ISSN 0265-8003

INFORMATION DISCLOSURE AND THE ECONOMICS OF SCIENCE AND TECHNOLOGY

Partha Dasgupta

Paul A David

Discussion Paper No. 73
September 1985

Centre for Economic Policy Research
6 Duke of York Street
London SW1Y 6LA

Tel: 01 930 2963

The research described in this Discussion Paper is part of the Centre's research programme in **Applied Economic Theory and Econometrics**. Any opinions expressed here are those of the author(s) and not those of the Centre for Economic Policy Research. The CEPR is a private educational charity which promotes independent analysis of open economies and the relations between them. The research work which it disseminates may include views on policy, but the Centre itself takes no institutional policy positions.

These discussion papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character.

CEPR Discussion Paper No.73
September 1985

Information Disclosure and the Economics of Science and Technology*

ABSTRACT

This paper takes an information-theoretic approach to the economics of science, extending Arrow's pioneering (1962) analysis of the allocation of resources for industrial research and invention. It addresses the questions: is there a valid economic distinction between scientific and technological research, and if there is, what implications may this have for public policy? A brief review points to deficiencies in several of the criteria proposed for distinguishing "scientific" from "technological" research, such as the degree of generality, abstractness, or practicality of the knowledge sought, or the source of the financial support. We suggest a primary differentiation arises between science and technology conceived as _social_ constructions, and is manifested in the greater urgency shown by the "scientific" community towards the disclosure of newly acquired information. Scientists, _qua_ scientists, may be thought to be devoted to the growth of the stock of knowledge as a public consumption good, whereas the technological community is concerned with the flow of rents that private parties derive from discoveries and inventions. The role of priority as a basis for allocating rewards among scientists, its compatibility with the norm of disclosure, and the ambiguous status of patent systems, are reconsidered from this perspective. Certain ineluctable conflicts between the goals of the two research communities point to the persisting economic need for public subsidies to sustain the scientific attitude.

JEL classification: 024, 026, 620

Keywords: information theory, economics of science, technological change, public goods.

Contd/...

Partha Dasgupta
St. John's College
Cambridge
England
(0223) 358944 or 61621

Paul A David
Department of Economics
Stanford University
Stanford CA 94305
U.S.A
(0101) 415 497 2371 x 2426

*This essay owes much to the many conversations that each of us has held over the years with Kenneth Arrow. Among these we can pleasurably recall pertinent discussions of the economics of information, allocation of resources to scientific research, and the organization of communities of academic scientists. Although these pages are offered in his honour, and we would ourselves be honoured to have them accepted as an extension of the line of thought represented by his seminal 1962 paper on the allocation of resources for inventions, Arrow himself must not be blamed for the views expressed here.

This is an April 1985 version of an essay, the first draft of which was prepared while Dasgupta was a Visiting Fellow at the Center for Economic Policy Research at Stanford University during the summer of 1984. The present version has benefitted from the comments and suggestions made by Bengt Lundvall, R.C.O. Matthews, Roger Noll, Jean-Jacques Salomon, Walter G. Vincenti, and participants in the Technological Innovation Program Workshop in the Department of Economics at Stanford University during Fall Quarter, 1984. Joshua Rosenbloom provided able research assistance.

To be published in a two-volume collection of essays in honour of Kenneth J. Arrow, edited by George Feiwel.

NON-TECHNICAL SUMMARY

Economists understand technology less deeply than some might hope. But they understand the world of technology far better than they do the world of science. But is it useful for economists to distinguish between science and technology? Can a reasonably precise analytical distinction be drawn between the forms of research and invention which are normally studied by economists and the information-seeking activities referred to commonly as 'science'?

We argue that the distinction between science and technology is not merely semantic. A real difference does exist between these two spheres of human endeavour, one which we believe should be more widely appreciated by those formulating economic policies affecting science and technology. Recognition of this difference enables us to better understand why the position of academic science in modern industrial societies is both exalted and yet so financially precarious as to require constant public nurture. Although the contributions of scientists and technologists to the search for knowledge may be interdependent, we shall suggest that science as a social entity today is in danger of being undermined by the technological community's conception of knowledge as a form of productive capital.

In particular, our analysis implies emphatically that science and technology, as social organizations, are <u>not substitutes</u> for one another; that the vision which sees technology as allocating resources towards greater 'economic' purpose is a deeply flawed one. We argue that an essential difference between science and technology lies in the respective goals that the two communities - scientists and technologists - have set for themselves. Roughly speaking, the scientific community appears concerned with the <u>stock</u> of knowledge and is devoted to furthering its growth. The technological community is concerned with the private returns or economic <u>rents</u> that can be earned from that stock. In the

social role of 'scientist' a researcher views the stock of knowledge as a *public consumption* good; in the role of technologist he or she regards it as a *private capital* good. Each community seeks to inculcate in its members, through training and incentives, attitudes concerning research procedures that tend to further its particular goals.

One important manifestation of this is the greater urgency shown by scientists in disseminating newly acquired information throughout the research community. This emphasis is not shared by technology-researchers, who are free to adopt information strategies ranging from disclosure to total secrecy in regard to their discoveries and inventions. Sociologically astute observers of the two research communities have remarked upon this differentiation. But they have not probed for explanations of the phenomenon.

The criterion we propose to distinguish science and technology suggests one explanation. It also leads to an understanding of the function that priority of discovery has as an incentive mechanism in science, and of its relationship to the complex incentive mechanism which the patent system seeks to create in the sphere of technology-research. Having distinguished between science and technology according to their respective objectives, incentive structures, and approved modes of behaviour for participants, we are able to see more clearly both the areas of compatibility and the sources of tension that characterize relationships between the two research communities. We are, in particular, able to see why on average there is movement of researchers from science to technology and not in the other direction.

1. Arrow, Information and the Underdeveloped Economics of Science

Economists understand technology less deeply than some might hope. But they understand the world of technology far better than they do the world of science. (See, e.g., Rosenberg (1982), esp. ch. 7) Kenneth Arrow's famous 1962 essay, and the literature it inspired, is in good part to blame for this state of affairs. In "Economic Welfare and the Allocation of Resources for Inventions," Arrow laid the foundations for modern economic analysis of research and development (R&D) activities. On that base, a large, and impressive edifice of research devoted to the economics of technological invention and innovation has since been erected. By absolute as well as comparative standards, the economics of science has remained lamentably underdeveloped. That too is traceable to the 1962 essay.

