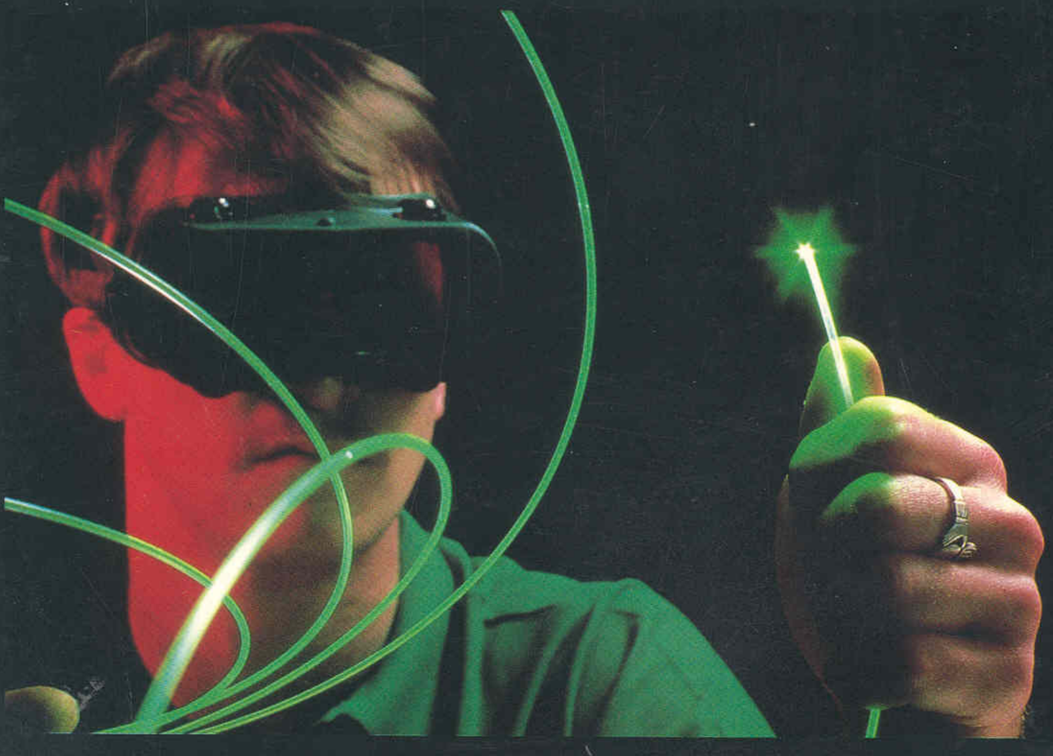
INNOVATION

AT THE CROSSROADS BETWEEN SCIENCE AND TECHNOLOGY



Edited by

M. Kranzberg, Y. Elkana and Z. Tadmor

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The S. NEAMAN PRESS

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INTRODUCTION

Why bother with expediting technological innovation? Some fifty years ago William Fielding Ogburn, a great American sociologist, claimed that there was a "cultural lag" in society's adoption of and adaptation to new technologies - and, to a certain extent, Ogburn's concept still holds true today. For while our futurists envision current technological advances as leading to a "post-industrial society" and a new "information age" wherein mankind will be free from laborious toil, and where enhanced productivity will provide ample supplies of food, energy, and housing to meet the growing needs of the world's growing population, we still seem far away from this Utopian ideal.

Indeed, many social critics put the blame for the world's ills on the development of our industrial technology, claiming that technology has grown so large and presented mankind with such awful byproducts that it threatens to engulf man. After all, they say, look at the inhuman uses to which technology has been put: What about the devastation wrought by wars throughout the centuries and the present possibility of destroying the human race and much of our planet through nuclear warfare? What about the deterioration of the environment created by air and water pollution, the spoliation of the countryside, the rot of our cities, the degradation and starvation of many peoples on the face of the earth? Technology, it is claimed, has destroyed the ecological balance between man and nature; not only that, but we are threatening mankind's future on this earth by profligate and wasteful use of polluting materials which might make our planet unliveable through climatic change, and we are robbing future generations of their inheritance by plundering the earth of irreplaceable natural resources.

Must all our troubles be ascribed to technological advance? Let us not forget that long before modern technology took root, pre-industrial man was destroying the environment and living in a state of constant impoverishment. The "Fertile Crescent," the home of the Biblical Garden of Eden, had been denuded by human failure to continue control over riverine water supplies; North Africa, the "breadbasket of the Roman Empire," had turned into an arid desert as a result of warfare, over-grazing, and human ignorance of ecological factors; and the "Promised Land" of "milk and honey" had become a ravished land - until today's Israelis, by a combination of hard work and applications of scientific technology, began making the desert bloom again.

Those critics who claim that technological advance is ruining our planet and that it is bringing suffering to mankind should take a closer look at the pragmatic realities. If modern technology is so harmful to man and nature, how do we account for the fact that the standard of living is higher in the industrialized portions of the world than in those where technology lags behind? Why are the citizens in the industrial nations better fed and longer-lived? Why is more being done to protect the environment and ecology in the technologically-advanced nations than in those which still retain a primitive, pre-industrial technology?

Why is it that, with but few exceptions, the most technologically-developed countries are those which enjoy the greatest amount of democratic freedom, have eradicated cruel and degrading punishments, uphold religious freedom, and, in short, endorse fundamental human rights? Why are the most industrially-advanced states the ones which have provided for equality of the sexes, abolished child labor, condemned racial discrimination, recognized the rights of workers to associate, developed social security systems for the aged, and, in short, upheld the concepts and practices of social justice? Can all this be ascribed to mere coincidence?

However, the above is not meant to imply that modern technology's advance is freeing mankind from all its problems and is totally innocent of harmful byproducts that bespoil Spaceship Earth and endanger its species. Instead, many of the dangers which threaten the world derive from misuse and abuse of technology, resulting oftentimes from human greed and hunger for power or sometimes from ignorance of the impacts of technology, such as the atmospheric effects of the byproducts of fossil fuel combustion, and the like.

As a result, we have learned that some technological efforts to improve the human condition and to make nature more hospitable to mankind have long-term effects which might prove harmful to mankind. Thus, for example, DDT was employed to do away with pests which damage crops and spread disease; then we discovered that DDT not only did that, but it affected the food chain of birds, fishes, and eventually man himself. So the Western industrialized nations banned DDT. But should this deter us from continuing to search for pesticides which will accomplish the same purposes without harm to the ecology?

Similary, modern medical technology and sanitary treatment of water and sewage supplies did away with some tropical diseases which had long plagued mankind; but along with this benefit, it led to a population "explosion" in certain tropical lands where sociopolitical conditions prevented the application of improved agricultural technologies which could provide sufficient food for their growing populations - a condition deriving from modern sanitary and medical technology which had reduced the threat of plague and infantile mortality. In brief, we need more and better scientific technology to cope with some unexpected problems created by the successes of our past technological applications.

This does not mean that technology by itself will provide us with Utopia. In the United States some two decades ago, youthful proponents of a "new consciousness" or "counter-culture" turned their backs on modern technology and advocated a "return to nature." Although they provided trenchant criticism of unthinking reliance on technology, they failed to demonstrate that their anti-technological lifestyle could solve problems like poverty, medical care, air pollution, and the hunger of most of the world's people. Instead, it became clear that an unvarnished anti-technological stance would mean the abandonment of all hope that we would ever be able to improve the lot of mankind and would bring us back to Thomas Hobbes's version of man's natural state: nasty, brutish, and short.

Does anyone seriously believe that we can meet the future food, water, and material needs of mankind by cursing technology? Can one truly object to our encouraging our scientific technology so as to meet the energy and food needs of tomorrow's world?

But this does not mean that a group of technocrats or a scientific and technological elite should make decisions for the future directions of our scientific-technological research and its applications. For the point is that many of the problems facing mankind today and tomorrow involve technology, human values, social organization, environmental concerns, economic resources, political decisions, and a host of other factors. These are "interface problems," that is, the interface between technology and society, and they can only be solved - if they can be resolved at all - by the application of scientific knowledge, technical expertise, social understanding, and humane compassion.

These interface problems affecting our future so greatly have another feature in common: scientists and engineers cannot solve them alone, yet they cannot be resolved without the aid of our scientific technology. Indeed, today's scientific technology is so innovatively productive that it provides us with many different options for resolving difficulties and for meeting new needs. The incorporation of political, economic, sociocultural, and other factors in our considerations provides the opportunity to make informed choices among these various options".

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And that is why the **S. Neaman Institute** sponsored this workshop at the Technion-Israel Institute of Technology. Inasmuch as science and technology have recently been coming together in a more fruitful fashion to advance the interests of mankind, scholars representing many fields of knowledge - the humanities and social sciences, as well as science and technology - participated in this meeting. At the same time, because the scientific-technological enterprise has become increasingly international in membership, the conference was international in its scope. And because today's scientific technology is carried on in academic, governmental, and industrial settings, representatives from these different institutional settings were included. We hope that the results of this international workshop, embodied in the contributions in this book, will help us learn how we can best utilize our growing capabilities in science-technology to advance the future betterment of mankind.

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THE ECOLOGY

OF INNOVATION AND POLICIES

THE DYNAMIC ECOLOGY OF INNOVATION

M. Kranzberg

The title of this international workshop, "Innovation at the Crossroads Between Science and Technology," conjures up a picture of science and technology going along separate roads and, when they meet at a crossroads, an innovation somehow appears. But that simplistic notion does not apply to most of human history, for innovations occurred long before science and technology began meeting one another. However, scientific and technological activities are increasingly coming together in planned, rather than accidental meetings, and that, plus a number of associated changes, is indeed changing the ecological environment for innovations.

Being a historian, I view innovation in historical terms in order to obtain perspective on the nature and dynamism of this changing ecology of innovation. Such historical understanding might enable us to guide the innovation process more meaningfully in the future. But first let us define what is meant by science, technology, and innovation; then let us see how these have changed over time and are still changing today.

Both science and technology are bodies of knowledge, including the means by which that knowledge is obtained. They are processes, institutions, and, indeed, social forces.[1]

To avoid overlong explanations and confusion between the two, I give my students simple working definitions of science and technology: Science is concerned with "knowing why," that is, comprehending natural phenomena and the principles underlying them; while technology is concerned with "knowing how" to make and things which is why we often refer to it as "know-how."

One can make and do things without necessarily knowing the physical principles underlying them. Indeed, for much of history men made and did things without knowing the chemical or physical composition of the materials they employed nor the scientific laws governing their operations. They relied mainly on practical, hands-on experience derived from the past.

As for innovation, it is not quite the same as invention, but they are related. Joseph Schumpeter, the famed economist, pointed out that invention is the initial event in creating a new tool or technique, while innovation is the final event, that is, when the new tool or technique is generally implemented and put to work, thereby bringing about changes in the way men make and do things.[2]

Innovation then, is a process involving the coupling of technical creativity -- inventiveness, with social needs, resource availability, economic constraints, and opportunities. It is affected by political policies, military requirements, and a host of other factors, including organizational, managerial, and marketing skills.[3] All these elements interact with one another as well as with the technical elements, and their combined interactions represent the ecology of innovation.

Just as there are different fields of science and technology, so there are various innovation types. These range from rather minor ("nuts-and-bolts") changes in methods or materials, to singular "breakthrough" innovations which change the character of an industry or create new ones, including sophisticated innovations involving entire "systems."[4]

"Nuts-and-bolts" changes are small, incremental advances, often as a result of suggestions for improvements offered by factory workers, who are very familiar with the technical problems in production, or by product users who desire something slightly different or better.[5] Their suggestions for incremental changes in techniques or materials can add up to major economies as well as better quality products.

Innovations of the "breakthrough" or "system" nature, however, are increasing dependent upon bringing science and technology together in new ways. The dynamic interplay between science and technology, and among the many other elements comprising the environment of innovation, has changed throughout history and is changing rapidly today.

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First let us look at several popular myths regarding innovation. One is the old saying, "Necessity is the mother of invention." But this "necessity" explanation is incomplete: many needs have not yet mothered inventions, and there are many unneeded innovations.

Furthermore, because in many cases inventions require additional technical advances in order to become fully effective, one might turn the adage around to state, "Invention is the mother of necessity." For every innovation seems to require additional technical advances in order to make it fully effective. Thus Alexander Graham Bell's telephone spawned a variety of technical improvements, ranging from Edison's carbon-granule microphone to central-switching devices.[6]

Innovative products require a bundle, or cluster, of technologies that are mutually supportive and also have an associated infrastructure. For example, the domestic washing machine required the development of small and inexpensive electric motors, new materials for insulating and isolating the water from the electric heating elements, electro-mechanical control mechanisms, and also that the household be connected to electric power lines and have adequate water supply and drainage.[7]

The fact is that inventions spawn other inventions, or to put it another way: "Technology comes in packages." The automobile shows how a successful technology begets auxiliary technologies to make it fully effective, for it brought whole new industries into being and turned existing industries in new directions by its need for rubber tires, petroleum products, and new tools and materials. Furthermore, large-scale use of the auto demanded many additional activities -- roads and highways, garages and parking lots, traffic signals and parking meters.

A popular view of the diffusion of innovation derives from a saying attributed to Ralph Waldo Emerson, an influential 19th-century American thinker: "If a man can...build a better mousetrap than his neighbor, though he builds his house in the woods, the world will make a beaten path to his door." Emerson's statement focuses attention on the linkage of social needs with inventive activity, but the fact is that many "better mousetraps" never achieve success, as shown by the small percentage of patented devices that even make it to the marketplace. Emerson failed to take account of the role of the entrepreneur in bringing together ideas, men, money and customers -- to popularize an innovation. Nor was he aware of the importance of diffusing information so that the potential customers would indeed make a beaten path to the door.

Perhaps the most powerful cliche, however, is the notion that scientific discoveries lead inevitably to technical applications. That idea derived from the fact that 19th-century discoveries in electromagnetism and chemistry gave rise to whole new industries, so that technology became known as "applied science." But if the notion that technological innovations derive from scientific discoveries can be said to have any validity, it would hold only for relatively modern times and only in a few cases. Yet Alan Waterman, the first head of the National Science Foundation, claimed that there is "statistical evidence...that most of the body of science ultimately achieves practical utility."[8] Although Waterman never adduced that "statistical evidence," it is almost impossible to quarrel with such a statement. After all, if one pointed to some scientific concept which never achieved practical application, one would simply be told, "Just wait until 'ultimately' comes along."

Nevertheless, this view of technology as deriving from science -- "applied science" -- exerts great power. It underlies the argument for government support for basic scientific research on the grounds that such research pays off in useful applications -- and that has indeed been true often enough to justify such support.

A similar notion is that innovation can come about on command. This idea is based on two questionable propositions. One is faith in the ability of science and technology to meet every challenge put to them. The other is the "men-money" syndrome, namely, the belief that "throwing money" at problems, that is, investing in human beings and facilities to carry on scientific research and technical development,will eventually produce desired innovations. During World War II, crash programs, notably the atom bomb and synthetic rubber projects, provided historical examples of such success -- as did the Space Program in the 1960s.

But throwing men and money at problems does not always bring the hoped-for results. Thus a great deal of money and effort have gone into cancer research; while some treatments have emerged that alleviate much pain and prolong some lives, the "war on cancer" has not yet been won. Similarly, America's investment in solar energy and synthetic fuels research during the "energy crisis" of the 1970s resulted in few, if any, breakthroughs, and the projects were abandoned by the Reagan Administration as being an unnecessary waste of money.

In 1776 Adam Smith's **Wealth of Nations** stimulated interest in industrial innovation by stating that a nation's wealth derived from its productive capacity. Some two centuries later Robert M. Solow won the Nobel Prize in economics for making much the same point. Although some economists such as Ralph Landau, might stress other elements in creating wealth, all agree on the importance of encouraging research and development to produce innovations.[9] Since technological innovativeness appears to be a prime factor in improving the material lot of mankind, economists and scholars from related fields thus became involved in tracing the cause and course of innovation.

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When scholars began investigating how and why inventions appear, two basic approaches emerged: social deterministic and individualistic. The deterministic explanation holds that an innovation occurs when the time is "right," and it stresses the role of external factors social, military, and economic in bringing about technological change. The individualistic, also known as the "great man" or "heroic" theory, stresses the role of the individual in invention and plays down the role of external influences.

But the individualistic and social deterministic theories are not mutually exclusive; they are really matters of emphasis. No scholar has ever claimed that the individual innovator runs his own race entirely unbridled or unaffected by external pressures, and even the most ardent adherent of the deterministic school recognizes that the manifold socioeconomic forces operate through individuals.

Not surprisingly, composite theories evolved which attempted to bring together the two theories. R.B. Dixon, the anthropologist, claimed that invention occurred as a result of a combination of "need, opportunity, and genius" the first two representing societal elements, while "genius" represented the contribution of the individual who saw the need, recognized the opportunity, and applied his creative talents to devise something that would fit the need.

Hence the modern view regards creativity as an interactional process between the individual and social environment, or as a social process in which the individual participates.[10] But how does innovation actually take place? And how can it be encouraged?

When a decade ago the NSF sponsored a research project at Georgia Tech to survey the literature on technological innovation, our preliminary analysis employed a linear-sequential approach similar to A.P. Usher's theory of innovation (founded upon Gestalt psychology), as expounded in his **History of Mechanical Inventions.**[11] Our block diagram showed five separate functional phases with arrows going directly from one to the next: (1) problem-definition and idea-generation; (2) invention of the prototype device; (3) research and development; (4) application, meaning the first use; and (5) diffusion, that is, its spread into a wider context.

However, as we moved more deeply into our study, we discovered that this scheme was too simplistic to explain the complexities of the actual innovation process. For example, although the obvious starting point in the innovation process would seem to be problem-definition-and-idea-generation, in many of our case studies the creative idea arose during research- and-development, or even during the diffusion phase. Indeed, in virtually every major innovation of recent times each functional phase was linked in some way to the others -- and not necessarily in a causal manner, but rather in a feedback relationship. So, instead of a linear, sequential picture of a neat flow chart with single lines going from one box to another, we ended up with a graphic portrayal of a plate of spaghetti and meatballs!

We were forced to conclude that technological innovation proceeds in a social, political, economic, and cultural environment where all parts interact with one another; in brief, an ecological system wherein many individual strands -- psychological, sociological, economic, technological, managerial, and the like - were eventually woven together to create an innovation.[12]

Entrepreneurial business practices are often essential in producing and distributing an innovation, which is why Peter F. Drucker[13] pointed out the importance of the market in successful innovations. Indeed, it is now recognized that whether "demand-pull" (responding to a perceived need) or "technology-push" (the drive to find applications prompted by the emergence of new technology), product innovation seems to require "an interactive loop, racing between research, marketing, and the consumer."[14]

In brief, many different factors enter into the innovation process. Most recently the confluence of scientific and mathematical knowledge with technological expertise has produced revolutionary changes in the innovative process. How and why did this occur?

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Throughout most of history there was a gap between science and technology in subject matter, methodology and institutions.[15] Science historically stems from philosophical, theological and speculative inquiries about nature in ancient societies; technology, on the other hand, derives from the arts and crafts carried on by humble artisans in the same societies. Scientists were considered intellectually and morally superior to technologists, as exemplified by Plato's distinction between the superior works of the mind compared with those of the hand.

This dichotomy between science and technology persisted throughout the Middle Ages. In that Christian "Age of Faith", study of nature was regarded as an adjunct to understanding God; so what we call "science" was a branch of theology, which was termed the "Queen of the Sciences" that is, the highest form of knowledge. Technology, still carried on in the craft tradition by menial or servile beings, thus continued to remain wholly separate from discourses on the meaning of the physical universe.

Nevertheless, there was some slight interaction between the two in medieval times, with technological innovations stimulating scientific speculation. Thus the mariner's compass, which came into Europe from China, provoked investigation of magnetic phenomena; later, gunpowder led to consideration and calculation of forces, impetus, and trajectories.

By the 17th century, however, a series of social, political, cultural, and economic changes had produced a secular attitude which freed science from its preoccupation with theological matters. The Age of Discovery and the growing wealth in Europe stimulated the consumption of material goods and spurred the advance of technology. Yet the association between science and technology remained both tentative and ambivalent.

In the Scientific Revolution, Galileo's reliance upon experiment brought him into the realm of technology: he built one of the first telescopes, a hydrostatic balance, a thermoscope, and, in general, advanced the art of instrumentation, which represents technology in the service of science. The other major figure in 17th-century science was Isaac Newton, and Robert Merton has pointed out that although Newton was influenced in his choice of problems by socioeconomic matters, the Scientific Revolution was not impelled by technical considerations.[16]

Yet the Scientific Revolution changed the medieval rationale for investigating nature to explain and extol the glories of God -- to understanding nature for its own sake. However, Sir Francis Bacon, not himself a scientist, but a courtier and philosopher of ideas, began linking science to technology, albeit indirectly. For Bacon regarded knowledge as power, and he claimed that scientific knowledge was especially powerful in making nature useful to mankind. Bacon's followers founded the Royal Society, whose aim was to promote "useful knowledge."

Nevertheless, the Royal Society and its Continental counterparts became devoted almost entirely to basic science, rather than utilitarian applications of science. As a result, separate societies of "arts" were founded in the 18th century for improvement of the practical arts: crafts, industries, agriculture, and husbandry. Thus science and technology still remained apart, institutionally and in practice.

Then came the Industrial Revolution. It did not derive from science any more than the Scientific Revolution owed its origins to technology, but it was eventually to bring the two together by changing the nature of the process of innovation.

The landmark inventions of the 18th and early 19th centuries came from men who were tinkerers with practical knowledge, but without special training in science. Although James Watt had some contact with Dr. Joseph Black, the discoverer of latent and specific heats, his steam engine owed little to Black's science, but derived from Watt's active curiosity and tinkering ability. Development of the steam engine led scientists to think about the relations of the steam engine led scientists to think about the relations of heat, force, and energy, thereby giving birth to the science of thermodynamics. But the point is that science was brought in to explain the physical forces at work in a device whose origins derived from workaday problems rather than from scientific inspiration.

Shortly thereafter, however, Faraday's scientific experiments on electromagnetic induction had the opposite effect, giving birth to an electrical technology derived from scientific experiments and theories. Similarly, laboratory investigations in organic chemistry led to synthetic dyes and other innovations in industrial chemistry. These developments in electricity and chemistry helped give rise to the notion that technology was merely the application of scientific discoveries.

While scientific findings were being translated into new technologies, technology itself was becoming more "scientific." As Edwin T. Layton has shown this was not merely "applied science'; instead, the growing complexity of technical devices required that technologists employ mathematics and develop scientific methods of their own.[17]

For the older craft tradition of innovation (the cut-and-try method) could not cope with increasingly elaborate machinery or apply recent discoveries in thermodynamics, electricity, and chemistry. Some understanding of science and training in mathematics were necessary for technological operations and/or innovations. However, since scientists had not investigated these phenomena in terms of application, the technologists had to conduct their own scientific investigations in order to fulfill their technological goals. Also some higher education was essential, since apprenticeship training was inadequate for this task. Hence many institutions of higher technical learning were established in the last half of the 19th century.

While engineers were becoming increasingly involved in mathematics and science, scientists in their turn began requiring more technical expertise for their experimental investigations. For example, tracing the evolution of experimental physics in the early decades of this century, the late Derek Price found that it increasingly depended upon elaborate technical instrumentation, and he implied that modern science had become "applied technology."[18] The dependence of scientific research upon technological instrumentation has since grown to the point where Philip H. Abelson states that today's technical instruments "often shape the conduct of research. They make discoveries possible. They determine what discoveries will be made."[19]

Paradoxically, even though an advancing technology required a science and mathematics base and scientists were becoming increasingly reliant upon technical devices for their own research, scientists and technologists, for the most part, still retained their professional autonomy and departmental differences within academia.

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While academicians were wrestling with the role of science and the amount of mathematics in an engineering curriculum, the industrial world began to institutionalize a new relationship between science and technology. This change in the environment for both science and technology took the form of the industrial research laboratory, whose beginnings can be traced to the German chemical and pharmaceutical industries during the latter 19th century. The industrial research laboratory - the research and development (R & D) lab - provided an institutional mechanism for applying science to technology.

Thomas Edison's West Orange Laboratory, opened in 1887, was the forerunner of the industrial research laboratory in the United States, but historians regard the General Electric Laboratory, established in 1900, as the first true R&D lab in the United States, for it pioneered in basic research as well as commercial applications. Its first director was Willis R. Whitney, a graduate of MIT who had achieved a chemistry doctorate in Germany and then returned to teach at MIT. His idea was to bring together scientists and engineers from various fields to work together in an organizational context whose goal was to develop utilizable products, and if need be, to undertake basic scientific research in order to assist that goal.[20]

Whitney's successes at GE encouraged other leading American industrial firms to establish corporate research laboratories. Since new technologies were rapidly coming into being and threatening existing markets, the main product of these laboratories was to be patents, which the corporations could use to protect their markets as well as create new ones.[21]

Bringing together science and technology in the industrial environment proved fruitful for both, and following World War I corporate laboratories grew in number and the phrase "industrial research" came into widespread use.[22] While industries had **used** science before, they now began **doing** science themselves, for the development of patentable devices and processes seemed necessary to assure a firm's competitive ability and hence its survival.

In the industrial research lab, the old divisions between science and technology grew meaningless, for, in the process of developing profitable innovations, the labs also contributed to "pure" science. Indeed, researchers from industrial research laboratories won -- and continue to win -- Nobel Prizes. This mingling of scientific and technological activity within the R&D laboratory made the old labels of "basic" and "applied" science lose much of their meaning. Instead, Lewis Branscomb, former head of the National Science Board and Chief Scientist of IBM, stated that one should not use either term, but simply speak of "applicable" science -- and in this context virtually every scientific act is "applicable" to some technological need.

Nevertheless, until World War II the lines of demarcation between science and technology in terms of place, personnel, and purpose still remained fairly clear. Corporate R&D laboratories sought patentable products. In academia, faculty and graduate students conducted modest research, funded largely by the university and with some grants from foundations; their research was pedagogical in purpose, that is, the teaching of scientific methods and the scientific "culture." The Federal government provided only modest sums for research, principally in agriculture, natural resources, and for the military.

World War II brought about major changes in the ecology of innovation. For one thing, engineers and scientists worked together to produce such new technologies as radar and the atomic bomb, further breaking down the old disciplinary barriers between them. Also, following the War, the U.S. government laboratories grew in size, number, and stature, and the Federal government gave financial assistance to scientific-technological research through the Department of Defense, National Institutes of Health, and the National Science Foundation.

Nevertheless, vestiges of the old division of labor remained. As Christopher Hill states, "Universities were to carry out basic research, unrelated to market or society demands, and to educate scientists and engineers for the other sectors.... The federal government was to supply funds to support university research, while its laboratories were to concentrate on applied research and technology development to meet the government's own missions."[23]

For almost two decades America maintained the scientific and technical predominance with which it had emerged from World War II. But mounting political and economic pressures made the U.S. government aware of the need to enlarge the crossroads between science and technology in order to encourage innovation.

First came the chilling impact of the Cold War, when America's security seemed threatened by Russia's development of nuclear weapons, and then American technological preeminence was challenged by the Russian Sputnik and the subsequent Space Race. That increased the U.S. government's awareness of the role of scientific and technological activities in power politics. But added to this were mounting economic pressures deriving from the spectacular growth of foreign competition, as production became increasingly internationalized, reflecting the globalization of the science-technology enterprise.

Such developments forced Americans to examine more closely the structure and functioning of their science-technology establishment. The sharply-separated functions of science and technology among government, industry, and academia were found to be "dysfunctional", interfering with the need to communicate and transfer science and technology from universities to industry, within industry, and from federal laboratories and industrial contractors to one another.

To remedy this situation, the U.S. government in 1960 began to encourage interdisciplinary research and break down institutional barriers. For example, disciplinary divisions in the study of materials such as separate departments of metallurgy, ceramics, and the like were no longer meaningful in the face of the growing complexity and sophistication of scientific-technological knowledge and practice. Since research had demonstrated that various materials differed from one another primarily in their structure, it made sense to investigate them holistically. So the government utilized block grants to establish interdisciplinary Materials Research Laboratories at a dozen universities -- and materials science and engineering became a unified field of study.

In addition to taking cognizance of the blurring of the old disciplinary divisions, the U.S. government also sought to encourage innovative research by breaking down the old barriers within industry and between academia and industry. The National Cooperative Research and Development Act (1984) facilitated joint research among industrial firms by offering them immunity from antitrust actions; at the same time, the government also began sponsoring cooperative research centers between industry and universities in order to stimulate innovation.

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Most unsettling to Americans was the Japanese competition. Japanese products had long been looked down upon as cheap and shoddy imitations of American technology. But that view began to change when the Japanese started systematically to develop their technical prowess.

First they obtained the newest technologies by acquiring licenses to manufacture American products; then by lower labor costs, investment in the most modern production machinery, and better managerial organization - pioneered by Americans but ignored by American industry - they began outdoing the United States in automobiles and steel. The Americans tended to shrug it off at first, claiming that Japanese productivity was due primarily to cheap labor; but the fact was that superior machinery enabled the Japanese steel worker to produce more steel tonnage per man-hour of work than his American counterpart. In the automotive area, Japanese management practices produced cars that were superior in quality, as well as cheaper than American ones.

To combat this challenge, Americans sought to keep ahead by innovations in "high-technology" defined loosely as anything having to do with electronics. But here again the Japanese began beating the Americans at their own game, taking the leadership in consumer electronics products and in making powerful computer chips.

However, the Japanese in their turn have now begun to face competition from Korea, Taiwan, and other parts of the globe. For an entirely new factor has entered into world technological competition: the internationalization of production.

The global diffusion of technical know-how means that innovations made in one country can sometimes be more successfully commercialized in another. Thus, although American firms pioneered in home electronic products, such as TV sets and microwave ovens, the Japanese so improved the products and manufacturing methods that today virtually all such devices marketed in the United States are made in Japan, despite their American trademarks.

The point is that the older industrialized nations no longer have a manufacturing monopoly. Things come from all over: automobiles from Korea and Yugoslavia, textiles from China and the Philippines, electronic products from Taiwan and Malaysia. Modern technology has indeed made the world into Marshall McLuhan's "global village" of interdependent neighbors.

This internationalization of production began with multinational corporations which sought to increase their international sales by establishing factories abroad to avoid tariff barriers and to take advantage of lower labor costs. But the industrializing nations so quickly assimilated technical knowledge and trained an adept labor force that it paid many American corporations to "out-source" production; that is, goods assembled abroad from American components with the manufactured articles shipped back to America for profitable sale.[24]

Most recently international corporate alliances have been formed for production. Thus American and Japanese motor corporations are assembling cars together in the United States, and Japanese electronic corporations are establishing connections with their American counterparts.

However, this globalization is not confined to production; it now extends to industrial R&D, reflecting a global widening of the crossroads of innovation. Recent advances in superconductivity research and materials, for example, have come from industrial and other laboratories throughout the world and from researchers of varying ethnic backgrounds, thereby showing that scientific and technological expertise is no longer confined to Europe and North America. And when researchers at IBM's Zurich Laboratory won this year's Nobel Prize for discovering superconductivity in an unusual class of copper oxide compounds, that showed that the old distinctions between "pure" and "applied" research disappearing even more, as well as the institutional barriers to interdisciplinary research.

Other developments also signalize this internationalization. In 1987 the top three recipients of American patents were Japanese firms. More than 80% of American engineering Ph.D. students are now foreigners, with more than two-thirds of these coming from Asian nations.

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America now fears that it will completely lose its international competitiveness. In order to strengthen America's innovative capabilities it was felt that the science-technology crossroad must be widened by encouraging new collaborative relationships between industry and academia. When this idea was first broached, questions were raised about the freedom of research, open publication of research results, and the like.[25]

But such fears are diminishing. For one thing, the contracts between corporations and universities represented acceptable compromises between the proprietary interests of corporations and freedom of the researchers. University faculty experienced little difficulty in broadening their basic research activities to include certain targeted research projects; in its turn, industry learned to formulate its problems to take advantage of the opportunities afforded by consultation and cooperation with university scientists and engineers. Many university/business research projects are now flourishing, and the universities recognize that they derive much benefit from ties with industry, especially in maintaining expensive lab equipment and keeping the faculty at the cutting edge of research -- and without damaging the university's academic standards.

Obviously, the old distinctions between basic and applied science, between science and technology, are no longer meaningful. And indeed, where does science end and technology begin -- and vice versa -- in such fields as bioengineering, genetic engineering, nuclear power, information and computer sciences, superconducting materials, and the like?

So novel institutional relationships are coming into being to widen the science-technology innovation crossroad. For example, some academic institutions are "incubating" new firms; state and local governments are encouraging collaborative deals between industry and universities, with many states setting up regional development authorities for this purpose; and corporations have joined in setting up "research parks" in the vicinity of academic institutions.

These new institutional arrangements, involving governmental units, manufacturing corporations, and academic scientists and engineers, resulted from a widespread feeling that America's decline in industrial competitiveness is owing partly to the inadequate coupling of scientific research with technological innovation. That diagnosis of America's industrial malady and a similar remedy for its cure were embodied in the 1986 report of a Presidential Committee, headed by David Packard, a founder of one of America's major computing firms, and Alan Bromley, a major figure in academic physics. That committee recommended that universities be involved directly in the application of research and that the government enter a "renewed partnership" with the universities by investing greater amounts in "university-based interdisciplinary, problem-oriented research and technology centers directed to problems of broad national needs and relevant to industrial technology."[26] Hence the National Science Foundation has sought to establish engineering research centers, each involving the cooperation of several different educational institutions and directed toward "creating the technology that the nation needs."[27]

These engineering research centers might be viewed as America's answer to MITI (Ministry of International Trade and Industry), the mechanism whereby the Japanese government provides financial support and coordinates the research efforts of Japanese companies to achieve dominance in selected fields of high technology. But the emphasis in Japan has been on cooperative research among corporations, rather than cooperative research with academia. In the United States, however, new ties among government, business, and academia are broadening the crossroad of technological cooperation. Note the variety of mechanisms and the plurality of institutional forms and endeavors aimed at bringing together science and technology for innovation. Not all of these different ways to encourage entrepreneurial innovations will prove successful, but the point is that the United States seems willing to try new and different approaches and, indeed, the varying nature of scientific and technological activities would seem to require such a variety of approaches.

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Even when science and technology are brought together at the crossroad, there is no guarantee that an innovation will result. As we have seen, innovation is a social process, not simply a scientific or technical one, and hence managerial and entrepreneurial expertise will be required to "direct the traffic" at the innovation crossroad. Thus, Martin Neil Baily and Alok K. Chakrabarti, of the Brookings Institution, blame the American business community for failing to invest in research and processes to increase productivity and efficiency; they call for an increase in research spending and wiser utilization of capital.[28]

Harvey Brooks also recognizes the importance of such extrinsic forces: "The application of the output of research to the economy depends on too many complementary investments, institutions, and private and public policies and on actions having little to do with R&D."[29] But Brooks also regards the crossroads of science and technology of special significance, for he states that major innovative advances are becoming "more and more dependent on bringing together the tools and insights of several specialties."

Indeed, there is abundant historical evidence that bringing together different fields stimulates creative efforts which result in innovation. For example, the revolutionary transformation in textile technology from natural to synthetic fibers serves as a classic example of "innovation by invasion," for it derived from new and extended applications of polymer chemistry rather than from the traditional textile industry.

Bringing together traditionally separate fields within and between science and technology is being encouraged, and it is proving extremely innovative. Thus Ohio's Thomas Edison Program seeks to revive that state's industrial activity by crossing traditional technological boundaries "to enhance existing product line and create whole new classes of products." They call it "Integrated Technology" when they combine acoustics with polymers, agriculture with industrial processing, and glass tempering and electronics design with solar energy.[30]

Scientists and engineers functioning in the new, dynamic R&D environment which brings them together - within and among government, industry and academia - share similar positive views. For one thing, their common reliance on computer usage serves to obliterate previous methodological differences. They also point out that the joint research efforts of individuals from different fields achieve synergistic results as they put together their specialized knowledge. Furthermore, the continual feedback from real problems enables researchers in industrial research laboratories, in military and governmental laboratories, and in universities to make their formulations applicable over a wide area of engineering and applied science. It is clear that they enjoy the excitement and, yes, the fun of being at the crossroads of scientific and technological thought and experimentation.[31] The ecology of innovation in the United States seems to be taking on a new and dynamic character.

Indeed, as I view the changing ecology of innovation, as represented by new and varying combinations of scientific and technological work in military, academic, governmental and industrial laboratories, I begin thinking we might be in for a new wave of innovations. For looking at these new relationships, I see a new acronym emerging: M for military, A for academia, G for government, I for industry, and C for their new cooperative endeavors. Put them together they spell "M-A-G-I- C" and let us hope this new conglomeration of background and approaches will work "Magic" in producing innovations. For it can transform the narrow crossroads of science and technology into scientific-technological superhighways which will speed the progress of all mankind into a better and more peaceful future.

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THEORY AND SYSTEM IN THE HISTORY OF TECHNOLOGY

R.A. Buchanan

The genesis of invention is the central problem of the history of technology. The subject is concerned with many related issues, such as the development of innovation and its transmission, and the impact of this process on society. But the primary question which we face as historians of technology, both conceptually and chronologically is: "What are the causes of invention?" The efforts which we make to answer this question will colour and be coloured by our philosophy of history. So from the outset the historian of technology must grapple with historiography and possess a consistent view of the nature of history.

Such consistency, however, is an elusive and personal quality, so that in the history of technology as elsewhere there are many different approaches to the understanding of history. In particular, the history of technology has attracted the attention of social scientists whose ideas of history have tended to be more rigid and mechanistic than those favoured by traditional historians, and this has encouraged the notion that a firm set of explanatory, and predictive criteria is available in answer to the question: "What are the causes of invention?" Without wishing in any way to undervalue the contributions of social scientists, most of which I welcome and find illuminating, I believe that it is necessary to sound a note of caution against this tendency towards dogmatic certainty in our discipline, and to urge instead the unique and contingent quality of human invention.

It is, of course, possible to set out the pre-requisites of technological innovation: that is, to determine the criteria the presence of which is essential to permit an invention to be taken up and developed successfully. Rostow did this thirty years ago in the seminal paper which launched us all into the aeronautical metaphor of preparing for "take off" and achieving "self-sustaining" speed down the runway.[1] I attempted a similar exercise at the foundation of ICOHTEC twenty years ago, when I tried to define "social needs" and "social resources" which made inventions commercially successful.[2] But such exercises, however useful they may be in helping students to grasp the role of many complex factors in the process of innovation, have nothing to say about the origin of inventions, the question of which is left shrouded in obscurity. In so far as such exercises purport to develop a "theory of invention" therefore, they must be regarded as failures. This does not imply an objection to theory in principle, but it does suggest a limit to the effectiveness of theoretical "explanations" of historical events and indicates a need for the utmost caution in considering any explanations, however theoretically elegant, on the nature of invention.

There have been a variety of theoretical approaches to the history of technology. Fifty years ago Lewis Mumford deployed his ingenious and immensely teachable analysis of successive epochs of technological evolution, and even earlier the Marxist diagnosis of endemic class conflict had stimulated another distinctive formulation of technical history, with authors like Samuel Lilley undertaking ambitious counts of inventions in different historical periods.[3] Neither the liberal progressivistic view exemplified by Mumford nor the Marxist interpretation are highly fashionable at present, although both have their supporters. More recently, however, the theoretical input into the history of technology has been methodological rather than ideological, and it is with the challenge presented by these modes of investigation that I am most concerned here. There are two major forms which this challenge has taken: that stemming from the application of econometrics, and that derived from systems theory. Econometric analysis has a limited but important application to technological history in so far as it has attempted by counter-factual arguments to determine what the effects of inventions have been and thus throw some light on the inventive process itself. This need not detain us for long. The challenge from systems theory, as used by sociologists and students of business management, is more serious because it presents a more fundamental attack on narrative history. It is also enjoying something of a vogue.

The distinguished British economic historian Sir John Clapham contributed a short article to The Economic Journal in 1922 entitled "Of Empty Economic Boxes"[4]. He imagined an economist visiting a hat-factory store, with the hats arranged on the shelves in boxes, and went on to express a gentle protest against the readiness of economists to arrange the facts of the real world in a series of conceptual "boxes" with labels such as "Diminishing Return Industries" and "Increasing Return Industries". Clapham argued that it is virtually impossible to fit industries with certainty into such boxes, and insisted that any attempt to categorize them in this way resulted in a distortion of the historical account. In particular, he pointed out that the place of invention in the conceptual boxes of the economists was obscure and problematic.

The chief object of Clapham's criticism was his friend the eminent economist A.C.Pigou, who made an equally good-natured response in a subsequent issue of the journal.[5] Pigou was able to display the analytical value of the conceptual boxes with cogent logic, although he also recognized their limitations and the two scholars arrived at a measure of agreement. But the crucial point of Clapham's critique, that historical study is different from economic analysis in that it cannot be neatly categorized in the manner of the conceptual logician, remained unanswered, perhaps because it had not been expressed with the clarity it deserved. There was thus a curious sense of disengagement about the exchange, in which both of the participants failed to pursue the real point at issue between them. It seems likely that, in the British tradition of economic history, with the history always dominant over the logical analysis of the economics, the danger envisaged by Clapham was marginalized. The subsequent development of a more rigorously theoretical methodology in American economic history made the danger real, but by that time both Clapham and Pigou had left the scene.

There is no way in which I could be considered competent to embark upon a general critique of econometric history, and even if I had the competence I would not wish to do so. I accept without reservation the need for full and accurate quantification in all those areas of historical research in which it is possible, and I admire much of the work which has been done in this field by Fogel, McCloskey, Floud, and others.[6] The essay by Charles K.Harley, for example, on the shift from sailing ships to steamships, I regard as a wholly admirable study in technological change.[7] But in the attempt to develop a method of counter-factual argument in order to establish significant facts about technological invention and innovation, I consider that econometric historians have pushed their expertise beyond the limits within which it should properly operate. The example with which I am most familiar is that of von Tunzelmann's attempt, in his otherwise excellent study of the development of steam power in Britain at the end of the eighteenth century, to demonstrate by counter-factual analysis that the invention of the steam engine had made only a marginal contribution to economic growth in this period.[8] The inference of this argument is that, if the steam engine had never been invented, all the other factors of production under consideration would have continued to develop in the way they did. Such an assumption depends upon the isolation of carefully selected factors from the historical matrix in which they are embedded,and it is an improper procedure for historical analysis.

This is not to deny the validity of a general measure of counter-factual speculation in any historical examination: the historian is bound to wonder about alternative scenarios if certain unique events had not occurred. But it is quite another matter to develop such speculations with the degree of specificity such as that of the case I have quoted. After all, the invention of the steam engine had absolutely incalculable (and therefore unquantifiable) ramifications throughout British society in the eighteenth century, and the fact that a particular manufacturer did not use steam power does not imply that he was not stimulated by the new technology to improve existing prime movers, or that he did not respond to the competition of steam power in other ways which materially affected his productivity. Such influences, however. are not discernible by the techniques of counter-factual analysis, but their existence is beyond dispute and their significance invalidates the highly specific form of counter-factual argument. To this extent, therefore, the historian of technology is bound to caution his econometric colleagues to restrain their zeal for theoretical constructions.

The development of systems theory in the social sciences and its application in various forms to historical analysis is more disturbing to the historian of technology, if only because of the enthusiasm of some of its exponents. The approach was pioneered over twenty years ago by Neil Smelser who spoilt an analysis of the growth of the Lancashire cotton industry in the Industrial Revolution, which was the result of some useful research into conditions of family life under early industrialization, by forcing it into a procrustean bed of theoretical analysis. Borrowing Clapham's terminology, Smelser set out a framework of "empty theoretical boxes" which sought to express the historical process in a form of flow-chart, and then quite blatantly made his research material fit into the boxes.[9] The exercise aroused rather bemused interest amongst reviewers at the time, but so far as I am aware it was not widely copied. More recently, however, systems theory as a method of trying to relate the parts of an intricate system into a single analysis has become fashionable in business management studies, and there have been attempts to apply it in the history of technology which deserve to be taken very seriously. Even the learned pages of **Technology and Culture** have carried some interesting representations of this new technique, and exponents of "Science Studies" have adopted it as an attractive means of extending their field of interest into technological history.

Perhaps the most substantial and successful expression of this technique has been by Thomas P. Hughes, who has thus given it the authority of a leading historian of technology. In his important study of electrification in western society, **Networks of Power**, published in 1983, Hughes gave detailed narrative treatments of the development of electrical power systems in New York, Berlin and London, which are admirable and probably definitive accounts. But he went to considerable pains in his introduction to outline "the overall model of system evolution that structures this study at the most general level"[10] and set out its phases from, first, "the invention and development of a system" through a process of transfer and "system growth" to periods "characterized by substantial momentum" and "contingencies" which include "the possibility of external forces redirecting high-momentum systems". This is unexceptionable so far as it goes, although it smacks a little of the "empty conceptual boxes" and adds little to the comprehension of what is, essentially, good old fashioned narrative history. The same can be said of some of the language which Hughes coins to describe the stages of his analysis, and particularly the term "reverse salients" It would hardly be worth mentioning this were it not for the fact that this phrase has been widely adopted as if it represented a moment of profound truth whereas, despite Hughes' protestations to the contrary, it is only another way of expressing the old image of "bottlenecks" in the development of inventions, except that with its overt military analogy it implies a measure of agreement about objectives - which cannot always be taken for granted in the history of technology. To this extent the phrase is potentially misleading because it might convey a false impression of unanimity about goals.

In a series of stimulating papers to the Newcomen Society, Michael Duffy has developed an interesting application of systems theory to technological development, or what he has termed "technomorphology" even though this also falls into the trap of excessive systematization. For example, the concept of "design impasse" is defined as the condition occurring: "when a design evolves to a stage from which further development to a more advanced type would meet with increasing difficulty, generating more problems than have been solved".[11] This can be quite a useful idea in describing such technological developments as the evolution of the Stephenson traction system on British railways and the obsolescence of the steam locomotive. But its implied predictive quality is entirely spurious, because it can never provide us with enough evidence to enable us to recognize the moment of "design impasse" when it arrives. Thus many engineers went on trying to improve the steam locomotive even when it was losing ground to diesel and electric traction, because they had no reason to believe that their ability to improve the efficiency of their machine would no longer be successful as it had been in the past, and in slightly different circumstances - such as the failure of oil supplies, or the avoidance of World War II - they might easily have succeeded. By the same token, the makers of reciprocating piston internal combustion engines could well have recognized a "design impasse" when the Wankel rotary piston engine came into service in the 1960s. As it was, however, they confounded the predictive authority of the theorists by managing to improve the competitive quality of their technology with the result that it is the Wankel engine which has been consigned prematurely to the industrial museums.

Both Hughes and Duffy are good historians, so that while I criticize some aspects of their methodology I am broadly in agreement with the findings of their historical research. But apart from the contribution of Hughes himself and that of one or two other historians of technology, I am not inclined to be so charitable about the work which, more than any other, represents the current state of the art in the application of theory and system to the history of technology: **The Social Construction of Technological Systems**, published in 1987 by MIT Press under the editorship of Wiebe Bijker, Thomas Hughes and Trevor Pinch.[12] This is the outcome of a conference in the Netherlands at which a selection of sociologists and historians explored the possibility of applying techniques hitherto developed for use in the field of "Science Studies" to the history and sociology of technology. As far as the history is concerned, the application is not a success. The imposition of a grandiose conceptual vocabulary on the subject matter of history is essentially pointless, leading to obfuscating and unjustified selectivity in the use of historical evidence. Evidence, the reader feels, is welcome when it fills the preconceived conceptual boxes, and is otherwise likely to be discarded. It is not practicable to deal with the whole volume here, but I make my point by referring to contributions by Bijker, Pinch and Law.

