What makes basic research economically useful? *

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Economic analysis has helped us understand the strong economic dimension in the explosive growth of science, and (more recently) the reasons for continuing public subsidies. However, the growing domination of the "market failure" approach has led to the analytical neglect of two major questions for policy-makers. How does science contribute to technology? Are the technological benefits from science increasingly becoming international?

On the former, too much attention has been devoted to the relatively narrow range of scientific fields producing knowledge with direct technological applications, and too little to the much broader range of fields, the skills of which contribute to most technologies. On the latter, national systems of science and of technology remain closely coupled in most major countries, in spite of the technological activities of large multinational firms.

Empirical research is needed on concentration, scale and efficiency in the performance of basic research, where techniques and insights from the applied economics of industrial R&D are of considerable relevance. There is no convincing evidence so far of unexploited economies of scale in basic research.

This evidence shows that many policies for greater "selectivity and concentration" in basic research have been misconceived. Economists and other social scientists could help by formulating more persuasive justifications for public subsidy for basic research, and by making more realistic assumptions about the nature of science and technology.

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1. Introduction

In this paper, I discuss what we know about the economic usefulness of basic research, and its implications for public policy. It is has become commonplace to argue that we need better knowledge about the economic impact of basic science to assist the agonising choices that must be made, given the "steady state" in the inputs that society is now able and willing to devote to it. Such a steady state of zero growth in science was first raised in the public consciousness by Derek de Solla Price [42] in his classic essay *Little Science, Big Science*, published in 1963. Some analysts say that it has now happened, or is about to happen (see, for example, Ziman [53]).

However, the available data suggest otherwise. At least since the mid-1960s, civilian R&D has grown in the OECD area, both in real terms and as a percentage of Gross Domestic Product. Growth has been particularly rapid in Japan, and slow in the UK. After a deceleration in the 1970s, the rate of growth has in fact increased in the 1980s [37,38]. Furthermore, data published by the National Science Board [32, p. 52] show that the total employment of scientists and engineers in the USA increased annually by 6 percent between 1976 and 1986, and is expected to increase by a further 36 percent by the year 2000. In Japan, the numbers of science and engineering graduates continue to increase, and, as in the USA, a growing proportion are finding employment outside manufacturing in professional services, finance and insurance [23]. These are not symptoms of a stationary state, but rather of vigorous growth in demand for professional and research skills in science and technology.

For basic research, the picture is more complicated. According to OECD estimates R&D in higher education grew in real terms in all OECD countries until the mid-1970s, when it stabilised and even declined slightly in the Federal Republic of Germany, Italy and the UK [37, p. 44]. According to a recent study by Irvine et al. [20], there has been real growth in academic science since then in most major OECD countries, but at a slower rate than Gross Domestic Product. Within the national totals, separately budgeted academic research has grown more rapidly than the share embedded within general university funds.

Thus, for most countries policies for basic science evolve within a regime for growth in demand for research skills and knowledge, no doubt reflecting the importance of technological change in economic efficiency and welfare (see, for example, Fagerberg [10,11]). The case for improving the knowledge-base for policies for basic science must therefore be the old-fashioned one of "timeliness and promise". It is timely to improve understanding of an activity that consumes considerable resources (approximately 0.3–0.5 percent of Gross Domestic Product), and that has a major influence on society's capacity to respond to economic and social demands. And there is considerable promise of such improvement, given our qualitative understanding of the nature and determinants of science, technology and the links between them [29], and recent advances in quantitative, bibliometric techniques.

In section 2, I identify two major contributions by (political) economists to better understanding: the growth of sciences as a factor of production, and the economic case for the public subsidy of basic research. I argue that unwarranted emphasis by contemporary economists on the "public good" and information-like properties of science (and sometimes even of technology) has led to the neglect of two centrally important problems of contemporary science policy: the contribution of science to technology (discussed in section 3), and the supposed "internationalisation" of science and technology (section 4). I then argue that economists can make a useful contribution to better understanding of the properties of basic research-performing institutions, and that there is no convincing empirical evidence of unexploited economies of scale in basic research (section 5). I conclude in section 6 that policies for greater selectivity and concentration have been misconceived, and that we need better theorising on the nature of science and technology, and on why basic science deserves public subsidy.

