Science and Technology: Which Way does the Causation Run ?

Nathan Rosenberg
Stanford University
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Not to prolong your suspense, the correct answer to the question in the subtitle of my paper is the obvious one: causation runs both ways. But I want to persuade you that the causation running from technology to science is vastly more powerful than is generally realized.

The reasoning is straightforward. A market economy generates powerful incentives to undertake certain kinds of scientific research. This is because the eventual findings of such research can be made to improve the performance, or to reduce the cost, of technologies that are vital to the competitive success of profit-making firms. Further, I want to suggest that there were powerful forces at work in the course of the 20th century that had the effect of expanding the ways in which changes in the realm of technology have led to changes in the various realms of science. I want to call your attention to some of the most significant organizational changes, and associated changes in incentives, that were responsible for strengthening the causal forces that flowed from technology to science.

In order to do this, I will need to introduce just one single bit of jargon: I will use the term "endogenous" from the perspective of the economist and not from the perspective of the scientist. Thus, when I refer to the endogeneity of science, I am referring to the extent to which scientific progress has been directly influenced by the working out of the normal forces of the market place. My justification is that I will be trying to identify forces that emerged in the course of the twentieth century that made scientific research more highly responsive to economic incentives.

I also need to emphasize one caveat that I cannot emphasize too strongly. I am not implicitly suggesting that the financial support of the country's scientific research should be left to the market place. Rather, I will be calling attention to the operation of market forces that have become increasingly supportive of scientific research. I believe that these developments were crucial to the rapid expansion of American industry, but that is very different from suggesting that market forces, by themselves, were sufficient.

Corporate Research Labs

The proposition that scientific research became increasingly endogenous in the course of the 20th century must necessarily begin by focusing on a key organizational innovation: the industrial research lab. It was these corporate labs that determined the extent to which the activities of the scientific community could be made to be responsive to the needs of the larger economy. But such a statement, by itself, cannot stand alone. This is because these research labs depended for their effective performance, in turn, upon a network of other institutions. These included, above all, research at universities. Before the Second World War, university research depended heavily, for its financing, on private philanthropic foundations, such as the Rockefeller, Guggenheim and Carnegie foundations. In the pre-war period, as well, universities often relied on financial support from local industry for carrying out certain classes of research, mostly of an applied nature. This was especially true of state universities, where it was essential to provide evidence of assistance to local industry [agriculture, mining, railroads] in order to justify the imposition of taxes upon the citizens of each state. In fact, with few exceptions, funds raised by state governments went, overwhelmingly, to support teaching and not research.

This situation was totally transformed in the post World War II period when the federal government became, overwhelmingly, the dominant patron of scientific research, and universities became the primary locus of such research. It is important to note that the concentration of basic scientific research in the university community where, I think it is fair to say, it has flourished, has been an organizational arrangement that has been almost unique to the US. Unlike the situation in western Europe, where basic research has been concentrated in government labs (Max Planck, CNRS) federal laboratories in US have accounted for less than 10 percent of basic research (9.1% in mid 1990s).

A further distinctive feature of great importance in the US is the very large commitment of private industry to scientific research that the NSF defines as basic. Private industry accounted for slightly over 30% of all basic research in the year 2000 (probably declining slightly in last few years). Although at last count there were around 16,000 private firms that had their own corporate labs, the vast majority of these firms conduct research of a predominantly applied nature. Only a very small number do basic research. Nevertheless, over the years, a few of these corporate labs have conducted research of the most fundamental nature - General Electric, IBM and, most important of all, Bell Labs before the divestiture of AT&T in 1984. Researchers in a number of corporate labs have won Nobel Prizes, most recently Jack Kilby, of Texas Instruments, won the Prize for Physics, in the year 2000, for research leading to the development of the integrated circuit [Kilby's research received financial support from the federal government].