In an early chapter in the book. Trevor Pinch and Wiebe Bijker set out to illustrate the "social construction" of technological artifacts by considering the development of the bicycle.[13] They stress the importance of a "multi-directional model" which takes account of the innovations which failed to develop as well as those which succeeded, and of the "relevant social group" in determining the "interpretive flexibility" of the artifact which promotes different "closure mechanisms" and the "stabilization" of the final form of the artifact. In the application of the pneumatic tire to the bicycle, for instance, they are anxious to demonstrate that this was successful for reasons other than those prescribed by the initial manufacturers: they succeeded because they made possible higher speeds rather than because they cushioned vibration. This is an example of what they mean by "interpretive flexibility" and it is a legitimate point for discussion even though it is uncertain how far it can be sustained by the historical evidence. However, the authors are not primarily concerned with historical evidence except in so far as it can be used to support the conceptual models which they construct. Despite some entertaining pictures and some highly vacuous diagrams, they have nothing to add to the history of the bicycle, and their conceptual terminology becomes a matter of obfuscating rather than clarification.

Bijker is on slightly stronger ground in a subsequent chapter in writing about the development of bakelite, because he is more familiar with the scientific history of the process. Here again, however, he is concerned to express the story in terms of "social construction" and, more particularly, in terms of what he calls "technological frame" and "inclusion". By these ideas he appears to mean that "actors" in a given situation agree on the assumptions relevant to that situation: "the concept of technological frame is intended to apply to the interaction of various actors" (p.173). Working together, in other words, generates corporate attitudes: we used to call it team spirit or some such innocuous term, and it clouds the issue to give it the deterministic character of an abstract concept such as that suggested by the words "technological frame". Such obfuscating may appear harmless enough in so far as there is quite a useful account of the emergence of bakelite as an important technological artifact and commercial product hidden somewhere behind it, but Bijker is keen to direct his argument "Toward a Theory of Invention" and that is more problematical. In the event this concluding section does not live up to the aspiration of its title, but merely recapitulates the concepts which have already been plucked out of the metaphorical bran tub as if these constitute a "theory" in themselves. The theory of invention remains as obscure at the end of the investigation as it was at the beginning.

Consider also the chapter by John Law[14]. This takes Portuguese maritime expansion in the fifteenth century as a case study "On the Social Explanation of Technical Change". While not specifically dealing with invention, this study is concerned with "reinterpreting the notion of system, adaptation, and technological testing for a historical case" (p.132) and with demonstrating "the strategies of system-building and, in particular, the heterogeneous and conflicting field of forces within which technological problems are posed and solved". It does so by expressing the factors involved in the Portuguese voyages of discovery in a highly contrived and artificial system whereby galleys become "emergent objects constituted by a heterogeneous engineer" (p.117) and instead of navigators needing a map it is said that it was "necessary to generate a metric from which the observations might be given absolute north-south meaning" (p.125). It is always unfair to criticize by selected quotations, but these expressions give substance to my contention that the whole exercise is profoundly unhistorical. As an historian, I am anxious to welcome social scientists into the history of technology. But in doing so it is not unreasonable to insist that they must recognize the peculiar characteristics of history as a discipline and accept them as the basis for any analysis they choose to make.

In view of the criticisms which I am making of The Social Construction of **Technological Systems** it is necessary to add that I am not condemning the exercise totally. Like the curate's egg, there are some good parts to it, while I have tended to draw attention to those parts which are especially prone to the sort of conceptual hyperbole which I find historically offensive. Many of the assumptions of these social scientists would be shared by most historians of technology: the belief in the importance of a wide explanatory context, for example, which includes all the social groups involved; or the notion that the history of failed inventions can sometimes be as illuminating as that of the successes (there will be an ICOHTEC symposium devoted to this subject at Hamburg next year). The irony of much of this recent work is that the actual historical investigation is often quite good and informative before it is subjected to a conceptual strait jacket which neither produces the historical explanation which it purports to reveal, nor does it possess any predictive power which would make it useful in forecasting future developments. Even worse, it destroys the historical authenticity of the account, for however refreshing in their attempt to relate a wide body of evidence to a central theme, and in raising stimulating questions, such attempts are always too dogmatic to be correct. They can never be wide enough to encompass all the variable factors which affect the issue under discussion. This applies as much to the great world-systems of Marx and Toynbee as to modern "systems theory". They all invite criticism in detail which invalidates their central theoretical assumptions.

My argument up to this point has been negative in so far as I have been critical of several forms of misuse of theoretical constructions in the analysis of the history of technology. To be more positive now I would like to offer an alternative approach and to reflect on the relationship between the history of technology and traditional history. The approach to the history of technology which I find most satisfactory is one which applies to all branches of historical research: it is the "critical narrative" approach which proceeds by pursuing all the available evidence in order to answer certain specific questions, and which provides the answers to these questions by drawing out a coherent narrative from the evidence. This approach does not deny the role of theory in defining the significant questions at the outset of the inquiry: indeed, it takes it for granted that the historian will arrive at his or her subject with certain assumptions and perceptions of what is wanted from the evidence. But when such a theoretical or philosophical stance is allowed to dominate the inquiry, in the sense of pre-judging the conclusions. it becomes a form of "whiggery" or partisan history and as such becomes academically disreputable. In the same way, genuine historical research is not averse to a systems approach, but is rather predisposed towards it by its insistence on the need to consider all the available evidence. Thus the masterpieces of historical analysis including in the history of technology micro-studies like Dickinson on the steam engine as well as macro-studies like Needham on Chinese technology, have all stressed the inter-relatedness of the evidence and have attempted to reconstruct a systematic account of what actually happened.[15]

Having established the terms of the investigation, the historian needs to examine all the relevant evidence which, in the case of the history of technology, can frequently involve physical, oral, and cultural evidence in addition to the documentary material which is the basis of most historical study. From this the historian will assemble his or her reconstruction of the events under examination, proceeding by means of constantly checking every new piece of evidence against the existing body of material for accuracy and consistency until a coherent pattern has emerged adequate to answer the questions with which the investigation began. This process has been called "colligation" by one philosopher of history,[16] but I prefer myself to call it "reticulation" by analogy with the map-making process of checking one set of bearings against another until accuracy is achieved. I have on the wall of my office a print of the map of New Zealand built up by a series of bearings taken by Captain Cook when he made the first journey round the islands in 1770. There are some intriguing errors, such as Cook's presumption that the Banks Peninsula was actually an island, and the opposite mistake in thinking that Stewart Island was joined to the mainland, but it is remarkable that a skilled navigator could create such a high degree of accuracy on a single short mission. It is the task of the historian to achieve similar accuracy as a result of his survey of the available evidence, and his painstaking reconstruction of past events on the basis of this evidence.

The end product of the process of reticulation should be a piece of narrative history providing answers to the critical questions posed at the beginning in a form which is intelligible to the non-professional reader and which is both internally consistent and coherent with the available body of scholarship in adjacent fields of study. It is likely, at this stage, to suggest conclusions which tally with the overall theoretical assumptions and philosophy of the historian who has made the inquiry, because of the framing of the original questions, either explicitly or implicitly, and the selection of what has appeared to the historian to be significant evidence. This element of personal bias is unavoidable and is therefore acceptable. It is quite different from the overt partisanship of Whig history, in which everything is made subservient to the theoretical objectives of the historian.

It is my contention that all good history of technology, like all good history, is cast in the molds of critical narrative such as I have described. It follows from this that, while continuing to perfect our discipline, we should be very cautious about theories and systems which are imposed upon our field of study. This is not to say that they are necessarily wrong, although I consider that their application is often misguided or misplaced. But however helpful they may be to individual historians in framing the initial questions of the inquiry and arranging the structure of the subsequent argument, they need to be treated with great care in the development of the substance of the narrative. In particular, they can have no predictive validity of the type desired by social scientists, and they can make no useful contribution to the question at the heart of the history of technology regarding the nature of invention.

Maybe there can be no precise answer to this crucial question. In the last resort, a critical narrative of the history of technology must recognize that invention possesses the qualities of an act of creation: it may be prepared for, in the sense that the potential inventor can be encouraged to pursue a good idea and resources can be put at his disposal, but the actual invention is a unique and individual event which cannot be predicted. Virtually every well-authenticated account of an invention has this inspirational quality: one need only think of James Watt musing in the University of Glasgow park one Sabbath afternoon about the possibility of separating the condenser from the steam cylinder; or the Wright brothers reflecting on the way in which sea gulls achieved three-dimensional control of flight by flexing their wings; or Werner von Braun devising rockets in order to fulfill a boyhood dream of space flight. There is always this indeterminate but vital component of inspiration to transform the massive perspiration induced by the hard work of preparation into a genuine act of invention.

Like a poet or an artist, therefore, the inventor participates in an act of creation, and no amount of theoretical construction can encompass the terms on which such creativity can be achieved. There is, however, plenty that can be done to prepare the ground for the inventor and to ease his way. Appropriate rewards may be forthcoming such as freedom to use his invention to make money or to achieve a higher social status, and legal aids such as an adequate patent law can be provided to ensure him some protection for his ideas. A society can make other inducements available to secure the maximum productivity and loyalty of the inventor, but the promotion of these devices is a matter of social policy rather than systems theory. All of them have become regular aspects of policy in the advanced industrial nations, and have been emulated to some degree by the governments of developing countries. In the context of "The Ecology of Innovation" we will have cause to consider several of them because planning for innovation follows easily and naturally from planning for invention. It is important to remember, however, that they are different: planning for invention is an act of faith which may or may not produce results, whereas planning for innovation and development assumes the existence of inventions and is a more calculable process. But as far as the historical analysis of innovation is concerned, it remains constrained by the same limitations as those which apply to the analysis of invention: that is to say, the critical narrative approach is the only appropriate method.

Increasing commitment to Research and Development in recent decades, and the vogue for Technological Assessment, have demonstrated the enterprise with which governments and corporations have applied themselves to technological development, and there can be no doubt that they have had some striking successes in promoting innovation. The case of the emergence of the transistor in the Bell Research Laboratories is an outstanding example, but for the most part such enterprise has achieved its major successes in the adoption of commercially viable innovations rather than in producing new inventions. The demonstration by Jewkes and his colleagues that the majority of inventions were, in the mid-twentieth century. the result of individual initiative and inspiration, still stands.[17]

The merits of the critical narrative approach to the history of technology, in contrast with the methods which try to impose theoretical constructions on the evidence, can best be demonstrated by a case study. For this purpose I choose to present an account of the hot-air engine. This has the advantage of being one of those "failed inventions" in which the standard general works of technological history have taken little interest. It is also one which I have previously brought to the attention of an ICOHTEC Symposium.[18] The evidence for the invention and early years of the hot-air engine is fragmentary, but enough is recoverable to make possible a reasonably coherent account, and unlike so many failed inventions which have disappeared completely into limbo and created the "asymmetry" in the history of technology about which some of the theorists have rightly complained, there has been a recent revival of interest in it as a possible alternative to other prime-movers.

The hot-air engine emerged from the intense experimentation which accompanied the successful adoption of the steam engine by manufacturing industry and transport systems, and in particular the introduction of high pressure steam in the early nineteenth century. Higher pressures than those near atmospheric pressure at which Watt engines normally operated were causing serious problems with boiler explosions, and stimulated inventors to explore alternatives to steam as the "working fluid" of which hot air was an easily available and relatively safe example. One American text book described it: "As a medium for transforming heat into work, air can be used with safety at much higher temperatures than steam, and therefore a larger proportion of the heat given to it can be transformed into work"[19]. Other working fluids such as easily liquified gases were also investigated, and much inventive effort went into machines such as Marc Brunel's "gas engine"[20]. Of all the early nineteenth century attempts to find an alternative to steam, however, the only one to enjoy even modest success before the introduction of internal-combustion was the hot-air engine. Thus we pose the questions: "How was it invented?" and "Why was it not more successful?"

The first serious idea for a hot-air engine was an "open cycle" machine invented by the aeronautical pioneer Sir George Cayley in 1807. In this engine, hot furnace gases operated intermittently on the working piston, and were then exhausted to the atmosphere. Despite subsequent development, however, this arrangement was never completely satisfactory. Much more significant was the invention in 1816 of the "closed cycle" hot-air engine by the Reverend Robert Stirling (1790-1878). He was then a newly-ordained Presbyterian Minister in Kilmarnock, and there is no good explanation of how the idea came to him, other than that he was a well-educated "gentleman-scientist" like many of his contemporaries. He moved to Galston in 1824 and spent the remainder of his long ministry there. His invention was granted Patent No.4081 in 1816, and in conjunction with his brother James Stirling, a full-time engineer, he developed his idea and obtained later patents in 1827 (No.5456) and 1840 (No.8652).[21]

The essential principle of the Stirling cycle is that a working fluid (normally air) in a cylinder is alternately heated and cooled to produce reciprocating motion in a piston. This is achieved by using a displacer-piston or plunger to move the fluid continuously backwards and forwards between the two ends of the cylinder, which are maintained at high and low temperatures respectively. In addition, Stirling's outstanding innovation, reached more or less intuitively without the benefit of thermodynamic theory which was then scarcely in its infancy, was the "regenerator" or "economizer" a network of wire or metallic plates through which the working fluid passed on each movement between the hot and cold ends of the cylinder, alternately transferring heat to or picking up heat from the regenerator as it did so. The regenerator was not essential to the operation of the hot-air engine, and thousands were made in the second half of the nineteenth century without using the device. But it was the feature which held the promise of development into a near-perfect heat engine, and it was capable of adoption for use in the glass and iron industries, as the work of the Siemens brothers, who almost certainly learnt of it from the Stirling engine, was to demonstrate with their open-hearth furnace. One late-nineteenth century authority on heat engines, the British academic engineer Professor Fleming Jenkin, acknowledged in 1884 that: "the regenerator is really one of the greatest triumphs of engineering invention".[22]

Stirling's first engine was a vertical cylinder heated at the upper end by the flue from a furnace, and containing both the piston and the displacer, linked through connecting rods and the rocking beam of an inverted beam engine to perform the correct sequence of movements. It generated about 2Hp and was installed as a pump in an Ayrshire quarry in 1818. The Stirling brothers later introduced double-acting engines and engines with an air-pump to increase the pressure of the working fluid, thus making it possible to reduce the size of the engine in relation to the power generated. Their most successful engine was a 45Hp machine converted from a steam engine and installed to power a foundry in Dundee in 1843, but after burning out the bottom of the hot end of the cylinder three times the owners re-converted it to steam power in 1847. The biggest problem of the early hot-air engines was this vulnerability to burning out, and it could not be entirely remedied until the development of modern heat-resistant alloys, although Stirling himself hoped that the use of Bessemer steel would overcome the problem.[23]

Even more serious, however, for the competition of hot-air engines with high-pressure steam engines, was the comparatively low power to weight ratio of the hot-air engine. This factor inhibited its use at high powers because of the great bulk and weight of the engine necessary at that range. The gifted Swedish inventor John Ericsson (1803-1889), who designed the locomotive "Novelty" for the Rainhill Trials in 1829 and went on to introduce many important innovations in ship design including the screw propeller, developed a form of open cycle hot-air or "caloric" engine using external combustion and a sequence of valves to direct the working fluid through the cylinder, as in his Patent No. 5398 of 1826. He encountered the problem of bulk in an acute form when he attempted to adapt his engine to provide the power unit for a ship, and he had to abandon the experiment as a failure when the **Ericsson** with its four massive 168ins diameter cylinders sank in 1854. Nevertheless, he achieved considerable success for an improved form of his engine for smaller applications, of which he had sold 3,000 by 1860.[24]

Ericsson was not alone in marketing a successful hot-air or caloric engine. A wide range of manufacturers in Europe and America managed to sell other models, virtually all of them for applications requiring a small but steady use of power. Water pumping was one of their most common uses, but they were particularly important in providing power for fans in room ventilation. The "Kyko" portable fan, for instance, was very popular in the tropics: it was four feet tall, and ran for twelve hours on one pint of paraffin.[25] The use of hot- air engines for such low-power functions continued until the 1920s, by which time they were being used to drive small electricity dynamos in districts remote from power supplies. But in this decade they were almost universally displaced by compact petrol engines and, as the electricity supply grids spread, by electric motors. Theorists might have leapt at this opportunity to pronounce that the hot-air engine had arrived at a "design impasse". But the artifact could not be relegated to the scrap-heap so readily. The Dutch firm of Philips developed a compact new hot-air engine working on the Stirling cycle, intended in the first instance as a power source for radios in areas of the world without regular power supplies. Unfortunately for this development, when it became available after World War II the need which it has been designed to meet was being met by the invention of the transistor and improvements in the manufacture of dry batteries.[26]

Undismayed by this disappointment, Philips turned their attention to the possibility of scaling up their design to develop hot-air engines of higher powers than anything achieved previously by such machines, and they succeeded once more by building both single and multiple cylinder engines which operated effectively up to hundreds of horse-powers. Other manufacturers have become interested and taken out licences from Philips to investigate the scope of this new range of hot-air engines for themselves, and work has been successful in the last two decades in developing Stirling engines for vehicular applications, for space and underwater uses, for vessel propulsion, and for stationary power. There has also been great interest in the application of Stirling cycle machines to refrigeration and air liquification, and as miniature engines for artificial hearts.[27] Nevertheless, it has to be admitted that there has still been no break-through to a mass market for any of the products of the hot-air engine, so that it remains as something of a problem for any theoretical analysis of the history of technology.

Doubtless this curious story of consistent development despite the lack of commensurate rewards could be fitted within one of the theoretical structures which we have examined. It could, for instance,be described within a terminology of "technological frames" "inverse salients" and "social constructions" but such an exercise would add nothing to our knowledge of the process or produce a more satisfactory narrative. The available evidence of the history of the hot-air engine suggests that here was an excellent idea which was never strong enough to challenge the established resources of steam technology or to mount an equally versatile alternative to the modes of power offered by internal-combustion or electricity. Whatever happens in the future to enhance the attractiveness of the hot-air engine, such as changes in the cost-effectiveness of oil-burning engines, or in the communal attitudes towards the ecological damage done by the waste combustion materials from such engines, the record of the hot-air engine to date remains one of disappointing under-performance and the fact that it is still able to mount a challenge to its rivals is a tribute to the conviction and persistence of a few individuals rather than to any theoretical considerations.

In the end, therefore, system and theory must be subordinated to traditional modes of historical investigation. No historian of technology would deny that individual inventors are part of a social matrix and, as such, subject to complex social influences and interactions, nor that these processes are capable of a degree of organized examination. But in the last resort it is more important to stress the individual, unique, and contingent quality of personal creativity than the element of social conditioning because it is this which is most likely to be sacrificed when considerations of system and theory are allowed to predominate. The conclusion of this paper must be that the historian of technology needs more of the subtlety and sensitivity of the historian of art than the dogmatic application of systems theory in his or her critical analysis of the sources of invention and the study of the innovation and development which stems from such invention. It is essential for the future health of the discipline that the force of this conclusion should be recognized.

Notes

1 W.W. Rostow: The Stages of Economic Growth - Cambridge 1960.

2 R.A. Buchanan: "Social Prerequisites of Technological Innovation" in M. Daumas (ed.): L'Acquisition des Techniques par les pays non-initiateurs, ICOHTEC Symposium, Pont-a-Mousson, Paris 1973.

3 Lewis Mumford: Techniques and Civilization 1st ed. New York 1934; S. Lilley: Men, Machines and History, Lawrence & Wishart, 1965 (first published 1946, with tables of inventions which were omitted from the second edition).

4 Sir John Clapham: "Of Empty Economic Boxes" The Economic Journal vol. 32, 1922, pp. 305-314.

5 A.C. Pigou: "Empty Economic Boxes: A Reply" The Economic Journal, vol. 32, 1922, pp. 458-465, to which the Editor allowed Clapham to make a short riposte.

6 The views of this group have been well expressed in David N. McCloskey (ed): Essays on a Mature Economy: Britain after 1840, London, 1971. The classic statement of Historical Econometrics is Robert W. Fogel: Railroads and American Economic Growth, Baltimore, 1964.

7 Charles K. Harley: "The shift from sailing ships to steamships, 1850-1890" in McCloskey (ed) op. cit. pp. 215-234.

8 G.N. von Tunzelmann: Steam Power and British Industrialization to 1860. Oxford, 1970.

9 Neil J. Smelser: Social Change in the Industrial Revolution; An Application of Theory to the Lancashire Cotton Industry 1770-1840, London, 1959; see p. 50 where he writes of the need to "implant the model".

10 Thomas P. Hughes: Networks of Power: Electrification in Western Society, 1850-1930, Baltimore and London, 1983, see p. 14 especially.

11 Michael Duffy: "Technomorphology and the Stephenson Traction System" in Transactions of the Newcomen Society, vol. 54, 1982 , p. 57.

12 Wiebe Bijker, Thomas P. Hughes and Trevor Pinch (eds): The Social Construction of Technological Systems: New Directions in Sociology and History of Technology, MIT Press, Cambridge Mass., and London, 1987.

13 The chapters involved are those by Trevor J. Pinch and Wiebe E. Bijker: "The Social Construction of Facts and Artifacts: Or How the Sociology of Science and the Sociology of Technology Might Benefit Each Other" (pp.17-50) and Wiebe E. Bijker: "The Social Construction of Bakelite: Toward a Theory of Invention" (pp. 159-187).

14 The chapter by John Law: "Technology and Heterogeneous Engineering: The Case of Portuguese Expansion" (pp.111-134). This is virtually identical to the article by the same author: "On the Social Explanation of Technical Change: The Case of the Portuguese Maritime Expansion" in Technology and Culture, vol. 28, no.2, 1987, pp. 227-252.

15 Joseph Needham: Science and Civilization in China, Cambridge, 1954. H.W. Dickinson: A Short History of the Steam Engine, Cambridge, 1938.

16 W.H. Walsh: An Introduction to Philosophy of History, London, 3rd ed. 1967.

17 J. Jewkes, D. Sawers, and R. Stillerman: The Sources of Invention, London, 1958.

18 R.A. Buchanan, "Promise and Disappointment: The History of the Hot-Air Engine" in Energy in History, The Proceedings of ICOHTEC Symposium at Lerbach (near Koln), Dusseldorf, 1984.

19 Frank D. Graham, Audels Engineers and Mechanics Guide 4, New York 1982 , Chap. 53, "Hot Air Engines" p. 1,733.

20 R.A.Buchanan: "Science and Engineering: a case study in British experience in the mid-nineteenth century" in Notes and Records of the Royal Society of London, vol. 32, no. 2, March 1978.

21 G. Walker: Stirling Engines, Oxford 1980; also D.W. Loveridge: "Robert Stirling - Preacher and Inventor" in Transactions of the Newcomen Society, vol. 50, 1978-79, pp. 1-10. The problem of the respective contributions of Robert Stirling and his brother James to the development of the hot-air engine is unresolved, but it was James who acted as the main public exponent of the engine: see his paper to the Institution of Civil Engineers, Proceedings, 1845.

22 T. Finkelstein: "Air Engines" in Engineer, vol. 207, 1959, No. 1 pp. 522-7, No. 3 pp. 568-71, and No. 4 pp. 720-3: Fleming Jenkin is quoted on p. 493.

23 Walker op. cit. p. 2 quotes Stirling: "These imperfections have been in great measure removed by time and especially by the genius of the distinguished Bessemer...".

24 Finkelstein, op. cit. p. 496. See also P.W. Bishop: "John Ericsson (1803-89) in England" in Transactions of the Newcomen Society, vol. 48, 1976-77, pp. 41-52, for a general account of Ericsson's work.

25 Finkelstein, op. cit. p. 527.

26 Walker, op. cit. p. 5.

27 Walker, op. cit. pp. 5-10 gives an account of these tantalizing but promising developments.

POLICY-MAKING ON INNOVATION: FROM REFLECTION TO ACTION

J-J. Salomon

In the current enthusiasm for innovation, it is easy to forget that concern for innovation **per se** took some time to emerge. Although certain limited measures were taken starting in 1971, it was only from 1973 onwards, in response to the oil crisis that people began to talk about innovation policies rather than science and technology policies, in order to stress the overwhelming priority accorded henceforth to the stakes involved in industry and foreign trade. The problem of maintaining oil supplies had shown what new or improved technologies could contribute to production, distribution and energy savings. With structural problems to contend with as well, the persistence of the crisis soon highlighted the potential benefits of a policy on technical innovation in helping to adapt the economy to the new situation, to overhaul the means of production, improve the balance of payments, and reduce unemployment, which had become chronic. It was at this period that the Japanese MITI replaced the American Office of Science and Technology Policy as the model followed by most OECD countries.

Yet if this concern for innovation appears now as topical, it is by no means a novelty. Already in the second half of the 19th century, countries were worried about "falling behind" technologically and used the argument to back up claims for support from governments for scientific research or technical innovation: Babbage's England measured itself against Liebig's Germany, Pasteur in France against the Germany of Virshow (or Meiji Japan, which compared itself with Europe and the United States combined). The use of this notion as an argument dates back to the industrial Revolution and the competition between empires that it engendered. Today's preoccupation with an "economic war" is merely a new version of the same argument - competitiveness.

What is new and revealing is not so much the notion itself as the way that its meaning has changed recently: in mastering the process, the sources of innovation seem less critical than the environment in which it occurs. Already from the 1960s onwards, the importance of the factors that were not properly scientific or technical began to be recognized, and at the same time there was growing awareness of the need to include policy measures that seemed at first sight to have little direct link with technical innovation itself. It is not enough, in fact, to have excellent universities and research teams, to turn out PhDs, to devote vast sums to R&D, nor even to win lots of Nobel prizes to be in the front rank of the "innovative nations" to win the productivity battle, to conquer and keep new markets: it is true that the development of innovation potential passes by way of the efficient working of the research system, but that is simply one step along the way, one condition among many others[1].

Today, it is a common-place to say that global competition, the competitiveness of firms or productivity in general is increasingly a matter of technological innovation, but it is more difficult to specify the conditions that ensure successful innovation. The literature on the subject is abundant, ranging from economics to sociology, organization theory, management science and systems analysis. But innovation is something that, by definition, does not yet exist: "If I knew what tomorrow's great dramatic work would be, I would write it" Bergson said[2]. The same goes for business leaders or political leaders: if they knew what the essential ingredients were for an innovation to be successful and had them in hand, they would have no problems about carrying the day with it.

In the end, as Schumpeter has argued, if technical progress were ever to become "mechanical" in the sense that it was no longer the result of a haphazard process, the entrepreneur would then cease to have a social function --and capitalism would have no future. This is a possibility that even Schumpeter in the darkest passages of his prophecies about the fate of capitalism destined to strangle on its own success considered "extremely distant". The entrepreneurs across the whole range of private sector activities "would find themselves in a similar position to generals in a society where peace had been guaranteed forever". We are therefore reassured: innovation "reduced to a routine" is not going to happen tomorrow[3].

Technology versus economics

This vast literature has certainly helped us to arrive at a better understanding of the sources, the determinants and the repercussions of technical change. But it has not provided decision-makers with any ready-made solutions: innovation is never assured ahead of time. This is why, rather than start by defining what it is, I propose to argue first what it is **not**. On the subject, since I am talking as if I were an "expert", let me stress at the outset that one should be very suspicious of "experts" in this field as in many others. The only good way of foreseeing innovation is in fact to do it oneself, like Bergson. Nathan Rosenberg tells a story that shows clearly the limits of experts in forecasting. In 1899,the commissioner of the Patent and Trademark Office in Washington advised President McKinley to close down the Office. Why this astonishing, suicidal proposal by a senior official? It is because, he said, "everything that could be invented has been invented" 'I am happy to report', Rosenberg adds, '86 years and approximately 3.8 million patents later, that the President did not heed this advice'[4].

There are very good reasons for being suspicious of experts. First of all, their specialist knowledge leads them to see only a tiny range of factors; but also the very nature of innovation means that there is a break with the past that the specialists are often the last people to notice in time. Those who are accustomed to a given technical system are not prepared to take part in the launch of a completely new one. Stage-coach builders played no part in the development of the railways; those who built the first steam engines were not at all involved in the success of the steam locomotive, which in turn had nothing to do with the creation of the diesel locomotive, and even less (obviously) with the motor car. Even those who worked on the development of piston engines for aircraft were not the same people who developed the jet engine. Insofar as innovation constitutes a break with the past, it comes up against prejudices and provokes resistance. The first thing that it has to overcome is precisely the obstacle of familiar habits, established views and entrenched interests.

There are many examples of experts who did not "believe" in the future of new technical developments, and among them the greatest scientists. Einstein, for example, for all that he was the person who drew the attention of President Roosevelt to the importance of atomic research in the famous letter dictated by Leo Szilard, did not believe that there would be practical applications of nuclear energy for many decades. Or Vannevar Bush, Roosevelt's scientific advisor, who opposed any investment in space research just after the Second World War, because he did not believe that strategic missiles were a real possibility. Or the experts from Western Union who refused to spend $100,000 to buy Graham Bell's patent for the telephone. Or again the people from Eastman Kodak who turned down Carlson's patent for reprography, which was the starting-point of the success of Xerox.

The uncertainty of expert advice arises out of the characteristics of innovation: what an invention is able to do, is not necessarily what it will be used to do. An inventor may think that he has developed a new product or a process for a particular purpose, whereas the applications may turn out to be quite different in practice. The most revealing example of this is Edison and his gramophone. Edison thought that the gramophone would be a tool for businessmen like the "dictaphone" today, and he put his invention back in the cupboard for ten years because he could not interest those for whom it was intended. It was only when an opera singer could record his voice that the gramophone became popular at fairs, before becoming the basis of the modern record industry. It should also be remembered that the gramophone was the only one of Edison's inventions, during the whole of his amazing career as an inventor with more than a thousand patents to his credit, which was not developed in response to an explicit request. Technology push was not enough -- there was no market pull. Another revealing example is the Minitel system in France, which the inventors in the Telecommunications ministry thought would be used mainly for professional message services, and they never foresaw - how could they have foreseen? - its success as an interactive personal message service, in particular for lonely hearts or even pornographic messages.

These few examples remind us what an uncertain matter innovation is, and what a complex process is involved. It is not simply a question of achieving a better or more efficient technical result, the innovation also has to satisfy some social or economic need, or be able to create a new need, and it also has to find its place in the wider context which is often very different from the technical environment in which it was conceived. Marconi, for example, invented the radio, but he did not think of it as a "system" that would be used universally, in the home or the car, to broadcast music or other new forms of cultural expression. The opposite was the case with Edison and the electric light bulb, which was merely an element - the least important, in point of fact - in a completely new technical line, the electricity supply system, whose use spread not just to the factory but to the office and the home. Besides, innovation leads to other innovations at the centre of new technical systems. The first steam engines were simply water pumps for use in mines; they led to a transformation of transport thanks to locomotives, then to turbines for electricity generating stations. The Volta battery led to the development of the telegraph, and then to the dynamo, hence small electric motors.

Furthermore and above all, there is no single family of innovations - there are many, each with a family tree as varied as its progeny, with tendencies to break away that are more or less visible at the time. The fame of "radical" innovations that completely transform production too often obscures the influence of gradual improvements in products and processes, which do not necessarily have a less critical function in economic activity and trade. Technical systems never stop evolving, they are always open to improvement, not only so as to perform better in a technical sense, but also to be better adapted to meet the need for their applications to be profitable over the long term, whether they are under or over-used. The internal logic of technical change is related to a complex interplay of economic relationships, within and between sectors, with periods when the new technologies carry all before them, followed by more prosaic periods that are no less critical, when processes of production and marketing are steadily improved.

Finally, purely technical innovation always occurs in an institutional and social context, and its success often depends on a new approach to the organisation of work (or leisure), of selling or of demand, as much as on its inherent qualities. The steam engine developed by Watt only began to find customers when Boulton, who became Watt's partner, had the bright idea of putting the machine out for hire rather than selling it - an example that IBM took to heart, since the success of their computers did not really start until the firm began to hire them out rather than sell them, as its competitors did.

For all these reasons, it is wrong to think that the spread of technical change has become much faster nowadays. Because of the daily impact of the media and the greater attention that we now pay to the idea of innovation, we have the impression that the pace of change has increased. But the almost immediate awareness of a much larger body of discoveries and inventions - and even the more rapid obsolescence of new products - does not mean that their time will come any more quickly than in the past. There was an interval of seventy years between the first major work on the steam engine and the industrial application of Watt's engine, which is just about the same time-lag as the one between the discovery of radioactivity and the success of the first nuclear power stations. In a fascinating article in Daedalus on the influence of technology on the arts, Brian Winston points out that the first patent for television goes back 102 years. In 1936, the Federal Communications Commission concluded that television was not yet ready, though that very year there were 20,000 sets of the American RCA type and 180,000 people saw the televised pictures of the Olympic Games in Berlin.

Brian Winston adds: "The constraints on the introduction of television into the United States were social, not technological. Such social constraints are associated with the 'law' of suppression of radical potential. This 'law' implies that technologies are introduced into society only when they do not disturb pre-existing arrangements of all kinds"[5]. One just has to remember the standards battles that are currently being waged between 8 mm video, VHS or BETA, or the time that it took for the compact disc to displace the long-playing record. The problem is not to know whether a given system is better than another, but to identify the right moment when that system satisfies public demand, or market potential, better than another and therefore commercial interest. The truth is that technology matters to the engineer who creates it, or the historian who tells the tale, but it does not of itself matter to the entrepreneur who develops it. As Carlo De Benedetti emphasizes: "In business terms, technology does not exist. What counts is how you make the connection between the idea and the market"[6].

As a last preliminary remark, I realize that I have not yet mentioned research, but that is because innovation is not research. Research and development may lead to innovation, and it is certainly the case that R&D is increasingly important because of the influence of both private and public research labs, but its role is not necessarily pre-eminent, and may often be quite secondary in the success of innovation. It is true that the new technologies - information technologies, biotechnologies, new materials - are very heavily dependent on laboratories, and are so capital-intensive that mastery of their production requires access to considerable means, in the shape of equipment and specialized skills. High-tech by definition involves large investments in R&D, where, therefore, only the big multinational companies and the major strategic programmes, with public support, seem to be in a position to "make" it in world markets.

But, even here, innovation depends on factors other than technology, in the strict sense, or - **a fortiori** - science; design and marketing, in particular, are also very important. And the example of the thriving small firms in Silicon Valley (Apple especially) shows that innovation is not confined to the largest companies. In fact, big firms are less well placed than small one when it comes to innovation, because they do not like change. IBM ignored the home computer market for years, and in the end did not really win the battle with Apple, in spite of the success of the PC. Tocqueville put it beautifully when he said that the threat of innovation is the **mot sacramental** - the word that cannot be uttered - of every administration, and the big companies are no different from any other bureaucracy, with the different parts in their own little compartments, separation between research, production and management - all of which effectively deters risk-taking.

Let us stop for a moment with this brief outline of innovation, since we can already get some general ideas from this basic sketch. First of all, every history of the Industrial Revolution shows that the international spread of innovation is a means of closing the gap, since no country manages to hold onto its lead indefinitely, and consequently there is no reason why a developing country should not be able to innovate in certain fields. The example of the "newly industrialized countries" - Brazil and the four "little dragons" of south-east Asia (South Korea, Taiwan, Hong Kong and Singapore) - show that there are shortcuts in the process of industrialization. The strategy of technological "leapfrogging" of course presumes that certain conditions are fulfilled, especially with regard to cultural background, the educational system and technical training, and also a readiness on the part of the public authorities to support the newly created industries. But it is the proof that the cards can always be shuffled and dealt again.

Furthermore, the example of Japan also shows that basic research does not play a critical role in the development of the technological capacity to innovate. Having copied Western industries for years, and having become increasingly able to compete with them, Japan did not build her technological success on an equivalent effort in basic research. It is only now that the Japanese have realized the need to encourage such research, and in connection with precisely those areas where they have been ahead industrially: the growing complexity of technology, which they had always coped with before through imitation rather than creation, now requires a far higher level of theoretical research back-up. But Japan's economic prosperity has allowed this change to take place - it was not required from the outset.

Basic research helps to develop knowledge and skills, which in turn makes it possible to understand how the technical system operates and therefore take advantage of it; but it is not essential to do basic research to make use of the technologies. It is essential if you want to teach science in the making to those who are going to take it a few steps further, but it is not necessary if you are merely teaching established knowledge to those who are going to apply its principles. In other words, a solid infrastructure of basic research is a plus if you want to be or to stay in the forefront of scientific progress, but it is not a guarantee of a place in the race for industrial innovation. The increasingly close links that the changes in the modern technical system have forged between the "new paradigms" of science and technological developments do not mean that it is enough to contribute to the advance of those paradigms to be sure of mastery of any given part of the technical system. In short, innovation is not played on the researcher' territory, but on the entrepreneur's.

But we should not conclude either that science, or more generally R&D, does not influence the sources of innovation today. It is clear that we have reached a stage of the industrial Revolution where organized research is an important factor in this process, whether at the level of firms or countries. But let's not lose sight of the wood for the trees: concentrated effort on R&D in certain areas is not an assured means of being successful in innovation in the future. In other words, R&D is a necessary but not a sufficient condition - far from it. The researcher can be an entrepreneur, but the entrepreneur does not have to be a researcher, and the innovator does not need to be either. In the 19th century, new institutions grew up to produce and sell new goods and services on a large scale: factories and automation. In the 20th century, new institutions have been designed specially to create new technologies on a large scale: laboratories. infrastructure is therefore indeed a prerequisite, as is an ever higher level of education. But that does not mean that the fate of innovation lies entirely within the laboratory, with a mechanical or linear chain of cause and effect. The logic of discovery and invention still has to face the logic of the market.

Out of 1800 successful innovations in the United States, only a quarter were linked to clear notions of the technical potential, whereas three quarters were developed with a clear idea of their market potential[7]. Moreover, the great majority of the inventions that received patents from the Patent Office were never marketed. Technology push and demand pull refer to two complementary phenomena, but the latter is more often a factor for success than the former. Which is to say that it is a process in which the elements tied to scientific knowledge are not the most decisive in achieving ultimate success: intellectual investment is essential, for the firm as for the nation, to ensure mastery of production and use of the new technologies, but it does not stop with scientific training alone.

We can now come back to the problem of definitions: innovation is what disrupts, what rocks the boat, what conflicts with habit. Technology is the application of knowledge and rational processes - scientific knowledge and know-how - to the satisfaction of economic and social needs, real or imagined, through the creation and distribution, the industrial organization and management of goods and services. Technology is a social process which is achieved via technical and institutional innovations[8].

The boom in policies for innovation

It was in fact at the very moment when Europe was becoming worried about lagging behind the United States - some of the member countries of the EEC were even suggesting a new version of the Marshall Plan, in order to fill the "technology gap" - that first-rate American experts began to be alarmed about the talling rate of productivity growth in the United States; some of them even blamed this reduction on a "decline" in the pace of innovation. Clearly, there is no direct relationship between the size of investments in R&D and economic performance: the United States, the champions in all aspects of science and technology, had growth rates below those of Europe and Japan.

Since the end of the Second World War, the United Kingdom has spent a higher proportion of its GNP on R&D than any other European country; but it is far from being the leader in technical innovation, and it has had one of the slowest economic growth rates. Italy, by contrast, which might have appeared to be far behind everybody else in the EEC as regards public expenditure on R&D during the same period, was one of the most innovative countries, with periods of outstanding economic growth and foreign trade. The examples of Japan, Taiwan, South Korea, Hong Kong or Singapore show that it is possible to compete with the United States in the most advanced production sectors, while still depending on America for a good part of the discoveries, inventions and know-how used in these sectors.

Obviously there is no exact overlap between science policy and technology policy. The first sets out to stimulate scientific discovery, applies especially to R&D, is essentially aimed at promoting basic and applied research, and is therefore concerned above all with higher education, training and employment of research workers, equipping laboratories and with the problems raised by directinq research efforts in one direction rather than another. The second attempts to encourage technical innovation, applies particularly to industrial activities, is essentially aimed at the pace and direction of technical change, and hence is concerned above all with business firms, the conditions necessary to encourage risk-taking by entrepreneurs, and the problems raised by competition and industrial restructuring.

In the best of all possible worlds, it would be desirable if the two policies worked together in harmony, moving at the same pace towards the same national strategic goals. Unfortunately, in every country, this ideal world is difficult to reconcile with reality, especially with the bureaucracy: the range of actors and institutions means that everywhere there are conflicts of interest and outlook, further accentuated by the fact that the details of these policies are usually worked out in rival ministries. Rothwell and Zegfeld have rightly pointed out that the concept of innovation policy presupposes that a science and technology policy will be integrated with an industrial policy[9]. In fact, in no country, not even Japan, is there even a hint of this happening. It would be already quite remarkable if the two policies were even co-ordinated.

In addition to this problem, which arises out of the nature of bureaucracies, there is another related this time to the nature of technological innovation. As stressed above, the studies of technical innovation over the last 25 years, have taught us a great deal about the descent lines of innovations, but that has done nothing to reduce the complexities of the interrelationships involved: the picture has become fuller but no clearer. The moral to emerge from these studies is that **we must avoid any pigheadedness**. No discipline or branch of a discipline has any monopoly on knowledge in this area: the most energetic research on innovation has been done by economists, but they are still far from providing a full analysis; historians of science and technology, sociologists and psychologists all bring some equally valuable information and enlightenment to the subject. Technical innovation is like the cube according to Alain: you can never see all the sides at once -that can only be done conceptually.

"It would be very tempting" as Melvin Kranzberg has put it so well, "to compare the literature on innovation with Aesop's Fable about the blind man looking at an elephant: every expert concentrates his research on one aspect of an elephant-sized problem and thinks that his limited knowledge is valid for the whole animal. But the analogy is something of an exaggeration. Those who have written about innovation cannot be compared to Aesop's blind man; they have almost all perceived the outline of the giant that they are dealing with. They choose to concentrate on certain aspects of the problem because of their scholarly inclinations, which are shaped by the particular concerns of their various disciplines"[10]. Furthermore, most of the studies of technical innovation have examined the successful examples, not the ones that have not worked - yet it has been shown that there is much to be learned, both for methodology and policy-making, from an analysis of abortive or failed innovations[11].

For all these reasons, a deliberate, coherent and systematic policy on technical innovation would appear to be extremely difficult to put into practice. "Technical progress" said an OECD report, "results from a combination of micro- and macro- economic factors which is by no means certain to arise and can be stimulated either by recession or growth. **Maximizing the chances that such a combination will arise is precisely the objective of government innovation policies**."[12] This excellent definition suggests both that, like the matter they are concerned with, these policies involve a certain element of luck and yet that the luck element cannot be stretched too far. There is no guaranteed winning formula.

In the last analysis, the sociological, anthropological or even "ecological"[13] definitions provide the best account of the various facets of technical innovation and give the most complete insight into the phenomenon. Innovation is to be understood in the light of its multiple dynamic interrelations with the social environment. In this light, an innovation policy seeks to improve the structures and the "trophic" conditions which regulate the transfer and translation of the information that the innovator exploits in order to innovate.

This aim is already sufficiently ambitious for policy-makers to be aware of the obstacles to their intervention: they can neither deal with the whole range of circumstances, nor be responsible for all the data, nor above all take the place of the innovators. Where luck is involved, there is always room for disorder and surprise. Even a successful innovator would have difficulty in claiming that he had consciously dealt with all the factors that had been necessary for that success. Innovations nowadays may indeed depend more closely on science than they did in the past, but it must be stressed that, like Mallarmés throw of the dice, science can never rule out chance.

Innovation : the state versus the market

And how much do they depend on state intervention? The reasons why the state should or should not become involved in stimulating innovation and financing R&D is the subject of endless debate between free-marketeers and interventionists. Is it perhaps not a false debate? The country that makes the biggest fuss about relying on market forces, the United States, does not hold back from intervention. Under the Reagan administration, at least three areas have been deemed to be "legitimately" the object of federal government intervention long-term research, because "the incentive for strong support by private industry are lacking"; defense, space and environmental regulation, where the government "is the sole or dominant buyer"; and finally areas such as agriculture and health, where the Federal Government shares responsibility with other sectors, and in which "there is less justification for a dominant Federal Government role in the near-term applied research and, especially, development"[14].

Unlike countries with a strong interventionist tradition, like France and Brazil, the United States refuses to have an explicit industrial policy: it relies on indirect support (tax relief, rapid depreciation allowances, grants to small firms for feasibility studies, and the like) rather than direct assistance to industry. Nevertheless, with regard to military and even "strategic" research in the widest sense, the Reagan administration has made a step towards an interventionist policy in the French or Brazilian manner. For national defense, for which the R&D budget has grown substantially since 1981 thanks to SDI, public programmes are able to make large grants to the private sector, they can guarantee it a market for most of the products resulting from advanced technologies, and provide new openings in the form of "spin-offs" in the civil sector.

In doing so, the Reagan administration was not being innovative so much as carrying on the R&D policy pursued by every American administration, Democrat or Republican, since the Second World War, though priming the pump very irregularly. "The Administration recognizes that there are clear substantive commonalities in military and civilian R&D. Such areas of prime military technological interests as aviation, communications, electronics and computers also are each the focus of a major United States industry (...) In such areas, subject to security constraints, contractors are able to incorporate advanced military technologies into their civilian product lines"[15].

A substantial part of the R&D carried out for defense purposes is nowadays of a "generic" nature, in the sense that - just as basic research tries to push back the frontiers of knowledge - some kinds of military research try to push back the limits of the basic technologies, rather than developing specific weapons. They can thus accelerate or improve the potential for new applications in the civil sector. No amount of "reaganism" could halt this irreversible trend since the Second World War, which means that science and technology, under cover or excuse of defense, even in the United States, that paradise of the free market economy, have brought the public and private spheres of interest closer together. As Galbraith said, "The imperatives of technology and organization, not the images of ideology, are what determine the shape of economic society"[16].

In any case, faced with the Japanese co-operative research effort on the fifth generation of computers, the same line of reasoning on generic civil research has led the Reagan administration to ease the anti-trust regulations, which prevented the creation of a consortium of several firms acting together on

R&D. And the same arguments, in response to the same Japanese "challenge" forced Mrs. Thatcher's government to launch the Alvey Programme in 1983, whereby the state is providing 200 million out of a total of 350 million for five years[17].

One could be ironical on the subject of whether there is a rule of liberal orthodoxy that has no exceptions. But if ever fiercer international competition in the field of high technology products has legitimized this exception, even under Mrs. Thatcher, it nevertheless involves a bending of the rules in a way that is as old as the Industrial Revolution: this allows governments to protect infant industries for the same reason that they can protect armaments manufacturers. "All is fair in love and war" since everyone is doing the same thing, even as they make pious noises about free trade.

Nevertheless, there is a real difference between the way that a country with a laissez-faire tradition behaves and the way that an interventionist once behaved. In the former, the state does not take over industry's role in production; in the latter, the state not only places the order and makes the investment, it also implements the programmes, either through state-owned companies or else through private firms that are so closely tied to the public sector (like the French "national champions") that they are private in name only.

This is what I have called in the French case "the strategy of the Arsenal" - approach that goes back to Colbert - which has emerged again, basing the policy on innovation, through lack of private initiative, on the firms that are closely linked to or managed by the state[18]. The approach is not without success, either, when innovation concerns (besides defense) the big civil programmes on energy, transport, telecommunications. But it also has its limits: the markets for these programmes are captive markets,and if the policy of public purchasing reduces the risks at the level of research, it in no way guarantees the success of the innovations on the international market, unless there are more or less disguised export subsidies as well.

