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ANALYSING THE ECONOMIC PAYOFFS FROM BASIC RESEARCH

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Cost benefit approaches for evaluating the contribution of basic research investment have severe limitations and a number of undesirable consequences. We propose an alternative, information-theoretic, approach for identifying the economic benefits engendered by basic research. We emphasize the role of information linkages, the economic value of both findings and non-findings, and the consequences of disclosure incentives for realizing the economic benefits from basic research investments. We introduce the concepts of homotopic mappings and analogic links as relevant for differentiating among the prospective contributions of competing basic research projects, and we examine how the lumpiness or indivisibility of basic research investments may influence the conduct of basic research. The methodology and reasoning of our approach is illustrated by an examination of the case of basic research in high energy physics.

1. INTRODUCTION

Much of the literature on the economic payoffs from public investments in basic research has adopted the framework of cost-benefit analysis, identifying critical “events” or discoveries and attributing the value of the applications developed from these discoveries to a related basic research investment. This paper argues that the cost-benefit framework is inappropriate for the economic analysis of basic research and that an alternative, information-theoretic approach is needed.

In place of conventional cost-benefit analysis, we propose that the economic effects of basic research should be evaluated within an analytical framework that emphasizes the importance of learning and information generation in the basic research process, the substantive and organizational linkages between basic and applied research activities, and the incentives for the rapid and widespread dissemination of the knowledge engendered by basic research activities. Unlike the typical cost-benefit assessment, our approach acknowledges that basic research projects may produce positive or negative information of value; the “non-findings” of basic research or the “rejections” of research hypotheses can also be illuminating. In either case, this information guides and informs the allocation of investments to applied research and development that may ultimately lead to marketable products and processes.

This approach builds on the familiar observation that the informational outputs of basic research rarely produce direct economic benefits or profits, but instead are “intermediate inputs” that are indispensable in the further research leading eventually

to commercial innovations. Moreover, basic research does not exist in a separate sphere, but is (or should be) linked in complex, interactive ways with applied research and innovation. Basic research may yield other results with immediate economic utility, e.g., scientific instruments or training of scientists and engineers (a "spinoff" whose importance is closely linked to the institutional structure of a nation's basic research system). Focussing attention exclusively on these "spinoffs," however, overlooks other, more important economic benefits from basic research.

2. DEFINING AND MEASURING BASIC RESEARCH

The goal of basic research is increased understanding of a subject or natural phenomenon, rather than the creation of specific applications with economic value; such application-oriented research activities are categorized here as applied research. Basic research generally has been conducted in conformity with the traditions of "open science," with complete disclosure of results and methods through rapid publication. Applied research activities are more often organized to create information whose benefits are privately appropriable, through patents or other institutional devices such as trade secrets.¹

The economic analysis of basic research focuses on the *economic* value of additions to the state of knowledge (Nelson (1959), Arrow (1962)). The private economic returns from a basic research discovery are limited by the infeasibility of establishing or defending property rights for many such discoveries. Economic benefits deriving from basic research discoveries, in the terminology of the economist, are not likely to be fully "appropriable" by the discoverer. The resulting divergence between the private and social returns to basic research investment constitutes a "market failure," resulting in underinvestment in such activities by private parties.² This "market failure" argument is a key justification for public funding of basic research.

Because the economic benefits of many basic research results cannot be captured by a single firm or industry, widespread diffusion of these results is often necessary to achieve their full social benefit. In contrast to the assumptions of many analyses of basic research, however, the transmission and the absorption of the results of basic research by applied R&D organizations are not costless activities (Mowery (1983); Mowery and Rosenberg, (1989).) Rather, they require considerable expertise within the "receiving" organization and frequent interactions between the source and the would-be user of basic research findings. Rosenberg (1990) and Cohen and Levinthal (1989) argue that private firms invest in basic research in order to create such "absorptive capacity."

¹ See Dasgupta and David (1987) on the significance of disclosure norms; Whitley (1984) on organizational distinctions among the sciences.

² Basic research represents a relatively small share of total national R&D investment. The National Science Foundation (NSF), whose data on basic research are based on reports from firms that have considerable latitude in their definition of this quantity, estimated that total 1989 U.S. expenditures for basic research amounted to \$18.6 billion and represented 14% of the national investment in R&D (National Science Board (1989) Appendix Table 4-5). Federal funds accounted for about two-thirds, \$12.0 billion, of spending from all sources on basic research. The NSF estimates industrially funded basic research was less than one-fifth of total basic research spending (\$3.3 of \$18.6 billion) and accounted for slightly more than 5% of all industrially funded R&D during 1989, substantially below the 17% share of the federal R&D budget that was allocated to basic research.

