Laboratory of Economics and Management
Sant'Anna School of Advanced Studies

Piazza Martiri della Libertà, 33 - 56127 PISA (Italy)
Tel. +39-050-883-343 Fax +39-050-883-344
Email: lem@sssup.it Web Page: http://www.lem.sssup.it/

LEM
Working Paper Series

Reflections on "The Simple Economics of Basic Scientific Research": Looking Back and Looking Forwards

Richard R. NELSON

Columbia University, New York (U.S.A.)

2006/20 **August 2006**

ISSN (online) 2284-0400

Reflections on “The Simple Economics of Basic Scientific Research” : Looking Back and Looking Forwards

June 11, 2006
Richard R. Nelson
Columbia University

I begin this essay by reflecting on my early paper (Nelson, 1859), and Ken’s (Arrow, 1962), as period pieces. These papers certainly have been influential in shaping the discussion of science and technology policy over the last forty years, at least among economists, but at the time they were written, economists were just beginning to get into analysis of the key processes and institutions involved in technological advance. A lot has been learned since that time, and the discussion has become much more sophisticated. I will highlight two of those intellectual developments: the growing recognition that technological advance must be understood as an evolutionary process, and recognition that the institutions involved in that process include much more than the simple market institutions on which economists usually focus.

Then I turn to the contemporary policy arena that is the focus of this conference: issues relating to intellectual property. Both to link the discussion to my old paper, and because my own recent research has been focused there, I shall focus particularly on the patenting of “science”. Finally, I will consider the institutional division of labor in research and development, and argue that the question of what should be private and what should be public about science and technology cannot be explored adequately without explicit consideration of mechanisms of funding, and of who is expected to do the work under what terms. I will focus particularly on the role of universities in the system of institutions that do R and D.

I. The Emergence of the Economics of R and D in the 1950s and Early 1960s, and Subsequent Developments in the Field

I began my career in this field when I joined a group at the RAND Corporation, headed by Burton Klein, working on the economics of R and D. In preparation for this conference, I went back to several of the early papers I prepared as a member of this group. In addition to my "Simple Economics Of Basic Scientific Research," (1959), I reread my paper on "The Link Between Science and Invention: The Case of the Transistor" (1962) which was published in the same NBER volume, The Rate and Direction of Economic Activity, as was Ken's landmark paper (Arrow, 1962).

I confess going back to these papers with some trepidation. I was just getting into this subject then, and I believe I have learned a lot since that time. I frankly was surprised to find that today I would go along with most (although certainly not all) of the things that I said in those papers. I and the other young scholars who worked with Burt Klein's research group clearly owe a lot to his sophistication and vision, which provided an orientation for all of us. I note that Ken had frequent interactions with Klein and his research group. Sidney Winter also was a part of this group.

The group working on the economics of R and D at RAND was part of an emerging community of economists who were beginning to study research and development, and technological advance, with a strong empirical orientation. That group included Zvi Griliches, Edwin Mansfield, Vernon Ruttan, and Nathan Rosenberg. Joe Peck, and Mike Scherer, also were members of that group, and like the collection of scholars at RAND , were engaged in very detailed work on particular R and D projects, as well as broader analyses. If that group had a senior statesman, it was Jacob Schmookler, whose career was sadly cut short by an early death.

At that time, the Americans were not familiar with Christopher Freeman and his work in the United Kingdom. But the Science Policy Research Unit at the University of Sussex, which Chris formed and led from the mid-1960s, relatively quickly became the leading world institution on the economics of R and D and technological advance.

By the mid-1960s, the community of economists working on R and D and technological advance was growing rapidly. However, by that time it was apparent to me that there was an emerging intellectual split, along two dimensions.

The first was regarding how seriously, and centrally, one should take the Knightian uncertainty highlighted by the RAND research project in one's analysis of R and D and the process of technological advance. If one took it seriously, the then contemporary neoclassical modeling of the subject, which did take aboard risk, in the sense of the agents knowing only a probability distribution of outcomes given any action they would take, missed the point that in many empirical cases the agents did not even imagine the path that actually unfolded, much less being able to assign ex-ante a probability to it. Those inclined to this view, like myself, began trying to construct an alternative theoretical scheme. The evolutionary economic theory that Sidney Winter and I began developing in the late 1960s was born of this effort.