The problem raised by the "major programmes" is not just that they are vulnerable internationally because of the political uncertainties of their customers, it is also because the institutions responsible for them are so separate and sometimes so turned in on themselves that there are no "spin-offs' in the civil sector outside the technological field that they were supposed to develop, for instance weapons or nuclear power.[19] The state-as-entrepreneur is then exposed to three technocratic risks: the wrong choice of technical solution; lack of concern with the cost price; failure to recognize the needs of the final user. The government official may pride himself on his technical knowledge, but he is deceiving himself and his milieu if he reckons that he knows better than an entrepreneur what a firm should do to be innovative. And there is a strong temptation to support projects that are extremely sophisticated from a technical point of view, but that are likely not to be very profitable.

There is worse to come: a policy on innovation can even less nowadays concentrate on "major programmes" in that the great battles of international competition are fought with new products aimed at the mass market rather than in the areas covered by government orders. The new technologies like computers, new materials, biotechnologies, involve "entrepreneurial" kinds of initiative, where demand is more important than supply, a situation which is totally foreign to the state-as-entrepreneur. The state can, at best, help infant industries to take off, but it cannot mother them forever in the international marketplace. Lastly, the problem is not that of state intervention **per se**, which is inevitable and desirable where the public interest or the risks involved require strategic action and the private sector cannot take the initiative. **The real problem is getting the balance right between the weight of public intervention and creating an environment where innovation will flourish in the private sector.**

It is clear that an innovation policy is not an end in itself, just as the same is true of an industrial policy. But it is less self-evident that it is not a means in itself. In order to understand whv and how research and innovation works the relationship between the level of invention and of productivity, or more generally the links between investment in R&D, competition between firms and economic growth - it is not enough to examine the amount, the sources and the deployment of R&D expenditures. These factors have to be seen in their institutional context (political, economic, but also cultural) where they play a dynamic role. In truth, the factors of success are not tied merely to quantities, they are also (and perhaps even more so) tied to qualitative matters such as the efficient internal organization of the research system, the way it works, the kind of links it has with industry and the (international) outside world, and so on.

Today, policy discussions on innovation policies tend more and more to stress the measures required to promote "high-tech" industries, which are of increasing importance in competition, trade and economic progress of the most highly industrialized countries. But this debate, as Richard Nelson has emphasized, ends up by obscuring factors with a much more general impact, affecting the whole of the economic and social context, which may ultimately be of far greater importance. No historian or economist of technical change would neglect these factors, which the decision-maker nevertheless tends to underestimate or dismiss.

It is from this direction, however, that the state can do most to encourage innovation: the countries that aspire to be leaders in high technology industry, says Nelson, should invest above all in technical education and support economic growth through appropriate policies and institutions. "The most important lesson here is that nations aspiring to strength in high technology industries had better attend to their general strength in technical education and establish and maintain a set of policies and institutions supporting general economic growth. A possible danger of the recent rhetoric about the importance of high-technology industries is that it may take attention away from these broader policy issues"[20].

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IDENTIFYING THE CHANGING FRONTIERS OF SCIENCE

E. Garfield and H. Small

In this paper we will illustrate how the ISI database can be used to identify and map research fronts across many fields of science. This workshop is concerned with how science is transformed into technology and eventually new goods and services which contribute to economic growth. Much has been written about this complex process and many theories put forward[1,2]. Ours is not a case study, or a theory of the innovation process.Rather it presents a possible scenario of how a specific bibliometric methodology might be used to foster an innovation within a country such as Israel.

We begin with some theoretical speculations and assumptions. We hypothesize that there are such things as "hot" fields or emerging areas in science, and that we have a way of identifying them bibliometrically, that is, by a systematic and quantitative analysis of the scientific literature. Second, that it is possible to identify for a country or other geographic entity the extent of its scientific research in specific areas as evidenced by publication and citation patterns. Further, that we can determine the extent to which a country participates in the "hot" fields. Third, we assume that it is more effective to stimulate or augment a field which is already active in a given setting with investigators already dedicated to it than to create such an activity de novo where it did not exist before. Finally, we speculate (and this is the key point) that as a result of strengthening such "hot" spots in a country's scientific effort, we can increase the likelihood that research will lead to practical applications, and ultimately economic benefit.

To illustrate this process, let us take an actual example for Israel, drawn from our 1987 cluster analysis of the Science Citation Index and Social Sciences Citation Index. I will not attempt to explain how clustering is actually carried out. There are many methods one could apply and we have experimented with only a few of them. In capsule, our method uses highly cited papers (core papers), a measure of association between them called co-citation, and single-link clustering routine.[3]

Let's begin with the notion of a "hot" field. We know that modern science can progress very rapidly, and within a short period of time dramatic gains in knowledge can be made. In citation analysis we have an analogue to this, namely the number of highly cited papers that are of recent origin. We sometimes call this "immediacy". By counting the number of "core papers" in each cluster published within the last 3 years, we have a simple and powerful measure of immediacy. In other words, in an area of science with high immediacy there have been a number of important recent findings.

Figure 1 shows the clusters from our 1987 database that have the most core papers from '87, '86, and '85. This number is given in the column labeled IMMED. Since we know that of all 54,095 core papers in 1987, only 14% come from these 3 most recent years, the clusters shown here all have a high degree of immediacy.

The next problem is how to select the high immediacy or "hot" fields for a particular country such as Israel? One way is to look at the author addresses of all the papers that cite the core documents in a cluster, and compute the percentage of these citing papers for the country in question. Figure 2 shows the same clusters for Israel, specifically selected for having 12 or more Israeli core papers and 8% or more of their total citing papers from Israel. These are all areas where Israel has a significant amount of research already going on. We know the percentage of papers in our complete file published in Israel is about 1%. In other words, the total citing population from Israel is only slightly over 1%. Hence any cluster producing papers above this level may represent an area of concentration for the country.

To pick the "hot" fields which Israel is strong in, we simply look down the IMMED column until we see a cluster above 14% in immediacy. On this list two jump out -- "lupus erythematous" (#1263) with 9 of 31 recent core papers and "random resistor networks" (#2254) with 6 of 13. Both areas currently have Israeli participation by citing authors well above the 1% expected level, 9.6% and 11.7% respectively.

Another way we can look for country strength is by the addresses of the core papers themselves. This indicates that a leading researcher or perhaps a research group is working in a given country on a specific topic. Addresses of cited papers require more effort to retrieve so we have only generated a partial analysis of 1987 clusters with 2 or more core papers from Israel with publication dates between 1973 and 1984 (Figure 3). We are in the process of updating this analysis to 1987 so this is only an interim result.

Despite this limitation an interesting pattern emerges. We find that a general area of physical science sometimes referred to as "chaos" (4) is associated in Israel with a number of leading institutions including the Hebrew University, Tel Aviv University, the Weizmann Institute, and the Technion. The "random resistor network" cluster also falls in this general area, and hence we have selected it as our example, although any number of others might have suited our purpose equally well. In passing we note that Benoit Mandelbrot has co-authored with Israelis on chaos.

As a next step let us orient ourselves to the field of chaos and within it, random resistor networks. At ISI we have developed methods for visualizing the complex web of research areas by what we call cluster mapping. (5) Cluster mapping utilizes the co-citation linkages between papers and clusters of papers, applies multidimensional scaling to those links to arrive at a two dimensional display of the relations between areas. Since our clustering is iterative and hierarchical we are also able to show various levels of detail from a very broad disciplinary view down to the individual research specialty composed of core papers. We illustrate this by a succession of maps, each nested within the one before it.

First, we show what we call a "global map" which is the most inclusive map we can draw (Figure 4). It is comprised mainly of two large groupings, one labeled physical sciences and the other biomedical and social sciences. The remaining smaller circles are various physically or biologically oriented sub-fields which are insufficiently linked to the major disciplinary groups to be encompassed by them, but not so isolated from them to fall off the map entirely.

Since our selected area is within the physical science circle, we blow up the display for it at the next lower level (Figure 5). Now we see three major groups: chemistry, physics and geoscience and also a number of smaller groupings. One of these smaller ones is labeled "chaos" and it is closely allied to physics. Zeroing in on this area (Figure 6), we see initially not much structure: two very closely related groups, one labeled "classical and quantum chaos" and the other "general chaos".

The circle on the right is the one we want and entering it (Figure 7) we get our first glimpse of the cluster we are seeking, namely, "random resistor networks" (#2254). We note the diverse structure of this "chaos" map with some unusual nomenclature such as "strange attractors" "eden growth" "lattice animals" etc. We noted before that Israeli scientists played a leading role in several of these, for example, #5201 on "deterministic chaos" #5949 on "Sierpinski carpets" and #526 on "anomalous diffusion" Note the proximity of these areas to our chosen example, "random resistor networks" #2254, in the center of the map.

Now we enter cluster 2254 (Figure 8) and see its internal structure in terms of the core papers that comprise it. We are first struck by the international diversity of its authors: U.S.A., France, Soviet Union and Israel. The last named country is represented by a paper from the Technion in Haifa whose first author is Ben Guigui, published in **Phys Rev Letters** in 1984 (Figure 9). When we recall again that 11 percent of the papers citing these core papers have Israeli authors, we know that we have a focal point of Israeli activity, and this focus broadens to adjacent, and closely allied topics in "chaos". This activity is significant both from the standpoint of important work being done (i.e. a leadership role in the field) and a significant quantity of active research workers laboring in these fields (i.e. followers).

In short, it seems that we have here a "hot" field with significant indigenous strength, which might be an ideal case to foster and stimulate for future economic payoff. Of course, the problem is we have no way of knowing whether there will ever be such practical spin-offs of this basic science endeavor. The beauty of basic science is simply that we can never tell for sure. Nature is full of surprises. However, putting a bet on an emerging field where local talent is already at work, seems as sound a basis for investment in basic research as can be devised.

This is not to say that all is well with basic science in Israel. In fact, we have reason to believe that it is in decline and in need of increased support and stimulation. Using our new Science Indicators file covering the years 1973 to 1984 (6) (soon to be updated to 1987), we have produced the graph of Figure 10 which plots the mean number of citations per paper for four countries (USA, UK, Israel, and Belgium). To show a time series we have constructed sliding 5 year cumulations of publication and citation data, beginning with the 1973-77 period and ending with the 1980-84 period. Within each period we take all ISI source papers published and cited at least one time in that interval and sum all citations to them from that same time interval. We then divide one by the other to get the mean citations per paper. As a normalization, we divide each mean by the corresponding 5 year mean for the overall file. This corrects for any fluctuations which may have resulted from changes In ISI's journal coverage over the 12 year period.

The resulting time series of relative means for overlapping 5 year periods, shows some clear trends: down for U.K, and Israel and up for Belgium and U.S.A. These data suggest that the Israeli government should give serious attention to the overall state of science and its support in Israel.

Fig. 1: 1987 CI Clusters Sorted by Immediacy

RANK	C4	C3	C2	CI	CORE	CITING	IMMED	IM-PR	CITE/CO	CLUSTER --NAME-
1	3	3	46	307	52	919	42	81	18	TUMOR NECROSIS FACTOR; CULTURED HUMAN-ENDOTHELIAL CELLS; INVIVO IMMUNE-RESPONSE
2	7	39	177	542	57	387	39	68	7	COMET P/HALLEY; MODELING HALLEY; ALFVENIC TURBULEN CE IN THE SOLAR-WIND FLOW
3	3	27	76	2039	52	545	39	75	10	HUMAN IMMUNODEFICIENCY VIRUS; HETEROSEXUAL TRANSMI SSION; AIDS IN AFRICA; HIV INFECTION
4	1	4	165	709	41	708	34	83	17	SUPERSTRING MODELS; EXTRA E6 NEUTRAL GAUGE BOSONS IN EP COLLISIONS; INTERMEDIATE MASS SCALES; HETERO TIC STRING THEORY
5	3	3	66	104	57	912	32	56	16	TRANSFORMING GROWTH FACTOR-BETA; CIRCULATING IMMUN OACTIVE INHIBIN; SINGLE-CHAIN MR 35,000 MONOMERIC PROTEIN
6	1	33	88	892	28	1093	27	96	39	HIGH-TC SUPERCONDUCTORS; BAND ELECTRONIC-STRUCTURE ; DOPED ORTHORHOMBIC LA2CUO4
7	3	34	92	2397	53	370	27	51	7	DOPPLER UMBILICAL ARTERY VELOCIMETRY; FETAL BLOOD-FLOW; VELOCITY WAVEFORMS; ORTHOTOPIC LIVER-TRANSPL ANTATION
8	3	3	83	1748	53	1392	26	49	26	RECOMBINANT MURINE GRANULOCYTE MACROPHAGE COLONY S TIMULATING FACTOR; REGULATION OF HEMATOPOIESIS; PR OTEIN TYROSINE KINASE-ACTIVITY
9	3	3	255	1000	43	670	26	60	16	DUCHENNE MUSCULAR-DYSTROPHY; DNA PROBES; CARRIER D ETECTION
10	3	78	433	1102	50	688	26	52	14	ATRIAL NATRIURETIC PEPTIDE; FACTOR IN BLOOD-PRESSU RE CONTROL; EXOCYTOTIC RELEASE
11	1	4	27	1355	44	372	25	57	8	ICOSAHEDRAL QUASICRYSTALS; DECAGONAL PHASE OF AL-M N ALLOYS; DIFFRACTION PATTERNS
12	1	13	32	761	51	615	22	43	12	DIFFUSION-LIMITED AGGREGATION; FRACTAL VISCOUS FIN GERING; DENDRITIC GROWTH
13	0	0	38	844	54	363	21	39	7	PERCUTANEOUS TRANS-LUMINAL BALLOON VALVULOPLASTY; CORONARY ANGIOPLASTY; TREATMENT OF PULMONIC STENOS IS

Fig. 2: 1987 Research Fronts with ≥ 12 Papers and ≥ 8% Total Papers from Israel

CI Cluster	Core Size	Citing Papers	Israel Papers	% Israel	Immed*	Name
1263	31	677	65	9.6	9	lupus anticoagulant in systematic lupus-erythematosus (biomedicine)
1431	22	234	48	20.5	0	evolution of the Dead-Sea Jordan rift system (geology)
5888	3	111	35	31.5	0	hairy-cell leukemia (biomedicine)
5792	5	231	28	12.1	0	differentiation of murine erythroleukemia cells (biomedicine)
5210	5	178	25	14.0	0	vitamin-D metabolism (biomedicine)
570	10	165	22	13.3	2	entamoeba-histolytica infection (medicine)
1086	26	112	21	18.7	3	posttraumatic stress disorder (psychology)
2335	10	231	20	8.6	1	blood-pressure control in children (medicine)
1860	6	199	18	9.0	0	rat peritoneal mast-cells (biomedicine)
2254	13	153	18	11.7	6	random resistor network (physics)
5984	5	60	17	28.3	0	suspected tibial stress-fractures (medicine)
3136	2	151	17	11.2	0	mouse bone-marrow cells (biomedicine)
3889	7	171	16	9.3	1	subacute cutaneous lupus-erythematosus (medicine)
238	4	60	16	26.6	2	organic solid-state reactions (chemistry)
8313	2	38	13	34.2	0	social support (psychology)
6803	2	153	13	8.5	0	differentiation of HL-60 promyelocytic leukemia-cells (biomedicine)
2877	3	74	12	16.2	0	plasma parathyroid-hormone (biomedicine)
4649	5	90	12	13.3	0	copper deficiency (biomedicine)

* Immed = number of core papers with publication years within the last 3 years, i.e., '85, '86, and '87.

Fig. 3: 1987 Clusters with 2 or More 1973-1984 Core Papers from Israel

C4	C3	C2	C1	# Core Papers From Israel	Name of Cluster
1	1	182	1431	6	evolution of the Dead-Sea Jordan rift system
1	13	32	5201	3	deterministic chaos
1	13	32	5949	3	Sierpinski carpets; fractal lattices
1	33	88	2726	3	quasi-one-dimensional conducting polymers
3	27	185	345	3	treatment of acute pelvic inflammatory disease
0	0	0	1405	2	fractal dimensions
0	0	0	1580	2	passive hyperthermia in humans
0	0	0	1635	2	anisotropy of magnetic-susceptibility
0	0	0	8169	2	lectin receptor expression
0	0	736	4146	2	propagation of stationary non-adiabatic combustion waves
1	4	27	330	2	random field Ising-model
1	4	168	439	2	isovector monopole resonances in N-not-equal-Z nuclei
1	4	270	1114	2	interacting Boson model
1	12	524	1848	2	threshold voltage in thin oxide mosfet
1	13	32	526	2	anomalous diffusion
3	3	83	1748	2	regulation of hematopoiesis
3	3	83	5279	2	small nuclear ribonucleoprotein-particles
3	45	683	2223	2	phytoplankton in coastal waters
3	62	191	3176	2	membrane fluidity

Fig. 4: Global Map

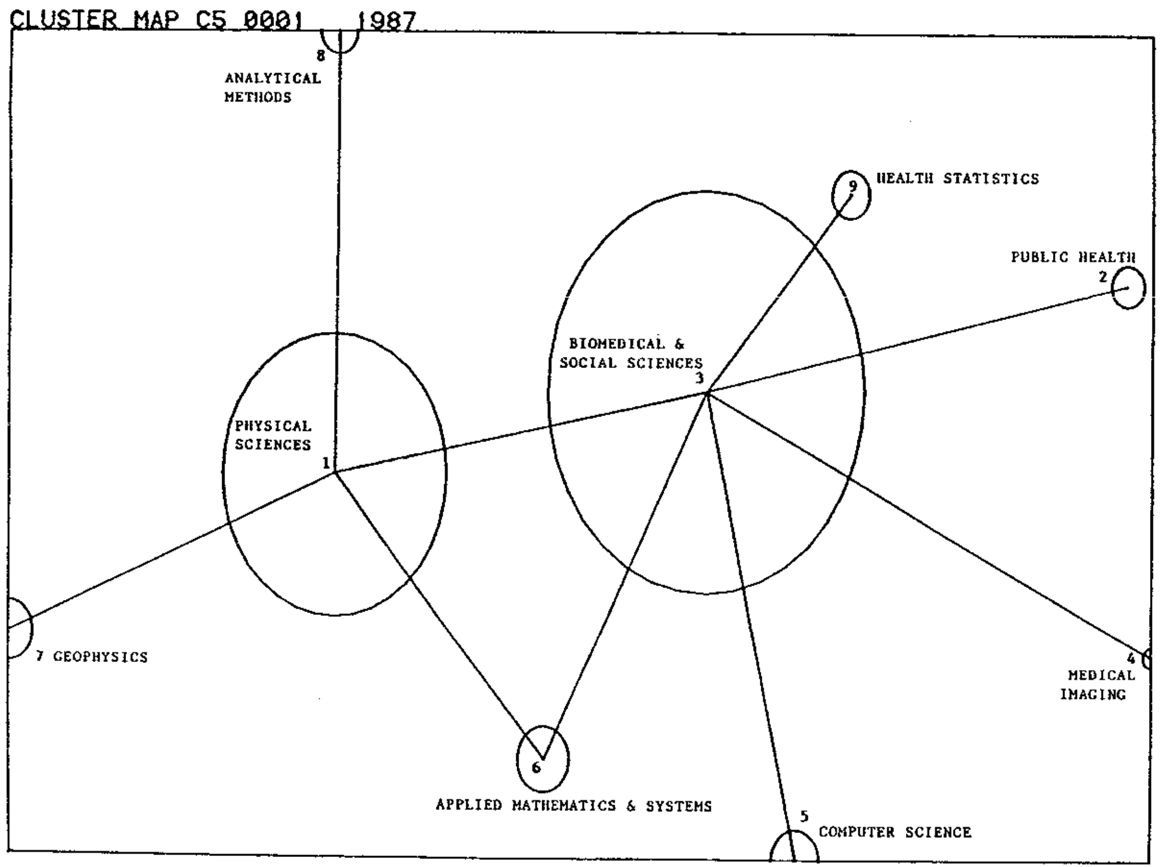


Fig. 5: Physical Sciences

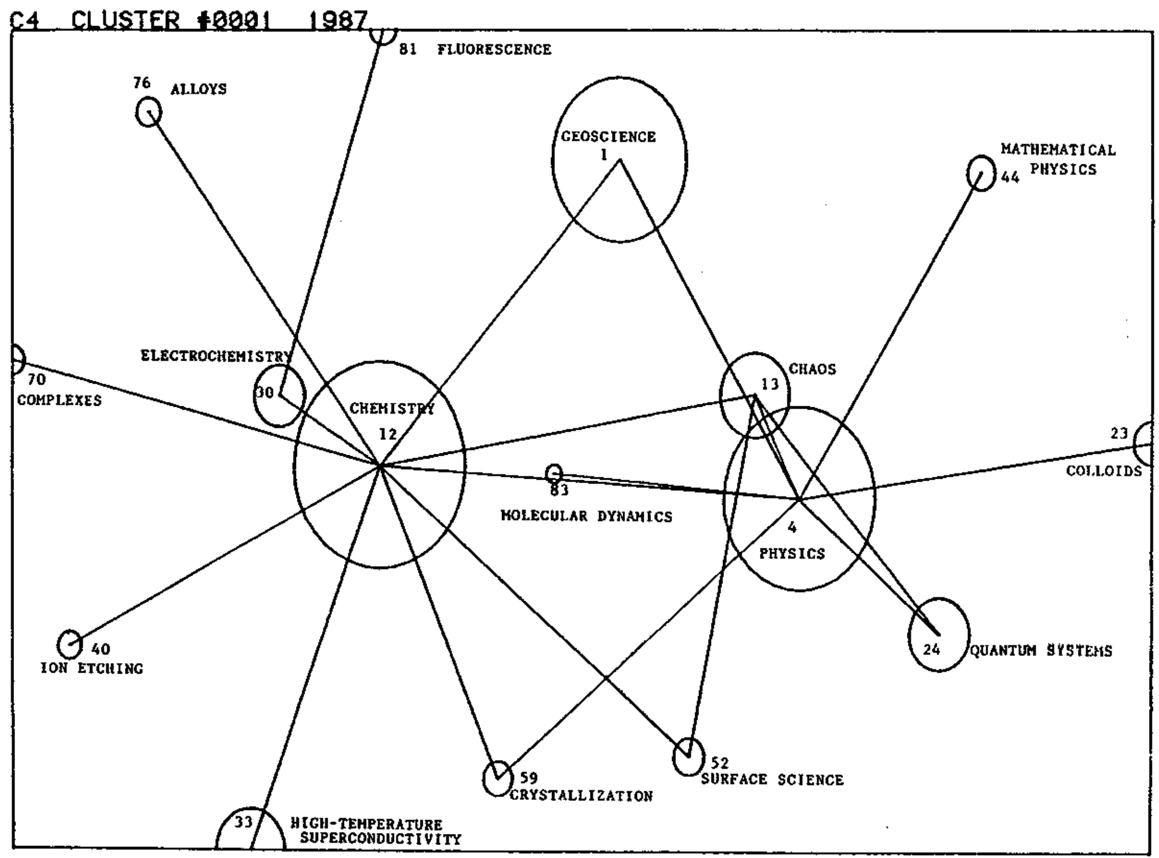


Fig. 6: Cluster Map C3 0013 1987

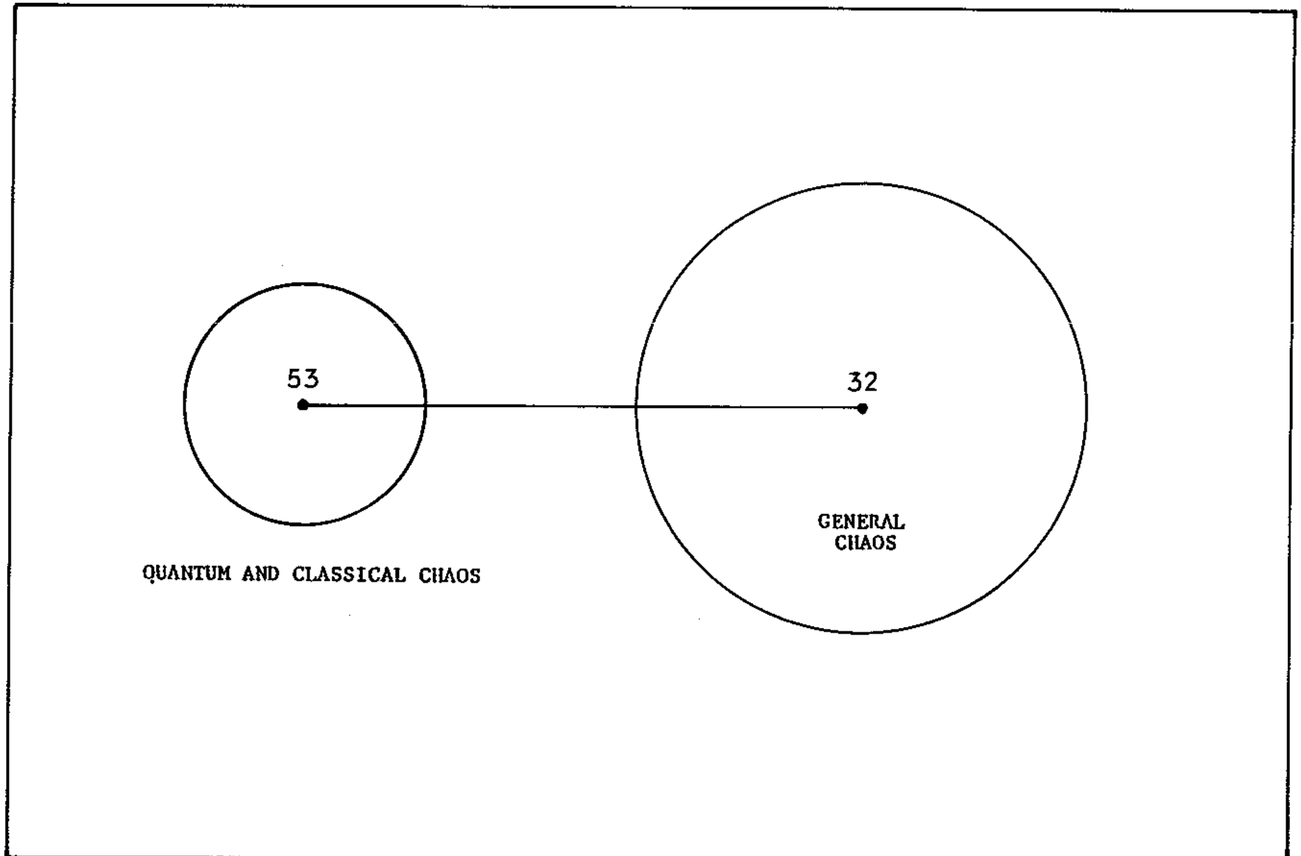
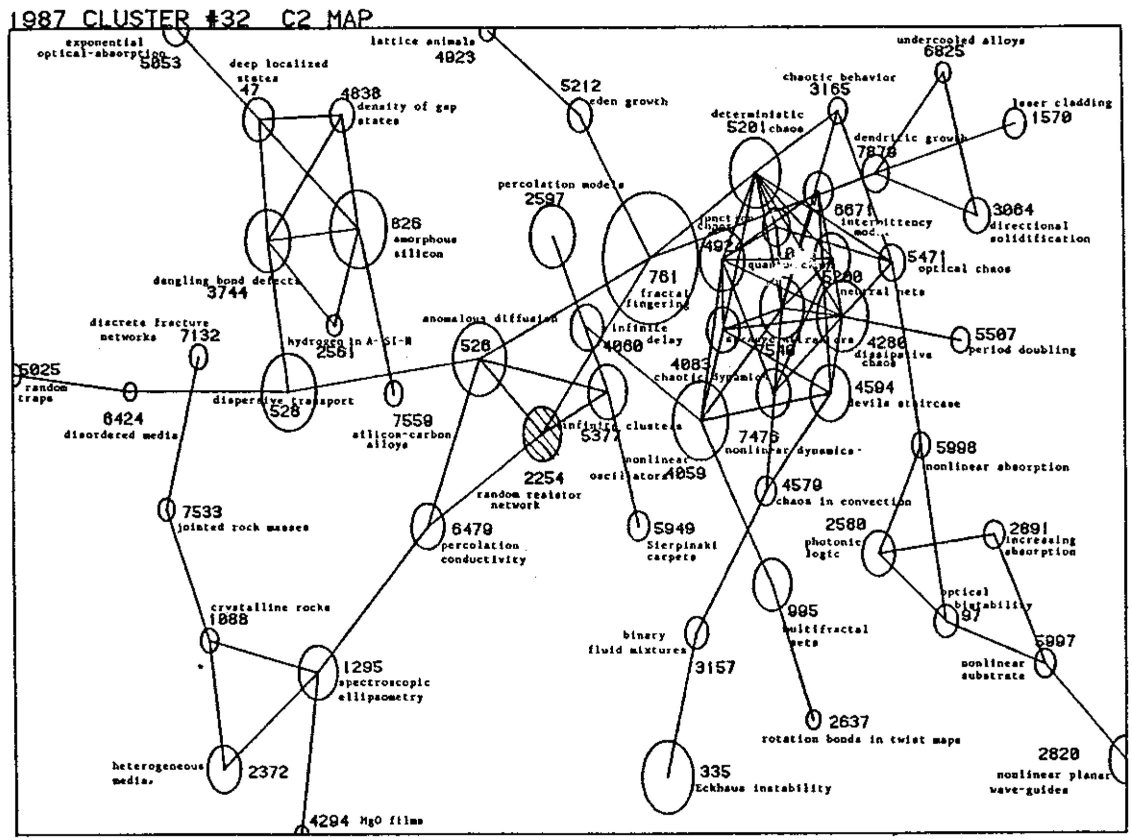


Fig. 7: Chaos



1987 CLUSTER #32 C2 MAP
exponential optical-absorption 5853
deep localized states 47
4838 density of gap states
826 amorphous silicon
dangling bond defects 3744
hydrogen in A-SI-H 2561
discrete fracture networks 7132
5025 random traps
6424 disordered media
dispersive transport 528
7559 silicon-carbon alloys
lattice animals 4023
5212 eden growth
percolation models 2597
761 fractal fingering
anomalous diffusion 526
infinite delay 4060
infinite clusters 5377
2254 random resistor network
6479 percolation conductivity
nonlinear oscillators 4059
5949 Sierpinski carpets
7533 jointed rock masses
crystalline rocks 1088
1295 spectroscopic ellipsometry
heterogeneous media 2372
4294 MgO films
undercooled alloys 6825
chaotic behavior 3165
deterministic 5201 chaos
dendritic growth 7878
laser cladding 1570
3064 directional solidification
6671 intermittency
5471 optical chaos
neural nets
5507 period doubling
4280 dissipative chaos
4083 chaotic dynamics
4594 devils staircase
7476 nonlinear dynamics
5998 nonlinear absorption
4570 chaos in convection
2580 photonic logic
2891 increasing absorption
995 multifractal sets
optical bistability 97
5997 nonlinear substrate
binary fluid mixtures 3157
2637 rotation bonds in twist maps
335 Eckhaus instability
2820 nonlinear planar wave-guides

Fig. 8: Random Resistor Networks

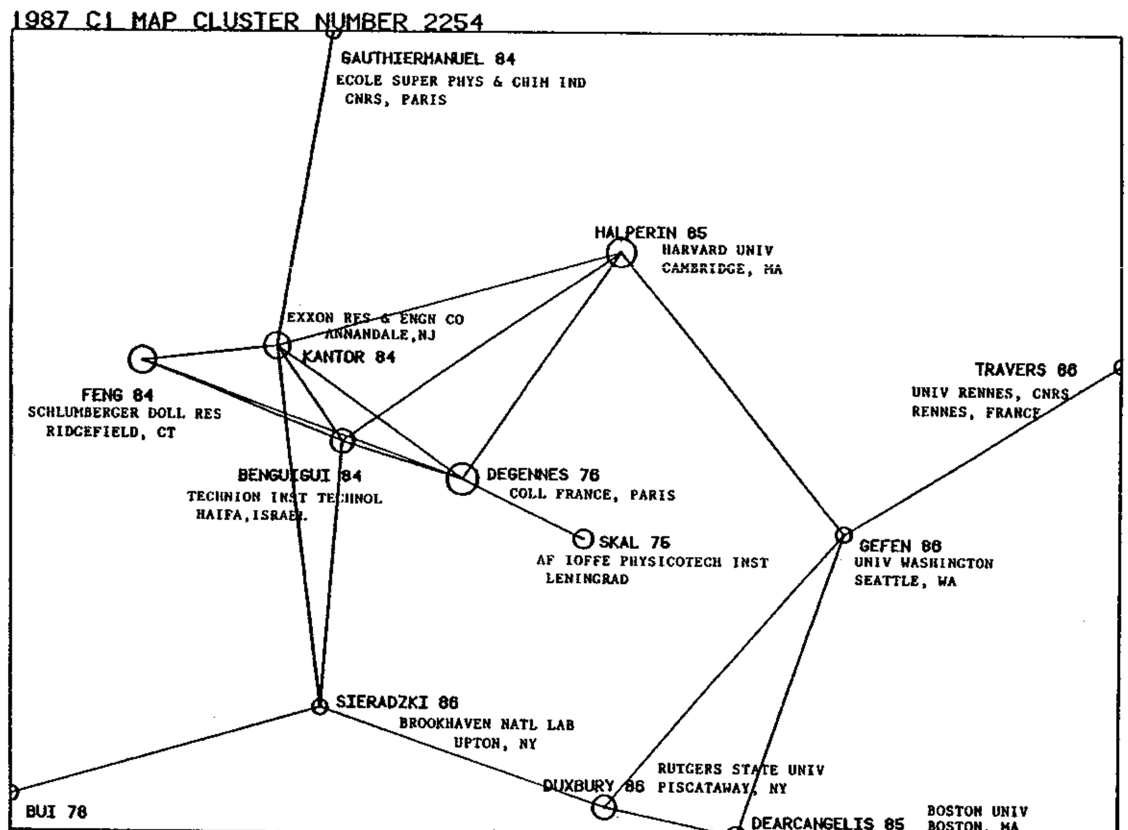
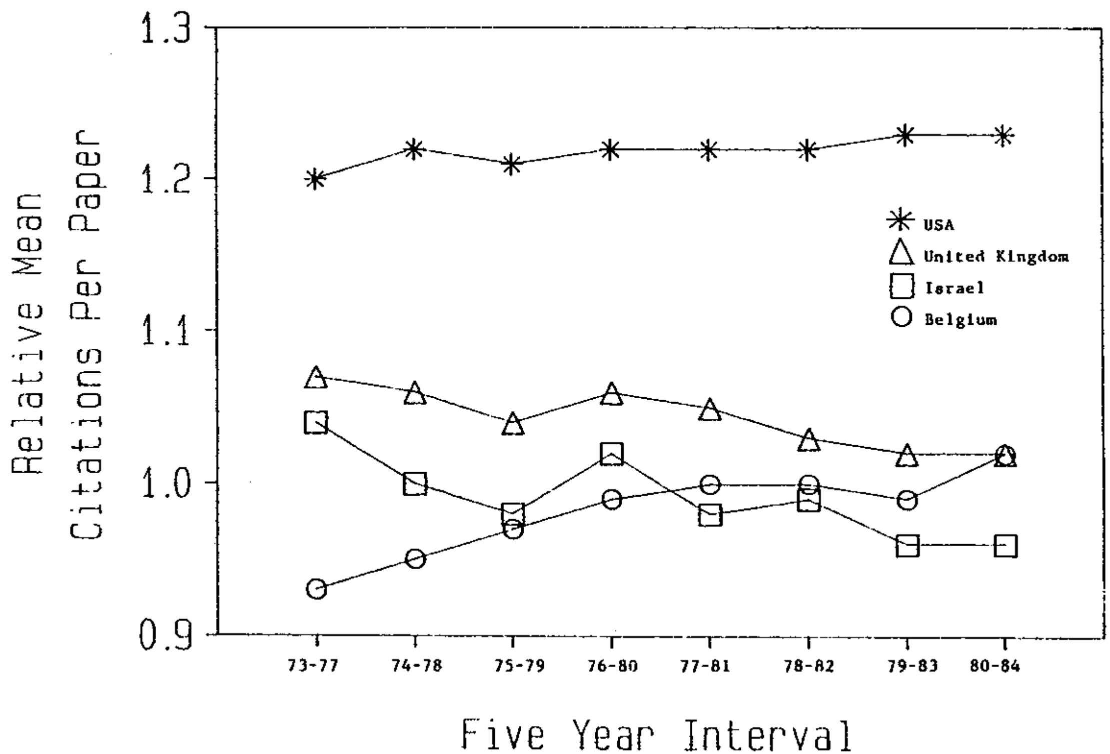


Fig. 9: Core Papers in 1987 CI Cluster #2254

C4	C3	C2	C1				citations in 1987
1	13	32	2254	BENGUIGUI L	PHYS REV LETT	53:2028, 1984	17
1	13	32	2254	BUI HD	MECANIQUE RUPTURE FR	0: 0, 1978	8
1	13	32	2254	DEARCANGELIS L	J PHYS LETT PARIS	46:3585, 1985	12
1	13	32	2254	DEGENNES PG	J PHYS LETT PARIS	37:3001, 1976	31
1	13	32	2254	DUXBURY PM	PHYS REV LETT	57:1052, 1986	18
1	13	32	2254	FENG S	PHYS REV LETT	52: 216, 1984	23
1	13	32	2254	GAUTHIERMANUEL B	J PHYS E	17:1177, 1984	7
1	13	32	2254	GEFEN Y	PHYS REV LETT	57:3097, 1986	8
1	13	32	2254	HALPERIN BI	PHYS REV LETT	54:2391, 1985	27
1	13	32	2254	KANTOR Y	PHYS REV LETT	52:1891, 1984	22
1	13	32	2254	SIERADZKI K	PHYS REV LETT	56:2509, 1986	7
1	13	32	2254	SKAL AS	SOV PHYS SEMICOND	8:1029, 1975	12
1	13	32	2254	TRAVERS T	J PHYS A	19:1033, 1986	7

Fig. 10: Mean Citations Per Paper Relative to Mean for Overall File



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WHERE SCIENCE LEADS DOES TECHNOLOGY FOLLOW?
Conceptual and Policy Conflicts in the Public Funding of British Science and Technology

P. Mathias

I want to juxtapose two debates in the brief half hour at my disposal. One concerns the historical controversy amongst historians of science and technology and economic historians about the role of science in technical change since the beginnings of modern industrialization, current for many decades now and still active. The other concerns arguments which are now becoming strident in the debate which relates to the current funding of scientific research and the strategy for science policy being pursued by governments. Both the level of the funding of science in relation to national wealth and the priorities to be accorded to the kind of scientific research deserving public support are in question. (I shall take only the United Kingdom as my territory but similar assumptions and propositions echo round the western world).

One should say at the beginning as well as at the end of such comments that relationships between science and technology continue to change over time; that the translation of advances of knowledge into commercially profitable wealth creating processes involves many variables, with unpredictability ex ante built into the equations; that the institutional context, the level of wealth, the role and status and power of science, together with the effectiveness of the scientific establishments in influencing policy, have all changed over recent years, not to mention over the decades and the centuries. At all times, however, enough examples of individual instances can be found on either side of any generalization **ex post facto** to make assertion by examples a non-definitive way of proceeding. I do not need to rehearse the disutilities of direct historical analogies to such an audience. But, in my view, the two debates deserve to be studies in conjunction. There are, indeed, conjunctions between them. The historical debate, which is contemporary both in the sense that it is still in progress and is responding, as historical debates always do, to changes in the consciousness about contemporary issues, should widen the range of awareness for the present policy debate, with simplistic assumptions and all manner of other false gods being exposed for what they are.

Backward looks are frequently being flung over their shoulders by scientists, politicians and administrators embattled in the present arguments but all too often are hasty, self-justifying in support of stances already determined and at the lowest common denominator of sophistication and intellectual awareness - one might even posit intellectual honesty - which characterized some of the early exchanges in the historical debate. Scientists, who would never dream of tolerating such naive methodologies in their own laboratories, peddle simplistic nostrums in the current policy debate and are then affronted when these are countered by equivalently naive assertions on the other side by hostile politicians.

The first phase of the debate concerning the relations between science and technology in the early phases of industrialization tended to be cast in simple polarized terms.[1] On the one side came assertions that technical change in the Industrial Revolution, if not the Industrial Revolution itself, was the gift of science to industry; the empirical legacy of the scientific revolution of the 17th century. The assumptions were that the linkages were one-way transfers, in direct linear terms, of additions to formal knowledge from scientific advance, to inventions, to innovations and the diffusion of technical change. The modern analogue, of course, is that technical advance currently runs from fundamental (or 'basic', or 'blue skies' or 'curiosity-orientated') academic science by similar direct linear connection, to applied research, to development and new products and processes.

In the historical debate these holistic assertions were buttressed by a listing of instances where specific innovations, or general areas of technical advance, could be seen as associated with scientific interest and experimentation. Piecemeal casual correlation was given causal significance. Increasing the number of instances where such superficial connections seemed to exist constituted increasing proof of the causality. It remains difficult to see the statistical significance of such accumulations of piecemeal instances in the absence of an agreed data base of homogeneous or weighted data, even though a lengthening list gives superficial credibility to the positive correlation.[2] Naive quantification was thus invoked in the service of naive theorizing. The evidence of intention, much proclaimed since (and indeed during) the Middle Ages, was accepted as evidence of results.

Confronting this set of assertions, supporting examples and assumed causal relationships, which gave science a primary role in technical advance, was a similarly holistic thesis: that technical change in the Industrial Revolution was a product of inspired amateurs and artizans of genius. Technology advanced under empirical stimuli - a sort of blind technological Darwinism was winnowed by a commercial Darwinism whereby the most efficient and lowest-cost species survived - quite divorced from advances in science.

Scientific knowledge progressed, in fact, by scientists clinging to the coat-tails of the innovating artisans, as they tried to understand the fundamental relationships involved in chemical reactions or theories of thermodynamics, or geological formations which lay behind advances in new industrial processes, new prime movers, advances in mining technology and the like - all moving forward under responses to available opportunities and constraints, innocent of awareness of such fundamental knowledge. "Science owed more to the steam engine than the steam engine to science" is a famous encapsulation of this thesis. If there was a correlation between science and technology before the mid-nineteenth century, the direction of causation within it was the reverse - but, on the whole, it was argued in this schema, science advanced under its own, quite separate, stimuli, following the dictates of intellectual curiosity, or reacting with theological imperatives about the origin of the universe and the like. Science and technology were sailing their own courses, like ships in a fog, subject to occasional rather random collisions.

Both these caricatures have lost their credibility as historical reality has become explored in greater depth and with less methodological naivete. That useful Cerberus, who stands before the gate of so many intellectual controversies, barked his warning about the differences between necessary and sufficient conditions. It was seen that interrelations were complex and changing over time (with the theory and practice governing steam power, for example, showing such complex interactions down to Carnot and after). With generalizations about the origins and dynamics of technical change, the scene varied between different sectors of the economy, and between different regions. Germany was different from France; England from Scotland. The cultural and institutional context within which both scientific knowledge and technical change were advancing also varied as did the changing culture of science, the "images of knowledge", questions of intellectual motivation as well as intellectual methodologies, the co-existence of intellectual and material interests of scientists. The intellectual battles in which science was but one contesting party, produced a complex, multi-dimensional pattern which is still being explored.

There is general agreement, I believe, that relationships between science and technology advanced structurally as they developed in significantly new ways, and have continued to do so, since the mid-nineteenth century. Here the terms of the debate have become clearer: and the focus is given to them not by the role of science in generating technical change in the Industrial Revolution, but lags in the promotion of science and research in Britain being one of the sources of the relative decline of British industry after 1870. This was characterized by the slowness of new industrial sectors to come forward, increasing import penetration coupled with the failure to develop export markets in what we would now call 'high-technology' sectors, and a severe attack of technological conservatism in the industries of established dominance - textiles, iron and steel, coal, boot and shoe making, glass, etc. This controversy, never totally silent, became vociferous once more to explain Britain's more recent relative decline and merge, with the contemporary debate about science policy.[3]

Already, in 1851, a diagnosis had been made by Lyon Playfair and a debate started, which has never ceased, that industrial success would in future be achieved through a 'competition in intellect', as he named it, through the products of 'philosophy' (i.e. natural philosophy or science) and that England was the only State in Europe unaware of this truth and neglectful of its national heritage in this respect.[4] The diagnosis centred upon the public funding of scientific education and research and its institutionalization in Britain. Partly it was a question of overall levels of investment - by any count of science graduates in proportion to population England was Europe's laggard - partly that of the orientation of British science. Pure science, although eminent in terms of intellectual status, was said to be the province still of rich gentlemen and noble amateurs or (in England) of endowed chairs in universities with no close links to industry. Kelvin, and a handful of others, were the individual exceptions which proved the general rule and Scotland, in any case, followed a significantly different intellectual and academic tradition.

In particular, England experienced a failure to develop the critical institutional links in the educational matrix which provided a transmission belt between science and industry - the Technische Hochschulen and Ecoles Polytechniques. With rare exceptions British industry did not recognize this need from its own viewpoint; it was not industrialists but a minority of scientists who publicized the issue and pressed it on Parliament, and research laboratories were not generated by British firms, as in Germany. Such chemists as were employed, as in the brewing industry after 1860, were usually scientifically educated in Germany and used for quality testing of products more than in a search for innovations.

The central causal assumption behind this debate was clear: technical change, in the new circumstances of the advances of science and new ranges of industry growing in the seedbed of the new technology, was increasingly a product of the application of science. Historians have broadly endorsed this verdict that where science led, technology could follow, of course, where other necessary pre-conditions for profitable operations were present. The First World War seemed to prove the case: when German imports were cut off, Britain was shown to be deficient in whole ranges of science-based products: dyestuffs, some chemical propellants, and fuses and other products of the fine-chemical industry, magnetos and other ranges of electrical engineering products; optical glass and the like. Indigenous capacity in new industries, such as electrical engineering of all kinds, tended to be of foreign origins, with names such as Siemens, Westinghouse being prominent.

While not disagreeing much with the diagnosis about the widening relationships between science and industry, in my view the backwardness of the U.K. has been uncritically overdrawn. Much important science, exactly in the matrix of the industrial areas and with industrial orientation, was based in non-degree bearing institutions in Britain such as Glasgow Technical College or Firth College in Sheffield. Every main engineering, industrial and mining centre in the country developed such institutions, virtually ignored in higher education until the 1960's when many of them miraculously broke surface as polytechnics, colleges of advanced technology and technological universities. The civic universities, as they evolved in the later nineteenth century, also had much closer links with their local business communities than Oxford - Cambridge. The engineering institutions published prestigious journals and trained technical cadres, while not degree-awarding bodies. Some important linkages developed, for example in heavy engineering for naval purposes, and special steels (where Sheffield led the world) sub rosa with academic consultants, such as Oliver Arnold, bound to commercial secrecy by the terms of his contract with the steel masters.[5]

Of course, when comparisons are made with continental countries, who ordered their affairs differently in this respect, by counting graduates the quantification suggests a situation worse than it was. And a second look at these relationships during the First World War reveals a rather more critical assessment in my view. When the crisis broke in 1914, in circumstances where the national emergency produced effective political will and public resources, the response was swift. By 1917 capacity had been created in all the missing sectors and the resulting products compared broadly both in quality and output terms with German exemplars.[6] This was also true of rapidly developing new 'high-technology' industries such as aircraft, with widespread linkages in the demand for correlated products of quite new technical standards - dope for wing fabrics, power-weight ratios in engines; higher octane fuels, new strength/weight limits in non-ferrous metals; rubber, etc. The speed and the extent of the response showed that the potentiality for translating scientific research into products existed on a wide scale in the U.K.