3. THE ECONOMICS OF BASIC RESEARCH "BYPRODUCTS"

Although the primary purpose of basic research projects is the production of scientific information, they can yield important "byproducts":

- (1) When conducted in universities and academic institutions, basic research projects serve as vehicles for the education of scientists and provide opportunities for advanced training in experimental techniques.
- (2) Especially when linked with scientific training, basic research projects create social networks through which information that has not yet been reviewed and published diffuses rapidly. Such "networks" may outlive the projects that brought them into existence.
- (3) Basic research projects often place great demands on technology, e.g. in the controlled production, observation and recording of phenomena, or in the analysis of the data thereby generated. Thus, they stimulate advances in the "technology of scientific research" (both techniques and instrumentation) that reduce the costs or increase the effectiveness of both basic and applied research programs.³

Important economic returns thus may flow as byproducts – or "externalities" – from the basic research process itself. These economic returns, however, are not unique to the *conduct* of basic research. Applied research projects may create similar "research byproducts," and, as noted in Rosenberg (1982), may generate fundamental questions and interesting phenomena for study by scientists engaged in basic research. Dasgupta and David (1987), (1990) and Nelson (1989), (1990) observe that the inherent characteristics of technological (often, more applied) and scientific (generally basic) research have converged in recent decades and may be equally productive sources of generic "byproduct" knowledge.

Nonetheless, the distinctive goals, and modes of organization of basic and applied research projects affect the flow and the utilization of "research byproducts" from each type of undertaking. Projects organized for the purpose of accomplishing a narrowly defined commercial or military mission are less likely to widely publicize ancillary discoveries and more generally applicable research techniques, or quickly place them at the disposal of other research groups. Those investing in applied research projects are more inclined to withhold such knowledge from their potential competitors.⁴ Secrecy concerning end-product information is infectious, is more likely in applied research projects, and logically extends to information about the byproducts of the research process itself.⁵

Conventions and incentives affecting the compulsion to disclose results, techniques,

³ Rosenberg (1991) has argued that advances in scientific instrumentation, a common byproduct of ambitious basic research projects, have yielded important economic payoffs in industrial applications, but have also molded and influenced the subsequent basic research agenda of numerous scientific disciplines.

⁴ This is not to say that researchers in academic science adhere strictly to "norms of disclosure" in regard to intermediate stage results, experimental "failures," or research techniques. See Dasgupta and David (1990) for discussions of secrecy within the domain of open science.

⁵ The contributions of basic research to the training of scientists and engineers depend on the institutional location of the research activity. In some industrial nations' research systems (e.g., the U.S. and the U.K.), basic (and much applied) research is closely linked to university education and the training "byproduct" of basic research activity is likely to be significant. In other research systems, however (e.g., Japan and France), the role of universities in the performance of basic research is much more modest, and basic research yields less by way of training "byproducts."

and other byproducts of the research process are pivotal to understanding the economic returns to public basic research investments.⁶ The disclosure of generic knowledge or "byproducts" will be influenced by funding sources and the disclosure norms of the research organization, and the latter are subject to modification by the policies of research performers or sponsors. There is no *necessary* correspondence between the "business" of research and "openness," or between applications-oriented research and the imposition of restrictions on disclosure of or access to the findings. Thus, although university engineering research is most often applied in character, in most circumstance it is subject to the same disclosure norms as research in scientific fields. Similarly, basic research performed in such industrial contexts as Bell Labs or Du Pont's "Purity Hall" has often been freely disclosed. Defense Department-funded basic research at many U.S. universities during the 1950s and 1960s, however, frequently was performed under secrecy restrictions, and more recently, leading U.S. research universities have been permitting longer and longer pre-publication delays or reviews for the findings of industry-funded basic research, as well as assigning exclusive patent rights to the sponsors of such research.

The boundaries separating public and private knowledge thus are elastic. They are not determined in a straightforward way by the institutional location or subject matter of the research activity, but rather by the joint influence of disclosure norms, the incentives offered to researchers, as well as the constraints negotiated (or imposed) by research patrons, and the linkage between education and research activities within the institution performing the research. They are subject to change over time and may vary among national research systems at any point in time.

4. COST-BENEFIT ANALYSES OF BASIC RESEARCH INVESTMENT

Although "tangible products" of basic research such as new products and processes emerging from company or government R&D labs are only one category of output of basic research, most studies of the economic returns from basic research have focused exclusively on these "tangible products." Cost-benefit analyses of basic research may prove very misleading, however, because the products and byproducts of the basic research process are difficult to measure, and the channels through which their economic impact is realized often are indirect and complex.

In order to illustrate this point, we identify three kinds of social benefits attributable to the results of basic research projects. First, information from basic research discoveries may be applied directly to the creation of new processes or products. Second, research outcomes may produce information that is an input into other basic and applied research activities and, with modification and refinement, forms the basis for new products or processes. Third, research outcomes may provide the information to improve processes or products that are primarily based on other scientific or technological discoveries. Since basic research results rarely lead directly to new processes or products without substantial modification, the bulk of our discussion below considers only the second and third sources of benefit from basic research.⁷

⁶ See Rothblatt (1985), Dasgupta and David (1987), (1988), (1990); David (1991).

⁷ New syntheses of organic chemical compounds might be thought to form an important exception to this assertion, but the development of methods of synthesis suitable for large scale production at commercially feasible unit cost levels is usually a separate (and expensive) applied research undertaking.

Considerable investment in applied research and development investments generally is necessary to translate any basic research advance into new products and processes. Inasmuch as the exploitation of basic research results depends on these complementary investments, it is difficult, and usually erroneous, to directly attribute to any single basic research output the entirety of the economic benefits associated with an application. These benefits typically are separated from the basic research "event" by a substantial period of time and additional R&D expenditures. The trail from basic research discovery to application is further obscured by the many refinements and additions to the original discovery that are necessary to realize the application.