Of course, we do not attribute to Arrow the profession's concern with the microeconomic processes of technological change. By the 1950's a rapidly growing number of economists appreciated that technological progress was not exogenous and had as one of its sources the purposive, profit-motivated quest for certain kinds of information on the part of individuals and firms. Yet, prior to Arrow (1962) no writer had so forcefully articulated an information-theoretic approach to the subject of technologically oriented research and invention; no one had so clearly seen that inasmuch as research and invention are directed to producing _information_, an economic analysis of R&D activities must inevitably rest upon recognition of the peculiar characteristics of information viewed as an economic commodity.

Arrow's essay began by pointing out that the production of information (additions to the stock of technological knowledge) itself is a business that must be conducted under great uncertainties, and, due to moral hazards, many of these risks cannot be efficiently insured. He next observed that the costs of transmitting information typically are very much smaller than the costs of producing it, so that, considered from the supply side, information has the attributes of a public good. Turning then to the demand for information, Arrow pointed out that its use was subject to certain indivisibilities, in the sense that once a certain piece of information had been acquired there was no value added in acquiring it again.[1] An awkward corollary followed: potential purchasers of information could not ascertain its value exactly, since to disclose it would be to convey the information without cost.

Together these properties made information a commodity quite distinct from the goods traded in the sorts of markets which economists normally analyzed. There was a strong presumption that decentralized resource allocation mechanisms which yielded socially desireable outcomes with "standard commodities" would break down in the case of a commodity having the properties of information. Uninsurable risk, the difficulties of recovering fixed production costs under competitive conditions of information transmission, and, most of all, the difficulties of appropriating the benefits derived by users, all suggested that a perfectly competitive market economy would underinvest in R&D. A rationale was thus suggested for public subsidization of R&D investment on grounds of "market failure," one that has become the principal intellectual underpinning for such technology policy as the United States currently may be said to have. (See Mowrey (1983).)

We can see now that the ramifications of this conceptual approach reach well beyond consideration of the category of scientific and engineering information associated with the performance of R&D. Arrow stopped short, however, without explicitly tackling the economics of science. While his discussion of the properties of information was carried through at a high level of generalization, the subject of inventive activity closely circumscribed the application of his framework on that occasion. He did not inquire into the relationship between the allocation of resources in the two lines of human endeavor whose intertwinned development has played so crucial a role in the epoch of modern economic growth.

Although an information-theoretic path had been marked by Arrow for others to follow towards better understanding the economics of science, as well as the economics of technology, very few economists actually have ventured to explore it. Unfortunately, in the intervening years the track has become somewhat obscured by an overgrowth of other approaches to the subject.[2]

We shall take up the inquiry on the present occasion just where Arrow left off, by posing the question: Is it useful for economists to distinguish between science and technology? Can a reasonably precise analytical distinction be drawn between the forms of research and invention with which Arrow was preoccupied in 1962, and the information-seeking activities referred to commonly as "science"?

The question is limited, but it is not an idle one. Behind the facade of sematics, a real difference does exist between these two spheres of human endeavor, one which we believe should be more widely appreciated by those formulating economic policies affecting science and technology. We will argue that to recognize this difference is to better understand why the position of academic science in modern industrial societies is at once exalted and economically so precarious as to require constant public nurturing. Although the contributions of scientists and technologists in the search for knowledge may be perceived to be interdependent and even symbiotic, we shall suggest that science as a social entity today is in danger of being undermined by the technological community's conception of knowledge as a form of productive capital.

The question is also not a new one--at least not to scholars in disciplines apart from economics. We will therefore need to consider (in Section 2) several of the differentiating criteria which have been proposed by historians and sociologists of science and technology. To summarize briefly, there are those who hold that scientific and technological research are to be distinguished from one another by the "technology" of research; for example, by the alleged fact that scientific research is on the whole the riskier of the two. Then there are those who would draw the contrast on the basis of the nature of the commodity produced by the research; for example, by whether or not the addition to knowledge has practical applications. Finally, we take notice of the observation that science and technology are distinguished from one another by differences in the way they treat the information-products; in particular, by the divergence of attitudes, behavior and institutions relating to the *communication* of research findings (see, Section 3 below).

Some of these distinctions, including the principle which Arrow (1962) proposed for separating "basic" from other research, turn out on closer inspection not to be so useful for our purposes after all. Others come closer to the heart of the matter, in our view, but still leave many loose ends. To tie these up, we will suggest (Section 4) that an economically consequential difference may be discerned between the respective *goals* that the two communities--scientists and technologists--have set for themselves.

Roughly speaking, the scientific community appears concerned with the *stock* of knowledge and devoted to furthering its growth, whereas the technological community are concerned with the private economic *rents* that can be earned from that stock. Put another way, in the social role of "scientist" a researcher is expected to view the stock of knowledge as a *public consumption* good, while in the role of technologist he or she regards it as a *private capital* good. As would be expected, each community seeks to inculcate in its members, through training and incentives, those attitudes and mores concerning research procedures and findings that tend to further its particular goals.

One manifestation of this--and the one we shall highlight--is the greater urgency shown by scientists in disseminating newly acquired information throughout the research community. The same imperative evidently is not shared by technology-researchers, who are free to adopt information strategies ranging from disclosure to total secrecy in regard to their discoveries and inventions. Sociologically astute observers of the two research communities (e.g., Salomon (1973)), are quite likely to remark upon this differentiation. But they have not probed deeply for explanations of the phenomenon. Why should these two cultures—which in the modern world are so similar in other respects, and between which the same individuals are observed increasingly to move with ease—diverge so sharply on the standards for acceptable behavior in regard to secrecy and disclosure of information?

Our classificatory principle suggests one mode of explanation. It also leads quite directly (in Section 5) to an understanding of the function that priority of discovery has as an incentive mechanism in science, and of its relationship to the complex incentive mechanism which the patent system seeks to create in the sphere of technology-research. Having distinguished between science and technology according to their respective objectives, incentive structures, and approved modes of behavior for participants, we are able to see more clearly (in Section 6) both the areas of compatibility and the worrisome sources of tension that characterize relationships between the two research communities.

Finally, we may remark that the contrast drawn here between science and technology also helps to explain why economists have less trouble turning their attention to the latter, but have generally shied away from studying the allocation of resources for science. Needs and desires for consumption goods are usually treated as data in modern economics. Their origins, and their influence upon the organization and conduct of consumption activities are regarded to be proper subject matter for biology, psychology and sociology, but not for economics.[3/] The demand for capital goods, on the other hand, is a derived demand and may be more readily disentangled from questions of "tastes." When information generated by research is conceived of purely as a capital good, as is the case in the realm of technology, it becomes immediately more amenable to the accepted style of economic analysis. But this has resulted in the underdeveloped condition in which we now find the economic analysis of science.