Instrumental in this transformation was the D.S.I.R. the Department of Scientific and Industrial Research (note the juxtaposition of terms) - set up in 1916 to mobilize scientists and research capacity to provide the missing products. From this body evolved the Science Research Council and Successively the other four Research Councils which now form the institutional apparatus through which the Civil Science budget is mainly deployed in the U.K. It is worth remarking in relation to the controversies which have grown since 1981, and now continue ever more fiercely, about the role of the Research Councils that the system was born not to promote academic science, fundamental research or national research facilities but to develop industrial capacity in strategic technological sectors as rapidly as possible in order to close a technological gap in circumstances of national emergency, even if the competition was then that of war rather than commerce.

A short paper can only single out a few aspects of what is a complex scene and, in switching to current policy in the U.K. concerning the funding of research, I exchange my own position from economic historian - that is as a professional **voyeur** of the earlier debate - to a participant as Treasurer of the British Academy and Member of the Advisory Board of the Research Councils, which makes the annual bid for the Civil Science budget, allocates the aggregate sum between the five Research Councils and advises the Secretary of State on research priorities.

Present conflicts over the public funding of science have produced greater resonance in academic circles, the media and Parliament than at any time since the nineteenth-century campaigns to promote technical education and establish a well-found national budget for science in the 1860's and 1870's. A new political lobby 'Save British Science' was born from a newly politicized scientific community which, in Oxford's case, was sufficiently enraged to publicly vote down a proposal to give Mrs. Thatcher an honorary degree, no insignificant symbolic act. The debate continues and, judging by the rhetoric at least, the system is in turmoil.

Some contextual data need to be mentioned. In certain respects the U.K. has been sharing international trends; in other ways reactions to the context have been culturally or politically specific. The funding of science broke trend in the early 1970's, in the U.K. as elsewhere, from an expansion of 5 - 7% p.a. - doubling every 15 years - according to Derek de Solla Price's famous extrapolation over three centuries, to supposedly 'steady state' funding.[7] But de Solla Price, in predicting that this had to end, also warned that dangerous instability might then ensue, rather than a steady state. The international comparisons now show the U.K. with a comparable percentage of G.D.P. (2 - 2.7%) being devoted to total 'research and development' spending to that in other major industrial economies such as U.S.A., Japan, Germany and France, with U.S.A. and Germany being a little higher in 1981-83, and France a little lower.[8] But removing defense R&D from the count reveals the U.K. to be significantly lower than these competitors, at 1.7% of G.D.P., with Germany, Japan and U.S. all above 2% of G.D.P.; while a lower percentage of Civil R&D in the U.K. is being financed by industry than elsewhere. In recent years the trend of research spending in the U.K. has been down while elsewhere levels have been rising. The official U.K. projections to 1991, for example, show an anticipated 0.2% p.a. decline in the Civil Science budget (one or two earmarked additions for AIDS and Antarctica apart), which compares with a projected growth of G.D.P. of 8% in the same period.[9]

However, such supposedly 'steady-state' funding in financial terms - or the gradual expansion in 'real' terms financially which official data show for 1979-87 (U.G.C. and the Science Budget 1162M - 1242M Pounds Sterling) - has produced a steadily declining volume of scientific research.[10] The Treasury 'deflator' used to calculate the inflation of costs has regularly undershot without subsequent compensation being provided for cash-limited budgets. Salary levels have moved consistently ahead of official allowances (those actual salary increases, of course, being officially negotiated but not then reflected in the aggregate budget, which remained firmly set in aggregate cash terms). International subscriptions, CERN costs, which cannot be re-negotiated by a single member state, have made greater inroads into the science budget, with a falling pound to 1988, again without adequate compensation in the cash limit. Redundancy costs, coming from an enforced contraction, also had to be sought from within the same cash budget. Science costs, with growing sophistication of instrumentation, have consistently moved ahead faster than the Treasury deflator or the retail price index; and this has not been acknowledged through the application of a special 'science price index'- although efforts have recently been made to construct one. University budgets have fared worse than the Research Council budgets in the U.K. since 1981, and research expenditures have been squeezed within that constricted university aggregate budget more than teaching costs: the students have to be taught and administered, and their number has been rising.

To a degree these constraints - absolute and relative to the claims of the scientific community (or the "needs of science" in the parlance of the claimer - were imposed by exogenous or objective circumstances, which owed little to the public status of science, the influence of scientists or political values. Given the growing capital-intensity of some research fields, increasing at exponential rates, the rising level of international expectations of what a 'well found' laboratory implied, the speed of advance of scientific potential and expectations, when translated against a slow growing economy of modest size, then by the late 1970's the party was clearly over. It was not a story confined to the U.K.[11] However, levels of expectations in the U.K. needed to be modified more than most, in scientific research terms no less than in power and defense terms, in a post-imperial age. We had to expunge the super-power image, or the American paradigm. Britain had, for the first time in science, to acquire the expectations of being a large small country rather than a small big country. That meant not trying to keep up at international levels in all fields (including the most expensive) or even that all universities - more particularly when there were 45 of them as there were by 1970 - should be trying to do all things, maintaining major research potential in all fields. And when attrition came to the aggregate U.G.C. and civil science budgets (in relation to demands) it also meant not implementing the requisite cuts by identical percentages across the board in all universities and all subjects, following a principle of fairness in adversity on the analogue of equal rations in the besieged city. All such expectations were recipes for spreading resources too thinly to be effective anywhere.

So rationalization, concentration of resources, selectivity, producing new institutional arrangements to capture the gains of interdisciplinary potential which was characterizing scientific advances in many fields (such as materials science, and the application of the new molecular biology in many branches of biotechnology) were the order of the day.[12] Again this is a much told tale in the western world. However, it is the responses to an exogenously imposed new situation which determine subsequent reality, not the exogenous circumstances alone. And here there are specificities to the U.K. which raise again great issues in the debate over the relations between scientific research and the wealth-creating process, which brings the argument back to the topic of this paper after a long excursus.

Science in a steady state, science with purchasing power declining in terms of the volume of science being funded, science certainly curtailed in funding relative to the claims of the scientific community meant that, for the first time, priorities had to be determined. If **anything** new was to be done some existing scientific commitments had to be foregone. According to what criteria were the newly scarce resources to be allocated? Allocative bodies previously added up the bills presented to them by the science community, rationalizing a little, imposing a queueing system perhaps, but essentially passing on the demands made from below without detailed assessment procedures about priorities beyond that of intrinsic scientific excellence and promise established by peer review. But now the allocative bodies had to become bodies through which priorities were set, or received from Government - the funding authority - and then passed from the centre to the periphery. Rationality and equity required justification for such decisions: some scientists in the Research Councils stood to lose their posts; while there would be fewer new jobs for young research scientists in some departments and institutes. Allocative bodies had to become investigative bodies: with progressive scarcity of funds at once came crowding in the demands for accountability , for demonstrating 'value for money', for post-research assessment, for the quantification of the quality of output as well as of its extent.

And what were the priorities? What was the justification for the claims being made by the scientific research community upon the Exchequer? The new political dispensation of 1979, with the arrival of Mrs. Thatcher as Prime Minister proclaiming a more specific political ideology than almost any of its predecessors (in fact as well as rhetoric) - in this case a new Tory radicalism - did change the context. To a large extent explicit political values dictated the assumptions within which the debate has been conducted, if only by necessity of convincing the paymasters, who demanded to be convinced on their own terms.

And what were those terms? The rhetoric, and the objectives of policy, were to turn round the economy, to increase rates of growth and national wealth after long decades of relative decline and low absolute rates of growth. Institutional change and attitudinal cultural change was required to inculcate the entrepreneurial society. The status of the public sector was to be diminished. State expenditures were to be reduced. The market would determine success or failure much more than in the past. 'Value for money' was to be the order of the day, with public accountability procedures enhanced and enforced to bring 'value for money' to the test of national utility - seen increasingly in wealth-creating terms; for which demonstrable utility would be required.

The public image of universities and their activities, which provided a measure of the political priorities which their funding required, suffered in this re-orientation of the reigning public philosophy. The voice of philistine anti- intellectualism, never absent in English cultural values, became more strident. The social sciences were widely supposed to be positively subversive (the previous Secretary of State for Education and Science tried to close down the Social Science Research Council but in the end was forced to settle for a change of name to the Economic and Social Research Council and more limited funding). The humanities and 'pure' science were cultural luxuries to be seen on similar terms as state-supported national opera.

This coincided with a wider disillusionment about the utility of traditional higher education and research. The high war-time status of science and its post-war after-glow had faded. The confidence of the 1950's and 1960's in the correlation between high investment in human capital (at least in the form of traditional university education) and high rates of growth, with the causal conclusions drawn from it, was widely under attack. And, in any case, which way round were the causal connections? The then Chairman of the E.S.R.C. argued resonantly in public, to the bewilderment of his fellow chairmen of other Research Councils, that high spending on basic research was a consequence of national wealth not its cause. Great investment in British science since the Second World War, it was said, had produced Nobel Prizes rather than new technology taken up by British Industry. A decoupling was evident between scientific research and the wealth-creating process. Look at Japan, went the argument, whose success owed little, at least up to the 1970's, to high levels of public investment in fundamental sciences: it was Thatcherism which was turning the British economy round not high levels of scientific research.

Suspicion and hostility were increasingly focussed upon concepts of 'academic science' or 'pure science', by which was meant fundamental science pursuing curiosity-orientated research not orientated towards potentially profitable new technology.

The developing strategy was twofold. In the first place cash-starved universities and Research Councils would be forced to search for industrial sponsorship, joint ventures (of which contract research offered the greatest opportunities) and exploit their own assets commercially. Steps were taken to encourage this through new regulations governing the ownership of property rights resulting from State-funded research and in other ways. Public provision for research was to be positively geared to success in gaining private funds. Secondly, new directions were to be imposed through the allocation of money via the U.G.C. and the Research Councils. No British administration has been so centralizing in the imposition of educational priorities as the present one, despite an official philosophy of getting the State off the back of the nation. Too much publicly funded research was remote, wasteful, unaccountable, particularly perhaps the estimated 700m.pounds p.a. from the U.G.C. going into the support of unspecified research in university departments, disappearing into the 'black hole' as it is now termed by the Department of Education and Science. The universities and the Research Councils had to be brought to account. The mold had to be broken; changes in motivation, objectives, values, priorities had to come.

You will readily see that assumptions about the relationships between research inputs and technology outputs are prominent in such policy prescriptions. As always the physician's remedy presupposes the diagnosis of the disease. I would argue that this simplistic assumption about the decoupling of advances in fundamental knowledge when not orientated towards a potentially utilitarian pay-off has added a dimension of significance in the curtailment and reorientation of British scientific research beyond that dictated by financial limitations. The level of naivete of assumption, based on a fundamental misunderstanding of scientific processes and encouraged by simplistic philistinism found expression in a rejection of investment in fundamental research with the argument that Britain could be a 'free rider'. Such research gets into the public domain; all can read the results in the journals and benefit without the investment. The backwoodsmen of the scientific establishment helped to polarize the positions by arguing in favor of the strict linearity of sequence from basic research to applied to development in their defense of basic research, and refusing to acknowledge the need for change over the rate of increase in budgets, structures, priorities, selectivity and concentration. Others lost confidence in the possibilities of university-based research in capital-intensive fields, seeing the future in national research institutes growing outside the university structure with a progressive divorce between research careers and university teaching careers. 'Max Planck' style institutes have become a paradigm for this view - cast in the role of the U.S. cavalry which comes to the rescue at the point when beleaguered university outposts have exhausted their meager resources.

Not all changes in response to constrained resources are destructive: crises create opportunities by breaking past molds. Where selectivity and concentration allow effective centres of excellence to be well funded there is gain; if the alternative is inadequate funding all round. In many fields, particularly those of exciting new science in such fields as materials science and bio-technology, the relationship between advances in fundamental knowledge and potential applicability of results is growing more intimate and the time-scale is shortening, so that opportunities for collaborative funding and the inter-penetration of industrially-funded research and academic research are being rapidly enhanced at the same time as the universities and Research Councils face financial pressures pushing them in this direction. New procedures need to establish safeguards for academic science, and doubtless conflicts of interest will occur, but the advantages of common ground beckon. The paths of advance of scientific knowledge, instrumentation and research procedures are breaking down the barriers between traditional sectorial disciplines in science, institutionalized in separate departments, so that new institutional structures evolving with concentration and selectivity, particularly the new Interdisciplinary Research Centres being promoted by ABRC and the Research Councils, offer major opportunities both for advances in research and potential exploitability. These will be university-based and the first is already in operation in Cambridge in super-conductivity, established at S.E.R.C. initiative. Up to 12 more per year are planned to 1991, from all the Research Councils, if funding allows.

It is ironic that the old, widely assumed dichotomy between pure and applied research is being abandoned in many fields just when it was becoming resurrected in ministerial philosophies. The fashionable new concept in this regard is "strategic" research: research aimed at advancing the frontiers of knowledge, without specific utilitarian objectives or predicted products and processes in view, but within research fields considered likely to yield material advantages at some future point - with time horizons up to 10 years or more. Not surprisingly the Research Councils and universities are discovering that most of their long-term research falls into this category. The new common criteria established for the public funding of research in the U.K. are both internal and external to the science involved: the internal being "timeliness, pervasiveness and excellence"; the external "exploitability, applicability, and significance for education and training." But all such potential benefits have to be weighed against cost.

The dangers of the present position in the U.K. for both science and the longer-term technological connections are manifold. The trend in other industrial countries is towards increasing total funding of research and development, increasing State funding and increasing the spend on longer-term fundamental research (with increased gearing between public and private funding). This is in recognition, I would judge, of rapidly developing new relationships between science and technology. Grave dangers therefore lie in contracting the present system (in the volume of purchasable scientific research) under prevailing priorities. The dangers are particularly acute for long-term academically motivated, free-ranging imaginative or "curiosity-driven" research. Research with long-term horizons will not be funded by industry on a large scale, biotechnology possibly apart, in areas which individual firms do not believe to be to their direct advantage. The direct and knock-on effects of government policies are likely to threaten the science base in this area in Britain without incremental funding. Impending proposals for the elaborate monitoring of research assessment procedures, and the pursuit of accountability over scientific output as well as the inputs of public money in pursuit of 'value for money' may produce unwieldy bureaucratic procedures, further adding to the entropy which always threatens large-scale dispensations of public money accountable to a government department, the Treasury, the Public Accounts Committee of the House of Commons and the Auditor General. With a 'hands-on' centralizing administration determined to impose priorities from the Cabinet table at No. 10 Downing Street - and often, it appears, from more private rooms in the same house - these propensities are enhanced. In such a context the scientific community as a whole (whether in academia, government service or industry), historians, philosophers and sociologists of science need to raise the level of debate, and hopefully the constructiveness of policy, by enhancing public awareness of reality. This embraces the complexities and subtleties, the necessary unpredictabilities, the continuing changes which characterize the relations between science and technology, research and development, the advance of knowledge and the pursuit of wealth and welfare.

Notes

1 See review of debate in Peter Mathias, The Transformation of England (London,1979) Chapters 3-4 ; A.E. Musson (ed.) Science, Technology and Economic Growth in the Eighteenth Century (London, 1972); P. Mathias (ed.) Science and Society 1600- 1900 (Cambridge, 1972).

2 A.E. Musson and Eric Robinson, Science and Technology in the Industrial Revolution (Manchester, 1969).

3 Corelli Barnett, The Audit of War (London, 1986), particularly chapters 11, 13-14.

4 Lyon Playfair, Literary Addresses (London and Glasgow, 1855) pp. 49-52. See also P. Alter, The Reluctant Patron (1987).

5 J.M. Sanderson, The Universities and British Industry, 1850-1970 (London, 1972); Michael Sanderson (ed.) The Universities in the Nineteenth Century (1975); Michael Sanderson, 'The Professor as Industrial Consultant: Oliver Arnold and the British Steel Industry, 1900-1914', Economic History Review vol. XXXI (4) 1978, pp. 585-600.

6 For one example see R. and K. MacLeod, 'War and Economic Development: government and the optical glass industry in Britain, 1914-18' in J.M. Winter (ed.) War and Economic Development (Cambridge, 1975), pp. 165-203.

7 D. de Solla Price, Little Science, Big Science... and Beyond (New York, 1963 and 1986).

8 For these international comparisons see Annual Review of Government Funded R&D (Cabinet Office, HMSO, London 1986). Also ABRC Science Policy Studies No.2: International Comparison of Government Funding of Academically Related Research (London, 1986).

9 ABRC has argued consistently for an expansion of the Civil Science budget, as well as for re-structuring. Its advice to the Secretary of State for Education and Science is regularly published. See ABRC, Science Budget: Allocation 1988-89 and Planning Figures 1989/90-1990-91 (DES, 11 December 1987). Most of the documents cited in the footnotes to this section contain detailed references to various official enquiries, evidence to Select Committees of the House of Lords and the House of Commons, statements of Government responses by the Department of Education and Science etc.

10 Ziman, Science in a Steady State; the Research System in Transition (Science Policy Support Group, c/o ABRC, December, 1987)

11 See, for example, National Science Foundation, Future Costs of Research: the next decade for Academe (NSF Policy Research and Analysis Report 87-1, Washington D.C. 1987).

12 The debate has been focussed by the ABRC published document A Strategy for the Science Base (HMSO London, 1987). The University Grants Committee has also been pursuing a policy of concentration and selectivity for university teaching as well as research.

INNOVATION CASE STUDIES

HAND AND MIND IN TIME MEASUREMENT: THE CONTRIBUTIONS OF ART AND SCIENCE*

D. S. Landes

I was dining some years ago at the Society of Fellows--a now venerable Harvard institution modeled largely on the Fellows of Trinity College, Cambridge--and had the good fortune to be seated next to a handsome, gray-haired gentleman by the name of Norman Ramsey. What do you do? I asked. I'm a physicist, he said. What kind of physics? I measure time. Imagine that, I said, it so happens that I'm writing a book on the history of time measurement.

Well it was a good dinner, and I learned a great deal about today's methods of high-frequency time measurement, of which more later. But the one sentence that made the most impression on me and that I have never forgotten was the remark: "Any stable frequency is a clock. The counting we can leave to the technicians."

The importance of this remark, to me at least, was twofold. The first was that it transformed my sense of the priorities. All the material I had been reading on antiquarian horology and the history of clockmaking focused on the escapement mechanism, that part of the clock which, among other things, counts the beats and thus ticks the passing of time. Now I came to understand (why hadn't I understood this sooner?) the primary significance in time measurement of the controller--the device that generates the frequency whose even rhythm tries to match the perfectly even units of ideal passing time.

The second thought was connected to the first: Why this traditional emphasis on the escapement? The answer was that the escapement had always been the chief sphere of interest of the inventive clockmaker, the focus of his creativity: make a better escapement, and one made a better clock. Now Ramsey seemed to be putting down the work of the "makers" as somehow of lesser importance. Anyone, he seemed to say, can work out the counting. And yet could one dismiss so easily the work of the these mechanician-technicians? It was they, the Dondis, the Burgis, the Tompions, the Grahams, the Harrisons, the Le Roys, the Breguets--who had always been the heroes of horological history. They made the machines and made them ever better. Should they now be reduced to mere agents or auxiliaries of men of science?

Ever since, the question has remained to trouble me. And it is with this in mind that I have thought today to review the history of time measurement over the last thousand years, from the sundials and water clocks of the first

* A shorter version of this essay was given as the Wilkins Lecture to the Royal Society in London on April 1988, and appeared in its **Notes and Records**, 43 (1989), 57-69.

Millennium to the atomic clocks of the late second, with reference to the respective contributions of art and science. I want to look for an answer by considering three major advances: (1) the invention of the mechanical clock; (2) the development of what we may call precision timekeeping or chronometry; and (3) the introduction of high-frequency timekeeping.

The Mechanical Clock (end 13th century)

The ancients knew two principal kinds of timekeeper: sun clocks (shadow sticks, dials, etc.) and water clocks. The mechanical clock, that is, a machine to track time by the movement of articulated parts, offered important advantages over these older techniques. Sun clocks obviously work only when the sun is shining, hence are useless at night and on cloudy days. Water clocks will work "around the clock," but their flow was extremely sensitive to temperature variations. Frost, of course, would stop them completely, but the temperature did not have to fall to freezing for viscosity to increase and the rate to change. Both techniques worked reasonably well in the Mediterranean and the Muslim lands of the Middle East; they were less reliable in Europe north of the Alps and Pyrenees. Consider in this regard not only the climate but the conditions of ancient and medieval housing. In a world without window glass, when openings were closed by hangings and the inside temperature was often not very different from the outside, water clocks were liable to serious diurnal and seasonal variations.

By way of contrast, the mechanical clock ran 24 hours out of 24 and was relatively indifferent to changes in temperature. (I say "relatively," because its rate was in fact affected; but given the more important sources of error in early mechanical clocks, variance due to temperature change could be ignored. Only much later, when greater sources of error had been eliminated, did clockmakers turn to the problem of temperature and find ways of compensation.) These two advantages proved decisive, so much so that the mechanical clock, within a century of its appearance and in spite of its own shortcomings, simply swept the earlier devices aside.

It was not these practical gains, however, that constituted the heart of the matter. The revolutionary character of the mechanical clock lay far more in its potential than in its immediate achievement; specifically in two things: in the introduction of the digital principle; and in the potential for miniaturization and portability. The first, the digital principle, seemed to countervene the logic of timekeeping: time, thought the ancients, being continuous, should be tracked and measured by something else continuous. Yet as we have abundantly learned, no phenomenon in nature or in art can be so regular, so even, as a repeating one. To be sure, the inventor(s) of the mechanical clock had no sense of this principle, much less of its eventual ramification. But that is often the way of discovery and innovation: the ultimate significance is for later.

The second, the potential for portability, was inherent in a device whose function could be made independent of position-- not at first, when clocks were weight-driven, but later, with the introduction of spring drive. The effect was to make possible, via the watch, continual access to temporal information and the development of what we would call a modern society capable of the most complex interactions by autonomous individuals attentive to time and temporal obligations.

We do not know who invented the mechanical clock or where. It seems to have appeared in England toward the end of the 13th century, and probably in such countries as Italy at about the same time. In spite of some speculation that it may have been inspired by the example of Chinese astronomical water clocks, it was almost surely a European invention, and it remained a European monopoly for some four hundred years.

Within this context, there remains a difference of opinion about the sources of inspiration, whether "scientific" or technical. (I put scientific in quotes because the use of the word in this connection is somewhat anachronistic.) The controversy was launched by the late Derek de Solla Price, who argued that the first clocks were complex astronomical machines (Dondi and, even earlier, Richard of Wallingford) built to track the movements of sun, moon, and planets for purposes of prediction and instruction. In saying this, Price was explicitly countering an older tradition (du Cange, Mumford) that the clock was the outgrowth of a monastic concern with time and a time-ordered daily schedule (the horarium). Price was convinced that the Europeans of that day did not invent the clock because they wanted to know the time; on the contrary, having built a few of these astronomical machines (astraria and planetaria), they then realized that they also had a device for telling time: "The escapement, which originally gave perfection to the astronomical machine, was also found useful for telling time, and as social development led to an increased social awareness and importance of time reckoning, simplified versions of this part of the astronomical device were made and became widely used as mere time tellers." (Note in passing the focus on the escapement, as though that were the clock.) "The mechanical clock," he contended in a particularly vivid metaphor, "is nought but a fallen angel from the world of astronomy!"

Price's thesis, which attracted considerable attention and won the credence of numerous scholars, does not stand up to scrutiny, whether on logical or empirical grounds. For one thing, as Price himself noted, technological innovation does not ordinarily proceed from the complex and highly sophisticated to the simple and rudimentary; for another, the extraordinary astronomical machines of Wallingford and Dondi were not, as Price believed, the first clocks built. They come rather some two human generations after the first clocks, and their authors, who provided contemporaries with detailed descriptions of their machines (detailed enough to enable later generations to construct models thereof), dismissed the clock part of the mechanism as something too familiar to warrant description. Dondi is explicit: anyone who is not capable of making a "common clock" "by himself and without written instructions" should not attempt the rest.

There is, however, a "soft" version of the Price thesis that warrants consideration. If I understand John North correctly, he now suggests that the earliest mechanical clocks may have been the work of astronomers who wanted to know the time for purpose of celestial observations. That may well be; there is simply no way of knowing. These early clocks were most likely derivatives of earlier, weight-driven oscillating alarm mechanisms attached to clepsydras and used in monasteries to wake the monks for matins, and astronomers may well have conceived and taken the opportunity to use these same oscillating devices to keep time. On the other hand, there were certainly clockmakers around, in and out of the church, who were capable of the same conceptual leap; and there was a growing demand, again in and out of the church, for a more reliable timekeeper and a better time service. In particular, the growth of cities and increasing complexity and intensity of urban life made good public time indispensable, and the multiplication of discrepant signals, particularly in the urban environment, made reliability a pressing issue of social harmony.

One thing is clear: once the new device became known, it spread widely and gave rise to a new profession; and it was this profession of clockmakers (who often did other things as well) that produced over the next two hundred years the improvements that made possible the watch, that is, a clock small enough to be worn on the person. What is more, in spite of a continued emulation in complexity--the fascination with multifunctional mechanisms that can drive pageants of figures and trace all manner of celestial events continues throughout horological history -- the most important developments took place in the improvement of the clock (or watch) as "mere time teller." This was the heart the story, and the "astronomical" devices were an epiphenomenon--a tribute to craft ingenuity, a foretaste of automations to come--but a by-product of time measurement. It was the clock **qua** clock that remained (pardon the pun) the star performer. Indeed, reversing the Price sequence, it was the clock that made possible the new astronomy (Christoph Rothmann at Cassel, late 16th century), based as it was on star mapping that used time as one of the two spherical coordinates.

Pendulums and Hairsprings (late 17th century)

The application of the pendulum to timekeeping (Huygens, 1656-57) gave us for the first time an oscillating controller with its own natural frequency. (The verge-and-foliot mechanism of the early clocks oscillated at a frequency that was in large part a function of the driving force, with all that that implies for perturbation and irregularity). The result was a gain in accuracy of one or two orders of magnitude. From a variance of 15 to 30 minutes a day, one went to less than a minute.

This was science at work - Galileo and then Huygens -- but one should not assume that all that was needed was the idea. Although the scientists of that day were used to working with their hands and doing the job of technicians (one thinks especially of their bench work in optics and microscopy), clockmaking was an art that took considerable experience, from the preparation of the metal to cutting and fitting, and Huygens depended on the close collaboration of a number of Dutch and French makers (Salomon Coster in The Hague, Isaac Thuret in Paris) whose names have come down to us in his correspondence and on the instruments that have survived.

It was in the course of these practical realizations that Huygens quickly became aware (as Galileo was not, indeed could not be because he never built a pendulum clock--though he had imagined and designed one) that the oscillations of the circular pendulum are not isochronous and set himself the task of finding the source of error (what horologists know as circular error). By a combination of experiment and mathematical reasoning, he was able to show that a cycloidal path would yield isochronous swings (Huygens was prouder of this result than he was of the invention of the pendulum clock, and it was indeed more original with him); and he then spent some effort, again in collaboration with his clockmakers, in imposing such a path on the pendulum by means of restraining "bumpers" (the so-called cycloidal cheeks) on both sides of the swinging pendulum suspension cord. But this device offered little advantage, partly because it required the clock to be perfectly plumb and the cheeks symmetrical, even more because the cheeks introduced an additional source of friction and perturbation.

The answer was found in practice in the reduction of the arc of oscillation, by way of confining the swing as much as possible to that central portion where the circular arc deviates least from a cycloid. Huygens was aware of this possibility and thought to achieve such a result by the insertion of an additional wheel into the train (by gearing down), but such a solution entailed further perturbation (every mobile brings its own friction and irregularity). The answer was found by the clockmakers, who devised a new escapement with a relatively narrow arc, the so-called anchor escapement, which made possible the use of a long, slower (again less perturbation), seconds- beating pendulum.[2] This new kind of clock made possible a further gain in accuracy of about an order of magnitude, as variance fell to a few seconds a day.

These new long-pendulum clocks posed a problem to the guardians of public time. Public time was solar time, and clocks were typically corrected by the sun whenever possible, preferably at noon. This made sense when the clock was so inaccurate that frequent adjustment was a relatively insignificant source of perturbation. But now that one had clocks that ran reasonably accurately day in and day out, it was preferable to let them run rather than disturb them. Instead of resetting the clock every day, one simply read off the correct solar time from a table of equation of time, usually posted inside the door of the clock case. But that then posed another issue, namely, why use solar time at all? Why not use clock time, that is, mean solar time? It took about 150 years to resolve this issue. The English were the first to adapt to the new temporality and were increasingly relying on mean solar time from the mid-18th century; the French did not make the move to clock time until 1826.

In one respect, the invention of the pendulum clock was a great disappointment. It did not solve the longitude problem, that is, the problem of ascertaining longitude at sea, which was a major concern of maritime nations. This costly conundrum became one of the great intellectual challenges of the day and a major stimulus to horological investigation and experiment. The invention of the pendulum clock seemed to promise a solution, but it was not long before discouragement set in. The problem, of course, was that the pendulum behaves badly when moved about, and all the ingenuity of Huygens and others did not suffice to overcome this difficulty.

After some decades of disappointment, scientific inquiry moved away from the horological solution to a long-understood but previously impractical astronomical one. The principle was obvious: if one knew the time of a given celestial event at a place of known longitude and the time of the same event as observed at sea, the difference in hour could be converted into a difference in space. Simple enough, but the execution was another matter. The key instrument was the telescope (early 17th century), which lent itself easily enough to the necessary celestial observations--typically of Jupiter's moons--on land, or to put it better, on **terra firma.** But the telescope would not yield good sightings from a moving deck, and again all efforts, however ingenious (such as putting the observer in a sling), proved a failure. If only one had a larger, faster-moving target, one that lent itself to easier sighting--our moon, for example, instead of Jupiter's. The difficulty there was that the path of the moon proved extremely difficult to predict with the degree of precision required. Even Isaac Newton confessed his despair: it was the only problem, he said, that ever made his head ache.

In the meantime, horological technology was advancing on another front. Again it was Huygens, the scientist, who made the breakthrough with his invention of the balance spring, a coiled spring whose function was to regulate the oscillations of the balance and impart to it a regular frequency. (I shall omit the contribution of Hooke to this development and the controversy over priority.) As with the pendulum clock, this change in the character of the controller made possible an important gain in accuracy of about an order of magnitude and turned the watch into a far more reliable instrument of everyday use, with obvious consequences for social time. But it did not make portable timekeepers good enough to serve as chronometers for marine use, and after about two generations of still inadequate improvement, the scientists, Isaac Newton among them,were ready to give up on this line as well. "Astronomy," he wrote, "is not yet exact enough"... but it had been much improved "of late" (accumulation of data on lunar orbits), and further improvement would make it "exact enough for the sea: & this improvement must be made at land, not by Watchmakers or Teachers of Navigation or people that know not how to find the Longitude at land, but by the ablest Astronomers"[3].

In the end, however, it was the watchmakers who solved the problem, and in a way that permitted any reasonably intelligent sailor to find his position. Beginning with John Harrison, carpenter, clockmaker, and autodidact, rustic from Barrow-on-Humber, whose fourth marine clock met the standards of the great British longitude prize in 1762, a string of first-class craftsman in England and France (Pierre Le Roy, Francois Berthoud, John Arnold, Thomas Earnshaw) learned to make reasonably compact and easily reproducible machines that would keep time within a fraction of a second a day. These chronometers would thus provide over the course of a voyage the hour, minute, and near second of time at a place of known longitude, which could then be compared with time **in situ** and converted into a difference in space.

This effort to ascertain the longitude by means of clocks took some fifty or more years in the face of considerable skepticism and some hostility on the part of scientists, who were inclined to see in these wooden boxes an intellectually unsatisfying, nay even a disreputably magical solution to the longitude problem. The scientists (John Flamsteed, Edmund Halley, Pierre-Charles Lemonnier, Jacques Cassini, Alexis Clairaut, Leonhard Euler, Tobias Mayer and others) followed instead the path sketched out (though not conceived) by Newton. They continued to gather data on lunar orbits, worked out the mathematics, calculated and published lunar tables, and these, in combination with a good instrument of observation (octant or sextant), an accurate pocket watch, tables of logarithms, training in trigonometry, pen and lots of paper and about four hours of calculations would yield a result almost as good as that obtained with a good chronometer.

This astronomical technique reached the point of practical use at about the same time as the horological one, and it achieved a certain popularity in naval academies, on ships of the line, and in handbooks on navigation. It never, so far as I can see, achieved much currency in the merchant marine, for obvious reasons. The clock method, on the other hand, went from success to success as cost of manufacture fell and as miniaturization made it possible to produce deck watches almost as accurate as the boxed marine chronometers.

Here I should pause a moment to consider the implications of this apparent victory for applied science and engineering. My initial reaction, I confess, was to see it on the simplest level: the craftsmen were right; the scientists, even the greatest scientists, wrong. But an exchange of views with Dr. Yakov Ben-Haim of the Technion (Israel Institute of Technology) in Haifa, Israel, suggests another interpretation. Dr. Ben-Haim suggests a comparison between the "black box" of Harrison, the marine chronometer, and the "black box" of late 20th-century science, the computer. In both instances, a device is substituted for thinking and thought: the machine does the thinking for you. The gain may be significant, because the machine can do some things better and faster. But there is also a loss insofar as the machine deprives the scientist of, or dispenses him from, fruitful thinking and speculation:

"The temperament and interests of the scientist differ from those of the engineer, and at a certain stage of their collaboration their paths must diverge. From the viewpoint of the scientist, this occurs approximately at the point when the practical problem no longer supplies abundant and fruitful conceptual stimulation. The innovative development of our understanding of the physical world depends on the willingness and ability of the conceptualizer, the scientist, to extract himself from the plane of implementation. The divorce of science from technology, at the appropriate phase of their collaboration, is no less imperative than the need for fertilization of scientific thought by the exigencies of practical problems.[4]"

Meanwhile the high-quality, long-case pendulum clock was the object of continued attention and improvement by clockmakers. It was Huygens the scientist who made the breakthrough, but it was such craftsmen and artists as George Graham and John Ellicott in the 18th century, Siegmund Riefler at the end of 19th, and William Hamilton Shortt in the 20th who successively reduced sources of error by compensating for temperature change, holding air pressure constant, and separating the controller as much as possible (electricity helped) from perturbations in the wheel train. The result by the 1920s was an instrument with a variance of less than a millisecond a day, accurate enough to detect irregularities in the motion of what had always been the clock of clocks, that is, the turning earth. Over the course of some 250 years, these clocks opened new possibilities of inquiry and experiment in astronomy and geodesy, to say nothing of testing and improvement in all branches of horology.

It goes without saying that these craftsmen were not scientific innocents. Even in the 18th century, it was not possible to make significant gains in horological performance on a purely empiricist, trial-and-error basis. Harrison, for example, thought enough of the lectures on physics by Professor Nicholas Saunderson of Cambridge to copy a set of notes in extenso, diagrams and text; and Pierre Le Roy kept abreast of the latest scientific work via meetings and publications of the Academie des Sciences. (Berthoud, Arnold, and Earnshaw come closer to the older model of the pure technician, although all of them were indirectly influenced by scientific notions.)

By the time we come to Riefler, we are talking about engineers. Riefler attended the Technische Hochschule and the University of Munich (his doctorate (1897) was honorary). Shortt was a railway engineer. These were not scientists in the strict sense of the word (in the German sense), but they had been trained in scientific concepts and method and knew whom to ask about what.

High Frequency Timekeeping

Shortt's free-pendulum clock hardly dominated (1925--) before it was superseded, much to the regret of amateurs of mechanical horology. It was supplanted by clocks using a quartz-crystal controller vibrating tens of thousands and then hundreds of thousands, even millions of times a second. These employed a technology so new, so different from traditional horological mechanics, that it is not easy to speak of a clockmakers' contribution. On the other hand, the role of the technician (but a very different kind of technician) remained crucial.

Just a bit of history: the quartz clock was an unanticipated by-product of developments in radio engineering. The radio itself was made possible by a number of advances in physics, in such areas as wave phenomena, electricity, and piezo behavior of crystals. But as always, this knowledge had to be converted into applications by such people as Marconi; and these applications provided the basis for several industries--of which one manufacturing receiving sets for the ordinary consumer and the other producing broadcast emissions of an instructive but even more of an entertaining character for capture by these receiving sets. These two complementary branches reached the stage of commercial feasibility in the 1920's and gave rise to an energetic proliferation of broadcasters, pushing into an increasingly crowded and anarchic band of available radio frequencies.

At this point it became obvious that something had to be done to police the air waves and stabilize broadcast frequencies; and this in turn required the manufacture of better, more reliable crystals that would maintain their rate of vibration. So the burden now passed to the craftsmen, who learned to make better crystals and align them better in the resonator circuits. And then it became apparent that in so doing, they had created a new kind of clock. All that mattered now was to find some way to divide these high frequencies and bring them down to a rate that could be counted and displayed. The first such instrument was the quartz clock of Horton and W. A. Marrison (Bell Labs) in 1928, less than a decade after the first appearance of Shortt's masterpiece. Within another decade, quartz clocks were good and reliable enough to be introduced into the world's observatories; and further advances have ensued (higher frequencies, constant-temperature containers, closed resonance circuits, automatic correction, miniaturization) that have largely been the work of lab technicians and engineers collaborating with scientists. Today, laboratory-quality clocks of this type can keep to a variance of a hundredth of a millisecond a day, or one second in 275 years.

The next step was the development of molecular and atomic clocks operating at frequencies of billions of cycles a second (10^9 hertz). Here again we have the marriage of experimental science with application and engineering. The new horology drew on quantum theory, in particular the work of Max Planck and others who had shown that atoms absorbed and emitted energy only at their natural frequencies; and on a long history of experiments in bombardment, transmission, reception, and detection made possible by the invention of devices for generating microwaves (the magnetron, and then Russell and Sigurd Varian's klystron, 1939). As with the quartz resonator, there was an element of by-product serendipity. The effort to develop accurate microwave radar transmitters and receivers yielded a technology that could be used in the construction of high-frequency resonators that would serve as clocks. The first of these was Harold Lyons and company's ammonia clock (a quartz clock using the ammonia resonance to keep it from drifting) at the U.S. National Bureau of Standards in 1949, which ran at a variance of a second in eight months (1 part in 10 billion = 10^{-10}. Critics noted that the best quartz clocks kept better time--a second in three years. Not so, said Dr. Lyons, noting that each crystal vibrated at its own frequency, whereas atomic frequencies were intrinsically uniform. With improvement, the ammonia clock would reduce variance to one second every three hundred years.

But it was not the ammonia clock that turned the trick. The molecules collided with one another and the walls of the container-guide, and the frequencies of the microwaves varied with source and direction. The answer was beam technology using methods devised by I. I. Rabi in the 1930s. But where Rabi had used low-frequency atoms, horological accuracy required high frequency. On the advice of Polykarp Kusch, a colleague of Rabi's at Columbia University, Lyons chose the cesium atom, with a resonant frequency of 9 billion cycles per second. Within three years a team of physicists, engineers, and technicians (with Kusch as consultant; as Lyons put it, "we weren't beam people") designed a clock that ran to the accuracy predicted by Lyons: one second in 300 years. But it ran only a month or two, and financial support dwindled as interest shifted to the new field of microwave spectroscopy and the view took hold that the ammonia molecular clock (based on the microwave absorption by ammonia at its inversion frequency) was more promising. Leadership then passed to England, where Louis Essen and V. L. Parry, working in the National Physical Laboratory with much less funding, produced in 1955 the first laboratory cesium beam apparatus to be extensively used as a frequency standard (Ramsey 1972).

One year later, the first commercial cesium beam apparatus reached the market: this was the Atomichron, developed by Daly and Orenberg in collaboration with Prof. Zacharias of MIT and manufactured by the National Co. Note that development had been financed largely by the U.S. Army Signal Corps and the Office of Naval Research; and that it was the purchase of a large number of these machines that made possible the introduction of mass-production techniques and quality engineering, with concomitant gains in reliability and savings in cost that made the product a commercial success (Ramsey 1983).

It was the cesium beam resonator, which kept time in nanoseconds, that made possible a new definition of the second as the fundamental unit of time. Where before, the second had been defined as a fraction of a presumably uniform day, it was now clear that the day varied and any units based thereon with it. For scientific purposes, then, the second was defined in 1967, not as a part of something else, not as an aspect of some astronomic or celestial event, but as the sum of extremely rapid, presumably constant atomic oscillations: 9,192,631,770 vibrations of the cesium atom.

Subsequently other high-frequency oscillating systems have been suggested and used as timekeepers. Of these the most important is the hydrogen maser (maser is an acronym for microwave amplifier by stimulated emission of radiation), which goes back to work by Pound and others on nuclear spin systems with inverted populations as intrinsic amplifiers. Again the first suggestions of horological possibilities came from scientists (Townes, Weber, Basov and Prokhorov, all in 1953-55), followed almost immediately (1955) by a working device. There ensued a rush of activity by scientists and engineers in a number of countries, culminating in 1960 (only five years!) by the first atomic hydrogen maser (Goldenberg et al.); and not long thereafter by commercial versions for use especially in long base-line interferometry. Here too, final performance depends on the quality of the engineers and technicians. There are masers and masers, and it is not an accident that some of the best come from such teams as that of Vessot at the Smithsonian Center for Astrophysics at Harvard. (Stability is such that over an hour they vary by the equivalent of 1 second in over 50 million years.)

It goes without saying that instruments of such accuracy are not required for everyday life. They have become, however, one of the most powerful experimental tools of modern science. The accuracy of these high-frequency measurements and the precision of the discriminations they make possible are such that scientists will when possible convert other measures-- distance, voltage, what have you--into frequencies and calibrate. They are the work of scientists and a new breed of clockmaker, someone with training in science and engineering.

This new breed of clockmaker is not the artist-mechanician of the 17th and 18th centuries. It is not only that he works to the plans and designs of others; so did the collaborators of Huygens over three hundred years ago. But whereas a craftsman of the 17th century might have imagined himself to be the creator of his machine (as Thuret of the balance-spring watch), the lab technician or mechanic of the 20th could never deceive himself on that score. By the same token, a gifted inventor such as Harrison could surpass the scientists of his time in nerve, conception, tenacity, and achievement. No artist of our day is likely to do that. The new technology is empirically opaque: it takes science to imagine it, to understand it, to improve it.

Even so, the craftsman remains a crucial element in the process, the more so as the fineness of the task and the precision of the product make the smallest differences perceptible and turn molehills into mountains. That is the paradox: the finer the specifications, the less room there would appear to be for the human touch; on the other hand, the finer the standards, the greater the difference the human touch can make. As Gernot Winkler, time service specialist of the U.S. Naval Observatory, puts it regarding the cesium atomic clocks manufactured by Hewlett-Packard: change one man on the assembly line, and we see the result one year later. It is well known that some atomic clocks are better than others, some much better, to the point where the Bureau de l'Heure weights the different clocks reporting by the quality of their performance.

In short, horology is now a science, not an art; but the artists remain and there will always be room and need for them.

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Notes

1. With the striking exception of a new invention, the portable sundial, which served as the watch of the pre-watch era. The rapid proliferation of this new form of an old device is the best evidence of a new temporal sensibility, nourished by the spotty availability of clock time and the growing dependence on temporal information.

2. Here, as in a number of other horological areas, there is a claim for priority on behalf of Robert Hooke, scientist- mechanician of great ingenuity and imagination, but of suspicious, umbrageous, and secretive temperament. Like other scientists working on horological matters, Hooke was dependent on the collaboration of clock- and watchmakers, and it is hard to say where science left off and art began--as shown by Hooke's quarrel with the great watch maker Thomas Tompion, That "clownish churlish Dog" Hooke called him.

3. Newton to Burchett, October 1721, in A. R. Hall and Laura Tilling, eds., The Correspondence of Isaac Newton, vol. VII: 1718-1727 (Cambridge: Cambridge Univ. Press, 1977), pp. 172-73. Newton's lunar tables of 1713 had reduced the error of prediction from 10-15 to 5-6 minutes, which worked out to 2-½ to 3 degrees of longitude, too much for safety. In tropical waters (near the equator), each degree of longitude is over sixty miles.

4. Letter to me of 26 May 1988.

ROTARY-WING AIRCRAFT AND THE EVOLUTION OF U.S. ARMY RESEARCH AND DEVELOPMENT PRACTICES, 1919-1944

I.B. Holley, Jr.

In light of the recent painful experience of the Israeli government with the Lavi jet fighter, it might help to put that traumatic event in perspective by looking at the troubled road which marked the evolution of research and development policies and practices in the U.S. Army. I have chosen to do this by retracing the many frustrating steps which led to an effective rotary-wing aircraft for the Army air arm in the United States.[1]

During World War I the need for sustained observation of enemy activities led to a number of experiments using the rotary- wing principle, an idea going back at least as far as Leonardo daVinci.[2] One of the very few qualified aeronautical engineers in the United States, E.P. Warner, in 1920 undertook a thorough-going scientific investigation of the rotary wing principle for the National Advisory Committee for Aeronautics, NACA, then the official aeronautical research agency for the nation. He identified four fundamental problems: lift; unpowered descent in case of engine failure; control or stability; and translational or forward speed. Of these, he concluded, lift would be the least difficult to attain.[3]

Close on the heels of Warner's analysis, the Engineering Division of the U.S. Army Air Service[4] received an unsolicited proposal from an aeronautical engineer, Dr. George deBothezat, to construct a helicopter. The deBothezat scheme held considerable appeal for Engineering Division officials. To begin with, deBothezat appeared to have impeccable credentials. He had studied aeronautical engineering at Gottingen, Paris and Berlin and served as a professor and aircraft designer at the Petrograd Polytechnical Institute until driven out by the Red Revolution. Currently a lecturer at MIT, the Massachusetts Institute of Technology, he also held an appointment with the NACA. But credentials aside, deBothezat rather artfully appealed to the pride and vanity of the Engineering Division chief by stressing the sensation that would result from being first to produce a successful helicopter, a goal deBothezat visualized as "so easy to me that I can build it without any preliminary tests...with complete certitude that my helicopter will lift itself ... and be ... fully maneuverable and stable." All this, he promised, could be had for $45,000 or "somewhat cheaper."[5]

The wording of this proposal probably should have put the Engineering Division officials on guard, but it did not. They went ahead and wrote a contract, without competition, authorizing deBothezat to build a helicopter at McCook Field. He was to receive $5,000 for a statement of principles, but, curiously, the contract failed to specify that he had to submit engineering data to support his alleged principles. The designer would receive $800 a month for six months while constructing the aircraft and, upon a satisfactory demonstration flight he would receive $15,000 more.[6]

Nothing but trouble followed. Dr. deBothezat continually complained that the Air Service engineers were trying to steal his ideas and refused to submit the detailed design drawings required by his contract. Finally, after a year of frustration, the helicopter with its four six-bladed propellors was ready for a trial flight. It managed to climb about six feet but in less than two minutes the ill-designed craft collapsed. Reworking the faulty parts proved unavailing; a series of subsequent flights never lasted more than a few seconds before parts broke.[7] Dr. deBothezat stomped off in a rage, leaving no engineering records behind.The Engineering Division chief was by this time glad to be rid of him.[8]

The Air Service may have learned some important lessons from the deBothezat helicopter caper, but they came at a high price. The total direct costs came to $135,000 but there were indirect costs of $90,000 more, a large fraction of the total funds available for experimental engineering in the Air Service.[9] One conclusion the authorities reached was that they could not be involved in the actual construction of an aircraft and at the same time be objective and impartial judges of its performance. They also learned that a developmental project is largely nullified unless there are detailed engineering reports recording every step along the way.

