *Prometheus Bound: Science in a Dynamic Steady State.* (Cambridge: Cambridge University Press); p. 104.

publication counts may itself skew incentives sufficiently to reduce whatever value such an approach might otherwise have had.[11] And obviously, publication counts can't measure quality.

This last problem has been addressed by shifting to citation counts. Citation counts are often used in combination with publication counts for large research units so that one can calculate an average of the number of citations per paper as a measure of relative effect. But again, there are various ways of inflating citation counts and not all types of papers are cited at an equal rate: there is evidence, for example, that methodological papers are cited more often than other types of papers. And it is important to note that citation counts measure the impact of a piece of research within the scientific community, which is not synonymous with quality. There is also the peculiar effect caused by negative citations: The discredited Pons and Fleischmann paper on cold fusion was among the 10 most cited papers in physics for several years.[12]

Some recent work has sought to explain the sources of relative differences observed between research teams on the basis of collected bibliometric data. Using the metric of citation counts per dollar of project cost as a measure of research efficiency, Averch (1989) utilizes an econometric approach to determining the contributions to this measure, regressing his metric on such factors as the currency of the investigator's degree, the labor intensity of the project, an assessment of investigator quality and so forth.[13] The data set is based upon a random sample of chemistry project renewals by NSF. As will be discussed below, the difficulty in making strong statements about the theoretical connection between the variables under examination make it difficult to attribute causality—and hence policy relevance—to the results.

Recent studies in bibliometrics have attempted to measure linkages from one article to another to measure the intellectual connections between organizations, and linkages between subject areas. Co-citation analysis identifies groups of articles cited together in other articles. These groups are used to construct a 'cognitive map' which can help to chart the evolution of research team's contribution to that field. But again, bibliometric

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[11] "Quantity No Longer Counts in Britain," *Science*, June 24, 1994, p. 1840.
[12] "Citation Rankings: No Technical Knockout?" *Science*, May 14, 1993, p. 885.
[13] Averch, H. A., 1989. "Exploring the Cost-Efficiency of Basic Research Funding in Chemistry", Research Policy, vol. 18, no. 3, pp. 165-172.

methods cannot directly measure quality and they also have difficulty in equating the outputs they measure with social and economic benefits.

To address this lacuna, patent analysis or patentometrics applies bibliometric methodology to patents or patent citations.[14] Patent analysis may be useful for R&D in which technology, as embodied in patents, is the primary output. But patent counts are of limited use in tracking the outputs from basic research, and the relationship between patents and economic benefits is unclear and can vary widely from patent to patent. Many firms rely on trade secrets, copyrights, or even on being first to market as the primary means by which they protect valuable innovations.

Patent citation counts are similar to citation counts. In theory, these citations enable evaluators to find key scientific papers which form the basis for commercial innovations with some form of economic impact. Although this may serve to establish a link between fundamental science and economic impact, the link is one step more indirect than counting patents themselves. It would be misleading to establish a one-to-one link between fundamental science research which is cited in a patent and an economic benefit, because there are many factors involved in the creation of a commercial innovation, specific bits of technical knowledge being but one of these.

A method developed by Martin and Irvine attempts to mitigate some of the negatives of bibliometric methods by combining several of them. This approach, known as converging partial indicators, is actually not a method in itself, but rather an approach that evaluates R&D by looking at several performance measures together. Ideally, the set of performance measures should all give matching results, meaning that one has more confidence in the answer than one would if the evaluation were conducted using only one indicator.[15] A problem remains, however, in that bibliometric indicators may all err in the same direction. Further, there is the unresolved problem of what course of action should be taken when a set of indicators are in disagreement. As performance

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[14] For detailed information on patent analysis, see: Papadakis, Maria, 1993. "Patents and the Evaluation of R&D," in Barry Bozeman and Julia Melkers, eds., *Evaluating R&D Impacts: Methods and Practice*, (Boston, Kluwer Academic Publishers), pp. 99-122; Narin, Francis and Dominic Olivastro, 1992. "Status Report: Linkage between technology and science." *Research Policy*, June, pp. 237-249.
[15] See Martin, Ben R. and John Irvine, 1984. *Foresight in Science: Picking the Winners.* (Dover, Frances Pinter Publishers).

indicators, converging partial indicators offer the same limited uses as bibliometrics in that direct links between goals and R&D are difficult to establish.

Bibliometric methods, in the last analysis, cannot by themselves make the connection between the outputs they measure and the influence on social goals except by an implicit syllogism: the greater the research output, the presumed greater welfare. Recently, some researchers have begun to use bibliometric tools to provide data which may then be utilized within the economic approaches to assessment which do speak more directly to the question of benefits stemming from basic research then do bibliometric methods. Some of these studies and approaches will be detailed below.

III. ECONOMIC APPROACHES TO ASSESSING EFFECT

Most direct indicators of R&D outcomes such as citation and other bibliometric indicators are only indirectly (and loosely) related to those aspects of economic and other goals of public improvement which are of greatest interest. They have not usually been the metrics of choice for developing convincing general measures of performance for research as an input to other systems. By contrast, the economic indicators are a direct expression of what most interests us, but must be viewed as indirect indicators of the output of the research and development process. This is because the relationship between R&D and economic outcomes is itself still the subject of intense scrutiny, research, and debate. The direct effects of research outcomes on a variety of goals are obscured by the multiplicity of complementary factors which also enter into the equation. This is the fundamental quandary bedeviling attempts to measure both fundamental and applied research and development performance.

A further complication is that most attempts to quantify the effect of research outcomes on social and economic welfare have concentrated on applied research and the development end of the R&D spectrum rather than basic research. This might not be explicitly stated, but unless pains are taken by the researcher, the data will, because of accounting practices and other technical reasons, tend to be weighted heavily toward the applied end which certainly reduces the potential applicability of these findings to basic research performance.

Yet, there is no question that the economic approach to assessment, by reason of its success and ready applicability, is far and away the most highly developed methodology for searching for the effects we seek to quantify.

Two fundamental approaches to economic assessment will be treated here: those methodologies based on production function analysis, and those which use social benefit and social rate of return as a metric.

PRODUCTION FUNCTION ANALYSES

The production function, a mathematical mapping of inputs to outputs, is the foundation for several economic approaches to research performance assessment. This methodology may be applied at the level of the firm, the industry, or the economy as a whole. Studies of the effect of research activities on productivity and on the usual measures of growth accounting such as national income, both in aggregate and on the margin, are predicated on the production function approach.

