The relationship between science and technology

Harvey Brooks

John F. Kennedy School of Government, Harvard University, 79 J.F.K. Street, Cambridge, MA 02138, USA

Science, technology and innovation each represent a successively larger category of activities which are highly interdependent but distinct. Science contributes to technology in at least six ways: (1) new knowledge which serves as a direct source of ideas for new technological possibilities; (2) source of tools and techniques for more efficient engineering design and a knowledge base for evaluation of feasibility of designs; (3) research instrumentation, laboratory techniques and analytical methods used in research that eventually find their way into design or industrial practices, often through intermediate disciplines; (4) practice of research as a source for development and assimilation of new human skills and capabilities eventually useful for technology; (5) creation of a knowledge base that becomes increasingly important in the assessment of technology in terms of its wider social and environmental impacts; (6) knowledge base that enables more efficient strategies of applied research, development, and refinement of new technologies.

The converse impact of technology on science is of at least equal importance: (1) through providing a fertile source of novel scientific questions and thereby also helping to justify the allocation of resources needed to address these questions in an efficient and timely manner, extending the agenda of science; (2) as a source of otherwise unavailable instrumentation and techniques needed to address novel and more difficult scientific questions more efficiently.

Specific examples of each of these two-way interactions are discussed. Because of many indirect as well as direct connections between science and technology, the research portfolio of potential social benefit is much broader and more diverse than would be suggested by looking only at the direct connections between science and technology.

Correspondence to: H. Brooks, John F. Kennedy School of Government, Harvard University, 79 J.F.K. Street, Cambridge, MA 02138, USA. Tel., (617) 495-1445; fax, (617) 495-5776.

Research Policy 23 (1994) 477–486
North-Holland

SSDI 0048-7333(94)01001-S

1. Introduction

Much public debate about science and technology policy has been implicitly dominated by a 'pipeline' model of the innovation process in which new technological ideas emerge as a result of new discoveries in science and move through a progression from applied research, design, manufacturing and, finally, commercialization and marketing. This model seemed to correspond with some of the most visible success stories of World War II, such as the atomic bomb, radar, and the proximity fuze, and appeared to be further exemplified by developments such as the transistor, the laser, the computer, and, most recently, the nascent biotechnology industry arising out of the discovery of recombinant DNA techniques. The model was also, perhaps inadvertently, legitimated by the influential Bush report, *Science, the Endless Frontier*, which over time came to be interpreted as saying that if the nation supported scientists to carry out research according to their own sense of what was important and interesting, technologies useful to health, national security, and the economy would follow almost automatically once the potential opportunities opened up by new scientific discoveries became widely known to the military, the health professions, and the private entrepreneurs operating in the national economy. (See United States Office of Scientific Research and Development (1945) for a recent account of the political context and general intellectual climate in which this report originated; see also Frederickson, 1993.) The body of research knowledge was thought of as a kind of intellectual bank account on which society as a whole would be able to draw almost automatically as required to fulfil its aspirations and needs.

Though most knowledgeable people understood that such a model corresponded only to the rare and exceptional cases cited above, it became embodied in political rhetoric and took considerable hold on the public imagination and seemed to be confirmed by a sufficient number of dramatic anecdotes so that it was regarded as typical of the entire process of technological innovation, though it was severely criticized by many scholars. (See Kline and Rosenberg (1986) for an example of criticism and an excellent discussion of a more realistic and typical model.) One consequence was considerable confusion in the public mind between science and engineering, an excessive preoccupation with technical originality and priority of conception as not only necessary but sufficient conditions for successful technological innovation, and in fact an equating of organized research and development (R&D) with the innovation process itself. The ratio of national R&D expenditures to gross domestic product (GDP) often became a surrogate measure of national technological performance and, ultimately, of long-term national economic potential. The content of R&D was treated as a 'black box' that yielded benefits almost independently of what was inside it (Brooks, 1993, pp. 30–31).

The public may be forgiven its confusions, as indeed the relationships between science and technology are very complex, though interactive, and are often different in different fields and at different phases of a technological 'life cycle'. Nelson (1992) has given a definition of technology both as "... specific designs and practices" and as "generic knowledge... that provides understanding of how [and why] things work..." and what are the most promising approaches to further advances, including "... the nature of currently binding constraints." It is important here to note that technology is not just things, but also embodies a degree of generic understanding, which makes it seem more like science, and yet it is understanding that relates to a specific artifact, which distinguishes it from normal scientific understanding, although there may be a close correspondence.

