Is Science a Public Good? Fifth Mullins Lecture, Virginia Polytechnic Institute, 23 March 1993

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Should governments accept the principle of devoting a proportion of their resources to funding basic research? From the standpoint of economics, science should be considered as a public good and for that reason it should be protected from market forces. This article tries to show that this result can only be maintained at the price of abandoning arguments traditionally deployed by economists themselves. It entails a complete reversal of our habitual ways of thinking about public goods. In order to bring this reversal about, this article draws on the central results obtained by the anthropology and sociology of science and technology over the past several years. Science is a public good, not because of its intrinsic properties but because it is a source of diversity and flexibility.

It is a great honor for me to be invited to give this fifth Nick Mullins Lecture. It is also an opportunity for me to escape, for a few moments at least, the tyranny of the disciplines. When reconstructing their own history, sociologists of science have generally not been very reflexive. Like other scientists, they list opposing schools, models, and ways of thinking. They talk in terms of breakthroughs, turning points, and ruptures. These differences are doubtless useful, allowing us to define ourselves through what we are opposed to. But they make us forget those who tried to link, to translate, and to create compatibilities. Nick Mullins was one of this tribe of mediators. He knew how to navigate between social networks and the dynamics of knowledge, between the qualitative analysis of texts and the quantitative occurrence of words. I only met Nick Mullins a few times, but in a way I feel very close to him. I share his impatience to see the continually reborn opposition between the qualitative and the quantitative. I share his impatience to see disciplines ignorant of each other, which would gain through cooperation. As

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a continuation of Nick Mullins's project, I have chosen as my theme today one that is at the boundary between economics and sociology—that of the progressive privatization of science.

Whether we like it or not, this question cannot be put off any longer. Scientists themselves are worried about it, as are we. During a session held on 28 October 1992 at the United Nations Educational, Scientific, and Cultural Organization (UNESCO) headquarters in Paris, Doctor Charles Auffray, research director at the Centre National de la Recherche Scientifique (CNRS), presented recent results obtained from the Human Genome Project. He announced the decision made by French researchers to offer their discoveries to the international community. The purpose of his talk was to "oppose some American initiatives to patent certain portions of the hereditary patrimony of the human race" (Nouchi 1992, 1). This is an exemplary episode. It demonstrates the increasingly open confrontation within the field of scientific research between two logics: that of disclosure and thus the free circulation of information and that of private property and thus the retention of information. Science is becoming important to economic interests and when the state intervenes, more and more often it takes the side of the latter.

This change and the controversies that go along with it constitute a fine topic for research in science studies. A number of studies would have to be carried out to understand better what is at stake in these confrontations: identifying the protagonists, charting their arguments, looking at the alliances into which they enter.

Today, I would like to follow another course and take some steps along a much more risky path. I do not intend to carry on business as usual in the sociology of science, but to look at some questions that we are all facing, questions that are difficult for us to avoid any longer and that would be cowardly to shelve. Should we accept the privatization of science or not? Should we defend at all costs the idea of a science accessible to all, circulating freely? Or, on the contrary, should we celebrate the fact that business, which for such a long time has stood accused of taking no interest in research, is finally recognizing its importance?

We need to look at these questions in a context wider than that of economic efficiency alone. Science constitutes an important element of our cultural life, and we would find it difficult to accept private interests taking it over completely. However, my topic today will be somewhat more restricted. I will limit myself to examining and discussing the economic arguments that might be advanced in favor of the support of science by the state. Should governments accept the principle of devoting a proportion of their resources, that is, our resources, to funding basic research? And if so, what are the grounds—in particular the economic grounds—for such a decision? My answer to the first question will be an unambiguous yes. From the standpoint of economics, science should be considered a public good, and for that reason it should be protected from market forces—if only to ensure a better operation of the market. However, I will try to show that this result can only be obtained at the price of abandoning arguments generally advanced by economists themselves. It entails a complete reversal of our habitual ways of thinking about public goods, and a new definition of them. In order to bring this reversal about, I will draw on the central results obtained by the anthropology and sociology of science and technology over the past several years.[1] This paper will discuss the nature of this reversal and its attendant consequences particularly in the political and organizational realms.

Science as a Public Good in Economic Theory

Let me start from the analysis proposed by political economists. Generations of students have learned that science is a public good. This principle has been embraced by economists of all shades. It has inspired science policymakers. It is supported by scientists themselves and even seems to fit in with the commonsense view. The argument usually developed by economists comprises three propositions: (1) Scientific knowledge has a certain number of intrinsic characteristics that make its complete transformation into a commodity impossible; (2) as a result, market mechanisms cause business to underinvest in scientific production; (3) to redress this market's failure, governments should stimulate investments both through direct intervention and through incentive schemes.

I will start, then, with the economic definition of science as a public good, or rather as a quasi-public good, and then go on to draw a certain number of conclusions. There are two parts in the demonstration: science as a good and then science as a quasi-public good.[2]

Science as a Good

First, scientific knowledge is assimilable to the class of goods. It is difficult to say here exactly what is meant by the word *goods*. One could just as well call it a thing. The main point is that scientific knowledge is endowed with a physical nature to the extent that it can circulate, be exchanged, or be engaged in commercial transactions. Being a thing, scientific knowledge can even be stolen. This materialism might seem vulgar and shocking but it is, nevertheless, perfectly defensible. In order to make it consistent and robust, economists use the concept of information. As I will show below, their reductionist conception of information is a first step toward a better understanding of science as an economic activity. According to Dasgupta and David: "Information is knowledge reduced to messages that can be transmitted to decision agents. We take the standard information- theoretic view that such messages have information content when receipt of them causes some action" (Dasgupta and David 1992, 9).

This definition has two important elements. First, the idea of a message presupposes a material base. A message might be one or a set of written or oral statements. It might also be something that is inscribed in a human being, a substance, a machine, or a product. A piece of information is knowledge put into a certain form, that is, inscribed into a more or less durable base that can be transmitted. I will develop the consequences of this fundamental idea later. For the time being, I just want to set out the position of many economic theorists.

Whatever means are used to transmit it, this message is not considered to be information unless it leads to an action, causes someone to act. The statement: "DNA's structure is a double-helix" is only information to the extent that it has a use value for the person receiving it. Knowledge that has not been transformed into information is of no interest to the economist, because it does not exist in a form that allows circulation and exchange. It is not a thing, a good that can be mobilized, and it cannot be transformed into a commodity.

Information can take very different forms. For example, Dasgupta and David propose readopting the classical distinction between explicit and incorporated knowledge. Explicit knowledge is also referred to as "codified knowledge," which is "expressed in a format that is usually standardized and compact, so as to permit easy, low cost transmission, verification, storage and reproduction" (Dasgupta and David 1992, 9). The archetypes of codified knowledge are obviously statements made in ordinary language: "DNA has a double-helix structure" or "the sun emits neutrinos." But many codes are in use. Each discipline has its own language: mathematics and sociology, for example, do not share the same code.