2. Some contributions by economists

Economic analysis has made two major contributions to the policy debate about the management of science. It has shown, first, that the growth of basic science must be understood and justified mainly by its contribution to economic and social progress; and, second, that basic science should be supported mainly through public subsidy.

2.1. Science as an economic activity

The importance of science as an economic activity was in fact recognised very early on. In Chapter One of *The Wealth of Nations*, Adam Smith pointed out that technical advances were made not only at the point of production, but also by suppliers of capital goods, and by "philosophers and men of speculation", which is what scientists were then called. Perhaps less well known in the UK are the predictions of Alexis de Tocqueville in *Democracy in America*. He observed the down-to-earth and problem-solving nature of US society early in the 19th century, and predicted the rapid expansion of science, for three reasons: first, as a form of conspicuous intellectual consumption, funded from the great accumulations of private wealth that de Tocqueville rightly predicted the US system would produce; second, as a foundation for the education of the large number of applied scientists that de Tocqueville predicted (again rightly) that modernising society would require; and third, as a source of fundamental knowledge needed to facilitate and guide the solving of practical problems.

Thus, the rapid growth of modern science must be seen as part of a more general process of the specialisation and professionalisation of productive activities in modernising societies. To this, we must add Marx's important insights into the major influence of economic and social demands on the rate and direction of scientific advance, through the problems that they pose, the empirical data and techniques of measurement that they generate, and the financial resources that they make available (see Rosenberg [43]). For all these reasons, economists are right to argue that large expenditures on science can be neither understood nor justified solely on cultural and aesthetic grounds; they inevitably have important economic and social dimensions. However, we shall see in section 3 below that the links today between science and technological practice are far from straightforward.

2.2. The public subsidy of science

In the meantime, another major contribution of economics to the management of science has been the analytical justification for regular and large-scale government funding of basic research. As is often the case, principle followed practice rather than led to it, since governments in some countries (and most notably Germany) had already been funding basic research for a very long time. After World War II, the USA followed suit, and in the early 1950s established the National Science Foundation. In 1959, Nelson published his pioneering paper entitled "The Simple Economics of Basic Research" [33], in which he argued that – left to itself – a competitive market will invest less than the optimum in basic research. This is because a profit-seeking firm cannot not be sure of capturing all the benefits of the basic science that it sponsors, given major uncertainties about the benefits for the sponsoring firm, and the difficulties it faces in extracting compensation from subsequent imitators. At the same time, a policy of secrecy aimed at stopping such imitation would be sub-optimal, since it would restrict applications with small marginal cost. If, in addition, profit-seeking firms are risk-averse, or have short-term horizons in their decisions to allocate resources, private expenditures on basic research will be even more sub-optimal.

Nelson's insights have been developed and modified over the past thirty years, notably by Arrow [1] and Averch [2] in the USA, and by Kay and Llewellyn Smith [22], Dasgupta [6], Stoneman [47] and – most recently – Stoneman and Vickers [48] in the UK. Risk aversion, low or zero marginal cost of application, and difficulties in appropriating benefits, have become standard explanations for the public subsidy of science. At the same time there has been a subtle shift in emphasis. Nelson's original paper was grounded in research on the development of the transistor [34], and his paper is spliced with examples of the development and application of science. Over time, progressively fewer references have been made to the empirical evidence, and more to the standard theorems of welfare economics. Whilst it might be advantageous in the economics classroom to stress the "public good" characteristics of science, and to minimise or ignore the distinctions and interactions between science and technology, this has effectively excluded economists from two of the major debates of contemporary science policy: the nature and extent of the contributions of science to technology, and the impact of national science on national technology [27].