2. Do Science and Technology Produce Different Kinds of Knowledge?

The word "science" means "knowledge" in Latin, but much time has been spent elaborating hierarchies of knowledge in which a place is reserved for "science" in the uppermost branches. Thus, medieval Western scholars drew a distinction between speculative, theoretical or abstract knowledge; and art, or practical knowledge. Labels were taken from the Greek: the former was referred to as episteme, and the latter as techne. Countless writers, down to the present day, draw a distinction along just these lines between "science" and "technology," regarding science to be occupied with the production of knowledge which is more general and fundamental to understanding the natural order but of less immediate practical applicability.

This perspective can be seen in the assignment made by Price (1967), who identifies knowledge about natural phenomena and activities involved in its discovery as belonging to science; knowledge concerning useful mechanisms and processes is assigned to technology. In Price's taxonomic scheme, science and technology are pursuits which proceed largely independently of one another, and may be distinguished principally by the nature of their respective products. Technology, he suggests, might almost be defined "as a field where the chief intended product is an object, a manufacture, a process, a chemical...", whereas the chief object of science is the production of "a paper"—a published record of one's discovery. (Price (1967): p. 10.)

The same notions, or ones very closely related, surfaced in the distinction that Arrow (1962: p. 618) sought to draw between "basic research" and other kinds of research, including invention. A "basic" form of knowledge-generation was defined implicitly as one "the output of which is only used as an informational input into other inventive activities." Although science was not explicitly placed in the category of "basic research" by Arrow, many readers probably found it natural to make that extension. After all, the argument being advanced was that the more "basic" the character of the research, the more in need it would be of public support. This followed from two propositions: (1) there was inherently greater difficulty in appropriating the value of information produced to serve as an input into further research, compared with appropriating the value of information applicable directly to the production of physical commodities; (2) "the value of information for use in developing further information is much more conjectural than the value of its use in production and therefore much more likely to be underestimated." (Arrow (1962): p. 618.)

Upon closer consideration, however, it seems less and less promising to set out to separate research in science from that in technology on the basis of the characteristics of the knowledge generated by these activities, or their mode of generation. The production of new knowledge and of useful devices and processes move along very similar lines. An outside observer would be hard-pressed to decide whether a research worker was a scientist or a technologist, merely by categorizing the sequence of activities in which he or she was engaged, or examining the results obtained at any given point in the research program. A working scientist frequently would appear to be producing what in Price's (1967) scheme would have to be counted as technology, in the shape of the specialized equipment developed to test hypotheses or to make more accurate measurements. The invention of the bubble chamber is an illustration that is oft-cited, as is the electron microscope. A "device-seeking" technologist, in turn, may generate new empirical findings and explanatory models that would qualify as science in the same classificatory scheme. The discoveries concerning the properties of semiconductors by the Bell Laboratory research group that invented the point contact transistor and the junction transistor, is a well known case in point. (See, e.g., Nelson (1962), Levin (1982).) Examples of this interplay abound in the fields of molecular biology, biochemistry, and solid state physics.

Moreover, observers of the modern science and technology scene have remarked upon the strongly convergent tendencies within the two research disciplines in precisely these regards. Brooks (1967: pp. 38-9) writes that "the newer technologies, such as nuclear energy, electronics, and computers tend to be more externally oriented than the older technologies. There is more conscious effort to conceptualize technological knowledge." Correspondingly, Salomon (1973: p. xi) describes the "specific characteristics of modern scientific research" as the fact that "it increasingly abolishes any lack of continuity between the generalization stages and the application stages of the process of discovery and invention." Whatever the historical differences that may have separated the methods of technologists from those of scientists, it would seem that as we look to the future there is more and more reason to treat research, both scientific and technological, as one continuous process of iteration between phases of generalization and application.

Be this as it may, many will still find it hard to relinguish the intuitive and commonly held idea that science seeks the abstract and general while technology pursues the concrete and particular; that science as a vocation is other-worldly, whereas technology compells its practitioners to be firmly mired in mundane facts. The difficulty in clinging to this outlook does not arise in the drawing up of boundaries and the dividing of the varieties of knowledge along such lines. One can make the distinctions, however approximately. The problem for the economist--indeed for social scientists more broadly--is that one does not know what to do with the resulting classification of research disciplines. Thus, to choose an example within the realm of science, chemistry as it is understood today is really atomic physics writ large. That is, chemical laws, such as those of molecular bonding, are based on quantum physics. In an obvious and trivial sense then physics is more fundamental, more general, than chemistry. Likewise, much of current-day biology, such a molecular biology, is chemistry writ not-so-large. To the extent this is so, chemistry is the more fundamental, the more general. But nothing of moment appears to follow from this. What advantage could be gained in grappling with resource allocation issues by insisting that somewhere within an extended ordering of this kind we may locate various technological disciplines?

Nor does it seem much more helpful to differentiate between science and technology on the basis of the degree of uncertainty in the products of research. There is a temptation to view the outcomes of scientific research as in being particularly clouded by uncertainties, and to depict technological research as time-consuming, and costly, but more routinized and thus comparatively predictable. One can agree with Arrow (1962) that some significant resource allocation consequences would follow from this distinction. But the empirical premise is not well founded. "Normal science"--and we are using the term in the sense of Kuhn (1962)--is not especially risky.[4] To be sure, even in normal science one does not know exactly what one will discover. But this is the case with all research. Indeed, it appears to be true even in contract research, where the commodity eventually produced is often different in design and use from the product for which the development contract was drawn up. (See, e.g., Klein (1962).) Quite apart from the issue of the predictability of the characteristics, or the commercial value of the fruits of R&D programs, the uncertainty of project completion times in industrial research organizations is widely acknowledged.

One further point should be noted, especially in regard to Arrow's (1962) designation of "basic" research as being intrinsically riskier because the value of information for use as a capital input in developing further information was "more conjectural". The main difficulty with this is that it cannot provide an operational basis for *ex ante* classification of research programs. As we have pointed out, scientific and technologically oriented programs of investigation both entail interaction between phases of generalization and application. It is always possible that the search for information to be used, in a roundabout manner, to yield further information, will be temporarily short-circuited by a discovery which has direct application in commercial production. Likewise, the search for specific, commercializable devices may uncover scientific principles which further the process of discovery. It is certainly conceivable that awards could be made in accordance with the ex post assessments of research programs' results on this scale of "basic-ness"; as a practical proposition one would expect such awards to closely resemble the prizes offered by scientific and engineering societies. An efficient *ex ante* incentive scheme intended to subsidize programs of research which are directed towards producing "pure" generalizations rather than applications must be harder to design, however; all programs would find it advantageous to seek support on the grounds that they aimed at laying a foundation for further research. The best one could do would be to ask some independent authority to guess at what an ex post assessment of program results on the scale of "basic-ness" would turn out to be, and award support accordingly.

