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The market economy, and the scientific commons

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Abstract

In principle there is a clear divide between science and technology. In practice there isn't. In principle, while practical inventions can be patented, scientific findings can't be. In practice, increasingly scientific findings are being patented. The argument of this paper is that this is bad for the advance of science and for the advance of technology. However, because of the blurry lines, it will not be easy to deal with. The paper lays out a strategy that at least has some promise.
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1. Introduction

Modern Capitalism has proved a remarkably powerful engine of technological progress. Most of the attention to its workings has focused on the business firms and entrepreneurs, operating in a market setting, who are the central actors in developing and introducing new products and processes. At the same time, it is widely recognized that the power of market stimulated and guided invention and innovation often is dependent on the strength of the science base from which they draw (Nelson, 1993; Mowery and Nelson, 1999). This science base largely is the product of publicly funded research, and the knowledge produced by that research is largely open and available for potential innovators to use. That is, the market part of the Capitalist engine rests on a publicly supported scientific commons.

The message of this essay is that the scientific commons is becoming privatized. While the privatization of the scientific commons up to now has been relatively limited, there are real dangers that unless halted soon important portions of future scientific knowledge will be private property and fall outside the public domain, and that could be bad news for both the future progress of science, and for technological progress. The erosion of the scientific commons will not be easy to stop. Here I want to call the alarm, and to suggest a strategy that has some promise.

But before I get on with this task, I need to clear some intellectual underbrush. A number of influential philosophers and sociologists of science have put forth a set of views, a theory, about the scientific enterprise that until recently has served well to protect the scientific commons. However, this theory no longer is adequate to the task, because the way it characterizes the nature of the scientific enterprise does not fit modern perceptions and the reality. Also, under this theory, it is hard to understand why privatization and markets are encroaching on the commons, and if they are, what is the matter with that. It is important, therefore, to scrutinize that theory.

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A key element of the theory is that, outside of industry, the work of scientists is and should be motivated by the search for understanding, and that the practical payoffs that often come from successful research are largely unpredictable. Bush (1945) is one among many proponents of public support of science who put forth this theme, and argued that it would be a mistake to look to likely practical payoffs as a guide to where scientific funds should be allocated. Serendipity is the reason why scientific research often has practical payoff, and the chances of serendipity are greatest when bright and dedicated scientists are free to attack what they see as the most challenging scientific problems in the way they think most promising.

For this reason, decisions regarding what questions to explore, and the evaluation of the performance of individual scientists and broad research programs, should mostly be in the hands of the scientists working in a field. Indeed, for the government or the market to intrude too much into how scientific research resources are allocated would be to kill the goose that lays the golden egg. In the terms used by Polanyi (1967), society should appreciate and protect "The Republic of Science".

An associated belief or ideal is that the results of scientific research are and should be published and otherwise laid open for all to use and evaluate. As Merton (1973) argued, the spirit of science is "communitarian" regarding access to the knowledge and technique it creates. All scientists are free to test the results of their fellows and to find them valid or not supported, and to build on these results in their own work. Because the results of scientific research are laid in the public domain for testing and further development, the bulk of scientific knowledge accepted by the community is reliable (as Ziman (1976) has emphasized) and scientific knowledge is cumulative. These are basic reasons why the scientific enterprise has been so effective as an engine of discovery. And economists often have argued that keeping science open is the most effective policy for enabling the public to draw practical benefits from it.

My argument in this essay is that the part of the theory about good science that stresses the value of open science is basically correct, but is in danger of being forgotten, or denied. A good share of the reason is that, as originally put forth, this part seemed a natural consequence of the other aspects of the theory: that the practical payoffs from scientific research were not predictable, but largely came about through serendipity, and that the allocation of scientific resources should not be guided by anticipation of particular practical payoffs, but rather by the informed judgments of scientists regarding the most important problems to work on. Keeping scientific findings in the public domain, with reward to the scientist being tied to the acclaim of his or her fellows, along with public funding of research based on peer review of the scientific promise of the proposal and the scientist, then would seem an important part of an incentive and control system for fostering productive science (for a discussion along these lines, see Dasgupta and David, 1994).

However, the notion that academic scientists have no idea and do not care about the practical problems that their research might illuminate never has been fully true. In this era of biotechnology it is obvious, if it was not before, that both the funders and the undertakers of research often have well in mind the possible social and economic payoffs from what they are doing. But if in fact, much of scientific research is consciously aimed, at least broadly, at problems the solution to which can have major, and broadly predictable, practical value, what is the case against harnessing market incentives to the undertaking of research and to the use of research results? In particular, why should the privatization of these kinds of research results be viewed as a problem?

The case for open scientific knowledge clearly needs to be reconstructed recognizing explicitly that much of scientific research in fact is oriented towards providing knowledge useful for the solution of practical problems, that the applications of new scientific findings often are broadly predictable, and that this is why control over scientific findings in some cases is financially valuable property. I think there is a case for keeping basic scientific knowledge open, even under these conditions. To privatize basic knowledge is a danger both for the advance of science, and for the advance of technology. I will develop my argument as follows.

Section 2 is concerned with how technological advance draws from science. I already have tipped my hand. Without denying the role of serendipity, I will argue for the most part science is valuable as an input to technological change these days because much of scientific research is in fields that are oriented to providing knowledge that is of use in particular areas. These are the scientific fields that Stokes (1996) saw as in "Pasteur's Quadrant", where the research aims for deep understanding, but the field itself is oriented towards achieving practical objectives, like improving health, or achieving better understanding of the properties of materials, or achieving a powerful theory of computing. I acknowledge that this is a somewhat more expansive view of what science is than that contained in earlier characterizations of a "Republic of Science". But in fact a large fraction of what is well recognized as science always has been undertaken with practical objectives in mind or not far out of mind. Stokes' example of Pasteur is apt. And this fact is vital to keep in mind when trying to understand how science operates, and the controversy this paper is about.

In Section 2, I discuss the rise and erosion of the idea that public support of open science is warranted because the expected returns are high but the areas of return are so uncertain that market mechanisms will not suffice. I begin by briefly reviewing the ideological and political debates that occurred after World War II that led to broad consensus regarding the value of public support of open autonomous science. As I noted that rhetoric stressed that the payoffs from science were almost completely unpredictable, and thus the allocation of funds to science should not be influenced by perceptions of social needs. The publicly supported science system that actually developed was in fact much more oriented to facilitating making progress on important practical problems than that rhetoric allowed, and this is now obvious.