The earliest studies in this field treated technical progress as a residual--literally what was left unexplained after accounting for the economic growth directly attributed to the increase in other inputs—labor and capital—available to the economy.[16] The fundamental relationship upon which all such analyses are predicated is the production function relating firm, sector, industry, or national economic outputs, **Y**, to the set of inputs used to produce them:

$$\mathbf{Y} = \mathbf{A}\,(\mathbf{T}, \mathbf{t})\; \mathrm{F}\,(\mathrm{K}, \mathrm{L}) \qquad (3.1)$$

In this case the simplest form of the production function has been extended explicitly to include technical knowledge as an input, represented by the bold-faced terms in equation 3.1. Output (**Y**) is said to be a function of the traditional inputs, capital (**K**) and labor (**L**), but also of a nested function which produces change in overall factor productivity, **A (T,t)**. This represents the apparatus through which basic and applied research provides useful inputs to the overall production function, leveraging the productive power of the other inputs. This latter function has as its arguments technical knowledge, **T**, and time, **t**, which captures disembodied technological change.

Because of its computational and interpretive ease, a frequently used specification of the production function is the Cobb-Douglas form which is conveniently linear in logs:[17]

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[16] The progenitor of much research along these lines dates to papers written in the late 1950s by Solow and Abramovitz. The growth accounting variations on this theme focus on increases in some stock variable of interest--Gross National Product, Gross Domestic Product, Net National Product, and so forth. Denison's work along these lines has been geared to reducing the residual as far as possible to identify the components contributing to rates of growth in national income. In Denison, 1979 he calculated that during the quarter century, 1948-1973, what is termed advances in knowledge contributed 1.1 percent to the rate of growth of national income. This represents a third of the total rate of growth. The principal proxy of the stock of technical knowledge is based on R&D expenditure, the problems with which will be explored below. E. F. Denison, 1979. *Accounting for Slower Economic Growth: The United States in the 1970s*. (Washington, DC: Brookings.)

[17] This form dates back to the 1940s and its inherent shortcomings have been dealt with by exploring alternative and in many ways more sophisticated specifications, such as the constant elasticity of substitution (CES) production function, over the years. Nevertheless, it remains a useful form for illustrating the basic relationships between

$$Y = A\,e^{at}\,(T)^{b}\,K^{c}\,L^{1-c} \qquad (3.2)$$

where **a** is a shift parameter, **A** is a constant, **t** is time, and the inputs are physical capital (**K**), labor (**L**), and knowledge capital, **T**. Depending on the specific study, researchers may then use this form to further define total factor productivity, the most inclusive single representation of change in the inherent productive power of economic inputs, as

$$TFP = Y/(K^{c}\,L^{1-c}) = A\,e^{at}\,(T)^{b} \qquad (3.3)$$

and the appropriate mathematics then yield that the growth of total factor productivity between time periods may be expressed as

$$GTFP = a + b\,((d(T)/dt)/T) \qquad (3.4)$$

where **b** is the elasticity of output with respect to technical knowledge, that is the percentage change in output given a 1 percent increase in technical knowledge inputs, or

$$GTFP = a + r\,(T/Y) \qquad (3.5)$$

where **r** is the marginal product of technical capital.[18] If **T** is stated as a net investment (roughly equivalent to assuming that the accumulated stock of R&D depreciates slowly,) **r** may be interpreted as its rate of return.

Before preceding further with a discussion of the particular uses to which this analytical approach may be put, its underlying assumptions should be made explicit.

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the variables and does capture the essence of the implicit simple model which lies behind this view of the economic process of technological change.

[18]Both forms of the equation for growth in total factor productivity may be estimated but they carry different implicit assumptions. Equation (3.4) constrains the elasticity of output to be the same across all the data in the sample, while in (3.5) it is the rate of return which is deemed to be constant. (Peter C. Reiss, 1985. "Economic Measures of the Return to Federal R&D," presented at the Workshop on The Federal Role in Research and Development, November 21-22.)

(1) It clearly implies a linearity in the underlying processes. There are no complex feedback relationships between the variables. The assumption is that there is a constant returns-to-scale technology.[19]

(2) It should also be noted that technology is considered to be neutral: it favors the factor shares of neither labor nor physical capital.

(3) Further, this form implicitly treats input and output decisions as sequential, ignoring economic simultaneities.[20]

(4) The various inputs to the Cobb-Douglas equation are lumped together as single entities which implies a homogeneity of inputs (and outputs). In line with the assumptions of linearity, spillovers effects are not captured. In particular, it is clear that factors other than those specified, such as differentials in managerial competence or other organizational particularities, will not enter into the equations.

(5) Finally, the Cobb-Douglas functional form blurs the distinction between technical knowledge as an input to the production function yielding change in total factor productivity (**A(x)**,) and the production function which maps the physical inputs, **K** and **L**, into output.[21] This form fails to provide an explicit statement of the relationship between cause and effect.

Even in production function analyses which do not bring in the complication of accounting for the contribution of technical knowledge, there are considerable problems of definition and data acquisition when aggregating capital and labor into one form. Yet both of these inputs present a greater aspect of analytical tractability when compared to the difficulties which arise with the inclusion of the technical information term, **T**.

Most often, **T** is proxied as an accumulated stock of past research and development expenditures, variously discounted and depreciated. This leads to conceptual and technical difficulties. When research and development expenditure is used as the proxy for the stock of technical knowledge, the net effect is to use the input to the technical knowledge production function as the measure of its own output. This problem underscores the conceptual deficiency which lurks behind all the economic approaches to aggregate performance: the nature of **T** is less clearly defined than are those of **K** or **L**,

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[19] One of the themes running through the work of those associated with the new "endogenous growth" school of the economics of technological change is that observable phenomena associated with instances of innovation are perhaps better characterized by complex non-linearities and increasing returns to scale.

[20] Zvi Griliches, 1979. "Issues in Assessing the Contribution of Research and Development to Productivity Growth", *Bell Journal of Economics*, vol. 10, pp. 92-116.

[21] Reiss, op. cit.

and the processes through which technical knowledge has its ascribed effect on the larger system are considerably less well-understood.

Further, the size of the technical knowledge or R&D stock is particularly prone to externalities because of the greater public goods aspect that attaches to technical knowledge. It is difficult, therefore to proxy the research and development input solely by tracking private expenditures because actions by state and federal governments also make a contribution to the stock of technical knowledge.[22]

If publicly funded research becomes the focus of this approach, however, other problems arise. Only a fraction of such research was even intended to affect the variables of most interest to economists and by which they calculate performance. The calculation needs, therefore, to be made somehow on a restricted set of such research. On the other hand, a good deal of research originally intended to serve other missions, and therefore likely candidates to be pared from a more restricted set of federal research and development, has, in fact, contributed to public welfare through indirect knowledge transfers. The phenomenon of spillover is probably larger for publicly funded than private research and development.