I note that, while a significant group of the economists doing research and writing on technological advance continue to work within a neoclassical framework which treats uncertainty as if it were risk, or at least do obeisance to that framework, by the late 1960s, even before the development of an explicit evolutionary theory, a group of scholars studying technological advance within an implicit evolutionary framework was clearly visible. With the development of an explicit evolutionary theory, the divide has become more apparent. As I suggested above, the heart of the matter is that explicit recognition of Knightian uncertainty requires, on the one hand, the acceptance of Herbert Simon's proposition (1955) that human rationality is bounded, and on the other hand that in contexts where new ways of doing things are continuingly being introduced, extant behaviors and beliefs are going to be some distance away from any equilibrium. These canons are built into the heart of modern evolutionary (Nelson and Winter, 1982). Neoclassical theorizing continues to resist taking them in. To do so clearly would require major modifications in its basic structure.

As I reread Ken's and my old pieces, both of us clearly understood, and highlighted, the uncertainties involved in R and D. However, neither of us yet understood as well as we should have the problem with treating these uncertainties in terms of nicely behaved subjective probability distributions. As I noted above, I came to that realization, which led me towards an evolutionary approach, only a number of years later.

A second divide that was emerging in the late 1960s was about how one should interpret the complex institutional structures involved in R and D. On the one hand, there were economists who viewed the processes and institutions involved as basically market ones, but with a number of public programs and non-market institutions, like universities, involved in the system because of "market failures". On the other hand, there was a growing group of scholars who were coming to believe that one had to recognize the range of institutions, non-market as well as market, engaged in R and D right from the start of the analysis, and that to view the non-market elements as a response to "market failures" was an asymmetric and distorting interpretation of what was going on. For those of the latter persuasion, it was simplistic and biased to try to understand, for example, public support of basic research at universities simply as a "response to market failure", and apparent that an analysis of university based basic research had to get into the nature of relationships in scientific communities, the way science advances, and the traditional roles of universities in science. From this perspective, an explanation of phenomenon A that is largely of the form "B does not work well here" is at best incomplete and for the most part not very illuminating.

I note that a more symmetric view of the institutional division of labor has been, in recent years, associated with the developing concept of "innovation systems" (see e.g. Nelson 1993), which highlights the variety of different institutions involved in different aspects of the innovation process, and their interactions. At the same time, there has grown up a serious body of theoretical and empirical research on "the economics of science" (David and Dasgupta, 1994), and considerable research has been done on the nature of university research and its connections with technological advance (Rosenberg and Nelson, 1994).

As I reread our old papers, Ken and I are looking at things largely through the lens of "market failure" theory, although in my paper on basic research I also highlight why it generally is better to do basic research in universities and public labs than in business labs, because the former are much more conditioned to make research findings fully public. However, I clearly was a long distance then from seeing the rich textured institutional landscape associated with the advance of science, or the need to consider, both positively and normatively, the institutional division of labor.

II. What is the Problem With the Patenting of Science?

It has been long time since I wrote those old papers. Since those early days at RAND, I have firmed up and fleshed out the two points of view that I held then only in embryonic form: that technological progress must be understood as an evolutionary process, and that one needs to recognize the wide variety of institutions that play roles in the process. Both of these perspectives strongly influence the way I look today at issues of science and technology policy. They very much influence my perspective on the problems involved with the patenting of science. (The following part of this paper draws heavily on Nelson, 1994)

Today virtually all empirically oriented scholars would agree that technological progress is evolutionary in the following senses. First, at any time there generally are a wide variety of efforts going on to improve prevailing technology, or to supercede it with something radically better. These efforts generally are in competition with each other, and with prevailing practice. The variety reflects differences in judgments about what prospects are most promising. The winners and losers in this competition are to a considerable extent determined by an ex-post selection process. 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 which radically transform best practice, for the most part technological advance is cumulative. Third, the advanced technologies of any era almost always are the result of the work of many inventors and developers. Technological advance is a collective, cumulative, evolutionary process.

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 illuminates, supports, 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, productive problem-solving methods, etc. However, in recent times virtually all powerful technologies have strong connections with particular fields of science. The nature of those connections, of course, is of central concern here.

The proposition that technological advance is an evolutionary process in the senses I described above 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 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.

There is a widespread belief that modern fields of technology are, in effect, applied science, in the sense that practice is directly drawn from and completely illuminated by scientific understanding, and that advancing technology is largely a routine task of applying new scientific understanding to achieve better products and processes. But this view overstates the illumination science provides to most technologies, and makes the connection between the advance of science and the advance of technology far simpler than it is.

