THE SIMPLE ECONOMICS OF BASIC SCIENTIFIC RESEARCH

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I. BASIC ECONOMIC FRAMEWORK

RECENTLY, orbiting evidence of un-American technological competition has focused attention on the role played by scientific research in our political economy. Since Sputnik it has become almost trite to argue that we are not spending as much on basic scientific research as we should. But, though dollar figures have been suggested, they have not been based on economic analysis of what is meant by "as much as we should." And, once that question is raised, another immediately comes to mind. Economists often argue that opportunities for private profit draw resources where society most desires them. Why, therefore, does not basic research draw more resources through private-profit opportunity, if, in fact, we are not spending as much on basic scientific research as is "socially desirable"? In order to answer some of these questions, it seems useful to examine the simple economics of basic research. How much are we spending on basic research? How much should we be spending? Under what conditions will these figures tend to be different? Is basic research marked by these conditions? If so, what can we do to eliminate or reduce the discrepancy?

How much are we spending on basic research? In 1953, the latest date for which relatively sophisticated estimates are available, total expenditure on research and development was about $5.4 billion. Of that total, much more than half was for engineering development, much less than half for scientific research. Even less of the total, about $435 million in 1953, was spent on "basic research." All evidence indicates that since 1953 expenditure on research and development has increased markedly; $10 billion seems a reasonable estimate for 1957. Expenditure on basic research has also increased at a rapid rate, perhaps at a faster rate than total research and development expenditure. But basic-research expenditure today is probably under $1 billion, less than one-quarter of 1 per cent of gross national product.[1]

How much should we spend on basic research? Replacing the X_i of the familiar literature on welfare economics with "basic research" provides the theoretical answer. From a given expenditure on science we may expect a given flow, over time, of benefits that would not have been created had none of our resources been directed to basic research. This flow of benefits (properly discounted) may be defined as the social value of a given expenditure on basic research. However, if we allocate a given quantity of resources to science, this implies that we are not allocating these resources to other activities and, hence, that we are depriving ourselves of a flow of future benefits that we could have obtained had we directed these resources elsewhere. The discounted flow of bene-

[1] National Science Foundation, *Basic Research: A National Resource* (Washington, D.C., 1957); *Science and Engineering in American Industry* (Washington, D.C., 1956); *Growth of Scientific Research in Industry—1945–1960* (Washington, D.C., 1957); *Federal Funds for Science—The Federal Research and Development Budget Fiscal Years 1956, 1957, and 1958* (Washington, D.C., 1957).

fits of which we deprive ourselves by allocating resources to basic research and not to other activities may be defined as the social cost of a given expenditure on basic research. The difference between social value and social cost is net social value, or social profit. The quantity of resources that a society should allocate to basic research is that quantity which maximizes social profit.

Under what conditions will private-profit opportunities draw into basic research as great a quantity of resources as is socially desirable? Under what conditions will it not? If all sectors of the economy are perfectly competitive, if every business firm can collect from society through the market mechanism the full value of benefits it produces, and if social costs of each business are exclusively attached to the inputs which it purchases, then the allocation of resources among alternative uses generated by private-profit maximizing will be a socially optimal allocation of resources. But when the marginal value of a "good" to society exceeds the marginal value of the good to the individual who pays for it, the allocation of resources that maximizes private profits will not be optimal. For in these cases private-profit opportunities do not adequately reflect social benefit, and, in the absence of positive public policy, the competitive economy will tend to spend less on that good "than it should." Therefore, it is in the interests of society collectively to support production of that good.[2]

Society does, in fact, collectively support a large share of the economy's basic research. About 60 per cent of our basic-research work is performed by non-profit institutions, predominantly government and university laboratories. And a portion of the basic research performed in industrial laboratories is government financed. (This flow of funds is about equal to the flow of funds from industry to non-profit laboratories in the form of grants and contracts for basic research.)[3] Much, though not all, of the government contribution to basic research is national defense–oriented. But defense-oriented expenditure aside, the American political economy certainly treats basic research as an activity that creates marginal social value in excess of that collectable on the free market.[4] Is this treatment justified? If so, since, in fact, society is collectively supporting much basic research and hence resources directed to basic research do exceed the quantity drawn by private profit opportunity, is present social policy adequate?