Even if these conceptual issues were not present, it is difficult as a practical matter to determine from the usually available data sources the use to which research and development expenditures were put. Allocating proportions of aggregate data between basic and applied research--and research in general versus development--is not easy. Most reporting and accounting methods will tend to be biased toward reporting activities occurring toward the development end of the spectrum and will underreport more basic work. Further, the production function approach in general will do a better job ascribing measurable performance effects to research and development designed to improve existing processes than to those efforts intended to create new products or to introduce revolutionary change into production technology. This is because of the lack of historical data upon which to base estimation.[23] Kochanowski and Hertzfeld point out that it is in the mission areas, usually geared toward product development, that most federal R&D

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[22]Harvey A. Averch, 1994. "Economic Approaches to the Evaluation of Research," *Evaluation Review*, vol. 18, no. 1, February, pp. 77-88.
[23]Ibid.

has traditionally been expended.[24] This is a partial explanation for the low rate of return of federal research and development compared to private R&D found by several studies.

The general result which stems from the body of studies in this field may be stylized as follows: the returns to research and development in general are high, but when private and public research and development are separated, the returns to federal expenditures in research and development are low, in some cases not significantly different from zero.[25]

This latter result would be troubling if only interpreted narrowly. As it is, it requires closer examination. As noted above, much federally funded research is directed to areas where there is only a limited market at best. Further, given the types of data available, the returns that result from most calculations must be interpreted as average rather than marginal rates. From the policy perspective, this means we cannot be certain of the implications from the aggregate analysis for what the effect of an additional dollar of expenditure on research might be. Because of these theoretical and practical difficulties as well as those which have already been alluded to, such results instead suggest the need for more detailed examination of the fundamental issues.

One example of research relevant to the purpose of this survey directly estimates the growth in total factor productivity, equation (3.5), to test the hypothesis that federal R&D expenditures for research conducted at firms will lower the observed marginal rate of return. That is, the increased resources will allow firms to augment their research staffs and also capture economies of scale, thereby lowering the effective marginal cost of firm financing of R&D. This would, in turn, effectively permit more research and

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[24]P. S. Kochanowski and Henry R. Hertzfeld, 1981. "Often Overlooked Factors and Measuring the Rate of Return to Government R&D Expenditures", *Policy Analysis*, vol. 7, pp. 153-167.

[25]Some summaries of the studies which support these stylized facts are Zvi Griliches, 1973. "Research Expenditures and Growth Accounting," in *Science and Technology in Economic Growth*, B. R. Williams, ed. (New York: Wiley) pp. 59-83; Zvi Griliches, 1980. "Returns to Research and Development Expenditures in the Private Sector," in *New Developments in Productivity Measurement and Analysis*, J. W. Kendrick and B. N. Vaccara (eds.) (Chicago: University of Chicago Press) pp. 419-454; Albert N. Link, 1981. "Basic Research and Productivity Increase in Manufacturing: Additional Evidence", *American Economic Review*, vol. 71, pp. 1111-1112, December; Terleckyj, E. Nestor, 1974. *Effect of R&D on the Productivity Growth of Industries: An Exploratory Study*. (Washington: National Planning Office; and Terleckyj, E. Nestor, 1980. "Direct and Indirect Effects of Industrial Research and Development on the Productivity Growth of Industries," in Kendrick and Vaccara, op. cit., pp. 359-377.

development investment, making a lower marginal rate of return attractive.[26] The result, reported by Link (1982) estimating over 51 firms receiving federal research and development funds, was that those receiving greater amounts of such moneys had a significantly lower marginal rate of return (23 percent vs. 41 percent).

This carries an important implication. Attempts to measure the results of federal research and development expenditure directly are to some extent misplaced. Indeed, studies have shown an apparently low return to such efforts. But the cost/benefit framework may itself be too restrictive, failing to capture the many types of benefit which may be derived from publicly-funded basic research. The true effect of such outlays may well be indirect, affecting productivity through changing the returns to private research and development rather than directly as a result of the specific research project.

In addition to the usual caveats with the Cobb-Douglas approach, the data used in the Link study were point estimates and not time series, nor were lags or other complications explored. Therefore, the results may only be considered as being indicators of the need for additional work. Further, the policy implications are uncertain: Are federal research and development expenditures best viewed as a complement, leveraging private research and development resources, or are they being used as a substitute for outlays that might otherwise have come from industry, allowing firms to reduce their private risk by utilizing or even pressing for the creation of federal research programs?[27]

There are other ways in which federal research presents greater problems for measurement and benchmarking than does private research and development. Methods for expensing federal research and development differ from those used for privately funded research. More often than not, studies have not been able to disaggregate federal research and development by taking into account the various categories of work covered by these outlays.[28] Different intentions led to establishing the various different programs

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[26]Albert N. Link, 1982. "The Impact of Federal Research and Development Spending on Productivity," *IEEE Transactions on Engineering Management*, Vol. EM-29, No. 4, November, pp. 166-169.

[27]Mowery, for example, suggests that the change in the federal government's attitude toward providing public support to academic research led to a lowering of the direct support of such activity by industry. (David Mowery, 1990. "The Growth of U.S. Industrial Research," Haas School of Business, University of California, Berkeley [photocopy]).

[28]A study which does do so, however, is D. M. Levy and E. Nestor Terleckyj, 1984. "Effects of Government R&D on Private R&D Investment and Productivity," *Bell Journal of Economics*, vol. 14, pp. 551-561.

under which the federal government offers R&D support. Some provide funding for work done under contract, others have been established as grants programs, while still others are intended to support research conducted intramurally by agencies. Is it reasonable to assume that they can best be measured for effect using the same metric?: The implicit assumption in doing so is that these different types of investment should somehow have the same outputs and similar effects on measured goals.