Efforts to trace the use of basic research are frustrated by the lack of any generally dependable means to meter use of the informational results of a given basic research program.⁸ As a consequence, the costs and benefits of eventual applications cannot be allocated among the various basic research investments responsible for the information used by the applied researchers. Once a research investment is embodied in processes or products used to produce other goods, or in techniques used to generate further research findings, the link between the "investment" and the downstream activities that create economic value is obscured. It is rarely possible to isolate "key" contributions of basic research to commercial applications.

Heroic assumptions are necessary to compare the costs of any specific basic research "result" with the revenues or consumer surplus generated by all the "applications" that can be "tagged" through expert opinion or analysis.⁹ This approach assumes that no alternative method could have generated the economic returns associated with the products or processes attributed to the basic research in question. Such an assumption may be valid for a narrowly defined product or process for which the timespan between discovery and application is very short, but such cases are rare. Moreover, most economists find this assumption to be an uncomfortable one, inasmuch as there are few new products or process completely lacking substitutes.¹⁰ In other words, in the absence of a specific basic research outcome, some alternative technique or application might be developed. The costs and benefits of the basic research advance and its subsequent commercial embodiment in a new product or process therefore should be compared with the costs and benefits of alternative scientific or technological solutions.

This difficulty can be overcome, in principle, by estimating the cost of the substitutes that would be required in the absence of a specific basic research outcome. In the counterfactual state of the world where a substitute product or process could be made available, however, one cannot immediately infer the substitute's cost from the observable structure of costs and prices. The more fundamental the impact of the

⁸ Analysis of co-citations of scientific papers in patents provides a tracing techniques that recently has attracted attention. But such indicators are at best partial, e.g. tacit knowledge transfers are not captured, and are subject to ambiguous interpretations (scientific papers are cited for reasons other than the acknowledgment of material contribution to the patent).

⁹ Examples of this methodology include Griliches, (1960), and Lederman (1985b).

¹⁰ In fact it is very difficult to construct an example of a product without substitutes. The availability of a neutron beam for the treatment of an inoperable brain tumor which would otherwise kill the patient is one possible example. But the use of neutron beams to create radioisotopes for altering genetic material aimed at improving crop varieties faces substitutes from sources of natural radioactivity and chemical mutagens.

hypothesized non-existence of the basic research outcome, the more dramatic would be the likely restructuring of the demand for and supply of substitutes. In other words, it is not enough to take account of the existence of substitutes; one must consider the sub-class of substitutes that are independent of the basic research result being examined. Construction of this counterfactual entails a deep and detailed understanding of the genesis of relevant bodies of scientific and technological knowledge. It is not a casual undertaking to speculate intelligently about the costs and consequences of having to find substitutes, say, for electrical power or digital computers during the twentieth century.

In addition, commercial innovations require a number of complementary scientific and technical advances in order to reach the market (Teece (1986)). Most benefit-cost analyses of basic research fail to value correctly these complementary investments' opportunity costs in calculating *net* economic returns. There may be a good reason for this omission, or at least an understandable reason: in the absence of the basic research discovery, the opportunity costs of complementary research activities might diverge widely from observed market prices.

Despite these difficulties, a number of studies have attempted to measure the economic returns to basic research discoveries from a retrospective assessment of the effects of these discoveries on new product and process innovation. The TRACES and HINDSIGHT studies are examples of this methodology (National Science Foundation (1969) and Office of the Director of Defense Research and Engineering (1969)). Significantly, neither the TRACES nor HINDSIGHT studies computed an estimate of the returns on the investments in basic research they analysed, despite the explicit charge to the authors of the HINDSIGHT study to do so.¹¹

The method of tracing particular applied research outcomes back to antecedent basic research investments, and computing the "rate of return" on the latter outlays from estimates of the social benefits derived from the former, has some obvious flaws. By focusing largely on "successes," the tracing procedure introduces various selection biases: it tends to ignore the costs of basic research investment outlays that utterly failed to yield tangible outcomes. It also may understate the economic yields on basic research – in ignoring the value of having *avoided* losses on certain lines of applied research by virtue of the basic research knowledge (positive and negative) that was acquired. Further, because it involves a retrospective "trace," this approach cannot take account of the economic value of information from basic research activities that does not lead to the commitment of complementary, applied R&D investments within the period of observation. It is incorrect to suppose that the quantitative significance of this omission would be rendered negligible by allowing a long period to elapse between the date of the basic research project's completion and the close of observation. Although it is true that "payoffs" long deferred will be heavily discounted in present value calculations, there remains the often-overlooked point that new scientific and technological information creates *options*, and options certainly do not have to

¹¹ The HINDSIGHT study rejected the "...possibility that any simple or linear relationship exists between cost of research and value received." (p. xxii).

be exercised immediately in order to have an immediate economic value.¹² Moreover there are no confidence intervals or bands of uncertainty surrounding estimated average rates of return.¹³

An alternative measurement approach focuses on the productivity performance of industries investing in basic research, using industry productivity growth as an indicator of the social returns to these research investments. A number of statistical studies have concluded that basic research investment has a statistically significant and positive impact on productivity growth (Mansfield (1980); Link (1981); Griliches (1986)). These studies adopt a high level of aggregation in their analysis and rarely control for inter-industry differences in technological opportunity or appropriability. Moreover, they do not reveal how the economic returns of basic research are realized nor do they provide any basis for comparison of the productivity impact of basic research in different scientific disciplines.

