Why do firms do basic research (with their own money)? *

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The question to be addressed is: Why do private firms perform basic research with their own money? Interest in this question derives from both analytical and utilitarian considerations. There is empirical evidence in the United States, which provides the main context for this paper, supporting the view that basic research makes a significant contribution to the productivity growth of the economy [4,7]. It is widely held that social returns from basic research are significant and higher than private returns and it is for this reason that most such activities continue to be financed by the taxpayer. This also implies that measures aimed at increasing basic research by the private sector will be welfare improving. In the United States, the federal government in the years since the Second World War has provided the vast majority of all funds devoted to basic research. Although the federal share has been declining in recent years, and although that share is at its lowest level in about 20 years, it still constitutes about two-thirds of the total [10].

If the goal is to encourage the private sector to spend more money on basic research, it is necessary to start out by asking why they would want to do so in the first place. Suppose it is taken as axiomatic that private industry is in business solely to make money, and therefore that firms are not prepared to spend money for merely sentimental or humanitarian purposes. Let us assume that they will spend their own money on basic research only when they are reasonably confident that it will yield a rate of return on this investment in the generation of knowledge that is at least comparable to the rate of return that they would expect on some other form of investment in more tangible capital.

It is important to think of basic research by private industry as a form of investment, and more will be said about this later. But it may be useful to anticipate some of that later discussion here by pointing out that basic research, from a private firm's point of view, is not only an investment. It is, more precisely, and really by definition, a long-term investment. There is, typically, no reasonable expectation that these expenditures will begin to generate a cash flow in the next few years or so.

Why, then, should private industry be willing to make such expenditures? The question is a crucial one for the academic economist as well as for policymakers in both the public and private sectors. Since the seminal papers by Arrow and Nelson [2,11], it has been accepted by most economists that a private enterprise economy fails to provide adequate incentives for investment in knowledge production. There are several reasons for this assertion. First, there is inherently a high degree of uninsurable risk and uncertainty that increases as we move along the basic research end of the research spectrum. Secondly – and this is specific to knowledge as a commodity – it is believed that knowledge, once produced, is in some meaningful sense "on the shelf". As a consequence, neoclassical/mainstream economics held that, once produced, knowledge was freely available to all, in-

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cluding those firms that may have made no contribution whatever to the production of the knowledge: A classic "free rider" situation. In certain cases, one could counteract this by creating property rights in knowledge; but not all kinds of knowledge are patentable in such a way as to preclude a competitor from exploiting that knowledge. For these reasons it has been held that there was a serious problem of appropriability, in that firms financing the research have no adequate recourse or mechanism for appropriating the benefits of the research to themselves.

It is important to note that this outcome is not the result of insufficient or imperfect competition. Although it is a fair charge against the main tradition of modern microeconomics that it tends to attribute almost any problem in resource misallocation to insufficient competition, this is not one of those cases. It has been pointed out that the market for knowledge is inherently imperfect and thin because, in order to determine the value of information, it would be necessary, in general, to *know* the information. This of course creates a fundamental difficulty because, once the buyer knows the information, she has no incentive to pay for it.

Arrow has pointed out that society would invest insufficient resources in research even under perfectly competitive conditions [2]. He also argued that a monopolist's pre-invention monopoly profits weakened the incentive to invent as compared to the competitive situation [2, pp. 175–179]. Indeed, in one sense competition even exacerbates the problem. The likelihood that competitors will quickly exploit the useful new knowledge weakens even further the incentives on the part of competitive firms to invest in knowledge production in the first place. The only way to strengthen those incentives is by offering the firm that conducts the research a proprietary control over the valuable findings that the research generates. But such control – e.g., in the form of patent rights – puts us on the other horn of the efficiency dilemma. This is because, once knowledge has been produced, it is costlessly available for other firms to utilize as well. Any restriction on such use is socially suboptimal because it would deprive some firms of opportunities to raise their productivity by making use of knowledge that is already produced and therefore available to society at no incremental cost. To deprive firms of truly costless opportunities to improve their efficiency is obviously suboptimal.