From the foregoing discussion it should be evident that we do not believe the more useful distinctions to notice between science and technology are the ones most often drawn, namely classifications of research activities according to the intended or realized characteristics of their "products." A more promising approach is to be found by focusing not upon the production of varieties of information, but upon observable differences in the social ethos of research which are reflected by researchers' attitudes and actions in regard to the transmission of information.

3. Institutionalized Disclosure versus Secrecy

The germ of this alternative classificatory principle certainly is contained, along with the other material we have already sifted, in Price's description of science as having for its chief objective the publication of "papers", public recordings of discoveries. Price (1967: pp. 10-11), however, did not elaborate upon it much beyond offering the aphoristic observation that scientists "are highly motivated to publish but not to read," whereas technologists read assiduously but are not motivated to publish. Both the principle of classification, and the source of the suggested difference in researchers' motivation regarding information transmission, deserve to be further developed.

One can only do this by treating research activities and researchers not as atomistic entities, but rather as parts of a larger, social construction. For, it is as social constructs that science and technology appear to differ most markedly. Individuals and teams engaging in research based upon "scientific methods" will spend their time postulating theories, developing models, which are intended to explain phenomena and predict "facts" yet to be discovered. When their attention is directed to the natural world, we distinguish them from other generalizers and fact-collectors and affix to them the label of "natural scientists". Not all actions in this mode belong to the realm of science. For we conceptualize "science" as a voluntary collective organization, a community, or club that imposes particular rules upon those who wish to be recognized as participating members.

Some of these rules have to do with acceptable procedures for the statement and testing of theories, and for the form in which predictions are cast. These matters have been much discussed by philosophers and sociologists of science and need not be elaborated here. Instead, we emphasize a crucial additional feature of the "scientific ethos", which is that scientists act as if they were obligated immediately to disclose all new discoveries and submit them for critical inspection by other members of the community. In other words, the community rules instruct scientists to regard a new theory, or the principles underlying a new piece of equipment, or a newly observed phenomenon, as a public good regardless of the identity of its originator. Thus, in submitting their own findings to their peer group, scientists qua scientists surrender claim to exclusive control of that information. In fact the social criterion is even more stringent: complete disclosure is the rule.

In an insightful and important contribution, Jean-Jacques Salomon (1973) argues that it is precisely in this respect that science and technology differ significantly. Scientists hasten to publicize their findings, whereas technologists are more likely to display reticense:

> "The channel of science is in principle open, based on criticism by peers, easily and rapidly accessible to all researchers in the specialized literature, while the channel of technology is less available, more subject to the restraints of industrial organization and competition, bound by secrecy or, more simply, by the difficulty of transmitting the subtleties of the 'know-how' of processes that depend more on apprenticeship on the job than on the understanding of concepts." (Salomon (1973), p. 80.)

Even when the latter difficulties of transmission have been erased--by the increasing tendency towards conceptualization and generalization which Brooks (1967) noted in the newly emerging areas of technological research--the difference in the patterns of institutionally sanctioned behavior between the two communities is more than likely to persist.

Let us then draw a sharp distinction between science and technology in regard to the disposition of their respective research findings, and express it in the form of a social imperative: if one joins the science club, one's discoveries and inventions must be completely disclosed, whereas in the technology club such findings must not be fully revealed to the rest of the membership. There should be little doubt that this is a caricature, rather than a careful piece of sociology. But a good analytical distinction which points the user in the right direction can afford to be overdrawn. Its defect takes another form: it describes without explaining. For we should by now be asking _why_ scientists and technologists persist in displaying such different attitudes towards the disposition of their findings.

4. Public Consumption versus Private Capital

To ask that question is to see almost immediately where an answer can be found. It must lie in the fact that there is a difference in the goals of the social organizations to which they belong. And the only thesis that can explain the phenomena of disclosure and secrecy is that science aims at increasing the stock of knowledge, while the goal of technology is to obtain the private rents that can earned from this knowledge.[5] Roughly speaking then, science views knowledge as a public consumption good, whereas technology regards it as a private capital good.[6]

The benefits to the scientific community from disclosure are two-fold. First, we may note that the existing stock of knowledge is a crucial input in the production of new knowledge. (Among other inputs are the ability and zeal of the investigator!) Disclosure increases the expected span of application in the search for new knowledge. To put it in other words, disclosure raises the social value of new discoveries and inventions by lowering the probability that they will reside with persons and groups who lack the resources required to exploit them. Second, disclosure enables peer groups to screen and to evaluate the new finding. The result is a new finding containing a smaller margin of error. The social value to the community of scientists is that scientific users of new discoveries can tolerate a higher degree of risk arising from other sources of incomplete information.

Contrast this with the situation in the community of technologists. If each member is obliged to be concerned with the private rents that can be earned from new discoveries (or new uses of old discoveries--it comes to the same thing), secrecy is precisely what would be practised. It would not matter so much that the discovery has not been screened by fellow-professionals. What matters more is that it proves useful in yielding a privately capturable rent. Disclosure would reduce the private rents to the discoverer because there would then be many people to share the rent with.

But a piece of knowledge can simultaneously be used by any number of people any number of times. Technically speaking it is a public good, that is, there need be no rivalry in its use. It follows that knowledge, once produced, ought to be freely available to all (assuming of course that transmission costs are insignificant).7/ Thus what we are identifying as the common purpose of science is consonant with society's aim. Disclosure in particular is a necessary condition for the efficient use of knowledge. This explains why so much science has throughout been supported by public institutions in centres of learning such as universities.8/ Secrecy and the efficient use of knowledge are inimicable.

The difficulty with disclosure, which Arrow (1962) made clear, is that the removal of appropriate "private" incentives hampers the production of knowledge is in a decentralized environment. Society at large may seek to solve this problem by allocating funds for science through public bodies. But what is the guarantee that scientists will not slack? The institutions of science appear as having been adapted to meet this particular problem at least partially, by nurturing the rule of priority. And somewhere between full disclosure and secrecy there lies another, related incentive mechanism: the institution of patents, which technology often relies upon. In the following section we re-examine these familiar social contrivances.