Incorporated knowledge stands in contrast with codified knowledge. As its name indicates, incorporated knowledge can be inscribed in human bodies (scientists, technicians, etc.) or in instruments or in machines. This knowledge takes the form of savoir faire, know-how, rules of thumb, and technical automata—all of which play an essential part in the interpretation of results and the setting up and conduct of experiments.[3] This involves looking on science as a craft activity. If one wants to play the piano, bore through metal, or resolve a partial differential equation, it is not sufficient to know how it is done; one needs to have incorporated the gestures and the know-how that cannot be reproduced simply by being made explicit. Indeed, no articulated description can exhaust the content of these gestures.

Codified statements, bodies, machines, substances are a few of the messengers that are put into circulation and that make those who appropriate them act. Whether codified or incorporated, knowledge can be treated as a good. Texts, scientists, samples, and measuring instruments can all be exchanged, stolen, dissimulated, or lent out.

We now need to ask under what conditions knowledge can be transformed into a commodity that can be exchanged in the marketplace. It is here that the concept of public good must be introduced.

Science as a Quasi-Public Good

The notion of public good was introduced into public finance to justify possible interventions by governments into economic life. It is based on the idea that any good has intrinsic properties linked to its form or its physical characteristics, and that these properties determine its ability to become a marketable product for the purposes of commercial transactions. This treatment of goods generally operates along two dimensions: excludability and rivalry. As we will see, there are other possible characteristics—also inherent ones—but the most important property is that of rivalry or nonrivalry.

a) To decide whether scientific knowledge that has been previously reduced to the state of information can be transformed into a commodity, one needs to ask first whether it can be appropriated or not. Indeed, for something to be a commodity it must be possible to transfer property rights. A good is appropriable (or exclusive) if it is possible for the person using or consuming it to prevent any other potential user or consumer from doing the same; otherwise it is nonappropriable (or nonexclusive). Is scientific knowledge an appropriable, exclusive good? In other words, if A sells information to B, is B then assured of enjoying the exclusive use of that information?

Economists qualify their treatment of this issue. Ease of appropriation appears to depend on the material or the base in which the information is inscribed. The more information is encoded in texts the harder it is to appropriate; the more it is inscribed in human bodies or technical artifacts, the easier it is to assure its excludability. For example, it is easier to duplicate a statement than to duplicate a mathematician or a sophisticated instrument whose blueprints are locked away in a safe (Romer 1992). However, this difference is by no means absolute or intangible. A code might be more or less widely shared, more or less easy to break. It is well known that seventeenth-century scientists sometimes encrypted their results in order to protect themselves in future priority disputes without divulging their results immediately. Thus, in 1610, Galileo sent the Tuscan ambassador in Prague the following anagrammatic announcement of his discovery of Jupiter's three moons: SNAUSNRNUKNEOIETAKEYNUBYBEBYGTTAYROAS (Biagioli 1990). Or, closer to home, one might recall the work of mathematicians and computer programmers to code messages during the Cold War. The choice of a code for which many hold the key, itself constituted as a public good, is not a necessary feature of science: it is a decision, not an inevitability.

From this we will need to retain the following conclusion: even in the absence of formal rules, that is, in the absence of intellectual property rights, a complete appropriation is possible because the producer can choose the substrate that facilitates it. The qualification of science as a quasi-public good rather than as a full-fledged public good derives essentially from the fact that it is to a certain degree appropriable—whereas in standard theory a true public good has to be completely inappropriable.[4]

b) The second attribute of a public good is nonrivalry. A good is rival when A and B compete for its use. "You can eat a fish, or I can, but not both of us" (Romer 1993, 354). A good is nonrival "because once it has been produced, A and B are not rival for its use. I can listen to the musical recording or take advantage of the software code without in any way diminishing its usefulness to you or anyone else" (Romer 1993, 354). From an economic point of view, the property of nonrivalry is essential. It means that the good's production costs are fixed: once the good has been produced, there is no need for continuing investment because there are no production costs in replicating it. In terms of economic theory, science—taken here as the production of codified statements—is a prototypical nonrival good. This is a consequence of the equivalence introduced between scientific knowledge and information. If I tell you that I have heard from a good source that the French air traffic controllers are going to go on strike next week, you will be able to use that information without preventing me from continuing to use it. In the same way, if I tell you the formula for a growth hormone, I will still be able to use that formula. Putting you in the know, giving you that information, I am not thereby depriving myself of its use. Thus Piet Hut and John Bahcall might at the same time be writing the same equation linking the fate of two galaxies, whereas John Bahcall cannot be going to Trenton in the same Ford that Piet Hut is at the same time driving to Edison.[5] This property applies also to skills incorporated in human beings. Mobilizing the skills and techniques of an expert does not prevent another expert from mobilizing the same skills and the same techniques at the same time.[6] As I will show a little further on, this reasoning, even when applied to codified statements, that is, to explicit information, does not hold water but it does appear on the face of it to make sense. Economists generally make a lot of this characteristic—if it does not hold, then science can be transformed into a commodity.

c) Scientific knowledge is said to possess two further characteristics that are important from our point of view. First, it is a durable good, not destroyed or altered by its use. Even better, the more it is used the more its value increases because it proves its fecundity, widens the scope of its applications, and becomes richer. Second, the production of knowledge is uncertain: in the most extreme cases it is impossible to predict either results or their usefulness.

Let me summarize the economists' arguments. In the absence of regulations, scientific knowledge is a difficult-to-appropriate, nonrival, and durable good. Its production is surrounded, in at least certain cases, with deep uncertainties. For an economist, this set of properties defines a public good, or rather a quasi-public good, because not all the conditions are completely satisfied. The production of a good, which by its intrinsic properties has the status of a public good, cannot be guaranteed at an optimal level in the marketplace: industry and business underinvest in scientific production. To make up for this market failure, government has to intervene either directly or through an incentive system.[7]

The Contribution of Sociology and Anthropology of Science: Science Is Not a Public Good as Defined in Economic Theory

Economists have displayed much skill in their attempts to prove an incompatibility between science and the marketplace. They do not discuss this issue in terms of ideological preferences. Science does not have to be defended against hypothetical threats from the marketplace because its very nature protects it from the market and its excesses. It has to be supported because the market is not sufficiently interested in it. The problem is that the news from the front that is streaming in every day casts doubt on this seductive argument. Public laboratories are one after another falling into private hands, either directly through takeovers and cooperative arrangements or indirectly through incentives and research programs. The thesis of underinvestment in research is becoming more and more difficult to support.

If the doctrine is unable to account for this dramatic evolution, it is because it is based on shaky hypotheses. Recent results in the sociology of science and technology make it easy to show that there is nothing in science to prevent it from being transformed into merchandise.

I will first reaffirm the materialist position adopted by economists. Science is a thing, or rather a set of complementary things: it does not exist outside of the diverse materials in which it is inscribed. The anthropology of science has gone even further in the description of these materials and their variety. Information is codified in articles and books, but also in patents, proposals, grants, reports, and more generally, in what Latour and Woolgar (1979) have called inscriptions. Furthermore, this anthropology has laid more emphasis than economics on "the importance of specific complexes of instruments and specialized materials, and the skills and techniques needed to utilize them" (Rouse 1993). In order to visualize this, try the following experiment adapted from Herbert Simon. Imagine coloring theoretical statements in red, and all other inscriptions and skills incorporated in human beings and instruments, in green. A Martian contemplating our science from its planet would see a vast green ocean streaked with very occasional and fragile red filaments.