Similarly, Nelson (1992, p. 349) defines innovation as "... the processes by which firms master and get into practice product designs that are new to them, whether or not they are new to the universe, or even to the nation." The current US mental model of innovation often places excessive emphasis on originality in the sense of newness to the universe as opposed to newness in context. In general, the activities and investments associated with 'technological leadership' in the sense of absolute originality differ much less than is generally assumed from those associated with simply staying near the forefront of best national or world practice. Yet R&D is also necessary for learning about technology even when it is not 'new to the universe' but only in the particular context in which it is being used for the first time (Brooks, 1991, pp. 20–25).

However, innovation involves much more than R&D. Charpie (1967) has provided a representative allocation of effort that goes into the introduction of a new product, as follows:

(a) conception, primarily knowledge generation (research, advanced development, basic invention) 5–10%;

(b) product design and engineering, 10–20%;

(c) getting ready for manufacturing (lay-out, tooling, process design), 40–60%;

(d) manufacturing start-up, debugging production, 5–15%;

(e) marketing start-up, probing the market, 10–20%.

It does not follow from this that R&D or knowledge generation is only 5–10% of total innovative activity because many projects are started that never get beyond stage (a) and an even smaller proportion of projects are carried all the way through stage (e). In addition, there is a certain amount of background research that is carried out on a level-of-effort basis without any specific product in mind. There is no very good estimate of what percentage of the innovative activity of a particular firm would be classified in category (a) if unsuccessful projects or background research are taken into account. The fact remains that all five stages involve a certain proportion of technical work which is not classified as R&D, and the collection of statistical data on this portion of 'downstream' innovative activity is in a very rudimentary state compared with that for organized R&D. Indeed, only about 35% of scientists and engineers in the US are employed in R&D.

In small firms, especially technological 'niche' firms whose business is based on a cluster of specialized technologies which are often designed in close collaboration with potential users, there is a good deal of technical activity by highly trained people which is never captured in the usual R&D statistics.

Thus, science, technology, and innovation each represent a successively larger universe of activities which are highly interdependent, yet nevertheless distinct from each other. Even success in technology by itself, let alone science, provides an insufficient basis for success in the whole process of technological innovation. In fact, the relation between science and technology is better thought of in terms of two parallel streams of cumulative knowledge, which have many interdependencies and cross relations, but whose internal connections are much stronger than their cross connections. The metaphor I like to use is two strands of DNA which can exist independently, but cannot be truly functional until they are paired.

2. The contributions of science to technology

The relations between science and technology are complex and vary considerably with the particular field of technology being discussed. For mechanical technology, for example, the contribution of science to technology is relatively weak, and it is often possible to make rather important inventions without a deep knowledge of the underlying science. By contrast, electrical, chemical, and nuclear technology are deeply dependent on science, and most inventions are made only by people with considerable training in science. In the following discussion, we outline the variety of ways in which science can contribute to technological development. The complexity of the interconnections of science and technology is further discussed in Nelson and Rosenberg (1993).

2.1. Science as a direct source of new technological ideas

In this case, opportunities for meeting new social needs or previously identified social needs in new ways are conceived as a direct sequel to a scientific discovery made in the course of an exploration of natural phenomena undertaken with no potential application in mind. The discovery of uranium fission leading to the concept of a nuclear chain reaction and the atomic bomb and nuclear power is, perhaps, the cleanest example of this. Other examples include the laser and its numerous embodiments and applications, the discoveries of X-rays and of artificial radioactivity and their subsequent applications in medicine and industry, the discovery of nuclear magnetic resonance (NMR) and its subsequent manifold applications in chemical analysis, biomedical research, and ultimately medical diagnosis, and maser amplifiers and their applications in radioastronomy and communications. These do exemplify most of the features of the pipeline model of innovation described above. Yet, they are the rarest, but therefore also the most dramatic cases, which may account for the persistence of the pipeline model of public discussions. It also suits the purpose of basic scientists arguing for government support of their research in a pragmatically oriented culture.