In fact, much of practice in most fields remains only partially understood scientifically. Medical scientists still lack understanding of just why and how certain effective pharmaceuticals do their work, and theories about that change from time to time. Much of engineering design practice involves solutions to problems that professional engineers have learned "work", without any particularly deep understanding of why. And of particular importance here, the pathways to significant advance of a technology are almost never brightly illuminated. Invariably there are differences in opinion regarding the best ways to go, and some fumbling along the way. Thus even in fields where the science base is strong, technological advance remains an evolutionary process.

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, advance in practice comes first, and leads to efforts to understand practice scientifically. Thus, the discovery by Shockley and his team at Bell Laboratories that a semiconductor 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. A non-trivial amount of research in the biomedical sciences is aimed at understanding better why the human body responds, or doesn't respond, to particular substances (including pharmaceuticals) the way it does. Rosenberg (1996) has argued that a number of 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 worked as they did. On the other hand, it is important to recognize that, where such scientific efforts are successful, the pathways to technological improvements can become much better illuminated, if almost never completely clear..

In this modern era, the ability of an agent to be competitive in efforts to advance a technology very often is dependent on that agent's knowledge about and ability to use the relevant underlying science. Much of the relevant science tends to be relatively old, but in many fields new scientific findings and techniques are of critical importance. When the relevant science is open, many parties can engage in the process of advancing the technologies that are illuminated by science. On the other hand, if the relevant science is patented, the patent holder has the option of monopolizing various pathways. This can be a serious obstacle both to the advance of science and to the advance of technology.

When I wrote my paper on "The Simple Economics of Basic Scientific Research," I still was partly captive to the proposition, which unfortunately remains the conventional wisdom now as well as then, that "basic research" in a "scientific discipline" proceeds with little or no awareness of, much less interest in, potential practical applications. My research on the origins of the transistor shook those blinders from my eyes, and I came to recognize that in many scientific disciplines a considerable portion of the work that is called basic research proceeds, as Rosenberg suggested, with puzzles about how technology works, or more general practical problems that have defied solution, very much in mind. Donald Stokes (1996) coined the term "Pasteur's Quadrant" to refer to fields of science where, while research often aims for deep understanding, the field as a whole, and programs of research in the field, are dedicated quite explicitly to solving particular kinds of practical problems, and advancing bodies of potentially practical knowledge. Here one certainly would place the engineering disciplines, fields like oncology, material science, or computer science.

A number of fields are a kind of hybrid. Thus, molecular biology is at once perhaps the field in biology that probes most deeply at the nature of life processes, and at the same time where a considerable amount of the research is aimed toward understanding the basic biological processes involved in human disease, and illuminating the pathways and mechanisms through which those diseases might be prevented or treated.

In fact, by far the lion's share of modern scientific research, including research done at universities, is in fields where practical application is central to the definition of the 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) asked industrial R and D executives to identify the fields of academic research that contributed most to their successes in R and D. The fields they listed were mostly in Pasteur's Quadrant.

In my old paper on basic research, I assumed that it was not legally possible to patent laws of nature or characterizations of natural phenomena that are created through basic research. I turned out to be wrong on this. A lot of science turns out to be patentable, under the current interpretation of the law. After I did my research on the history of the transistor, I should have recognized then, but didn't, that issues about "patenting science" were likely to crop up in fields in Pasteur's Quadrant. And that, of course, is what has happened.

I also argued, way back then, that it would not be a good thing for the advance of technology, much less the advance of science, if basic scientific findings were patented. Here I think I was, and am, perfectly right. As I have stressed, the advance of practice in a field remains an evolutionary process, and rapid sustained progress requires the involvement of many minds and competing ideas as to the best ways to make progress. Effective participation in advancing a field where new science provides important guides as to where to go and how requires ability to work with and from that science. Where key elements of the science are patented, the patent holders have the power to control who is able to work with that science, and in a number of recent cases have used that power to keep out competitive researchers.

Most analyses of the effects of granting patents assume that the patents are on new final products (consumer goods or production equipment) or the processes use to make them. The social costs of the control given by a patent are seen in terms of higher prices on the products than would be the case if the technology were open for all to use, and the benefits seen in terms of higher incentives for inventing. Until quite recently, there was little recognition that the patented material could be primarily an input to research and development. When this is the case, the presumption that the granting of patents spurs the advance of technology may be quite wrong. And that is my concern about patents on science.