But economists, who are justifiably materialist in their definition of science, then adopt a strangely idealist vision when dealing with the issue of nonrivalry. Take the case where this thesis seems incontestable: that of codified statements.

One of the first—and maybe one of the only—results of social studies of science has been to show that an isolated statement or theory is quite simply useless. You might print thousands of copies of an article or a book and air-drop copies in Lapland or in Bosnia-Herzegovina. You might similarly send well-trained students or well-calibrated instruments to the far corners of the earth. However, if all these elements do not come together in a single place at the same time, then the dissemination will have been a waste of time. Nobody will adopt the statement; the skills will not have any object to which they can be applied; the instruments and the machines will remain in their boxes. I cannot resist telling you the following anecdote, not borrowed from the sociology of science, but that brings out the necessity of this complementarity.

On 7 May 1992, following up on the Los Angeles riots, Reuters sent the following dispatch: "It is reported that a rioter who could not work out how to use a VCR he had stolen during the riots took it straight back to the police." This fable, which clarifies what, following Austin, we could refer to as the conditions of the felicity of usage of technology, can be applied perfectly well to science and its statements. You will never see a mafioso carrying out a holdup in a theoretical physics laboratory. If Watson was able to remove the X-ray diffraction diagrams from Franklin's wastepaper basket, they were only useful because Crick was there to decipher them. I propose calling this thesis *the thesis of the intrinsic inutility of statements* (the thesis can be applied equally to skills and instruments). It is quite simply a consequence of Harry Collins's (1974) fundamental work on duplication. If I were not totally opposed to prizes being awarded for individual work, I would suggest that we call it Collins's Law. What he successfully demonstrated—contrary to what he sometimes affirms—was not so much the thesis of experimenters' regress as the impossibility of endowing a statement with any meaning if the work of the duplication of skills and instruments has not been done. In other words, it is impossible to mobilize the different elements independently of each other.

From the point of view of my topic today, that of economics, these results have far-reaching consequences. Economic theorists tell us that if A uses statement E, then the latter is not damaged by the fact that B also uses it. That is true, but only in exactly the same way as I can go out in my Ford Taurus, registration number BCD109876, without being inconvenienced by Mr. Brown going along in the same Ford Taurus, but this time with registration number BCD109877. A statement as used by A is neither more nor less similar to one used by B than one Ford Taurus is to another—or one tower of the World Trade Center to its twin. Two similar statements used in two different situations constitute two different goods, whose use and implementation presuppose specific investments. Science, even in its most codified forms, cannot therefore be considered a nonrival good (for more details see Insert).

Insert: Is Science a Local Public Good?[8]

Some economists might respond to this analysis that it is not necessary for a good to be accessible at zero cost in order to be nonrival. To take into account the investment and accumulated capabilities (skills, equipment, learning processes, and more generally, complementary assets) necessary to make use of a nonrival good, all one needs to do is to introduce the concept of a local public good (Cornes and Sandler 1986). Thus a statement can be considered freely available to members of the community of specialists. For all those who have made the necessary investments, the statement is a nonrival good. This property distinguishes the statement from other common goods. The use of a Ford Taurus, just like the use of a statement, necessitates complementary investments (road infrastructure, learning how to drive). But once these investments have been made, it remains a rival good. If they actually want to use a car, any person who knows how to drive, has a license, and has access to the road network has, over and above these, to lay out an amount of money corresponding to the marginal cost of producing another Ford Taurus. What distinguishes the statement from a Ford Taurus or a cigarette is that its reproduction cost is negligible. Thus one has to distinguish between free use of a good (its being without cost) and its availability (being "free").

To demonstrate the superficiality of this standard argument, consider the complete cost of the process leading from the production of a statement to its effective use (by way of its reproduction) instead of making an arbitrary distinction between costs associated with supply and those associated with demand. Let us put ourselves at the point where the codified statement S^1 has just been produced by A, and let us call the investments necessary for its elaboration I(0).[9]

Let us consider some actor B (or C) who wants to use S^1, and let us reconstitute the various investments that have to be made in order to make this use possible. These investments fall into four categories:[10]

1. *Investments in the reproduction of S^1* and in its delivery. Let us call the resultant statement S^1_2. S^1_2 is formally identical to S^1, but it is inscribed in a different material base; its delivery puts the statement in B's possession. Call these investments I(1). Compared to reproduction costs of goods like Ford Tauruses or Marlboros, I(1) for a codified statement is, with reason, generally considered negligible.
2. *Investments in complementary assets.* In order to give meaning to S^1_2 and to be in a position to use that statement, B has to acquire embodied skills, know-how, instruments. He or she must also be able to mobilize other statements, without which S^1_2 would remain a meaningless statement and which themselves call for complementary investments. Let us call these investments I(2). This amount varies according to one's field (it can be a criterion for distinguishing between big and small science) but it is always significant.
3. *Investments in maintaining complementary assets, without which these investments lose their utility or their pertinence.* Let us call these investments I(3). They serve, for example, to maintain the physical and intellectual skills of researchers, engineers, and technicians; and to ensure the proper operation and replacement of instruments, libraries, databases, and metrological networks. Their amount varies by field, but this cost can never be considered null or negligible.

   An actor B who has agreed to make the investment $I_1 = I(1) + I(2) + I(3)$ is, at time t, in a position to understand S^1_2. This amount, then, represents the price to pay in order to be able to vest significance in that statement. If B did not go beyond these first investments, he or she would be in no position to engage S^1_2 as a resource in any production process.

4. *Investments in the mobilization of $S^1{}_2$.* This corresponds to the investments required to introduce $S^1{}_2$ as an input in a production process whose output may be other statements S^2 (in the case of academic scientific research), a piece of technology, or a finished product. These investments, in turn, demand the acquisition of instruments, machines, incorporated skills, and, very often, other statements accompanied by complementary investments required for their use. B constructs a new configuration. Without that, he or she could not produce anything at all different from what is already there. Let us call these investments I(4). They can be very large—a much higher order of magnitude than I(2) and I(3), which themselves, as we have seen, are already very high. This amplification is well captured in the following formula: an investment in basic research of 1 unit leads to an investment in applied research of 10 and an investment in development of 100. I(4) justifies the now widely accepted assertion that knowledge cannot be applied without being transformed—this transformation results from its insertion in a new productive configuration. With I_1, but without I(4), B can repeat $S^1{}_2$ ad nauseam. He or she can understand it and verify its meaning but is condemned to parroting it. $S^1{}_2$ has no use value without I(4).