I do not want to argue that most academic researchers working in, for example, the bio-medical sciences define their goals as dealing with particular diseases. Much of the most important work in such fields is quite fundamental in nature, in the sense that it explores basic processes and phenomena, without a clearly defined specific practical objective in mind. However, the fundamental questions and appealing lines of research in sciences in Pasteur's Quadrant are strongly influenced by perceptions of what kind of knowledge is relevant to problem solving in a field. Thus, one of the reasons why cell biology now is such a fashionable field is belief that basic understanding won here might just unlock the cancer puzzle, or enable us to understand better how receptors work.

This perception of how the modern science system actually works has eroded the notion that it is important to keep science open. My argument is that this is a serious mistake.

While perceptions of possible applications of research are not as vague as proposed in the earlier rhetoric about serendipity, the actual paths to application of apparently promising scientific discoveries are in fact very uncertain. Understandings that come from science seldom lead immediately or directly to the solution of practical problems. Rather, they provide the knowledge and the tools to wrestle with them more effectively. I propose that for just this reason, that the findings of basic science set the stage for follow-on applications work, for society to get maximal benefit from its support of basic science requires that there be open access to scientific research results. Open access permits many potential inventors to work with new knowledge. Privatization closes off access to only those whom the owner allows to make use of it. This is why some of the recent developments are so worrisome.

In Section 4, I discuss the current situation and the dangers in more detail. Then I turn to a number of measures that I believe have some promise as attacks on the problem.

2. The coevolution of practice and understanding

Virtually everybody these days appreciates that the power of modern technological innovation depends to a considerable extent on its ability to draw from modern science. But there is little general understanding, and some quite wrong beliefs, about the nature of the science–technology links. Understanding these correctly is a precondition, I believe, for having an effective discussion about what public policy towards science ought to be. This certainly is so regarding the current controversies about patenting in science. Thus, this section discusses what scholars studying technological advance know about these issues.

Technologies need to be understood as involving both a body of practice, manifest in the artifacts and techniques that are produced and used, and a body of understanding, which supports, surrounds, and rationalizes the former. For technologies that are well established, an important part of the body of understanding supporting practice generally is grounded in the empirical experience of practitioners regarding what works and what does not, things that sometimes go wrong, reliable problem solving methods, etc. However, in recent times, virtually all powerful technologies have strong connections with particular fields of science. These connections, of course, are central in the discussion of this essay.

There is a widespread belief that modern fields of technology are, in effect, applied science, in the sense that practice is directly drawn from scientific understanding, and that advancing technology essentially is a task of applying scientific knowledge to achieve better products and processes. This task requires scientific expertise, but in most cases is relatively routine once the target is specified. Indeed, in his Capitalism, Socialism, and Democracy, Schumpeter (1942) argued that by the mid twentieth century that was largely the case, and the kind of competition among firms that had over the prior century made capitalism such a powerful engine of progress no longer was necessary. With strong science, technological advance could be planned. Schumpeter's views were in accord with those of many prominent scientists of his day, and today. Yet, careful studies of how technological advance actually proceeds in this modern era clearly show that the process remains unplanable in any detail, and competitive exploration of multiple paths remains an essential part of it (see e.g. Rosenberg, 1982; Nelson and Winter, 1982).

Virtually all empirically oriented scholarly accounts of how technology progresses have highlighted that the process is evolutionary in the following senses (see e.g. Basalla, 1988; Constant, 1980; Dosi, 1988; Metcalfe, 1998; Mokyr, 1990; Nelson and Winter, 1982; Petroski, 1992; Vincenti, 1990; Ziman, 2000). First, at any time there generally are a wide variety of efforts going on to improve prevailing technology, or to supersede it with something radically better. These efforts generally are in competition with each other, and with prevailing practice. And the winners and losers in this competition to a considerable extent are determined through an ex-post selection processes. Second, today's efforts to advance a technology to a considerable extent are informed by and take off from the successes and failures of earlier efforts. While there are occasional major leaps that radically transform best practice, for the most part technological advance is cumulative. And scholars of technological advance also have generally stressed that the advanced technologies of a given era almost always are the result of the work of many inventors and developers. Technological advance is a collective, cultural, evolutionary process.

The proposition that technological advance is an evolutionary process in the above sense in no way denies, or plays down, the often extremely powerful body of understanding and technique used to guide the efforts of those who seek to advance it, at least in modern times. A strong body of scientific understanding of a technology serves to enlarge and extend the area within which an inventor or problem solver can see relatively clearly and thus make informed judgments regarding what particular paths are promising as solutions, and which ones are likely to be dead ends. Also, the sciences and engineering disciplines provide powerful ways of experimenting and testing new departures, so that a person or organization who commands these can explore the merit of designs without going to full-scale operational versions. Thus, strong science enables the process of designing and inventing to be more productive and powerful than it would be were the science base weaker.

However, it does not change the fact that the process of advancing the technology remains evolutionary. Strong science provides tools for problem solving, but usually in itself does not solve practical problems. If anything, strong science increases the advantages to society of having many competent actors striving to improve the art.

The connections between the "body of practice" aspect of a technology and the "body of understanding" part need to be understood in this context. Virtually all modern technologies are supported by a strong body of science or science-like understanding that illuminates how the artifacts and techniques employed work, provides insight into the factors that constrain performance and provides clues as to promising pathways toward improvement. But at the same time, much of practice in most fields remains only partially understood, and much of engineering design practice involves solutions to problems that professional engineers have learned "work", without any particularly sophisticated understanding of why. Medical scientists still lack good understanding of just why and how certain effective pharmaceuticals do their work, and theories about that can change from time to time.

Technological practice and understanding tend to coevolve, with sometimes advance of understanding leading to effective efforts to improve practice, and sometimes advance in practice leading to effective efforts to advance understanding. Thus, the germ theory of disease developed by Pasteur and Koch, by pointing clearly to a certain kind of cause, led to successful efforts to get certain diseases (now known to be caused by external living agents) under control. Maxwell's theory of electromagnetism led to Hertz, Marconi, and radio. But in many cases, advances in practice come first and lead to efforts to understand scientifically. Thus, the discovery by Shockley and his team at Bell Laboratories that a semiconducting device they had built as an amplifier worked, but not in the way they had predicted, led him to understand that there was something wrong, or incomplete, about the theory in physics regarding the electrical characteristics of semiconductors, which in turn led to his own theoretical work, and a Nobel Prize. Rosenberg (1996) has argued that a number of the most challenging puzzles science has had to face have been made visible by or been created by new technologies, and the puzzles of why they work as they do.