These problems of the cost-benefit analysis of basic research do not exhaust the mischievous potential of that framework. Uncritical acceptance of the results of cost-benefit analyses of the economic effects of basic research supports an unrealistic view of public science and technology policy. Demonstration of a positive benefit-cost ratio for a basic research investment, as we noted above, leads one to downplay or ignore the complementary assets and investments needed to commercialize technological advances derived from basic research (Teece (1986)). If basic research is the critical factor for national innovative performance, then improving such performance need only focus on expanding basic research expenditures by industry and government. The evidence on the competitive performance of the United States and other nations during the postwar period, however, suggests that at least in the short run, expanded investments in basic research alone will not improve national economic or innovative performance.¹⁴

¹² See Cox and Rubinstein (1985) on option valuation; Pindyck (1991) integrates option values into the calculus of investment decisions involving irreversible commitments. A financial call option gives its holder the right, for some specified time-period to pay an "exercise price" and receive in return a valuable asset (e.g. a share of stock). The option has an intrinsic value because the future value of the asset that it may be used to obtain (perhaps after additional investment), although uncertain, is expected to be positive. Options values exist for basic research findings, but there are some special complications arising from potential "public goods" nature of the knowledge gained through basic research. If others are free to make the same fundamental discovery and undertake the complementary investments to develop a commercial application, the value of the option held by the basic researcher who keeps her findings secret will be subject nonetheless to extinction, albeit at a rate that depends upon the probability of success of others' efforts.

¹³ Mansfield (1991) examination of the social returns to "academic" research avoids these pitfalls by calculating the social benefits (revenues and consumer surplus) associated with a sample of major corporation's R&D investments that were judged to have been made possible by university research breakthroughs, and by extrapolating these estimates to cover all university science and engineering research and all corporate R&D. Although this approach is a conceptual advance over the "tracing" procedure, the validity of the extrapolations remains in doubt. In addition, the estimated returns from the technological advances examined by Mansfield must be attributed to both the basic research investment and the complementary investments of corporate R&D funds. Acknowledging the infeasibility of such an attribution procedure, Mansfield does not attempt it. This procedure yields an estimate of the social rate of return on *all* basic (academic) R&D, and cannot guide the allocation of resources among research fields. Like many cost-benefit calculations, it is more useful as an *ex post* rationale for historic public research investments, rather than as a criterion for future investments.

¹⁴ See Rosenberg (1987; 1990); Mowery and Rosenberg (1989).

This conceptual approach to the evaluation of basic research has another potentially harmful implication. Once one is convinced that the national benefit-cost ratio of basic research investments would be increased by capturing all of the "downstream" benefits of applying the results, the strategy of monopolizing for exclusive national use the fruits of the quest for basic knowledge acquires a superficial plausibility. Policymakers, thus seduced, may strive to restrict foreign access to publicly financed basic research. Some recent U.S. government actions and proposals reflect such a mindset. These measures include the prohibition on foreign attendance at the 1987 conference sponsored by the White House Office of Science and Technology Policy on basic research on high-temperature superconductivity (see Mowery and Rosenberg (1989), for additional discussion), or the proposed revisions in the federal Freedom of Information Act that would restrict disclosures of the results of publicly financed research performed at the federal national laboratories, when such disclosure could harm "national competitiveness." These policies, and others in the same vein, are harmful to the conduct of scientific research and, by impeding the diffusion of the results of basic research, may ultimately reduce the payoff to applied research activities. Basic scientific research in the late twentieth century is an international activity. Any nation that attempts to control the diffusion of scientific results achieved within its borders does so at the risk of impoverishing its own scientific and technological base.

Similarly perverse consequences may result from actions by university administrators or state government policymakers who attempt to "capture" the results of basic research to strengthen university finances or to support regional development. As Feller (1988) has noted, the belief that university basic research is a powerful engine of regional development and/or appropriable profit for universities overlooks the critical importance of the complementary investments necessary to realize the returns to the results of university basic research. Like governmental efforts to restrict international dissemination of the results of basic research, these policies may reduce the economic returns to basic research. Moreover, attempts to capture the "profits" through extensive patenting and patent-licensing efforts based on such research overlook the modest commercial value of the vast majority of such licenses. They also underestimate the need for licensees to have access to noncodified "knowhow" associated with such patents, and the potentially harmful consequences of attempts to reorient the incentives of university researchers so as to provide commercial licensees with the complementary, tacit "knowhow" they require.¹⁵

Our alternative framework for the analysis of basic research focuses on the informational outputs of basic research and the connections among these outputs, applied research, and innovation. This framework emphasizes the interaction between basic and applied research activities as the ultimate source of the economic benefits of basic research. We also suggest some preliminary criteria to differentiate among the potential economic payoffs of basic research in different scientific disciplines.

¹⁵This problem is discussed by Dasgupta and David (1990).

5. AN ALTERNATIVE, INFORMATION-THEORETIC APPROACH TO THE ECONOMIC ANALYSIS OF BASIC RESEARCH

Both basic and applied research yield information about the physical universe. Applied research generates information that is utilized along with other inputs for the development of specific goods or services. By enhancing the economic returns from investment in applied research and development, the informational results of basic research contribute to economic growth.