This is the major conclusion of economic theory: Market incentives are insufficiently strong to generate the socially optimal amount of investment in research – because of nonappropriabilities and uncertainties. However, attempts to alleviate that problem – by allowing firms to appropriate the findings of research – create an equally serious problem because they impose restrictions upon the use of valuable knowledge that has already been produced.

The economist's conclusion that normal market forces do not provide strong incentives for the performance of research, especially basic research, is quite consistent with observations of the real world. The obvious empirical fact is that the overwhelming majority of private firms do not finance the performance of any basic research. Basic research is, in fact, highly concentrated in two senses: (1) the great bulk of all such research is performed in a very small number of industrial sectors, and (2) within these sectors there is a handful of firms, typically large firms, that dominate the basic research picture.

Only limited data are available with respect to company spending on basic research. With respect to sectoral concentration, incomplete data published by the National Science Foundation [10, p. 59] indicate that, in 1984, 61 percent of company-financed basic research was in four sectors.

Chemicals	$ 677 million
Electrical equipment	450
Aircraft & Missiles	248
Machinery	209
subtotal	$1584/$2578 = 61 % [1]

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The question persists: Why do the firms that do basic research do it? Alternatively, why do some

[1] The sectoral breakdown for *all* company-funded R&D in 1984 was as follows:

Electrical equipment	$ 9157 million
Machinery	8455
Chemicals	7802
Motor vehicles and equipment	5413
Prof. and scientific instruments	4250
Subtotal	$35077/48065 = 73%

[10, p. 56].

firms find it profitable (or expectedly profitable) to do it?

Economists, as we have seen, have stressed the problems associated with appropriability as the main deterrent to basic research – the difficulty, under normal market conditions, of appropriating the benefits generated by the research findings. But it should be noticed, first of all, that this requires an important qualification. Even if a firm's basic research generated many benefits that it could not appropriate, the mere *existence* of such nonappropriabilities is never an adequate explanation for the reluctance to perform basic research. So long as the performing firm can capture some of the benefits, that might be sufficient for it to do some research. It is not necessary to capture all of the benefits – indeed, it would be undesirable if it did. Research is socially desirable precisely because it often generates such widespread and indiscriminate benefits. All that is necessary is that market forces allow the firm to capture *enough* of these benefits to yield a high rate of return on its own investment in basic research.

Thus, the existence of spillovers and nonappropriabilities that allow competitors a free ride is not a decisive case against the performance of R&D (or specifically basic research) by private firms. If the production of new knowledge generates commercial opportunities to the performer, the relevant calculation involves, not the size of the spillovers, but whether the performing firm can capture enough of the benefits generated to yield a high rate of return on its investment. Even in the extreme case of basic research, where there is no prospect of establishing proprietary control over the research findings, commercial benefits may nevertheless be very great.

These potential benefits largely take the form of what are called "first-mover advantages". This is a big subject that will not be addressed here beyond indicating the categories into which such advantages may fall. They include a variety of learning experiences. Firms that move down such learning curves first – whether these curves pertain to cost reductions or performance improvements – may be able subsequently to exploit the advantages conferred as a barrier to the entry of new firms. First-movers may be able to acquire valuable assets – e.g., of a geographic location or a mineral deposit whose commercial worth will be favorably affected by new research findings. To the extent that the findings of basic research can be translated into patentable assets farther downstream, first-movers may be able to consolidate their market position through patent protection. Furthermore, buyer switching costs may be significant and may constitute a significant form of protection against competitors for firms that are first to enter the new product line.

Although first-mover advantages may thus be substantial, there may also be substantial first-mover disadvantages as well as late-mover advantages. If there are significant spillovers of knowledge between firms, then a late-mover could gain the same knowledge at a lower cost while, at the same time, avoiding the major mistakes that the first-mover made en route. Nevertheless, for present purposes I wish merely to assert that first-mover advantages may frequently survive the offsetting disadvantages and serve as a significant incentive for the performance of basic ressearch [6].