Much of the development of modern science should be understood as the result of institutionalized responses to these challenges and opportunities. Quite often, specialized fields of applied science or engineering developed out of the experience of more generally trained scientists working on the problems of a particular technology or industry. Thus, the field of metallurgy came into existence as chemists worked on problems of quality control in the rapidly growing steel industry (Rosenberg, 1998). As the industries producing chemical products expanded, chemical engineering developed as a field of research, as well as teaching. The physics of mechanical forces long had been useful for civil engineers designing buildings and bridges. But with the new physics of electricity and magnetism, a whole new set of science-based industries was launched. As complex electrical "systems" came into place, the new field of electrical engineering grew up. Later on, the invention of the modern computer would spawn the field of computer science. Stronger knowledge in chemistry and biology led to the development of a collection of specialized fields involved in agricultural research. Fields like pathology, immunology, and cardiology, grew up for teaching and research at medical schools.

All of these fields of science are in "Pasteur's Quadrant". Research done here often probes for quite deep understanding. But the field as a whole, and broad programs of research in the field, are dedicated quite explicitly to solving particular kinds of practical problems, and advancing bodies of practical technology. I have developed this story at considerable length because in much of the writings on science, and the institutions governing science, these applied sciences tend to be ignored. However, in the US, Western Europe, and Japan, they account for the lion's share of the resources going into the support of science.

Popper (1989), Campbell (1974), Ziman (1976), Kitcher (1993), and other scholars of the advancement of science have stressed that science is a system of knowledge. The test that guides whether new reported findings or theories are accepted into the corpus of accepted knowledge is "Is it valid" "Is it true?". Popper and his followers have argued that there can be no firm positive answer to that question. Ability to stand up under attempts at refutation, or (probably more commonly) for apparent implications to hold up when they are explored, may be the best humans can do. But in any case, from this philosophical perspective, the quest in science is for understanding in its own right. And there certainly is a lot of truth to this position as a characterization of the nature of scientific debates.

On the other hand, as Vincenti and others who have reflected on the similarities and differences between technological and scientific knowledge have argued, the central test for technological knowledge is "is it useful". Technological knowledge is part of a cultural system that is concerned with achieving practical ends, rather than knowledge for its own sake. The objective is to get something that works, or works better, and "understanding" is important only in so far as it helps in that effort.

However, the selection criteria for new science and for new technology cannot be kept sharply separate for sciences in Pasteur's Quadrant. In these fields, an important and often stringent testing ground for science is provided by those who think they see how it might be applied in practice. And failure to understand why something works is a strong motivation for scientific research.

By far, the lion's share of modern scientific research, including research done at universities, is in fields where practical application is central in the definition of a field. And, not surprisingly, these are the fields on which efforts to advance technology mostly draw. Two recent surveys (Klevorick et al., 1995; Cohen et al., 2002) have asked industrial R&D executives to identify the fields of academic research that contributed most to their successes in R&D. The fields they listed were exactly those discussed above.

The most recent of these studies (Cohen, Nelson, and Walsh) also asked about the kind of research output that was most valuable to industry, and the most important pathways through which industry gained access. Contrary to much of the current discussion, prototype technologies were not rated an important output of academic research for most industries (biotechnology is an exception), but rather general research results and research techniques (and even in biotechnology these kinds of research outputs were rated as useful much more often than prototypes). Relatedly, in most industries the respondents reported that the most frequent use of university research results was in problem solving in projects, rather than in triggering the initiation of projects.

In most industries, the respondents said that the most important pathway through which people in industry learned of and gained access to what was coming out of public research was through publications, and open conferences. Put another way, today industry gets most of its benefit from academic science through open channels. In their more narrowly focused but more detailed study of the pathways through which research results of the MIT departments of mechanical and electrical engineering get to industry, Agrawall and Henderson (2002) arrive at a similar conclusion.

I want to conclude this section by again stressing that in all the fields of technology that have been studied in any detail, including those where the background science is very strong, technological advance remains an evolutionary process. Strong science makes that process more powerful, but does not reduce the great advantages of having multiple paths explored by a number of different actors. From this perspective, the fact that most of scientific knowledge is open, and available through open channels, is extremely important. This enables there to be at any time a significant number of individuals and firms who possess and can use the scientific knowledge they need in order to compete intelligently in this evolutionary process. The "commitarianism" of scientific knowledge is an important factor contributing to its productivity in downstream efforts to advance technology.

3. The governance of public science

World War II and the period just after marked something of a watershed in broad public and political recognition of the important role that public science plays in technological progress, particularly in the US and the UK. To be sure, much earlier visionaries like Francis Bacon had argued for support of science as a means through which societies could progress materially. Scholars like Price (1962), Hart (1998), and Guston (2000) have described the earlier history of debate about science policy in the US. But, it was the World War II experience, where government-supported and -focused R&D was so successful both in the development of weapons that won the war, and in the development of medical capabilities that greatly reduced casualties both from wounds and from infectious diseases compared with earlier wartime experiences, that gripped the public attention. The title of the Vannevar Bush report (1945) advocating a major postwar program in the US of support of science caught the spirit: "Science, the Endless Frontier".

In both the US and the UK the discussion about the appropriate postwar role of public science was structured and constrained, for the most part, by recognition of the central role of companies with their own R&D capabilities in the process of technological advance; the point of view there was implicitly Schumpeterian. While there were exceptions, the discussion was not about contesting that role. Rather, the focus was on the system of public science, done in universities and public laboratories, that was separate from the corporate system but strongly complementary, and which needed public support. The argument of those who advocated stronger government support was that this would make the overall system of innovation more powerful.

In both the UK and the US, the debate about the governance of public science squared off along much the same lines. In the UK, J.D. Bernal (1939), a distinguished physicist, and a socialist, argued for a government program in which the allocation of public funds to science would be strongly guided by the weighing of social needs, and the support program as a whole would be closely monitored by the government. To this point of view Michael Polanyi, a distinguished philosopher of science, took strong exception, advocating a largely self-governing "Republic of Science" (1967), which would be publicly funded, but in which the scientific community itself would set priorities and decide on what was good science.