By focussing upon the complex interdependencies among various kinds of knowledge-seeking activities, and examining the ways in which the informational outputs from basic research enhance the prospective distribution of economic returns on applied research expenditures, we hope to arrive at a more realistic and less potentially mischievous way to view the economic significance of basic research. Our approach draws on an analytical framework originally developed to study the problem of allocating resources between "basic" and "applied" science activities (Evenson and Kislev (1975) and David and Stiglitz (1979)), which, in turn, was built on the economics literature devoted to "optimal search theory".¹⁶

Applied research uses fundamental information about physical relationships, properties of matter, etc., to explore the array of opportunities for development of products or processes. In this respect, applied research is analogous to "prospecting" for potentially valuable mineral deposits in a territory whose geology has yet to be thoroughly studied. We view applied research as an activity that resembles sampling from a distribution of potential products (or processes), each of which has a particular set of economic attributes. The development stage involves the selection of the potential products "found" in the sample whose (perceived) attributes offer the highest economic payoff to the agent undertaking design, development and commercialization. We may think of the information derived from basic research as informing us of the nature of the underlying distribution of "potential products" derived from applications both close at hand and further afield. Possession of such knowledge obviously would be of value to agents considering committing substantial expenditures to realize the latent product or process innovations.

The economic yields that result from the selection of what appear to be the most promising development projects are by no means certain. These uncertainties reflect market risk, the needs and tastes of potential customers, the actions of competitors, and so forth. But another portion of the risk stems from the uncertainties that surround the design of the search or "sampling" procedure itself and the "scientific" interpretation placed upon the sample findings.

Basic scientific information offers rules for empirical generalization from specific indications. Basic research essentially improves one's theory of the properties of the distribution being sampled. Theory guides the conduct of the sampling activity itself, the object of which is to find the "high (economic) payoff" items in the underlying distribution, and has a twofold effect on the conduct of and the economic returns from applied research. First, by providing a stronger informational basis for decisions as to where and how much to sample, a theory improves the effectiveness with which the resources devoted to applied research can be allocated among competing alternatives. Second, better information about the fundamental properties of various classes of distributions allows one to utilize smaller and less costly samples to identify the

¹⁶See McCall and Lippman (1980) and David (1974).

particular type of distribution from which one is drawing and may reduce the risk surrounding the interpretation of the results.¹⁷

To take up the "prospecting" example again, three distinguishable operations are necessary to produce information on which to base exploitation decisions: (a) the choice of the most promising territory in which to go prospecting; (b) the collection of mineral samples; and (c) the conduct of assays and interpretation of the assay results. Improved scientific knowledge allows all of these decisions to be made more efficiently. Where the applied scientist-pro prospector has a good theory of the underlying geological formations, more readily obtained (and therefore less costly) information (such as the nature of the surface terrain) can be used to decide where to start prospecting, and how best in any particular terrain to go about obtaining informative samples. Furthermore, a theory that makes reliable predictions about the kinds of mineral deposits found in conjunction with other minerals, and the conditions accompanying the presence of deposits of various sizes, will enhance the information that may be extracted from any specific set of assay results.

In other words, basic geological theory allows the results of the prospector's effort to be interpreted with greater reliability. Information revealing *ex ante* that a territory is either an unpromising or very costly prospect for sample selection also is valuable. Companies searching for oil deposits are willing to pay for information on the location of "dry holes" drilled by other companies' exploration teams. Knowing where *not* to search further is part of knowing whether and where to devote resources to future searches.

Information concerning the shapes of the potential payoff distributions associated with different areas of applied research also may improve the economic returns from the allocation of resources among these areas. For example, how should the exploration budget in the prospecting example be allocated among available areas for exploration? How large a sample should be drawn from each? We may draw some insight from the statistical proposition that for a large class of probability distributions, the expected maximum value drawn in a given sample tends to increase with the size of the sample, but at a diminishing rate.¹⁸ This suggests that if the incremental costs of the sampling activity are constant or rising, there will be an optimal sample size to draw from every property. One maximizes the net expected applied research payoffs by stopping where the incremental gain in the expected maximum value is just equal to the incremental cost of enlarging the sample.

For a given sample size, the expected maximum value will be greater where the variance of the underlying distribution is larger.¹⁹ Being able to locate such higher-variance distributions, or discovering how to manipulate organic or inorganic matter so as to create new distributions that have higher cumulative probability at the upper tail, offers another route through which basic research can affect the payoffs available from complementary applied R&D investments. Where basic research "prospecting" provides evidence of differences in the underlying shapes of these distributions, the

¹⁷ Recent basic research advances in computational chemistry, for example, have led to techniques that greatly reduce the number of actual chemical syntheses that need to be performed in order to obtain a molecule with desired industrial or pharmacological properties.

¹⁸ This statement is restricted to the class of continuous, unimodal probability distributions. A proof of the proposition that under these conditions the expected extreme value of a sample of size n is a positive concave function of n can be found in David (1974:p.26).

¹⁹ Thus it is possible that even under conditions of risk aversion (from which the present discussion abstracts) it would be attractive to 'sample' from a higher variance distribution of potential applications.