A more common example of a direct genetic relationship between science and technology occurs when the exploration of a new field of science is deliberately undertaken with a general anticipation that it has a high likelihood of leading to useful applications, though there is no specific end-product in mind. The work at Bell Telephone Laboratories and elsewhere which led eventually to the invention of the transistor is one of the clearest examples of this. The group that was set up at Bell Labs to explore the physics of Group IV semiconductors such as germanium was clearly motivated by the hope of finding a method of making a solid state amplifier to substitute for the use of vacuum tubes in repeaters for the transmission of telephone signals over long distances.

As indicated above, much so-called basic research undertaken by industry or supported by the military services has been undertaken with this kind of non-specific potential applicability in mind, and indeed much basic biomedical research is of this character. The selection of fields for emphasis is a 'strategic' decision, while the actual day-to-day 'tactics' of the research are delegated to the 'bench scientists'. Broad industrial and government support for condensed matter physics and atomic and molecular physics since World War II has been motivated by the well-substantiated expectation that it would lead to important new applications in electronics, communications, and computers. The determination of an appropriate level of effort, and the creation of an organizational environment that will facilitate the earliest possible identification of technological opportunities without too much constraint on the research agenda is a continuing challenge to research planning in respect to this particular mechanism of science–technology interaction.

2.2. Science as a source of engineering design tools and techniques

While the process of design is quite distinct from the process of developing new knowledge of natural phenomena, the two processes are very intimately related. This relationship has become more and more important as the cost of empirically testing and evaluating complex prototype technological systems has mounted. Theoretical prediction, modeling, and simulation of large systems, often accompanied by measurement and empirical testing of subsystems and components, has increasingly substituted for full scale empirical testing of complete systems, and this requires design tools and analytical methods grounded in phenomenological understanding. This is particularly important for anticipating failure modes under extreme but conceivable conditions of service of complex technological systems. (See Alic et al., 1992, Chapter 4). For a discussion of technical knowledge underlying the engineering design process, cf. Chapter 2 (pp. 39–34).)

Much of the technical knowledge used in design and the comparative analytical evaluation of alternative designs is actually developed as 'engineering science' by engineers, and is in fact the major activity comprising engineering research in academic engineering departments. This research is very much in the style of other basic research in the 'pure' sciences and is supported in a similar manner by the Engineering Division of the National Science Foundation, i.e. as unsolicited, investigator-originated project research. Even though it is generally labelled as 'engineering' rather than 'science', such research is really another example of basic research whose agenda happens to be motivated primarily by potential applications in design 'downstream' though its theoretical interest and its mathematical sophistication are comparable with that of pure science.

3.3. Instrumentation, laboratory techniques, and analytical methods

Laboratory techniques or analytical methods used in basic research, particularly in physics, often find their way either directly, or indirectly via other disciplines, into industrial processes and process controls largely unrelated either to their original use or to the concepts and results of the research for which they were originally devised (Rosenberg, 1991). According to Rosenberg (1991), "this involves the movement of new instrumentation technologies...from the status of a tool of basic research, often in universities, to the status of a production tool, or capital good, in private industry." Examples are legion and include electron diffraction, the scanning electron microscope (SEM), ion implantation, synchrotron radiation sources, phase-shifted lithography, high vacuum technology, industrial cryogenics, superconducting magnets (originally developed for cloud chamber observations in particle physics, then commercialized for 'magnetic resonance imaging' (MRI) in medicine). In Rosenberg's words, "the common denominator running through and connecting all these experiences is that instrumentation that was developed in the pursuit of scientific knowledge eventually had direct applications as part of a manufacturing process." Also, in considering the potential economic benefits of science, as Rosenberg says, "there is no obvious reason for failing to examine the hardware consequences of even the most fundamental scientific research." One can also envision ultimate industrial process applications from many other techniques now restricted to the research laboratory. One example might be techniques for creating selective chemical reactions using molecular beams.

2.4. The development of human skills

An important function of academic research often neglected in estimating its economic benefits is that it imparts research skills to graduate students and other advanced trainees, many of whom "go on to work in applied activities and take with them not just the knowledge resulting from their research, but also the skills, methods, and a web of professional contacts that will help them tackle the technological problems that they later face." (See Rosenberg (1990) and Pavitt (1991).) This is especially important in light of the fact that basic research instrumentation so often later finds application not only in engineering and other more applied disciplines such as clinical medicine, but also ultimately in routine industrial processes and operations, health care delivery, and environmental monitoring.