I find that many people are puzzled when they learn that patents are being taken out on genes or genes codes, or more generally are intruding into the realm of science. Many well informed and sophisticated people believe today, as Ken and I did when we were writing those old papers, that scientific facts or principles or natural phenomena more generally are not patentable. And 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 U.S. patent law case was concerned with whether purified human adrenaline was a natural substance and hence not patentable (although it was conceded that the process of purification certainly was patentable), or whether the fact that adrenaline 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. 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.

Thus consider (Bar-Sholam and Cook-Deegan, (2004) a patent recently granted on a monoclonal antibody (antibodies are natural substances, but homogeneous antibodies cloned by a particular process have been judged not to be natural) that 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. I would argue that the inclusion in the patent claim of "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 that the particular antibody actually used was just an exemplar.

Note that the discovery (or the invention?) here certainly was not something that could be used directly to deal with human diseases, but rather knowledge and technique useful in doing research that might result in something practical. The patentability of a research output whose use is largely in future research seems almost inevitable in Pasteur's Quadrant, for obvious reasons. The research in question clearly was fundamental, but at the same time was aiming to find or develop techniques that would be useful in dealing with cancer. And the research results seem to have been a useful contribution to furthering that aim.

The argument I made many years ago is that the social value of these kinds of advances would be much greater if they were left open and available for all researchers to use, than if they were patented and the patent used to restrict access to them. I certainly did not dream then that these kinds of patents could be used to block relatively wide paths of research. Yet this is what happened in this case.

It is standard, not atypical, that when a patent is granted the technology still requires considerable work before it is operational, and success may require considerable creativity as will as significant investments of time and resources. In the case considered here, research that resulted in the patent was done by an oncologist at Johns Hopkins university, and the patent was then licensed exclusively to a single company who was interested in doing follow-on research and exploring commercial possibilities. However it turned out that another company achieved a workable process for isolating stem cells first. They were sued for violation of the (obviously very broad) patent. While the company argued that the method they had used was outside the scope of the patent, the Court ruled that there was infringement, and the offending company in effect closed down.

Another well known case involves the patents on the genes BRAC1 and BRAC2 held by Myriad Genetics. The particular character of these genes in a woman provides clues as to her susceptibility breast cancers. Obviously Myriad did not create these genes, but rather was the first to map out the chemical sequences, and something about their variance. Similarly to the case discussed above, the patent that was granted gave the patent holder the right to exclude all other agents from testing to see if a woman possessed versions of these genes indicating susceptability. Perhaps even more serious, in the United States Myriad has used its patent, and threat of suit to control and restrict the research going on that involves these genes, their variants, and their products. I note that, in the EU, the scope of these patents have been considerably restricted.

These are just examples of the problem. While it is not now clear how widespread and serious the problem presently is, there is ample evidence that there is a problem here and that it will get wider and more serious, unless some changes are made in patent law and practice.

What are the things that can be done? First, one can urge more care not to grant patents on discoveries that are largely of natural phenomena, by requiring a strong case that the subject matter of the patent application be "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 results of research that are a "substantial transformation" of the natural. The lines here are blurry. But the slope 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 a case of purified natural substances, this would call for a greater proclivity to limit the patent to process and not allow the purified product per se to be patented.

Second, one can urge a stricter 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 research aimed to achieve something obviously useful. I have been arguing that the problem here is that progress towards a practical goal is likely to come more quickly if the knowledge and capability were not monopolized and access kept closed, as in the cases above. A stricter interpretation here would require a more compelling demonstration of significant progress toward a particular practical solution than seems presently required, and particularly if combined with the suggestion below about the reigning in of patent scope, would be a major contribution to dealing with the problem.

Third, there is the issue of 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" which identify a particular substance is a case in point. While there are obvious advantages to the patentee 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 not allowing such broad blocking of potential competitive efforts.

In my view there is particular importance in 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 exemption for research not commercially oriented into patent law, analogous to the fair use exemptions in copyright law. However, for reasons I trust I have illuminated above, it often is not easy to distinguish sharply between an endeavor that is aimed at advancing understanding and one that is aimed at identifying a path towards a commercial product; many research project in the sciences in Pasteur's Quadrant involve both.