5. Priority and Patents

The priority rule, which is used by the scientific community to reward its members, serves two purposes at once. First, it establishes a contest for scientific discoveries. Since effort cannot in general be monitored, reward cannot be based upon it. So a scientist is rewarded not for his effort, but for his achievement. An alternative would be a fixed fee, but as one collects the fee whether or not one has produced anything of interest, this dulls the incentive to work hard. Since it is difficult in general to determine how far behind the winner the losers of a scientific race are when discoveries are made, it is not possible to award prizes on rank. (Science does not pay the "runners-up", unlike tennis tournaments). The remaining type of payment scheme that is compatible with individual incentives is the one where the "winner takes all." Priority mimics this.[9]

But taken alone, the priority rule places all the risk firmly on the shoulders of the scientist. This cannot be efficient if scientists, like lesser mortals, are risk-averse. So of course scientists must be paid something whether or not they are successful in the races they choose to enter. It is in this light that Arrow's ((1962): p. 623) remark, "... the complementarity between teaching and research is, from the point of view of the economy, something of a lucky accident," assumes its full significance.

The second purpose that the rule of priority serves is in eliciting public disclosure of new findings. Priority creates a privately owned asset--a form of intellectual property--from the very act of relinquishing exclusive possession of the new knowledge. It is truly a remarkable device. In science priority often _is_ the prize, for "moral possession" is thereby awarded to the discoverer even when legal possession is neither possible nor desired by any party. (On "moral possession," see Medawar (1982): p. 260.) Priority is the basis upon which scientific societies award various tokens of public recognition and is also the ground for claims to informal recognition of one's accomplishments by one's scientific colleagues. The most prestigious awards are those that are made by scientific bodies possessing the most extensive scope. In particular, the widest possible publication of a research contribution is a prerequisite for claiming the greatest honours that the scientific community can bestow.

We have now at hand an economic rationale for the extraordinary, and otherwise puzzling degree of importance which scientific communities accord to resolving priority disputes among contestants. The rule of priority is a particular form of payment to scientists. It is often a non-pecuniary award. We have noted that it fills two roles, both of which are instrumental in furthering the common purpose of science. It is surely to be expected that scientists, as individuals and as members of collective bodies, will devote great attention to priority disputes.[10]

Compare this form of reward with the one in technology. The rewards of the technologist, qua technologist, are linked to the privately appropriated rents from knowledge. The beneficiary of such additions to knowledge—which may or may not have met the test of being additions to scientific knowledge—is presumably willing to pay for them. This creates the possibility of a reward structure that is not linked with priority of discovery. A commercially successful application of a long-accepted scientific principle will typically award the adaptor, not the originator of the theory, even when the adaptor has contributed nothing more than the restatement of the principle in terms that have exposed its commercial relevance.

We have noted that secrecy provides a means of capturing rents from new findings. But secrecy is not completely reliable. Apart from anything else there may be little to prevent rivals from making the same discovery at a later date and sharing the rent. The institution of assigning patent protection attempts to remedy this. Patent systems in principle allow people and firms to disclose an addition they have made to the stock of knowledge, without obliging them to share the rents that can be earned from their finding. The system in effect offers a private reward for disclosure and makes the award on the basis of priority of disclosure. The reward itself is tied to the private rents that can be earned from the new knowledge which in turn the patent is intended to help secure. (In contrast, the reward in science may be entirely non-pecuniary.) By connecting the realm of <u>techne</u>, through conveyance of a right to exclusive use, with the realm of <u>episteme</u>, through the requirement of disclosure, the patent system undertakes to solve the problem of <u>financing</u> the pursuit of scientific--that is, publicly disclosed--knowledge.[11]

The patent system is both interesting and problematic because it represents a conjunction of the distinctive and antithetical mores of science and technology in regard to the treatment of new information. Looking backward it seeks to reward additions to knowledge that are disclosed, and does so on the basis of priority. But to finance the award it looks ahead to a contrived limitation of access to the new knowledge. As a social invention it incorporates the fundamental feature of the reward structure of the scientific community which seeks to create intellectual property from a public good. By leaving the determination of the economic value of that property to the workings of the market, the assignment of patent rights necessarily inhibits the utilization of that public good.

While the patent system itself is a remarkably ingenious social device, economic analysts properly persist in asking whether the social benefits it confers through the encouragement of invention are worth the social costs of creating a private property right whose economic value derives from restricting the use of knowledge which has already been acquired.[12] Moreover, it has been shown that under a wide array of circumstances the device works in a way that elicits too much expenditure of resources in the races among rival research groups to obtain patent-properties which are allocated on the basis of priority (see, e.g., Dasgupta and Stiglitz (1980a, 1980b)). The social inefficiency manifests itself in excessive duplication of research effort—leading, on average, to too many of Professor Merton's "multiples"--or to too fast a pace of advance of the frontiers of knowledge.[13]

More to the immediate point, however, are the imperfections of the patent system which are traceable to the attempt to engage private rent-seeking as a means of eliciting the disclosure of certain kinds of useful knowledge. Here we are laying more stress upon the problem of "disclosure" than upon the problem of "utilization" of that which has been disclosed. On the one side, a complete monopoly over the use of the knowledge cannot be conveyed (even for a finite period) by an arrangement which is designed to elicit some significant degree of information disclosure. From the standpoint of the patentor, the instrument appears defective in risking the communication of sufficient information to render alternative, and commercially competitive solutions less costly to obtain than was the original invention. The original patentee will remain uncompensated for the depreciated value of the patent right in the event of that likelihood being realized.

At some level this failing may be ineluctable, since the mere disclosure that a problem is solvable (i.e., that at least one solution has been verified by an independent authority) may serve to channel inventive resources in directions that increase the likelihood that a substitute (or, worse still, a superior solution) will be found. For example, it has been said that research on semiconductors was sufficiently far advanced in many places by the close of 1947 that "from the mere knowledge that such a thing as a transistor was possible, there were perhaps twenty-five organizations which could have made one." (Braun and MacDonald (1978): p. 52.) The Bell Laboratories group, who had discovered the point contact transistor in late December of that year, therefore faced a conflict between its perceived need to better understand the transistor for the purposes of filing patent applications, and the interests of the inventors (John Bardeen and William Brattain) in establishing their scientific priority by publishing a paper in the Physical Review. Although the first patent application was filed in February 1948, the discovery was kept a close secret within Bell Labs for some seven months, up to the eve of publication of the Bardeen-Brattain paper.[14/]

On the other side, the patent system may permit inventors to advertise in a more credible way their claims to possess useful knowledge, whilst not compelling them fully to divulge it to others. It is a commonplace observation that the research experience leading to a patentable invention generates technical knowledge which is not contained in the patent application itself, but is complementary to it. Such information may be difficult to systematize and costly to transfer to a potential licensee. (See Teece (1976) on the resource costs of technical information transfers.) But when such necessary knowledge can be transmitted readily, the purpose of filing the patent may be less that of seeking to deter imitators than of signalling the availability of trade secrets for sale by the patentor.