Manifestly, in the early 1920's the fledgling Air Service and its Engineering Division had a great deal to learn about the complex challenges of developmental engineering. The predominant view was that the division should design military aircraft and then send its specifications and blueprints out to industry to build in quantity.[11] Experience with the deBothezat project, however, substantially strengthened the arguments of those officers who contended that the proper role for the service was to learn from the operational units what their tactical requirements were and then equate these with what was technically feasible in order to define a statement of performance characteristics. These could then be submitted to the aircraft manufacturers whose designers would compete to attain the best possible performance.[12] This was clearly a sound conception, for it would leave the Air Service free to define the performance sought and then to judge the results obtained, while eliciting the best creative energies of the industry as a whole.

Unfortunately, the Air Service was slow to adopt the proposed system. The Engineering Division had no established procedure for ascertaining the tactical requirements of the operating units. Moreover, the division engineers encountered great difficulty in trying to express performance characteristics in entirely objective, scientific terms. Too often, for example, they were reduced to specifying maneuverability in such vaque, subjective terms as "superior or equal to" that of existing planes.[13]

Subjective judgments, based on prejudice and mere opinion, often colored the decisions of the staff. When, for example, a University of Michigan engineering professor came in with an ingenious design for an articulated helicopter rotor blade, an idea eventually crucial to the success of helicopters, the division Chief Engineer rejected it curtly in view of its "questionable value to the Military service."[14] The inference seems to have been that since the deBothezat experiment was a failure, the helicopter principle had no military future. Indeed, the chief of the Division's Airplane Section had earlier flatly declared "any combination of aircraft and helicopter is impractical."[15] ironically, almost at this very moment, the U.S. Army military attache in Spain was reporting on the successful two-and-a-half mile flight of an autogiro developed by Juan de la Cierva, a military pilot and son of a former minister of war.[16]

The deBothezat fiasco was but one of the many factors which led Air Service leaders to recognize the need for an improved organization and more sophisticated procedures for research and development. And in 1926, after extensive public hearings, Congress authorized a major restructuring of the Army air units as a combat arm, the Air Corps. This legislation not only authorized an increase in strength but established competitive procurement procedures differentiating between experimental and production model aircraft. In addition, funds were provided to build a major developmental center at Wright Field, Dayton, Ohio, equipped with wind tunnels and other such facilities for testing aircraft designs. Henceforth the air arm would leave development to contractors, confining itself to setting the goals and testing the products for compliance.[17]

Although the intent of the new legislation was perfectly clear, effective execution would depend upon the formulation of detailed procedures, the soundness of which onlv trial and error could determine. Well aware that engineers at Wright Field could not define the "military characteristics" or performance specifications desired by the tactical units, this task was assigned to the Air Board composed of faculty members at the tactical school. Experience was to show that a single part-time board simply could not cope with the necessity of continuously updating the requirements for the full range of types, bombers, fighters, transports, etc. Moreover, since there was no Infantry representative on the board, the whole question of close air support of ground troops tended to be seriously skimped. It took five or six years of groping before reaching the decision to have a separate board for each aircraft type with the members selected from those with special competence with that type. And only after several years more were the Wright Field engineers allowed to be represented on each such board as a voting member.[18]

If procedures were slow to evolve to carry out the reforming legislation imposed politically by Congress, so too the attitudes held by many officers were resistant to change. For example, when the U.S. military attache in London reported on Cierva's autogiro, he dogmatically declared that it held "no promise at all in its present state of development" for military use. Happily, the Chief of the Wright Field Materiel Division traveling in France watched a demonstration of the autogiro and determined to secure one for testing. When he returned home he discovered that the Pitcairn Aircraft Corporation of Philadelphia, Pennsylvania, had already secured license rights to manufacture the Cierva in the United States.[19]

Pitcairn constructed an autogiro on Cierva principles and began to demonstrate it at air shows in various parts of the country. The autogiro had a conventional tractor propeller for forward movement, conventional empennage and stubby wings with ailerons. For vertical flight it depended upon four unpowered rotor blades freely revolving about a vertical shaft. A short forward run set the blades in motion to produce a near vertical lift. Air Corps engineers who watched the autogiro demonstrations revealed their mindset when they reported that while the craft could "almost hover," it was "decidedly inferior" to conventional observation aircraft in rate of climb, speed, and ceiling. This, of course, was the wrong comparison. The autogiro was not conceived as a substitute for observation aircraft but rather for the highly vulnerable tethered "sausage" type observation balloon. The probable source of this negative view seems evident in the curt dismissal of a rotary-wing proposal by a Wright Field engineer: "Autogiros and helicopters have previously been considered by the Air Corps and abandoned as unsatisfactory for the performance of any tactical or incidental tasks".[20] The legacy of prejudice engendered by the deBothezat fiasco died hard!

If various Wright Field engineers took a dim view of rotary wing aircraft, this was not the case with Field Artillery officers who attended demonstrations of the Pitcairn autogiro. They perceived at once the potential of an aircraft which could "almost hover" as a substitute for the captive balloon in adjusting artillery fire.[21] When the Chief of Infantry suggested that the slow speed of the autogiro would make it most useful in observation work, the Air Corps assembled a board to compile a report dismissing these uninformed suggestions. The autogiro, the Air Corps report concluded, far from being superior to the balloon, was not even a good substitute. And as for the autogiro's slow speed, in combat, the board concluded, it would be suicidal.[22]

The report dismissing the whole rotary-wing principle so emphatically was a clear indication that the Air Corps had not yet established well-grounded procedures for the objective analysis of the problems presented to it. Here opinion played for fact. The board offered no empirical evidence of actual tests to support its conclusions. For example, the assertion that slow-flying autogiros would be suicidally vulnerable, rested on no test employing a fighter aircraft armed with guncameras. Several years later when such a test was actually performed, it turned out that the rotary-wing aircraft, by its ability to make tight turns, was usually successful in evading a high speed attacker.[23]

Not long after the Air Corps had resoundingly rejected the rotary-wing idea, the President of the United States, impressed by the autogiro demonstrations staged by Pitcairn and others, expressed a desire to see the novel concept tried out by the Artillery, the Cavalry and the Infantry.[24] Reluctantly, the Air Corps Materiel Division engineers set about complying with this mandate by ordering autogiros for service tests. In doing so, however, they went out of their way to underscore the notion that the vulnerability of such craft would limit them to operations behind friendly lines. They did not add that this limitation applied even more so to the balloons which they still favored![25]

To comply with the presidential directive, the Air Corps issued contracts to two firms, Pitcairn and another Pennsylvania company, the Kellett Autogiro Corporation, both under Cierva licensing.[26] The Air Corps engineers were unenthusiastic; they realized that the combat arms had been attracted by the principle of a hovering aircraft, but as aeronautical engineers, they were well aware that the autogiro in its present state was far from perfected. Because it depended upon elevator surfaces and ailerons for control, it was decidedly unstable at low speeds. To be sure, the autogiro was undeveloped because the firms involved were under-financed and the Air Corps had not funded a research and development program on the rotary-wing principle since the deBothezat debacle. Now belatedly, they realized that they would be judged by the performance of the aircraft they procured. Both contractors were trying to develop autogiros which could dispense with conventional wings and ailerons by injecting a tilt to the rotor. The obvious drawback to this approach to control was that it imposed almost impossible stick forces on the arms of the pilot, a circumstance which indicated a great deal of development work remained to be done.[27]

When Kellett's experimental tilt rotor model crashed during trials, Material Division engineers realized that a thorough- going scientific research program on rotor behavior would be necessary. Unfortunately the Division had no specialized unit established for rotary-wing projects, and the Division chief opposed creating one, saying there was "no enthusiasm" for this type of aircraft in the tactical units of the Air Corps.[28] This betrayed the typical mindset in the Materiel Division, for it was not the Air Corps but the other combat arms of the Army which wanted to see the autogiro perfected. So the Kellett autogiro was sent to the National Advisory Committee for Aeronautics laboratories at Langley Field, Virginia, for scientific investigation. Since no vertical wind tunnel was available, the scientists there had to rig a trial using a conventional tunnel. Here on the first day the rotor blades disintegrated during the test.[29]

For want of a carefully planned long range Air Corps developmental program for rotary-wing aircraft, the two principal autogiro firms teetered toward insolvency.[30] Just about the time the Air Corps seemed to despair of achieving success with the rotary-wing principle, a report arrived from Germany indicating that Dr. Heinrich Focke, the famous designer for the Focke-Wulf aircraft factory in Bremen, had developed a successful helicopter after a long period of methodical scientific research. The Focke helicopter's performance, hovering, climbing to 8,000 feet, flying backward, and then cross country for some 80 miles, convinced the Chief of the Air Corps that the helicopter, with its powered[31] rotors, was the way to go.

When Congress, responding to industry lobbying, made $300,000 dollars available explicitly for rotary-wing aircraft, the Air Corps solicited proposals for an experimental model. This elicited four qualified bidders. These were Pitcairn, Kellett, the Vought-Sikorsky Aircraft Company of Connecticut, and the Platt-LePage Aircraft Company of Pennsylvania.[32] Although the competition allowed bidders to propose either the unpowered autogiro rotor or the powered helicopter rotor principle, it was obvious in the light of Dr. Focke's impressive performance that a helicopter bid would be favored. This narrowed the competition down to Platt-LePage and Sikorsky.

By this time the Material Division had substantially improved its evaluation procedures. First the bids were submitted to an engineering board which tabulated the data presented and arrived at a figure of merit for a review panel consisting of a representative from the Office, Chief of Air Corps, another from the Materiel Command, and a third from a tactical unit. In addition non-voting advisors were called in from the several using agencies such as the Department of Agriculture, the Post office, and the like. The review panel was to take account of the engineering evaluation, price, and the promised delivery date as well as performance.

The final award went to Platt-LePage; its bid at $150,000 was higher than Sikorsky's at $112,000, but the former had the advantage of the Focke-Wolf license and a half finished helicopter, while Sikorsky could only show an ingenious test rig to demonstrate his yet unproved design. In light of the subsequent history of the two firms, the Air Corps would have been well advised to probe the bids more deeply. Sikorsky asked only $112,000 for the helicopter but $117,275 for the experimental engineering data which was to accompany the bid. In short, Sikorsky knew from long experience that the physical artifact was easier to produce and less of a problem than the scientific and engineering data which lay behind it. On the other hand, Platt-LePage, with far less experience in serious research and development, asked only $25,000 for the experimental engineering data[33] which was to accompany the aircraft.

The failure of the Air Corps evaluation process to explore all the factors behind the proffered bids soon came to light. In contrast to the Vought-Sikorsky Aircraft Company, a division of United Aircraft Corporation, a large conglomerate capitalized at more than 60 million dollars, Platt-LePage had only $300,000 in working capital and, lacking a factory of its own, operated in rented space. Five months after the promised delivery date, the firm's helicopter finally made its first flight. It lasted only a few seconds, but this was long enough to reveal a serious lack of control. Months of reworking and further testing ate up the contractor's working capital. A succession of bail-out grants of funds allowed the contractor to continue experimentation, but five years later the firm still had not produced a satisfactory helicopter.[34]

Retrospective investigation revealed that Platt-LePage's Focke-Wolf license did not include Focke's design data or manufacturing know-how, precisely the elements which the evaluating panel had mistakenly regarded as significant factors in the firm's favor. Moreover, it turned out that the firm had only three qualified engineers on its staff, clearly an insufficient team to address the dozens of design problems which dogged the development of rotor blades, transmission systems and the like.[35] From painful experience air arm policy makers were learning that evaluation of bids for experimental projects must consider a contractor's financial resources, scientific competence, engineering talent, machine tool resources, inventory control, and the soundness of the firm's procedures for reporting unsatisfactory performance and the design fixes undertaken to remedy any such shortcomings. Above all, they learned that the costs of experimental development could never be satisfactorily covered by a single aircraft contract. What was needed was a policy of funding experimental work with a series of pay-as-you-go contracts for specific increments of design.

Meanwhile, Vought-Sikorsky, having lost the competition to Platt LePage, went right on developing its ingenious design, using company funds. Igor Sikorsky was blessed with remarkable talents as an aeronautical engineer. He realized at the outset that conventional control surfaces were not appropriate for a craft in which hovering was its principal virtue. He also realized that any design which relied upon tilting the angle of the vertical rotor shaft would induce excessive and unmanageable control stick forces for the pilot. His solution was to replace the conventional rudder with a small propeller at right angles to the fuselage which would give directional control as well as countering torque from the powered, three- bladed overhead rotor.[36]

When Sikorsky demonstrated his helicopter for a group of Air Corps observers at his Connecticut plant in the summer of 1940, they were frankly amazed at its performance. It was perfectly stable while hovering; it could land within its own rotor dimensions, or leap 50 feet in the air with a simple flick of the wrist on the controls. What is more, its simple welded steel tubular construction lent itself to fabrication by firms without aircraft experience. Despite these impressive qualities, the Wright Field authorities were not interested in Sikorsky's offer of an experimental helicopter for $50,000 without engineering data. They preferred to wait until the helicopter principle had been thoroughly tested by the Platt- LePage craft which they already had on contract.[37]

The Air Corps officials who resisted Sikorsky reckoned without the agencies most interested in employing a helicopter. These included not only the Army ground arms but representatives from Agriculture, the Post Office and others, who pointed out that Sikorsky had a flyable helicopter and Platt-LePage did not. So the Air Corps relented and contracted for a beefed up Sikorsky model, the XR-4, with a more powerful engine which doubled the craft's lifting capacity.[38]

By May of 1942 Sikorsky was ready to deliver the XR-4 helicopter, so, with a fine flair for showmanship he flew the craft the 780 miles from his Connecticut plant to Wright Field in Ohio where it was received by duly impressed Air Corps officials. The demonstrated proficiency of the XR-4 set up a clamor from potential users even before service tests could be conducted. The Surgeon General of the Army wanted helicopters for the evacuation of stretcher cases; OSS, the Office of Strategic Services, saw its utility in landing agents in enemy territory, and many others such as the Corps of Engineers, envisioning a flying crane to tote fuel piping into inaccessible terrain, urged immediate production.[39]

The sudden clamor from every quarter for immediate mass production of the XR-4 helicopter horrified the project officer at Wright Field who was monitoring the Sikorsky contract. Despite the spectacular performance of the helicopter, he knew that every novel design was certain to have bugs which could only be detected by extended service testing leading to modifications in design before investing funds in elaborate production tooling. Nevertheless, General Arnold, the commander of the Army Air Forces, yielded to the calls from every side, and urged immediate production of a somewhat more powerful model, the XR-5.

When air arm officers sought a contract for 250 Sikorsky XR-5 helicopters they ran into a snag. The Navy raised a number of objections, most of them addressing the untested status of the aircraft. The real reason for Navy opposition appears to have been the fear that a helicopter contract would slow down production on the existing Navy contracts for Vought-Sikorsky F4U fighters and TBU torpedo bombers. Eventually this turf contest with the Navy was resolved by having Sikorsky spin off a separate corporate organization for helicopter production under Air Force cognizance.[40] To insure that there would be no conflict with production on the Navy's carrier planes, Sikorsky agreed to license Nash-Kelvinator to undertake mass production of an improved model, the R-6A. Since Nash-Kelvinator was not an aircraft firm, there was no fear of encouraging a postwar competitor in the rotary-wing market. Sikorsky did, however, retain his helicopter research staff in the parent company to continue developing the type.[41]

What followed was a classic tug-of-war between the engineers, who sought to inject a steady stream of improvements in the helicopter, and the production staff who were primarily concerned with numbers, with turning out as many units as possible for deployment to the troops. Achieving a balance between these two contrary forces was to prove exceedingly difficult. The developmental engineers were all too conscious that many bugs remained to be overcome in the helicopter: engine-cooling, pylon vibration, and rotor hub control difficulties all needed to be corrected by scientific investigation of the principles involved as well as improved engineering design. And of course everyone clamored for greater weight-lifting capacity.[42]

From the point of view of the developmental engineers at Wright Field, mass production should wait until the more serious bugs could be eliminated and the ratio of maintenance hours to flying hours drastically reduced. For the manufacturer, however, the only way to pay for the experimental work needed to iron out the malfunctions and defects identified in a stream of Unsatisfactory Reports turned in by users, was to apply the earnings from production contracts. But such profits materialized only when quantity production led to actual deliveries. Once again it became obvious that the appropriate solution was to perfect a pay-as-you-go policy by which the government contracted with the manufacturer to develop engineering designs to solve specific problems. Even after the Army belatedly began to use such contracts, it proved to be painfully difficult to price them realistically and even more difficult to administer them for maximum effect.[43]

By the end of the war, Sikorsky and Nash-Kelvinator between them had turned out a grand total of 385 helicopters.[44] Despite their limitations, these were enough to precipitate the rotary-wing aircraft revolution which was to characterize the postwar decades down to the present. But that is another story.

For our purposes the question is: what did U.S. Army officials learn in the years between deBothezat and Sikorsky about the procedures and practices needed to procure effective military aircraft?

From the deBothezat fiasco in the 1920's they learned that there are too many pitfalls when the government itself attempts to build an experimental model to turn over to industry for mass production. By 1926, they had learned that the air arm should leave development to industry. The procurement function should begin by defining the requirements of the tactical units in terms of a set of performance characteristics. After inviting design proposals to meet the specified performance, the procurement organization must develop effective procedures for evaluating these proposals objectively. Finally, when the winning design has been reduced to practice with an experimental model, it must be tested to see that it complies with the specified performance requirements.

Several years of groping were to pass before the air arm learned to differentiate between engineering testing and tactical testing. Crucial to these processes was the composition of the boards of officers who assessed the results. High turnover in officer assignments impaired continuity and the level of experience brought to bear on the decision-making process. So too did the failure to develop detailed instructions as to evaluating procedures.

Difficulties in defining the line between fundamental and applied research also caused trouble. Throughout much of the between wars period the Army was content to dismiss fundamental or scientific research as the province of the National Advisory Committee for Aeronautics. But the problems associated with the development of the rotary-wing principle made it increasingly evident that a clear separation of fundamental from applied research on developmental engineering was not feasible. This in turn led to a whole new conception of contracting. Where the air arm had long been accustomed to buying an aircraft, a tangible article, which could be subjected to test, contracting for research meant buying information. And this meant devising contract specifications of a very different sort.

Because of the high turnover in personnel, all these steps which seem obvious in retrospect were slow to be perfected. For want of carefully drafted after-action reports by project engineers reflecting on the experience gained in monitoring their projects, faulty or less than effective procedures tended to be repeated over and over again. Under-staffed because of budgetary constraints during the years of peace, no one was available to study the procurement process as a whole.

What conclusions should we draw from this brief glimpse at one small facet of the U.S. Army's aircraft procurement practices? One observation above all others seems warranted: while officials were willing to expend significant sums of money in buying hardware, they were remarkably slow to appreciate the importance of investing appropriate resources in the effort necessary to perfect the procedures by which they procured weaponry. Is it too much to suggest that the processes by which we accommodate the interface between scientific research and technological applications to procure weaponry may be as important as the end items purchased? As such, they deserve the kind of study this symposium was contrived to provide.

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2 E. P. Warner, "The Problem of the Helicopter" NACA Technical Note No. 4, May 1920.

4 The U.S. Army Air Service Engineering Division, organized during World War I, was located at McCook Field, Dayton, Ohio.

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6 Capt. R. H. Fleet to Mr. Schnacke, attorney, 2 June, 1921, and Chief, Engineering Division to Contracting Officer, 3 June 1923, Wright Field AAG, 452.1 Helicopters.

7 Chief, Engineering Division to Chief, Air Service, 16 June, 1922; Asst. Chief, Information Division to Chief, Engineering Division, 16 January 1923, and 1st endorsement, Engineering Division to Chief, Air Service, 26 January, 1923; Acting chief, Engineering Division, to Chief, Air Service, 10 January, 1923, Wright Field, AAG, 1923, 452.1 Helicopter.

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14 Chief Engineer to Chief, Engineering Division, 14 October, 1924, Wright Field, AAG 1924, 452.1-11 Helicopter, Gerhardt.

15 Chief, Airplane Section, to Patent Section, 11 September, 1922, Wright Field, AAG 1920, 452.1-11 Invention, Zettle 1.

16 U.S. Army Military Attaché, Spain, report 2427, 3 March, 1923, and report SP 3195, 20 December, 1924, Wright Field Tech Data Library A10.22/1 and 2, Cierva.

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36 Memorandum Report Exp-M-50-430, 1 June, 1940, Wright Field, AAG, 1945, 451.1 Helicopters Sikorsky.

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I am indebted to my colleague Professor Alex Roland for constructive criticism of an initial draft of this paper.

INNOVATION DYNAMICS IN OECD COUNTRIES BY TECHNOMETRIC ANALYSIS

H. Grupp

1. Early Detection of the Technical Opportunities of Scientific Research

It is more difficult, both theoretically and in practice, to record the returns from research, development (R & D) and innovation than it is to cover expenditures incurred in such activities. The results of R & D as well as the market success of technically new products or processes cannot be measured in terms of the customary scientific understanding of "measuring" a variable. A way out of these difficulties is the use of indicators. They must be viewed as "representatives" for the actual variables and are not identical to R & D or innovation output or success. Major effectiveness can therefore only be achieved through qualitative interpretation and the synopsis of the largest possible number of different types of indicators for studying innovation dynamics.

The systematic distinction can be made in line with the chronological sequence of innovation processes between expenditure, throughput and returns (or output) indicators.[1] Consequently, the early detection of innovation processes best starts with R & D budget figures ("expenditure analyses", scientific literature statistics "bibliometrics", or patent statistics "throughput analyses"). But not every million spent on research yields corresponding results, and not every patent application, assuming a patent is even issued, is converted into a new product or new process.

In terms of market results of innovations, above all indicators for external trade involving research-intensive products are promising, scope for interpretation of the causes of success or failure remaining open. Also, the role of technical progress in the products traded and the production methods involved can at best be inferred indirectly from economic figures. By way of remedial action, a "technometric" concept, a unique concept to date at an international level, has been developed and applied since the end of 1984 as part of the activities pursued by the Fraunhofer Institute for Systems and Innovation Research. This concept permits systematic international comparison between specifications covering new products and processes. Technometric indicators are established in direct relation to the technological trade figures as well as patent statistics. Thus, it is possible to detect lines of development and innovation dynamics from the early stages and to convert them into a secured technological trend, albeit not totally without contradictions. This paper reports briefly on the current state of this work.[2]

2. Technological Competitiveness

For some time different - and at times contradictory - lists and definitions have been used to describe the international competition situation surrounding research-intensive and other goods. Without entering into detail (see[3]), the so-called NIW lists (drawn up by Harald Legler at the Lower Saxony Institute for Economic Research, Hanover, West Germany) would appear to be best suited to accurately describing technology trade. A particular advantage is the fact that these lists divide the research-intensive sector into two, differentiating between leading-edge products and products forming part of high-level consumer technology.

In product groups in leading-edge technology, by definition, the level of R&D expenditure exceeds 8% of turnover, amounting to 3 to 8% of turnover in high-level consumer technology. Product groups with research expenditure of below 3% are designated as miscellaneous consumer technology. The definition may also be viewed conversely: a high percentage of expenditure on R & D signifies low turnover expectations. Indeed, for every million spent on research in leading-edge technology, the average turnover is less than 12 million, while the typical figure for high-level consumer technology is 30 million and more for each million invested in research. Leading-edge technology includes sectors subject to protectionism such as aviation and aerospace, pharmaceuticals and electronics, in addition to office machines, electronic data processing and scientific instruments. The category of high level consumer technology includes electrical and chemical products, domestic industrial goods like cameras, and in particular motor vehicles. Miscellaneous consumer technologies cover non-ferrous metals, plastics and rubber goods, food, drink and tobacco, leather goods, textiles etc.[3]

Figure 1 clearly shows that the Federal Republic of Germany does not figure among those countries that concentrate their exports on selected leading-edge technology. Instead, the West German economy offers a wide and varied range of goods belonging to high-level consumer technology and human-capital-intensive goods (human-capital-intensive refers to goods, the manufacture of which involves significant contributions by scientists, engineers, office personnel and managerial activities: synthetic textile fibers and dyes, optical goods, hand and machine tools, pipes and fittings etc.).

The indicator used in the illustration is the so-called "RCA indicator" which measures the relative export surplus over the average of all manufactured industrial goods. Positive values indicate an even greater export surplus than the average of all industrial goods (average FRG exports in 1985 were 1.7 times higher than imports!). Negative values down to approximately -50 point to a below-average market result at international level, though exports slightly exceed imports. Negative values of -60 and below point to an import surplus.[3]

As Figure 1 shows, the trend in recent years has seen a moderate worsening in the worldwide market position in high-level consumer technologies. There has been a general process of catching up by many smaller national economies, but no fall-off in competition particularly with respect to Japan (as could have been assumed, but in bilateral terms the indicator does not differ between 1980 and 1985). The strengths of West German exports continue to be found in automotive and mechanical engineering, precision mechanics, optics and metalworking.

The competitive situation of human-capital-intensive goods on the international market is still proving to be above average, albeit with a clearly downward trend. The somewhat below-average position of energy intensive goods and slightly negative trend (at a low level!) of environment intensive goods are not causes for alarm for the Federal Republic of Germany with its dense population and shortage of raw materials. The improvement in the competitive position of goods based on raw agricultural materials is due to pricing in the European Community and undoubtedly fails to represent a ray of hope for West German exports.

Of interest are the trend and situation relating to leading-edge technologies. In recent years German exports have enjoyed no marked position of strength. The United States continue to dominate this sector covering office machinery, computer equipment, aviation and aerospace projects, as well as nuclear fuels and fertile materials (see Figure 2). Although the Federal Republic of Germany was able to maintain its position in the international market in such leading-edge technologies, at least up until 1984, there has been a regional shift in structure: the competitive situation with respect to Japan has suffered, but at the same time it has been enhanced to roughly the same extent vis-a-vis its European neighbors.

In realistic terms clear specialization by the Federal Republic of Germany in the narrow and costly sector of leading-edge technologies can scarcely be anticipated. Instead, it should continue in the future to **maintain a strong position in the relatively broad sector of product groups with medium innovation potential** (synonymous with high turnover expectations). All-out efforts to achieve success in the narrow, at times reglemented, markets of leading-edge technologies proper would be expensive and altogether hazardous as far as the competitive position is concerned.

As Table 1 indicates, several countries of the European Community (EC). i.e. Belgium, Luxembourg, Greece, Spain, France, Italy, and the Netherlands, follow the specialization pattern of Japan and West Germany and are more active in high-level consumer goods and human-capital-intensive goods than in leading-edge technology. Great Britain, Ireland, and Denmark are stronger in leading-edge products - like the US. This is true at least in 1980. More recent figures are not available. An update is in preparation.

3. Measurement of Technical Performance Level: Technometrics

Consideration of economic indicators leaves the causes of success or failure open. The role played by technical progress cannot be read directly from market results alone. Consequently, quantitative procedures are required to describe the technical status of products and production processes. Appropriate indicators have frequently been put forward and often called for, but have only seldom been realized in practice. The new path pursued at the Fraunhofer Institute for Systems and Innovation Research with a view to calculating a suitable technical indicator ("technometrics") has since produced its first results. The methods employed in collecting technical variables will not be explained at this point (see[2,4]).

Approximately ten thousand technical specifications covering products and processes from the Federal Republic of Germany, the United States, Japan, and other countries have since been stored in the Institute's computer-aided technometrics cadastre. The various data have been derived from technical data sheets and exhibition literature, but above all as a result of a large number of personal discussions with experts in industrial development laboratories. More than two hundred such discussions have so far been conducted in these countries.

The technometrics cadastre includes detailed specifications on solar cells and modules, industrial robots, laser beam sources, sensors, immobilized biocatalysts and genetically produced therapeutic agents for humans. As an example, the technometric profile of semiconductor lasers is given in Figure 3, the aggregate indicators comparing various lasers are shown in Figure 4. Such diverse details of other technologies are not currently stored for cost and time reasons; however, more superficial information and a smaller number of specifications are available for another 43 products covering industrial technology. Thus, the technometrics cadastre permits not only detailed appraisals of the above selected technologies, but also representative statements on industrial technology as a whole.

If this technometric data bank is split into the same categories as the economic indicators for external trade, it is possible to clarify the question of

Fig.1: Specialization of West German external trade (RCA indicator) from 1978 to 1985 by category of goods

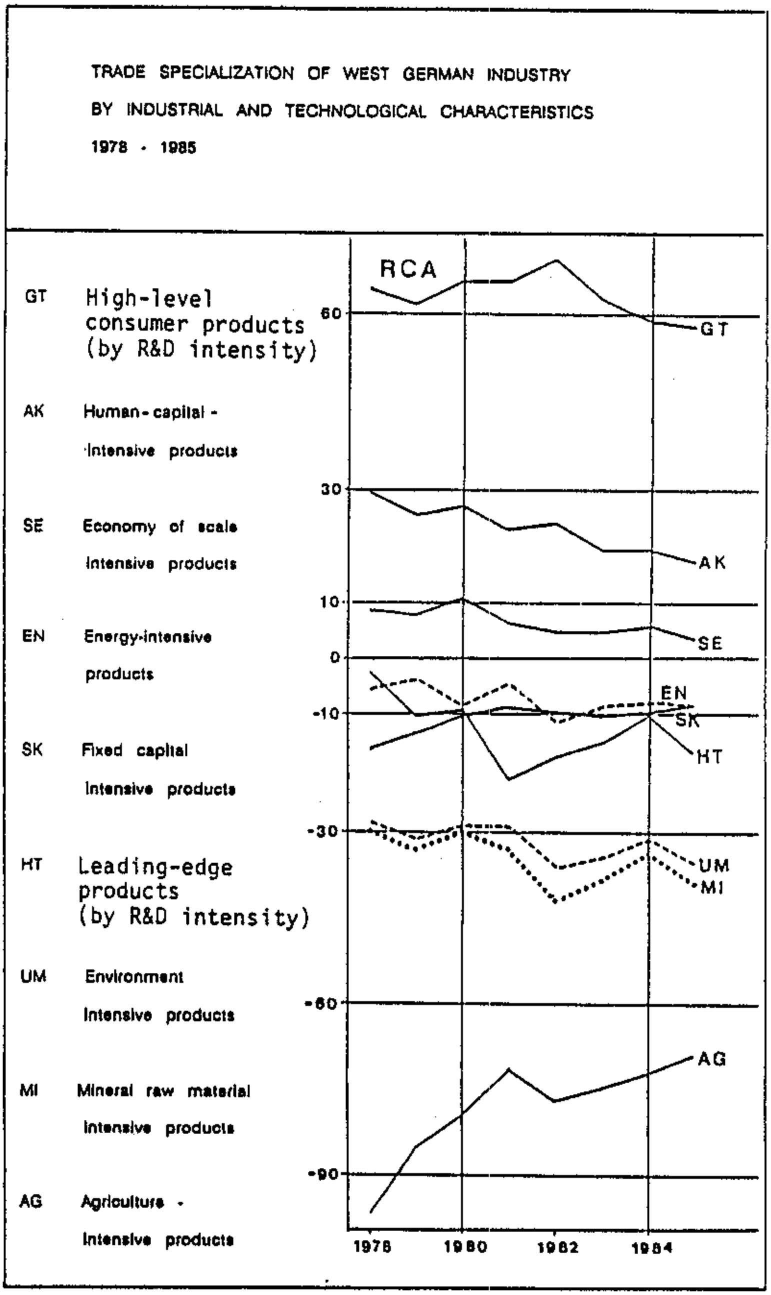


Fig.2: Specialization of the United States and Japan as compared to West Germany in external trade with R & D - intensive goods (RCA indicator) from 1978 to 1986

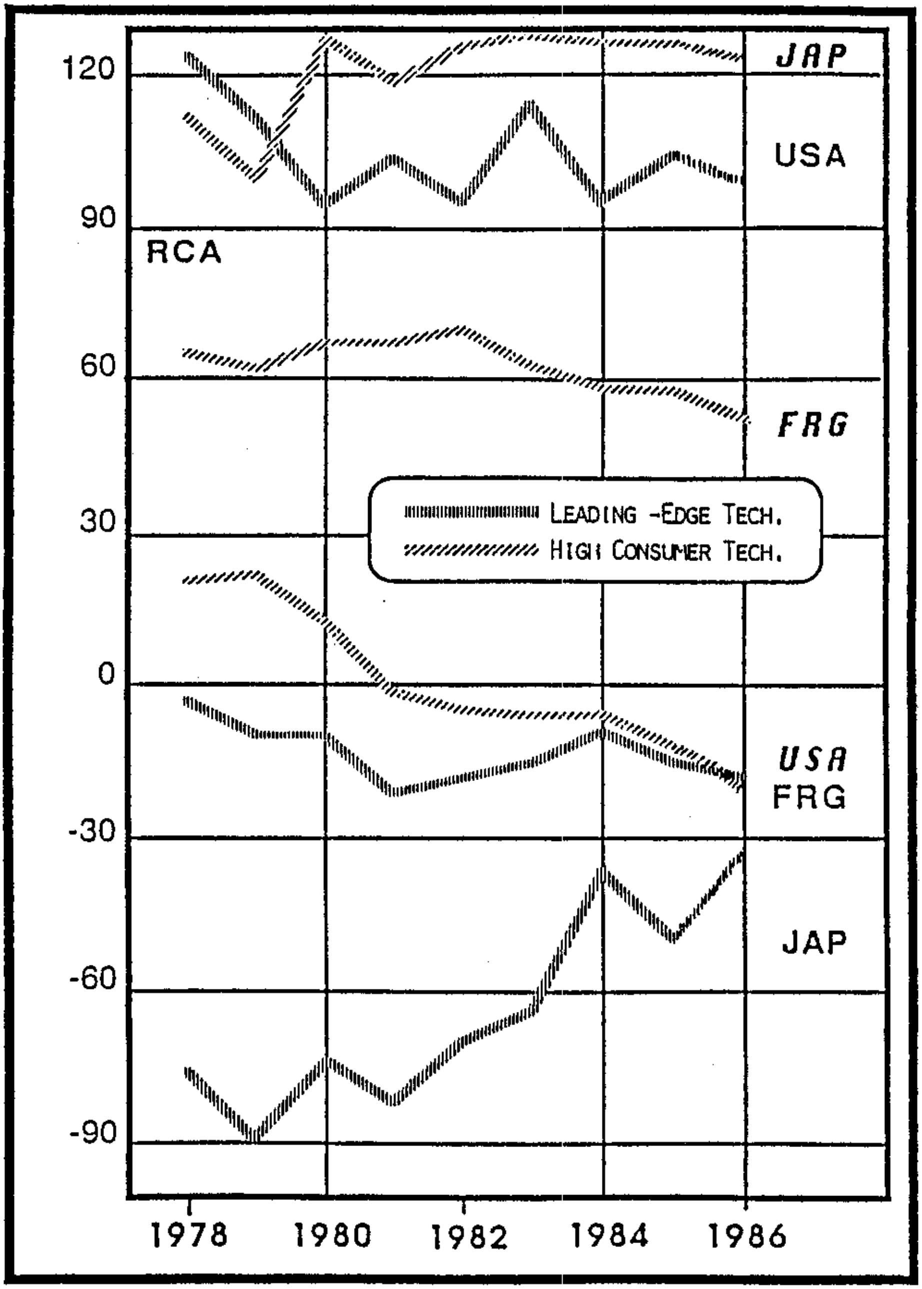


Table 1: Specialization of EC countries in external trade with R & D - intensive goods (RCA indicator) as of 1980

	RCA Indicator 1980		
	HT	GT	AK
Belg./Lux.	-26.2	+29.0	+21.8
Bundesrepublik Deutschland	-9.6	+63.9	+26.4
Danmark	+3.1	-12.4	-33.1
Eire	+48.4	-24.1	-33.4
ΕΛΛΑΣ	-118.8	-90.8	-50.0
España	-105.7	-8.9	-10.2
France	-10.3	+18.3	+8.8
Italia	-28.8	+0.3	-14.9
Nederland	-12.7	-4.3	+17.8
Portugal	-48.5	-103.0	-70.6
United Kingdom	+25.1	+15.4	+20.0

HT: Leading-edge products

GT: High-level consumer products

AK: Human-capital-intensive products

Fig.3: Technometric profile analysis for semiconductor diode lasers in the μm range for optical communication (as of 1986)

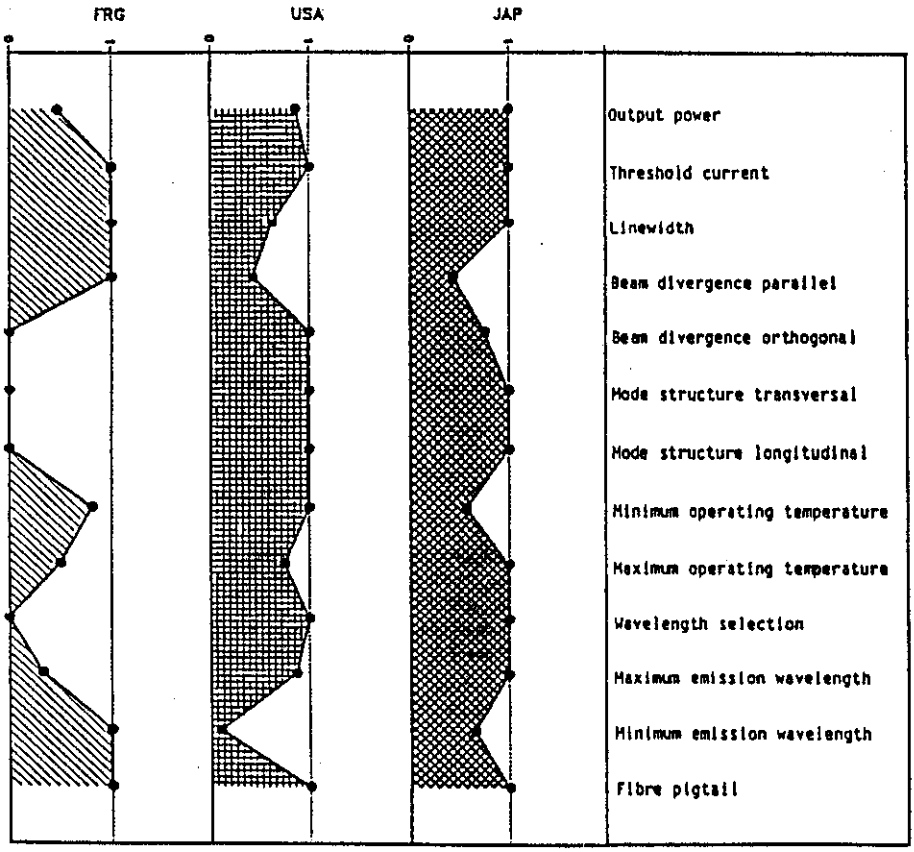
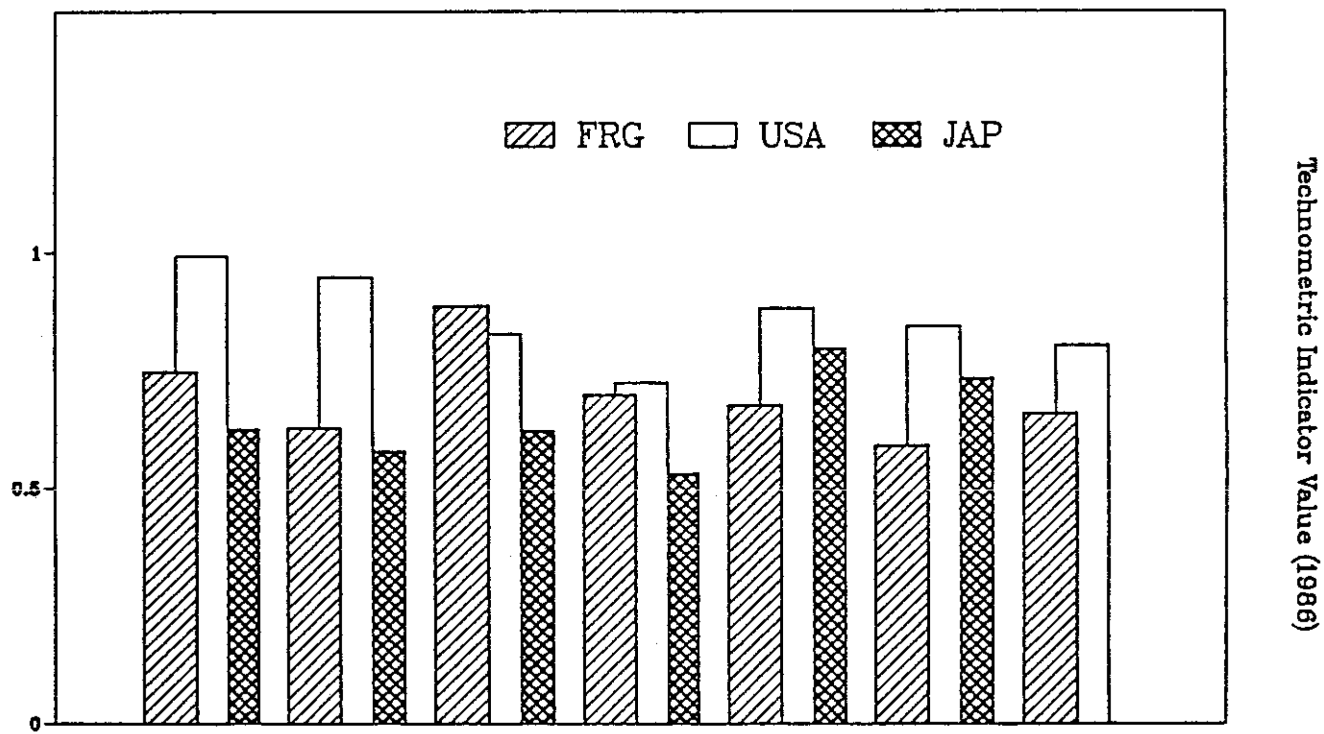


Fig.4: Technometric indicator values grouped by type of laser source (as of 1986)



whether the decisive factor in innovation dynamics is the technical standard of products or whether other factors play a significant role (e.g. management, market proximity, protectionist measures, rates of exchange etc.).

Fig. 5 shows the result. American technology and Japanese technology stand out in opposing directions: US technology dominates in the most research-intensive sectors of leading-edge technology, while the strengths of Japanese technology lie in product areas involving medium or low expenditure on research (such as video recorders. shipbuilding, steel or desulfurizing plants). Compared to these two countries, West German technical standards are balanced and high-level: West German standards are on average above Japanese standards in leading-edge technologies and above American standards in the other consumer technologies. Of all factors involved in innovation dynamics, **the "technology" factor,** and hence the returns of applied R & D, would appear to be **highly significant for external trade** since corresponding conditions are also obtained for the RCA indicator covering external trade (see Fig. 1). Further breakdown reveals that for individual sectors such as laser technology, West German (and European) companies have to date been able to defend their number 2 ranking in leading-edge technology, in spite of all prophecies of gloom (cf. Fig. 4). The reason, as in the United States, is a differentiated, traditional scientific and research system outside of industry, which is not (yet) to be found on a comparable scale in Japan.

Japanese companies continue to achieve amazing success in mass technologies. However, the limited resources, e.g. in terms of development personnel in Japanese research, are also giving rise to deficits: In new, particularly research-intensive sectors such as laser technology or genetics, American and European standards have not yet been substantially reached.[4]

4. Changes in Research Budgets

But the changes in research budgets point to existing deficits in Japanese R&D being systematically made good. The formula is "Technological Synergism". This is understood to mean the embarking on new development work in industry outside of the traditional centres of production concentrated on to date; thus, the objective is to set up new product lines by combining what were originally separate technologies.

Table 2 lists the industrial product areas where development expenditure has been increased by more than 50% within two years. Increased attention to development in companies in sometimes "foreign" technologies can be designated probably justifiably, as a development area with strategic interest.

Fig.5: Three-country comparison of rankings as between R & D intensity (FI), state of technical achievement (K*) and external trade (RCA), 1980 to 1983. FI is the product-specific industrial R & D expenditure divided by turnover in the product groups concerned, K* is the aggregate technometric indicator, the competitiveness indicator RCA is defined in the text (section 2)

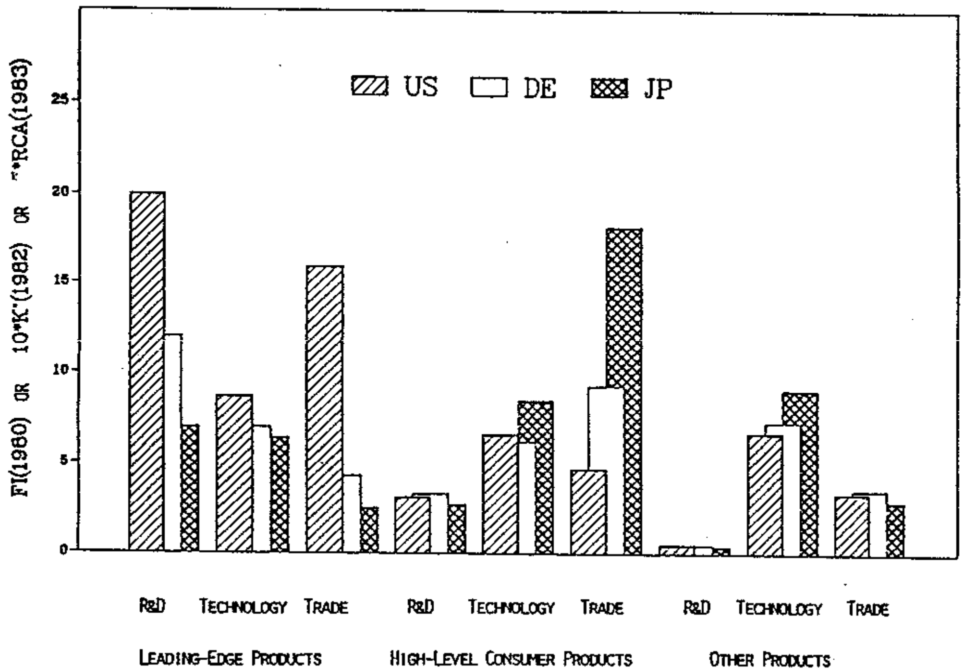


Table 2: Strategic R & D sectors in Japanese branches of economic activity (increase in 1985 over 1983)

R & D Performing Industrial Branch (Standard Industrial Classification for Japan)	Target Product Field of Intramural R & D (Japanese Classification)	Principle Product Field	R & D Expenditures (Billions Yen)	Increase in R & D Expenditures 1985 (1983 = 100)
Construction (091-119)	General Machinery	no	12	190
Printing and Publishing (191-199)	Printing and Publishing	yes	10	180
Industrial Chemicals and Chemical Fibres (201-204)	CE*)	no	9	240
Petroleum and Coal Products Manufacturing (211-219)	Non-Ferrous Metals	no	6	160
Ceramics (251-259)	CE*)	no	14	750
Iron and Steel Manufacturing (261-269)	Fabricated Metal Products	yes	10	210
General Machinery Manufacturing (291-299 and 331-339)	Other Electric Equipment	no	10	220
	Precision Instruments	no	27	490
Electrical Machinery, Equipment and Supplies (301-303 and 309)	CE*)	no	213	150
	Motor Vehicles	no	54	190
	Precision Instruments	no	9	190
Communication and Electronics Equipment (304-308)	CE*)	yes	660	160
Precision Instruments Manufacturing (321-327)	Household Electric Appliances	no	8	440
	CE*)	no	18	180
Other Manufacturing Including Plastic Products (136, 161-179, 221-229, 241-249, 341-349)	Chemical Fertilizers and Inorganic and Organic Chemical Products	yes	23	240

***) CE stands for "communications and electrical equipment"**

Table 2 only includes the largest development areas of this type in Japanese companies: the lower cut-off limit was set arbitrarily at 6000 million Yen, i.e. approximately 30 million US $ per annum.