A possible way out of this problem would be to limit the research exemption to non-commercial research organizations, perhaps just universities. In the United States, universities for a long time proceeded as if there was such a research exemption for the work they did. However, the recent decision of the Federal Circuit (Madey vs. Duke, October 2002) has changed thinking regarding this. While the Court's ruling that there was no research exemption in this case was based on the proposition that universities benefit financially from the research they do, through increased ability to get grants, attract first rate students, enhanced reputation generally, the fact that in recent years universities have been active in seeking patents and making money on the intellectual property they have must have influenced the thinking of the Court.

In an earlier paper I suggested the following structure for a research exemption (which is a variant of a proposal put forth by Rochelle Dreyfus, 2002). A university or not-for-profit laboratory would be immune from prosecution for using patented materials or phenomena in research if (1) access to what is patented were not available on reasonable terms and (2) the university agreed not to patent anything that came out of the research (in effect a "copy-left" agreement) or if they did patent to allow access on a nonexclusive royalty free basis . Under this structure, a university would have a de-facto general research exemption if it adopted a policy of not patenting its research outputs.

This issue leads me directly into the next topic I want to address: the institutional division of labor in R and D, and the role of universities.

III. What Should Be the Role of Universities?

Until quite recently, virtually all writings by economists on technological advance focused on for-profit firms and profit seeking entrepreneurs as the key actors. My early paper on basic research, and Zvi Griliches's on hybrid corn (1957) were relatively lonely exceptions. As I suggested earlier, for the most part economists who did recognize the role of public finance for certain kinds of R and D, and of non-profit organizations, like universities, in the undertaking of R and D, tended to see these as responses to "market failures", rather than in terms of programs and institutions with strengths and limitations in their own right. More recently, scholars of technological advance have begun to see more clearly the variety of funding and performing institutions involved in modern innovations systems, and to explore the advantages and disadvantages of different kinds of arrangements for doing different kinds of things.

In our old papers, Kenneth and I both recognize that for-profit firms spending their own money on R and D is not an arrangement that will get much basic research done. In my paper I sought to explain why some big firms do have significant programs of basic research, but that was a prelude to my argument that the major part of the funding burden needed to be on government. I also proposed there that universities were a better locus than industry for the performance of publicly funded basic research, because both the motives for university researchers, and general beliefs regarding the mission of universities, encouraged open publication. Obviously I did not anticipate the surge of university patenting that has occurred over the past quarter century.

My argument above is that policies should be adopted that would cut back significantly on the patenting of basic research findings, and enable basic research to proceed with little encumbrance from patents. But wouldn't the cutting back of patentability sharply reduce the incentives of for-profit companies, for example biotech research firms, to engage in such research? And, given the argument that supported passage of the Bayh-Dole Act of 1980, wouldn't a cutting back of university patenting, and strong pressures for broad non-exclusive licensing, interfere with technology transfer from universities to industry?

My answer to the first question is "almost certainly yes". But there are good reasons to believe that the biotech research firm is not a viable kind of enterprise anyhow, at least in the long run. Very few of these kinds of firms have made profits, and there is no indication that the situation is becoming more favorable for them. And in any case, for the reasons I proposed a long time ago, it might be better to have universities substitute for biotech firms by increasing their undertaking of the kinds of basic research these firms do, particularly if universities were restrained from exclusive licensing of the research results except under particular circumstances.

My answer to the second question is that there is hardly any evidence that, as a general rule, exclusive licensing of university research results is necessary if industry is to pick up and develop those results, and growing evidence against the argument. There certainly are exceptions. But the case studies developed in Colyvas et al (2002) show only a few cases in which the exclusive licensing of a university patented "invention" was a necessary part of the technology transfer incentives and mechanisms, and a number where clearly it was not. It is noteworthy that in the most famous case of industry pick-up of a university patented technique in biotech, the Cohen-Boyer gene splicing process, industry was using the technique broadly before the patent even was granted. While the universities and the university scientists who did the work got a lot of money out of patent licenses, the license fees charged should be understood as a tax on use collected by the universities, and certainly not as a vehicle that was necessary for "technology transfer"

Historical experience argues against the need for university patenting to effect technology transfer. (See Rosenberg and Nelson, 1994). Thus since the late nineteenth century, university research in the United States 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 university affiliated agricultural experimentation stations. These organizations explored basic concepts and techniques of hybridization, and the techniques they developed were made public knowledge. Experimentation stations also made available on generous terms the pure lines of seeds they had 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. Chemical and electrical engineering were developed as scientific fields largely within universities. 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. And while patents were sometimes taken out on particular products (streptomycin identified by team led by Rutgers University scientists 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.