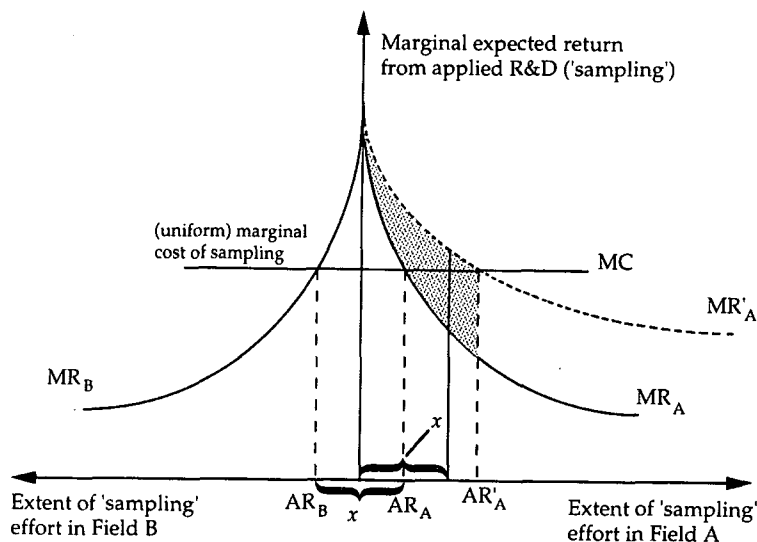


Figure 1

optimal extent of sampling among different properties will differ even if the incremental sampling costs are identical for all exploration properties (i.e., in all the specialized applied research areas). Under the latter conditions, one could raise the expected yield on the total outlays by drawing larger sub-samples from those properties where the mean and/or the variance of the underlying distribution of potential payoffs were greater than was the case elsewhere. Only then would it be possible to achieve the equalization of incremental expected returns from sampling in all applied areas (and hence equate these returns with the hypothesized uniform incremental costs of sampling activity).

Figure 1 depicts a simple comparison of the marginal expected return from applied R&D (sampling) in two different fields. In the absence of basic research knowledge, the marginal expected returns from applied research appear much the same in Fields A and B (i.e. MR_A MR_B), and, given a uniform marginal cost of sampling, applied R&D efforts are allocated equally between them (i.e. AR_A AR_B). Basic research, however, shifts the relevant expected payoff schedule from MR_A to MR'_A in field A. The effect is to raise the optimal total applied R&D outlays, and to increase the expected net social surplus by the amount indicated in the shaded area. Note that if the applied R&D budget were fixed at X, the effect of basic research would be to cause all applied R&D efforts to be optimally concentrated in Field A, abandoning Field B.²⁰

Thus, basic research information may contribute economic benefits by improving the allocation of research resources among competing areas of applied research and

²⁰ Of course, other factors such as issues of appropriability may intervene to prevent this socially optimal outcome.

by improving the payoffs to these applied research investments. These economic benefits from basic research do not require discoveries that open up entirely new areas for applied research, but they do hinge on relatively liberal dissemination of basic research results.²¹

6. COMPARING THE ECONOMIC IMPACTS OF BASIC RESEARCH IN DIFFERENT SCIENTIFIC DISCIPLINES

Different areas of basic research will vary considerably in the amount of information that they reveal about the potential distributions of payoffs in related areas of applied research. Not all basic research has the same potential to contribute to gains from reallocating investments in applied research portfolios. What fields of basic research are likely to be most promising in their illumination of applied research possibilities?

The number and richness of links between the knowledge generated by basic scientific projects and other scientific and applied research endeavors are important determinants of the potential economic returns from discoveries in a specific discipline.²² These linkages may influence the utilization by other fields of knowledge from basic research in one field; they may also affect the utilization of basic research by applied research endeavors. We distinguish between two types of links, "homotopic mappings" and "analogic links." An additional characteristic of basic research projects, "lumpiness," may influence the formation of these mappings and links. Each of these concepts has implications for the empirical examination of basic research projects and the allocation of public investments among basic research fields.

The discussion of topological theory in Hocking and Young (1961; p. 149) notes that "two mappings are homotopic if one can be deformed continuously into the other. Or we may view homotopic mappings as being members of a one-parameter family of mappings with a continuous parameter." Applying this concept to the comparison of different fields of basic research asks, in effect, whether a parametric transformation connects one domain of basic scientific knowledge with another. In the field of mechanics, a theory predicting the reaction of a stationary physical object to an external force will remain true for objects of a much larger mass than have been experimentally tested. We are sufficiently confident of the homotopic nature of this mapping that we apply mechanical principles to objects whose mass exceeds empirically testable limits. Similarly, results about the property of waves may be homotopically mapped into new

²¹ Our analysis here reaches conclusions very similar to those of Nelson (1986), who argues that "...university (frequently, basic) research rarely in itself generates new technology; rather it enhances technological opportunities and the productivity of private research and development, in a way that induces firms to spend more both in the industry in question and upstream." (p. 188).