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One fairly obvious, but nonetheless important statement about basic research in private industry is that most firms that have engaged in it have had fairly strong and well-entrenched positions of market power. Precisely because the potential payoff to basic research is so long term, only firms that were reasonably confident of being around in the long term would be likely to consider the possibility of making such commitments. Thus, the most successful basic research labs have been in firms with strong market positions: Bell Labs (especially before divestiture), IBM, DuPont, Dow Chemical, Eastman Kodak, etc. GM is reputed to have done quite a bit of basic research some years ago, but the commitment to basic research is said to have declined along with the decline in GM's market share in the automobile industry (GM has had the largest total R&D budget of any private American firm – \$3.6 billion in 1986. What proportion of that amount is basic research is not public information, but it is probably very small). Another obvious reason why small firms hardly ever do basic research is that if research findings are difficult to patent and hence the flow of payoffs cannot be capitalised, then these payoffs must be appropriated via the means mentioned earlier – essentially through incorporating the knowledge in the form of improved goods or processes. This means, of course, that a larger market share will offer the prospect of a higher payoff.

A separate but related point is that it is not size alone or market power that matter. Large firms may be more willing to undertake basic research when they have a diverse range of products and strong marketing and distribution networks that increase their confidence that they will eventually be able to put the findings of basic research to some good commercial use. In view of the high degree of uncertainty that surrounds the outcome of basic research, it is probably very important to a firm to have the confidence that it will know how to exploit new knowledge that may turn up in unexpected places, and that it will have the complementary assets that will enable it to do so. For an illuminating treatment of related issues see Teece [14].

The considerable number of small biotechnology firms appears to contradict the view that investment in basic research requires a strong market position. It is certainly true that, in the drug industry proper, the most active research programs seem to be concentrated in the large firms – Merck, Johnson and Johnson, Lilly, Pfizer, Upjohn, etc. But in the biotechnology field a good deal of basic research is currently being performed by small companies such as Genentech. The really curious aspect of these small companies is that many of them have no marketable products at all, and hardly any have more than just a few.

What seems to be happening here is that the small biotechnology firms are engaged in basic research that is believed to be close to the commercialization stage. At the same time, it is a highly speculative game that is being financed by venture capitalists, as well as some large firms and wealthy individuals, who are lured by the possibility of a very high payoff.

What appears to be driving the small firms that perform basic research in biotechnology is the first-mover advantages – or at least an expectation that first-mover advantages may be critical. Much of the investment in this basic research took place before the recent changes in the patent law that extended patent protection to live organisms. But this extension of the law must certainly strengthen the incentives to do basic research. Furthermore, patents have, in general, provided more effective protection to proprietary knowledge in the pharmaceutical industry than in most other industries. In this respect the innovative "output" of small biotechnology firms is likely to be more readily appropriable than is the case for small firms in other industries.

The venture capital industry seems to be treating biotechnology as a kind of lottery. The vast majority of firms will almost certainly be losers when the eventual "shake-out" takes place, but some successful innovations may yield a very high return – as Genentech hopes will be the case with its tissue plasminogen activator (TPA) for heart attack victims.

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In understanding why some private firms do basic research it is necessary to recognize that businesses do not live in a neat, orderly world where causal relationships are always clearly defined and where causality always works in one direction only. The business environment is much more interactive, full of "feedbacks" where some "downstream" development reacts back upon, and alters behavior "upstream". Perhaps most important, it is full of unplanned, or accidental, developments that then turn out to have an important set of consequences of their own.

It is essential to emphasize the unexpected and the unplanned, even if – or especially if – it renders serious quantification impossible. In fact, the difficulties in precisely identifying and measuring the benefits of basic research are hard to exaggerate. While this might seem to be just an interesting academic point about the limits of certain methodologies, it has important decisionmaking consequences. The point has been expressed succinctly: "Project selection methodologies of a formal, quantitative nature reduce the tendency to perform basic research" [8].