A study based on a ranking by 650 industrial research executives in 130 industries of the relevance of a number of academic scientific disciplines to technology in their industry, first, on the basis of their skill base and, second, on the basis of their research results, showed strikingly higher ratings for the skill base from most disciplines than from the actual research results. In the most extreme case, 44 industries rated physics high in skill base (second only to materials science, computer science, metallurgy and chemistry, in that order), whereas physics was almost at the bottom of the list in respect to the direct contribution of academic research results to industrial applications. Only in biology and medical science were the contributions of skill base and research results comparable (Nelson and Levin, 1986; Pavitt, 1991, p. 114 (Table 1)). The conclusion was "that most scientific fields are much more strategically important to technology than data on direct transfers of knowledge would lead us to believe" (Pavitt, 1991). From these data, Pavitt inferred that "policies for greater selectivity and concentration in the support of scientific fields have probably been misconceived", for the contribution of various disciplines to the development of potentially useful skills appears to be much more broadly distributed among fields than are their practically relevant research contributions. A part of the problem here is, of course, that this conclusion is contrary to much of the rhetoric used in advocating the support of basic research by governments.

As a further example of the importance of the widely usable generalized skills derived from participation in any challenging field of research, the National Research Council in 1964 surveyed about 1900 doctoral scientists working in industry in solid state physics and electronics. By that date, most of the basic ideas underlying the most important advances in solid state electronics had already been developed. It was found, however, that only 2.5% of the scientists surveyed had received their Ph.D. training in solid state physics; 19% were chemists, and 73% had received their doctorates in physics fields other than solid state, with nuclear physics predominating (Brooks, 1985). In fact, the shift of physics graduate study into solid state and condensed matter physics (about 40% of all physics Ph.D.s by the early 1970s) occurred after many of the fundamental inventions had already been made. The skills acquired in graduate training in nuclear physics had been readily turned to the development and improvement of solid state devices (Brooks, 1978).

2.5. Technology assessment

The past two decades have witnessed an enormous growth of interest and concern with predicting and controlling the social impact of technology, both anticipating new technologies and their social and environment implications and the consequences of ever-increasing scale of application of older technologies (Brooks, 1973). In general, the assessment of technology, whether for evaluating its feasibility to assess entrepreneurial risk, or for foreseeing its societal side-effects, requires a deeper and more fundamental scientific understanding of the basis of the technology than does its original creation, which can often be carried out by empirical trial-and-error methods. Further, such understanding often requires basic scientific knowledge well outside the scope of what was clearly relevant in the development of the technology. For example, the manufacture of a new chemical may involve disposal of wastes which require knowledge of the groundwater hydrology of the manufacturing site. Thus, as the deployment of technology becomes more extensive, and the technology itself becomes more complex, one may anticipate the need for much more basic research knowledge relative to the technical knowledge required for original development. This has sometimes been called 'defensive research' and, it can be shown that, over time, the volume of research that can be described as defensive has steadily increased relative to the research that can be described as 'offensive' – i.e. aimed at turning up new technological opportunities. This has led me to call science the 'conscience' of technology.

2.6. Science as a source of development strategy

Somewhat similarly to the case of technology assessment, the planning of the most efficient strategy of technological development, once general objectives have been set, is often quite dependent on science from many fields. This accumulated stock of existing scientific (and technological) knowledge helps to avoid blind alleys and hence wasteful development expenditures. Much of this is, of course, old knowledge, rather than the latest research results, but it is nonetheless important and requires people who know the field of relevant background science. One piece of evidence of this is the observation that very creative engineers and inventors tend to read very widely and eclectically both in the history of science and technology, and about contemporary scientific developments.

3. Contribution of technology to science

While the contributions of science to technology are widely understood and acknowledged by both the public and scientists and engineers, the reciprocal dependence of science on technology both for its agenda and for many of its tools is much less well appreciated. This dependence is more apparent in the 'chain-link' iterative model of Kline and Rosenberg (1986) than it is in the linear-sequential model more common until recently in public discussions of technological innovation and technology policy. The relationships here are also more subtle and require more explanation.