In the US, Bush's manifesto "Science, the Endless Frontier" argued strongly for a self-governing scientific community, but with national priorities playing a role in setting broad research directions, at least in certain areas. In particular, national security and health were singled out as areas where the overall research budget and broad research priorities needed to be made through political and governmental processes. But given the funding within those broad areas, the scientists themselves were to have basic discretion for devising the research programs they thought most appropriate. Government non-scientists were not to meddle in this. Regarding the role of public science in supporting economic progress more broadly, Bush saw the government's role as supporting basic research, with the science system self-governing, both with respect to identification of the broad fields of greatest promise, and the details of allocating funds and carrying out research.

There is no question but that, like Polanyi's response to Bernal, Bush's articulation of a basically self-governing community of science was put forth in good part to counter, to block, proposals for a postwar publicly supported science system that would involve much more political and government control of the allocation of resources. Senator Harley Kilgore took much the same position as did Bernal in the UK. Bush believed this would destroy the creativity and power of science, and it would be far better to have the top scientists running the show.

There also is no question but that Polanyi and Bush felt it of extreme importance that government support fields like theoretical physics and mathematics, where perceptions of potential practical payoff have little to do with the way the fields unfold, yet which provided important knowledge and technique that helped to win the war. Hence, the emphasis on serendipity, and the unpredictability of areas of potential payoff. It is almost certain that both men knew well that much of scientific research was not of this kind, but rather was in fields where perceptions of practical problems played a significant role in defining the broad agenda, if not the short run priorities of resource allocation. However, the rhetoric of Polanyi and Bush obscured the fact that most of science is in Pasteur's Quadrant.

It is not surprising, therefore, that in both the US and UK it turned out that mission-oriented agencies became the primary government supporters of basic research Thus in the US the Department of Defense funded basic work in computer and materials science, and electrical engineering. The Atomic Energy Commission (later the Department of Energy) has had principal responsibility for funding high energy physics. The National Institutes of Health became the primary funder of university research in the bio-medical sciences. The National Science Foundation, the only significant research funding agency in the US without a mission other than support of science, always has been a small supporter relative to the mission-oriented agencies. The lions share of the research done in the US, funded by government, and undertaken in universities and public laboratories, is in fields in Pasteur's Quadrant.

This fact both removes the puzzle of why science has contributed so much to technological advance, and enables one to understand better why Vannevar Bush (and most of his science trained followers writing about science policy) had such strong faith in the ability of the scientific community to steer their efforts in socially productive directions. But this recognition also signals that the lines between basic science and applied science are fuzzy not sharp. And it raises the question of where the publicly supported Republic of Science ought to leave off, and the market begin. It is fair to say that for the most part the postwar debates were somewhat ad hoc about this. Thus, Bush recognizes a central role for market organized and induced R&D, and saw public science as providing inputs to that market system, but being separate. But he provided little in the way of coherent argument about where the one stopped and the other began. Indeed, despite its obvious importance, outside of economics, this question has aroused little analytical interest.

Economists have grappled with the question of the appropriate spheres of government activity in the science and technology system using two theoretical concepts: externalities, and public goods. The externalities concept is about benefits (and costs) of private economic activity that those who make the relevant decisions do not see as benefits (or costs) to them. Here economists have highlighted the "spillovers" from industrial R&D, information and capabilities created by a firm's efforts to create better products and processes that it cannot fully capture, and hence which benefit other firms, including competitors. In general, the analyses by economists oriented towards the externalities from R&D have not served as a base for arguments for a domain of public science, but rather for arguments that industrial R&D in some instances should be encouraged by favorable tax treatment, and perhaps subsidies of various kinds to reduce private costs. Indeed, the policy discussion proceeding under the conception that research yields externalities naturally tends to be pulled towards devising policies that will make the results of R&D more proprietary, less public. An important part of the current policy discussion in fact is oriented in just this way.

The public good concept of economists is much more directly relevant to analysis of the appropriate domain of public science, or at least the range where "communalism of knowledge" should apply. For our purposes here, the most salient aspect of the economists' public good concept is that a public good is "non-rivalrous in use". By that it is meant that, unlike a standard economic good, like a peanut butter sandwich, which either you or I can eat but not both (although we can split it), a public good can be used by all of us at the same time without eroding the quality for any of us.

Knowledge is a canonical case of something that is non-rivalrous in use in this sense, and this is not a proposition conjured up by economists. The notion that I can tell you what I know, and then you will know it, and I will too, almost surely has been widely understood by sophisticated persons for a long time. There is no "tragedy of the commons" for a pure public good like knowledge. And to deny access, or to ration it, can result in those denied doing far less well than they could if they had access. In the case in point, if access to certain bodies of scientific knowledge or technique can be withheld from certain researchers, they may be effectively barred from doing productive R&D in a field.

Now the fact that something is non rivalrous in use does not mean that its use cannot be restricted. However until relatively recently, it was broadly assumed that it was difficult to restrict access to scientific knowledge. Certainly scientific knowledge could not be patented. This effectively took science outside the domain where market incentives could work. Indeed, the presumption that the returns to scientific research could not be appropriated was a central part of the argument why public funding was necessary.

However, over the last quarter century there have been two key developments that have challenged this view of basic science. First, the courts have ruled that at least some of the results of basic research can be patented. And about the same time that the implications of these rulings were becoming evident, Congress passed the Bayh–Dole act of 1980, which strongly encouraged universities to take out patents on their research results where they could, on the basis of a (not well supported) argument that this would facilitate firms who could make practical use of the results to do so under a protective license (for a detailed account, see Eisenberg, 1996). The first of these developments significantly increased the incentives for for-profit firms to engage in the areas of basic research where the results can be patented, and to try to make their living licensing patented research results to other firms that can make use of them. The second has brought about profound changes in the way universities give access to their research results. As a result, important areas of science are now much more under the sway of market mechanisms than used to be the case. And in particular, in some important fields of science important bodies of scientific understanding and technique now are private property rather than part of the commons.