Part, but only a part, of the problem is that the output of basic research is never some final product to which the market place can attach a price tag. Rather, the output is some form of new knowledge that has no clear dimensionality. The output is a peculiar kind of intermediate good that may be used, not to produce a final good, but (perhaps) to play some further role in the invention of a new final good. These connections are, however, extraordinarily difficult to trace with any confidence, even *ex post*. But even if these difficulties could be overcome, the problems of evaluating the knowledge, and of providing an appropriate incentive system to reward the knowledge producers, would appear to be insuperable.

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Thus, it is doubtful that business decision-makers often sit down and ask, in an abstract way: Should we do basic research? How much basic research should we do? Obviously, private firms feel no obligation to advance the frontiers of basic science as such. Presumably, they *are* always asking themselves how they can make the most profitable rate of return on their investment.

In this context, my own emphasis on the unexpected and the unplanned is deliberate, because the history of basic research in American industry suggests that a very large part of this research has been unintentional. That is to say, basic research findings of major significance have emerged as the unplanned byproduct of the attempt to solve some very specific industrial problem. The fact is that the distinction between basic research and applied research is highly artificial and arbitrary. The distinction is usually made to turn upon the motives, or goals, of the person performing the research. But that is often not a very useful, or illuminating, distinction. If Pasteur had been asked what he thought he was doing back around 1870, he would have replied that he was trying to solve some very practical problems connected with fermentation and putrefaction in the French wine industry. He solved those practical problems – but along the way he invented the modern science of bacteriology. Similarly, if that other great Frenchman, Sadi Carnot, had been asked, some fifty years earlier, what he thought he was doing, his answer would have been that he was trying to improve the efficiency of steam engines. [2] As a byproduct of that particular practical interest, he created the modern science of thermodynamics.

[2] Carnot made this utilitarian concern perfectly clear in the title of his short but immensely influential book, published in 1824. *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance.*

But it is not necessary to go back to nineteenth century France. Those two spectacular scientific breakthroughs are cited simply because they *were* so spectacular. We could, instead, look at Bell Labs in the twentieth century. Back at the end of the 1920s, when transatlantic radiotelephone service was first established, the service was poor because there was lots of static. Bell Labs asked a young man, Karl Jansky, to determine the source of the noise so that it could be reduced or eliminated. He was given a rotatable antenna to work with. Jansky published a paper in 1932 in which he reported three sources of noise: Local thunderstorms, more distant thunderstorms, and a third source, which he identified as "a steady hiss static, the origin of which is not known". It was this "star noise", as he labelled it, which marked the birth of radio astronomy [12].

Jansky's experience (as well as the earlier experiences of Carnot and Pasteur) underlines one of the reasons why the attempt to distinguish between basic research and applied research is extremely difficult to carry out in practice. Fundamental breakthroughs often occur while dealing with very applied or practical problems. Attempting to draw that line on the basis of the motives of the person performing the research – whether there is a concern with acquiring useful knowledge (applied) as opposed to a purely disinterested search for new knowledge (basic) – is, in my opinion, a hopeless quest. Whatever the *ex ante* intention in undertaking research, the kind of knowledge actually acquired is highly unpredictable. Historically, some of the most fundamental scientific breakthroughs have come from people like Carnot, Pasteur and Jansky, who *thought* they were doing very applied research, and who would undoubtedly have said so if they had been asked at the time.

But the distinction breaks down in another way as well. We have to distinguish between the motives of the individual scientists and the motives of the firm that employs them. Many scientists in private industry could honestly say that they are attempting to advance the frontiers of basic scientific knowledge, without any interest in possible applications. At the same time, the motivation of the research managers who decide to finance research in some basic field of science, may be strongly motivated by expectations of eventually useful findings. Thus, Bell Labs decided to support basic research in astrophysics because of its relationship to the whole field of problems and possibilities in microwave transmission, and especially the use of communication satellites for such purposes. It turned out that, at very high frequencies, rain and other atmospheric conditions became major sources of interference in transmission. This source of signal loss was a continuing concern in the development of satellite communications. It was out of such practical concerns that Bell Labs decided to employ Arno Penzias and Robert Wilson. Penzias and Wilson would undoubtedly have been indignant if anyone had suggested that they were doing anything *other* than basic research. They first observed the cosmic background radiation, which is now taken as confirmation of the "big bang" theory of the formation of the universe, while they were attempting to identify and measure the various sources of noise in their antenna and in the atmosphere. Although Penzias and Wilson did not know it at the time, the character of the background radiation that they discovered was just what had been postulated earlier by cosmologists favoring the "big bang" theory. Penzias and Wilson appropriately shared a Nobel Prize for this finding. Their finding was about as basic as basic science can get, and it is in no way diminished by observing that the firm that employed them did so because they hoped to improve the quality of satellite transmission [12]. The parallelism between the fundamental discoveries of Jansky and Penzias and Wilson is very striking.