II. SCIENTIFIC RESEARCH AND ECONOMIC VALUE

What are the social benefits derived from the activity of science? It is sometimes argued that most of our great social and political problems would simply evaporate if all citizens had a scientific point of view and, hence, that the benefits derived from scientific research are only in small part reflected in the useful inventions generated by science, for science helps to make better citizens. And

[2] Of course, the resources supplied to the industry must be withdrawn from other industries which generate no external economies or less external economies. Although significant external economies are probably rare, they almost certainly exist in education and preventive medicine as well as in basic research. Though the burden of this paper is that more resources should be allocated to basic research, the argument is probably invalid if these resources are taken, for example, from education or preventive medicine.

[3] National Science Foundation, *Basic Research: A National Resource*.

[4] This paper will not consider the vital question of whether the Department of Defense is spending enough on defense-oriented basic research. It probably is not, but the analysis of the paper is independent of this.

many scientists and philosophers take the point of view that the very activity of science—considered as the search for knowledge—is itself the highest social good and that any other benefits society might obtain are just by-products of the activity of science—social gravy. Dissents on both of these points are often sharp. The economist, after the usual perfunctory statement that he is fully aware that his definition does not capture everything, might define the benefits derived from the activity of science as the increase resulting from scientific research in the value of the output flow that the resources of a society can produce. In order to examine the extent to which a private firm can capture through the market the increased value of output resulting from the scientific research, in particular the basic scientific research, that it sponsors, it is necessary to examine the link between scientific research and the creation of something of economic value.

Scientific research may be defined as the human activity directed toward the advancement of knowledge, where knowledge is of two roughly separable sorts: facts or data observed in reproducible experiments (usually, but not always, quantitative data) and theories or relationships between facts (usually, but not always, equations). Of course, no strict line can be drawn between scientific research and all other human activities. Men have always experimented and observed, have always generalized and theorized; thus all men have always been, at least in a limited way, scientists. And knowledge has often (usually?) been acquired in activities in which pursuit of knowledge was of no, or negligible, importance. But even fuzzy definitions often have value. Scientific knowledge rests on reproducible experiments, but science is more than experimentation leading to new observations of facts which are believed observable by any other scientist undertaking the identical experiment. Science is most fruitful when it leads to ability to predict facts about phenomena without, or prior to, experimentation and observation. Scientific knowledge has economic value when the results of research can be used to predict the results of trying one or another alternative solutions to a practical problem.

Scientific research has increasingly been coupled to invention, where invention is defined as the human activity directed toward the creation of new and improved practical products and processes. But though many inventions occur as a result of a reasonably systematic effort to achieve a particular goal, many other inventions do not. They are a by-product of activity directed in a quite different direction, often a scientific research project directed toward solving an unrelated problem. Mauve, the first analine dye, was discovered by W. H. Perkin while he was attempting to synthesize quinine, and calcium carbide and the acetylene gas that it produces were invented by a group attempting to develop a better way to extract aluminum from clay. And though many inventions are made possible by closely preceding advances in scientific knowledge, many others require little knowledge of science or occur long after the relevant scientific knowledge is available: scientific knowledge certainly had little to do with the development of such useful inventions as the safety razor and the zipper; scientists have long known that expanding gases absorb heat, thus cool whatever they contact, but the gas refrigerator is an invention of the twentieth century. But particularly in the institution of the industrial research laboratory, applied science and invention are closely linked, and inventions usually result from a systematic attack on a problem.

In the activity of invention, as in most goal-directed activities, the actor has a number of alternative paths among which he must choose. The greater his knowledge of the relevant fields, the more likely he will be eventually to find a satisfactory path, and the fewer the expected number of tried alternatives before a satisfactory one is found. Thus, the greater the underlying knowledge, the lower the expected cost of making any particular invention.

A rationally planned inventive effort will be undertaken only if the expected revenue of the invention exceeds the expected cost. In many instances the economic utility of a particular invention is so great that an inventive effort is economically rational, even though the underlying scientific knowledge is scanty and hence the expected cost of making the invention is great. Edison's attempt to develop an incandescent lamp and Goodyear's attempt to improve the characteristics of rubber are cases in point. In these cases, since there was little useful underlying scientific knowledge, the invention procedure was trial and error, the next trial being roughly—but only roughly—indicated by a very loose theory formulated as the research proceeded. But though the inventors knew that it would probably prove costly to achieve their objective, they believed that the gains, if they were successful, were sufficiently great to make the effort profitable.