A widespread reaction is "So what is the problem with that?". There is a strong presumption these days that if market organization can and will do a job, that obviously is a good thing. From this point of view, the main argument that needs to be made for government support of basic research is that the long run benefits to the society are high, and that for-profit firms have little incentive to do much of it because of the difficulties in establishing property rights, and the long time lags and uncertainties involved in moving from research results to commercial product. If these barriers to market organization are lowered for some reason, let the market move in.

I note that knowledge of an effective product design or a production process, what customarily is considered as technological knowledge, shares with scientific knowledge the property of being non rivalrous in use. Yet society relies largely on the market to induce R&D aimed at creating new products and production processes, and there is little dispute that granting patents on product and process inventions is reasonable social and economic policy. So why not allow patents on the stuff of basic science, if that will induce the market to move in?

My response is that the outputs of scientific research almost never themselves are final products, or even close, but have their principal use in further research, some of it aimed to advance the science farther, some to follow leads that may enable a useful product or process to be found and developed. But in both cases, the latter as well as the former, there is considerable uncertainly about the best paths to pursue. Progress calls for a number being explored. My concern is not with patents on the outputs of scientific research that are directly useful or close to that, so long as the scope of the patent is limited to that particular use. It is about not hindering the ability of the scientific community, both that part interested in advancing the science farther, and that part interested in trying to use knowledge in the search for useful product, to work freely with and from new scientific findings.

I do not know of a field of science where knowledge has increased cumulatively and, through cumulative advance, dramatically, that has not been basically open. It is easy to argue that scientists never have fully followed the canons of science identified and laid out by Robert Merton: universalism, communitarianism, disinterestedness, and organized skepticism. Scientists are well known to keep their work secret until they are ready to publish. There certainly is a lot of self-interest, opportunism, hostility, and downright deviousness and lying that one observes in the histories of the progressive sciences. A scientific paradigm held by the elite in a field can hold intellectual tyranny. It is valuable to bring new organizations into the basic research scene, and in some case for-profit business firms have explored paths that the academic community snubbed.

But on the other hand, a careful reading of important scientific controversies, for example the argument about the nature of combustion at the start of the 19th century, or of the nature of the genetic code, or of whether the expansion of the universe is decelerating or accelerating, shows the importance and the power of a public science system where by and large all participants have access to much the same facts, and the debates about whether new proposed facts or theories are valid are open to all working in a field. One cannot come away from reading Horace Judson's (1969) The Eight Day of Creation, a history of the development of molecular biology as a field of science, without respecting the power of open science to progress.

This is equally true for sciences that are strongly in "Pasteur's Quadrant". Porter (1997) history of medical knowledge and practice, *The Greatest Benefit to Mankind* (1997) gives case after case where progress was made through a system where researchers were free to try to replicate or refute the arguments and findings of others.

While my argument above has focused on the advantages of an open science for the advancement of science, much of my discussion in Section 2 was concerned with developing a case why open science is important to technological progress. These arguments of course are mutually reinforcing. Keeping the body of scientific knowledge largely open for all to use, in the attempts to advance science, and in the attempts to advance technology, is in my view an extremely important matter. Its importance is not recognized adequately in the current discussions.

I want to conclude this section by putting forth three views on what should be done about the encroachment of proprietary property claims into what had been the domain of public science. The first is to cede the contested turf. If research findings can be patented, accept and embrace that. If universities can patent their results, and limit access to the highest bidder, fine. And welcome the presence of private firms motivated to do research by the lure of patents, and control of subsequent work in a field, or royalty incomes. Indeed, these developments diminish or even eliminate the need for publicly funding of certain fields of science.

The second is to coexist and compete on the contested terrain. This is pretty much the policy that developed regarding research on the human genome. The argument here is that publicly supported research, and keeping open the results of that research, provide useful competition to private research, even if some private firms do not like the competition (Eisenberg and Nelson, 2002).

A third position is to resist and try to roll back the invasion of privatization. This point of view sees that invasion not only as probably undesirable, but also as something that is occurring under a given set of policies, which can be changed. Thus if the movement of patentability upstream into the sciences, together with the expectations under the Bayh–Dole act, are leading to for profit companies engaging in research to identify the genetic code, and to the patenting of that code by them and by universities operating under public funding, maybe patent law and practice, and Bayh–Dole, need to be revised.

Above I have given my reasons for rejecting the first position. My position on this is a combination of the second and third. I believe it important to preserve as much of the commons as possible. However, doing so will not be easy.

4. The importance of protecting the scientific commons

The major expansion of patents into what used to be the realm of science is well documented I am persuaded that there is enough of a potential problem here to call the alarm. However, I confess that the evidence that there already is a problem, that access to scientific research results having high promise of enabling solution of important practical problems is being sharply limited by patent holders, presently is very limited. The most detailed study is by Walsh et al. (2002).

This study involved interviews with a number of researchers in the biomedical field, asking about whether their research had been hindered by patent rights that blocked access to certain paths they wanted to explore.

Scholars studying this potential problem have identified at least two different kinds of situations where the presence of patents can hinder research (for a general discussion, see Merges and Nelson, 1990). One of these is the problem caused by patents on “research tools” (see NRC, 1997) where research techniques of widespread use in a field, materials that are inputs to a wide range of research endeavors, or key pathways for research (like the use of a particular receptor), are patented, and the patent holder aggressively prosecutes unlicensed use or reserves exclusive rights to further research using the tool. The second, highlighted recently by Heller and Eisenberg (1998) is focused on contexts where development of or advance towards a useful product or technique may involve transgressing on several patents held by different parties.

The latter problem, that of the need to assemble a large number of permissions or licenses before being able to go forward, was found by the Walsh, Arora, Cohen interviews and case studies not to be particularly important, as of yet. Regarding research tools, a number of the more important general purpose ones are available to all who will pay the price, and while in some cases there were complaints about the price, at least they were available.

On the other hand, the study did identify a number of instances where the holder of a patent on an input or a pathway (for example a receptor) that was important in a particular field of exploration did not widely license, and in some cases sought to preserve a monopoly on use rights. It is clear that in a number of the cases, the patented finding had been achieved through research at least partially funded by the government. This policy well may have been reasonable from the point of view of the patent holders. But the burden of this paper is that it is not good from the point of view of society, seeking to maximize the benefits of publicly funded research.

The authors of the study take a cautious position regarding the implications of their findings. I find them sufficient evidence to indicate that there is a real problem here, or there will be soon, and it is time to think about what can be done to contain it.