A vivid instance of the coupling of patent disclosure with secrecy is contained in Vincenti's (1985) history of the Davis airfoil design. The latter specified the shape of the fore and aft-sections of the airplane wing which was selected in 1938 for the B-24 bomber built by the Consolidated Aircraft Corporation of San Diego. In 1934 David R. Davis, a lone inventor, had filed a patent ("Fluid Foil") which described a method of generating airfoil sections by using a pair of mathematical equations. The derivation of these equations remained rather mysterious, and the patent itself did not reveal the values of the two constants that would generate an optimum airfoil. On the basis of the superior wind-tunnel performance of a wing model produced in 1937 by the inventor, Consolidated first set one of its engineers to discover the profile by guessing at values for the constants and drawing the corresponding airfoils from the equations of Davis's patent. The Company soon abandoned the attempt as hopeless, and, in February 1938, signed an agreement to pay royalties on a sliding scale if they adopted the Davis airfoil on any Consolidated airplane. In exchange, the inventor supplied the coordinates of the airfoil section for the model which had tested so well, and a corresponding pair of parameter values.[15]

The imperfections we have examined in the patent as a devise for rewarding disclosures of knowledge are not at all surprising; a stone flung at two birds really ought not be expected to make a clean strike on either.

6. Science and Technology--The Perilous Balance

The intellectual property that is created once priority has been established derives much of its value from a desire for recognition and esteem. Given the common purpose of science it is clear why among other things scientific training is so designed as to arouse this desire in a particularly sharp form. It is true that part of the reward enjoyed by a scientist is the research activity itself. But the pleasure and excitement of conducting research must be comparable in technology since, by our classification, programs in science and technology can easily entail what from an epistomological perspective is the same course of research.

An alternative source of value of a finding to the discoverer is, as we have noted, the private rent that he can capture from its use through a patent, secrecy and so forth.[16] It would seem then that the value of scientific reward (through priority) needs to be measured against the prospective economic rents that can be collected by taking one's discovery to the realm of technology. But this would suppose that the investigator pursuing the method of science is able freely to decide <u>after</u> the discovery whether to disclose it, as in science, or to capture the private rents, as in technology. Such discretion is sometimes open to investigators, but more often organized scientific research projects demand precommitment from their members. If the findings are proprietary and are not to be disclosed publically, one has a project precommitted to what we have classified as technology. Pre-commitment to public disclosure is the hallmark of projects organized in the realm of science. An economic basis of choice between alternative commitments on the part of the investigator can be obtained by comparing the expected values of the returns derived under each set of conditions regarding disclosure.

But now consider what would happen if over a period the value of privately appropriable rents from knowledge--or anyhow, certain types of knowledge--increases at a sharp rate. The cost of maintaining the level of resources engaged in science rises as research workers are drawn increasingly to pre-commit themselves to technology. The economic returns in technology thus affect the state of science. This is hardly surprising and does not need elaboration.

There is, however, a different effect to be considered, one that runs in the opposite direction: from science to technology. The existing pool of knowledge is an essential input in the production of new knowledge. That is why technology draws so heavily upon the infrastructure provided by science. If, to take an extreme example, science were to close down, each enterprise in technology would, roughly speaking, have to rely on its private knowledge pool. This would dampen technological progress enormously, as technological enterprises would then for the most part be conducting duplicative research. The public good-producing aspect of science is of course recognized to be of considerable importance to the technological community, which is why one on occasion sees groups of technological enterprises spontaneously joining to support activities organized under the rules of science. But the preceding discussion also indicates why this is inadequate and leaves science constantly in need of shoring up. Unless investigators are socially conditioned to be imbued with the scientific spirit, the material rewards offered for participation technology will draw them away. Science faces a continual struggle to command talent in the face of competition from technology, and the more closely the two research disciplines resemble each other, the more vulnerable science must become.

There is, however, one exceptional feature. Fame and recognition and prizes are not the only possible private returns that one can expect from participation in science. Establishment of priority, which requires disclosure, provides a clear signal about the discoverer's talent, and this typically affects the conditions of a researcher's future employment--including the option of finding employment in technology. A reputation won in science increases the quality of offers the researcher can obtain for entering projects in technology. As a limiting case, entrance into science can be viewed purely as an investment in acquiring the appropriate reputation for subsequent entry into technology.

It follows immediately that in this limiting case--on which we now will concentrate our attention--the attraction that science offers to the eligible researcher is negatively related to (i) the rate of time discount and (ii) the rate of obsolescence of signals acquired by gaining recognition for achievement in science. If the rate of growth of knowledge is rapid, and if gaining recognition requires some minimum time (for discoveries to be publicized, confirmed and for priority to be ascertained), the risk of obsolescence is high. Other things remaining the same, fewer research workers will be motivated to enter science for the purpose of acquiring a visible signal of their creative capabilities. When the growth rate of scientific knowledge is high, the scientist needs to show not only that he is creative, but also flexible intellectually. In order to demonstrate the latter he has to remain in science and continue to be creative. But this postpones entry into technology!

What this means is that scientific research <u>programs</u> cannot continue to gain cumulative momentum simply by the chance attraction of participants. A program in its infancy may even display increasing returns to scale, since the risk of obsolescence is then very low. Of course, a more "progressive" research program, (which, in the sense of Latakos (1980), generates more new theories with any given number of participants) can gain adherents because the chance of achieving scientific recognition is higher even though the risk of obsolence is also higher.

The preceding discussion leads us to the somewhat surprising conclusion that science, as a social institution, could be maintained even when no pecuniary prizes are awarded for priority of discovery, and even when people are not motivated by the quest for fame.[17/] What it requires is a parallel social organization, called technology, and a capital market for aspiring technologists to borrow from so that they may subsist while making a reputation in science. But we have also noted that if the returns in technology become very high, science is likely to suffer; scientific reputation may be less of an advantage, and individuals' continuing participation in scientific research will be curtailed as they seek to "cash in" on their past credential-generating investments.

Consider now what happens in science when for some reason the rate of entry into it drops. Assuming that people embark on careers in science in the hope of establishing a public record of achievement indicative of their talent (the signal!), the inherently more able will on average acquire their signal sooner and depart for the realms of technology. The only thing that prevents the average level of talent among scientists from falling by this selective exit mechanism is the continuing entry by new cohorts, among whom the talent for research is presumably distributed randomly. When the rate of attraction into science falls, the average talent among scientists also falls. Given the programs of scientific research, the combined effect of a reduction in the number of participants and a decrease in the average level of talent among them would tend to reduce the flow of new scientific theories. Feed that back into the system we have been describing and the result is depressing. For, if the public knowledge pool becomes stagnant, technology suffers.