From the above, it follows that, in order to become an economic good susceptible of being mobilized in consumption or production activity, a statement must be accompanied by a series of investments [I = I(1) + I(2) + I(3) + I(4)] without which it lacks any use value. Abandoning the rigid distinction between costs associated with supply and those associated with demand—a meaningless distinction in the case of a continuous process where the user, as with any service, participates in the coproduction of the good that he or she "consumes"—I measures the global costs of the transformation of a statement into a complete economic good. In order to be relevant, the classification of economic goods must therefore be anchored in a comparative analysis of global costs, instead of just summing the costs of the reproductive operations alone. In the case of the statement, I(1) is low, but I(2), I(3), and in particular I(4) are high. For the Ford Taurus, the cost profile is markedly different. I(1) is high, I(2) and I(3) are middling, and I(4) can be considered negligible. The global cost of the transformation of a cigarette into use value is different yet again: I(1) is high, I(2) and I(3) are low, and I(4) is null. These structural differences demonstrate the flaw in concentrating on one particular link in the chain of costs, instead of taking them as a whole. Asserting that an isolated copy of a statement has a use value is like saying that a photograph of a cigarette provides as much satisfaction as the cigarette itself! It is only possible to say of statement S that it constitutes an intrinsically nonrival good when one reduces the chain of integral costs to those investments necessary to producing a (photo) copy of the statement. Now the property of nonrivalry, which holds only for the very few who have borne $I = I(1) + I(2) + I(3) + I(4)$ (and which in the case of science or technology constitutes the community of specialists), is the result of a series of strategic (investment) decisions taken by those actors. It is in no way an *intrinsic property* of the statements themselves: it would be better to call it an *extrinsic property* and to consider variable degrees of (non)rivalry.[11]

Is scientific knowledge by its very nature inappropriable? Does it possess the attribute of nonexcludability? First, economists themselves, as we have seen, have qualified their response to this question. They consider the degree of appropriability or nonappropriability as strongly dependent on the form of the knowledge, that is, on the choices made by its producers: incorporated skills are easy to appropriate, codified information more difficult. Second, (non)appropriability appears much more context dependent when we take into account the intrinsic lack of utility of statements. These can only be of interest to a restricted circle of potential users: the few scientists who have the necessary savoir faire and access to the necessary instruments. Further, scientists worldwide know through their experience that the difficulty lies not so much in preventing their colleagues from reading what they write, but in convincing them that they should read it. In fact, what is striking about scientific knowledge with respect to other goods is the ease of its appropriation and the amount of work needed to create a situation in which other actors are interested.[12]

Nor does its supposed durability withstand closer scrutiny. Scientific knowledge is indeed durable, but only at the price of the heavy investments needed to maintain it. In order to make the law $f = ma$ available in Singapore in 1993, a large number of textbooks had to be published and sold, teachers had to drum the message into stubborn heads, research institutions and enterprises had to develop, researchers had to be trained and paid. Compared to the cost of maintaining a so-called universal law, the cost of maintaining the American army in Kuwait pales into insignificance.

Does the uncertain character of knowledge production lead to an underinvestment that discourages actors with risk aversion? Not at all. All studies of innovation in businesses bring out the degree of uncertainty involved. Contrary to what many believe today, uncertainties about the state of the market are of infinitely greater magnitude than uncertainties about technology. Yet firms continue to invest and do not hesitate to take very great commercial risks. Rather, as the Japanese case shows, what is needed is for the state to force companies to shoulder their own responsibilities: this will result in heavy industrial investment in research and development (R&D).[13] Compared to the uncertainties of the marketplace, the uncertainties of science are a bagatelle—and a cheap one at that.

Through mobilizing some elementary findings from the sociology and anthropology of science—that of the multiplicity of the material supports for knowledge and that of their necessary complementarity—I have succeeded in completing the work begun by economists themselves, who, however, have not dared to bring it to its conclusion because of their wish to defend the independence of science. Scientific knowledge does not constitute a public good as defined in economic theory. The private or nonprivate nature of science is not an intrinsic property. Degrees of appropriability and of rivalry are the outcome of the strategic configurations of the relevant actors, of the investments that they have already made or are thinking of making.[14] Insofar as they both can be seen as commodities, there is no difference between a Ford Taurus and the general theory of relativity. In other words, without institutions that have been created and reinforced over centuries, without intense energy invested by scientists and the state to make scientific knowledge public, the theory of relativity would have never ceased being what it has always been: a potentially privatizable good, no different from other goods.[15]

Private Science: Irreversibility and Convergence

Why continue to spend a lot of money to maintain something as a public good when all it wants is to be absorbed by market forces? It takes a significant amount of money to divulge and circulate scientific information, and to invest in complementary assets (training, equipment, etc.) to make knowledge mobilizable.

Should we not just let the market go and move over to a decentralized allocation of resources? In order to examine the consequences of such a decision, let me try one of those thought experiments so dear to physicists. Imagine that science is completely privatized, that its production is assured exclusively by for-profit organizations.[16]

Recently, economists have carried out studies that allow us to anticipate the consequences of such a situation.[17] In their analysis of the relationships between technical change and economic competition, they have in effect uncovered two essential phenomena. The first is that of increasing returns and the second is that of cooperation.

The concept of increasing returns can be summarized in one sentence: the more a technology is produced and offered in the marketplace, the more it becomes interesting for the supplier to produce it and for the user to consume it. Increasing returns are the outcome of two mechanisms. The first relates to supply and is a result of what economists call learning. This takes various forms: learning by doing, by using, or even by interacting. It is by applying and using knowledge in all its forms (statements, machines, skills) that new ideas appear, new statements are produced, skills evolve, and machines are transformed. Learning renders production procedures more performative and products better adapted. The second source of increasing returns is linked to the sociotechnical environment that emerges progressively alongside demand. Certain—ever more numerous techniques—give rise to so-called network externalities. The value of products for the user increases with their diffusion. It is better to be the ten-millionth purchaser of a fax or a telephone than the first. More generally, when a piece of technology is distributed, the skills necessary for its use become more common and easily available. Adjacent techniques come along to make its use easier and to enrich it. A car without gas stations and distribution networks, without petrol tankers, without U.S. foreign policy, without the Gulf War would very quickly lose its usefulness. In the same way a computer becomes more attractive the more varied and available its programs and peripherals are. The construction of this sociotechnical environment takes time, but once it is in place it also generates increasing returns of adoption.

The law of increasing returns means that the conjunction of scientific and technical knowledge and the marketplace lead to the creation and consolidation of acquired advantages. The more investments there are, the larger the market becomes and the more varied the interests that work together to develop the same technoeconomic trajectory. This produces lock-ins—irreversible situations. From an economic point of view it is increasingly less profitable to return to options that were previously abandoned. Scientific and technical knowledge suffers under a grave injustice: that which has prospered will be rewarded, that which has been unable to develop will fade away completely. Paul David (1994) has developed the concept of QWERTY economics to designate this radical injustice. You might hate the QWERTY keyboard, but you have no other choice than to use it. The possible world in which you have access to some other keyboard has quite simply disappeared—as irreversibly as Kanak culture disappeared after French colonization. Through the grace of increasing returns—that strange conspiracy between technology and the marketplace—we live in a world of products that others have chosen for us, without knowing that they were making any choice. This is what economists call path dependency: the first decisions predetermine the trajectory (Arthur 1989).