There are two broad policy arenas that bear on this issue, to which I want to call attention here. One is intellectual property rights law. The second is the policies of universities and public laboratories regarding their research findings, and government policy regarding the university research it funds. My discussion below is oriented to what is needed, in my view at least, to preserve an appropriately wide area of public scientific knowledge.

4.1. Can we protect the republic of science through patent law?

I find that many people are puzzled when they learn that patents are being taken out on genes or gene codes, or more generally are intruding into the realm of science. There is a widespread belief that scientific facts or principles or natural phenomena are not patentable. Indeed, the courts have endorsed this position strongly, as a general philosophical principle.

But the problem is that the lines between natural substances and principles and man made ones are blurry not sharp. Nearly a century ago a landmark patent law case was concerned with whether purified human adrenalin was a natural substance and hence not patentable (although the process for purification certainly was patentable) or whether the fact that adrenalin never was pure in its natural state meant that the purified substance was man made and hence patentable. The decision was the latter, and while it can be argued that the decision was unfortunate, one certainly can see the logic supporting it. In any case, the precedent set here has held through the years. (Parke-Davis & Co. v. H. K. Mulford & Co., 1911). Recent patents on purified proteins and isolated genes and receptors are couched in terms that highlight something that man has created or modified from its natural state.

A recent article by Bar-Sholam and Cook-Deegan (in press) is concerned with the consequences of a patent granted on a monoclonal antibody (antibodies are natural substances, but particular antibodies cloned by a particular process have been judged not to be natural) which binds to a particular antigen (a natural substance) on the outer surface of stem cells, and hence is capable of recognizing such cells and serving as a basis for processes that would isolate stem cells The patent also claimed "other antibodies" that can recognize and pick out that antigen. The latter part of the claim in effect establishes ownership of the antigen. The authors argue, correctly in my view, that the inclusion in the patent claims of all "other antibodies" meant that the patent was unreasonably broad and should have been pruned back by the patent office and the courts. However, one can clearly see the blurry lines here between the natural and the artificial. And the patentee could well argue that the "invention" was a method of recognizing a particular antigen (such a method would seem to fall within the bounds of patentability) and the particular antibody actually used was just an exemplar. In the case in question this patent was licensed exclusively to a particular company and, in turn, later used effectively to close down another company that had achieved a process capable of isolating stem cells earlier than the licensee using a method judged to infringe.

The issue of undue patent scope aside for the moment, the problem of determining the patentability of a research output whose future use is largely in further research seems almost inevitable for research in Pasteur's Quadrant, for obvious reasons. The original work in question was done by an oncologist at Johns Hopkins University. The research clearly was fundamental, and at the same time was aiming for understandings and techniques that would be useful in dealing with cancer.

The problem becomes even more complicated in scientific fields that are concerned with advancing understandings of technologies, fields like computer science and aeronautical engineering. Thus, Vincenti (1990) describes at some length the research done at Stanford during the 1920s that aimed to develop good engineering principles (reliable if rough "laws") that would guide the design of aircraft propellers. The results of this research were laid open to the general aviation design community and were not patented. But had the researchers had the motivation, they probably could have posed their results in terms of processes useful in propeller design, which might have been patentable then, and likely would be today. A significant portion of the work within the modern field of computer science is concerned with developing concepts and principles that can help improve design. Up until recently, at least little of this work seems to have been patented, but portion of it clearly could be.

In each of these cases, the research outputs were (are) at once important inputs to a flow of future research, and useful inputs for those who are focused on solving practical problems. In much of this paper, I have been arguing that, because of the latter, there are major general economic advantages if those understandings and techniques are part of the general tool kit available to all those working to advance practice in the area. The obvious objection is that the ability of the discoverer or developer of these understandings and techniques to control their use is an important incentive for the research that creates them. I would reply that, at least in the case of research at universities, funded by a government grant, this usually is not the case. I will discuss university policy shortly.

But to return to the present discussion, I am not optimistic about how much of the problem can be dealt with by patent law. The focus here is on patent law on research outputs that provide tools for advancing a science or technology, as contrasted with a final product or process per se. Here, one can urge several things of the patent office and the courts. But the problem of innately blurry lines will remain.

First, one can urge more care not to grant patents on discoveries that largely are of natural phenomena, by requiring a strong case that the subject matter of the patent application or patent is "artificial", and by limiting the scope of the patent to elements that are artificial (more on the patent scope problem shortly). Demaine and Fellmouth (2003) make a similar argument, that patents should be allowed only on outputs of research that are a "substantial transformation" from the natural. The lines here a blurry. But the slope clearly is slippery and a strong argument can be made that the dividing line has been let slip too far, and leaning hard in the other direction is warranted. In the case of purified natural substances, this would call for a greater proclivity to limit the patent to the process and not allow the purified product per se to be patented.

Second, one can urge a relatively strict interpretation of the meaning of 'utility' or usefulness. This issue is particularly important for patent applications and patents that argue very broadly that the research result in question can be useful in efforts to achieve something obviously useful—a case for usefulness once removed. But the problem here is that the direct usefulness then is as an input or a focus of research, and this is the kind of generic knowledge and capability I have been arguing is important to keep open and in the public domain. A stricter interpretation here would require more compelling demonstration of significant progress towards a particular practical solution than seems presently required, and particularly if combined with the suggestion below about reining in patent scope, would be a major contribution to protecting the commons.

Third, there is the issue of the allowed patent scope. There is a strong tendency of patent applicants to claim practice far wider than they actually have achieved. The case above of a claim covering "all antibodies" that identify a particular substance is a case in point. While there are obvious advantages to the patentee of being able to control a wide range of possible substitutes to what has actually been achieved, there are great advantages to society as a whole of not allowing such broad blocking of potential competitive efforts. I believe that getting the patent office and the courts to understand the real economic costs of granting too broad patents is of the highest priority.

I have argued the special importance of not allowing patents to interfere with broad participation in research going on in a field. One way to further this objective would be to build some kind of an explicit research exemption, analogous to the fair use exemptions in copyright law, into patent law. Indeed there is a long history of statements by judges to the effect that use in pure research is not a violation of a patent. Universities clearly have been clinging to this theory to justify their freedom of research.