2.3. Technology as science?

It is comfortable as well as convenient to treat science and technology as the same thing, given the similarities in their inputs (scientists, engineers, laboratories) and their outputs (knowledge), and given the well-known examples of outstanding science performed in corporate laboratories. However, this neglects the very different nature and purpose of the core activities of university and business laboratories. In universities, basic research seeks generalisations based on a restricted number of variables, and results in publications and reproducible experiments. In business, a combination of research, and (more important) development, testing, production engineering and operating experience accumulates knowledge on the many critical operating variables of an artefact, and result in knowledge that is not only specific, but partly tacit (uncodifiable) and therefore difficult and costly to reproduce.

Given these differences, basic research is more likely to meet the conditions for private under-investment, as defined by Nelson and others, which explains the higher proportion of public funding in basic research than in development in all OECD countries. Economists conscious of the distinction between science and technology have made a major contribution to the policy debate by stressing the complementary nature of private and public investments in science and technology, with the former concentrating on the short-term and specific, and the latter on the long-term and the general. They have also warned of the dangers and inefficiencies of heavy public funding of commercial development activities [9,21]. However, insufficient attention has in general been directed by economists to the interface between science and technology.

2.4. Science as a "free good"?

One other reason for this lack of attention has been a common confusion between the reasonable assumption that the results of science are a "public good" (i.e. codified, published, easily reproduced and therefore deserving of public subsidy), and the unreasonable assumption that they are a "free good" (i.e. costless to apply as a technology, once read). In a paper entitled "Why do Firms do Basic Research (with Their Own Money)", Rosenberg [45] argues that basic research financed and performed in (mainly large) firms often grows out of practical problem-solving, and that the two are highly interactive. He also argues that in-house basic research is essential in order to monitor and evaluate research being conducted elsewhere:

> "This point is important...in identifying a serious limitation in the way economists reason about scientific knowledge and research in general.... .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 the appraise knowledge that has been placed on 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 plugged in to the scientific network is to be a participant in the research process."

This has implications for the way we view the impact of science on technology, and for the reasons for public subsidy. We shall take them up in sections 3 to 6 below.

3. The impact of science on technology

The impact of science on technology is bound to be of central concern to science policy-makers. I summarise below what we already know from earlier studies, and identify subjects for future research.

3.1. Calculating the economic return from basic science?

Resource-starved basic scientists no doubt welcome studies demonstrating a high economic return to basic research. On such study has just been completed in the USA, by a distinguished economic expert on R&D – E. Mansfield [25]. It is one of the most ingenious and persuasive of its kind but, as pointed out by David and his colleagues [7], calculations of this kind do not satisfactorily reflect the nature of the impact of science and technology:

> "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 innovations,... Policies that focus exclusively on the support of basic research with an eye to its economic payoffs will be ineffective unless they are also concerned with these complementary factors.
>
> The alternative conceptualization...that we have developed 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. The development of links between the basic and applied research enterprises are critical to the productivity and economic payoffs of both activities" (pp. 68–69).

3.2. The complexity of science's impact on technology

We know from the results of past research that these links between basic science and technology are in fact complex along at least four dimensions [49].

(i) The intensity of direct transfers of knowledge from basic science to application varies widely amongst sectors of economic activity, and amongst scientific field. The most systematic analyses have been made in the USA, on the basis of patent citations to journals [4,30], and of a survey of industrial R&D directors [36]. They both confirm strong links in chemicals and drugs firms to basic research in biology, whilst the links of electronics firms are also intense but to more applied research activities in physics. In mechanical and transport technologies, on the other hand, the links to science are weak.

(ii) The nature of the impact of basic research on technology also varies widely from the generation of epoch-making new technologies (e.g. electricity, synthetic materials, semi-conductors; see Freeman et al. [13]), through accumulated improvements in continuous flow industries resulting from routine chemical analysis [44], to insights and methods for dealing with applied problems. In all cases, operationally viable technology requires combinations with knowledge from other sources, including design and production engineering.