Having said this, it is essential to realize that the research activities of industrial labs should not be evaluated, as they often are by academics, by the usual academic criteria - such as publications in prestigious professional journals or the winning of Nobel Prizes. Such labs have a very different purpose. The industrial lab is essentially an institutional innovation (of German origin) in which the research agenda is largely shaped by the short-term needs but also, in a few notable cases, by the longer-term strategies of industrial firms. Within the industrial context, the intended role of corporate scientists is to improve the performance of their respective firms in the competitive context of (mostly) high tech sectors of the economy. Thus, the critical achievement of the growth of the American industrial lab in the course of the 20th century has been to subject science, more and more, to commercial criteria. In so doing, it has rendered science an activity whose directions were increasingly shaped by economic forces and concentrated on the achievement of economic goals - which is to say such scientific research should be regarded as largely endogenous.

One further strategic role of the corporate lab arises from the fact that a firm cannot effectively monitor and evaluate the findings, and the possible implications, of the huge volume of university research unless it has its own internal capability for doing such things. The importance of this point cannot be overestimated. In advanced industrial societies that are now simply flooded with the flow of information, not only from universities, but from professional journals on library shelves or electronically via Internet search engines such as Yahoo and Google, the exploitation of this vast flow of information requires an internal competence that, typically, only inhouse scientists can provide. Indeed, America's remarkable commercial successes in high tech markets over the past 50 years have owed a great deal to these internal competences in private industry. Industrial scientists have played a critical role in the transfer of potentially useful knowledge generated by university research, not only because of their scientific sophistication, but also because they have had a deep awareness of their firms' commercial priorities and technological capabilities **[See Nathan Rosenberg, "Why do firms do Basic Research?" (<u>Research Policy</u>); Nathan Rosenberg, "America's University/Industry Interfaces, 1945-2000", unpublished manuscript, May 2002;**

and David Mowery and Nathan Rosenberg, <u>Paths of Innovation: Technological Change in 20th America</u>, Cambridge University Press, 1998].

<u>How Engineering Disciplines have Shaped Science</u>

I would like now to call your attention to another major force for advancing the endogeneity of science in the course of the 20th century. I would like to pose the question: what specific role is played by engineering disciplines in determining the scientific agenda of private firms? Let me respond, first, by offering a clarification. It is a common practice to characterize engineering disciplines as being essentially applied science. This is, in my view, a seriously misleading characterization. A more careful unwinding of the intertwining of science and technology suggests that the willingness of profit-seeking firms to devote money to scientific research is very much influenced by the prospect of converting such research findings into finished and marketable products. The actual conduct of scientific research may not be undertaken with highly specific objectives in mind, but rather with an increased confidence that, whatever the specific research findings, an enlarged engineering capability will substantially increase the likelihood of being able to use these findings to bring improved or new products to the market place.

From this perspective, there is a serious sense in which the economist may argue that the science of chemistry should be thought of as an application of chemical engineering!

Alternatively put, the growing sophistication of engineering disciplines has had the result of strengthening the endogeneity of science. I do not want this point to be made to sound too paradoxical. I mean to suggest that the willingness of private industry to commit financial resources to long-term scientific research has been considerably strengthened by the progress of the appropriate engineering disciplines. Such progress raises the confidence of corporate decisionmakers that the findings of basic research may eventually be converted to profitable uses.

This argument seems particularly pertinent to the specialty of polymer chemistry, a field that was opened by the researches of Staudinger, Meyer and Mark in Germany in the 1920s. In the US at least, polymer chemistry is a field that has long been dominated by the industrial research community. The fundamental research contributions to polymer chemistry of Wallace Carothers at du Pont, beginning in 1928, owed a great deal to the increasing maturity of chemical engineering in the preceding decade or so, an engineering discipline to which du Pont had made important contributions **[See Hounshell and Smith]**. Carothers' research findings led directly to the discovery of nylon, the first of a proliferation of synthetic fibers that came to constitute an entirely new subsector of the petrochemical industry after the Second World War. But it is doubtful that du Pont would have committed itself to Carothers' costly, fundamental researches in polymer chemistry, in the first place, in the absence of the progress in chemical engineering in the decade preceding 1928. Thus, progress at the technological level (chemical engineering) increasingly strengthened the willingness to spend money on science, which I regard as a growth in the endogeneity of science.**[See**