3.1. Technology as a source of new scientific challenges

Problems arising in industrial development are frequently a rich source of challenging basic science problems which are first picked up with a specific technological problem in mind, but then pursued by a related basic research community well beyond the immediate requirements of the original technological application that motivated them (Rosenberg, 1991). This research then went on to generate new insights and technological ideas from which new and unforeseen technology originated. This process has been especially fruitful in the fields of materials science and condensed matter physics (Materials Advisory Board, 1966). In fact, materials science was created as a new interdisciplinary field of academic research initially as an outgrowth of an effort to understand some of the materials processes and properties that were important to improving the quality and performance of semiconductor devices.

One of the most dramatic examples of the generation of a stimulus to a new field of basic research by a discovery made in the course of a technology-motivated investigation was the discovery and quantitative measurement by a Bell Laboratories group in 1965 of the background microwave radiation in space left over from the original 'big bang', for which Penzias et al. ultimately received the Nobel prize. A brief account of the development of this subfield of cosmology is given in Physics Survey Committee (1972b). Other examples are tunneling in semiconductors (Suits and Bueche, 1967, pp. 304–306), the pursuit of which as a basic science beyond practical needs led ultimately to the discovery of the Josephson effect in superconductors and the invention of the Josephson junction. The development and application of superconducting junctions is briefly summarized in Physics Survey Committee (1972a, pp. 490–492). In this example, it is more difficult to decide whether research was motivated by technology. The Physics Survey Committee (1972b) gives numerous examples of the mutual reinforcement of theoretical and technological stimuli in the co-evolution of a new field of science and technological application, where the triggering events are difficult to disentangle.

Observations "are sometimes made in an industrial context by people who are not capable of appreciating their potential significance" (Rosenberg, 1991, p. 337) or, perhaps more frequently, lack the incentives or resources to pursue, generalize, and interpret the observation, thus lacking the 'prepared mind' which is so essential to fundamental scientific discovery. This may be so simply because the organization is dependent on commercial revenue for support, so it cannot afford to pursue promising concepts unless their potential application is fairly clear and immediate, or it may be because of a mindset that is belittling of mere theory. A classical example is the so-called Edison effect originally discovered by Thomas A. Edison, but not pursued because he was too "preoccupied with matters of short-run utility". To quote Asimov (1974, p.5), "The Edison effect, then, which the practical Edison shrugged off as interesting but useless, turned out to have more astonishing results than any of his practical devices." Indeed, many important observations made incidentally during the course of major industrial or military technological developments may, because of the highly specialized context in which they are made, or because of military or proprietary confidentiality, never get into the general scientific literature, nor get properly documented so that they can be understood and appreciated either by other industrial researchers or basic scientists interested in and capable of pursuing their broader scientific significance (Alic et al., 1992, pp. 390–393).

In addition, of course, technological development indirectly stimulates basic research by attracting new financial resources into research areas shown to have practical implications. This has happened repeatedly for radical inventions such as the transistor, the laser, the computer, and nuclear fission power, where much of the science, even the most basic science, has followed rather than preceded the original conception of an invention. Indeed, the more radical the invention, the more likely it is to stimulate wholly new areas of basic research or to rejuvenate older areas of research that were losing the interest of the most innovative scientists, e.g. classical optics and atomic and molecular spectroscopy in the case of the laser, and basic metallurgy and crystal growth and crystal physics in the case of the transistor, as well as the burgeoning of the new science of "imperfections in almost perfect crystals" (Shockley et al., 1952; Bardeen, 1957).

There are two areas in which the search for radical technological breakthroughs has been unusually important; defense and health care. In each case, the value of improved performance almost regardless of its cost, not only in R&D. but also in ultimate societal performance, has played a fundamental role in stimulating not only technological development but also related fields of basic research. In the defense case, it has been generally believed that even a small technological edge in the performance of individual weapons systems could make all the difference between victory and defeat. In the biomedical case, where much of the focus has been on curative technology, anything which could improve the survival chances of the individual sick patient, compared with the statistical morbidity or mortality of populations, has been accorded highest priority, especially in the US. This has led to industries that are disproportionately R&D intensive with a corresponding emphasis on the science base in related fields in academia and government laboratories. The same motivation has seemed to pervade the environmental field in respect to regulation. However, this has not so far led to a corresponding R&D intensity, although there are some signs that this might be about to change (cf. Wald, 1993).