A recent decision of the Federal Circuit (Madey v. Duke, October 2002) has changed the situation. In a ruling on an infringement suit against Duke University, the court argued that doing research, basic or applied, was part of the central business of a university, and that the university benefited in terms of funding as well as prestige from the research it did. Thus, university interests, not simply scientific curiosity, were at stake in the research. Therefore, it was quite reasonable under the law for a patent holder to require that the university take out a license before using patented material in research. After this ruling, it is highly likely that patent holders will act more aggressively when they believe that university researchers may be infringing on their patents. While there is a chance that the Supreme Court will reverse, that is not a good bet. It now looks as if an exemption for use in basic research will come into place only if there is new law.

However, under current university policies, a case for such new law is not easy to make. Among other things, there clearly is a problem of how to delineate basic research. As I have been highlighting, much of university research is in Pasteur's Quadrant, where in many cases there are practical objectives as well as the goal of advancing basic understanding. And in recent years, universities have been patenting their research results.

Discussions with industry executives suggest that, until recently, industry often gave university researchers a de facto research exemption. However, now they often are very reluctant to do so. In many cases, they see university researchers as direct competitors to their own research efforts aimed to achieve a practical result which is patentable. And they feel themselves burdened by the requirement to take out licenses to use university research results that are patented, and see no reason why they should not make the same demands on universities. In my view, the obstacles to a serious research exemption are largely the result of university policies.

Of the several proposals for a research exemption that have circulated recently, I find one of the most interesting to be that put forth by Dreyfuss (2002). In what follows, I amend it slightly. Under the proposal a university or non-profit research organization (under one version of her proposal, any research organization) would be immune from prosecution for using patented materials in research if: (1) those materials were not available on reasonable terms (this is my amendment); and (2) if the university or other research organization agreed not to patent anything that came out of the research, (or if they did patent to allow use on a nonexclusive royalty free basis—my amendment). Certainly, there could be some difficulty in determining, if the matter was brought up, whether or not the patented material was available at reasonable terms, or just what "reasonable" means, but in many of the most problematic cases this proposal is designed to fix, the answer is that they are not available at all. In some cases, it would not be easy to determine whether a patent emanated from a particular research project or from some other activity. But these problems do not seem unusually difficult compared with other matters often litigated. And it is likely that, for the most part, if a research organization proceeded under this law, there would not be much litigation, and there would be much reduced fear of such.

After the Duke decision, the road to a university research exemption almost surely must go through Congress. The advantage of a proposal like that of Dreyfuss is that it would trade open access to research results for university researchers for agreement of university researchers not themselves to add to the problem of patents in science. The principal obstacle to such a deal I believe is the universities themselves.

4.2. Will universities come to the defense of the scientific commons?

I believe the key to assuring that a large portion of what comes out of future scientific research will be placed in the commons is staunch defense of the commons by universities. Universities almost certainly will continue to do the bulk of basic scientific research. If they have policies of laying their research results largely open, most of science will continue to be in the commons. However, universities are not in general supporting the idea of a scientific commons, except in terms of their own rights to do research. In the era since Bayh–Dole, universities have become a major part of the problem, avidly defending their rights to patent their research results, and license as they choose.

Bok (2003) has argued persuasively that the strong interest of universities in patenting is part and parcel of trends that have led universities to embrace commercial activities in a variety of areas, for example, athletics as well as science. Earlier, I proposed that Bayh–Dole, and the enhanced interest in universities for patenting, should be regarded as one aspect of a broad increased public acceptance of the importance of intellectual property rights.

But these factors do not make the problem any less significant, only harder to deal with.

I note that the current zeal of universities for patenting represents a major shift from the universities' traditional support of open science. This does not mean that traditionally university research was largely distanced from practical applications. There long have been many university research programs designed to contribute to economic development (see Rosenberg and Nelson, 1994; Mowery and Rosenberg, 1989). Since the late 19th century, university research has played a major role in the development of American agricultural technology. The hybrid seed revolution which was key to the dramatic increases in productivity made during the half century after 1930 in corn and other grain production was made possible by work at agricultural experimentations stations that explored basic concepts and techniques of hybridization. These basic techniques were made public knowledge. Universities also made available on generous terms the pure lines of seeds the universities developed to serve as the basis for commercial efforts to design and produce hybrids. University-based research on plant nutrition and plant diseases and pests helped companies identify and design effective fertilizers and insecticides. Very little of this university research was patented.

American engineering schools and departments have had a long tradition of doing research to help industry. As noted earlier, chemical and electrical engineering were developed as scientific fields largely within universities. Earlier I recounted Stanford's role in developing principles of propeller design. Several universities played key roles in developing the early electronic computers. There was some patenting of devices that came out of university engineering research, but also an apparent continuing commitment to contribute to the advance of basic engineering understanding as the common property of the professions.

American medical schools also long have been contributors to technical advance in medicine and the enhanced ability of doctors to deal with human illness. Medical schools occasionally have been the sources of particular new medical devices and new pharmaceuticals, although this was not common prior to the rise of biotechnology and modern electronics. And while patents were sometimes taken out on particular products (streptomycin, identified by a team led by a Rutgers university scientist, is a good example) by and large until the 1980s there was little patenting, and many medical schools had an articulated policy of dedicating research results to the public commons.

The sea change, or the schizophrenia, began to emerge as a result of several developments (see Mowery et al., 2001). First, during the 1970s and 1980s there was a broad general ideological change in the US in attitudes towards patents, from general hostility in the 1930s and the early postwar years, to a belief that patents were almost always necessary to stimulate invention and innovation Actually, several empirical studies provide evidence that in many industries patents are relatively unimportant as a stimulus to R&D (see Cohen et al., 2000). However, much of the argument for Bayh–Dole concentrated on pharmaceuticals, and patent protection was and continues to be important for pharmaceuticals companies.

There was, second, the rise of molecular biology as a field of science and the development of the principal techniques of biotechnology, which for a variety of reasons made university biomedical research a much more likely locus of work leading to pharmaceuticals or potential pharmaceuticals, and of techniques that could be used in such work. Third, as noted, several key court decisions made many of these developments patentable. The apparent possibility of substantial income from university research clearly attracted some university officials, and university scientists. The patenting of the Cohen–Boyer gene splicing process, and the quick flow of substantial revenues to the two universities that held the rights, provided a strong signal that there now was substantial money that could be brought in from licensing university inventions.