Nathan Rosenberg, "Technological Change in Chemicals: The Role of University-Industry Relations," chapter 7 in Ashish Arora, Ralph Landau and Nathan Rosenberg (eds.), Chemicals and Long-term Economic Growth, Wiley, 1998].

Let me sketch out the intermediate steps that underly my argument. The discipline of chemical engineering really had its beginnings in the second and third decades of the 20th century, mainly at MIT, in response to the spectacular expansion of the automobile industry and, along with that industry's growth, a voracious demand for refined chemical products (primarily, of course, for high octane gasoline). The scale of that growth can be captured in the following numbers: In 1900 the automobile industry was so insignificant that the Census Bureau classified cars under the category "Miscellaneous." [In that year there were only 8,000 registered cars in the US]. By 1925 the automobile industry had leaped to the status of the largest manufacturing industry in the whole country (measured by value added).

It was the growth of the automobile that gave birth to the discipline of chemical engineering. Chemical engineers, during the 1920s and later transformed the petroleum refining industry from small-scale batch production into one of vastly larger scale and continuous processing. The emerging chemical engineering discipline accomplished this by developing a new conceptual framework within which it became possible to introduce scientific concepts and methodologies from such fields as fluid flow (fluid dynamics), heat transfer and, in the 1930s, the pervasive power of thermodynamics. In other words, the design of chemical process plants could now draw heavily upon a number of different scientific realms. Thus, it was the establishment of a new engineering discipline, in responding to the rapid expansion of a new transportation technology, that, in turn, laid the basis for the profitability of scientific research, not only in du Pont and petroleum refining firms, but in a very wide range of industries that also made use of chemical process plants. It is worth emphasizing how pervasive chemical process plants became in the course of the 20th century. Large chemical plants could be found in petroleum refining, rubber, leather, coal (by-product distillation plants), food-processing, sugar refining, explosives, ceramics and glass, paper and pulp, cement, and metallurgical industries (e.g., aluminum, iron and steel).

How New Products Have Shaped Science

The next related observation with respect to the growing endogeneity of scientific research goes beyond the role played by engineering disciplines in strengthening the private incentives to perform scientific research. The argument here is that the development of some specific new **product**, that is perceived to have great commercial potential, may provide, and often **has** provided, a powerful stimulus to scientific research. This proposition is surprising only if one is already committed to a rigid, overly simplistic linear view of the innovation process, one in which causality is always expected to run from **prior** scientific research to "downstream" product design and engineering development. There is in fact, however, a straightforward endogenous explanation at work here. A major technological breakthrough typically provides a strong signal that a new set of profitable opportunities has been opened up in some precisely-identified location. Consequently, it is understood that scientific research that can lead to further improvements in that new technology may turn out to be highly profitable.

The problems encountered by sophisticated industrial technologies, and the anomalous observations and unexpected difficulties that they have encountered, have served as powerful stimuli to much fruitful scientific research in the academic community as well as the industrial research laboratory. In these ways the responsiveness of scientific research to economic needs and technological opportunities has been powerfully reinforced.