A striking point of interest is the "communications and electrical equipment' product area in which the traditionally "responsible" sector invested substantially more R & D resources in 1985 than in 1983 (+160%, though the budget for research was already high). Other branches of the economy are increasingly involved in R & D: industrial chemicals (including fiber chemistry) +240%, ceramics +750%, electrical engineering +150% and precision instruments +180%.

The strengthening of R & D in other production areas is in keeping with the worldwide trend, which was already nicknamed "Mechatronics" at the end of the 1970's in Japan. This term is intended to illustrate the growing together particularly of the mechanical and electronic industries. Table 2 shows the extent to which "Technological Synergism" has since expanded. This also applies fundamentally to the Federal Republic of Germany. Since, however, West German research statistics are not as up-to-date and finely classified as Japanese statistics and since data protection requirements prevent a number of figures from being published, it must remain undecided whether Technological Synergism in the Federal Republic of Germany will come about with the same diversity as in Japan. However, increased strategic research by the mechanical engineering industry on electrical products, transport and communications and conversely by the electrical industry on mechanical engineering products can be supported by statistics[3]. On average it takes four years for the market returns for this research to make themselves felt[3]. Consequently, an additional indicator is required which heralds future economic power and offers additional insight into the mechanism of industrial innovation.

5. Patents as a Technological Indicator for Assessing Industrial Development

There are two faces to patent indicators: on the one hand development success is documented, on the other economic interest in certain future markets is indicated. Thus, it is critical that activities should be observed at more than one patent office. The results presented here are based on simultaneous searches at the American, German and European patent offices[5].

The importance of regional preferences particularly in the case of leading edge technologies is illustrated by the following examples: German companies are relatively more active at the American patent office than at the German or European office, for instance, in the areas of laser and enzyme technology. The American market for these technologies would appear to be so attractive that precautions are required in the form of patents. The same applies to

Japanese companies, which are also keen to obtain patent protection in the USA in many technologies. This is not the case, however, in the industrial robot sector. Both American and Japanese robot companies endeavors to lodge their technical innovations with the German or European patent office almost in equal relations; US companies show a definite interest in the German market in terms of enzyme patents[5].

In addition to paying attention to regional preferences on the part of the applicant companies, a well-contrived search strategy is required for patent analyses. Particularly with technically new developments, the conventional method of patent classification agreed at an international level is frequently no longer very accurate. This means that research based only on a suitable selection of patent classification symbols may disregard the new decisive trends. In the work of the FhG-ISI, therefore, a combined research strategy has been elaborated which, in addition to a well-considered selection of patent classification symbols, also takes into account key words laid down by experts. The combining of patent symbols and key words to form a sometimes complex logical composition commensurate with the subject matter makes it possible for a technical area under analysis to be covered as completely as possible. The search for key words is performed throughout the abstract as opposed to being limited to the title of the patent, which is, for legal reasons, often irrelevant in terms of technology.

The combined key word and patent class search strategy offers a chance to break down modern technologies finely. In Figures 6 and 7 the laser beam sources are divided into "big" and expensive sources (gas, liquid, and solid lasers; Figure 6) and "small" sources (semiconductor diode lasers; Figure 7). The patent positions and market strategies are clearly different for the two sets of laser patents. The US are still leading in the laser field with a steady decline since 1973, whereas Japanese and German inventors seem not to speed up their activities. The inventions are nearly equally targeting the US and the German market.

The laser diode field is now dominated by Japanese enterprises with a clear take-off since 1978. There is a strong targeting at the future US market with relatively little attention to the West German market. As patent applications are ahead of the market introduction by some three years[5], the economic effects may be witnessed during these days.

The combined search strategy is suited above all to ensuring early detection of the spread of a new technology to other technical and economic areas (diffusion of technology). This is illustrated in the following figure with reference to laser technology. It compares the patent specifications relating to this technology at the US Patent and Trademark Office insofar as the inventors

Fig.6: Relative Technological Performance (RTP indicator) of inventors in the field of laser beam sources except diode lasers compared at the US and the West German market (the RTP values are indexed, West Germany in 1973 is 1.0)

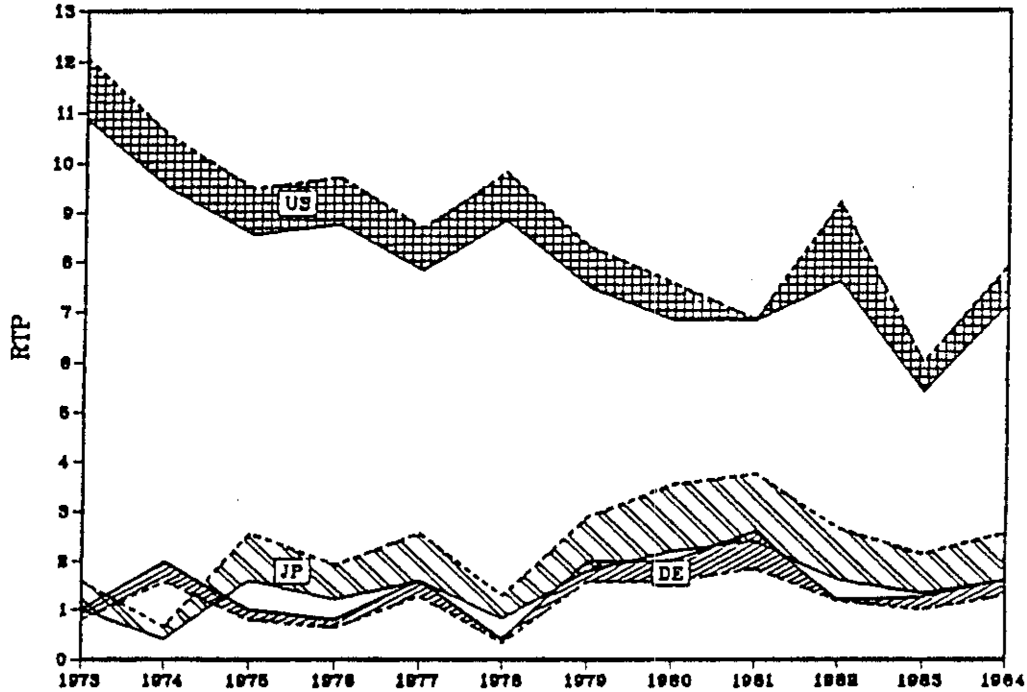


Fig.7: Relative Technological Performance (RTP indicator) of inventors in the field of diode lasers compared at the US and the West German market (the RTP values are indexed, West Germany in 1973 is 1.0)

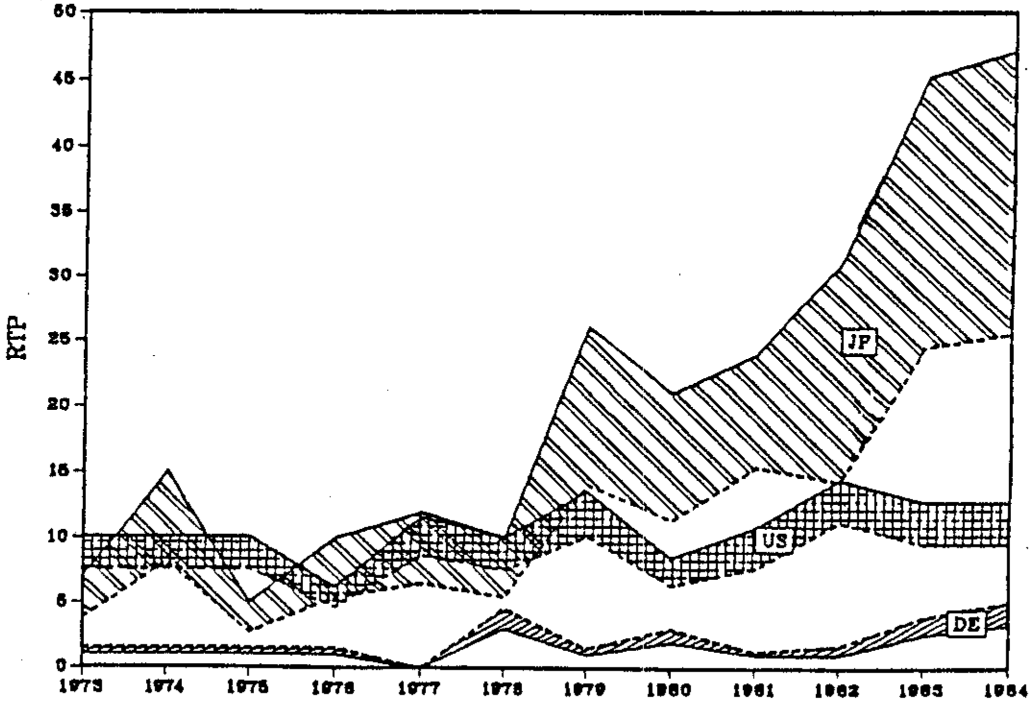


Fig.8: Patent class analysis for US and West German laser patents at the USA patent office (USPTO) covering 1975 to 1985 (USA on the left, West Germany on the right; different yardsticks used!)

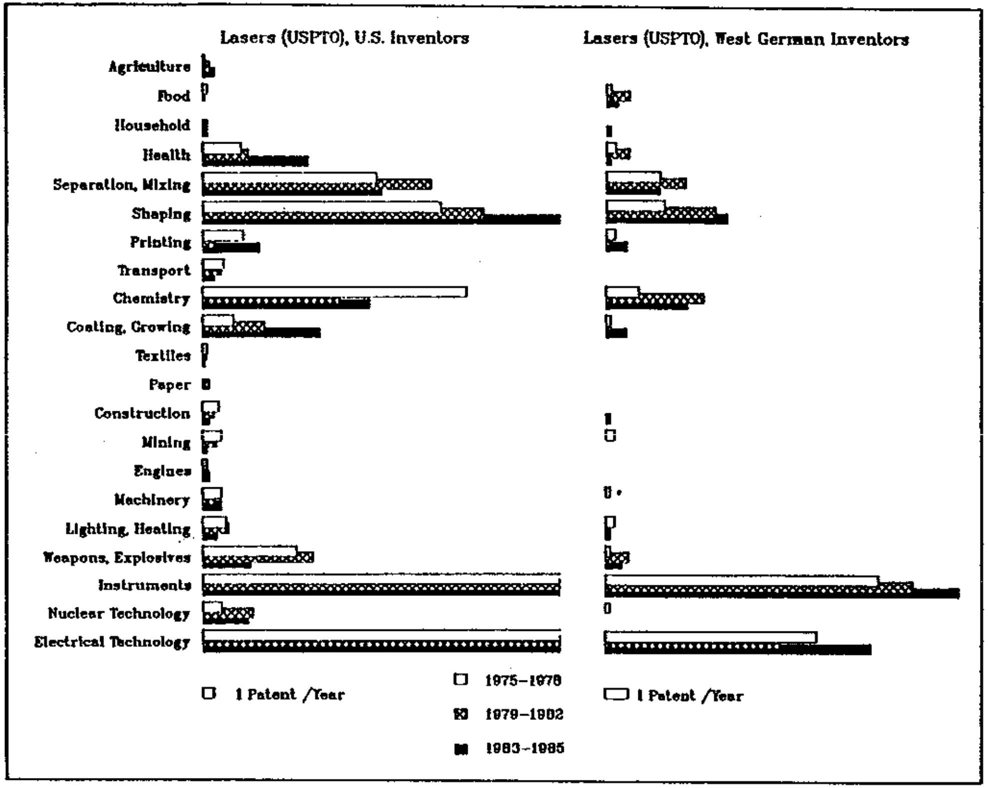


Table 3: Science intensity of West German technology in selected fields (share of citations to scientific literature in German patents 1981 to 1984)

Technology	Share of Literature Citations
Lasers	27 %
Industrial Robots	21 %
Solar Generators (Photovoltaics)	19 %
All fields of technology (average depends on discipline)	10-15 %

are domiciled in the Federal Republic of Germany or the United States. It can be seen that the central patents from both countries are filed under "(physical) instruments" or "electrical technology" (the bars in the illustration are so long that they had to be cut off on the right-hand side).

With a view to ensuring a finer breakdown, the analysis shown in Figure 8 can of course be further broken down. The patent data banks which have since come into existence in Europe and the Federal Republic of Germany permit relatively high-speed and accurate access to events, provided that patent law expertise and expert knowledge are available. If the preliminaries of the patent application prior to commercialization are considered, the courses of action adopted by competing companies and entire national economies can indeed be detected at an early stage. A list of companies that have filed appropriate patent applications can be readily specified for each of the bars shown in Figure 8. It is, thus, perfectly understandable that use can be made of patent analysis down to company level.

With a view to ensuring a finer breakdown, the analysis shown in Figure 8 can, of course, be further broken down. The patent data banks which have since come into existence in Europe and the Federal Republic of Germany permit relatively high-speed and accurate access to events, provided that patent law expertise and expert knowledge are available. If the preliminaries of the patent application prior to commercialization are considered, the courses of action adopted by competing companies and entire national economies can indeed be detected at an early stage. A list of companies that have filed appropriate patent applications can be readily specified for each of the bars shown in Figure 8. It is, thus, perfectly understandable that use can be made of patent analysis down to company level.

The science base of a company's (or country's) technology may be studied by compiling the citations in patents to the scientific literature. Table 3 indicates that science-intensive fields of technology do exist (i.e. lasers). Further breakdown may also reveal the institutional links and, thus, the efficiency of science and technology transfer.

6. New Patent Information Service for Small Firms

A pilot project promoted by the Federal Ministry for Economic Affairs of the Federal Republic of Germany has been embarked upon in 1987 in an attempt to render the above instruments and tools usable for small and medium-sized firms that do not have patent analysis capacity of their own. The idea, concept and scientific backup for the project are being contributed by the Fraunhofer

Institute for Systems and Innovation Research, while the practical side of the pilot phase has been entrusted to the Patent Interpretation Office of the Bavarian Institute for Trade and Industry (Landesgewerbeanstalt) in Nuremberg, West Germany[6]. Future expansion to cover other patent interpretation offices in the Federal Republic of Germany is planned.

Why have another patent information service? Virtually all patent research has so far been instigated for legal purposes since companies frequently attach great significance to preventing the possibility of their own innovations contravening or ruining foreign patents. Thus, patent research is in fact carried out too late as opposed to being foresighted.

The new Patent Information Service on the other hand makes it possible to carry out investigations at the very start of development work in order to obtain information as to the state of the art at the earliest possible stage and covering the broadest possible base and to avoid duplicated developments. The number of unnecessary individual development projects in practice is shown by the statistics of the German Patent Office: approximately one in every two patent applications is rejected because the supposed "inventions" were already known, either in their entirety or to a large extent.

Patent information cannot replace the customary sources of information for companies but can act as a valuable supplement. The reasons are as follows:

- The degree to which information from trade and technical journals and patents overlaps is very small, i.e. patents provide genuine additional information.

- The technical information provided by patents is highly up-to- date, almost always more so than exhibition literature.

- Whereas articles in science and technical journals are mostly limited to general theory, patents offer genuine technical approaches.

- Precisely specified technical facts can be located repidly and specifically by means of the patent classification system in addition with key words.

- Patents frequently record technical facts which should actually be kept secret from the competition but which have to be disclosed in order to secure patent rights. This type of information is not to be found in company literature.

Patent information at an early stage makes for economies in terms of development costs since many patents are only protected abroad or have already expired. In a number of instances licensing is far more cost- effective than in house development. Finally, a broad-based search for information provides a good overview of future competition at both home and abroad, which may be an important aid in decisions on risks.

Details are outlined in:

1 Grupp H, Hohmeyer O. and Schmoch U. (1987) Indikatoren zur Fruherkennung von Entwicklungslinien im Entwicklungs- und Innovationsprozess. FhG-Berichte 1:24-27

2 Grupp H (editor) (1987) Problems of measuring technological change. Zukunft der Technik series of publications. Verlag TUV Rheinland, Cologne (In English.)

3 Grupp H and Legler H (1987) Spitzentechnik, Gebrauchstechnik, Innovationspotential und Preise: Trends, Positionen und Spezialisierung der Westdeutschen Wirtschaft im Internationalen Wettbewerb. Verlag TUV Rheinland, Cologne

4 Grupp H, Hohmeyer 0, Kollert R and Legler H (1987) Technometrie - Die Bemessung des Technisch-Wirtschaftlichen Leistungsstandes. Verlag TUV Rheinland, Cologne

The abridged results can also be found in:
Grupp H and Hohmeyer 0 (1988) Technological Standards for Research Intensive Product Groups and International Competitiveness. In: van Raan A F J (editor) Handbook of Quantitative Studies of Science and Technology, Elsevier, Amsterdam.

The technometric model is outlined in:
Grupp H and Hohmeyer 0 (1986) A Technometric Model for the Assessment of Technological Standards and Their Application to Selected Technology-Intensive Products. In: Tech. Forecast. Soc. Change 30, 123-137

5 Schmoch U, Grupp H, Mannsbart W, Schwitalla B (1988) Technikprognosen mit Patentindikatoren. Verlag TUV Rheinland, Cologne.

6 Schmoch U, Grupp H, Schwitalla B (1988) Erschliessung der Informationsfunktion von Patenten und Gebrauchsmustern fur kleine und mittlere Unternehmen. Ongoing research project at FhG-ISI as commissioned by the Federal Ministry for Economic Affairs, Bonn, West Germany. Interim reports 1987, final report November 1988.

INNOVATION AND ENGINEERING DESIGN: MAX JAKOB AND HEAT TRANSFER AS A CASE STUDY

E.T. Layton Jr.

On a blustery day in March, 1937, a family of Jewish refugees from Hitler's Germany arrived in Chicago[1]. The head of the family was Max Jakob, a distinguished engineer and scientist in the area of heat transfer and thermodynamics. Jakob brought with him almost no money or material goods. Nor was he the kind of engineer Americans were familiar with: the practical inventors and innovators in the mold of Thomas Edison and Henry Ford. Ford and Edison had in common an American tendency to depreciate the value of theoretical and scientific knowledge. But it was precisely theory and science that Jakob had to offer.

It was more than enough; Jakob's gifts to America were to be of enormous value. They included the transplantation of the modern engineering science of heat transfer as it had been developed in Germany in the preceding generation. Heat transfer, along with other engineering sciences, had an enormous impact upon engineering, technological innovation, and economic growth. In absorbing more science, engineering was transformed in America as in the rest of the world. Engineering became more scientific and its theory gained a much greater generality and rigor. This more scientific engineering has been instrumental in producing innovations on the boundary between science and technology, particularly in the last 50 years.

Germany had been a leader in the incorporation of scientific methods and scientific research within the institutional infrastructure of engineering. Jakob was a product of this important tradition. He had earned an engineering doctorate in 1906 working at the Munich Technische Hochschule under Oscar Knoblauch, a professor of "technical physics". We have no exact equivalent in English for this title, but "applied physics" or "engineering physics" catch spirit. Knoblauch trained several engineering students who were to do much to found the modern engineering science of heat transfer. His students included Wilhelm Nusselt, Ernst Schmidt, and Jakob. Jakob took an engineering degree, but he made "technical physics" his field of specialization within engineering. He thought that he was the first engineer to earn an engineering degree with "technical physics" as its engineering specialization. He may well have been first. The degree suggests something of the nature of the scientific changes taking place in German and world engineering.

There is some misunderstanding of the term "engineering science"[2]. Since Jakob was an engineering scientist and his great contribution was to help develop the engineering science of heat transfer, and to transplant this science to the United States, a brief examination of "engineering science" is appropriate. The term is not universal, and there are many who prefer the term "applied science" or other names. But the term "engineering science" is widely used to refer to the scientific disciplines produced by systematic research in engineering. They are the sciences which minister most directly to the engineering profession, just as the medical sciences serve the medical profession. They are not at war with the basic sciences; the growing emphasis upon engineering science has been accompanied by a parallel emphasis upon the basic sciences in the engineering curriculum and the development of closer links between engineering and the basic sciences.

The engineering sciences have been recently characterized as:

> Subject matters dealing with engineering systems, to which the laws of natural sciences were applied, that were researched and analyzed by the scientific method, and were **classified and formulated into commonly shared disciplines**[3].

The same authors then indicate a principal change in engineering and engineering education produced by the scientific emphasis in modern engineering education generally and the engineering sciences in particular:

> They (the engineering sciences) do not deal with any specific system, but stress the fundamental principles applicable to conditions, systems, machines, and processes of special interest to engineers. Thus, the "movement toward fundamentals" in engineering consists of emphasizing both the **natural sciences** and the **engineering sciences** in engineering education ... the engineering faculty has accepted the challenge and responsibility of formulating and organizing the engineering sciences into teachable bodies of knowledge ... It is a monumental undertaking of great utility to industry and society. These engineering sciences form the core of the present day engineering curriculum[4].

Jakob was an engineering scientist with an engineering degree specializing in "technical physics". No doubt the juxtaposition of engineer and physicist to describe one and the same person can be confusing. Was Jakob a physicist or an engineer? The answer is that in some sense he was both. The sciences to which he devoted his life were the related disciplines of engineering thermodynamics and heat transfer. Our concern here will be limited to heat transfer. Heat transfer is typical of the new scientific disciplines which have been produced by systematic engineering research building upon foundations laid by physicists in the preceding century or more. Jakob was a physicist in his scientific methods and spirit, and in his insistence upon a very fundamental scientific approach to engineering. He was in the vanguard of those raising engineering to a higher level of sophistication in theory and experiment.

There can be no doubt, however, that Jakob's heart lay with engineering. Jakob's orientation toward engineering found expression in his active membership in engineering professional societies in Germany and America (the Verein fur Deutscher Ingenieure and later the American Society of Mechanical Engineers). He devoted his life to engineering research, publication, and teaching. Perhaps even more importantly, Jakob's research was driven by the needs of engineering application rather than the sort of abstract understanding sought by those involved in basic physics.

The direction of Jakob's research work was shown by the exceptional cases where he contributed to the discussion of issues of basic physics. When recovering from a wound received during the First World War, Jakob became interested in Einstein's theory, and he later defended it in professional publications. This led to some correspondence with Einstein. Jakob also corresponded briefly with Max Planck concerning an apparent error he had found in Planck's work[5]. Jakob, however, returned to his researches in engineering thermodynamics and heat transfer, and was not active in research and discussion of basic physics topics, such as those associated with Einstein's theory. Jakob's foray into basic physics was thus exceptional. A physicist might have asked why an able scientist such as Jakob would devote himself to dull subjects like the properties of saturated steam or calculation of problems in heat transfer. To the practitioner of one of the basic sciences these might appear to be dull subjects; the exciting physics research in heat transfer had been done in the Nineteenth Century by Fourier, Maxwell, Boltzmann, and other noted physicists. Indeed, Jakob was close enough to physics to have had some qualms. Jakob had some mixed feelings about his orientation toward engineering; like many engineering scientists he had absorbed many of the values of the physics community. But despite some qualms, his work focussed with increasing emphasis upon engineering issues, and particularly those associated with heat transfer[6].

Though the further development of heat transfer did not produce further dramatic advances in knowledge of the sort that led to Nobel Prizes, it held out enormous promise to technology. An engineer would not consider heat transfer dull. Its more fundamental understanding of a vast array of engineering systems had enormous implications for technology. It was this promise that lured engineers into painstaking and laborious researches into the science of heat transfer[6].

The results of heat transfer research led to designing or redesigning of an enormous number of important technological systems, both old and new, particularly in the generation following the end of the Second World War. Some of the new designs were improvements in existing systems such as steam boilers, heat exchangers, chemical reactors, heating and cooling systems of all sorts, and indeed, all thermal devices[7]. But there were other technologies, only dimly foreseen when Jakob arrived in America, which would require the sophisticated technological understanding provided by heat transfer and other engineering sciences. Jet propulsion and rocketry are two examples. Thus Jakob's choice of field indicated that in the final analysis he ranked the engineering value of "doing" higher than the physicist's value of "knowing".

Jakob, while well known in his own professional area, was not a world- famous celebrity. One might contrast Jakob with another Jewish engineering scientist who came to America from Germany in the 1930's, Theodore von Karman. In many ways the contrast between Jakob and von Karman was quite large. Von Karman was already world famous at the time of his arrival in America just prior to the rise of Hitler. Von Karman was not even a refugee in the strict sense. He was visiting America when Hitler came to power. He chose to accept a prestigious chair at the California Institute of Technology rather than return to a Germany dominated by Nazism[8].

Jakob's departure from Germany was much more painful. He kept a diary all his adult life, and his daughter Elizabeth who preserved this diary and some other papers, has recently written a deeply moving account of her father's life and work from which I have gathered most of my information about Jakob[9]. Jakob had a successful career as a researcher on the boundary between science and engineering. In 1910 he got a research position at the Physicalische-Technische Reichsanstalt (PTR) in Berlin. The PTR had been founded at the urging of the great scientist, Hermann von Helmholtz, to bring pure and applied science together for the benefit of industry. It was the model for the American Bureau of Standards, the British National Physical Laboratory, and other governmental agencies intended to foster technological innovations at the interface between science and technology[10].

The PTR was not alone: Germany had pioneered in a system of **Technische Hochschulen** (in English one would say "Institutes of Technology) which fed scientifically-trained engineers into the economic system and which fostered the development of the sciences most valuable to engineering[11].

Jakob became the head of a major research laboratory at the PTR and gained a certain measure of international recognition. He often represented the PTR at national and international professional meetings. Jakob also founded a journal **Forschung** (or "Research") , aimed at the engineering sciences. The nature of the journal is suggested by alternative titles discussed: "Archiv fur Mechanik and Thermodynamik" "Technische Mechanik und Thermodynamik" and "Forschung auf dem Gebiet des Ingenieurwesens" (The last is the current title of the journal.) Clearly, it was to be a journal for engineering science. This of journal reflected some dissatisfaction on the part of scientific engineers with the handling of scientific topics by the journal **(Zeitschrift)** of the leading German Society of engineers (the Verein fur Deutscher Ingenieure)[12].

Jakob's story nearly ended in disaster. After a successful career of more than twenty years at the PTR, Jakob's position started to deteriorate with the rise of Hitler. Initially Jakob received support from his colleagues in the PTR. This indicated his high standing and respect he had earned. But it only postponed the inevitable for two years. Jakob found that assistants were transferred to other laboratories, he was ousted as editor of the journal he had founded, he could no longer represent the PTR at professional meetings, and he faced mounting discrimination until 1935. In that year all government employees, including Jakob, with at least 25 percent "Jewish blood" were summarily dismissed.

It is a tribute to Jakob's high character that in his personal diary, he chose to record instances in which colleagues showed sympathy for his plight rather than the many slights and insults that he had to endure as a Jew in Nazi Germany. The number of supporters was perhaps disappointingly small. It became unwise to support or sympathize with a Jew in Nazi Germany. Thus, Jakob recorded "a very cordial" supportive letter from Ernst Schmidt (a fellow student of Knoblauch's). It may be significant that shortly after this Schmidt was selected to fill a coveted university chair at the Technical University of Dresden, but his appointment was vetoed by the local Nazi Gauleiter[13].

Jakob's move to America, though not without difficulties and painful adjustments, opened opportunities for useful service and professional achievement. Jakob's move can be seen as part of a process by which some of the finest fruits of European culture were brought to American in the 1930's through the emigration of Jews seeking to escape Hitler's persecutions. American science and engineering were particularly benefitted. The refugees from Hitlerism carried with them experience with the remarkable institutional mechanisms which German academic statesmen had developed for institutionalizing applied or engineering science in the era prior to the rise of the Nazis to power.

Theodore von Karman was another product of the institution building by German academics. At the University of Gottingen, for example, the great mathematician Felix Klein had sought to create a series of technical institutes to enable engineering to address modern, scientific technology. One of these was the Institute for Technical Physics[14]. In 1904 Ludwig Prandtl was appointed to head the new institute[15]. Von Karman soon became Prandtl's student at Gottingen.

In the same year as his appointment, Prandtl published a classic paper which lies at the foundation of modern aeronautics and fluid mechanics[16]. The physics of fluids had been developed by figures such as Daniel Bernoulli and Leonhard Euler at the expense of an idealization which neglected the messy, difficult phenomena associated with viscosity (which is somewhat analogous to friction for a fluid). The result was that the idealized mathematical science of hydrodynamics left out much of the phenomena of interest to engineers. Without viscosity there can be no turbulent flow. As Osborne Reynolds was to point out in the 1880's, turbulence arose out of the interplay of inertial and viscous forces. When viscous forces predominated the flow of a fluid such as air or water tends to be in smooth layers ("laminar flow"). But when inertial forces predominated, the laminar flow became unstable with eddying and finally turbulence[17]. Most fluid technologies involve turbulence (or measures to avoid turbulence). In practical terms turbulence meant loss of power in a water wheel or loss of lift for an airplane wing and other effects of great technological significance.

In his paper, Prandtl showed how unrealistic idealization could be eliminated, by the assumption that viscosity acted mainly in a thin boundary layer adjacent to an object around which the fluid flowed. This concept of the boundary layer and the analysis associated with it became fundamental to modern aerodynamics and to the closely-related science of fluid mechanics. Prandtl's paper also contributed to the science of heat transfer, since for convective heat transfer problems, the fluid flow problem must be solved as part of the heat transfer problem.

Prandtl's original degree had been in engineering mechanics, as had that of Theodore von Karman. Von Karman, who had been born in the old Austro-Hungarian empire, moved to Gottingen to work with Prandtl, and soon rivalled his old mentor by the brilliance of his discoveries.

Though primarily concerned with mechanics, aeronautics, and fluid mechanics, von Karman made important indirect contributions to the science of convective heat transfer as well[18]. Von Karman's direct involvement with heat transfer was not very great; he did some heat transfer consulting at General Electric. It left no permanent mark[19]. But the implications of his science were wide-ranging. His work in fluid mechanics had important applications to understanding convective heat transfer. The generality and the concern for fundamentals in the engineering sciences meant that work done in one area will find important applications in another quite different area[20]. Older engineering approaches concerned themselves with particular technological systems. Engineers had studied steam engines, or pumps, or other specific systems. This character of fundamental scientific knowledge in engineering was also illustrated by the fact that von Karman's greatest discovery, the Karman Vortex Sheet (a particular kind of turbulent flow) found application not only in aeronautics, his own immediate concern, but in many other areas. He showed, for example, that the failure of the Tacoma Narrows Bridge in America was a consequence of his sort of turbulence[21].

It would be harder to imagine a greater contrast than that between von Karman and Jakob. Von Karman was brilliant, Jakob was more of a plodder. Von Karman had a naive, even childish, ego which could be endearing[22]. In contrast Jakob was very modest, and his diary is filled with self-questioning and a profound moral sense. Jakob was a good family man, von Karman never married, but loved to be photographed in the company of Hollywood starlets. Hard working and able, fair minded and modest, Jakob was universally respected and liked. But he made no spectacular discoveries named after hi. Von Karman cultivated close ties to the military throughout his career; Jakob was increasingly opposed to the militarism of Germany long before the rise of Hitler[23].

Jakob was more fortunate than many of his fellow Jews. Though no celebrity, he was an internationally respected scientist who was well known in engineering circles. He made an American lecture tour and was offered two academic jobs. He accepted a joint appointment as Director of the Division of Heat Exchange and Research Professor at Illinois Institute of Technology and the Armour Research Foundation in Chicago, when he arrived without fanfare in 1937[24].

It often happens that engineering scientists do get involved in actual innovations. Von Karman in his memoirs linked his work with many specific innovations. Though he sometimes overstated his case, his contention is basically correct. He was, for example, one of the founders of the Aerojet-General Corporation, a pioneering rocket technology firm. There can be no doubt that in addition to his scientific work, he was associated with a number of important technological innovations.

Jakob was not associated with any major innovations. This is not the primary task of engineering scientists. Their lasting contributions derive from basic researches in engineering science or engineering design problems. That von Karman should feel it necessary to bolster his claims to fame by linking his fundamental researches to specific innovations is, in part, a commentary on the lack of public recognition and lack of prestige granted to engineering research.

Jakob was an engineer-scientist with talents in mathematical theory, experiment, writing, and teaching. Though Jakob did some industrial contact research at Illinois Institute of Technology, this represented more of a burden thrust upon him by American custom rather than his own inclination. He also had the satisfaction of playing a minor role in the defeat of Hitlerism after America entered the War. But his primary contribution lay in transplanting to America a very advanced and sophisticated engineering science, heat transfer, and in training young Americans in this demanding discipline in the heat transfer laboratory he founded at Illinois Institute of Technology. The students he trained there became, in turn, important figures in further establishing heat transfer at American engineering schools, as for example his first, S. Peter Kezios. In addition to his research and teaching, Jakob wrote a classic text book. The text was a monumental exercise, intended to preserve for America and the world the great achievements of German engineering work in heat transfer, since Jakob was convinced that German science had virtually committed suicide by its alliance with Hitler[25].

Jakob was, of course, not the only route by which the recent German work in heat transfer reached America. At the University of California L. M. K. Boetler and his students read and synthesized German heat transfer publications, and in 1932 a set of teaching notes became available[26]. Boetler's students became important figures in diffusing heat transfer in America. After the war E. G. R. Eckert, a student of Schmidt's, came to America, and worked at Wright Field and the Lewis Laboratory of NASA before accepting a professorship at the University of Minnesota, where he established a famous heat transfer laboratory. Eckert also wrote an influential heat transfer text book[27]. The German tradition in heat transfer interacted with a rather more empirical native tradition of considerable vigor, which found expression in an early text by W. H. McAdams, which was published in 1933[28]. It is scarcely surprising that Jakob was not the only channel by which modern heat transfer got to America. It is, however, a measure of his accomplishment and his importance in the establishment of this science in the United States that the most prestigious American award in the field of heat transfer is called the Max Jakob Award[29].

Jakob's failure to involve himself directly in the process of innovation is one of the things that makes him interesting. To understand his economic impact, we cannot, as might be done with von Karman, equate his value with the innovations with which he was directly involved. Indeed, Jakob's example demonstrates that such a method for evaluating an engineering scientist may be quite misleading. So Jakob presents a paradox and a challenge to students of economic growth. It is easy to show that the knowledge he transferred to America and the students he educated had a large impart on American technology an economic growth. But by existing methods Jakob's own contributions cannot be measured. To understand Max Jakob's contribution it will be useful first to examine studies of the sources of technological innovations.

Recently Robert M. Solow received the Nobel Prize for his pioneering work on the role of technology in economic growth. It used to be assumed that economic growth was a function of the increase in capital, labor, and materials inputs into the economic system. In 1957 Solow showed how one could construct an aggregate production function which allowed one to separate the influences of capital, labor, and material inputs from the influence of technology in economic growth[30]. What distinguished Solow's work, and no doubt contributed to the Nobel Prize, was his emphasis upon technology, which had long been neglected by economists. His function allowed one to measure, if somewhat crudely, the role of technology in economic growth. The results were startling. Solow found that the growth of technology accounted for over eighty percent of the increased per capita consumption in the United States between 1909 and 1949. Since 1957 a great deal of work has gone into the aggregate production function by Solow, Kenneth Arrow, and other economists[31].

The exact nature of technology's contribution has been a source of debate in the scholarly community for some time. One of the problems with Solow's aggregate production function was that technology was not measured directly. It was a residual. That is, it included everything left over when the inputs of land, labor, and capital were subtracted. There have been numerous efforts to reduce the figure 80 percent, which appears intuitively too high to many students of economic growth. (It should, however, be pointed out, that some of the simplifying assumptions necessary to construct the aggregate production function have the effect of reducing the percentage of economic growth allocated to technology.)

Certainly the direct impact of technology in producing innovations can be distinguished from other things, such as education, knowledge, and the institutional infrastructure. But it is important to bear in mind that the institutional and educational infrastructure contain some very important things essential for the healthy functioning of engineering and science. Max Jakob was both the product of a complex set of technological institutions in Germany, and he was one of those who helped to create and to transplant some of these institutions in America. American analogs of the PTR and the Technische Hochschulen had already appeared in America prior to Jakob's arrival (as, for example, in the founding of the United States Bureau of Standards and the Massachusetts Institute of Technology). But Jakob established an important engineering laboratory, both to do research and to train graduate students. He did not found another engineering research journal in America, but it may be significant that the **Journal of Heat Transfer** was founded through the efforts of Jakob's first American student, S. Peter Kezios. Kezios also edited the journal for many years. As noted, Jakob's heat transfer text was an attempt to codify the progress made in the previous generation in Germany. By implication, of course, it raised the question of institutional support for engineering sciences such as heat transfer. Clearly Jakob had an enormous impact and his students (and his students' students) carried the new engineering science forward, with enormous impact upon American economic and technological development. But the role of Jakob and other founders of heat transfer and the role of the institutions which fostered heat transfer research cannot be measured in economic terms as yet. For example, the value of basic engineering science still defies economic quantification. As something usually produced without thought of immediate gain, it does not fit the traditional "economic man" of classical economics. But we can say that the thing that distinguishes modern innovations from those of earlier centuries is that they (or at least many very important ones) are scientific in some sense, and that part of this science represents systematic engineering and scientific research.

It might help to sharpen the focus of the issue for pure research in engineering if we refer to categories used in innovation studies. Innovation studies such as Hindsight and TRACES set a pattern that has been followed by many innovation studies. That is they analyze specific innovations to isolate "events" critica to the innovation. These events are then classified to distinguish between science and technology, and between pure and applied research. In most cases the "events" were then linked, explicitly or implicitly, to present some sort mode, often simplistic, of technological innovation.

Our understanding of the sources of economic innovations has been enlarged by modern innovation studies[32]. But these studies are deeply flawed in some cases. A few innovation studies have been biased, reflecting the differing perspectives of various governmental agencies. This was true of Project Hindsight sponsored by the Department of Defense of the United States and the TRACES study sponsored by the same nation's National Science Foundation. The thrust of Hindsight was to minimize the role of "undirected" or basic research while TRACES emphasized the role of basic science in certain major innovations[33].

Before the spectacular rise of Japan as a technological superpower, Europeans were concerned with the apparent advantage of the United States in Innovations. A study by the OECD of 110 significant innovations, which had appeared since 1945, was intended to isolate gaps in European technology. The study concluded that America's margin did not lay in discoveries or research but in the speedy translation of these findings into innovations[34]. Britain has become a major center for innovation studies because of a deep concern over national economic stagnation.

Among the ablest of all of the innovation studies was a study of fifty successful British innovations by J. Langrish and others. They noted that virtually all innovation studies have assumed some sort of linear-sequential model of the innovative process. These models are of two types: "Discovery push" and "demand pull". Each of these in turn fall into two groups. "Discovery push" can come from either science or technology. "Demand pull" can come directly from the market, or from management's estimate of prospective demand. Categories of this type ("demand pull" and "technological push") appear also for example, in such standard and respected works as Edwin Mansfield and others, **The Production and Application of New Industrial Technology**[35].

Perhaps the most striking result of the researches of Langrish and co-workers was their finding that **none** of the fifty innovations that they studied fit any linear-sequential model[36]. Now technological innovations (the sort of innovations central to almost all innovation studies) are also the fruits of engineering design, and may be termed "designs" in this context. It is surprising that almost none of those making innovation studies have attempted to link their work with the extensive theoretical work done by engineers on the nature of engineering design. The non-existence of an orderly "innovations chain" came as no surprise to engineers. It is precisely what studies of engineering design would lead one to expect.

An engineer with experience with innovations has recently written a paper, "Innovation is Not a Linear Process"[37]. Attempts to model engineering design have led to flow charts (or "morphologies") of varying degrees of complexity[38]. None are linear sequences. While students of engineering design differ on the number of boxes in their flow charts, they agree that the process is not linear. A discovery or problem at one stage will cause the designers to go back to earlier stages of the design process. This is usually indicated by feedback loops connecting each stage of design with every other.

The recursive and iterative nature of engineering design has been treated by Morris Asimow and Thomas T. Woodson as a fundamental characteristic, perhaps the most fundamental characteristic of design. Thomas T. Woodson, in his **Introduction to Engineering Design** defined design as "an iterative decision-making activity to produce the plans by which resources are converted, preferably optimally, into systems or devices to meet human needs"[39]. The return to earlier stages in design would not occur under conditions of perfect knowledge. It is the need for new knowledge or the belated realization of criteria that need to be met, that gives design this character.

Many innovation studies have assumed an orderly progress from basic research to applied research to engineering design and production.

Much of the controversy in innovation studies has arisen because this chain cannot be documented in the general case. For example, one of the largest and best of the innovation studies done by the well-known economists Sumner Myers and D. G. Marquis founded that 95 percent of the successful innovations in the industries studied did not originate with research. Indeed, the great majority involved no systematic research at all. In those cases where systematic research was involved in most cases it came in implementing a technological ideal. It did not initiate the process of innovation[40].

Models of the engineering design process assume that both market pull and discovery push are always both present in significant innovations. One distinguished engineering critic of innovation studies concluded that attempts to distinguish between market pull and discovery push in producing innovations were "essentially irrelevant and should be dropped"[41]. It might be more helpful to see these as necessary and sufficient conditions of both successful design and successful innovation. There are no doubt many cases of routine design where there is little or no new knowledge involved. But even in very research-intensive innovations there is a lack of an adequate documentation for anything like a "knowledge push". There are other examples of attempted innovations which failed because the designers were unable to come up with the new knowledge needed for the innovation. Conversely, there are some cases where a new design or innovation fails for lack of a market. However, these represent cases where engineering managers have miscalculated; no responsible manager would commit funds for the design of a product which had no potential market or user[42].

However, there is a more fundamental objection to the idea of a linear innovations chain. The idea that there should be a linear innovations chain is, in part, a deduction from an older conception of a hierarchical relation between science and technology that has been abandoned by serious historical scholars of technology. In its place we have what it often called the "interactive model' in which science and technology are seen each as complex, co-equal sub-communities. Each is autonomous and each generates knowledge. Each is involved in the design of artifacts or systems. Each interacts with the others and makes use of the other sub-communities' fruits. The differences between them are as much or more those of values (or social goals) and identity as they are functional. It just is not true to say that the basic sciences generate all the knowledge and the task of engineers is merely to apply this knowledge[43]. Needless to say, such simplistic schemes have made it more difficult for the public and policy makers to grasp the nature and role of engineering research, engineering design, and engineering science, since none of these have any meaning within the older, hierarchical model.

The older, hierarchical model of science-technology relations and hence of innovations has been a major source of bias in innovation studies.

In the TRACES study the clear intent was to show the priority and the fundamental importance of basic scientific research. The authors assumed an orderly chain from basic scientific research to engineering application. But they were honest enough to take note of exceptions to this presumed model. Thus the authors noted that "a better understanding needs to be achieved concerning the two-way influence between science and technology. The tracings revealed cases in which mission-oriented research or development effort elicited later non-mission research, which often was found to be crucial to the ultimate innovation"[44]. Such a statement is technologically naive. This situation is so commonplace in engineering design that engineers have incorporated it into his definition of engineering design[45]. The cyclic (or non-linear) nature of design has sometimes been considered the most important characteristic of engineering design[46].

To clarify our critique of the categories used in innovation studies it might be useful to examine the case of the turbojet revolution in aeronautics. Just in the last few years we have had several major studies published which deal, in varying degrees of specificity with this revolution, and several important unpublished studies are nearing completion[47].

From the point of view of the engineering design of the first jet aircraft, it is quite clear that a sharp dichotomy between need and knowledge is not particularly helpful as far as the innovation is concerned. In the British case, at least, the discovery did not, in fact, push the innovation. Nor did the needs pull call the novel technical ideas into being. At most one can say that knowledge and need were necessary and sufficient for Britain's development of jet aircraft. The countries which developed jet planes did so after they had gained a clear sense of national needs that such aircraft could meet. In particular, a turbojet aircraft offered the potentialities for very high speed flight at very high altitudes, but at the price of high cost and very short range. (This limit on jet engines remained a major constraint on jet aircraft development for several years after the Second World War. It was overcome for transport aircraft by the development of "bypass" and "turbofan" engines tha dramatically increase efficiency and lowered costs.) Therefore, the two powers that did most to pioneer in jet aircraft, Great Britain and Germany, both had a clearly defined national need for high-performance fighter planes. This was demonstrated by the fact that before funding work on jet engines, both nations had initiated urgent efforts to develop high performance fighters. (England built two, the Spitfire and Hurricane. Germany concentrated on one the Messerschmitt Bf 109.)

In the case of Britain, Frank Whittle, the inventor of the turbojet, had his basic insights in the 1920's. At that time a European war did not seem imminent, and there was little emphasis on the development of fighter planes.

Whittle was motivated by a desire to improve performance of aircraft, rather than any sense of national need. The British authorities showed no interest in Whittle's ideas. After the rise of Hitler and his build up of the German air force, the British became acutely aware of their need for air defense. It was in this new climate that Whittle returned to the turbojet and received governmental funding. The idea was first, but it did not push the innovation when no need for it was apparent. Once the need appeared, the innovation was funded and given high priority. But there is no sense at all in the notion that the national need called forth the idea of jet propulsion in either Britain or any other country. It makes little sense, then, to try to categorize the turbojet innovation as either "knowledge driven" or a result of "demand pull".

My conclusion is that innovation studies could be greatly enriched by using the insights provided by the large literature on engineering design, as well as the insights of successful designers of technological systems. A full exposition of all of the potential benefits deriving from the essential identity between a technological innovation and an engineering design would go far beyond the scope of this paper. I will not attempt to solve the problem of quantification. But I can point out how the study of innovations as case studies can eliminate biases built into some innovation studies.

The most fundamental bias of all is that the measure of economic progress and technological change is discontinuous, large leaps in technology, and that such leaps are expressed as technological innovations. This bias toward seeing technological progress as a series of discontinuous leaps is rooted in naive ideas of invention and heroic inventors. If found a certain degree of respectability in the highly original, if rather romantic view of innovation and innovators developed by Joseph Schumpeter[48]. As against this S. Collum Gilfillan developed a theory, based on the historical evolution of the ship, which stressed technological growth as a gradual evolutionary accumulation of small incremental advances[49]. Now it is interesting to note that long before the modern innovations debate started, an important pioneering work, by Abbott Payson Usher, sought to reconcile the differences between the two different views of technological change, in the introductory chapters which he added to the revised 1954 edition of his **History of Mechanical Inventions.** Usher's approach was successful and has proven fruitful because his conception of an innovation of sequence of innovations is isomorphic with the view presented by engineering design. That is Usher saw innovations in terms of gestalt psychology as "gestalts" or particular configurations. Modern students of design see technological artifacts as systems. The similarity between "gestalt" and "system" is, I believe, what gave Usher's work an explanatory power missing in many more recent studies of innovation[50].

However, the perspectives of engineering design suggest that innovations are not the only measure of technological change. There are, as Gilfillan, Usher, and other scholars have suggested, incremental advances which, in the aggregate, may be equal in economic impact and technical content to larger changes which can be labelled "innovations". This is clearly where Max Jakob and heat transfer come in.