²² Panofsky has offered an optimistic assessment of these linkages in the case of high energy particle physics: "That many more such links do exist than are now apparent is based on the faith that complex phenomena ultimately have their roots in more basic relationships. It would indeed violate all our past experience in the progress of science if nature had created a whole family of phenomena which govern the behavior on a large scale without at the same time establishing any links between these and the small-scale phenomena governing the behavior of elementary particles. Therefore, suggesting that the study of elementary particles is "remote" from the other sciences is tantamount to denying the lesson taught us by all past evolution of knowledge." (U.S. Congress, Joint Committee on Atomic Energy, (1965b), p. 760) The faith on the part of physicists exhibited in Panofsky's statement has been shaken by subsequent developments in nonlinear dynamics, chaos theory, and the gestalt perspective associated with the newly emerging "sciences of complexity." See, e.g. Ruelle (1991) for a brief non-technical introduction.

domains of applied research. For example, knowledge of frequency modulation of radio energy is applicable to the design of optical systems.²³

Scientific information that can be homotopically mapped to different scientific or applied research problems is potentially applicable to problems quite far removed from those of concern in the original inquiry. The extension of the domain of 'application' proceeds by validation of the hypothesized regularities, subject to the parametric change. Once a theory exhibits such homotopic mappings, progress in other fields of basic and applied research can focus on issues of feasibility, refinement, and practical implementation rather than on the discovery of new phenomena.

The notion of "homotopic mapping" also is relevant to the pace and impact of progress within a scientific discipline. In some sciences, examination of a portion of an entire system of interrelated phenomena provides useful generalizations and applications in other areas. The results of molecular biologists' studies of a single living organism with DNA, e.g. *E. coli*, have yielded results that are applicable to other organisms. Discovery of DNA opened up the search for homotopic mappings that would connect the mechanisms of protein synthesis, energy cycles, and other cell functions across all DNA-containing cells. Failures as well as successes in establishing such mappings have been important sources of both basic and applied research knowledge.

A second type of link between knowledge from basic and applied research and scientific progress is "analogic" or "metonymic".²⁴ Analogic links are based on the surmise that nature is conservative in the use of concepts and structures, and posit that physical regularities in one field underlie other natural phenomena. For example, the concept of symmetry has proven fruitful not only in mathematics and physics, but also in chemistry and crystallography.²⁵ The establishment of analogic links among fields of research reflects these fields' historical paths of development as well as their conceptualization of and preoccupation with "fundamental" phenomena. The nature and extent of analogic links in a given area of basic research will affect its potential economic payoff, since these links will affect both the interactions between this and other areas of basic research, and the influence of basic on applied research.

The ease with which homotopic mappings and analogic links can be formed between basic research and other areas of applied or basic research affect both the economic payoffs from these results and the costs of advancing scientific knowledge. Scientific disciplines yielding results that are amenable to homotopic re-mappings will affect other fields of science with a more modest incremental investment than will disciplines in which such mappings are less dependable. Similarly, scientific fields in which analogic links are broader and more easily extended will contribute more to progress in other scientific fields than disciplines lacking such links.²⁶ In both cases,

²³ "Another technical question for the future concerns 'wavelength multiplexing' – the idea of simultaneously transmitting through a single fiber signals from several lasers operating at different wavelengths... This seems to be a logical way to increase the already prodigious communications capacity of an optical fiber," Gunderson and Keck (1988), p. 225.

²⁴ Whereas 'metaphors' assert that A is B, even while paradoxically recognizing the necessity of distinguishing A from B, metonymies assert a semblance or congruence – A is like B – while acknowledging important elements of differences. See, e.g., Johansson (1989).

²⁵ On symmetry in crystallography, see Polanyi (1962), pp. 43–48.

²⁶ It is important to note, however, that such analogic links may emerge much later than the initial basic research results.

the breadth of mappings and links to applied or basic research endeavors are potential measures of the payoff from basic research projects in a given discipline.

Finally, additions to the stock of scientific information may be more or less "lumpy," i.e., subject to indivisibilities in the resource inputs required to expand the scale of experimentation or research. "Lumpy" economic activities are often characterized by a large minimum efficient scale of production. In addition, in some "lumpy" areas of basic research, the production of useful information requires that one solve a large number of sub-problems that are interrelated in complex ways.

Both types of lumpiness are likely to be greater in scientific disciplines in which homotopic mappings are limited. In such disciplines, additions to the stock of scientific information may require the construction of new, significantly larger and more specialized research equipment. The financial and temporal requirements of such projects mean that basic research in these disciplines may advance discontinuously. A discipline may encounter apparently diminishing returns, as the immediate implications of previous basic research breakthroughs are absorbed and research resources are reallocated to exploit these breakthroughs. The completion of a new set of facilities or the solution of a new array of research problems, however, may suddenly advance the basic research agenda, yielding significant new homotopic mappings and analogic links with other fields of research. In a given field of basic research, the development of mappings and links with other basic or applied research activities will also be affected by the lumpiness of the process of basic research in this discipline.

Public and private research investors will face continual pressure to reallocate funds from lumpy scientific projects to those that offer more readily apparent mappings and links to applied research. Decisionmakers must employ a long time horizon in evaluating investments. Otherwise, they may undervalue the prospective economic returns from basic research investment in areas characterized by apparently limited homotopic mappings, analogic links that are difficult to discern *ex ante*, and pronounced lumpiness. Moreover, it may prove important to distinguish among areas of basic research in terms of their "lumpiness" or potential richness of analogic links and homotopic mapping in comparing the prospective payoffs from investment in one or another discipline or project.