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I have deliberately examined those instances of basic research emerging out of practical and applied concerns because they provide a valuable entry into the question of how basic research gets to be carried out in private industry. It is often carried out unintentionally. It is, moreover, difficult to understand if one insists on drawing sharp distinctions between basic and applied research on the basis of the motivations of those performing the research. In fact, I would go much further: When basic research in industry is isolated from the rest of the firm, whether organizationally or geographically, it is likely to become sterile and unproductive. The history of basic research in industry suggests that it is likely to be most effective when it is highly interactive with the work, or the concerns, of applied scientists and engineers. This is because the high technology industries are continually throwing up problems, difficulties and anomalous observations that are most unlikely to occur outside of a high technology context. High technology industries provide a unique vantage point for the conduct of basic research, but in order for scientists to exploit the potential of the industrial environment it is necessary to create opportunities and incentives for interaction with other components of the industrial world. Bell Labs before divestiture is probably the best example of a place where the institutional environment was most hospitable for basic research.

The emphasis on interactions and feedbacks suggests a way of thinking about basic research that, I believe, is potentially fruitful. That is, the performance of basic research may be thought of as a ticket of admission to an information network. This network includes a variety of information flows with no particular attempt to distinguish or classify into basic or applied categories. There is a high degree of interactivity, even embracing work that goes on within the realm of Development as well as Research.

It is worth observing that the attempt to classify research into basic and applied categories is particularly hard to take seriously in some areas and disciplines, e.g., in the realms of health, medicine and agriculture. A strict application of the most common criterion for basic research – research that is undertaken without a concern for practical applications – could easily lead to the conclusion that the National Institutes of Health are not deeply involved in basic research, or that current university and industrial research in the realm of biotechnology contain no basic research, which is absurd.

In conducting its resource surveys the NSF defines basic research as research that has as its objective "a fuller knowledge or understanding of the subject under study, rather than a practical application thereof." By contrast, applied research is research directed toward gaining "knowledge or understanding necessary for determining the means by which a recognized and specific need may be met" [9]. These definitions appear to mean that, if the National Institutes of Health directed a major research thrust into cellular biology to provide the knowledge necessary for the development of a vaccine against AIDS, or a cure for a specific form of cancer, that none of the resulting research could be classified as basic. It is difficult to see why the determination to deal with a particular disease cannot give rise to research that provides "a fuller knowledge or understanding of the subject under study", even when there is a "practical application" in mind. Here again the introduction of motives, or goals, is less than useful, as the NSF is forced to acknowledge with respect specifically to research in private industry. Thus the NSF adds to its definition of basic research the following qualification:

> "To take into account industrial goals, NSF modifies this definition for the industry sector to indicate that basic research advances scientific knowledge 'not having specific commercial objectives, although such investigations may be in fields of present or potential interest to the reporting company'" [9].

A further point that needs to be emphasized is that there are a number of activities that are essential to the success of business firms in high technology industries that depend heavily upon a basic research capability, even if that capability does not play a direct role in solving industrial problems. For one thing, firms often need to do basic research in order to understand better how and where to conduct research of a more applied nature. Indeed, that must be a major reason for the performance of basic research in private industry. Many firms need to have a basic research capability because that capability is essential to making effective decisions about their applied research activities. For another thing, a basic research capability is essential for evaluating the *outcome* of much applied research and for perceiving its possible implications.