But often, though the inventor believes that there is great demand for a particular invention, it is not rational for him to attempt the invention, given the state of scientific knowledge. Expected cost will exceed expected revenue unless additional scientific knowledge can be obtained. If the expected cost of acquiring the relevant scientific knowledge is low, an organization interested in making a particular invention may undertake an applied scientific research project. A profit-maximizing firm will undertake a research project to solve problems related to a development effort if the expected gains—for example, reduction in development costs, or improvement in the final developed product—exceed expected research costs and if total research and development cost is exceeded by the expected net value of the invention. To the extent that the results of applied research are predictable and relate only to a specific invention desired by a firm, and to the extent that the firm can collect through the market the full value of the invention to society, opportunities for private profit through applied research will just match social benefits of applied research, and the optimum quantity of a society's resources will tend to be thus directed.

However, by no means all scientific research is directed toward practical problem-solving, though the line between basic scientific research and applied scientific research is hard to draw. There is a continuous spectrum of scientific activity. Moving from the applied-science end of the spectrum to the basic-science end, the degree of uncertainty about the results of specific research projects increases, and the goals become less clearly defined and less closely tied to the solution of a specific practical problem or the creation of a practical object. The loose defining of goals at the basic research end of the spectrum is a very rational adaptation to the great uncertainties involved and permits a greater expected payoff from the research dollar than would be possible if goals were more closely defined. For commonly, not just sometimes, in the course of a research project unexpected possibilities not closely related to the original objectives appear, and concurrently it may become clear that the original objectives are unobtainable or will be far more difficult to achieve than originally expected. While the direction of an applied research project must be closely constrained by the practical problem which must be solved, the direction of a basic research project may change markedly, opportunistically, as research proceeds and new possibilities appear. Some of the most striking scientific breakthroughs have resulted from research projects started with quite different ends in mind.

Pasteur's discovery of the value of inoculation with weakened disease strains is one of the more famous cases in point, but what is important is that the case is so similar to many others. While studying chicken cholera, Pasteur accidentally inoculated a group of chickens with a weak culture. The chickens became ill but, instead of dying, recovered. Since Pasteur did not want to waste chickens, he later reinoculated these chickens with fresh culture—one that was strong enough to kill an ordinary chicken—but these chickens remained healthy. At this point Pasteur's attention shifted to this interesting and potentially very (socially) significant phenomenon, and his resulting work, of course, brought about a major medical advance.

Applied research is relatively unlikely to result in significant breakthroughs in scientific knowledge save by accident, for, if significant breakthroughs are needed before a particular practical problem can be solved, the expected costs of achieving this breakthrough by a direct research effort are likely to be extremely high; hence applied research on the problem will not be undertaken, and invention will not be attempted. It is basic research, not applied research, from which significant advances have usually resulted. It is seriously to be doubted whether X-ray analysis would ever have been discovered by any group of scientists who, at the turn of the century, decided to find a means for examining the inner organs of the body or the inner structure of metal castings. Radio communication was impossible prior to the work of Maxwell and Hertz. Maxwell's work was directed toward explaining and elaborating the work of Faraday. Hertz built his equipment to test empirically some implications of Maxwell's equations. Marconi's practical invention was a simple adaptation of the Hertzian equipment. It seems most unlikely that a group of scientists in the mid-nineteenth century, attempting to develop a better method of long-range communication, would have developed Maxwell's equations and radio or anything nearly so good.

The limitations of an applied-research project constrained to the solution of a specific practical problem, and the practical value of many research projects where the goal is simply knowledge, not the solution of a practical problem, is well illustrated by the development of hybrid corn. During the latter half of the nineteenth century several attempts were made to improve corn yields. Many of the researchers directed their attention, at one time or another, to the inbreeding of corn to obtain a predictable and profitable strain. But as corn plants were inbred, though they tended to breed true, they also tended to deteriorate in yield and in quality. For this reason, applied researchers attempting to improve corn dropped this seemingly unpromising approach. But George Harrison Shull, a geneticist working with corn plants and interested in pure breeds not for their economic value but for experiments in genetics, produced several corn strains that bred true and then crossed these strains. His project was motivated by a desire to further the science of genetics, but a result was high-yield, predictable hybrid corn.