The sea change in university patenting that has occurred over the last quarter century has been the result of several developments (see Mowery et al. 2001). First, during the 1970s and 1980s there was a broad, general, ideological change in the United States in attitudes toward patents, from general hostility in the '30s and the early post-war years, to a belief that patents were almost always necessary to stimulate invention and innovation. This belief was already in place when, in the late 1970s and early 1980s, worries about Japanese technological competition rose up in the United states, along with the theory that the Japanese were stealing American technologies that were not adequately protected by patents. Some viewed this latter problem as often involving inadequately protected findings of Government funded research done at universities.

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 can be used in such work. And by 1980 several key court decisions had ruled that new forms of life, and a significant part of the findings coming out of biomedical research, were patentable.

Third, in Congressional hearings on patent policy, arguments were made to the effect that, especially for university research results that seemed to point the way to new pharmaceuticals, a patent plus the ability to grant an exclusive license was necessary if industry was to pick up the project and do the additional work needed to develop and introduce to the market a new pharmaceutical. I have argued above that this argument was made far too broadly, and that as a general rule exclusive licensing is not necessary for technology transfer. Nonetheless, Congress bought the argument, and in 1980 passed the Bayh-Dole Act.

Even before Bayh-Dole, the apparent possibility of substantial licensing income from university research clearly attracted the attention of some university officials and university scientists. The patenting of the Cohen-Boyer gene-splicing process and the quick flow of substantial revenues, which occurred prior to 1980, provided a strong signal that there now was substantial money that could be brought in from licensing university inventions. While the notion that universities can get rich from licensing revenues is, except for a few cases, misguided, dreams die hard. Universities will not give up the right to earn as much as they can from their patenting unless public policy pushes them hard in that direction. In the United States, I see the key as reforming Bayh-Dole. What I say below also signals the advice I would give to countries that already have their equivalent of Bahy-Dole, and to countries contemplating new policies regarding university patenting of the results of publicly funded research.

My preferred position would be to make it very hard for universities to patent, except in exceptional cases, but my belief is that the road to that reform is now too steep. A second best solution, which would complement the reforms I have proposed above for patenting in general, would be not to try 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 who want to use them at very low transaction costs, and reasonable financial costs. This would not foreclose exclusive or narrow licensing in circumstances where this is necessary to gain effective technology transfer. But it would establish the presumption that such cases are the exception not the rule.

Regarding the Bayh-Dole Act itself, the problem is more in the ideology that has surrounded it than the language in the Act itself. There is nothing in the legislation that says that exclusive licensing is usually the best way to effect technology transfer, or that a strong if secondary objectives is to enable universities to make as much money as they can. But there is widespread belief that the former is true, and without question Universities have taken the Act to be a mandate to make money.

What is needed, I believe, is language that recognizes much better than current language that what comes out of university research most often is most effectively disseminated to users if placed in the public domain, and that exclusive or restrictive licensing should be employed only when there is evidence that this is needed to effect transfer. Willingness of firms to take up university research results without an exclusive license should be taken as evidence that an exclusive license is not appropriate. To grant an exclusive license a university would have to give evidence that it has attempted to license non-exclusively, but had no reasonable takers.

I want to stress that my position that the results of university research generally should be open and available for all to use should not be confused with an argument that universities should be "ivory towers". From the days I wrote my early article on basic research, I have seen university research as a vital part of the system that generates technological progress. Rather, my argument is that university research has made and can continue to make a wider and more powerful contribution to the solving of practical problems, and to the advance of technology, if as a general rule the research results are placed in the public domain freely available to all, rather than being patented.

I am constrained by limits on space and time from elaborating an argument that the reforms I outlined above would help pull universities back from the schizophrenic position they have moved into in recent years, on the one had arguing that they are public institutions dedicated to the advance and spread of knowledge, and on the other hand that they are active players in commercial enterprise. The literature on the importance of corporate "coherence", and the generally unhappy experience of firms with conglomerate lines of business, indicates strongly that it is a mistake to try to be all things to all people. I think it obvious that the central mission of universities should be the traditional one of the advance and spread of knowledge, and that this role requires dedication to having knowledge in the public domain.

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