After increasing returns, a second phenomenon must be taken into account: that of cooperation. In order to explain it, we must deal with two elements. The first is that of sharing costs and uncertainties. As I have said, firms have for a long time known how to manage uncertainty by coordinating and cooperating. In the domain of science and technology this translates into a bundle of relationships with universities (such as agreements, recruitment of students, common laboratories) (Etzkowitz and Peters 1991) and into interfirm agreements (knowledge exchange, common research centers, etc.) (Lundvall 1992). In general, and particularly in cases of cooperation between universities and industry, scientists' right to publish is recognized. But this right is a result of a politics of exchange and of pooling of knowledge between partners who dispose of a monopoly over the investments necessary for the use of this knowledge. So this science, which seems to be a public science, is nothing other than a private good shared between several owners.

The second element to take into account is tied to what economists call the complementarity of assets. This is a horrible phrase, but one that describes a fundamental phenomenon. A given scientific and technical resource has no intrinsic use. It must be associated with other scientific and technical resources (this is more and more apparent in fields like bio-optics) and also with production units, commercial distribution networks, financial strategies, and so on. A veritable collective machinery is required to give knowledge a use or economic value (Teece 1988).

Taken together, the iron law of increasing returns and the ineluctability of cooperation leads to two major consequences: first, science and technology—whether they are codified or incorporated—are endogenized, absorbed into the economic system; and second, control is exerted within collectives made up of firms, university laboratories, and captive users who make up what I have called flexible technoeconomic networks. These networks subsist and evolve relatively autonomously, following their own trajectories (Callon 1992, 1993).

These brief considerations cast light on the thought experiment that I suggested earlier. In a regime of perfectly privatizable science, science would be privatized so rapidly and so brutally that it would become a captive of the technoeconomic networks I have evoked. And there would be a double movement toward irreversibilization and convergence.[18] Irreversibilization would occur because the economic actors would follow the natural path of increasing returns together and so would continue along trajectories selected by chance. The convergence—or more precisely, stabilization of technological variety—would occur because they would share the same knowledge, the same generic technology, and they would only be differentiated in minor ways.[19]

In such a configuration, the boundary between a science that divulges its results and one that assures their confidentiality is a result of private strategic decisions that may lead to its transformation into a local public good, which we have seen as one possible mode of privatization (see insert)[20] What is called public science is merely an adjunct of private science. From this perspective, state support, even when directed to publicly divulged science, can easily be interpreted as aid for actors who, for strategic reasons (risk sharing, cooperative agreements for the purposes of profiting from complementary assets), have preferred to make a fraction of the knowledge that they produce nonrival and nonexclusive.

This logic of private science underlines a new kind of market failure that is much more serious than all the others. In this scenario, the market transforms itself into a powerful machine for constructing irreversibility and limiting the variety of technological options or the range of possible choices. It is not the market that is endangering science here, it is science that is paralyzing the market. A surfeit of market smothers the market.

How should we analyze and react to this diagnosis? There are three possible attitudes.

First, we could let science become a private good again and celebrate increasing irreversibility and convergence that render investments more effective and make them yield greater returns. I would challenge this attitude on the grounds that it does not deal with the question of variety. In my opinion—and here Leibniz says exactly the same thing—a world in which there is a great diversity of technology and goods accessible to as many people as possible is better than a world with less diversity.[21]

The second attitude is to cling desperately to the old idea of science as a public good while accepting the investments necessary for science to remain and/or reshape itself as a public good. This could be done by making its appropriation as costly as possible. It is easy to show that in this case the spring of competition is broken, because no monopoly, even a temporary one, can be envisaged. Not enough market kills the market. The compromises advocated by some involving peaceful coexistence between public and private science lead to divergence, as coordination between the two is not assured—and if it were, then we would be back in the realm of private science.

The third position is one that I myself would adopt. Drawing on social studies of science, I define science as a source of variety and flexibility so as to escape, in part, from its commodification and to open the way for a renewal of the economic definition of a public good.

Science as a Source of Variety and Flexibility

In my discussion of science as a public good, I took it for granted that it was possible to reduce scientific knowledge to the status of information. Now we have just seen that such a definition leads to a dead end. It restricts one's choice to two possible outcomes, both of which are highly problematic. One could reinforce institutions that at great cost would transform science into a nonrival, nonappropriable good. In this case, however, the economy would be ground to a halt. Or one could, inversely, ensure that science achieves the status of a rival and appropriable good, but in this case one would condemn the economy to less variety and to making choices irreversible.

To avoid this impasse, we need to abandon the notion of information and use that of network in its place. In effect, the main result of scientific activity is not to produce information but to reconfigure heterogeneous networks.[22] From this point of view economic and sociological analyses are complementary. The economics of technological change has made great strides in understanding how the market attached to technology has produced irreversibilities and impeded the increase of variety. For its part, the anthropology of science has enabled us to understand the production of variety as well as the progressive irreversibility of choices. By linking the two approaches, it is possible to envisage a dynamics in which market irreversibilities are perpetually counterbalanced by science. Science as information is either absorbed by the market or is opposed to it. Science as a network, and as a source of variety, fights against market rigidities and in that struggle in some cases ends up tied to the marketplace.[23]

Networks and Networks . . .

The networks I am talking about must not be confused with the technical networks of engineers (e.g., the electronic superhighways that Mr. Clinton wants to develop) nor with the social (friendship, specialist, confidence, reputation) networks of sociologists, nor for that matter with the networks of statements or texts that philosophers or specialists in discourse analysis love so much. Mine are a hybrid of these three forms of networks (Callon 1994).

If, for example, I wanted to talk about the network of Einsteinian physics (always assuming that it is unified in some way), then I would include articles, books, and textbooks that present and circulate its constitutive statements. But I would also include the equipment and machines in which it is inscribed, and skills embodied in human beings (physicists, chemists, and also college students who are struggling with Lorentz transforms). To this we should add the institutions that support and develop it: laboratories, government agencies, universities. This is today a sprawling network—admittedly fragile, but extended. Here and there it mingles with other networks, such as, for example, that of Newtonian physics, with which it shares the same human bodies. (What physicist, or even what college student, is not capable of shifting between the one and the other as needs be?) The two networks together make certain machines hold together and function. They commingle in that they form successive chapters of the same textbooks.

Such networks can be found in all fields and all disciplines. A striking example of the variety and heterogeneity of elements that can be associated is given by Robert Friedman (1989) in his description of the network of meteorology. In that network, he juxtaposes military and civil aviation, departments of fisheries and agriculture, planes that collect data, standards that assure the coordination of measurements, and calculations and models that make predictions.

Dynamics

This leads to the following question: how do networks come into existence and how do they extend themselves? In other words, how can one describe their dynamics in terms that describe simultaneously the creation of variety and the operation of processes of irreversibility? The response to these difficult questions can be found in works carried out by sociologists and anthropologists of science over the past few years. It can be summarized in two related concepts: the concepts of local and extended reconfiguration.