7. AN APPLICATION: THE CASE OF HIGH-ENERGY PARTICLE PHYSICS (HEPP)

High-energy particle physics is a classic example of a scientific discipline characterized by limited possibilities for homotopic mapping, numerous analogic links that are difficult to discern *ex ante*, and pronounced lumpiness.²⁷ Basic research in particle physics during the twentieth century has focused on the underlying structure of matter and the operation of physical forces determining interactions in the natural world. As this research has progressed to ever-finer levels of subatomic analysis, homotopic mappings have become a less reliable guide for research inquiry (i.e., results obtained at a lower level of energy do not "map" to higher energy levels requiring major revisions in experimental procedure and new scientific equipment), the lumpiness of both the investments and the character of scientific advances has increased, and the "analogic links" to other fields of basic and applied research have, if anything,

²⁷ This section condenses the more detailed discussion and accompanying case studies of David, Mowery, and Steinmueller (1988).

become greater, although predicting where these links will next be formed is nearly impossible. These trends have significantly affected the organization of scientific research in HEPP.

The central tool for HEPP investigation is the particle accelerator. Particle accelerators have made it possible to increase the number of events from which to sample in testing scientific theories.²⁸ Generating such information by other means is prohibitively expensive, if not technically impossible. For example, the naturally occurring high energy events associated with cosmic rays are too few in number and do not cover enough types of events to advance basic scientific understanding (NRC (1986e), p. 153). By expanding the number of events available for analysis and interpretation, particle accelerators create new information, in the form of experimentally validated theories, more rapidly.

As HEPP research has focused on more disaggregated levels of the subatomic structure of matter, the reliability of homotopic mappings has declined. Results attained in the analysis of atomic interactions, or subatomic interactions at a higher level of aggregation, are no longer applicable to the new subatomic levels of research. Indeed, new scientific information about matter and energy now is created by observing the extent to which physical phenomena do not scale up to higher energy levels. Improvements in accelerator technology have made higher energy levels attainable and have repeatedly forced revisions in theory.²⁹

Because theories of energy and matter cannot be homotopically mapped to higher energy levels, improved scientific understanding requires the achievement of ever higher particle accelerator energies. If empirical information is to be generated, new accelerators and instrumentation must be developed to analyse subatomic interactions and particles.³⁰ The capital requirements of these new facilities in turn make research in high-energy particle physics increasingly "lumpy." Substantial incremental investments and long periods of preparation now are necessary to advance HEPP research.

High-energy particle physics has yielded numerous analogic links with other disciplines. Such fundamental scientific principles as asymmetry, conservation principles, "state" transitions, and the continued extension of quantum mechanics from HEPP research have influenced the basic research agenda in many other scientific disciplines, including molecular biology, chemistry, and medical diagnostic procedures (Adair (1987)).

8. CONCLUSION

The effort to derive summary estimates of the costs and benefits of large basic research investments appears to be misguided. The channels through which basic research yields economic payoffs are so complex, and the assumptions necessary to develop estimates of the returns on an investment in basic research are so fragile and unrealistic, that this exercise is of little use in guiding actual policy decisions. Moreover, this analysis relies on a conceptualization of basic research that likens the research

²⁸ U.S. Congress, Joint Committee on Atomic Energy (1965b), Testimony of A. H. Rosenfeld, p. 324.

enterprise to a water resources project or other large public works investment, ignoring the essential differences in the nature and time profile of the "results" of these investments in information.

The outputs of basic research rarely possess intrinsic economic value. Instead, they are critically important inputs to other investment processes that yield further research findings, and sometimes yield technological innovations. Focusing solely on the eventual commercial applications of information obtained through basic research also can lead to efforts to restrict the flow of basic research findings and ideas within the global or domestic economy in an effort to appropriate the benefits of such research. Such policies could be seriously detrimental to the pursuit of scientific knowledge, and thus may have negative economic impacts upon the very countries that mistakenly suppose they would gain by keeping their basic research findings "secure," as exclusive national assets. In addition, this conceptualization overlooks the central role of other "downstream," complementary investments and actors in realizing the economic returns from basic research. Policies that focus exclusively on the support of basic research in an effort to improve national competitiveness, for example, will be ineffective unless they attend to these complementary factors. Distinguishing more clearly between the "direct" and the "byproduct" outputs of basic research also yields important insights into the implications of structural differences among national research systems.

The alternative information-theoretic conceptualization of basic research that we have presented here focuses on basic research as a process of learning about the physical world that can better inform the processes of applied research and development. Rather than yielding outputs that are marketed commercially, basic research interacts with applied research in a complex and iterative manner to increase the productivity of both basic and applied research. Interactions among the various stages of the innovation process, including exchanges of personnel and information between applied and basic research, contribute to innovative success. The returns to investments in basic research are likely to be higher when this research is performed in organizations with strong links to prospective innovators.

Using high-energy particle physics as an example, we propose some criteria that might be extended to compare the effects and costs of investment in different fields of basic research. These criteria require substantial refinement and extension to yield operational bases for investment decisions, but they hold some promise for the comparison of basic research in different disciplines, a significant improvement over current cost-benefit and other techniques. Longitudinal and cross-sectional bibliometric and patent citation analyses could provide a useful starting point for this work. The rapidly escalating costs and demands for basic research funds within the global research community make such an analysis increasingly urgent.

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