(iii) Basic science has an impact on technology not just through direct knowledge transfers, but also through access to skills, methods and instruments [40].

(iv) Knowledge transfers are mainly person-embodied, involving personal contacts, movements, and participation in national and international networks [14].

3.3. Is basic research a growing source of technology?

Some analysts (for example, Martin and Irvine [26]) claim that we are now witnessing a significant increase in the direct use in technology of the results of basic research. Others claim that such "strategic" areas of science should receive priority support from government. In my view, the evidence is ambiguous and incomplete (see also, Williams [50,51]).

Narin and Frame [31] have produced the most persuasive quantitative evidence so far. They have shown sharply upward trends in the frequency with which US patents, originating in a number of countries, contain citations to publications other than patents: from about 0.2 cites to "other publications" per US patent in 1975, to between 0.9 cites for US patents of US origin – and 0.4 cites for US patents of Japanese origin – in 1986. On this basis they claim that the technology reflected in US patents is much more "science-dependent" than ten years ago. They further show that the time-lags in the citations from patents to other publications are diminishing rapidly, and that science-intensive patents are relatively highly cited.

Whilst suggestive, this evidence has its limitations. It is not yet clear to what extent the "other publications", cited in patents, reproduce basic or applied research, from universities or from corporate laboratories. In addition, a high proportion of technology is not patented, because it is kept secret (e.g. process technology), because it is tacit and non-codifiable know-how, or because – as in the increasingly important case of software technology – it is very difficult to protect through patenting. This non-patented technology is likely to be less dependant on science, and more on cumulative design and engineering skills. Together with a number of colleagues, I have argued elsewhere that it is increasing as a proportion of total technological activity [46].

In addition, it is worth noting that, in the USA, the recent report from MIT *Made in America* [8] has claimed that it is precisely because of deficiencies in these engineering skills that US firms are not capturing the full economic benefits from exploiting scientific advances. They further claim that engineering education in the USA has become too science-based.

More generally the evidence from US R&D statistics are ambiguous. Whilst there are signs of increasing corporate commitment to basic research in the 1980s, this follows an extended period of decline, and it has only just regained its share of the early 1960s (National Science Board [32], Appendix Tables 1–40 and 5–1). According to Mowery [28], the increasingly generous provision of funds for academic research by the Federal Government after World War II had led to a reduction in the direct funding by business firms, and:

> "...(b)oth the recent upsurge in state funding of applied research and the proliferation of collaborative research relationship between universities and industry thus represent a partial revival of earlier relationship that were sundered by the dramatic changes in the structure of the

U.S. national research system during and after World War II" (pp. 23–24).

Even if certain fields of basic research make increasingly important direct knowledge inputs into technology, it is misleading to assume that only they contribute to technology, and other fields do not. There are at least two other influences of science and technology that are equally, if not more, important: research training and skills; and unplanned applications.

3.4. *The broad demands for research skills*

One important function of academic research is the provision of trained research personnel, who go on to work in applied activities and take with them not just the knowledge resulting from their research, but also skills, methods, and a web of professional contacts that will help them tackle the technological problems that they later face.

In one of their less well-known studies, Irvine and Martin [19] have shown that Masters and Doctoral graduates from British radio-astronomy benefited in subsequent non-academic careers from the research skills – rather than the research knowledge – that they obtained during their post-graduate training. A more comprehensive survey undertaken at Yale University suggests that this is the rule rather than the exception.

The relevant results are summarised in table 1. They show the responses of 650 US industrial research executives, spread across 130 industries, who were asked to rank the relevance to their technology of a number of fields of pure and applied science. Table 1 lists the number of industries in which each scientific field was given high ranking according to two criteria: first, the relevance of the skill base in the science to the technology; second, the relevance of the academic research knowledge to the technology. As the authors point out:

> "Industrial scientists and engineers almost always need training in the basic scientific principles and research techniques of their field, and providing this training is a central function of universities. Current academic research in a field, however, may or may not be relevant to technical advance in industry, even if academic training is important" (Nelson and Levin [36]).