Local reconfigurations. Let me begin with the process of local reconfiguration that allows us to abandon the fuzzy notion of "laboratories."[24] The reconfiguration of networks—that is, the production of new statements, the development of new instruments, or the elaboration of new skills and techniques—operates within groups with restricted membership. These groups' frontiers are fairly well-defined; they are either smaller or larger than a laboratory and are only rarely coextensive with one.

We are beginning to have enough descriptions of these collectives to be able to draw more general conclusions about them. First, one of their most striking characteristics is the diversity of elements that these collectives draw together. Therein one can find articles already complete or in the process of being written, technicians, researchers, managers, specialists in industrial property, engineers on secondment from business, machines and instruments, Ph.D. students, samples, lists of numbers, or frozen organs that circulate from one lab to another. Panathenian festivals with their long trails of citizens, metics, maidens, ephebes, animals, cavalry, and chariots constitute a less motley crew than those that mingle together in these collectives.

Second, each of the elements drawn into the collective plays an active role. It interacts with the others. The chromatograph produces diagrams of which the technician takes a selection that he or she passes on to the research scientist who takes a look at some article and then carries out a calculation that he or she converts into a draft article—do not quote or circulate—which is then passed on to a colleague, whose convictions are weakened thereby, and so on. The nature of these interactions, the sequences of the elements are as varied as the local collectives. Here a machine is the principal actor, and everything depends on the inscriptions it produces. There, the researchers and the rats that they sacrifice are the main agents. Elsewhere, phantoms circulate from one laboratory to another, unifying practices and diagnoses, or equations proliferate and engage mathematicians, almost against their will, in new lines of inquiry.

These interactions modify and transform the entities concerned and make new ones appear in the form of statements, instruments, skills, beliefs, and substances. Thus it is appropriate to talk about the process of the production of scientific knowledge as the work of reconfiguration (Knorr-Cetina 1992) or as the mangle of practice (Pickering forthcoming).

The reconfigurations produced by these collectives depend for the most part on elements that are brought together, on the local culture that they constitute. Problems posed, deciding between giving preference to experiments or theories, favoring certain types of explanation, aversion to or, on the contrary, interest in applications obviously depend on the identity of the elements making up the collective and on the organization of their interactions. Change the composition of the collective, and you change the content of its productions. For example, Peter Galison (1987) has argued that some basic concepts of particle physics were altered by the use of counters in the 1930s; they transformed electrons from an aggregate to an enumerable concept. As Simon Schaffer (1992) and Freeman Dyson (1992) have shown, if one changes astronomers' instruments, then the description of the behavior of planets changes. This principle does not apply only to instruments. If you introduce new texts or new embodied skills into collectives, then the reconfigured group will move in new directions. From this point of view, I must pay homage to Mullins, who first linked the form and composition of these local collectives to the dynamics of the content of their productions (Mullins 1972).

The more numerous and different these heterogeneous collectives are, the more the reconfigurations produced are themselves varied. The source of this diversity is the multiplicity and diversity of these local cultures. A lot still needs to be done to cast light on these cultures, which are mixtures of the particular and the cosmopolitan. They are similar to the collages that Clifford Geertz (1986) talked about. It may be that some of these collectives, like some species in the Amazonian forest, are under threat of extinction.[25]

Intermediaries and extended reconfigurations. To understand how the irreversibilities created by the market can be threatened by the production of diversity, I need to explain how local collectives succeed in coming to grips with existing irreversibilities, and I must also explain how they extend the variety that they have created outside their own milieu. I want to understand how these restricted, local reconfigurations expand so that in the end they can sometimes reconfigure entire, long networks. Here again, the anthropology of science points the way to an answer to this important problem. The answer can be found in the concept of an intermediary.

Each element drawn together in a local collective refers to other elements that it represents and that are present, through it, in the collective. The polysemy of the concept of representation needs to be treated with respect. In a local collective, the electron microscope manifests a whole network of other microscopes, experts, observation routines, more or less stabilized rules for the interpretation of slides. Scientists themselves in the collective represent a whole network of colleagues who have read the same articles, taken the same courses, attended the same conferences, been trained to use the same instruments. The same thing holds for the statements they mobilize that refer to other statements, and also to other collectives that use them, as well as to the instruments and savoir faire they are associated with. Grants with a few well-protected requirements can ensure the presence of the state and its will. Agreements with firms or trade associations introduce these latter into the local collective. This collective seems to be confined within these boundaries, which is why I have called it local. In reality, each one of the elements that constitute it stands for networks that can thus be found drawn together, in confronting each other, or interacting with each other through representatives. Accordingly, I have chosen the concept of an intermediary to designate the heterogeneous entities that constitute the local collective. Whether one speaks of boundary objects like Star and Griesemer (1989) or mediators like Norton Wise (1992), in each case one is referring to the double nature of these intermediaries. They are at once things, statements, and bodies (visible, tangible, and heavy) *and* networks that are represented, punctualized (Callon, Law, and Rip 1986; Latour 1987). These local collectives are like Leibniz's monads. They are a microcosm, but one that contains a whole world in some sense enfolded into them. It is because sociologists and anthropologists have been able to unfold these pleats one by one that we have succeeded in perceiving the networks that are drawn together in these collectives.

The variety of knowledge produced and its capacity to shake up networks made irreversible by the market will depend on the composition of these collectives. We could get an idea of the multiplicity of possible dynamics by looking at two opposing extremes. In the first case, the large networks present in the collectives via their interposed intermediaries are already in close contact and linked to each other. The local collective does some minor reconfiguration work, but this will not shake up existing connections. A bit of patchwork has to be done—a few stitches resewn—but the fabric itself is not greatly changed. The current state of play is consolidated. In the second case, the large networks present via their interposed intermediaries are not yet connected. In this case, the local collective is in a position to propose some very original, innovative reconfigurations linking together networks that had been separate. This leads to the proliferation of new states of the world.

The first outcome is associated with routine work, consolidation, and continued and stubborn improvement. Connections are reinforced; there is greater irreversibility and increasing returns. The second outcome corresponds to what is generally called invention: an unexpected association of several preexisting networks that up to that point were strangers to each other. In this knitting process new statements are proposed, new skills are developed, and new instruments are designed. They allow bridges to be built and links to be forged. This kind of reconfiguration is more improbable and more radical the greater distance and less connection there was between the networks concerned.[26] Numerous works on the emergence of new specialties provide material for illustrating this thesis.

In each case, the work of the reconstruction of the circulation space of new intermediaries will also be very different. In the first case, new statements and skills will meet networks prepared to receive them. Diffusion will be swift. In the second case, new circulation spaces need to be completely reconfigured. There is a need to convince, to translate sometimes contradictory interests, to create compatible technologies, to install a technical infrastructure, to extend metrological chains, to train specialists, and to reconfigure society (Latour 1987; O'Connell 1993). The cost of enlarging the collective through this reconfiguration work can be very high.