III. BASIC RESEARCH AND PRIVATE PROFIT

It is clear that for significant advances in knowledge we must look primarily to basic research; the social gains we may expect from basic research are obvious. But basic research efforts are likely to generate substantial external economies. Private-profit opportunities alone are not likely to draw as large a quantity of resources into basic research as is socially desirable.

Significant advances in scientific knowledge, the types of advances that are likely to result from successful basic-research projects, very often have practical value in many fields. Consider the range of advances resulting from Boyle's gas law or Maxwell's equations. On Gibb's law of phases rests the design of equipment in fields as diverse as petroleum refining, rubber vulcanization, nitrogen fixation, and metal-ore separation. Few firms operate in so wide a field of economic activity that they are able themselves to benefit directly from all the new technological possibilities opened by the results of a successful basic research effort. In order to capture the value of the new knowledge in fields which the firm is unwilling to enter, the firm must patent the practical applications and sell or lease the patents to firms in the industries affected.

But significant advances in scientific knowledge are often not directly and immediately applicable to the solutions of practical problems and hence do not quickly result in patents. Often the new knowledge is of greatest value as a key input of other research projects which, in turn, may yield results of practical and patentable value. For this reason scientists have long argued for free and wide communication of research results, and for this reason natural "laws" and facts are not patentable. Thus it is quite likely that a firm will be unable to capture through patent rights the full economic value created in a basic-research project that it sponsors.

A firm with a narrow technological base is likely to find research profitable only at the applied end of the spectrum, where research can be directed toward solution of problems facing the firm, and where the research results can be quickly and easily translated into patentable products and processes. Such a firm is likely to be able to capture only a small share of the social benefits created by a basic research program it sponsors. On the other hand, a firm producing a wide range of products resting on a broad technological base may well find it profitable to support research toward the basic-science end of the spectrum.

A broad technological base insures that, whatever direction the path of research may take, the results are likely to be of value to the sponsoring firm. It is for this reason that firms which support research toward the basic-science end of the spectrum are firms that have their fingers in many pies. The big chemical companies producing a range of products as wide as the field of chemistry itself, the Bell Telephone Company, General Electric, and Eastman Kodak immediately come to mind. It is not just the size of the companies that makes it worthwhile for them to engage in basic research. Rather it is their broad underlying technological base, the wide range of products they produce or will be willing to produce if their research efforts open possibilities. (Eastman Kodak entered the vitamin business when a research project resulted in a new way to synthesize vitamin B.) Strangely enough, economists have tended to see little economic justification for giant firms not built on economies of scale. Yet it is the many-product giants, not the single-product giants, which have been most technologically dynamic, and, to the extent that we wish the private sector of the economy to support basic research, we must look to these firms.

The importance of a broad technological base as a factor permitting a company to engage profitably in basic research is clearly illustrated by Carothers' famous research project for Du Pont. Carothers' work in linear superpolymers began as an unrestricted foray into the unknown with no particular practical objective in view. But the research was in a new field of chemistry, and Du Pont believed that any new chemical breakthrough would probably be of value to the company. The very lack of a specific objective, the flexibility of the research project, was an important factor behind its success. In the course of research Carothers obtained some superpolymers which at high temperatures became viscous fluids and observed that filaments could be obtained from these materials if a rod were dipped in the molten polymer and then withdrawn. At this discovery the focus of the project shifted to these filaments. Nylon was the result, but at the start of the project Carothers could not possibly have known that his research would lead him to the development of a new fiber.

A wide technological base (usually involving a diversified set of products) does not imply a position of monopoly power in any or all of the product markets, nor does a monopoly position in a market imply a wide technological base. Focusing attention on market position, a business firm operating in a competitive environment will seldom find it profitable to engage in a research project which is not likely to result quickly in something patentable, even if the firm can predict the nature of the research results, unless the firm keeps tight secrecy. For if the results of research cannot be quickly patented and are not kept secret, other firms producing similar products using similar processes will be free to use the results as an input of a development program of their own, designed to achieve a similar patentable objective. If competing firms develop a patentable product first, or develop a competing product, these firms will in effect steal from the research-sponsoring firm, through price and product competition, a large share of the social utility created by research. In fact, many companies engaging in research keep their research findings secret until the new knowledge is put to practical use and the results are patented.