Table 1 shows that, in most scientific fields, whether pure or applied, academic training and skills are relevant over a far larger number of industrial technologies than is academic research. The expectations are the pure and applied biological sciences where we know from other studies that academic research is at present very close to technology [30]. These results show clearly that most scientific fields are much more strategically important to technology than data on direct transfers of knowledge would lead us to believe.

Table 1
The relevance of scientific fields to technology (USA)

Field of science	Number of industries (out of 130) ranking scientific field at 5 or above (out of 7) in relevance to its technology of:	
	Science (i.e. skills)	Academic research (i.e. knowledge)
Biology	14	12
Chemistry	74	19
Geology	4	0
Mathematics	30	5
Physics	44	4
Agricultural Science	16	17
Applied Maths & OR	32	16
Computer Science	79	34
Materials Science	99	29
Medical Science	8	7
Metallurgy	60	21

Source: Nelson and Levin [36]; Nelson [35].

3.5. *Unplanned applications*

Another major influence of science on technology is through unplanned applications, where useful knowledge emerges from research undertaken purely out of curiosity, without any strategic mission or expectation of application. Two US studies – one undertaken in the late 1960s and the other some 20 years later – both show the importance of such research for achieving relatively short-term technological objectives [18,25]. In both cases, important innovations would have been substantially delayed without contributions from unprogrammed research performed in the ten years preceding the commercial launch of the innovations. Furthermore, in both studies, unprogrammed research contributed about 10 percent of the important knowledge inputs.

One implication of these findings is that programmed R&D should be built on a wider spread of non-programmed research. Analysts like Nelson [33], and Kay and Llewellyn Smith [22], have gone further and used various examples to suggest that more useful knowledge is produced in the long term by allowing basic scientists to pursue their own interests, than by fixing practical objectives for their work. It is a view that needs to be considered seriously by analysts in future (see, for example, Council for Science and Society [5]).

4. Is the application of science (at last) being internationalised?

The analytical apparatus developed by economists to justify public subsidy to basic research has in general assumed a closed economy. This is paradoxical given that the main stimulus for public policies for science and technology have not come from any notions of (national) market failure, but from what is perceived as best practice in a world system of international competition where technological leads and lags are of central importance. It is also perhaps fortunate that the subject has not been pursued too often within the mainstream analytical framework: if we assume that basic research is a "free good", an open international world would in principle permit any one country to live off the rest of the world's basic research [22].

But the real world is more complicated. As Rosenberg has pointed out, the ability to assimilate the results of other people's basic research depends in part on the performance of basic research, oneself. An active national competence in basic research is therefore a necessary condition for benefiting from research undertaken elsewhere in the world; indeed it can be viewed as a national scientific intelligence system. And since most transfers of knowledge and skills between science and technology are person-embodied, the constraints of distance and language have meant that nation-based transfers between science and technology have been the rule rather than the exception.

Now, it is argued, conditions are changing. The barriers of distance and language are lower than they used to be. And firms are increasingly internationalising their R&D activities, which enable them more easily to benefit from academic science in foreign countries, through personal contacts and the hiring of scientists and engineers with research experience from local systems of higher education. Does this mean that linkages between science and technology will become internationalised? Does it mean that an increasing proportion of the benefits of national governments' investments in basic research will "leak away" through foreign-controlled firms to other countries?

This is a subject that deserves further research. Suffice to suggest at this stage that the degree of "leakage" depends, as a first approximation, on the proportion of a country's corporate technological activity that is controlled by foreign firms, which reflects their capacity to monitor and absorb local basic research skills and knowledge. Similarly, the importance of the foreign technological activities of nationally owned firms will reflect a country's capacity to benefit from basic research undertaken in other countries.