3.2. Instrumentation and measurement techniques

Technology has played an enormous role in making it possible to measure natural phenomena that were not previously accessible to research. One of the most dramatic recent examples of this, of course, has been the role of space technology in making a much greater range of the electromagnetic spectrum accessible to measurement than was possible when observation was limited by the lack of transparency of the atmosphere to X-rays, γ rays, the far ultraviolet, and some parts of the infra-red. The sciences of cosmology and astrophysics have been revolutionized by the opening up of these new windows. In this particular case, the new capability would probably never have been created for scientific purposes alone, but basic scientists were quick to seize the new opportunities that were made available by the space program.

In other cases, such as nuclear and elementary particle physics, much of the new technology has been developed and engineered by the physicists themselves. In perhaps the majority of cases, laboratory instruments have been originally developed by research scientists, but were later commercialized to be sold to a much broader research community. This latter process has been very important for the rapid diffusion of new experimental techniques and is probably a prime mechanism for knowledge transfer between different disciplines, which in turn has greatly accelerated the progress of science overall. The pattern of interaction has been described in the following terms for the case of the transfer of physics techniques to chemistry, but this pattern is similar for transfer between any two disciplines, or, indeed, for diffusion among researchers and subfields of a single discipline:

> When the method is first discovered, a few chemists, usually physical chemists, become aware of chemical applications of the method, construct their own homemade devices, and demonstrate the utility of the new tool. At some point commercial models of the device are put on the market. These are sometimes superior, sometimes inferior, to the homemade machines in terms of their ultimate capabilities to provide information. However, the commercial instruments generally are easier to use and far more reliable than the homemade devices. The impact of the commercial instruments is rapidly felt, is often very far-reaching, and sometimes virtually revolutionizes the field. Chemists with the new instruments need not be concerned with developing the principle of the device; they are free to devote their efforts to extracting the useful chemical information that application of the device affords. This pattern characterizes the development of optical, infrared and radio frequency spectroscopy, mass spectrometry, and X-ray crystallography. (Physics Survey Committee, 1972a, p. 1015)

The effectiveness of this pattern depends on close collaboration between vendors and scientific users, and between engineers and scientists, so that instruments and laboratory techniques often become a mechanism by which some of the pathologies of overspecialization in science are moderated. The existence of an entrepreneurial scientific instrument industry, closely coupled to research scientist users, and enjoying the economies of scale derived from one of the largest markets of research activity in the world, has been an important, and perhaps underestimated, source of competitive advantage for the US research system in basic science – an advantage which was achieved earlier than in other countries because of the enormous government investments in defense-related R&D in the US compared with other countries during the first two decades following World War II. This instrument industry, combined with other research supply industries, comprised an unexcelled infrastructure, which may have had much broader general utility for commercially oriented innovation than the specific 'spin-offs' from highly specialized defense R&D.

4. The positive externalities of innovative activity

The interest of economists in the economics of research, particularly in the economic rationale for both public and private investment in basic research, is of relatively recent vintage. As pointed out by Pavitt, economists have made an important contribution by being the first to articulate the 'public good' aspects of science and consequently its eligibility for public or collective support. However, as Pavitt also emphasizes, there has been considerable confusion in the resulting public discussion "between the reasonable assumption that the results of science are a public good...and the unreasonable assumption that they are a free good" (Pavitt, 1991). The latter interpretation has led to a rapidly growing view that the generous public support for academic research in the US has been, in effect, a subsidy to our overseas competitors who have beat us out in the marketplace by taking advantage of the openness of our academic system to commercially exploit research results for which they have not paid. The 'pure public good' assumption about basic science neglects the fact that a substantial research capability (and indeed actual ongoing participation in research) is required to "understand, interpret and appraise knowledge that has been placed on the shelf – whether basic or applied... The most effective way to remain plugged into the scientific network is to be a participant in the research process" (Pavitt, 1991) Similarly, Dasgupta has also argued that training through basic research enables more informed choices and recruitment into the technological research community. These arguments are certainly valid, but have proved very difficult to quantify.