We have seen that the science-technology interaction is quasi-stable in the upward direction. If the growth of scientific knowledge rises, the rate of recruitment into science is kept in check as entrants go directly into technology. This implies that while the launching of a new highly progressive research program could initiate a science-technology boom, the boom cannot be expected to last even if the program's potential for generating new theories remained undiminished. But things are alarmingly unstable in the downwards direction. Should the exhaustion of a previously progressive program of scientific research diminish flow of new theories, causing a displacement of the system downwards, the whole process appears to slide to some low level equilibrium. Along that dismal path both science and technology appear less and less economically attractive in comparison with other fields of human endeavour until, presumably, enterprises engaged in technology find it worthwhile to finance research projects committed to public disclosure and so stabilize the system at some much lower level.

The implications of this are serious. It suggests that the growing dependence of modern economic growth upon the science-technology nexus has made the stability of this growth at acceptably high levels a hostage of what we would think are some quite extraneous features of the cultural and political environment. It is the taste for the lifestyle of academic science, the compatibility of research with teaching, and the persistence of public authorities in subsidizing science at a level to which none of the constituents would willingly subscribe, that prevents the collapse of the economic structure erected upon a high level of scientific activity. If the support is removed, the effects in our view would be quite disasterous.

Of course, the spontaneous appearance of a new program of scientific research could initiate a boom. The point though is that the waiting time between such occurrences will be lengthened if the resources commanded by science are allowed to settle at their low level equilibrium. Modern economic growth under those conditions would continue to be grounded in the exploitation of scientific knowledge, but it would lose the sustained character which has been taken by many to distinguish it fundamentally from the process of economic change in earlier epochs.

Footnotes

1/ A formal demonstration of the fact that this implies a non-convexity in the value of information under a wide class of circumstances is provided by the first example in Wilson (1975).

2/ Why this happened is less than obvious. An explanation is perhaps to be found in the fact that Arrow's 1962 paper was really a pair of essays packaged as one. The first part examined information as a commodity and inquired what economic theory has to say about its production and allocation in a decentralized, free enterprise system. The second part analyzed the effect of market structure upon the incentive to produce a cost-reducing invention, under conditions in which the structure of the market would not affect the inventor's ability to appropriate the benefits. The economics profession took longer to absorb the full implications of the first part of Arrow's essay, so that its second part for a time exercised the greater influence upon the literature--specifically, the literature devoted to studying the relationship of invention and innovation to the structure of markets.

This was a minor misfortune in itself. The mathematically formalized analysis in the essay's second part succeded in fixing theoretical attention upon the way incentives for R&D investment were affected by the structure of product markets. Without explicitly addressing the suggestion by Schumpeter that an existing monopoly position was an ideal platform for undertaking innovation, Arrow showed that "preinvention monopoly profits" *per se* constituted a comparative disincentive to invent. The conclusion drawn (Arrow (1962), p. 622) was that "the only ground for arguing that monopoly may create superior incentives to invent is that appropriability may be greater under monopoly than under competition."

Arrow's restatement of Schumpeter's hypothesis in these terms, stressing the possible appropriability effects of monopoly power, appeared as the natural adjunct to the main message which economists took from the essay as a whole: incomplete appropriability of the benefits of research and invention would result in the provision under a competitive system of a less-than-socially optimal allocation of resources for such activities. The theoretical contribution made in the second part thereby perpetuated the Schumpeterian tradition in which, by and large, the reciprocal influences of R&D performance upon market structure were ignored. See Scherer (1984), esp. pp. 59-65, 170-206, on research in this vein; Dasgupta and Stiglitz (1980a, 1980b) for treatment of market structure as endogenous. Dasgupta (1985) elaborates further upon these points.

3/ We are not defending this treatment, merely stating it as a fact.

4/ Witness the fact that university science departments continually produce doctorates in science on an average of five to six years of graduate studies with little by way of variance.

5/ We are ignoring important aggregation problems here. Of course, there are different <u>types</u> of knowledge. Nothing of importance is lost by our ignoring this issue at this point.

6/ By this we do not mean that science is not interested in applications. Nor that it is interested exclusively in knowledge for the sake of knowledge. Scientists regularly investigate phenomena with a view to applications. But science insists on the publicness of knowledge and is ultimately concerned with knowledge and its applications as consumption goods.

7/ It does not follow that all ought to be trained to use the knowledge, since training involves costs.

8/ That the monastic tradition in the West contributed greatly to the methodology of the "new science" of the 17th Century is well known. An important feature was the tradition of reproducing ancient texts and checking their accuracy. This established an association between the open transfer of knowledge and the contents of both ancient and Arabic manuscripts, which were the sources of such theoretical science as the medieval world inherited. One should note that the financial support of the monk-copyists within an institution that was in turn supported by the economic surplus of a rural society required dispersion of the repositories of knowledge. The tradition of open exchange of texts was important in widening the community of scholars and assisting their labours. We would argue that the financial support of the Church was important for the establishment of the rule of disclosure. Other social groups, such as the Sanskrit scholars of classical India, tended to be less generous in sharing their knowledge. These scholars were, generally speaking, obliged to provide for their own support by acquiring pupils and by performing religious rites.

9/ We are discussing an intricate matter in a rough and ready way here. There are complicated reward systems that can be devised under the incomplete information we have implicitly postulated in the text. The problem at hand is one that combines adverse selection (how does one ensure that the right people undertake the research?) with moral hazard (how can one guarantee that the scientists will not slack?). The problem of incentive compatibility has been much discussed in the recent economics literature.

10/ Contrast this with the socio-psychological explanation offered by Robert Merton: "... scientific *knowledge* is not the richer or the poorer for having credit given where credit is due: it is the social *institution* of science and individual men of science that would suffer from repeated failures to allocate credit justly." (Merton (1957), p. 648.) On the history of the priority rule see Boorstin (1984), Chapter 53. On Merton's "multiples"; that is, the more or less simultaneous discovery of a phenomenon (or a theorem) by more than one research unit, see Merton (1973).

11/ It is not unusual to think of patents as belonging to the realm of *techne*, not least because English and American patent law, as forerunners of modern patent laws elsewhere, made it impossible to patent a "fact of nature." This might suggest that a useful distinction between technology and science is to be found in whether the knowledge is patentable. The problem is that it is not self-evident what is a fact of nature. This was made clear in the recent litigation over the Stanford and the University of California at Berkely patents on recombinant DNA.