This was dramatically demonstrated in the case of the advent of the transistor, the discovery of which was announced at Bell Labs in the summer of 1948. Within a decade of that event solid-state physics, which had previously attracted the attention of only a small number of researchers and was not even taught at the vast majority of American universities (mainly MIT, Princeton, and Cal Tech) had been transformed into the largest subdiscipline of physics. It was the development of the transistor that changed that situation by dramatically upgrading the potential financial payoff to research in the solid state. J.A. Morton, who headed the fundamental development group that was formed at Bell Labs after the invention of the transistor, reported that it was extremely difficult to hire people with a knowledge of solid-state physics in the late 1940s. Moreover, it is important to emphasize that the rapid mobilization of intellectual resources to perform research in the solid state occurred in the university community as well as in private industry, immediately **after** the announcement of the momentous findings of Shockley and his research colleagues at Bell Labs. As one strong piece of evidence for this view, the number of publications in semiconductor physics rose from less than 25 per annum before 1948 to over 600 per annum by the mid-1950s **(Herring, unpublished manuscript, n.d.)**.

The chronology of the events that I have just referred to is essential to my argument. Transistor technology was not the eventual consequence of a huge prior buildup of resources devoted to solid-state physics, although it was of course also true that some of the twentieth century's most creative physicists had been devoting their considerable energies to the subject. Rather, it was the initial breakthrough of the transistor, as a functioning piece of hardware, that set into motion a vast subsequent commitment of financial support for scientific research. Thus, the difficulties that Shockley encountered with the operation of the early point-contact transistors led him into a systematic search for a deeper explanation of their behavior, expressed in terms of the underlying quantum physics of semiconductors. This search not only led eventually to a vastly superior amplifying device, the junction transistor; it also contributed to a much more profound understanding of the science of semiconductors. Indeed, Shockley's famous and highly influential book, <u>Electrons and Holes in Semiconductors</u>, drew heavily upon this research, and the book was the direct outgrowth of an in-house course that Shockley had taught for Bell Labs' personnel. Moreover, Shockley also found it necessary to run a six day course at Bell Labs in June 1952 for professors from some thirty universities as part of his attempt to encourage the establishment of university courses in transistor physics.

Clearly, the main flow of scientific knowledge during this critical period was from industry to university, and not the other way around. Indeed, for a considerable period of time, Stanford and the University of California at Berkeley had to employ scientists from local industry to teach courses in solid-state physics/electronics.

A similar sequence can be seen in the commitment of funds to research in surface chemistry, after problems with the reliability of early transistors pointed in that direction. More recently, and to compress a much more complex chain of events, the development of laser technology suggested the feasibility of using optical fibers for telephone transmission purposes. This possibility naturally pointed to the field of optics, where advances in scientific knowledge could now be expected to have potentially high economic payoffs. As a result, optics as a field of scientific research experienced a great resurgence in the 1960s and after. It was converted by changed expectations, based upon recent and prospective technological innovations, from a relatively quiet intellectual backwater of science into a burgeoning field of research. This growth of activity in the discipline was generated, not by forces internal to the field of optics, but by a radically altered assessment of the potential opportunities for laser-based technologies. Moreover, different kinds of lasers gave rise to different categories of fundamental research. As Harvey Brooks has noted: "While the solid-state laser gave a new lease of life to the study of insulators and of the optical properties of solids, the gas laser resuscitated the moribund subject of atomic spectroscopy and gas-discharge physics."**[Harvey Brooks, "Physics and the Polity," Science, 1968, vol. 160].**

I draw the conclusion from this examination that, under modern industrial conditions, technology has come to shape science in the most powerful of ways: by playing a major role in determining the research agenda of science as well as the volume of resources devoted to specific research fields. One could examine these relationships in much finer detail by showing how, throughout the high tech sectors of the economy, shifts in the technological needs of industry have brought with them associated shifts in emphasis in scientific research. When, for example, the semiconductor industry moved from a reliance upon discrete circuits (transistors) to integrated circuits, there was also a shift from mechanical to chemical methods of fabrication. When Fairchild Semiconductors began to fabricate integrated circuits, they did so by employing new methods of chemical etching that printed the transistors on the silicon wafers and also laid down the tracks between them. This chemical technique did away with expensive wiring, and also produced integrated circuits that operated at much higher speeds. At the same time, the increased reliance upon chemical methods brought with it an increased attention to the relevant subfields of chemistry, such as surface chemistry.