A study by Adams departs from most aggregate analyses in that it provides an approach which, if more fully developed, could explicitly address not only the phenomenon of spillover for which it was intended, but might also provide a means for examining the indirect effects of research likely to prove crucial to assessing the benefits of fundamental research.[29] This study uses an alternative measure of the store of technical knowledge. It uses the standard Cobb-Douglas form but introduces two distinct knowledge inputs: the representative firm's own stock of knowledge, and spillovers or knowledge borrowed from others. To represent these knowledge inputs theoretically, Adams further nests relations modeling the production of the two types of knowledge within the main production function. Thus, for example, the stock of own knowledge available before production is itself a product of the number of scientific workers in the field and the prior knowledge which existed at an even earlier time when search began, weighted by industry. This specification is intended to capture the heterogeneous (industry- and discipline-specific) nature of fundamental science. The fundamental unit used to construct the knowledge stocks for the estimation equations is not the proxy of discounted R&D expenditure, but the number of scientific papers produced by year deflated over time by obsolescence rates. In this regard this study suggests how to connect the economically oriented type of study and those predicated upon bibliometric and other computational output assessment methodologies.

The principal findings to emerge from this study are that lags between the appearance of research in the academic community and the resulting effect on productivity in the form of knowledge absorbed by an industry, represented by the search and gestation parameters of the estimation equations, are on the order of 20 years. If, on the other hand, academic technology and academic science enter into the equation through the spillover mechanism, they exhibit lags on the order of 10 years and 30 years respectively. All figures cited are, of course, averages around which there can be considerable

[29]James D. Adams, 1990. "Fundamental Stocks of Knowledge and Productivity Growth", *Journal of Political Economy*, vol. 98, no. 4, pp. 673-702.

variance. These are considerably greater than the lags assumed elsewhere.[30] If these estimates are accurate, this would suggest another reason why the individual firm might appear to underinvest in basic research in spite of findings which suggest a high return to such effort. Finally, it is important to note that this study methodology also finds support for the contention that knowledge is an important contributor to productivity growth.

This last result is not surprising in that the model was constructed on the explicit assumption that changes in theoretical understanding, as opposed to activities more in the nature of evolutionary tinkering with existing technology, underlie most recent technological change and hence growth.[31] Further, the delicate structure of this analysis rests upon a problematic store of data. As the author points out, the science and engineering employment data are suspect and the time series on publications begs fundamental questions such as the implicit equivalencies in value between papers in different fields. Note also that at these long periods of lag, there may be a violation of the assumption of variable independence between the knowledge proxy and the scientist number. Individuals may make educational decisions based upon what fields are "hot" academically. Finally, although intended to assess basic knowledge contributions, it is not possible on the basis of these data to distinguish between basic and applied contributions. Nevertheless, this is an avenue which might be usefully explored as an alternative to standard proxies for the research and development component in production function analyses.

Another study spanning the space between the bibliometric "bean counting" (research output-oriented) and the production function (effects-oriented) methodologies is one by Adam Jaffe.[32] He examines the dynamics of the relationship between patterns of industry research and development, university research and development and corporate

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[30] Edwin Mansfield, 1991a. "Academic Research and Industrial Innovation," *Research Policy*, vol. 20, pp. 1-12. Note, however, that the Mansfield study focused on the effect of "recent" academic research and so was deliberately bounded.

[31] In fact, given the long lags found in this study between basic scientific idea generation and use, the author suggests it is possible that the downturn of productivity in the 1970s might have been partly a result of the interruption of scientific paper production during the Second World War. The assumption is that the formal scientific paper is the primary product and transmission vehicle of basic research.

[32] Adam B. Jaffe, 1989. "Real Effects of Academic Research," *American Economic Review*, vol. 79, no. 5, December, pp. 957-970.

patent activity, by state.[33] The question being asked is what are the spillovers to the commercial sector from having academic research activities located in geographic proximity to industrial research and development. Patent activity in four areas was examined across 29 states: drugs and medical; chemicals; electronics, optical, and nuclear; and mechanical. (Whereas there are over 300 patent classes, the statistics on academic research are given by discipline; the two schemes may be matched only at a coarse-grained level of aggregation.) Again using a Cobb-Douglas form of a knowledge production function, the results of several regressions provide some evidence at the state level of commercially applicable spillovers from university research. The effects are significant not only statistically, but also in the sense of being of sufficient magnitude to suggest that there are phenomena occurring which warrant the attention of federal research managers.

The problem with the approach is that it is not suited to detail what these phenomena might be. How spillover might occur, through which channels it moves, and even what its precise nature might be cannot be resolved by the purely technical relations needed for regression estimation. The results of the analysis show a greater effect attributable to academic research when split into the four broad areas than it does to university research overall. That is, it is the more disaggregate analysis which yields the greater measure of economic benefit using this metric. This suggests "that spillovers are limited to specific areas and not just the diffuse effect of a large research university."[34]

The result also suggests once again that we still do not understand enough about the specifics of knowledge generation, transmission, and utilization processes to be able to provide meaningful interpretation to general findings. The discussion in the paper provides the appropriate caveats and then suggests a causal relationship to the basic findings: "university research causes industry research and development and not vice versa," by attracting industrial research and development ventures and augmenting the productivity of the resulting activity. In other words, this strengthens the claim that the indirect effects of basic research might be the most significant. This argument, however,

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[33]The author notes that using states as geographical units is somewhat arbitrary and highlights the desirability of pursuing this methodology using data based on SMSAs. Even so, one wonders about the bias introduced by the fact that major research universities are largely coincident in large cities with the location of major corporate headquarters, the place where patents are likely to be filed irrespective of where the primary research work would have been performed.

[34]Ibid., p. 968.

would not appear to be a well-supported inference from the data and analysis provided. In any case, because of the lack of an adequate model for spillovers, inferences about the marginal effect of raising research spending cannot be made on the basis of this study or the current state of its methodology.

Nevertheless, the larger implication conveyed by these recent studies is that there is a prospect of employing modifications of the standard production function approach to measure more accurately the specific performance of fundamental research with respect to desired outcomes. This would require employing a broader set of variables than has traditionally been the case and altering the fundamental specification of relations between variables in order to more accurately reflect the phenomena we seek to understand.

SOCIAL RATES OF RETURN

The second main method for measuring economic returns to research departs from the narrow specifications of a production function and instead seeks to determine the sum of benefits accrued from changes in technology and compares these benefits with the cost of the technology investment. This is fundamental cost/benefit analysis. The researcher may then, by analogizing to the internal rate of return calculations performed in private firms, proceed to compute a "social rate of return" to the innovative activities being examined. These benefits are most often measured as change in consumers' surplus--the additional benefit enjoyed by consumers who are able to buy the goods they desire at prices lower than they would have been willing to pay, and in producers' surplus--the additional benefit enjoyed by producers who receive a greater margin over their cost for the goods they sell. The social benefit may therefore be considerably greater than the private benefit taken in the form of profit by the producer.