12/ Arrow (1962), p. 617, phrased the problem this way: "In a free enterprise economy, inventive activity is supported by using the invention to create property rights; precisely to the extent that it is successful, there is an underutilization of the information. The property right may be in the information itself, through patents and similar legal devices, or in the intangible assets of the firm if the information is retained by the firm and used only to increase its profits." For more recent, empirical evaluations, see Taylor and Silberstoon (1973); Mansfield et al. (1982), Ch. 7 and references therein.

13/ See also Dasgupta and Maskin (1985). We should add that this can be the case in science as well, provided that the reward to the discoverer under the priority rule is tempting enough.

14/ J. Bardeen and W. H. Brattain, "The transistor, a semiconductor triode," Physical Review, (15 July 1948) 230-1, acknowledged the help of William Shockley and others at Bell Laboratories. See Braun and MacDonald (1978), pp. 41-51 for this account, much of which draws on personal interviews. Nelson's (1962) account, written closer to the events and more from a Bell Labs viewpoint, dwells upon the creative "link between science and invention" and glosses over the tensions between them.

15/ The B-24 went on to become one of the most successful World War II bombers; 19,000 of them had been built when production was terminated in 1945--more than any other bomber designed in history. Vincenti (1985): n. 37, notes that although Davis signed an agreement with the Government in 1943 limiting his royalties on Davis-Wing aircraft bought by the U.S., after the War he sued the

U.S. for additional payments on their sales of war-surplus B-24's to private buyers. The Court of Claims, ruling in favor of the Government's refusal of these further royalties, declared the original patent to have been invalid because it required experimentation with the values of the constants to be used in the equations. Davis's right to retain the royalties he had previously received, however, does not appear to have been challenged; it seems doubtful that his patent would have been contested had he not sued the U.S.

16/ Patents are a means of obtaining both fame (through public disclosure) and fortune (through monopoly rents), but they are often an unreliable means for the latter, since rivals often invent around patents.

17/ There is in fact a particular set of circumstances where scientific knowledge, as we have defined it here, is capable of generating economic rents. This is when the knowledge in question is complementary in production with some resource that has been monopolised. The rents derived by owners of mineral resources from scientific advances in geology and organic chemistry are a case in point, where owners of resources will clearly pay for scientific theories. In a well-known article Hirshleifer (1971) recalls that Eli Whitney, the inventor of the cotton gin, died a pauper because of inability to establish a patent. He then remarks that Whitney could have avoided this fate had he purchased large tracts of land in South Carolina prior to announcing his invention.

References

Arrow, K.J. (1962), "Economic Welfare and the Allocation of Resources for Inventions," in R.R. Nelson (ed.), The Rate and Direction of Inventive Activity: Economic and Social Factors, Princeton University Press.

Boorstin, D. (1984), The Discoverers: A History of Man's Search to Know His World and Himself, Random House (New York).

Braun, Ernest and Stuart MacDonald (1978), Revolution in Miniature: The history and impact of semiconductor electronics, Cambridge: Cambridge University Press.

Brooks, H. (1967), "Applied Research: Definitions, Concepts, Themes," in U.S. Congress, House Committee on Science and Astronautics, Subcommittee on Science, Research and Development, Applied Science and Technological Progress, Washington, D.C.: U.S.G.P.O.

Dasgupta, P. (1985), "The Theory of Technological Competition," in J. Stiglitz and F. Mathewson (eds.), New Developments in the Theory of Market Structure, Macmillan Press (London) and MIT Press (Cambridge, Massachusetts).

Dasgupta, P. and E. Maskin (1985), "The Economics of R and D Portfolios," mimeograph, Faculty of Economics, University of Cambridge.

Dasgupta, P. and J. Stiglitz (1980a), "Industrial Structure and the Nature of Innovative Activity," Economic Journal, 90: 266-93.

Dasgupta, P. and J. Stiglitz (1980b), "Uncertainty, Industrial Structure and the Speed of R and D," Bell Journal of Economics, 11: 1-28.

Hirshleifer, J. (1971), "The Private and Social Value of Information and the Reward for Inventive Activity," American Economic Review, 61: 561-74.

Klein, B.H. (1962), "The Decision Making Problem in Development," in R.R. Nelson (ed.), The Rate and Direction of Inventive Activity: Economic and Social Factors, Princeton: Princeton University Press.

Kuhn, T. (1962), The Structure of Scientific Revolutions, Chicago: University of Chicago Press.

Lakatos, I. (1980), The Methodology of Scientific Research Programmes: Philosophical Papers, V. 1, J. Worrall and G. Currie (eds.), Cambridge: Cambridge University Press.

Levin, R. (1982), "The Semiconductor Industry," in R.R. Nelson, ed., Government and Technical Progress: A Cross-Industry Analysis, New York: Pergamon Press.

Mansfield, E., et al. (1982), Technology Transfer, Productivity, and Economic Policy, New York: Norton.

Medawar, S. (1982), Pluto's Republic, Oxford University Press.

Merton, R.K. (1957), "Priorities in Scientific Discovery," American Sociological Review, 22: 635-59.

Merton, R.K. (1973), The Sociology of Science: Theoretical and Empirical Investigation, edited by N.W. Storer, University of Chicago Press.

Mowery, D. (1983), "Economic Theory and Government Technology Policy," Policy Sciences, vol. 16, pp. 27-43.

Nelson, R. R., (1962), "The Link Between Science and Invention: The Case of the Transistor," in R. R. Nelson (ed.), The Rate and Direction of Inventive Activity: Economic and Social Factors (A Conference of the Universities-National Bureau Committee for Economic Research), Princeton: Princeton University Press, pp. 549-83.

Price, D.J. de S. (1967), "Research on Research," in Journeys in Science: Small Steps--Great Strides, David L. Arm (ed.), Albuquerque: University of New Mexico Press.

Rosenberg, N. (1982), Inside the Black Box: Technology and Economics, New York: Cambridge University Press, (esp. ch. 7: "How Exogenous is Science?").

Salomon, J.-J. (1973), Science and Politics, translated by Noel Lindsay, London: Macmillan Press.

Scherer, F. M. (1984), Innovation and Growth: Schumpeterian Perspectives, Cambridge, Mass.: M.I.T. Press.

Taylor, C. and Z. Silberston (1973), The Economic Impact of the Patent System, Cambridge: Cambridge University Press.

Vincenti, W. G. (1985), "The Davis Wing and the Problem of Airfoil Design: Uncertainty and Growth in Engineering Knowledge," unpublished manuscript, Stanford University School of Engineering, (Winter).

Wilson, R. (1975), "Informational Economics of Scale," Bell Journal of Economics, 6: 184-95.