The great achievement of engineering research and of the engineering sciences has been a much more fundamental understanding of technology. In the case of heat transfer this understanding found expression not only in major "innovations" such as jet propulsion and modern rockets, but also in innumerable improvements, both small and large, in the design of the entire range of thermally-dependant devices. Heat transfer, by providing deeper and more fundamental insights into such devices, provided the means for improvements in the design of the entire range of such devices: boilers, heat exchanges, air conditioners, heat engines, and so forth. These include not only heat engines or other devices that are actuated by heat, but all systems, including electronic devices, which generate waste heat that they must dispose of somehow.

In some cases an important advance, such as cooling the turbine blades of jet engines, might not be ranked as an innovation at all. Heat transfer studies, conducted at the University of Minnesota and elsewhere, showed how turbine blades could tolerate considerably higher temperatures if cooled by "film cooling" in which a cooler fluid (the outside air) is admitted through one or more holes in a hollow turbine blade. The cooler fluid then forms a film which cools the turbine blade and allows the higher inlet temperatures and hence greater power and efficiency. Film cooling is one of several methods developed theoretically and experimentally by engineering scientists.

While film cooling, and other insights derived from heat transfer, have had an enormous technological and economic impact, virtually none of these would be ranked as "innovations" as these are defined in design studies. Designs are hierarchical structures, and "innovations" are defined in terms of the top layers (those that define the character of the system. If sufficiently new it becomes an "innovation"). The effects of the engineering sciences upon design usually consist of improving performance of component levels. But neither engineering nor basic science typically define the nature of the system. One may grant exceptions such as the atomic bomb. But analysis seldom leads directly to a new synthesis. The more common result is better understanding and control of design at the component level. Hence engineering scientific contributions do not normally show up directly as innovations to be counted. Such improvements in engine performance as have been made by the use of heat transfer could be incorporated into existing designs and generally would rank as an improvement of an existing model or product, rather than the introduction of a new product.

Thus, there are many cases of important intellectual advances which lead to technological improvements and economic benefits but which are not usually counted as "innovations" in the usual sense of putting a new **system** into use. In many other cases the improvements would be small, reflected in gradual improvements in efficiency, durability, safety, and cost. By the use of heat transfer research a variety of fin-cooled devices, such as automobile radiators, have been significantly improved by many incremental advances. Some of these would be counted innovations, but many others would not[51].

The perspective from the study of engineering design suggests that innovations are not the only indices of technological advance and of economic growth. The growth of knowledge and also of technique leads to small incremental advances as well as major leaps forward. A pioneering economic study by Samuel Hollander, which demonstrated that small advances by production personnel were of greater economic value that the products of the research laboratory, can find a ready explanation in the framework of engineering design[52]. One reason Hollander's work has not been adequately followed up is because his findings are hard to understand within the framework of innovation studies. But Hollander's data fits very well into the framework provided by studies of engineering design. The amount of new knowledge may vary enormously. There is much evidence to suggest that the aggregate of small incremental advances has a greater economic impact than the large innovations. Both must be taken into account, but innovation studies only look at the "high end". Engineering design as a framework, is not biased toward changes at a particular scale range.

Max Jakob had no direct role in the turbojet revolution or any other significant innovation. But the heat transfer which he and his students did so much to advance and diffuse has had an enormous economic impact which innovation studies have yet to capture or understand. His career raises the question of the economic value of engineering research and its impact on technology and economic growth. If a way can be found to disaggregate the intellectual capital of research from the immediate inputs from technology, it might be possible to quantify the role of research in engineering. Until then we can say that case studies show that the value of engineering research is very great.

Reference notes

1 My account of Jakob is based upon Elizabeth Jakob, "Max Jakob, July 20, 1879 - January 4, 1955, Fifty Years of His work and Life" in Edwin T. Layton Jr. and John Lienhard, eds., History of Heat Transfer, Essays in Honor of the 50th Anniversary of the ASME Heat Transfer Division (New York: American Society of Mechanical Engineers, 1988), pp. 87-116. See also Ernst Schmidt, "Max Jakob zum 75 Geburtstag" Forschung auf dem Gebiete des Ingenieurwesens, 20, (no. 3, 1954), p. 65, and W. Fritz, "Erinnerung an Max Jakob" Warme-und Stoffubertragung, 8, (1975), pp. 45-48.

2 See Edwin T. Layton, "Science as a Form of Action: The Role of the Engineering Sciences" Technology and Culture, 29, (January 1988), pp. 82-97.

3 Zehev Tadmor and others, Engineering Education 2001, (Haifa, Israel: The Neaman Press, 1987), p. 14.

4 Ibid.

5 Jakob, "Max Jakob" pp. 95-97.

6 Ibid., p. 97. On the physics background of heat transfer see Stephen G. Brush, "Gaseous Heat Conduction and Radiation in 19th Century Physics" in Layton and Lienhard, History of Heat Transfer. See also Stephen G. Brush, The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century, (New York: American Elsevier, 1976).

7 The impact of heat transfer research can be gauged by Arthur E. Bergles, "Enhancement of Convective Heat Transfer: Newton's Legacy Pursued" pp. 53-64, and other essays in Layton and Lienhard, History of Heat Transfer, notably those by Dawson, Eckert, Fuji and Uehara, Somcerscales, Wise, and Cheng and Fuji.

8 Theodore von Karman, The Wind and Beyond, (Boston: Little Brown, 1967), pp. 144-148. In his letter of resignation to the German Minister of Education, Von Karman hoped that the minister would do for German science as much as he had accomplished for foreign science in one year (p. 145, note). I assume this referred to the mass dismissals of Jewish professors, though von Karman could have meant himself. However, von Karman visited Germany in 1934, and was shown around by Goring's subordinate, Dr. Adolph Baumker, and von Karman was offered a position working as consultant for the Nazi Air Ministry. This is the basis of a famous anecdote to the effect that goring offered von Karman a job, and the latter replied that he was a Jew, whereupon Goring said "Who is or is not a Jew is up to me to decide" (p. 145). Actually, von Karman, who gives the Goring quote, is careful not to claim directly that this was Goring's response to his refusal, but at the same leaves the door open to interpretation that this was in fact a statement about himself, (p. 146).

9 Jakob, "Max Jakob" pp. 87-116.

10 George S. Emmerson, Engineering Education - A Social History, (New York: Crane, Russak, 1973), pp. 193-194.

11 Ibid., pp. 85-90.

12 Ibid. Jakob chaired a meeting of PTR scientists for the first time in 1919. In 1929 he reported to his colleagues about steam research in America at meetings in 1929 and 1930. He was later invited to Prague (still part of an independent nation in 1930). His quasi-official position as representative of the of the PTR and of German science was highlighted by the presence of the German Ambassador at the ceremonial part of his visit. He also attended a number of international conferences on steam nomenclature and standards.

13 Jakob, "Max Jakob". The Nazi Gauleiter's veto of Schmidt's appointment is noted in Ernst R. G. Eckert, "Ernst Schmidt - As I Remember Him" in Layton and Lienhard, History of Heat Transfer, p. 142. Schmidt also wrote a tribute to Jakob in 1954 on the occasion of his 75th birthday (see Note 1).

14 Werner Burau and Bruno Schoeneberg, "Christian Felix Klein" Dictionary of Scientific Biography, s.v.

15 John H. Lienhard, "Ludwig Prandtl" Ibid. s.v.

16 Ludwig Prandtl, "Uber Flussigkeitsbewegung bei sehr kleiner Reibung" in F. W. Reigels, and others, eds., Ludwig Prandtl Gessamelte Abhandlungen, 3 vols. (Berlin: Springer Verlag, 1961), II, pp. 575-584.

17 Osborne Reynolds, "An Experimental Investigation of the Circumstances which Determine Whether the Motion of Water Shall be Direct or Sinuous and the Law of Resistance in Parallel Channels" in Osborne Reynolds, Papers on Mechanical and Physical Subjects, 3 vols. (Cambridge: Cambridge University Press, 1900- 1903), II, pp. 51-105. This paper originally appeared in 1883.

18 Von Karman, The Wind and Beyond, pp. 34-41, 204.

19 Ibid., p. 204. On heat transfer at General Electric see George Wise, "Heat Transfer Research in General Electric, 1910-1960: Examples of the Product Driven Innovation Cycle" in Layton and Lienhard, History of Heat Transfer.

20 Wise, "Heat Transfer Research in General Electric" cites a number of cases of heat transfer research initiated for one product which found eventual application in a very different engineering system. In the case of problems in forced convection the problem could be divided into two parts, one concerned with fluid flow and the other with heat transfer. In this way work done by the founders of fluid mechanics

such as Prandtl and von Karman was of direct utility to heat transfer. It should be emphasized that fluid mechanics involved enormous computational difficulties and its importance, along with that of heat transfer and other sophisticated engineering theories, was greatly enhanced by the coming of the computer.

21 ibid., pp. 62-65..

22 Von Karman's memoirs contain numerous examples of this naive egotism. For an example see his evaluation of Einstein (Ibid., pp. 180-184).

23 Charles Susskind, "Theodore Von Karman" Dictionary of Scientific Biography, s.v.

24 Jakob, "Max Jakob" pp. 105-107.

25 Ibid., pp. 108-113. His book was Heat Transfer, 2 vols. (New York: John Wiley and Sons, 1949, 1957). (The second volume was published posthumously with the technical and editorial assistance of Jakob's first American student, S. Peter Kezios.)

26 L. M. K. Boelter, V. H. J. Cerry, H. A. Johnson, and R. C. Martinelli, Heat Transfer Notes, (New York: McGraw-Hill, 1965). This formal publication was a reprint of notes originally produced by Boelter, Cherry and Johnson for use by their students in 1932.

27 E. R. G. Eckert, Introduction to the Transfer of Heat and Mass, (New York: McGraw-Hill, 1950). See also "From Braunshweig to Ohio: Ernst Eckert and Government Heat Transfer Research" in Layton and Lienhard, History of Heat Transfer.

28 W. H. McAdams, Heat Transmission, (New York: McGraw-Hill, 1933).

29 Edwin T. Layton and John H. Lienhard, "A History of the Heat Transfer Division" in Layton and Lienhard, eds., History of Heat Transfer, surveys the diffusion of heat transfer to America including the role played by Jakob and a brief discussion of the Jakob Award.

30 Robert M. Solow, "Technical Change and the Aggregate Production Function" Review of Economics and Statistics, 39, (August 1957), pp. 312-320.

31 See for an example of a large literature, Kenneth Arrow, and others, Mathematical Models in the Social Sciences, (Stanford: Stanford University Press, 1959), which includes a follow-up paper by Solow, "Investment and Technical Progress" pp. 89-104.

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33 C. W. Sherwin and R. S. Isenson, "Project Hindsight" Science, 156, (June 23, 1967), pp. 1571-1577, and the same authors' First Interim Report on Project Hindsight: Summary, (Washington: Office of the Director of Defense Research and Engineering, Publication AD-642-200, 1966). For TRACES see Illinois Institute of Technology, Research Institute, Technology in Retrospect Critical Events in Science, (TRACES), 2 vols., (Chicago: Illinois Institute of Technology Research Institute, 1968). A follow-up study extended the number of innovations covered to ten. See Battelle Memorial Institute, Columbus Laboratories, Interactions of Science and Technology in the Innovative Process: Some Case Studies, NSF contract NSF-C667 (Columbus, Ohio: Battelle Memorial Institute, 1973).

34 OECD, Gaps in Technology, 2 vols. (Paris: OECD, 1970, 1972).

35 Edwin Mansfield, and others, The Production and Application of New Industrial Technology, (New York: W. W. Norton & Co., 1977), p. 26.

36 J. Langrish, and others, Wealth from Knowledge, (London: MacMillan, 1972).

37 Stephen J. Kline, "Innovation is Not a Linear Process" Research Management, 18, (1983), pp. 36-51. See also the same author's "Research, Invention, Innovation, and Production: Models and Reality" Report INN-1, 1985, Thermosciences Division, Department of Mechanical Engineering, Stanford University.

38 In many ways the starting point of modern design is Morris Asimow, Introduction to Design, (Englewood Cliffs, N.J.: Prentice-Hall, 1962), who defined design "morphology" and who insisted that any such model of the design process should include feedback loops, making the models of design non-linear (pp. 11-12). A comparative analysis of differing design flow charts (or morphologies) may be found in Gerhard Pahl and W. Beitz, Engineering Design, (London: The Design Council, 1st English translation, 1984), p. 2, fig. 1.2; p. 3, figs. 1.3, 1.4; p. 10, fig. 1.5; p. 11, fig. 1.6; p. 12, fig. 1.7; p. 14, fig. 1.8; p. 16, fig. 1.9; p. 17, fig. 1.10; p. 1.9, p. 41, fig. 3.3.

39 Thomas T. Woodson, Introduction to Engineering, Design (New York: McGraw-Hill, 1966), p. 3. See also Morris Asimow, Introduction to Design, (Englewood Cliffs, New Jersey: Prentice-Hall, 1962).

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41 Kline, "Innovation is Not a Linear Process" pp. 36-44.

42 Center for the Study of Industrial Innovation, On the Shelf, (London: Center for the Study of Industrial Innovation, 1971) is a study of innovations which failed because of market miscalculations by managers.

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47 In addition to Edward Constant's The Origins of the Turbojet Revolution, (Baltimore: Johns Hopkins, 1980), recent works in the NASA history series include Alex Roland, Model Research: The National Advisory Committee for Aeronautics, 1915-1958, 2 vols., (Washington: NASA, SP-4103, 1985), and James R. Hansen, Engineer in Charge, A History of the Langley Aeronautical Laboratory, 1917-1958, (Washington, NASA, SP-4305, 1987). I was privileged to read one chapter of the forthcoming history of the Lewis Research Laboratory by Virginia Dawson, as well as benefiting from conversations and correspondence with her. I was also privileged to direct the recent dissertation of Brian Nichelson, "Early Jet Engines and the Transition from Centrifugal to Axial Compressors: A Case Study in Technological Change" unpublished Ph.D. dissertation, University of Minnesota, 1988). The last work is, I believe, the first effort in the history of technology to use engineering design as the central organizing concept for understanding a significant technological innovation.

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49 S. Collum Gilfillan, The Sociology of Invention, (Chicago: Follett Publishing Co., 1935), p. 5.

50 Abbott Payson Usher, A History of Mechanical Inventions, rev. ed., (Cambridge: Harvard University Press, 1954), pp. ix-x.

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ANALYSIS AND SYNTHESIS IN ENGINEERING

A Methodology for Inventing Machinery in Polymer Processing

Z. Tadmor

Introduction

This paper discusses a methodology recently proposed by the author (1), for systematically inventing polymer processing machines. That is, it discusses a formal, systematic, possibly axiomatic methodology by which new alternative geometrical design configurations can be generated, conceptually tested, and designed.

Invention is a prime example of engineering **synthesis,** yet much of the methodology involves primarily **analysis.** Therefore, the relationship between 'synthesis' and 'analysis' in engineering is briefly explored. This relationship, however, is a manifestation of the more general and more complex relationship between SCIENCE and TECHNOLOGY. A subject of some controversy, which fortunately receives an increasing amount of attention by engineers, scientists, philosophers and historians (2).

Science and technology

'Science' is defined by the dictionary as "a branch of knowledge or study dealing with a body of facts or truths, systematically arranged and showing the operation of general laws". It is clear definition. Unless the body of knowledge shows the operation of general laws, it may be a branch of knowledge, but it is not science. In dealing with natural sciences, the general laws are, of course, the Laws of Nature.

'Technology' is defined by most dictionaries, and it is perceived by society as the "scientific study of industrial arts", or the "application of science to industry", or simply "applied science". These definitions are historically incorrect and greatly misleading[1]. To begin with, Technology was practiced by man long before he invented the concept of science and the scientific method. How could then 'Technology' be 'Applied Science'? "Where there is man there is Technology", claim logically Bugliarello and Doner (2), who defined Technology as "the domain of man-made". Indeed, as most practicing engineers would intuitively argue, Technology is our accumulated knowledge of 'making all we know how to make'. Technology, unlike science, could be successfully practiced without understanding the fundamental laws underlying its nature. There are many examples that can be quoted in support of this contention. Metallurgy, for example, was actively and successfully practiced in the fifth and fourth millennia BC, thousands of years before the chemistry of metals was understood by man. The same argument holds, of course, for pottery and ceramics. Man built magnificent structures long before stress analysis was conceived. Rubber as well as plastics were processed into useful products before Herman Staundinger proposed his hypothesis on the structure of macromolecules, certainly before his hypothesis was finally accepted and converted into a shared paradigm and long before the complex rheological behavior of plastics and rubber was elucidated and mathematically formulated. Clearly, then Technology is **not** the theory of the practical arts, but the **practical arts themselves,** and Science and Technology differ in purpose and nature. Herbert Simon (3) in his series of lectures on the "Science of the Artificial" expounded the same idea in even broader terms. He suggested that in 'science' one deals "with things the way they are"; whereas, in technology one deals "with things the way they ought to be", and the difference between the two is fundamental.

However, Science and Technology interact in very important and complex ways. They feed upon each other and accelerate each other's progress. The key aspect of this interaction takes place in the process of **invention.** It is not surprising, therefore, that many attempts were made to clarify its nature and elucidate its mechanisms. The preoccupation with 'invention' is, of course, far from being just a pure academic endeavor in pursuit of the elusive relationship between Science and Technology, but rather is motivated by the desire to fully elucidate the nature of the **technological progress** itself, which is **driven by the process of invention.** Indeed, Kranzberg (4) succinctly points out that the search for the wellsprings of creativity is one of the chief intellectual commitments of the 20th century, with enormous economical and practical consequences.

Historically, there are two contradicting notions on invention. One claims invention to be a 'flash of intuition' with no methodology, serendipitous in nature and without structure, although not necessarily without logic or sequence. The other notion originates from Francis Bacon, who was apparently the first to discuss at length the relationship between Science (the way he understood it) and Technology. He proposed a clear cut program for the advancement of Technology by Science through **systematic invention.** Yet, as pointed out by Cardwell (5) and others, Bacon was unable to show how his 'Science' related to Technology, by what **formal** means the scientific knowledge gained by his method could be applied to systematic invention. Yet, Bacon recognized the importance of invention and innovation, and he also had an understanding of the role of new knowledge in enabling additional invention to be made. He distinguished between **'empirical invention'** or innovation, which requires no further understanding or knowledge from what is available, and **'invention'** which requires breaking new knowledge. The latter invention we would call today 'science based' invention and, as noted earlier in science based technology, science and technology are intimately intertwined to an extent that the characterization of 'Technology' as 'Applied Science' becomes relevant and perhaps an accurate definition. To these two categories Cardwell (6) adds two additional ones, 'systematic improvement' and 'scientific research', in pursuit of a specific invention or innovation.

Indeed, there are many inventions that can be quoted which are the result of scientific discoveries, as are many scientific discoveries that resulted from technological advances. To quote Lawrence J. Henderson's famous aphorism: "Science owes more to the steam engine than the steam engine to science", and, of course, the whole science of thermodynamics was 'technology driven', by the desire of Carnot to understand the limits and efficiency of heat machines.

Yet, all the models that were hitherto proposed, proved unsatisfactory in showing the exact relationship between Science and Technology in **innovation** (6). Clearly, Science and Technology interact, and this interaction triggers innovations. But the exact nature of this process is yet to be elucidated. A classical example demonstrating the difficulty in identifying the exact role of science in the invention process is, of course, the invention of the steam engine by Watt[2]. A clear understanding of this process should bring us closer to the Baconian concept of systematic, axiomatic, 'science' based technological evolution.

In dealing with invention, specifically with the role of science in the invention process, the point of view adopted in this paper is that an invention, innovation or discovery has a technological impact only if it has been transformed through **design** into an artifact and the artifact has been **tested** and **applied.** Therefore, in searching for a clear understanding of the interaction of Science and Technology in invention, it is necessary to explore the full **chain of events** leading from the **conception of an idea** to its final **application.** It is the **kinetics** of this chain process that must be researched, elucidated and explained. If the term **'synthesis'** is chosen to be applied to the process of **creation of an artifact,** then it may be perhaps meaningful to state that it is the **analysis** that plays the key role in the 'invention process'. Indeed, as Resnick (6) points out, synthesis and analysis in engineering are practically inseparable. "They interplay and interact, supplement and complement, like Yang and Yin." The relationship between these two key concepts, 'synthesis and analysis', in engineering is discussed next.

Analysis and synthesis in engineering

The main concern of engineering should be, and in fact it is, **synthesis** or **design** which by definition is the 'combining of separate elements into a whole'. This is its fundamental characteristic. More specifically, engineering's main concern is the construction of artifacts that have a function, a role or a purpose. In other words the main concern of engineering is to practice Technology. Yet, much of the recent engineering research, teaching, and activity, in general, is completely preoccupied with **analysis.** The analysis of processes and machines. **Analysis** is defined as the 'separating of an entity into its constituent elements', and it falls well within the realm of the natural sciences, because the objective of the Natural Sciences is to analyze facts about Nature and discover its fundamental laws.

The roots of the preoccupation of engineering activity with analysis sprung from the phenomenal success of the natural sciences in the 20th century that expanded our understanding and horizons beyond all expectations. This created a desire to apply 'scientific' methods to engineering and to emulate in technology the success of the natural sciences. This trend also had important repercussions in engineering education and in a drive started by Karl Taylor Compton of MIT in 1930[3], which really gained momentum after world War II, engineering schools and curricula were purged from vocationalism and replaced by fundamental sciences. The newly acquired scientific methods and tools, were quickly put to use, bringing about a remarkable improvement in the understanding of machines and processes, their quantitative formulation and optimization. But understanding how a given machine works or optimizing its function still deals with 'things the way they are', with but a vague and indirect relationship to **design** and **invention.** We must recall that any existing machine or process is but **one** design solsution out of **many** possible alternative solutions to the originally posed problem. Science alone, with all its laws, tools and methods, cannot suggest any alternative solution. If a solution is suggested it can, of course, **analyze** it, determine its scientific viability, logic, sense, or lack of all these[4].

Yet, we are in the midst of not only a **scientific** revolution but also of a **technological** revolution of historical scale brought about by the scientific revolution. Clearly, the accelerated scientific advancements broke new ground and it uncovered new invention opportunities, just as Bacon suggested. Moreover, in dealing with the Technological revolution and trying to explain its sources, one should not lose sight of the fact that in the pursuit of technological progress, the technologically advanced societies invested increasingly **more resources** into generating new technology. Nevertheless, just a quantitative increase in the number of scientists and engineers and allocated resources cannot explain the full scope of the Technological revolution. There must be other 'non-linear' factors at work. This factor must be associated with the penetration of the 'scientific method' of **analysis** to engineering.

Part of the answer may, perhaps, become evident if the foregoing **chain** process of invention, consisting of the creation of a new idea per se, its design into an artifact, and its implementation is carefully scrutinized. The question that immediately arises is which of these sequential steps is rate controlling? The rate controlling step is the slowest, most difficult or the most expensive one. Is the present rate controlling step (or steps) the same as the one in the past? Will the situation change in the future? As far as the past is concerned (and perhaps the present as well) it is easy to argue that the rate controlling step might not be the conception of new ideas, but rather the identification of the good ones and the weeding out of the bad ones, as well as the conversion of the idea into an artifact. Traditionally, the useless ideas were weeded out through a painfully slow process of trial-and-error. **Engineering analysis,** however, frequently makes possible an a priori, almost instantaneous elimination process. Similarly, alternative design solutions can be quickly analyzed and attention can be focused on the most promising ones. It is not surprising, therefore, that the permeation of 'analysis' into engineering which is common practice, has accelerated non-linearly technological progress through the process of invention. Thus, it served an immensely useful role in engineering.

But, what about the conception process itself? Does the first step in the 'invention chain' defy rational explanation? Can the tools of analysis be applied to this step as well? In other words, can new ideas be systematicaly generated?

Methodology of invention

In discussing engineering machinery the 'conception of new ideas' takes a somewhat more restrictive form, namely the conception and design of new machine configurations. Thus, the question at hand is whether new machine configurations can be **systematically conceived.**

The answer to this question, as proposed here is that **analysis and the tools of engineering sciences** could serve yet another function. They could be used to uncover the 'Machine Elements' which are the underlying forms, principles or characteristics of all possible machines, then limit the vast number of alternative design solutions by determining the fundamentally different ones which will form the 'Building Blocks' to real machines. These 'Building Blocks' could in turn be systematically transformed into desirable design solutions. Then by using **material, process, economical** and other constraints as **guidelines** the unfit solutions could be weeded out.

The first step of the methodology suggested here and discussed briefly below, is not fundamentally different from the apparently still the only systematic treatment of machinery by Frantz Reuleaux (8) who in his 1977 "Principes d'une Theorie Generale des Machines" suggests to find an 'elementary structure of the machine simple enough to be general and exhaustive enough to provide designs for special constructions'. Reuleaux's dream was to reduce technology to pure 'science'.

This concept was reformulated in broader terms by H. Simon (3) who coined and discusses the concept of the 'Science of the Artificial'. Clearly, by the definition of Science, this would require not only a body of systematically organized knowledge, but also it must show the **operation of general laws.** In this sense, even if a useful methodology can be devised for invention and design, it would be very hard to claim generality of 'laws', because in the process of narrowing down the number of alternative solutions, some fundamentally different ones, based perhaps on yet undiscovered mechanisms, may have been lost. Nevertheless, such a methodology may be a first step toward the formulation of 'Science of the Artificial'.

Machine elements and building blocks in polymer processing

The analysis for uncovering the 'Machine Elements' and 'Building Blocks' in polymer processing is discussed in detail elsewhere (1). Here we only note, that by analyzing the physical mechanisms in existing polymer processing machines **the 'inner structure' of the machine becomes apparent. This 'inner structure' will consist of a simple geometry, which contains the principle of operation of the machine.** The ideal tool to discover the 'inner structure of a machine' the concept that makes it 'work', is **mathematical modeling.** In mathematical modeling it is seldom possible to formulate accurately the 'real' process, so we simplify the process. But 'simplification' is not really the right definition to what is done in modeling. A better description would be the **'construction of analogs'.** These may be physical analogs or mental analogs, which are amenable to mathematical formulation. A successful modeller is one who is imaginative in creating these analogs, but in such a way that the **essential features** of the real process are retained. In the course of such a process the **underlying inner structure of the machine must surface.** For example, the simplest mathematical model of the single screw extruder - one of the most important machines in polymer processing - is 'two parallel plates in relative motion'. It gives a good approximation to the real process because it **captures the key element of screw extrusion**, namely **the drag induced by the motion of one surface relative to another surface. By analyzing other machines, similar simple geometrical features emerge.**

These characteristic features, the simplest modelling geometries, that captures the essence of the process, are in fact the 'building blocks' from which new machine configurations can be synthesized.

Are the 'building blocks' themselves the most fundamental components of the machines? Clearly, they are not. They are all made of only two fundamental elements **a surface moving parallel to its plane and a surface moving normal to its plane.** These are termed **machine elements.**

From the 'machine elements' that evolve 'building blocks' can be constructed by pairing the elements with stationary surfaces and among themselves. This synthesis results in twelve distinguishable building blocks depicted in Figure 1. Not all of them have the same significance, not all of them may lead to practical or any design solution, but they are all distinguishable 'building blocks'.

MACHINE SYNTHESIS

All the building blocks in Figure 1 consist of at least one 'moving' surface. A continuously 'moving surface' implies either an infinite surface (for a plane moving parallel to itself) or an infinite space (for a surface moving normal to its plane). How can we devise ways to provide essential options for machine operation in a practical way? Considering the surfaces moving parallel to its plane, Figure 2 shows four practical solutions to the problem: an 'infinite' belt, the outer surface of a cylinder, the inner surface of a cylinder, and the surface of the disk. These are four fundamentally different solutions. There are many other solutions. Thus, rotating spheres, cones, spheroids, etc., are also 'design solutions' to 'infinite moving surfaces' but they are not fundamentally different, and may neither be practical. By selecting these fundamentally different moving surfaces, the overall number of machines that can, in principle, be designed is **radically restricted** to the most promising ones.

Restricting the other element - the surface moving normal to its plane - to a limited number of practical alternatives, is somewhat more difficult. One possibility is to attach normal sections to the infinite moving surfaces of Figure 2, as shown in Figure 3. All machines based on these design solutions will be periodic in nature. Figure 3e and 3f show a ram-type element and rolling cylinder moving in a channel. The channel may be straight or curved as in annular space or in a helical channel.

The next step in the synthesis is the pairing of building blocks to the 'infinite surfaces'. Out of this pairing process, through a number of simple logical steps, a whole spectrum of machine concepts emerges. The procedure via numerous examples is discussed in detail elsewhere (1). Results show, that among these machine concepts there are not only all the existing machine configurations, that are thus systematically reinvented, but also **many novel machine configurations.**

Thus, by applying the proposed methodology to the field of polymer processing, new machine configurations can be systematically generated. New machines can be invented not through the 'magic' of a 'spark of intuition', but by a careful analysis of existing machines and processes. This analysis first narrows the focus to a few fundamentally different 'Building Blocks' out of which a broad spectrum of new machine burst out. **The principles of operation of the new machines, however, will be restricted to those of the existing machines.** If machines based on other principles are to be created, then **one must examine the fundamental laws of the particular process** and further expand the scope and range of design solutions.

Of course, the philosophical question whether in principle the combined steps of the methodology and the analysis of the fundamental laws can discover **all** possible alternative useful machines, remains wide open. The answer is probably negative. However, the question that should be posed is perhaps not whether **all** design solutions can be generated, but following Herbert Simon's approach, it should be whether a **satisfactory** number of new design solutions can be generated. The answer to this question is affirmative, though qualified.

Footnotes

1 Although, in certain science based technologies it is becoming a reality today and certainly it may come true in the future.

2 The development of the steam engine, or rather the separate condenser to the Newcomen engine by Watt, an invention of profound significance, is wrought with controversy on the role of Black's science on latent heat of vaporization, in the invention process.

3 This was the main these of his presidential inaugural address: "I hope ... that increasing attention in the Institute may be given to the fundamental sciences, ... that all courses of instruction may be examined carefully to see where training in details has been unduly emphasized at the expense of the more powerful training in all-embracing fundamental principles".

4 Although sometimes caution should be perhaps exercised in discarding engineering experience before scientfic authority, as painfully experienced by Sir Thomas Bauch, whose great bridge and reputation collapsed in 1879. When asked why he designed a weak bridge when all his other bridges were strong and good, he said that he accepted the advice of the renowned scientist Sir George Airy who assured him the "greatest wind pressure on a plane surface will be subjected to is 10 psi ...", P. Sporn, "Foundation of Engineering", Pergamon Press Book, the Macmillan Co., New York, 1964.

Figure 1: Machine Elements and Building Blocks. The building blocks are obtained by pairing the machine elements among themselves and with stationary surfaces.

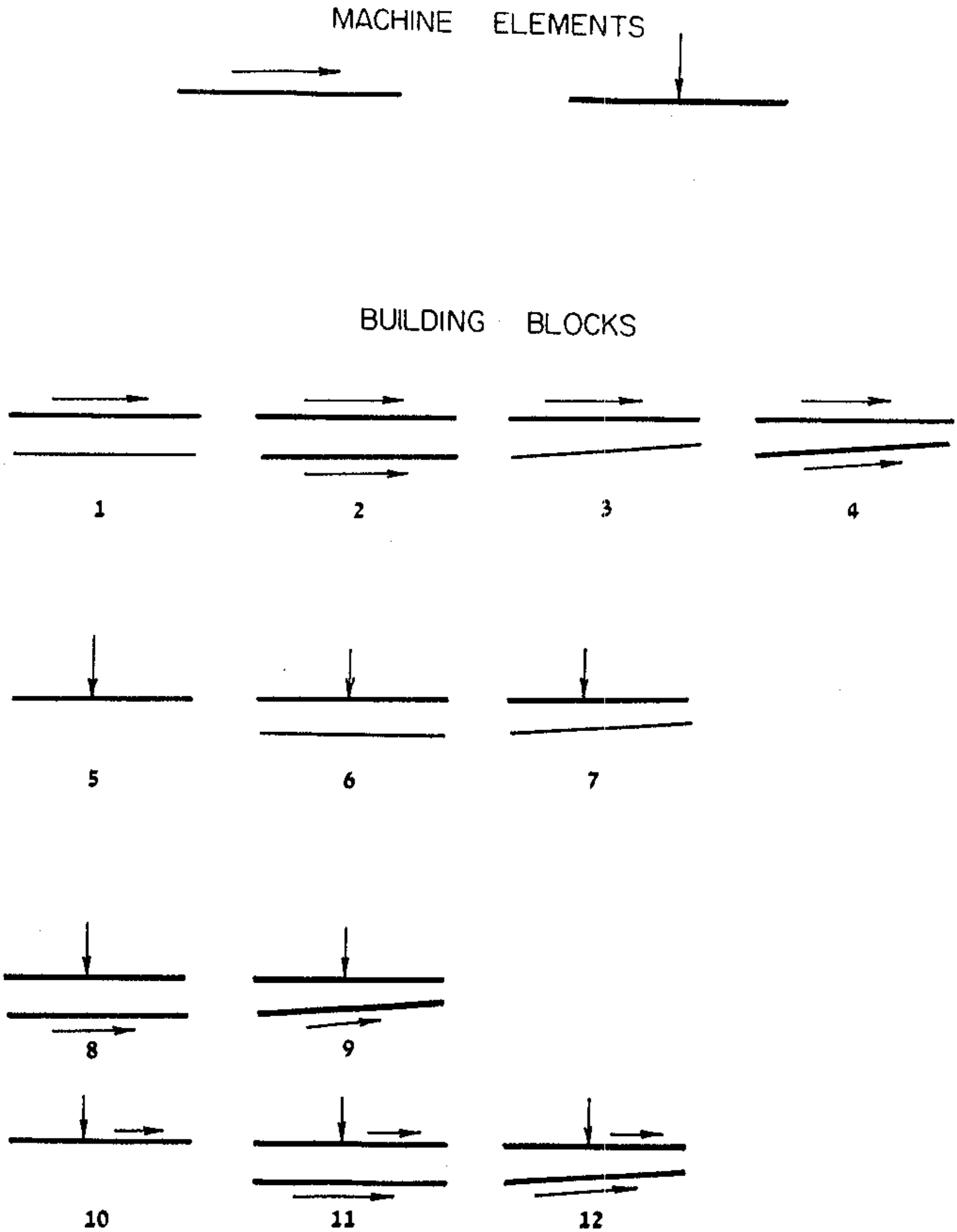


Figure 2: Some simple design solutions for creating infinite surfaces moving parallel to their plane.

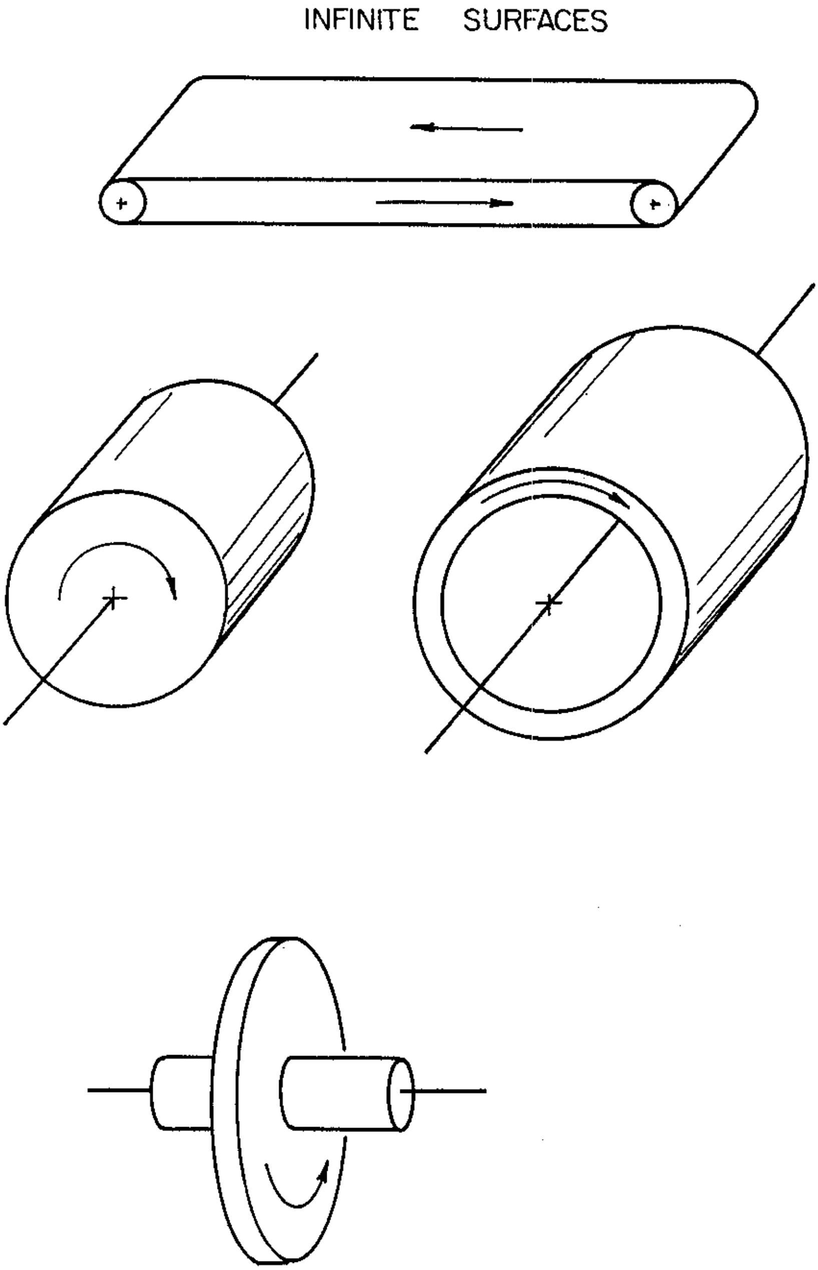
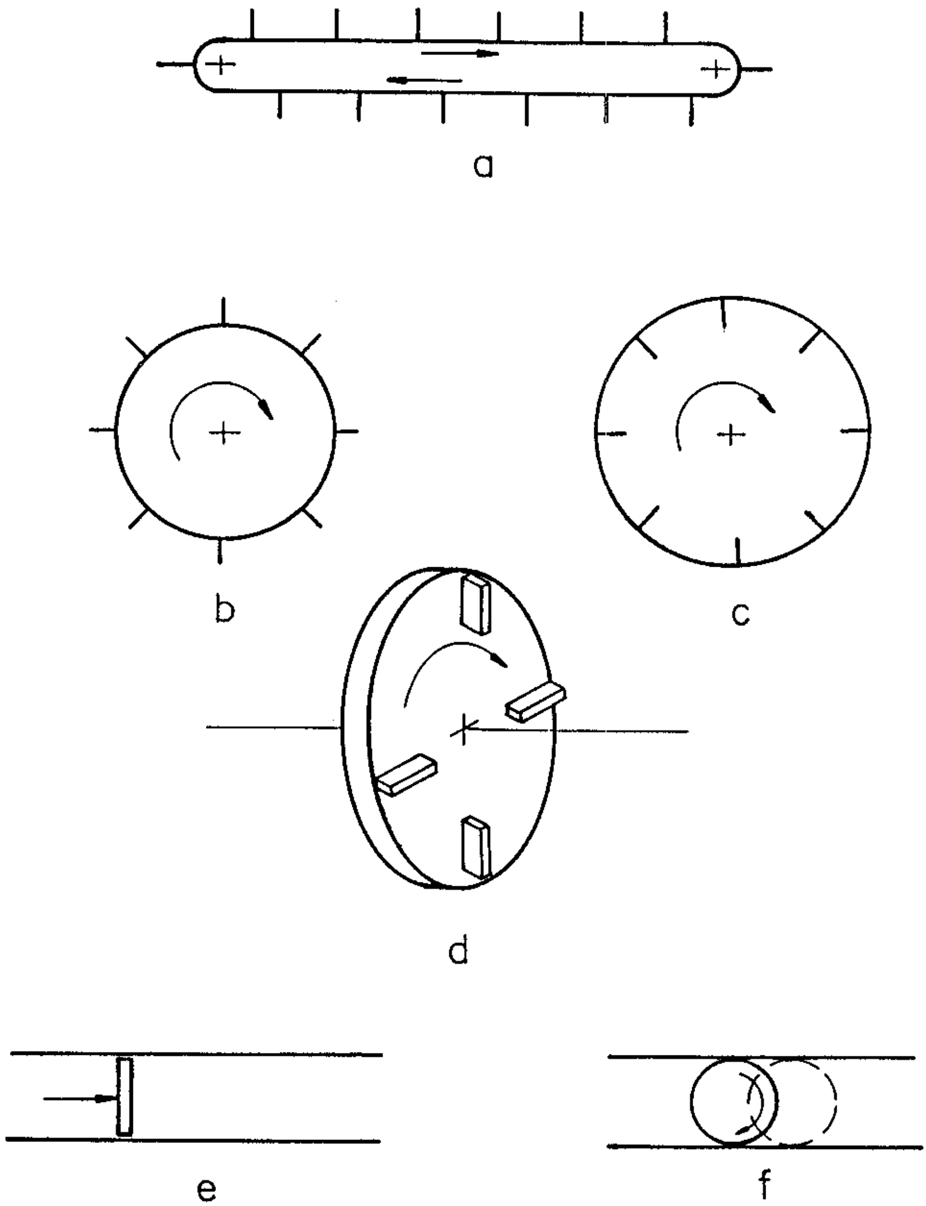


Figure 3: Some possible design solutions to create periodic continuous motion of a surface normal to its plane.



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INNOVATION STRATEGIES
AND APPLICATIONS POLICIES

STRATEGIC MANAGEMENT IN THE 21st CENTURY: THE ROLE OF CIM TECHNOLOGY

M. Jelinek and J. D. Goldhar

I. Introduction

Two key realities seem focal for management in the 21st Century: vastly more complex, competent and faster-paced global competition; and vastly enhanced manufacturing technology, based on expanded scientific knowledge. Both hinge on electronics, which connects markets, transferrs knowledge, increases the pace of scientific change, and controls manufacturing processes. These realities produce a competitive world far different from that of even the immediate past. Formerly successful ideas, assumptions and approaches from the past are pernicious now, for they distract our attention from the new realities and mislead us into thinking nothing has changed.

We intend to make the case for Computer Integrated Manufacturing (CIM) as the approach of choice for the future. In place of older assumptions of economies of scale and "the one best way" ideas -- which made excellent sense in their time -- we propose "economies of scope" assumptions that favor repeated innovation. We will begin with a brief survey of the older world, to show how its assumptions and underpinnings have changed, most particularly as the result of new manufacturing technologies that permit programmable change. Our focus will be two-fold: on the capabilities of CIM technology, and on its strategic potentials. We will highlight central elements of the changed competitive environment facing managers today, noting how old approaches fail.

Potentials and technology translate to strategic advantage only if managers can understand how CIM can be used. So next, we will show how CIM technology permits exploitation of economies of scope thinking to benefit from the new conditions. CIM's potential for driving a flexibility-based strategy will be explored. Because realizing the potential of CIM requires new organizational responses, we will address the key elements of a CIM-based organization. Because new strategies will be required, different from those of mass manufacturing of the past, we will sketch the highlights of CIM-based business strategies. Finally, we will close with some suggestions for implementing CIM.

II. The Limits of Traditional Manufacturing

Traditional manufacturing evolved during the last century along "scientific management" and industrial engineering lines. It focused on a narrowly defined concept of "efficiency", based on scale economy thinking. Typically, this has meant minimizing variation in favor of standardized products; synchronized and rigidly specialized production and labor practices; and long production runs. Productivity -- more output at lower cost -- was seen to operate as a tradeoff against quality and variety. Quality emphasis was thought to slow production rates and increase costs, while variety meant a loss of learning curve benefits and higher set-up costs. Achieving high productivity also meant that manufacturers had to curtail product innovation. To maximize any one of these dimensions, traditional factories seemed inescapably to incur increased costs in the others.

How did the managers' dilemma evolve? As traditional factories grew in size, they became progressively more difficult to coordinate. Complex, paper-based information systems for managing them could handle only small amounts of variation at a time. Changing anything required arduous effort to bring operations back into control (see, for example, Hounshell, 1984, p. 12-13). No variation could be handled very quickly, whether due to new product features, new materials, or changed methods. Thus factory practice tended toward long production runs of standardized products, building large inventories to minimizing set-ups and changeovers.

With such practices came lengthy queues and lead times, high levels of work-in-process inventory, and heavily capital intensive production facilities focused for high volume production in an effort to achieve low-cost operation. Emphasis upon schedules and direct costs often impeded quality improvements, as well. These factors encouraged "proper" industrial engineering and "scientifi management practices, aimed at reducing change in order to reduce cost and quality problems. What resulted was "management by exception" and a rigid factory that emphasizes "efficiency," but is fundamentally unresponsive to change: the all too frequent dilemma of U.S. factory managers in recent years (Hayes and Abernathy, 1980; Skinner, 1986).

Traditional technology factories mandate a business strategy of standard products, produced over a long time period in high volume with few variations. This is scale-economy thinking: if the product can be sufficiently standardized and manufactured in sufficient volume, so the reasoning goes, it can be made cheaply enough to withstand competitors by low-cost leadership. The problem: this commits the organization to large, unwieldy, often inefficient manufacturing operations, and easily duplicated products. Diseconomies of scale, though seldom mentioned, become increasingly important (Hayes and Wheelwright, 1984).

There seems no way out of the trap, because "more of the same" scale-economy reasoning makes the problem worse. Bigger factories, cheaper products, more standardized offerings do not address current market needs. Under attack by more nimble competitors, especially those with lower input factor costs, the strategy of the "standard product" (even in a global context) must accommodate its inherent severe limitations by draconian measures: outsourcing, complex networks of specialized factories, and off-shore manufacturing in pursuit of the chimera of cost leadership (Lipman, 1988). This, in its turn, induces further price competition and still more imitative products.

Even if short term efficiencies are achieved this way, they do not mitigate underlying weaknesses. Worse still, as fixed product design and process become even more rigid, they require an even longer product life cycle for a favorable return on investment, which ultimately invites clones and counterfeiters. The longer the product life cycle, the longer competitors have to match or undercut what may once have been distinctive product characteristics or features. In short, longer life cycles mean increasing risk and vulnerability. Yet large, standardized production cannot meet shorter life cycle needs either. Shorter product life cycles, a faster pace of product development, accelerating technical change and growing market fragmentation put the rigid, unresponsive factories of the past at an ever-increasing disadvantage, despite their apparent low costs and experience curve advantages.

III. The Demands of Global Competitive Markets

A look at marketplace trends will illustrate the inadequacy of the traditional product strategies. Compelling and dominant market trends, visible today in numerous market segments from consumer products to industrial systems and components, include:

Shorter product life cycles -- especially in high-technology markets, "leading edge" products have short lives and are soon superseded, or copied. Some semiconductor products have a window of opportunity only six to twelve months long, instead of the years of unchallenged opportunity typical of most products in the '60's and '70's.

Greater product diversity, variety and complexity -- proliferating scientific knowledge and complex production processes have made possible greater product variety. Even simple consumer products like athletic sneakers, Coke, mustard, or magazines now come in variety unimaginable not long ago.

Systems selling and account management -- custom components and systems, rather than the stand-alone product, offer keys to service customers and create long-term account relationships. As a result, more complex and integrated relationships between vendors and customers will emerge. On the service side, "just-in-time" delivery, after-sale service, and highly responsive customer support can even outsell low price.