Many industries have attempted to reconcile their need for new knowledge with the lack of incentives to individual private firms to produce that new knowledge by establishing co-operative industry research organizations. To the extent that an industry rests on a field of science that is likely to get little attention in the absence of sponsorship by the firms in the industry, it may be in the interests of all the firms that research in this field be pushed, though each firm would prefer the others to do the financial pushing. An industrial co-operative research laboratory may well develop under these conditions, supported by all or by a large number of the firms in the industry, and undertaking research likely to be applicable to the technology of the industry. The motivation for these co-operative laboratories is only in part the high cost of research. More importantly, these laboratories are motivated by the fact that most of the firms will gain from the results of relatively basic research in certain fields whether or not they pay for it; hence little research will be undertaken in the absence of co-operation.

The preceding argument has been focused on external economies that open a gap between marginal private and marginal social benefit from basic research. Two other factors, working in the same direction, must be mentioned, if not discussed. First, the long lag that very often occurs between the initiation of a basic-research project and the creation of something of marketable value may cause firms much concerned with short-run survival, little concerned with profits many years from now, to place less value on basic-research projects than does society, even in the absence of external economies. This is not to say that all firms have a greater time-discount factor than does society as a whole, but it can be argued that many firms do. Second, the very large variance of the profit probability distribution from a basic research project will tend to cause a risk-avoiding firm, without the economic resources to spread the risk by running a number of basic-research projects at once, to value a basic-research project at significantly less than its expected profitability and hence, even in the absence of external economies, at less than its social value.

IV. IS CURRENT SOCIAL POLICY ADEQUATE?

It seems clear that, were the field of basic research left exclusively to private firms operating independently of each other and selling in competitive markets, profit incentives would not draw so large a quantity of resources to basic research as is socially desirable. But in fact basic research has not been the exclusive domain of private firms. Government and other non-profit institutions (principally universities) together spend more on basic research, and undertake more basic research in their own laboratories, than does industry. Since we are presently supporting collectively such a large share (more than half) of basic research, is it not possible that total basic-research expenditure (the sum of private and public efforts) equals or exceeds the social optimum? This is a tricky theoretical question. However, if basic research can be considered as a homogeneous commodity, like potato chips, and hence the public can be assumed to be indifferent between the research results produced in government or in industry laboratories; if the marginal cost of research output is assumed to be no greater in non-profit laboratories than in profit-oriented laboratories, and if industry laboratories are assumed to operate where marginal revenue equals marginal cost, then the fact that industry laboratories do basic research *at all* is itself evidence that we should increase our expenditure on basic research.

Public support of basic research has primarily been in the form of contracts let with private firms and in the establishment and support of a large number of non-profit laboratories. Save for the effects of tax laws (which apply to all business cost-incurring activities), public policy has not acted to *shift* the marginal cost curve of the basic-research industry. Public policy has resulted in shifts *along* the curve. Nor has public policy acted to drive marginal social utility to marginal private utility. External economies still exist at the margin. Clearly then, if industry laboratories are in profit-maximizing equilibrium, society would benefit from an increase in basic-research expenditure in industry laboratories, holding research efforts elsewhere constant, for the marginal social benefit of basic research in private laboratories exceeds marginal cost to the firm, which under our assumptions still equals alternative cost. But perhaps non-profit laboratories are spending too much on basic research —are operating beyond the point at which marginal cost equals marginal social benefit—and therefore it is desirable to reduce research expenditure in this sector. Given our assumptions, this cannot be. For, if marginal cost is no greater in non-profit laboratories than in industry laboratories, and society cannot distinguish between the fruits of research undertaken in the two kinds of laboratory—that is, if marginal social benefit is the same in the public and the private sector—and if it is socially desirable that expenditure on basic research be increased in industry laboratories, then it is also socially desirable that research expenditure be increased in non-profit laboratories. For if marginal social benefit exceeds marginal cost in industry laboratories, so does it in non-profit laboratories.