In providing a deeper level of understanding of natural phenomena, basic research can provide valuable guidance to the directions in which there is a high probability of payoffs to more applied research. In this sense, William Shockley's education in solid state physics during the 1930s may have been critical to the decision at Bell Labs to look for a substitute for the vacuum tube in the realm of semiconductor materials – a search that led directly to the invention of the transistor. In this respect a basic research capability is essential for making strategic decisions about the future product line of the firm and the kinds of process technologies that ought to be adopted. It can also be thought of, therefore, as providing some defensive capability – offering protection against the possibility of a new, competitive product introduced from an unexpected direction.

In an even more general sense, a basic research capability is often indispensable in order to monitor and to evaluate research being conducted elsewhere. Most basic research in the United States is conducted within the university community, but in order to "plug in" to these research centers and to exploit the knowledge that is generated there, a firm must have some in-house capability. A firm is much less likely to benefit from university research unless it also performs some basic research.

This point is important also in identifying a serious limitation in the way economists reason about scientific knowledge and research in general. As I suggested earlier, such knowledge is regarded by economists as being "on the shelf" and costlessly available to all comers once it has been produced. But this model is seriously flawed because it frequently requires a substantial research capability to understand, interpret and to appraise knowledge that has been *placed* upon the shelf – whether basic or applied. The cost of maintaining this capability is high, because it is likely to require a cadre of in-house scientists who can do these things. And, in order to maintain such a cadre, the firm must be willing to let them perform basic research. The most effective way to remain effectively plugged in to the scientific network is to be a participant in the research process.

These assertions require some qualification and shading. Much can be accomplished in monitoring and evaluating many kinds of research activities conducted elsewhere by in-house personnel who are strongly motivated and who place a high value upon such activities. The Japanese have effectively demonstrated these possibilities in the last 30 years or so. Nevertheless, the success of this monitoring capability will often be determined by the sophistication of the in-house staff in evaluating the significance of basic research findings. Moreover, Japan's monitoring achievements were carried out primarily with respect to technological knowledge while Japan was still in a "catch-up" mode, rather than with respect to research that was at or near the scientific frontiers. For an insightful treatment of related issues see Abramovitz [1].

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A final factor that influences the willingness of private firms to finance basic research is the role of the federal procurement process, particularly military procurement. The existence of this enormous market obviously influences the R&D decisions of private firms that want to improve their visibility and their eligibility for government military procurement contracts. An obvious way to do this is to signal one's capabilities by performing R&D of the relevant sort. This practice is directly encouraged by government sponsorship of design and technical competitions in which potential contractors participate, at least partially at their own expense. According to one set of estimates, in 1984 about 30 percent of R&D expenditures by private industry was stimulated by the prospect of securing government procurement contracts (primarily defense) [5]. There are a number of problems with these estimates. Furthermore, for our present purposes, they do not disaggregate total R&D into separate components such as basic research. Nevertheless, they suggest that a large share of private R&D may not be directed toward normal commercial markets where they might contribute directly to productivity growth and improved competitiveness in domestic or international markets; rather, they may be shaped by the desire to signal the capabilities of the firm as an attractive candidate for delivering weapons systems to the federal government. In addition, the reporting of R&D expenditures inevitably contains a certain amount of subjective judgment, and the numbers reported will undoubtedly be influenced by the desire to reduce tax liabilities. The sharp increases in reported R&D expenditures that have occasionally followed quickly upon the introduction of new tax incentives for R&D in several countries may have been more apparent than real.

The growing role of defense R&D in the federal budget suggests that these concerns are of increasing significance. During the 1980s there has been a major increase in the relative importance of military R&D. In 1980 defense R&D constituted 50 percent of all federal R&D expenditures. Thereafter it rose steadily to an (estimated) 72 percent in 1986 [9, p. 226]. Although it is not entirely clear what the impact of this growth has been on company-funded basic research, the larger role of the military tends to reduce the importance of basic research spending within the federal budget. This is because defense R&D expenditures are very highly development-intensive compared to nondefense R&D expenditures. Weapons systems involve immense development costs and skew the federal R&D budget heavily in that direction, as the following data indicate [3].