It is notable that almost all the countries that have successful diffusion-oriented technology policies (Germany, Switzerland, Sweden, Japan, Korea) that emphasize the rapid adoption and diffusion of new technology, especially production technology, as a national strategic objective (Ergas, 1987), have among the highest ratios of R&D expenditure (public and private) to GDP among industrialized countries, as well as exceptionally high levels of educational performance at all levels. It seems reasonable to assume that a significant fraction of R&D support in these countries is for the purpose of enhancing awareness of what is going on in the world of S&T rather than necessarily for generating new knowledge for the first time "in the universe" (to use Nelson's phrase) (Nelson and Rosenberg, 1993).

In principle, one could argue that there is a trade-off between investment in R&D and investment in information infrastructure for the efficient distribution of R&D results to their potential users. The main reason that the performance of R&D is necessary for the absorption and appraisal of technology is that scientists engaged in research actually spend a large fraction of their time and effort communicating with others in order to be able to take the fullest advantage of the progress made by others in planning their own research strategy. Thus their excellence as a conduit for research knowledge to the organizations in which they work tends to be an automatic by-product of their active engagement in research. But still this is no guarantee that their information retrieval habits are optimal from the point of view of fellow engineers or scientists engaged in technological development or new product design. Thus these scientists are not automatically matched in their information retrieval behavior to the information needs of the 'downstream' phases of the innovation process.

Weed (1991) has studied this problem from the standpoint of medical practitioners delivering health care appropriate to unique individual patients, a process he describes as "problem-knowledge coupling". The challenge is how to map the vast body of collective knowledge embodied in the biomedical literature with the knowledge needed to deal with the specific needs implicit in the symptoms and medical history of the individual patient. According to Weed:

> Our confidence in our innate human capacity to make judgments as sound and reliable as our collective knowledge theoretically allows is simply unsupported by over 30 years of intensive research in clinical and cognitive psychology. Furthermore, there is extensive, often polemical as well as careful medical documentation that testifies to the rampant nonapplication or misapplication of medical knowledge to everyday clinical situations... The difficulties follow from the limitations of unaided human minds in applying a very large body of knowledge, when any portion of that knowledge base is intermittently and unpredictably relevant in day-to-day work. Specialization represents an attempt to deal with the problem. Unfortunately it runs afoul of the persistent failure of real problems to fit within the socially and historically defined boundaries of medical specialties. Medical knowledge, viewed as a whole, is as highly interconnected as the minds and bodies of its subjects. Tracing these interconnections wherever they lead in response to a real problem, as if following a map, is what medical problem solving requires. (Weed, 1991, p. xvi)

Much the same could be said about the huge body of engineering and scientific knowledge as related to the problems presented in the process of technological innovation and new product development in industry. In addition, of course, a significant proportion of the knowledge required in technological innovation is 'tacit' or 'embedded' in people, not codified or written down, and not communicable (at least at present) except by people working side-by-side. In the innovation process the importance of personal contact and geographical proximity between the generators and users of knowledge is supported by the observation from patenting studies that the academic research cited in industrial patents originates to a surprising extent in universities in relatively close geographical proximity to the patenting industrial laboratory (Pavitt, 1991, p. 116; Jaffe et al., 1993, pp. 577–598). But there is ample other literature citing the importance of embedded knowledge. The question suggested by Weed's work is whether dependence on personal contact, tacit knowledge, and 'serendipity' to inform the application of knowledge could be gradually reduced by more systematic exploitation of some of the tools of modern information technology, so that performance of research in organizations might become less essential to their capacity for the absorption of technology. I am rather inclined to doubt it because the growing 'scientization' of technology is likely to offset greater efficiency in formal systems of knowledge transfer from science to technology. Nevertheless, more effective use of modern information tools, and better documentation for future use of organizational experience in the product development process could still be of significant value in its own right.

5. References

Alic, J.A. et al., 1992, Beyond Spinoff, Military and Commercial Technologies in a Changing World (Harvard Business School Press, Boston, MA), April.

Bardeen, J., 1957, Crystal imperfections, in J.E. Goldman (editor), The Science and Engineering of Materials (Wiley, New York), Chapter 4, pp. 82–102.

Brooks, H., 1973, The state of the art: technology assessment as a process, International Social Science Journal 22, (3) (UNESCO, Paris).