Table 2 is a first attempt to measure and compare these variables across countries. The first column compares the proportion of each country's US patenting originating from foreign-controlled firms. It shows that, in most countries, large

Table 2
Foreign controlled domestic technology compared to nationally controlled foreign technology (based on US patenting, 1981–86)

Home country	US patenting from inside country by foreign firms (as % of country's total US patenting)	US patenting by national firms from outside home country (as % of country's total US patenting)
Belgium	45.7	16.5
France	11.8	3.8
FR Germany	11.5	8.5
Italy	11.2	3.0
Netherlands	9.5	73.4
Sweden	5.4	16.7
Switzerland	12.5	27.8
UK	22.3	24.5
W. Europe	7.4	9.3
Canada	28.1	12.5
Japan	1.2	0.5
USA	4.2	4.4

Source: Patel and Pavitt [39].

foreign firms still play a relatively small role in national technological activities; only in Belgium, Canada and the UK do they account for more than 20 percent of the total. The second column compares the US patenting of nationally-controlled firms from outside their home country, as a proportion of total national patenting in the USA. For the Netherlands, this amounts to more than 70 percent of the national total, and more than 20 percent for Switzerland and the UK.

Taken together, the two measures show that most national technological systems are relatively self-contained. Both measures of internationalisation are less than a quarter of total technological activities in eight out of the 11 countries. In Belgium and Canada, foreign-controlled domestic technological activities are much greater than domestically controlled foreign technological activities, whereas for the Netherlands, Sweden and Switzerland the opposite is the case. When Western Europe is considered as a whole, the degree of internationalisation is much less than for most of the European countries taken individually, but still greater than that of either Japan or the USA.

These results show that complete internationalisation of links between science and technology is not at all likely in the immediate future. In most countries, national science will still be feeding into largely nationally controlled technology, and close links with foreign science through personal contacts and recruitment will in most cases be small compared to national links. Contrary to conventional wisdom, Japan is not well positioned to benefit from foreign countries' basic science, since their firms undertake such a small proportion of their technological activities outside Japan. Our data suggest that the Dutch are much better at it.

5. The properties of basic research-producing institutions

In addition to the links between science and application, we need a better understanding of the properties of basic research-producing institutions, particularly universities and university departments. Public policy in the UK (and perhaps in other countries) increasingly assumes that there are advantages to greater scale and concentration in basic research activities, although there is no systematic evidence that this is the case.

In this context, policy would be better informed as the result of a research programme that combines recent advances in bibliometric methods, with accumulated experience in industrial economics in understanding the links between technological activities, firm size and industrial concentration. There would no doubt be similar room for debate over the adequacy of the various measures used. But similarly useful results would probably emerge, showing considerable variations amongst scientific fields in concentration and economies of scale.

5.1. What are the unexploited economies of scale in basic research?

Partly as what they would consider as legitimate acts of academic self-defence, British scholars have been among the first to identify the problems to be clarified. As Hare and Wyatt [16] have recently pointed out, little systematic evidence is available on economies of scale in basic research. In the USA, Frame and Narin [12] found no economies of scale in biomedical research, when output was measured by numbers of publications. In the UK, Hicks and Skea [17] have come to similar conclusions to Frame and Narin in a preliminary analysis of 45 physics departments in Britain: no unexploited economies of scale, when output is measured in numbers of publications. Williams [51] came to the same conclusion in an unpublished study of chemistry departments.

This type of analysis should be extended to other scientific fields, and down into sub-fields. The sensitivity of results to various measures of inputs and outputs should also be tested: for example, McAllister and Narin [24] found no economies of scale in US biomedical research in terms of the number of publications, but they did find higher citation rates amongst the larger institutions. This might reflect the greater quality of large institutions' basic research, but it might also reflect their greater visibility.

5.2. How do basic research institutions evolve?

Just as in the analysis of firms' technological activities, cross-sectional comparisons of size, concentration and efficiency, while useful, will also raise further important questions for theory and policy: in particular, how and why do the existing patterns come about? This leads on to four further questions, each of which is also central to the analysis of the dynamics of technical change, concentration and efficiency in industry:

- are large and productive research institutions good because they are big, or big because they are good?
- what are the characteristics of productive institutions? To what extent do they grow out of accumulated scientific and managerial skills?
- what is the appropriate organisational unit in which such skills are accumulated? Preliminary analysis by Platt [41] suggests that it is not at the level of a university as a whole, but (if at all) in closely related subjects;
- what are the mechanisms through which good research practice and productivity are diffused (or not) throughout the research community?