	1982 Federal R&D expenditures (% share)	
	Defense	Nondefense
Basic research	3.2	33.7
Applied research	11.0	35.3
Development	85.8	31.0

Thus, the growing role of military procurement plus the rising share of defense in the federal R&D budget may be exercising major indirect effects upon the performance of the civilian economy. It is obviously essential to examine more carefully the effects of large military purchases upon the composition of all R&D activities, including those in what are regarded as the civilian sector. It is also important to examine the range of activities within the huge defense R&D budget. For example, even though only 3.2 percent of defense R&D is classified as basic, that is 3.2 percent of an extremely large number, and it constitutes a significant fraction of all basic research that is financed by the federal government. How can these basic research activities be characterized? On what categories of problems are they concentrated? What connections do they have with company-funded R&D? Where are they complements and where are they substitutes? What are the prospects that the output of military R&D may be new technologies of value to the civilian as well as to the military sector? It seems apparent that these questions do not admit of categorical or general answers. There are reasons to believe that the value of spillovers from the military to the civilian sector has changed substantially over the past 40 years or so. These spillovers may vary considerably, depending upon the specific composition of research projects in the defense R&D portfolio. It is especially important to know what are the prospects for the emergence of genuinely "dual use" technologies from defense R&D spending.

In the past there have been specific civilian technological systems that have realized substantial benefits from military R&D. At one time or another in the past forty years, military and space R&D have made major contributions to commercial jet aircraft (including airframes, jet engines and avionics), to computers, to semiconductors, to communication satellites, and to nuclear power. Although it is relatively easy to identify specific technologies where military R&D has generated important civilian benefits, measuring the size of these benefits is far more difficult. Even so, there are strong reasons to believe that the nature of these spillovers is changing over time and that, in many of the industries mentioned, the connection between military and commercial research programs has grown more tenuous. As the requirements of military R&D have become concerned with an increasingly arcane set of needs of modern weapons systems, they have moved further apart from the requirements of civilian markets. In some military technologies it seems that the strong emphasis on product performance and improvement, and the neglect of cost considerations, has created a gap that has drastically reduced the possibilities of significant spillovers to the civilian economy. For a further discussion of spillovers, see [13].

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Although this paper is not primarily concerned with questions of policy, it may nevertheless be appropriate to make a final observation of direct relevance to policy. That is that basic research is, in a very real sense, a long-term investment and needs to be thought about in such terms. Basic research represents a commitment of resources to certain present uses that may eventually have a financial payoff, but there is an unusually high degree of uncertainty attached to this possibility. What is clear is that the payoff, if it comes, is very unlikely to come in the near term.

Although thinking about basic research in the same way that an economist thinks about long-term investment in tangible goods does not exhaust what can usefully be said about basic research, it is nevertheless an extremely valuable intellectual exercise. The point is that we have been considering that portion of basic research that is financed by private industry, where decisions are expected to be made in terms of calculations of present costs and prospective benefits. Thus, anything that strengthens the prospects for *eventual* financial returns is likely to strengthen the willingness to perform basic research. All the usual forces that would strengthen the willingness to commit financial resources to long-term projects become directly relevant to decisions concerning basic research. In fact, I would suggest that the litmus test in thinking about how to influence basic research decisions through government policy is to ask: Does this action improve the prospects for deriving a financial return (eventually) from any useful products that may be generated by the basic research? From this point of view, all government macroeconomic policies that improved the economic environment for long-term business investment would also increase the willingness of business to spend more on basic research. Basic research, in order to be successful, requires the making of stable, long-term commitments. Put negatively, it is likely to be discouraged by erratic and unexpected changes in the business environment. It is favored by the reduction of uncertainties, by increasing business confidence, and by the sense of stable future prospects, including the confidence that government policies themselves will not be subject to frequent change. Not least important, it is favored by low interest rates and reductions in the cost of capital, as is inherently true of all long-term investments.

Finally, a greater confidence in the strength of one's downstream commercialization capabilities should increase the willingness to perform basic research, by strengthening the prospect that the firm will capture a larger share of the potential downstream benefits that may be generated by such research.

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