I cite the experience of changing methods of wafer design and fabrication to indicate the ways in which the changing needs and priorities of industry have provided the basis for new priorities in the world of scientific research. But it is essential to emphasize that these new priorities exercised their influence, not only upon the world of industrial research, but upon the conduct of research within the university community as well. I need only point out that Stanford University has had, for some time, its own Center for Integrated Systems. This Center is devoted to laboratory research on microelectronic materials, devices, and systems, and is jointly financed by the federal government and private industry.

SERENDIPITY

There is a further source of causation running from technology to science to which I would like to call your attention. I refer to the role of serendipity. It is, of course, to be expected that well-trained scientific minds are likely to turn up unexpected findings in many places. As Pasteur expressed it in the mid-19th century: "Where observation is concerned, chance favors only the prepared mind." By way of contrast, consider Thomas Edison, by universal consent a brilliant inventor, but someone who had little interest in observations that had no immediate practical relevance. In 1883 he observed the flow of electricity across a gap, inside a vacuum, from a hot filament to a metal wire. Since he saw no practical application and had no scientific training, he merely described the phenomenon in his notebook and went on to other matters of greater potential utility in his effort to enhance the performance of the electric light bulb. Edison was, of course, observing a flow of electrons, and the observation has since even come to be referred to as the "Edison Effect" - named after the man who, strangely enough, had **failed** to discover it. Had he been a curious (and patient) scientist, less preoccupied with matters of short-run utility, Edison might later have shared a Nobel Prize with Owen

Richardson who analyzed the behavior of electrons when heated in a vacuum, or conceivably even with J.J. Thomson for the initial discovery of the electron itself. Edison's "prepared mind," however, was prepared only for observations that were likely to have some practical relevance in the short-run.

A distinctive feature of the 20th century in dynamic capitalist economies was the vastly-increased numbers of scientifically "prepared minds" in both the universities and private industry. The pursuit of the possible implications of unexpected observations became the basis, on many occasions, for fundamental breakthroughs that occurred serendipitously when "prepared minds" were available to pursue the possible implications of the unexpected. Surely the most spectacular instance of serendipity in the 20th century - not achieved in an industrial laboratory - was Alexander Fleming's brilliant conjecture, in 1928, that the unexpected bactericidal effect that he had observed in the bacterial cultures in his Petri dish, was caused by a common bread mould that had accumulated on his slides. Fleming published this finding in 1929, but no substantial progress was made in producing a marketable product until more than a decade later, when the exigencies of wartime led to a joint, Anglo-American "crash" program to accelerate the production of the antibiotic **[Elder, Albert Lawrence (ed.), <u>The History of Penicillin Production</u>, American Institute of Chemical Engineers, New York, 1970].**

It is at least a plausible speculation that, had Fleming made his marvelous discovery while working in a pharmaceutical lab, penicillin would have become available, in large quantities, far more swiftly than was in fact the case **[For a contrary view, see Bernal, volume 3, pp. 926-7]**. In the context of this paper it is also worth pointing out a little-known historical fact, that the technology to produce the antibiotic in bulk was achieved not, as would ordinarily have been expected, by the pharmaceutical chemist, but by chemical engineers. It was the chemical engineers who demonstrated how a technique called "aerobic submerged fermentation," which became the dominant production technology, could be applied to this complex product **[Elder, <u>op. cit</u>].**

The growth of organized industrial labs in 20th century America vastly enlarged the number of trained scientists in the industrial world who encountered strange phenomena that were most unlikely to occur, or to be observed, except in some highly specialized industrial context. In this sense, the huge increase in new high tech products, along with dense concentrations of well-trained scientific specialists in industry, sharply increased the likelihood of serendipitous discoveries in the course of the twentieth century.