Brooks, H., 1985, Returns on federal investments: the physical sciences, Workshop on The Federal Role In Research and Development (NAS/NAE/IOM), November (available from the NRC Information Office).

Brooks, H., 1991, Innovation and competitiveness, in The Technology Race: Can the U.S. Win? The J. Herbert Hollomon Memorial Symposium, MIT, April (presented by The Technology and Culture Seminar and the Center for Technology Policy and Industrial Development).

Brooks, H., 1993, Regions don't live on R&D alone, in Connection: New England's Journal of Higher Education and Economic Development, Vol. 8, No. 1, Spring/Summer.

Charpie, Robert A. (Chair), 1967, Panel on Invention and Innovation, Technological Innovation: Its Environment and Management, Department of Commerce, January, Chart 7, p. 9.

Frederickson, Donald S., 1993, Biomedical science and the culture warp, in: W.N. Kelley, M. Osterweis and E. Rubin (editors), Emerging Policies for Biomedical Research, Health Policy Annual III (Association of Academic Health Centers, Washington, DC).

Ergas, H., 1987, Does technology policy matter?, in B.R. Guile and H. Brooks (editors), Technology and Global Industry: Companies and Nations in the World Economy (National Academy Press, Washington, DC), pp. 191–245.

Jaffe, A.B., M. Trajtenberg and R. Henderson, 1993, Geographic localization of knowledge spillover as evidenced by patent citations, The Quarterly Journal of Economics 108, (3): 577–598.

Kline, S.J. and N. Rosenberg, 1986, An overview of innovation, in R. Landau and N. Rosenberg (editors), The Positive Sum Strategy: Harnessing Technology for Economic Growth (National Academy Press, Washington, DC), pp. 275–306.

Asimov, I., 1974, Of what use?, in E.H. Kone and H.J. Jordan (editors), The Greatest Adventure: Basic Research that Shapes Our Lives (The Rockefeller University Press, New York).

Materials Advisory Board, National Research Council, 1966, Report of the Ad Hoc Committee on Principles of Research–Engineering Interaction, ARPA, MAB-222-M (National Academy of Sciences–National Research Council, Washington, DC).

Nelson, R.R., 1992, National innovation systems: A retrospective on a study, Industrial and Corporate Change 1, (2): 347–374.

Nelson, R.R. and N. Rosenberg, 1993, Technical innovation and national systems, in R.R. Nelson (editor), National Innovation Systems: A Comparative Analysis (Oxford University Press, New York/Oxford), pp. 1–21.

Nelson, R.R. and R. Levin, 1986, The influence of university science research and technical societies on industrial R&D and technical advance, Policy Discussion Paper Series Number 3, Research Program on Technology Change (Yale University, New Haven, CT).

Pavitt, K., 1991, What makes basic research economically useful?, Research Policy 20: 109–119.

Physics Survey Committee, 1972a, Physics in Perspective, Vol. II, Part A (National Academy of Sciences, Washington, DC), pp. 490–492.

Physics Survey Committee, 1972b, Physics in Perspective, Volume II, Part B, The Interfaces (National Academy of Sciences, Washington, DC), pp. 796–798.

Rosenberg, N., 1990, Why do firms do basic research (with their own money)?, Research Policy 19: 165–174.

Rosenberg, N., 1991, Critical issues in science policy research, Science and Public Policy 18 (6): 335–346.

Shockley, W.E., J.H. Hollomon, R. Maurer and F. Seitz (editors), 1952, Imperfections in Nearly Perfect Crystals (Wiley, New York.

Suits, C.G. and A.M. Bueche, 1967, Cases of research and development in a diversified company, in Applied Science and Technological Progress, House Committee on Science and Astronautics, 90th Congress, 1st Session (June), pp. 297–346.

United States Office of Scientific Research and Development, 1945, Science: The Endless Frontier, A Report to the President on a Program for Postwar Scientific Research (U.S. National Science Foundation, Washington, DC) (reprint, 1960).

Wald, M.L.,1993, Government dream car, washington and Detroit pool resources to devise new approach to technology, The New York Times, 30 September, pp. A1, D7.

Weed, L.L., 1991, Knowledge Coupling: New Premises and New Tools for Medical Care and Education (Springer, New York).