Toward a Political Economy of Networks for the Production and Mobilization of Science

I am now in a position to reformulate my initial question in new terms to defend the following thesis. The scientific enterprise should be organized so as to permit the development of the greatest possible number of reconfigurations and so as to assure that each one of them has the same chance to grow. This presupposes that at certain moments these networks can go as far as the marketplace, with the creation of new products and services offered to users. The market cannot do this because it operates essentially according to a logic of increasing returns. Economic agents are caught in a strategic network that encourages them to continue doing what they know how to do or to want what others have wanted.

Science is a public good when it can make a new set of entities proliferate and reconfigure the existing states of the world. Private science is the science that firms up these worlds, makes them habitable. This is why public and private science are complementary despite being distinct: each draws on the other. This definition is independent of the identity of the actors involved. A firm that funds diversity by supporting new collectives is producing a public good and the government agency that contributes to a yet stronger linkage between the research it funds and the perfecting of Tomahawk missiles are supporting a science that can doubtless be called private.

The reversal that I have been proposing has led us to choose as our point of departure the dynamics of hybrid collectives rather than the concept of information. The mainly economic discourse that has held sway in recent years has resulted in our forgetting this reality by papering it over. If we want to grasp the real economic significance of science, we need to recognize it as a source of variety and to admit that it can be more or less rival or appropriable according to the strategic configurations into which it enters.[27] Considering the dynamics of science in terms of the hybrid collectives and networks leads us to question the role of the state. The new doctrine will necessarily be different from the old one. Its elaboration is doubly urgent because we are entering into a period in which the respective roles of the state and private enterprise in R&D work are being cast into doubt. From an economic point of view, what then matters is the incentive system, or if you prefer, the rules of the game that enable this complex dynamic to come into being. From this point of view, three principles can be invoked.[28]

a) The first is a principle of free association. The knowledge produced, and particularly the statements uttered, depend on the collection of intermediaries gathered in a local collective. If you change the composition of the collective, then you will get other statements, which will be neither more nor less robust, merely different. No collective should be ostracized a priori, regardless of the association of intermediaries that it is proposing. To assure a minimum of variety, representatives of existent but excluded social groups should be included in these collectives. Introduce the point of view of women into medical research, and you will suddenly get new statements, new techniques, and new skills. New states of the world will begin to proliferate in new directions. Representations of the body and of the nature of certain diseases will change. But the principle of equity between all intermediaries must go further than this. It should also be extended to instruments, machines, embodied skills. These should have an equal right to associate with, and participate in, local collectives. This principle of free association is also a principle of free circulation, not of merchandise but of intermediaries.

b) The second principle is that of freedom of extension. Once a local collective exists and has begun its work of reconfiguration, it must have the means of constructing the circulation space of statements and other intermediaries that it produces. This costs money.[29] The necessary transactions and negotiations enabling one to interest other actors, to create technical compatibility, to adapt production to expectations, to convince, to displace, and to duplicate represent considerable investments. These investments assure the passage from the local to the global and reconfigure the networks that are drawn together by the local collective. This principle is one of the right to the progressive production of irreversibility through the multiplication of connections and alliances, and the accumulation of experience.

c) The third principle is that of the fight against irreversibility and convergence. Once the networks are in place, they tend to perpetuate themselves and reinforce their connections. Science becomes private science and acts henceforth to reduce variety. It provides significant contributions to the reinforcement of irreversibilities. This struggle can take various forms. First of all, it can involve the practice of measured, but systematic, injustice toward networks made irreversible. These should not have any support. Moreover, certain constraints should be imposed on them, notably with respect to an obligation to divulge knowledge produced, to the duration of protection offered in return for this obligation, and to compatibility between products proposed to consumers. But the most effective way of fighting irreversibility and convergence remains lending support to emergent collectives and encouraging their proliferation.

An immense amount of effort will be necessary in order to conceive of and implement the procedures and tools essential for an application of these principles. How can one measure the degree of originality of a new collective? How can one help it grow without giving it too much support? How can one measure the degree to which it has been made irreversible? This could be the subject of another article.[30]

Finally, I would like to add a few words about the tone adopted in this presentation. It might appear somewhat strange, even reprehensible, for a sociology of science that proclaims itself agnostic, to adopt a point of view that is largely normative. I believe that, in taking this step, I am merely completing the process of reflexivity. Such theoretical reflexivity involves exhibiting not only one's results but also the interest of these results: the social space that they designate and construct.

Sociology and anthropology are concerned with showing the role of irreducible contingency within the sciences. Rules, practices, cultural forms, and relationships with things all vary from one collective to another. Diversity and the local are at the heart of science. By relentlessly pursuing the task of charting diversity within an activity—science—that is generally accused of creating uniformity and destroying the wealth of traditional cultures, anthropology has made an important discovery. Science is a public good, which must be preserved at all costs because it is a source of variety. It causes new states of the world to proliferate. And this diversity depends on the diversity of interests and projects that are included in those collectives that reconfigure nature and society. Without it, without this source of diversity, the market—with its natural propensity to transform science into a commodity—would be ever more doomed to convergence and irreversibility. In the end, it would negate itself. Like Carnot's cycle, the economic machine can only function with a source of heat and a source of cold!

—Translated by Geof Bowker

Notes

1. There is little work establishing an explicit link between the anthropology and the economics of science. Steve Fuller's work provides a notable exception (Fuller 1993).

2. The following presentation is directly inspired by Dasgupta and David's (1992) article, which presents economic doctrine on the subject with great clarity. See also Romer (1992, 1993). What is striking about this literature is the consensus of economists, whatever their school, on calling scientific knowledge a (quasi) public good.

3. There have been a number of propositions for alternative classifications of the various kinds of knowledge. The idea that some kinds of knowledge are tacit, whereas others are explicit is widely accepted. Authors disagree, however, on the extent to which tacit knowledge can be made explicit. For Harry Collins (1990), for example, there will always be a hard core of tacit knowledge that cannot be articulated. On the other hand, in their article, Dasgupta and David seem to feel that the form taken by knowledge (codified, embodied) does not depend on the nature of knowledge itself but simply on the institutional norms at play. For them, the difference between science and technology is not one of nature, but is a consequence of forms of incentives. In the case of the scientific institution, researchers are rewarded for divulging the knowledge that they produce by putting it into circulation in the form of codified statements. Those working in technological enterprises, on the other hand, are rewarded for making their knowledge as inexplicit as possible—in order to ensure better confidentiality. The arguments I am advancing do not force me to take sides in this debate. For the purposes of my analysis, it is sufficient that one accepts that there are many kinds of material substrates for science, and that one substrate can at least partially if not totally substitute for another—that is, there is a possible redistribution of any given piece of knowledge between the different materials in which it can be inscribed. For a good summary of the various positions, see Cambrosio (1988).

4. The reasons why economists nevertheless hesitate to consider science as an exclusive good like others are not clear. Some would doubtless say that, in order to be validated, and thus discussed, scientific knowledge must necessarily take the form of codified statements. Others would probably underline the importance of the cost linked to the transformation of knowledge into a completely exclusive good—a cost that can prove dissuasive for the actors.