Consider the realm of telephone transmissions. Back at the end of the 1920s, when transatlantic radiotelephone service was first established, the service was discovered to be poor due to a great deal of interfering static. Bell Labs asked a young man, Karl Jansky, to determine the source of the noise so that it might be reduced or eliminated. He was given a rotatable antenna to work with. Jansky published a paper in 1932 in which he reported that he had found three sources of noise: Local thunderstorms, more distant thunderstorms, and a third source which he described as "a steady hiss static, the origin of which is not known." It was this "star noise" as Jansky labelled it, which marked the birth of the entirely new science of radio astronomy.

Jansky's experience underlines why the frequent attempt to distinguish between basic research and applied research is extremely difficult to carry out in practice. Fundamental scientific breakthroughs often occur while dealing with very applied or practical problems, especially problems relating to the performance of new technologies in an industrial context.

But the distinction breaks down in another way as well. It is essential to distinguish between the personal motives of the individual researchers and the motives of the decisionmakers in the firm that employs them. Many scientists in private industry could honestly say that they are attempting to advance the frontiers of basic scientific knowledge, without any concern over possible applications. At the same time, the motivation of the research managers, who decide whether or not to finance research in some basic field of science, may be strongly motivated by expectations of eventual useful findings.

This certainly appears to have been the case in the early 1960s when Bell Labs decided to support research in astrophysics because of its potential relationship to the whole range of problems and possibilities in the realm of microwave transmission, and especially in the use of communication satellites for such purposes. It had become apparent that, at very high frequencies, annoying sources of interference in transmission were widely encountered.

This source of signal loss was a matter of continuing concern in Bell Labs' development of the new technology of satellite communications. It was out of such practical concerns that Bell Labs decided to employ two astrophysicists, Arno Penzias and Robert Wilson. Penzias and Wilson would undoubtedly have been indignant if anyone had suggested that they were doing anything other than basic research. They first observed the cosmic background radiation, which is now taken as confirmation of the "Big Bang" theory of the formation of the universe, while they were attempting to identify and measure the various sources of noise in their antenna and in the atmosphere. It seems fair to say that this most fundamental breakthrough in cosmology in the past century was entirely serendipitous. Although Penzias and Wilson did not know it at the time, the character of the background radiation that they discovered was just what had been postulated earlier by cosmologists at Princeton who had devised the Big Bang theory. Penzias and Wilson shared a Nobel Prize in Physics for this finding. Their findings were as basic as basic science can get, and it is in no way diminished by observing that the firm that had employed them did so because the decisionmakers at Bell Labs hoped to improve the quality of satellite transmission.

The parallelism between the fundamental discoveries of Jansky and Penzias and Wilson is, of course, very striking. In both episodes, the Bell Labs researchers stumbled upon discoveries of the greatest possible scientific significance while involved in projects that were motivated by the desire of Bell Labs to improve the quality of telephone transmission. In the case of Penzias and Wilson, they were conducting their research with a remarkably sensitive horn antenna that had been built for the Echo and Telstar satellite projects. Wilson later stated that he was originally attracted to work at Bell Labs because working in the Labs would provide access to a horn antenna which was one of the most sensitive of such antennas in existence **[Steve Aaronson, "The Light of Creation - an Interview with Arno A. Penzias and Robert C. Wilson," Bell Laboratories Record, January 1979, p. 13].**

I have called attention to 2 episodes at Bell Labs in which industrial researchers discovered natural phenomena of immense scientific significance while the firm that employed them did so in the hope that they would solve serious problems connected with the performance of a new communications technology. In one sense it is fair to say that important scientific findings by profit-making firms are sometimes achieved unintentionally - they have discovered things that they were not looking for, which I take to be the generic meaning of Horace Walpole's mid-eighteenth century neologism - serendipity. Such breakthroughs in the private sector, moreover, are difficult to understand if one insists on drawing sharp distinctions between basic and applied research on the basis of the motivations of those performing the research. I find it irresistible here to invoke, once again, the shade of the great Pasteur: "There are no such things as applied sciences; only applications of science."