This approach also has its pitfalls. Once again, causality is imputed by the research design while the mechanism of causation is not detailed. There are also difficulties of measurement. When assessing consumers' surplus, it is relatively straightforward to account for goods and services which become cheaper. But what of commodities which are qualitatively different from those which had gone before, the true revolutionary innovations which establish entire new markets? (This is to say nothing of the social benefits whose value may be difficult to express in economic terms.) Similarly, accounting for the welfare gains for producers whose products gain market share and revenue through innovation is usually more tractable than to account for the losses by those producers whose products are rendered less attractive and who may even be forced from the market.

As a practical matter, such studies involve selecting a sample of specific innovations upon which to perform these calculations. This is both expensive and subject to non-randomnesses of one sort or another in the selection process. The production function approach reduces the need for this.

Further, there are conceptual difficulties with interpreting what the social rate of return (SRR) calculation may be telling us. SRR cannot accurately be interpreted as the compound annual yield of an investment. Typically in the case of R&D the initial investment occurs over a period of several years and the benefits derived from it, also spread over a long period, generally come at a non-uniform annual rate.[35] This, combined with the uncertainties inherent in research activities, makes SRR a poor tool for *ex ante* research selection.

There are also problems with this approach as a means for retrospective assessment of basic research. When placed into the type of estimation formulae developed for calculating the return on (short term) investment in industry, basic research that yielded results many years before those results could be incorporated into a commercial product will be heavily discounted. It is not certain that the results from this peculiar type of long-term investment should be discounted in the same way that prudence and good accounting practice would dictate in the case of the more familiar short-term investments for which these analytical techniques were developed. Further, it must be recognized that costs will be easier to identify and account for than benefits. This is especially true for basic research and even more so for that research financed in part or in whole by the federal government. The externalities, spillovers, and other indirect effects may be difficult to ignore and difficult to quantify. The resulting policy conclusions might be based on an undervaluation of the contribution of such research.

When looking at broadly defined research and development, Mansfield[36] concludes there is a social rate of return to research and development from the selection of 17 innovations

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[35]Gregory Tassey, 1994. "The Rates of Return to Private and Public Investment in Technology", National Institute of Standards and Technology, draft, March.
[36]Edwin Mansfield, John Rapoport, Anthony Romeo, Samuel Wagner, and George Beardsley, 1977. "Social and Private Rates of Return from Industrial Innovations," *Quarterly Journal of Economics*, vol. 91, no. 2, May, pp. 221-240.

included in his study. The median is 56 percent compared to a lower private rate of return of 25 percent. In a similar study of 20 process and product innovations, Tewkesbury finds a SRR of 99 percent.[37] Nadiri's compilation of such studies finds that this 2:1 spread holds on average.[38] Note, however, that these are the returns to the total investment in the selected innovations, not just the research component.

For the more narrowly defined category of basic research funded by industry, Mansfield[39] finds a SRR of 178 percent. In a similar vein, Link concludes that the SRR to basic research in firms is 231 percent.[40] The same study found the SRR to government funded basic research was 117 percent. Again, note that these studies were performed for selected specific research areas.

In a later paper which gained considerable attention from federal research managers, Mansfield attempted to isolate the social rate of return due solely to academic research, (not, however, necessarily basic research.)[41] The study was based on a survey asking business executives in a number of sectors to provide information on innovative projects at their firms. The survey asked which products and processes would either have been seriously delayed or were developed with considerable assistance from recent academic research. Using the surveys and ancillary data sources, the study concluded that on the basis of the sample of 76 firms across seven industries 11 percent of new processes and 9 percent of new products introduced by those industries in the U.S. in 1975-1985 could not have been developed (without substantial delay) in the absence of recent academic research. Using similar extrapolation, calculations of the 1985 sales of products dependent on academic research first commercialized in 1982-1985 yielded an estimated value of $24 billion or 3 percent of total sales of major firms in the seven industries. The corresponding figures for processes are $7.2 billion and 1 percent.

The survey indicated that a further 8 percent of products and 6 percent of processes could be categorized as ones which could have been developed without, but received "very substantial aid" from, recent academic research. These amounted to an estimated $17.1

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[37]J. G. Tewkesbury, M. S. Crandall, and W. E. Crane, "Measuring the Societal Benefits of Innovation," *Science*, Vol. 209, August 8, 1980, pp. 658-209.
[38]M. Ishaq Nadiri, 1993. "Innovations and Technological Spillovers", (NBER Working Paper No. 4423), August, as cited in Tassey, op. cit.
[39]Edwin Mansfield, 1980. "Basic Research and Productivity Increase in Manufacturing," *American Economic Review*, vol. 70, December.
[40] Link, op.cit.
[41]Mansfield, op. cit., 1991.

billion (2.1 percent) and $11.3 billion (1.6 percent) in sales, respectively for the major firms in the seven industries. The paper further calculated the lag between crucial research finding and commercialization. On average, these appear to be on the order of 6-7 years. Generally, the smaller the firm the shorter the lag. This may be explained, in part, by the fact that many small firms are established precisely to bring specific research findings to the commercialization stage. It may also suggest that the research being utilized tends to be that which lies most naturally toward the applied end of the research continuum rather than the basic work which would characterize the bulk of academic research.

But the portion of the paper which attracted the most attention was the calculation performed on the basis of the survey and subsequent extrapolations suggesting there exists a social rate of return to academic research of 28 percent.

This research would appear to provide at least a partial answer, by virtue of its canvassing approach, to the fundamental problem of lacking a well-demonstrated theoretical link between R&D and measures of social benefit. It also seems to provide some quantitative measure of what academic science, if not necessarily fundamental science, has to offer the nation. Yet, it would be inappropriate to draw heavily upon this finding and methodology for policy purposes without recognizing the presence of certain shortcomings which must be addressed by further work.

One of the problems with using a survey approach to determining aggregate benefit is that at some point of the process extrapolation is required. The researcher makes a series of (usually conservative) assumptions. The sensitivity of each of the successive partial analyses may be demonstrated to be relatively small. However, the multiplicative procedure required, given the number of assumptions of involved, means that it becomes increasingly difficult to assess the robustness or reliability of the results.

An even more fundamental concern with the published results on the social rate of return to academic research is that it is difficult to define the context in which to place them.[42] What does a return to academic R&D investment of 28 percent mean and may it be compared in some sense to the average 12-15 percent internal rate of return usually

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[42]It should be understood that these concerns apply not just to the work cited here but more generally to the methodology of attributing a social rate of return to research investment.

sought as a rule of thumb by private investment managers? Mansfield devotes considerable attention to elucidating and estimating the social benefits from research-and-development-intensive products and processes. But less attention is paid to the cost side.