5. Piet Hut and John Bahcall are both Fellows at the Institute for Advanced Study at Princeton. This text could only have been written with the support of the Institute, where I was invited to work during the year 1992-1993.

6. There are differences between economists on this point. Romer (1993), for example, explicitly said that knowledge embodied in human beings—in the last analysis reducible to neural pathways—is a rival good. But Romer has also stressed the completely public character of codified statements, which he calls "strings of bits" and which are for him the prototype of nonexclusive and nonrival goods. Romer adds an interesting element here. He recalls that there is often a correlation between exclusivism and rivalry: nonrival goods are generally quite difficult to appropriate. But according to him, this should not lead us to ignore the necessity for an analytic distinction between the two dimensions. There are rival goods that are difficult to control—such as, for example, fish in the sea or sterile insects that farmers release into their fields to fight parasites. By the same token, according to him there are goods that are intrinsically nonrival but exclusive such as information codes or encrypted messages.

7. The state's three modes of intervention are procurement, property rights, and patronage (the three Ps). As Romer (1993) stresses, goods that are both nonrival and nonappropriable pose a severe economic problem. If one renders them appropriable, which is always possible, but which has a certain cost (that of reconfiguring them or giving them legal protection), one creates a suboptimal situation (because all the economic agents who could have made free use of them see this right being abrogated). This is why any measures taken have to be a compromise between encouragement and optimalization. For an analysis of these compromises and their economic effects in the case of patent systems, see Foray (1994) and Kabla (1993).

8. The following lines draw heavily on the comments of Dominique Guellec and on Dominique Foray's skepticism as well as on the reaction of my many economist colleagues during oral presentations. I would like to thank them all here for having helped me formulate

more clearly the argument proposed, which, I fear, has not completely convinced them. Also, I have tried to respond to the friendly and sharp criticisms of one of the three anonymous referees of this paper.

9. The production cost of a paper varies by discipline, but it is rarely less than $100,000.

10. For a similar classification, see Machlup (1984).

11. Within standard economic reasoning, the decisions of investments would depend on several things: the amount and degree of certainty of expected profits (linked to the mobilization of S) as reckoned by B, the associated costs *I*, and the conditions of the appropriability of S. As shown later, appropriability or exclusivism of scientific knowledge is variable and depends notably on strategic choices made by the producers of knowledge whose evaluations take evidently intellectual property legislation into consideration. This legislation contributes, all things being equal, to defining the conditions of availability and appropriability of different goods (whether this be knowledge, techniques, or embodied skills). From the point of view I am taking here, it thus participates indirectly (by the effect that it has on the actors' investment decisions) in the determination of the degree of nonrivalry of scientific knowledge, reinforcing the extrinsic nature of this characteristic.

12. This case can obviously be greatly enhanced through intellectual property legislation, which, all things being equal, contributes to define the conditions of availability and appropriability of statements, techniques, or embodied skills.

13. In Japan, 85 percent of research and development (R&D) spending is undertaken by private enterprise. If one takes into account the fact that in countries like the United States or France a significant proportion of public funding goes to military research or to help companies, one gets percentages of the same order of magnitude for public support of academic research not directly linked to programs or interests that could be called private—in the sense that I will give this term in the section "Science as a Source of Variety and Flexibility" (Nelson 1992).

14. The many kinds of configuration possible explain the profusion of models that have been proposed to account for the forms of competition/cooperation to which discovery and innovation strategies give rise. These models integrate more or less systematically and completely the different variables that we have shown participate directly or indirectly in defining the degree of exclusivity or rivalry of the goods concerned (Joly and Ducos 1993).

15. For achieving such a privatization one would need a very strict intellectual property system, extended to science, and a low cost of protection, all rules that exist for other goods and make market transactions possible.

16. We have just seen that science cannot be a public good in the sense of economic theory without the outlay of investment costs. As a major part of these investments have already been made, the transformation of science into a completely private good would suppose a profound transformation of the rules of the game, a transformation that would involve no less costly investments. For example, one would need to encourage economic agents to produce embodied skills rather than codified knowledge, to protect all statements that could not be embodied without thereby rendering their disclosure obligatory and rendering the transmission of embodied skills themselves difficult. Given that this is a thought experiment and not a realistic scenario, I can ignore the cost entailed by the privatization of science in societies that have invested heavily in rendering part of it nonprivatizable. Indeed, here I am looking just at the economic dynamics for the case of a completely privatizable science!

17. For an overview see, for example, Dosi et al. (1988).

18. It is interesting to note that neoclassical economists, who are working from hypotheses quite different from those of evolutionary economists (who have developed these notions of irreversibility and path dependence), have been led to draw almost identical conclusions. In a

situation of imperfect information, apprenticeship, network externalities, and the total protection of innovations, it can be shown that companies competing in a single market tend both to minimize the diversity of their R&D projects and to continue along a given trajectory in order to develop their technical capital (Tirole 1989).

19. Such a situation, which I put forward as an imaginary one, is in fact not so different from what we see before our eyes. In a recent article based on statistical analyses, Nelson and Wright (1992) point to what they call a technological convergence between advanced industrial systems: the multiplicity of connections and the logic of increasing returns leads to a situation in which technologies become more homogeneous and this dynamically.

20. Dasgupta and David recognize (see note 4) that there is no epistemological difference between public and private science (they call the latter technology). But given that they reduce science and technology to information, they cannot see that the disclosure alone is not enough to ensure their nonrivalry and their nonexcludability.

21. This preference evidently introduces a normative point of view. But it is of the same order as Pareto's definition of the optimum or Rawls's principle of justice (Rawls 1972). If one is considering two situations, that one is collectively preferable in which the diversity of technologies and goods is greatest. The concrete measure of diversity is of course a very sticky problem (Weitzman 1992).

22. This is the reason why it would be useless to refine the analysis of science as information by introducing concepts such as asymmetry or speed of circulation. What is important is precisely the operation of a break with the traditional conception of information.

23. The framework of analysis proposed here allows us to give a new and more relevant meaning to the term *information*, by linking it to the networks in which it circulates, and which have been developed at the same time as it has.

24. Karin Knorr convincingly demonstrates the diversity of roles that the laboratory plays, as an organizational structure, in the dynamics of the production of scientific knowledge, according to specialty (Knorr-Cetina 1992).

25. The analysis of these collectives has come a long way in the last years. For a stimulating presentation, see Haraway (1994).

26. For a good example of an analysis showing the possible duality of these dynamics, see Cambrosio and Keating (1994).

27. It would be interesting to reread history in the light of this new definition of science as a public good. One could then pay attention to incentives, rules, and institutional arrangements as a function of their greater or lesser proclivity to favor the emergence of new configurations and to ensure the progressive extension of the networks that they give rise to.

28. For an analysis closely related to that given here, see Cowan (1991).

29. It could be said that it is a question of making market construction possible, that is, a supply and a demand linked to a new good.

30. A rereading of history, such as that suggested in note 27, could be useful in helping draw some lessons from the past. How, over time in different disciplines, has this diversity been supported (or, on the other hand, weakened)? How has it had or not had the means for regenerating industrial structures?

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