6. Conclusions

6.1. Gaps in empirical knowledge

Conclusions for policy are bound to be tentative, given the still shaky theoretical and empirical base, which is why I have signalled throughout the paper where further research is required. The three most important subjects (in my view) are:

- the economic and social benefits of "unstrategic" science, particularly the development of useful research skills and networks, and unplanned applications;
- the nature and effects of the internationalisation of scientific and technological activities;
- the structure, efficiency and dynamics of national systems of basic research.

6.2. Misguided policies seeking "relevance"

In the meantime, our analysis suggests that the objectives of many policies seeking to make basic research more useful may turn out to have been badly misconceived. Policies of high priority for basic research that are directly and obviously applicable ignore the considerable indirect benefits across a broad range of scientific fields resulting from training and from unplanned discoveries. Policies for concentration in larger units in basic research are based on the unproven premise that big is necessarily beautiful (i.e. efficient). Both policies neglect the all-important fact that the application of basic research depends overwhelmingly on the size and persistence in investment in downstream activities by business firms. Dealing with deficiencies in business R&D by making basic research more "relevant" is like pushing a piece of string.

6.3. A revised case for public subsidy for basic research

Our analysis suggests that the justification for public subsidy, in terms of complete inappropriability of immediately applicable knowledge, is a weak one. In fact, the results of basic research are rarely immediately applicable, and making them so also increases their appropriability, since – in seeking potential applications – firms learn how to combine the results of basic research with other firm-specific assets, and this cannot be imitated overnight. In three other dimensions, the case for public subsidy is stronger.

The first justification was originally stressed strongly by Nelson [33], but has been neglected since then: namely, the considerable uncertainties before the event in knowing if, when and where the results of basic research might be applied. We now know from transaction cost theory that high uncertainty is one reason why markets are not necessarily efficient [52]. The probabilities of application will be greater with an open and flexible interface between basic research and application, which implies public subsidy for the former. The case for such a subsidy is strongest for "unstrategic" fields of curiosity driven research, the application of which cannot be foreseen.

A second, and potentially new, justification grows out of internationalisation of the technological activities of large firms, discussed in section 4. Facilities for basic research and training can be considered as an increasingly important part of the infrastructure for downstream technological and production activities. Countries may therefore decide to subsidise them, in order to attract foreign firms or even to retain national ones. Recent interest in so-called "science parks" might sometimes be one manifestation of this trend. Clearly there are dangers of competitive subsidy, the implications of which should keep game and trade theorists busy for some time.

The final and most important justification for public subsidy is training in research skills, since private firms cannot fully benefit from providing it when researchers, once trained, can and do move elsewhere. There is, in addition, the important insight of Dasgupta [6] that, since the results of basic research are public and those of applied research and development often are not, training through basic research enables more informed choices and recruitment into the technological research community.

6.4. Better conceptualisations of science and technology

This last justification illustrates a broader conclusion emerging from this paper (see also Mowery and Rosenberg [29]): economists and other social scientists will benefit enormously in both the accuracy and impact of their analyses, if they drop their conceptualisations of science and technology as activities producing easily transmissible and applicable "information", and recognise them instead as search processes and skills embodied in individuals and institutions. In this context, they would more easily appreciate the importance of basic research as both training and a cumulative body of knowledge. As we have seen, this was clear to de Tocqueville a long time ago. It was also clear to one of the major figures in the development of modern policies for basic science, Vannevar Bush, who pointed out in 1945 that "(t)he responsibility for the creation of new scientific knowledge – and for most of its application – rests on that small body of men and women who understand the fundamental laws of nature and are skilled in the techniques of scientific research" [3, p. 7].

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