Research and development expenditure is but one part of the investment required to bring a new good to commercialization. The output of basic research may be modeled as a good, but only an intermediate good at best--and it may well be that a more accurate characterization of the benefit flowing from academe to industry possess less of a "good" character as traditionally understood than as a process, one difficult to define precisely. At any rate, there are certainly obligatory complementary expenditures required to bring research outputs on line as commercial commodities. The analogy between this partial rate of return and the more typical rate of return calculation based upon the totality of costs for an investment should be recognized in interpreting these results.

This vitiates somewhat the claim to a conservative bias for the estimation procedure. It is difficult to say what a social rate of return of 28 percent, using the definition provided here, might mean to either public or private potential investors in research and development. When related to the total costs, what is the contribution of the research and development component to bringing forth this improvement to the public welfare?

In the foundation paper by Mansfield and his group which asked the more general question of what are the social and private rates of return to industrial innovations, the full range of costs are explicitly stepped out.[43] But even if the same methodology is followed in the later paper on academic research alone, though this is not explicitly stated, it raises the question of whether, in the presence of so much that is unknown or uncertain about the innovation process and particularly about how basic research findings are translated into commercial applications, it is accurate to assume implicitly linear relationships among the factors which combine to produce industrial innovations. The arithmetic approach to zeroing out other costs assumes that there are no multiplicative relations among factors. This may be so, but the present state of knowledge in this area does not place us sufficiently in the position to make this determination. This implicitly linear treatment of what the authors themselves would be

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[43]Mansfield, et al., 1977, op. cit.

the first to suggest is almost certainly not a linear underlying model is, therefore, an additional source of concern.

As the author himself is the first to suggest, the figure of 28 percent is presented for "exploratory and discussion purposes. It is important that this figure be treated with proper caution and that the many assumptions and simplifications on which it is based (as well as the definition of a social rate of return used here) be borne in mind. While interesting, it is by no means a full or satisfactory solution to the long-standing--and extraordinarily difficult--problem of evaluating the payoff to society from academic research. It is at best a very crude beginning."[44]

[44]Ibid., p. 11.

IV. CONCLUSIONS AND IMPLICATIONS

The discussion above presents several approaches to determining the effect basic research has on the larger society and economy. One conclusion must be that in their present state, while several of these approaches point to potentially fruitful evaluation protocols, none are presently sufficient in themselves to answer the basic question. This does not mean that the current methods are without usefulness. Rather, the point is that of the several overall benefits to research we could name only a subset have actually been subjected to empirical testing. There is a potential richness of alternative measures which has not been explored.

In searching for alternative means to measure performance, the audience who is intended to be informed must be kept in mind. The questions are not only what is to be measured and against what yardstick, there is also a question of for whose benefit the measurement is to be made. The starting point for the imaginative Cozzens paper (1994) is that people and their interests are implicitly part of the measurement process. Formal scientific papers are sufficient to convey the value of fundamental research findings to their own community and oversight funding agency. The question is how to convey the value of this research to third parties? How to convey the value of the fundamental research enterprise considered as a totality? While no authoritative answers may be provided here, certain factors to be borne in mind may be stated as general considerations for any system of performance evaluation. In particular, the questions of measurement revolve around certain conceptual issues of model specification, data, and policy.

SPECIFICATION ISSUES

All the approaches outlined in this survey utilize models, implicit or explicit, which are at best approximations of a technical relationship. They may or may not be accurate representations of the complex series of economic, social, behavioral, and technological interconnections which underlie them. This in itself is not necessarily damning: a model which explains and predicts well is a good model for its intended purpose. That it might not be an accurate model for descriptive and heuristic purposes may or may not be important, depending solely on a consideration of the use to which it is likely to be put. One problem with the effort to develop models for assessing the performance of basic research is that it is not certain what type of model we wish to possess. In the case of basic research with all its attending complexities, a model which will allow us to develop a useful analytical tool might also require that it be based upon as accurate a description of the underlying realities as we are able to muster. This uncertainty is rooted in the protean nature of the entity we would wish to examine and in the multiplicity and subtlety of the avenues through which it works a (presumed) effect on the social and economic goals of ultimate interest.

Clearly, there are questions attached to specific methodologies. Beyond this, there are several issues which arise in general with the measurement of research output as an input to production irrespective of method:

Extreme Heterogeneity. Basic research, by its nature is conducted in a wide range of fields with methods of operation which differ from field to field. The mechanism for integrating the findings in one field with practical development work may differ considerably from that operating in another. Further, there are serious problems of nomenclature. What is considered "basic" and what "applied" is in itself often a difficult call to make on the merits to say nothing of the complication of incentives which arise from the tax code and from internal management pressures in organizations and firms. Beyond this, history is replete with "applied" research which proved seminal in developing "basic" theoretical understanding, and vice versa. Finally, one is more likely to successfully describe the scope of influence of an applied research project and so limit the task of assessing effect, than is the case of basic work which may have considerable effect many years later in an unexpected venue.[45]

Underlying Processes Not Well Understood. Among other concerns, all the approaches discussed in this survey carry an implicit assumption of linearity. Yet, it is precisely the non-linearities and interdependencies which are the hallmark of the innovative system and make the attribution of causality so difficult. Does fundamental science research feed technological development and subsequent commercialization? To be sure; but just as certainly there are flows running in the opposite direction as well.

In a purely theoretical study, Hare and Wyatt make the case that the nature of the function which produces research is unknown and may therefore be consistently

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[45] As an example, the author of this survey was present at a talk on the prospect of bio-optical solutions to the problem of computer mass memory storage and retrieval. The researchers in this promising field acknowledged that they were as surprised as anyone to discover that the core techniques upon which they rely were originally developed for use in the human genome project.

modeled according to many alternative specifications.[46] They demonstrate that small changes in specification of the basic research production function will lead to results supporting widely differing policy conclusions. (In the example they use, one proposed specification yields a result suggesting the efficiency of a dispersal strategy for research assets while another, treated to the same analysis, diametrically supports the need for concentration at a few core institutions.) Many different allocations could prove to be efficient, depending on the state of the research production function. Further, in as much as research areas and disciplines vary in the nature of their organization, approach to the research task, and utilization of research personnel, the work in the paper suggests a need to specify different research production functions by discipline.

This underscores the susceptible sensitivity of any methodology which attempts to determine the influence of basic research on economic outcomes. It also calls into question the implicit assumption of studies which aggregate across disciplines using the same functional forms. The conclusion is that since little is really known about such notional production functions, there is need for considerable analysis and research at the micro level to illuminate the meaning for public policy of measurements of research performance.