In fact, I would go much further: when basic research in industry is isolated from the other activities of the firm, whether organizationally or geographically, it is likely to become sterile and unproductive. Much of the history of basic research in American industry suggests that it is likely to be most effective when it is highly interactive with the work, and the concerns, of applied scientists and engineers within the firm. This is because the high technology industries have continually thrown up problems, difficulties and anomalous observations that were most unlikely to occur outside of specific high technology contexts.

The sheer growth in the number of trained scientists in industrial labs, along with the growth of new, highly complex, specialized products that appeared in the course of the 20th century, powerfully increased the likelihood of serendipitous findings. High tech industries provide a unique vantage point for the conduct of basic research but, in order for scientists to exploit the potential of the industrial environment, it is necessary to create opportunities and incentives for interaction with other components of the firm. Bell Labs before divestiture (1984) is probably the best example of a place where the institutional environment was most hospitable for basic research. I do not suggest that Bell Labs was, in any respect, a representative industrial lab. Far from it. It was a regulated monopoly that could readily recoup its huge expenditures on research. But, perhaps even more important, it came to occupy a location on the industrial spectrum where, as it turned out, technological improvements required a deeper, scientific exploration of certain portions of the natural world that had not been previously studied.

INSTRUMENTATION

Of course my examination of the endogeneity of science has been no more than a very modest and partial sketch. Entire categories of the influence of technology upon science have been completely ignored here, such as the pervasive impact of new instrumentation, i.e., technologies of observation, experimentation and measurement. Indeed, scientific instruments may be usefully regarded as the capital goods of the research industry. Much of this instrumentation, in turn, has had its origins in the university world and, to underline the extent of the intertwining of technology and science in recent years, some of the most powerful of those instruments, such as Nuclear Magnetic Resonance, had their origins in fundamental research that was originally undertaken in order to acquire some highly specific pieces of knowledge, such as a deeper understanding of the magnetic properties of atomic nuclei. Indeed, Felix Bloch was awarded Stanford's first Nobel Prize in physics for precisely such research. **[See N. Rosenberg, "The Economic Impact of Scientific Instrumentation Developed in Academic Laboratories," in John Irvine et al., Equipping Science for the 21st Century, Edward Elgar, 1997. See also, in the same volume, Carlos Kruytbosch, "The Role of Instrumentation in Advancing the Frontiers of Science," chapter 2.].** Nuclear Magnetic Resonance spectroscopy, in turn, became an invaluable tool in chemistry for determining the structure of certain molecules (e.g., hydrogen, deuterium, boron and nitrogen atoms **(Kruytbosch, pp. 32-4).**

Clearly, instrumentation and techniques have moved from one scientific discipline to another in ways that have been highly consequential for the progress of science. In fact, it can be argued that a serious understanding of the progress of individual disciplines is generally unattainable in the absence of an examination of how different areas of science have influenced one another. This understanding is frequently tied directly to the development, the timing and the mode of transfer of scientific instruments among disciplines. The flow of "exports" appears to have been particularly heavy from physics to chemistry, as well as from both physics and chemistry to biology, to clinical medicine and, ultimately, to the delivery of health care. There has also been a less substantial flow from chemistry to physics and, in recent years, from applied physics and electrical engineering to health care. NMR eventually became the basis for one of the most powerful diagnostic tools of twentieth (and twenty-first) century medicine (MRI).

The transistor revolution was a direct outgrowth of the expansion of solid-state physics, but the successful completion of that revolution was in turn heavily dependent upon further developments in chemistry and metallurgy which provided materials of a sufficiently high degree of purity and crystallinity. Finally, physics has spawned subspecialties that are inherently interdisciplinary: for example, biophysics, astrophysics and materials science.