The assumptions on which the preceding argument is based rest but shakily on fact. Basic research certainly is not a homogeneous commodity. The types of knowledge generated, say, in an air-force project on high-speed gas flows, a Du Pont project on high polymer chemistry, or a Harvard project on solid-state physics are not perfectly substitutable. The knowledge generated will certainly be different, and in a reasonably predictable way. And, once the non-homogeneity of basic research is admitted, the concept of relative marginal cost becomes fuzzy. Thus one cannot make an airtight statement, based on welfare economics, that we are not spending as much on basic scientific research as we should. But I believe that the evidence certainly points in that direction.

V. SOME POLICY IMPLICATIONS

Though the profit motive may stimulate private industry to spend an amount on applied research reasonably close to the amount that is socially desirable, it is clear from the preceding analysis that under our present economic structure the social benefits of basic research are not adequately reflected in opportunities for private profit. Indeed, there is a basic contradiction between the conditions necessary for efficient basic research—few or no constraints on the direction of research with full and free dissemination of research results—and full appropriation of the gains from sponsoring basic research in a competitive economy.

This is not to say that some firms could not profitably increase their basic-research effort. Some may presently be operating well to the left of their maximum profit point. But to the extent that we want our economy to remain competitive and want efficient use of basic-research funds, the laboratories of colleges, universities, and other non-profit institutions must perform a large share of our basic research if we are to put as much of our resources into basic research as we should. Although several laboratories of private industry have made significant contributions in the field of basic science, these contributions have been few and far between. If we advocate that basic research be increasingly undertaken by business and if we believe that business should be motivated by profit, we must accept the growth of large firms with a wide technological base, with virtual monopolies in several markets. If we do not want such an economic structure, then only to the extent that we think it desirable that private firms look to motives other than profit can we argue that industry laboratories should perform a significantly enlarged share of our basic research. In either case we undermine many of the economic arguments for a free-enterprise economy. If we want to maintain our enterprise economy, basic research must be a matter of conscious social policy.

This is not the place to suggest a menu of policies—the bill of fare offered in the National Science Foundation booklet on basic research lists some of the actions that might be considered. However, it does seem appropriate to suggest that public policy on basic research should recognize the following points:

1. The problem of getting enough resources to flow into basic research is basically the classical external-economy problem.[5] External economies result from two facts: first, that research results often are of little value to the firm that sponsors the research, though of great value to another firm, and, second, that research results often cannot be quickly patented. It therefore seems desirable to encourage the further growth of a "basic-research industry," a group of institutions that benefit from the results of almost any basic-research project they undertake. University laboratories should certainly continue to be a major part of this industry. However, an increasingly important role should probably be played by industry-oriented laboratories not owned by specific industries but doing research on contract for a diversified set of clients. Such laboratories would usually have at least one client who could benefit from almost any research breakthrough.

2. The incentives generated in a profit economy for firms to keep research findings secret produce results that are, in a static sense, economically inefficient. The use of existing knowledge by one firm in no way reduces the ability of another firm to use that same knowledge, though the incentive to do so may be reduced. The marginal social cost of using knowledge that already exists is zero. For maximum static economic efficiency, knowledge should be administered as a common pool, with free access to all who can use the knowledge. But, if scientific knowledge is thus administered, the incentives of private firms to create new knowledge will be reduced. This is another case in which static efficiency and dynamic efficiency may conflict. It is socially desirable that as much of our basic research effort as possible be undertaken in institutions interested in the quick publication of research results if marginal costs are comparable. In the absence of incentives to private firms to publish research results quickly (such incentives might be legislated) a dollar spent on basic research in a university laboratory is worth more to society than a dollar spent in an industry laboratory, again, if productivity is comparable.

3. If society places the brunt of the basic-research burden on universities, funds must be provided for this purpose. The current Department of Defense policies of letting huge applied research projects with universities should either be reconsidered or complemented with other policies designed to prevent the increased applied-research burden from drawing university facilities and scientists away from basic research. This is not to say that universities cannot effectively undertake applied research. Rather it is to say that their comparative advantage lies in basic research.

[5] The external economy aspect of basic research reacts back through the price system to undervalue pure scientists relative to engineers.