The Nature of Technical Knowledge. Especially in models which combine capital and labor with some proxy for knowledge, it is not certain that the latter should necessarily be treated in a manner equivalent to the former two: there are qualitative differences between knowledge treated as a good and the more traditional commodities of economic exchange. The dollar valuation placed upon capital and labor comes closer to approximating the essence which these factors contribute to the production process than does the similar valuation placed on "knowledge". General performance measures are necessarily restricted to measuring science outputs as a series of products. But if the crucial outcomes ensuing from science and subsequent technological change are best viewed as elements of a *process*[47] as much as a product, this can only be a proxy. The question is how crude this proxy actually is and on that there is to date no definitive word.

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[46]Paul Hare and Geoffrey Wyatt, 1988. "Modeling the Determination of Research Output in British Universities," *Research Policy*, vol. 17, pp. 315-328.
[47]Steven W. Popper, *Science, Technology, and U.S. Economic Competitiveness: The Issues for Public Policy*, RAND DRU-820-RC.

The usual methodology of proxying this input is by deflated R&D expenditures. This specification fails to capture the nuances of research or distinguish between types of science. Basic research is characterized by its openness, generality, and design for reproducibility. Applied research is tacit, specific, and costly to reproduce.[48] Yet the origination process of the technical knowledge in both areas shows some similarities: it is intimate (that is, knowledge is embodied in and transferred by people,) context-dependent, and path-dependent. This might be a partial explanation of the observation of one survey that the academic research cited by industry as having contributed most importantly to industrial innovation was conducted in small or medium-sized projects with a median research budget of under $250,000 per year.[49]

Uncertainties Over What Is Being Transferred. What precisely is being generated in the pursuit of fundamental science efforts which when transferred to industry will result in productivity increases and new commodities? The concrete and formalized research findings which are the most obvious product may, in fact, be the least of it. Although almost the exclusive focus of our attention, this formal product may be most important in providing the vehicle and/or the occasion for the transfer of other assets which may have as great or even greater effect on economic and social goals:

- Training in scientific fundamentals. One major product of the system of basic research is the production of trained researchers. Indications are that industrial research managers view that as the principal contribution of academic research rather than the specific knowledge generated by basic research.[50]
- Expertise and experience. The conduct of fundamental science produces individuals who are intellectually and perhaps even instinctually predisposed to be sensitive to phenomena and instrumentation in a certain specialized field. As much as the cognitive information gained during the course of research, what might prove valuable in an individual performing fundamental science is the "touch in the tips of the fingers" which comes from years of familiarity working with a particular physical system.
- Some of this experience is gained collectively, as well as individually. Fundamental science activity allows for the development of "schools" of

[48]Keith Pavitt, 1991. "What Makes Basic Research Economically Useful?" *Research Policy*, vol. 20, pp. 109-119.
[49]Edwin Mansfield, 1991. "Social Returns from R&D: Findings, Methods and Limitations," *Research Technology Management*, November/December, pp. 24-27.
[50]Pavitt, op. cit.

researchers who represent an informal community focused on a problem, system, or methodology. Historians of science as well as students of technological development have always laid stress on the social aspects of technological change.

- Experimental design and protocols. Related to the above point, although basic research findings are intended to be reproducible in the ideal, in practice there are often aspects of experimental design and procedure which are difficult to transmit along the formal information band of publication. There is a technology and learning-by-doing aspect to even fundamental science which must be appreciated if the fundamental science findings in frontier areas are to be converted to industrial applications.
- Increased number of guesses. Investing in fundamental science means not only accepting a greater burden of risk than is usual for other investments, but a considerably greater degree of uncertainty as well. All else being equal, in the face of uncertainty it is useful to purchase as many different guesses about the future as one can afford. But investment in fundamental science provides not merely more guessers and hence guesses about the future; it also provides more occasion for fortuitous accidents and spurious observation to occur. The presence and magnitude of potential outcomes from accidents may be one of the principal reasons private firms do fundamental science.[51] Many of the results which prove to be of interest to industry are, if not random in the classic sense, certainly unanticipated. In order to benefit from such serendipity you need at a minimum personnel engaged in work which will generate such occurrences and individuals who are sufficiently creative and trained to recognize the unusual when it has occurred and fit it within a term of reference which allows it to become reproducible and usable.
- Even absent the serendipity inherent in the search, there remains uncertainty over the eventual value and application of a basic research finding. One of the more subtle but potentially most fruitful ways of thinking about the value of a basic research finding is that it purchases an option on the future. We are now in possession of a fact or body of facts which may well prove useful in the future—but obviously could not be had we not troubled to acquire them. Assigning a present value to such an option is obviously a challenging exercise. Yet are inability to assign value should not be equated with lack of

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[51]Nathan Rosenberg, 1990. "Why Do Firms Do Basic Research (With Their Own Money)?" *Research Policy*, vol. 19, pp. 165-174.

value. Clearly, the options value of some fundamental science being engaged in today will prove to be quite large.

- There is a difference in basic research between generating a negative result, which also conveys understandings of potential value, and no result. Negative results also are an outcome of value if only because they allow us to be more effective in directing the follow-on effort which may then lead to formal results.
- Finally, it should be recognized that if fundamental research acts, in part, as our society's version of the monumental architecture engaged in by earlier civilizations, then there is an intangible benefit conferred on society and the public imagination by the search itself. Though intangible, again, this benefit may not be unimportant. In a world filled with the turmoil of change, it may well be that fundamental science findings are an important ingredient in placing the positive connotation of "progress" upon such turbulence and potential disruption. Science may be important for how it makes us think about ourselves as a collective.

These points also serve to highlight that to produce benefits it is not sufficient that fundamental science merely be ongoing. It also must be connected in some way with the process of production to have a measurable economic or social effect. The fact that fundamental science exists will do industry no good without the means of bringing either the researchers or the products of those researchers into contact with its own researchers and development people. It is interesting to speculate how changes in the mechanism for information transfer likely to occur at an increasingly rapid pace in the coming decades may affect the spillover effect from fundamental science. The concern is not an idle one. It also raises the analytical question of how relevant studies of the past may be for understanding the dynamics of the future given that many of the underlying behavioral relationships could possibly have changed.

This discussion underscores once again a need to examine the assumptions underlying any particular method to measure performance. To be a useful policy tool, the implicit economic model of the role played by technical knowledge must not only specify what it will do as an input to the firm/sector/industry production function. It must also specify explicitly the economic assumptions about how research investment will be allocated and utilized by economic agents. This is important for interpreting the policy implications of the results which the analysis might yield.