The Cohen–Boyer patent was granted prior to the passage of Bayh–Dole. Bayh–Dole legitimated, even warranted, university patenting. And universities have not been slow in adopting policies where patenting anything that can be patented is the rule.

In my view, there is nothing wrong per se with universities patenting what they can from their research output. In some cases, such patenting may actually facilitate technology transfer, although in many cases it is a good bet that technological transfer is not enhanced but rather the university is simply earning money from what it used to make available for free (see the cases studies in Colyvas et al., 2002). The cases that worry me are ones where the university is licensing exclusively or narrowly a development that is potentially of wide use, or where it is limiting the right to take a particular development further to one or a few companies in circumstances where there still is sufficient uncertainty regarding how best to proceed to make participation by a number of companies in that endeavor socially desirable. The argument that if an exclusive license is not given, no one will try to advance, seems particularly dubious for research tools of wide application, or for findings that appear to open up possibilities for new research attacks on diseases where a successful remedy clearly would find a large market. Thus, the Cohen–Boyer patent was licensed to all comers, and there were plenty of them. The report by Colyvas et al. (2002) gives several examples showing the willingness of pharmaceuticals companies to work from university research findings that appeared to point towards promising treatments, without receiving an exclusive license. I do not see a major problem if access to certain parts of the commons requires a small fee. What I want to see happen is that universities recognize that for research results of these sorts, if they patent them, they have an obligation to license them to all who want to use them at reasonable fees. (Similarly, with respect to "research tools" created by industry research and patented, my difficulty is not so much with those where use is open but users are charged a fee, provided the fee is not too high, but with those that are not made widely available.) Bok (2003, p. 143), recognizing the problem I am discussing here, proposes that the major universities come to an agreement to license widely and easily, not grant exclusive licenses, to research results that basically are inputs to further research. However, a policy of open licensing of research results of certain kinds is not likely to be adopted voluntarily by universities, because this practice will not always be seen as maximizing expected revenues from intellectual property. And that is what many universities are aiming for now.

The recent report signed jointly by a number of university presidents and chancellors, and foundation presidents (Atkinson et al., 2003), shows the tension here. The authors clearly recognize the problem that can, and has, been caused by university patents (their focus is the field of agricultural research) that block or cause high transaction costs for downstream research to advance agricultural technologies, and announce the establishment of a "Public Sector Intellectual Property Resource for Agriculture" which would make access easier. But the authors stop far short of agreeing to a general policy of open licensing of university research results that can set the stage for down stream applied R&D.

Universities will not give up the right to earn as much as they can from the patents they hold unless public policy pushes them hard in that direction. I see the key as reforming Bayh–Dole. The objective here, it seems to me, is not to eliminate university patenting, but to establish a presumption that university research results, patented or not, should as a general rule be made available to all that want to use them at very low transaction costs, and reasonable financial costs. This would not be to foreclose exclusive or narrow licensing in those circumstances where this is necessary to gain effective technology transfer. Rather, it would be to establish the presumption that such cases are the exception rather than the rule.

I note there is nothing in Bayh–Dole that explicitly encourages exclusive or narrow licensing, but nothing discourages it either, and the rhetoric associated with the legislation pushed the theory that generally dedicating research results to the public commons did not encourage use. There is nothing in the legislation that says universities should use their patenting and licensing power to maximize university income, but there is little in the language that discourages that. What is needed, I believe, is language that recognizes much better than the current language that much of what comes out of university research is most effectively disseminated to users if placed in the public domain, and that exclusive or restricted licensing may deter widespread use at considerable economic and social cost.

The act as currently written does include the clause stating that the objective of the act is: "to ensure that inventions made by nonprofit organizations ... are used in a manner to promote free competition and enterprise without unduly encumbering future research and discovery". However, apparently presently this clause has no teeth. My proposal is that this statement of objective be highlighted and supplemented by the proposition that in general this objective calls for licensing that will achieve the widest possible use. Exclusive or narrow licensing by a university should require an explicit rationale. Willingness of firms to take up university research results without an exclusive license should be regarded as evidence that an exclusive license is not appropriate.

Such language would encourage universities to move in the right direction on their own, by strengthening the hand of those at universities who believe that universities should be contributing to the scientific and technological commons. At the present time, such university researchers and administrators seem to be bucking the law as well as internal interests. It also would provide legitimacy to Government agencies funding university research to press for licensing that gives broad access. The recent tussle between the NIH and the University of Wisconsin regarding stem cell patents illustrates the value of such an amended Bayh–Dole. In this case, the University originally had in mind arranging an exclusive license for a firm, and that would have been very profitable for the university. The NIH in effect indicated that unless the university licensed widely and liberally, it would consider the universities licensing policies when evaluating research proposals. The university then went along with the license policies advocated by NIH. Several legal scholars have proposed that, under the current law, the NIH in this case was skating on thin ice. There is nothing in the law that explicitly calls for open licensing. And had the NIH been forced to follow its bark with a bite, they might well have been taken to court. Rai and Eisenberg (2001) make a similar argument for amendment of Bayh–Dole.

Or consider how case analyzed by Bar-Shalom and Cook-Deegan might have gone had the amendment I am proposing been in place. It is likely that the NIH recognized quite early in the game the value of allowing more than one company to work with the new technique for identifying stem cells, and of having widespread research use allowed, and would have balked at the exclusive license that was given had it felt itself on a firm footing for doing so. Later in the game, the NIH was asked to open use of the patented technique, under the 'march in' provisions of Bayh–Dole, but did not do so because the way the legislation is written such a step clearly is exceptional. It would have been in a far stronger position to accede to the request to open up use if the language I propose were in the legislation.

Many university administrators and researchers certainly would resist such an amendment, on the grounds that it would diminish their ability to maximize financial returns from their patent portfolio. As I observed above, the principal support for university patenting with freedom to license as they wish now comes from universities and is based on their perception of their own financial interests; the case for it on grounds that this facilitates technology transfer no longer is credible. If pressed hard, the case that the current policy is against the public interest should carry the day. And it is interesting that, if universities were so constrained in their licensing policies, that might damp their resistance to a research exemption of the sort proposed by Dreyfuss, since the financial costs to them of agreeing not to patent or not to charge for licenses would be diminished.

The burden of this essay is that our scientific commons is in danger, the costs of having it erode further are likely to be high, and that we ought to move to protect it. What I have proposed above is a strategy for protecting the commons.

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