One further point, however, is implicit in what has already been said. The availability of new or improved instrumentation or experimental technique in one academic discipline has often been the source of interdisciplinary collaboration. In some critical cases, it has involved the migration of highly trained scientists from one field to another, such as those physicists from the Cavendish Laboratory at Cambridge University who played a decisive role in the emergence of molecular biology. This emergence had depended heavily upon scientists, trained in physicists' skills at Cavendish, who transferred the indispensable tool of x-ray crystallography into the very different realm of biology. Molecular biology was the product of interdisciplinary research in the special sense that scientists trained in one discipline crossed traditional scientific boundary lines and brought the intellectual tools, concepts and experimental methods into the service of an entirely new field [See the magisterial, yet highly accessible volume by Horace Judson on the early history of molecular biology, The Eighth Day of Creation].

The German physicist, Max von Laue, discovered the phenomenon of x-ray diffraction in 1912. Its applications were, in the early years, employed by William Bragg and his son, Lawrence Bragg, primarily in the new field of solid-state physics but also, later on, in developing the field of molecular biology. The main center of the methodology of x-ray diffraction was, for many years, the Cavendish Laboratory, presided over by Lawrence Bragg. Numerous scientists went there in order to learn how to exploit the technique, including Max Perutz, at the time a chemist, James Watson, Francis Crick, John Kendrew, all later to receive Nobel Prizes in Physiology and Medicine?). The transfer of skills in x-ray diffraction was facilitated by the unusual step of the establishment of a Medical Research Council unit at the Cavendish, headed by Perutz but under the general direction of the physicist Lawrence Bragg **[Francis Crick, What Mad Pursuit, Penguin 1988, p. 23. James Watson later reported Bragg's**

obvious delight over "...the fact that the X-ray method he had developed forty years before was at the heart of a profound insight into the nature of life itself." The Double Helix (Simon Schuster, 1968) p. 220].. To infer the three-dimensional structure of very large-molecule proteins by the new technique of x-ray crystallography, which offered only two-dimensional photographs of highly complex molecules, appears to have been a hellishly difficult enterprise, but it provided much of the basis for the new discipline of molecular biology. Rosalind Franklin who, sadly, died very young, is widely agreed to have been the most skilful practitioner of x-ray crystallography.

Moreover, it is important to observe that the two separate communities - university scientists (including medical school clinicians) and commercial instrument makers - interacted with and influenced one another in ways that were truly symbiotic. Precisely because these two communities marched to the tunes of very different drummers, each was ultimately responsible for innovative improvements that could not have been achieved by the other, had the other been acting alone **[See Annetine Gelijns and Nathan Rosenberg, "Diagnostic Devices: An Analysis of Comparative Advantages," chapter 8 in David Mowery and Richard Nelson (eds.), Sources of Industrial Leadership]**. It should be added that the applications of physics research have usually moved more readily across disciplinary boundary lines in industry than they have in the academic world. Profit-making firms are not particularly concerned with where those boundary lines have been drawn in the academic world; they tend to search for solutions to problems regardless of where those solutions might be found **[NRC 1986]**.

Thus, the technological realm has not only played a major role in setting the research agenda for science, as I have argued. Technology has also provided new and immensely more powerful research tools than existed in earlier centuries, as is obvious by mere reference to electron microscopy in the study of the micro-universe, to the Hubble telescope in the study of the macro-universe, and to the laser, which has become the most powerful research instrument throughout the realm of the science of chemistry. In addition, the laser has found a wide range of uses in medical care.

Finally, since this article has been written within easy walking distance of the Stanford Linear Accelerator, it seems appropriate to close with the following observation: in the realm of modern physics it appears that the rate of scientific progress has been largely determined by the availability of improved experimental technologies. In the succinct formulation of Wolfgang Panofsky, the first director of SLAC: "Physics is generally paced by technology and not by the physical laws. We always seem to ask more questions than we have tools to answer." Exactly.