DATA ISSUES

Even if theoretical problems in understanding the relationship between variables were somehow resolved, it is frequently the case that there are difficulties in obtaining the data to perform the calculations. Often the types of data required are uncollected or unreliable or proprietary—or an uncomfortable combination of all three. The researchers in this field as a whole are remarkable in their capacity for developing a range of ingenious approximations and work-arounds, but when these begin to accumulate, the multiplicative—and what is worse, unknown—effects work to undercut the confidence the reader may place in the results. If particular assessment methodologies are to be employed by federal research managers as a means for understanding economic and social benefits of fundamental science, some thought should be given at the outset to the types of data required for analysis. Formal statements about reporting protocols and requirements could make the task of data collection more consistent and more routine. They could also spur intramural discussion about the nature of the data which would be required, and upon whose shoulder the obligation for collection should rest.

There is also a philosophical problem, clearly too broad to be more than alluded to here, with the conceptual treatment of technical knowledge as a stock input. In particular, to what extent should standard notions about depreciation and discounting apply to the commodity aspect of technical knowledge, and particularly fundamental science findings? If a lag of some duration is almost a logical necessity for the input of fundamental science findings to have an economic effect, to what extent does the concept of depreciation of this stock apply? Perhaps knowledge should not be analogized to a bread which grows stale but to a wine which matures and improves with time. And is it appropriate to apply typical discount rates to investment in this commodity when the pursuit of this knowledge might with justice be viewed as the recompense, paid to our future selves or succeeding generations, for the front-loaded technical knowledge made available to us by past generations?

This is a fundamental question for measuring the aggregate performance and effect of what is, at base, a long-term investment which must by its nature be handled across a broad sweep of time. Mansfield[52] found in his sample that 7-8 years was the typical lag between an academic research finding and its application. This came as the result of canvassing industrial managers who themselves had suggested the specific products or

[52]Mansfield, op. cit.

processes which were studied. Even if the sample is not deliberately bounded as it was in the Mansfield study,[53] the short-term perspective of businessmen as well as their views about causal links between academic research and commercial realizations would likely bias any sample based on a survey of industry toward shorter lags. This suggests that longer lags might reasonably be expected to prove the norm. This argues a need for federal research managers to be committed to the long course and to select assessment methodologies that accurately capture this intuition about lengthy average payback periods for basic research investment.

POLICY ISSUES

Any survey of this field must necessarily dwell upon the practical and conceptual shortcomings that are prevalent--and whose existence would be readily acknowledged by all scholars working in this field. In the interest of achieving balance we must ask if, after examining all the appropriate reasons for caution, there is value in conducting such studies. The answer must be a qualified "Yes."

The qualification arises not out of the concerns for methodology used nor the theoretical underpinning of the studies themselves. Rather, it arises from concern about the use to which the studies might be put. There is value in conducting such studies if for no other reason than that there are substantial sums being spent to support fundamental science and technology outreach programs at the state and federal levels. The question of what general good is being served by these allocations arises naturally. It is useful to determine whether by using conservative assumptions, the suggestion can be supported that these programs do yield a benefit which otherwise would not have been captured. This could not, by itself be used to support the claim that more money spent in this venue would yield a commensurate result. If, on the other hand, using these techniques, researchers were not able to identify a demonstrable return, this would certainly act as a flag—again, not definitive by itself—that the program under consideration might not be well-suited to achieving its intended goals.

It is not a conclusion of this brief survey that current work on measures of performance is of negligible value. In the field of technology and science policy, any well-formed analysis utilizing any approach is of value. It is reasonable to conclude, however, that for

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[53] The result was bounded because respondents were asked to focus on "recent" research findings defined as those appearing no more than fifteen years before its subsequent application.

the purpose of understanding the nature of the underlying processes and their effects, existing quantitative methods can only be at best one half of the tool used to probe the issues. More work will be required before such approaches can achieve the fullest utility in determining the performance of federal research and development expenditures *ex post*, and perhaps determining allocation *ex ante*.

It cannot be overemphasized: the investment in fundamental science, to the extent it is intended to have a some economic or other social payback, is 1) risky, 2) uncertain, and 3) long term. Most of the models used in the business world to inform investment and portfolio allocation decisions are geared to investments which are inherently less uncertain, where risk can be better assessed, and where payoff is expected over the short term. These are not useful models to incorporate wholesale into the calculus of federal research portfolio managers.

The conclusions of this survey may seem to some to be unduly negative. This should be viewed in context. The studies and methods described in this paper are valuable contributions toward providing answers to the questions the researchers seek to address. However, these are not the same purposes as those sought by federal research managers seeking to measure performance of basic research by tracing its benefit to society and the economy. It is precisely because such analyses allow us to translate complex relationships into comparatively tractable terms that they have a commensurably great potential power to exert influence. When a study develops a hard number, such as the much-cited 28 percent return to academic research in Mansfield's work, it both sends what appears to be a powerful conclusion for policy, in spite of the author's forthright attempt to send warnings to the contrary, and also potentially conveys an "illusion of control" by apparently taking the murky phenomenon of technological change into more familiar territory. The danger of the latter is that if the resulting idealized form is taken as a model sufficient to describe the underlying processes, it then makes it easy to ignore or lose sight of the particularities which distinguish the actual process of creating, transmitting, and utilizing basic research as it operates in the real world.

It is easy to allow the many methodological concerns arising from quantitative analyses to become so preponderant that they overshadow more fundamental issues. Among these, any approach to the assessment of social and economic benefit must consider from the outset what question the studies are being asked to answer. If the question may be well-posed and clearly outlined it provides a guide in the task of building an appropriate analytical tool. The result is that the answer yielded by the methodology might then be more easily related to the fundamental question being asked.

The studies cited here have not had as their main purpose the development of tools to sustain efforts to assess and guide investments in fundamental science. They have only succeeded in demonstrating the existence of evidence supporting the contention that the investments made in the past have been of value to society and that this value appears greater than that portion captured by private industry. They are able to speak only to average rather than to the marginal tendencies upon which decision making would need to be based.

It would behoove federal research managers to examine what is most applicable in the body of work which already exists, bearing in mind that much work still needs to be done. In particular, any approach to assessment should draw out what insights might be gained from aggregate assessment but combine such analyses with other approaches. Analytical tools which are more specific and disaggregate can be used to probe and illuminate the complexities of process, to complement the aggregate type studies with micro level investigations which will allow us to more clearly understand what the results of the former are trying to tell us.

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