Fragmented market segments -- many more competitors and widespread, exponential growth of technical knowledge create frequent opportunities for products designed for specific markets. Hewlett-Packard, for example, introduced 23 different models of electronic calculator between 1972 and 1980. H-P has essentially no competition in top end calculators; the constant stream of advanced features discourages potential competitors.

Sophisticated customers -- who demand customized, better targeted products, and far more responsive support. Since many alternatives are available, now consumers demand fresh ideas, greater variety, better functionality, as well as high quality and low cost.

Vastly higher standards -- quality, price, serviceability, value, reliability, and customer support requirements have risen, even (or perhaps especially) in supposedly mature markets.

As one example, consider the athletic shoe market. Just over a decade ago, the lowly sneaker was a commodity product. Today, a vast array of specialized (and often premium-priced) athletic shoes is available. Finite element analysis, CAD, CAM and other advanced technologies are being used to produce a constant flow of improvements (Compressed Air, 1985). Consumers are willing to pay for choice, style, quality, and service matched to their particular needs. Often, the consumer is buying information, advice, convenience and availability, no less than the design and quality of the shoe itself.

These trends militate against standard products, mass market strategies and the manufacturing arrangement - that supported them. The conditions that used to encourage economy-of-scale technologies, despite their high exit costs, are gone. Thus the marketing strategies that were driven by older technology factories and their limitations no longer reflect either marketplace needs or technological realities. In today's changed market conditions, **standard product strategies are fundamentally misleading and dysfunctional**; they actively discourage the nimble responsiveness required for global success.

IV. CIM-Based Flexibility as a Strategy Driver

Two basic ideas result from the changed logic of CIM, and are essential for understanding the implications of advanced manufacturing technology. First, the new technology is fundamentally different - in design, operation, and capability - from older factory equipment. Factories using the new technology will be "smarter," faster, more closely coupled to vendors and customers, and more highly integrated across departments and business functions than those using older technology. They will not operate like factories of the past.

A second CIM difference is integration, of a different nature from that of the past. Older factories isolated and simplified individual production processes in order to control them. "Integration" was first fostered by buffering uncertainty or disruption to contain it within any single element -- often by WIP inventories. Although this was not true integration at all, it did allow for smooth union of processes. It also brought increased cost and decreased control. Improving various steps or stages as stand-alone operations improved the situation, as did attempts to bring all production operations into closer coordination -- as JIT does, for instance. CIM goes further.

It is relatively easy to envision integration from end to end in the production sequence, as islands of automation first link local sequences and then join them together. CIM factories will also integrate the business, from product design to tool point, through the warranty period and to the marketplace and back again to product redesign. This linkage offers potential to multiply the benefits gained by production sequence links many times over. Integration benefits go well beyond advantages of current "best practice" like JIT and TQC, to tasks impossible, or impossibly difficult and expensive, without computer technology.

Consider a current example: advanced integrated circuits can no longer be economically designed without the use of CAD; the designs are too complex and time consuming for unaided human designers. CAD, simulation and testing enable electronics firms to design complex circuits with high reliability, and do so quickly enough to make ASIC's (application-specific IC's) the hottest growth market in the industry.

CIM-based flexibility permits and even demands radical shifts the stance and approach of a business to its markets and customers, to product design and the pace of innovation, and thereby to vendors and competitors as well. As the heart of the flexibility advantage, CIM technology opens a multitude of strategic doors. Because these advantages are available, they will become telling competitive capabilities; they are quite consistent with global marketplace developments we described earlier. We should expect no less, given the

U.S. experience in steel, automobiles, textiles and consumer electronics. The advantages CIM permits are those that have bedeviled unresponsive U.S. manufacturers during recent decades.

CIM permits flexibility without loss of quality: Any automated factory or production process requires great precision, since computers, robots and other electronic equipment perform only, and precisely, according to instructions. A wide variety of monitoring and control devices can collect a constant stream of data on performance, to be analyzed and used to improve performance systematically, over time. The factory's flexibility permits constant incremental improvements, since process changes in response to product improvement are most often software (not hardware) changes. Updates in product or process design can be quickly and economically incorporated into the data base, and into manufacturing. Moreover, because machines will shut down on error, production will be both reliable and self-diagnosing. Poor quality will not be manufactured.

Flexibility without Loss of Productivity: Effective use and reuse of information gathered in previous operations will bolster flexibility. For instance, simulation and debugging of designs with real data, rather than textbook approximations, will improve designers' ability to insure correct operation the first time through, with no run-in or run-out wastes. Because the simulation will be done off-line, and because switch-over and set-up time and costs are nil with electronic data, short-run or special production can be worked in without loss of productivity. Indeed, such custom work will improve "real productivity" by increasing the likelihood of sale by producing just what the market demands. As some researchers have pointed out, "productivity" is a figment if what is being produced is of poor quality (Garvin, 1987, 1988; Schonberger, 1984), or if it is not what is salable (Meredith and Hill, 1987).

Flexibility affects market strategy: Since flexibility in a CIM-based system is relatively inexpensive, it acquires much greater importance as a strategic tool, enabling manufacturers to exploit and enhance trends toward short product life cycles. This means that "cream skimming," niche and specialty marketing, and custom goods (often with premium returns) will increasingly displace standard product, long life-cycle approaches. However, it also means that managers must conceptualize across multiple potential markets and products to which the firm's flexible capabilities might be applied. This is no small task.

Since the old tradeoffs of quality and price versus variety no longer obtain, consumers will be able to have it all. In general, custom products of high quality and economical price will be available as alternatives to most standard manufactured products. The company that is "fast on its feet" and able to generate a constant stream of new product and process ideas will be into the market, established and poised to move on or improve its product, should copycat competitors appear. In market after market, including "commodity" markets, customers have proven willing to pay for quality or features, reliability and custom benefits. Where these advantages can be made available at relatively small premiums or at comparable prices, competing standard goods will be displaced.

Flexibility affects vendor relations: Because product and process life cycles decrease, vendors become especially important. Their specialty knowledge of the parts or materials they provide can best be incorporated into changing product and process designs when buyer and seller can communicate freely -- a relationship enhanced by time, trust, and a track record of reliable quality performance. These needs, no less than the proprietary or vulnerable character of the information and access required between the partners, and the potential for close coordination so enhanced by CIM-based data links, argues in favor of a well-established, long-standing relationship between buyer and seller. Long-term relations make common data parameters and close links feasible.

Preliminary signs of this change are already visible in the shift from the adversarial vendor relationships (so long typical of the U.S. automobile industry) to closer the ties with fewer vendors visible today. As in the case of Japan today, we believe that the creation and maintenance of unique (perhaps monopoly) buyer-seller couplings is a key ingredient for successful strategic alliances in the 21st Century. Current alliances of this type include the recently announced DEC-Apple arrangement, AT&T's link with Sun Microsystems, and GM-Toyota, among others.

Each of these factors highlights the scope of the departure from traditional practice that CIM demands. Investments in CAD, CAM, FMS and the rest of the alphabet soup of advanced technology will provide acceptable returns only **if flexibility embraces and integrates engineering, distribution, and marketing as well.** Taken together, these factors argue for a shift from "industrial" manufacturing to something far more service-based, whether end users are OEMs or consumers or industrial customers. In this new environment, short production runs -- of highly customized products -- even one-of-a-kind -- are feasible. Indeed, Ingersoll Milling Machine is already operating in essentially this fashion, as is Sun Microsystems, with its potential for 4 million different product configurations.

V. **Exploiting CIM with New Business Strategies**

Worldwide markets, manufacturing and sourcing create unique structural demands. These have, in the past, been addressed most typically via short-term, least-cost considerations. Thus lower direct labor cost was the argument for moving much U.S. manufacturing offshore to the Far East and for moving much Japanese manufacturing and assembly out of Japan. Yet despite impressive figures for hourly labor cost reduction in some manufacturing operations, direct labor is rarely a major factor in competitive cost, however much it is bemoaned.

Direct labor is almost always less than 20% of the cost of most manufactured items (Merchant, 1984), so moving abroad and looking to lower foreign labor costs as the solution to manufacturing woes seems both inadequate and wasteful. Direct labor is under 8% in North America automobile manufacture at present -- not enough to make up the difference in manufacturing cost between the U.S. and Japan, even if all labor cost were eliminated. In contrast, machine utilization alone translates to almost a six-fold difference between Japanese and U.S. automobile manufacturers' asset productivity rates, for instance (Schonberger, 1986; McElroy, 1984). Consequently, focus on direct labor is misleading (Hayes and Clark, 1986; Skinner, 1986).

Nor does moving abroad address sources of much higher proportions of cost -- materials waste, low yields and quality problems, overhead, staff, general and administrative costs, design errors and engineering costs, lower engineering productivity and machine utilization differences, for instance. GM's layer upon layer of staff and administrative personnel outnumber staff at Japanese competitors by more than three to one -- and typically at much higher managerial salaries, not direct labor rates. GM's announced plan to eliminate 100,000 employees in the next two years goes well beyond direct labor, necessarily.

Issues of waste and quality, yield and asset efficiency, staff, overhead, design and administrative costs are addressed by CIM's information intensity. These far more serious structural issues create needs for smooth information flow, and a new sort of organizational structure prepared to respond to the changed realities of global markets and a CIM environment.

1. **Structural needs:** Basic organization structure must enhance swift, accurate, open information sharing, and closely couple distant operations. Sophisticated computer and communications links must unite all aspects of the organization. To utilize factories flexibly, market intelligence must be brought together with expertise in design, process, and raw materials. Information sharing and use must be free-flowing, innovative, and willing. Formal structure must enable specific linkages to form and reform as needed.

2. **Incentives:** where "follow the rules" was the criterion in the stable-state bureaucracies that spring from economies of scale logic, "follow correct CIM principles" is the criterion for the change-embracing CIM-based organizations

that spring from economies of scope logic. The principles of good CIM organization are already quite clearly in view. Incentives must encourage connections and cooperation of the sort managers have long recognized as desirable. Control in the context of innovation is a new challenge for organization theorists and designers. Innovation must be encouraged, while guidance and control are neither abandoned nor exaggerated. People will truly be key resources, for their creativity and insight, rather than as the rules-following functionaries of the past. While the best firms of the past have always treated people as key resources, CIM-based operations accentuate the trend because they rest so fundamentally on exploiting opportunities for creativity and innovation. This requires the development of management approaches fundamentally different from past emphasis on "the one best way."

3. **Strategy needs:** Both the content and process of strategy will change in response to the underlying logic of economies of scope. Worldwide markets and customers, worldwide information, and manufacturing flexibility mandate a new approach to strategy. Vastly increased participation and strategic openness translate to a different sort of strategy-making -- and a different sort of strategic control system. Embracing change means encouraging diversity, not merely tolerating it, and thinking strategically about it as a source of competitive advantage. Deliberate cannibalization of products, truncation of life cycles, and the pursuit of market segments formerly thought to be too small are examples of the new style of strategic thinking made feasible by CIM.

4. **Support needs:** Engineering and product design capabilities, marketing knowledge of changing customer needs, and intelligence about competitors' actions and strategies all become far more important than in the past. Amidst competitive pressure from abroad, multi-company quality improvement associations are emerging. MCC, the Microcomputer Consortium, and SEMATECH, the semiconductor manufacturing technology consortium offer two examples. BAQIN, the Bay Area Quality Improvement Network is another. BAQIN members IBM, Amdahl, Hewlett-Packard, Raychem, Measurex and others meet bimonthly to share ideas for support. A similar WINOC, wak in Northern Ohio Council, provides workshops, quality group programs and simlar assistance to its members.

Inside firms too, broad-ranging mutual support needs are visible across disciplinary and departmental lines. Traditional leisurely product life cycles allowed firms to maintain separate and independently operating engineering, design and marketing departments. Ford's design experience with the Taurus is far more indicative of the future: everyone from engineers, designers and marketers to factory floor personnel contributed ideas and suggestions, 80% of which were used -- and Ford had its best-built car ever, a marketplace hit.

Shorter windows for new-product introduction and greater emphasis on quality require much better cooperation among functional departments. CIM factories will rely on much expanded capabilities in each, together with greater integration among them to lever their expertise into marketplace advances and competitive advantages by innovating repeatedly.

CIM's flexibility creates the potential for embracing change. But change per se has never been enough. The question how to utilize flexibility to create long term, sustainable competitive advantage. This means, of course, that something unique and difficult to duplicate is required. Simply installing the latest equipment won't do it. The competitive CIM organization will offer something unique in its very essence, uniting organizational strengths and resources with customer needs. The key is to embed the product's value to the customer as deeply as possible into the technology and organization of the design, production, and distribution systems, inside the firm, not in the physical design of the product itself.

Rather than basing advantage on a single, easily cloned product feature, or even many such features, effective CIM-based strategy will rely on more intangible and less readily copied characteristics. These systems characteristics offer more defensible and less easily undermined bases for strategic advantage. Low price, high quality or reliable delivery are no longer sufficient, when numerous competitors can duplicate them or undercut them with lower labor costs, for instance. Low cost and high productivity are necessary, but not sufficient for profitable survival and market success.

At minimum, we have argued, speedy new product introduction promises some protection. But even here, new design in a physical or functional sense is no longer the sole bases for advantage (Leavitt, 1983). The basis for producing an extended string of rapid new product introductions is an organization primed and poised for innovation. To know what to innovate requires close links with potential users, good knowledge of distribution, and client followup. Capturing customer goodwill and creating switching costs by building difficult-to- duplicate advantages into the relationship -- not merely into the product -- offers the surest route to continued effectiveness. Just-in-time delivery is an increasingly obvious example today, but so too are software "look and feel," close design and subcomponent production coordination and the like.

Such approaches "augment" the value of the product to the customer, as the product becomes associated with extra convenience, closer suitability to a particular customer, more reliability, or other user benefits not even visible in the product itself. Thus when a manufacturer of drilling bits improved quality a little bit, there was no noticeable market response. When quality was improved further enough to eliminate incoming inspection for customers market share shot up as the product became clearly distinct from competitors' offerings (Leavitt, 1983).

Augmented value can be created through service, quality, variety, delivery conditions, special functionality, customizing, and other intangibles. This constitutes, in essence, a service approach to manufacturing (Leavitt, 1983). It implies deliberately utilizing complexity based upon a detailed knowledge of the product and its manufacture, the customer and the use to which the product is put, and the markets within which the company might compete. This is not the unnecessary complexity of poor systems design, but complexity combined with skills and capabilities that allow the business to handle variety at low cost. With CIM, such complexity need not carry a cost penalty or an operational disadvantage. However, to successfully implement complex strategies, managers must rethink old assumptions, both in themselves and in their organizations.

Three examples will highlight these new demands. First, under old assumptions, the manufacturing function's task was "make it, as cheaply as possible." Manufacturing entered the picture only after designs were set, and often fought a rear-guard action resisting change. Manufacturing and marketing at best would coexist, each committed to divergent interests marketing to change, meeting market demands and urging faster response; manufacturing to stability, cost-containment and avoidance of "unnecessary" design changes (e.g., Shapiro, 1977; Hayes and Wheelwright, 1984). These responses are understandable and rational where change, variety and quality are costly; they make far less sense in a CIM environment, where variety, high quality and high productivity are no longer tradeoffs.

In typical traditional manufacturing organizations, new projects are often "thrown over the wall" from department to department as the project progresses through the stages of initial concept, design, engineering, manufacturing engineering and ultimately into pilot production and regular, high volume manufacture. There is typically little interaction or overlap among stages or departments -- and much room for difficulty, misunderstanding, turf fights and NIH ("not invented here") resistance (Szakonyl, 1988; Wheelwright, 1985). This problem, recognized and bemoaned for decades, acquires far more importance as market changes demand increasing variety, quality and responsiveness. Full exploitation of CIM will emphasize a far more integrated operation of the business -- not just mechanical or electronic integration of steps in the manufacturing process.

The third example concerns quality. Studies in a wide range of manufacturing industries show the increasing importance of quality as a competitive feature (e.g., Garvin, 1984, 1987, 1988; Hayes and Abernathy, 1980). Quality improvements often come from changes in manufacturing engineering: substituting a new material, changing the design of a product, or using a new method to accomplish some step in the process. Although such changes may well be invisible to the consumer, except in improved functionality, they impose a constant stream of changes on the manufacturing process. Resisting seems quite rational when change is costly and difficult. With CIM, changes become easier simply because detailed knowledge about the production process is available. The electronic record of operations can be monitored, analyzed, simulated and debugged off line. Changes become more certain and far less risky.

In each case, abundant evidence exists to show that these advantages can be obtained. Exhaustive information capture at design and engineering stages, and during manufacturing itself - is required. Automatic sensors and computer-driven data capture make the information available. In short, CIM creates the option; changes in management attitude and approach to manufacturing will be required to make the option bear fruit.

VI. Implementing the CIM-Based Strategy

Where to begin, now, to move toward CIM-based strategies? And how quickly can we move? Managers must recognize just how different the CIM-based factory is, and how important its consequences are. To manage CIM effectively, we must begin with excellent control and knowledge over existing manufacturing processes. For this purpose, initially, simplification and close attention to production detail are desirable. JIT, total quality control and zero inventory approaches can help focus attention on the bottlenecks, uncertainties and problem areas that must be resolved in order to effectively utilize flexible automation. (Schonberger, 1982 and 1986, discusses JIT and TQC strategies, although he does not address CIM.)

So far, so good: effective mastery of manufacturing details is a decided improvement over past practice, and a necessary precursor to swift implementation of robotics or other automated procedures. But excellent traditional manufacturing alone is insufficient, when our competitors are racing ahead into more advanced manufacturing technologies. The Japanese are widely recognized as world leaders in robotics, for instance; in a Booz-Allen survey of executives in Japan, the U.S. and other Asian countries, 86% of the U.S. executives and 90% of the Japanese executives cited Japan as the leader (Schachter, 1988). With many leading Japanese factories routinely operating under unstaffed, lights-out conditions, U.S. firms must match Japanese quality and costs or lose market share.

The danger is in moving too slowly under the guise of care and deliberation, or in thinking that "best current practice" will be good enough. The challenge is to go well beyond even the best of today's practices, to tomorrow's best. Any firm that falls behind the leading competitors for very long may not be able to catch up. Should every firm move into CIM? Must every firm be near the leading edge? The questions seem to envision a long transition era of slowly increasing competitions, with numerous protected niches for the less advanced competitors to inhabit, within which profits might be somewhat lower.

In fact, the U.S. manufacturing experience of the past five years in consumer electronics, automobiles and textiles and even semiconductors suggest that no such niches will be available, that they will be of very short duration, that their profits will be markedly lower (because cost differentials may be on the order of 30-50% greater, as in small truck, electronics, pump or automobile manufacture: Walleck, 1985) -- or all of the above. The options of "wait and see" or "proceed cautiously" could prove fatal in interconnected global competition, where some 80% of the U.S. economy is experiencing significant foreign pressure.

Moving slowly may avoid small mistakes, such as non-optimal choice in robots or higher implementation costs. But it may result in far bigger errors, such as being leap-frogged by competition and deserted by customers because of wholly inadequate capabilities. This is a special danger where the firm is doing "better than it ever has" in comparison to its own historical performance - but where worldclass standards have simply outpaced its abilities and awareness. Inventory turns provide an example: where two or three turns per year were pretty good not long ago, and 18 to 20 were very good indeed, today's best approaches 80 to 100 turns -- and it may well not be the same inventory at the far end of the year.

CIM-based operations permit a wide range of product variations with essentially no manufacturing set-up time: tools are changed and new instructions downloaded virtually instantaneously. In design, CAD assistance can permit on-going incremental product upgrades and improvements for 20% of the cost of new design (Hyer and Wemmerlov, 1984). Links between CAD and CAM can cut NC programming time by 60% (Pottorf, 1988) while also eliminating errors caused by faulty data entry. Once NC programming is recorded, of course, even the remaining 40% time at this stage is eliminated, as instructions are simply downloaded. The repertoire of product "recipes" permits quick product mix responses to market changes, improving effective utilization of capacity.

CIM's principal benefits are to be obtained by doing new things, not merely by doing old things better or faster. Thus managers must reconsider their approaches to justifying the investment in CIM equipment. Instead of trying to justify payback in terms of direct labor cost, which is virtually impossible as labor percentage declines, managers might consider the strategic benefits of reducing manufacturing lead time from 12 months to 6 days, for instance, or eliminating defects, enabling the firm to offer strikingly improved warranty terms or enabling clients to eliminate costly incoming inspection.

Complexity and flexibility, so central to CIM's benefits, are precisely the characteristics most foreign to traditional factories and thus to many managers' thinking. It will require considerable effort for managers to recognize the strategic importance of these capabilities, and to translate them into market opportunities. New business strategies based upon these characteristics may well be quite different from those of the past, and thus difficult to initially envision. However, because these strategies seem counterintuitive in the context of the past, those firms that succeed may well enjoy an unexpectedly long window of opportunity, as others struggle to comprehend the nature of their advantage. Such strategies will be foreclosed to those who have not made the necessary investments, both in equipment and in the hard work of rethinking basic assumptions and approaches to competing in the new context.

Managers must also share with their organizations the urgent need for rethinking strategic assumptions and approaches. It is no longer feasible to send strategic directions down from the top, as in the past. Nor is hesitation and resistance to change a viable alternative. Instead, organizations face a choice between changing to meet the paradoxical, contradictory demands of simultaneous flexibility and efficiency, or watching their competitors erode market share and profitability with copycat products, lower costs, and better service.

Without awareness of how great the change is, this urgently needed rethinking may be shirked or delayed -- thus impeding both the decision to move toward CIM and the implementation process itself, after the decision is taken. Both decision and implementation turn on changing underlying assumptions, some of them built into our cost and benefit calculations (Gold, 1982; Kaplan, 1984, 1985, 1988; Meredith and Hill, 1987; Skinner, 1986). CIM cannot be justified by comparison with past practice under past conditions. Justification for advanced systems does not turn on direct labor savings, for instance, for the simple reason that direct labor does not constitute the most significant part of manufacturing costs.

However, past conditions no longer exist, and past practice is no longer a viable option, or a useful guide. Machine tools, where once the U.S. dominated, will illustrate. More than 34% of U.S. machine tools are twenty or more years old, in contrast to 24% for England and only 18% for Japan. Only 31% of

U.S. tools are less than ten years old, where in Japan that figure is 61% (Bylinsky, 1983). During the last ten years alone, machine tools have been essentially revolutionized by the advent of electronics for control and operations monitoring. Japanese and European manufacturers have taken much of the U.S. domestic market.

Reduced leadtime, improved responsiveness and quality, reduced turnaround time and improved capability for handling surges in demand are vital today, as they were not even a few years ago. Failure to recognize the very different nature of competition in today's fast-paced, highly competitive global marketplace is the key barrier to CIM implementation, more important by far than technical problems, cost or manufacturing complexity.

Beyond the obvious acquisition of equipment keyed to flexibility in design, in engineering, and in marketing, as well as in manufacturing -- managers need to prepare themselves and their organizations for a variety of other changes. To prepare effectively for CIM manufacturing, managers should:

1. Begin by rethinking the strategic basis of business success. Asking "What if . ." and exploring answers broadly can generate a clearer picture of business essentials in your industry and help prepare subordinates for change by including them in the thinking process. Their input is important -- and their awareness is crucial to successful implementation.

2. Examine existing systems and incentives for information sharing: where current systems inhibit effective linkages, they are candidates for change. Other companies may offer useful examples, but pay most attention to the real information sharing needs your own people identify, as the sources of friction or bottlenecking in moving new products or processes to efficiency.

3. Aggressively apply information to improving performance, by focusing information on problems. Begin with operational problems, but actively move to build useful complexity and augmented value into your relationship with customers.

4. Build flexibility and integration knowledge. As flexible capabilities improve, seek improvements that obsolesce or cannibalize your own products, enabling you to get closer to targeted client needs and farther ahead of the competition.

5. Hone people's skills to think creatively, to solve problems by increasing integration, flexibility and responsiveness, and to exploit technology capabilities.

6. Focus strategies and product line extensions around economies of scope and flexibility by preference, not economies of scale and rigidity as in the past.

7. Use flexible equipment, advanced employee training programs, innovative attitudes, adaptive organization structures and change-friendly systems to embed product differentiation within the organization into the way you do business where it is difficult or even impossible for others to copy.

VII. Summary and Conclusions:

CIM is a large scale, discontinuous innovation involving significant changes in manufacturing technology, operating style, organizational arrangements, strategy and relationships among organization members to say nothing of the organization's relationship with its markets. True CIM requires integration of these changes so that they may freely interact not merely packaging a few pieces of flexible equipment together, or independently optimizing various current business functions in isolation. CIM goes beyond FMS to integrate business functions, not just production activities. This integration permits people to bring vast amounts of information to bear, to direct and improve business activities at every level.

Industry is at the beginning of the learning curve for CIM, so there is much to learn. Many potential benefits require careful advanced planning and implementation. As with any large-scale, truly new endeavor, mistakes are being made, and these are well-documented. But many of them are FMS mistakes, or even old-style hard-tooled automation mistakes. Often, mistakes are in implementation -- such as failure to include users early in the the specification process -- not in the CIM concept.

In addition, in many situations where expectations have not been met, companies have adopted only part of the CIM technology, and have not truly integrated their systems. Attempts to operate advanced equipment on the old assumptions of "deferrable" maintenance or trial-and-error debugging have also caused failure. So, too, have poor choices of technology, or equipment simply not connected. to needed data. We should not assume that future users will make the same mistakes; the history of technological change suggests otherwise.

Even where FMS (not CIM) has been implemented, often the equipment may not be used flexibly (Jaikumar, 1986). The failure here is not in equipment, in implementation, or even in operation. It is, instead, a failure of management imagination and strategy, failure to see any need for exploiting the capabilities inherent in the equipment, and misguided complacency that a quick "technological fix" will be sufficient. Failure to exploit lower cost, increased flexible and shorter lead-times are strategic and tactical failures -- not errors attributable to the CIM system itself, or its principles.

In some instances, even partial and preliminary efforts have produced extraordinary results, transcending initial difficulties. Sun Microsystems brought an advanced facility on line in June of 1986, and achieved full production by October 1986. Sun's system is capable of producing over four million different combinations of circuit boards, monitors, and other peripherals. In second quarter 1987, the new facility produced over 6,000 units; before the new system, "In late 1985, we struggled to ship less than 2,000." Increased volume was accompanied by dramatically improved quality, and inventories lower by half as a result of improved tracking and control capabilities (Krepchin, 1987). Such successes may not be "complete CIM," but they clearly point the way. The point of CIM is not "automation," but "integration with flexibility," the creation of the data-driven factory that links CAD and CAM and to other business functions.

There are, of course, alternatives to CIM. Managers can focus factories on standard products, source offshore, attempt to drive inventory risks back onto suppliers, or subcontract. Each offers some limited benefits, and increases flexibility somewhat in the short term -- but each also carries serious long-term limitations, such as less control over quality, design and schedule, and far more dependency upon distant others.

Subcontracting, for instance, whether at home or abroad, removes the manufacturing requirements to the subcontracting firm -- but means that key designs must be shared, proprietary insights revealed to others, and potential for incremental improvements lessened. Often, manufacturing lead times are also expanded.

Each of these alternatives also moves directly away from the tight integration across business functions and departments that is so characteristic of CIM -- and so crucial to fast, successful new product introduction. Repeated innovation and constant product changes seem essential for competition where savvy adversaries often copy leading products quickly. Some at least -- among them Intel, IBM, and DEC -- have decided that keeping proprietary information inside and managing tight new product introduction windows are benefits well worth the manufacturing effort required. Our conclusion? **No alternative offers the benefits of simultaneous quality, speed, flexibility and reliability of CIM.**

You can't buy CIM, although its components, CAD, CAE, CAM, FMS, are for sale. CIM must be uniquely created for each individual business out of a whole set of managerial decisions that mutually reinforce one another to produce the integration that permits CIM. New competitive strategies, new organizational approaches, and revised thinking are all required to take advantage of intelligent automation technology. Effective CIM is based on information use, in a customer-oriented, service- based approach to manufacturing. Attitude and strategic approach are at least as important as equipment. For example, just- in-time design and custom manufacture offer value-added to the customer but they are the result of process organizing capabilities, not product design or factory equipment.

The demands of highly competitive global markets offer both threat and opportunity. They constitute threats to traditional, rigid organizations and manufacturing arrangements. They offer significant opportunities to those who can understand the benefits of CIM flexibility, quality, and efficiency. Many factories today still run as if they were dependent on direct human intervention for flexibility. Even supposedly advanced factories, like Mazda's, for instance, typically seek elimination of variety as the source of quality and productivity gains, and picture flexibility and productivity as opposite poles in a fundamental tradeoff. A simple graph drawn by a Mazda manager, with productivity as the vertical axis and flexibility as the horizontal axis, highlights the dilemma: machines are typically positioned high and to the left (productive but inflexible); people are low and to the right (flexible but not very productive) (Poe, 1987).

Such a view suggests direct human intervention as the sole source of flexibility, and the low-cost alternative to the limited capabilities of automation. This view is woefully outdated, and dangerous in its failure to adequately assess the programmable automation and CIM. The programmable technology available today and increasingly characteristic of modern manufacturing is fundamentally different from the so-called "hard automation' (sometimes called "Detroit automation") of the past. CIM is a long term strategic investment that provides the key to both the productivity and the variety needed for sustainable competitive advantage in the 21st Century.

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INDUSTRIAL CORPORATE RESEARCH: PERSPECTIVES ON INNOVATION

R. Pariser

Introduction

A question which is frequently asked is the following: what is the purpose of a corporate or central laboratory in a large, technologically oriented company, such as the Du Pont Company? And, how does such a laboratory pursue its goals?

As the title implies, our emphasis will be on innovation. By innovation we mean the process of discovery and development of technology which results in success in the marketplace and, hopefully, is of benefit to society[1]. We will focus on the role which a corporate laboratory has in this process. This role is normally, but not exclusively, played in the initial stages of the process. This role is normally, but not exclusively, played in the initial stages of the process. We will comment on motivation toward discovery and, in particular, on the transfer of technology from the corporate laboratory towards the first stages of development. We will also consider the conditions which appear to lead to successful technology transfer, as well as to successful further business development.

Du Pont's Research Organization

As background, we will first present a brief overview of Du Pont's research organization.

The total R&D budget for the current year approaches $1.3 billion. The general aspects of this budget are shown by an analysis of the 1984 budget, as summarized in Fig. 1[2]. As can be seen (Fig. 1a) the R&D budget relates to a very diverse scope of businesses, ranging from life sciences to petroleum exploration. About one-third was devoted to new product development (Fig. 1b); some 19% of the budget was in basic and discovery type research (Fig. 1c), and the majority of R&D, some 85% was performed by the by the business departments (Fig. 1d).

Fig. 2 provides a broad overview of the current R&D organization. The departments shown there are engaged in research and development. All, except for the Engineering Department and the Central Research and Development Department (CR&DD), represent complete business units including manufacturing and marketing operations. Our emphasis will be on the CR&D Department, which is funded primarily by corporate funds and only to a minor degree through direct support by the business departments for specific, mutually agreed upon projects.

The CR&D Department includes a Research and a Development Division. The Development Division strives to initiate new enterprises in markets where the Company does not already have established businesses.

The Research Division operates overwhelmingly in the realm of basic and discovery oriented research. Most of the basic research in the company is performed by this division, as well as a substantial part of the discovery research. Basic research deals with the generation of fundamental scientific understanding of pertinent technology. Discovery research is aimed toward a defined, potentially useful goal. Basic research can be, but is not frequently, the direct precursor of discovery research.

The science areas of research in the Research Division are mainly in chemistry, physics, and biology. Major technological areas of emphasis include advanced materials and biotechnology, both of which cover a broad scope and represent prime growth areas for the Company's businesses. For example, research in advanced materials includes specialty polymers, composite materials, ceramics for electronic packaging, superconductors, and electronic materials for optoelectronic communication and information storage. These research efforts interact with the R&D Divisions in the business departments by transferring new technology to them.

Corporate Research

What are some of the important characteristics of a central or corporate research laboratory? These may be listed as follows:

* It provides a research environment which is well protected from immediate business pressures, thus allowing uninterrupted concentration or longer range, strategic objectives;

* It attracts high quality scientists, both as permanent staff and as visiting scientists from academic institutions;

* It is effective in discovering new, potentially attractive technologies for the existing businesses, and especially for new business opportunities, with the aim of securing a competitive advantage;

* It provides the "scientific underpinnings" to a great variety of the company technologies, ranging from the existing businesses to new enterprises and joint ventures and acquisitions. Thus, it helps insure the firmness and dependability of the technical foundation;

* Since much of the research is "discretionary" it provides a pool of talented scientific manpower for rapid deployment and concentration when an important research lead is uncovered;

* It serves as a major interactive interface with university and government basic research efforts, thus facilitating potentially valuable knowledge transfer into the company;

* Through frequent publication of scientific results, it enhances the reputation and good will of the Company by contributing to the store of scientific knowledge.

These characteristics are the essence of the primary strategic mission of Central Research, namely, to provide the scientific base for a corporate strategy which strives to "pioneer" new technologies and products into the marketplace. This is to be distinguished from a business strategy which may be described as that of a "follower" or a "me too" type[3].

For a successful pioneering type strategy, one must be able to realize or recognize potentially important scientific discoveries, to proceed effectively and rapidly in developing such discoveries, to secure strong proprietary positions, and to have the continuing (and often times very patient) support of corporate and business management and its willingness to risk substantial new investments. A central or corporate laboratory is an effective means for insuring the implementation of the above objectives.

On the other hand, the "follower" or "me too" type strategy is based on entering the market after the "pioneer" has already created a market for the new product[3]. "Follower" and "me too" strategies normally place great emphasis on process and value engineering to attain lower costs and/or higher quality than the product first introduced by the "pioneer". Thus, these strategies seek to develop and extend the technology rather than to achieve new scientific discoveries.

Innovation in the Corporate Laboratory

Innovation, as it proceeds from research, may be thought as being motivated by either of two ways[1]: exploration-driven research or market-driven research.

To quote from Forney:

> "Exploration-driven research* has as its chief goal the advancement of scientific knowledge. When pursued by a corporation, such research entails considerable risk because there are no assurances that it will ever offer ideas or leads for commercial development, much less bring them to fruition. This research requires considerable investment in time. It is also very expensive ... However, the potential economic rewards from exploration-driven research are tremendous. Such research ... can produce leads to dramatic and revolutionary developments and a subsequent strong proprietary position in very marketable products".
>
> "On the other hand, market-driven research - often keying away from an exploration-driven program - has been very successful at producing useful materials ... Market-driven programs are not necessarily less expensive ... but they are more conductive to planning, ... Such concerns as ultimate use, cost structures, distribution and integration into existing systems are taken into consideration ... because the goal of the research program is understood in advance".

In other words, market-driven research is in harmony with an already existing business plan or strategy, whereas successful exploration-driven research may in extreme cases necessitate the creation of an entirely new enterprise and business strategy.

The concepts of market-driven and exploration driven research are useful in assessing the probability of successful innovation derived from research, the first step of which is technology transfer out of the research laboratory.

Other studies[3] have indicated that the success of new product introduction depends strongly on the following three criteria:

- A Strong proprietary position, as exemplified by a strong patent position.

- A marketing position, as exemplified by a customer base with which the Company is already engaged through existing businesses.

* We have used the term "exploration-driven" in place of "discovery driven" which is used in reference[1]. This is done to avoid confusion with discovery type research as defined in the section entitled Du Pont's Research Organization. Discovery type research can be either exploration- or market-driven.

- A manufacturing position, as exemplified by experience with similar manufacturing processes; a situation where development quantities may be produced with a minimum of new investment capital is especially advantageous.

Market-driven research is more likely to meet the last two of the above criteria than is exploration-driven research. Thus, the risk is generally less for attaining commercial success derived from market- driven research. The attainment of a strong patent position, on the other hand, is more likely in exploration-driven research, where there is normally less prior art and competitive activity.

It is not surprising that a high proportion of Du Pont's R&D in the business departments is market-driven. Such research is essential in maintaining the health of the existing businesses. By contrast, a relatively higher proportion of research in our Central Research Laboratory is exploration-driven. As we have already implied, the risk for successful innovation is higher in exploration-driven research. Initially, such success depends strongly on the effective transfer of technology from the laboratory into development and marketing.

In the following section, we will discuss briefly the case histories of four recent developments which originated from research in advanced materials and which illustrate the concepts which we have been presenting.

Exploration and Market-Driven Research: Four Examples

The history of Du Pont is replete with successes of exploration-driven research. For example, this year marks the 50th anniversary for nylon and Teflon® fluorocarbon resins, both being prime examples of highly successful exploration-driven research. Similarly, market-driven developments, such as engineering plastics and packaging films, have had a major impact on the way we live.

However, rather than reviewing these past successes, we chose to concentrate on some recent examples where the outcome is not yet fully known but the developments are timely and reflective of present conditions.

Since, in view of our preceding discussion, marketing position is a key ingredient for success, we have chosen four examples as illustrated by the matrix below:

	MARKET-DRIVEN	EXPLORATION-DRIVEN
EXISTING MARKETING POSITION	GREEN TAPE ®	GROUP TRANSFER POLYMERIZATION
NEW MARKETING POSITION	ADVANCED POLYMER COMPOSITES	HIGH TEMPERATURE SUPERCONDUCTORS

Increasing Risk (↓)

Increasing Risk (→)

Green Tape®

Ceramic multilayer interconnects are of prime importance in high performance electronic systems. These multilayered structures consist of stacked ceramic sheets, each supporting electronic circuitry. Normally, the ceramic sheet is first prepared by sintering a ceramic powder. An ink of conducting or resistive components is then printed on the ceramic and the product is fired again to produce the electronic circuitry. This process is repeated for each ceramic layer. Finally the multilayer package is assembled and electrically interconnected through holes or vias drilled in the ceramic layers.

Green Tape® is a newly introduced product developed by the Electronics Department to provide a more economic and versatile process for ceramic multilayer packages. The product consists of a tape composed of ceramic particles and other ingredients dispersed in a polymer-based binder. The customer prints electronic circuitry onto this tape, punches the necessary vias, assembles the tape into a multilayer structure, and then fires the assembly to produce a finished ceramic multilayer package in a single firing step.

The product concept, based on several novel extensions of existing technologies, was clearly market-driven. To help with future product development and to define the next generation of products, a team was formed which was coordinated by the Central Research Laboratory and which consisted of scientists from Central research and other departments, each contributing their respective expertise. First-line supervision and strong leadership were provided by a supervisor "on loan" to Central Research from the Electronics Department.

The success of this program fulfilled the criteria already discussed: an existing marketing position, as well as polymer technology and manufacturing capability for the binder. A good patent position was achieved. This development also illustrtes an important factor which contributes to successful technology transfer, namely, the team approach insures close personal collaboration between the basic science and the product development.

Advanced Polymer Composites

The development of advanced polymer composites was market-driven by the need for very light weight and strong materials, initially for aircraft and aerospace applications[5]. the feasibility of such composites, however, was made possible by an exploration-driven discovery, namely, ultra-high strength/modulus fibers, e.g., Kevlar® aramid fibers. Advanced composites are based on high modulus fibers, such as Kevlar®, carbon or glass fibers, imbedded in polymeric matrix.

Having a strong position in Kevlar® and in polymer technology, Du Pont's Textile Fibers Department decided to manufacture and market advanced composite parts and systems. A new venture organization was established in the Department, and an existed composites systems business was acquired.

The role of the Central Research Laboratory was to provide basic scientific knowledge to aid in the discovery and development of improved fibers, matrices and composites. To augment this effort, scientists from the Textile Fibers Department were transferred to Central Research. The Advanced Polymer Composites business illustrates a situation where existing strengths in technology and manufacturing were rounded out by purchase of a composites parts business. The role of the Central Research Laboratory was to provide the "scientific underpinnings" to a new, market-driven enterprise. The physical presence of Textile Fibers scientists in Central Research facilitated the effective transfer of technology to the business.

Group Transfer Polymerization

Group Transfer Polymerization, or GTP, was an exploration-driven discovery made in Central Research by Dr. Owen Webster in 1979[6]. GTP is an entirely new polymerization method applicable to certain acrylic monomers, e.g., the commercially important monomer, methyl-methacrylate, or MMA. MMA is polymerized commercially by a "free radical" method, which is the basis for products such Lucite® acrylic resins, Corian®, and many coatings and finishes for the automotive industry.

The discovery of GTP was motivated by the desire to exercise more control over the structure of polymers than possible by the free radical route.

Previous exploration-driven research had shown that a high degree of structural control was possible by an anionic polymerization route performed at very low temperatures, e.g., -78°C, however, such polymerization conditions were not deemed to be economically attractive for commercialization. On the other hand the discovery of GTP provided a potentially economic route with even better and more versatile structural control.

The Central Research Laboratory was able to very quickly assemble a task force of approximately ten principal scientists to understand and exploit the scientific breakthrough as represented by the discovery of GTP. In addition, the businesses which utilize MMA-based products - recognizing the broad potential but, as yet, undefined impact of GTP - placed several of their scientists into the Central Research Laboratory task force. As is characteristic of exploration-driven research, a very strong patent position was attained, and leads to a number of potential new products were defined. As further development was implemented by the Business Department, the scientists who were assigned from the business returned to their home laboratories. Thus, the center of gravity of the research and development effort shifted to the Business Department, while a much smaller, basic research effort remained in Central Research. Several new products based on GTP have been commercially introduced.

GTP reproesents an exploration-driven innovation in Central Research which happened to fit into an existing marketing position, although the business plan was modified to exploit the opportunities presented by GTP. A task force was quickly assembled, and a strong proprietary position was achieved. Successful technology transfer was greatly enhanced by the movement of technical personnel into the Central Research Laboratory and back into the business. It should also be mentioned that this development received the early, strong support from the top management of the concerned Business Department. Such support is essential to the success of any new venture.

High Temperature Superconductors

Our final example deals with exploration-driven research for which there is little, if any, developed marketing position. As already mentioned, successful research of this type can have a revolutionary commercial impact, but the risk is also very high. Successful examples include the discovery of nylon and Teflon® fluorocarbon resins, the discovery of the transistor, and the discovery of DNA and the genetic code. The recent breakthroughs leading to superconductivity at temperatures above the boiling point of liquid nitrogen[7] is clearly exploration-driven research. As is characteristic of this type of research, it might lead to new, revolutionary applications and markets.

We will discuss briefly the impact and response of Du Pont's Central Research Laboratory to these recent discoveries. By way of back-ground, it should be pointed out that Du Pont's research interest in superconducting oxides dates back to the mid-seventies when, in fact, a precursor of the current family of high temperature, copper oxide- based superconductors was first discovered at Du Pont[8]. Also, Du Pont has well established businesses in related technologies, such as in powdered metal oxides, as exemplified by titanium dioxide and chromium dioxide, as well as in aluminum oxide-based fibers. Expertise in solid state chemistry and physics was already present in Central Research, as well as in several of the business departments.

Soon after the announcements of the new copper-oxide-based superconductors, the Central Research Laboratory was able to quickly assemble a relatively large research team, mainly by diverting effort from other exploration-driven projects. Since there was no existing marketing operation in the Company in the field of superconductors, a business group was established in the Development Division of the CR&D Department to develop a business plan and to explore potential applications. The membership of this business group was composed from personnel of those business departments which have a potential interest in superconductors. In addition, several of these business departments have also initiated research in their own laboratories and in their respective areas of expertise, augmenting the more fundamental studies in Central Research. At this early stage, the overall research effort is coordinated by a committee representing the active departments. Although products and specific uses are not yet defined, the seeds for effective technology transfer have already been planted.

This example of exploration-driven research in high temperature superconductivity clearly illustrates the function of a central or corporate laboratory in helping to pioneer an entirely new business opportunity.

Conclusion

In the preceding discussion and examples, we have tried to put in perspective the innovation process in a large corporate research laboratory. Some general conclusions are the following:

* A central or corporate laboratory which is "protected" from the short range demands by the business, appears to be essential to implementing a corporate strategy which places a high priority on the pioneering of new, technologically based businesses.

* A corporate laboratory provides a beneficial climate for scientific and technological discoveries, however, breakthrough discoveries may occur anywhere where research is done[9]. A corporate laboratory provides skilled resources for rapid deployment and follow-up.

* Both market-driven and exploration-driven research can lead to profitable innovations. Exploration-driven innovations usually have a lower probability of success, but if successful, their impact can be tremendous.

* Successful technology transfer from the central laboratory to the businesses is the essential first step in the chain of events which leads from research discovery to useful innovation.

It is greatly facilitated if the new technology fits into an existing marketing fits into an existing marketing position and business plan.

A proven mechanism for transferring technology is by the appropriate interchange and movement of technical personnel between the central laboratory and the business organization.

Fig. 1 Du Pont's Research and Development Budget, 1974
Budget $1.05 billion (5.7% of Sales)[2]

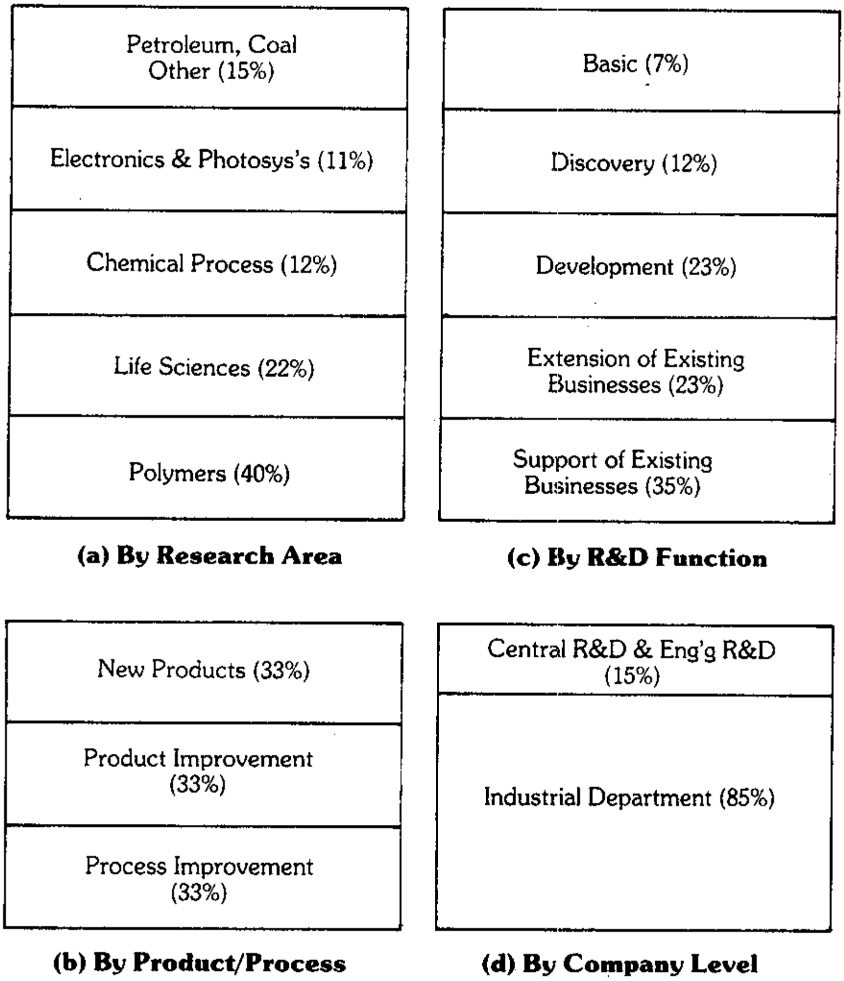
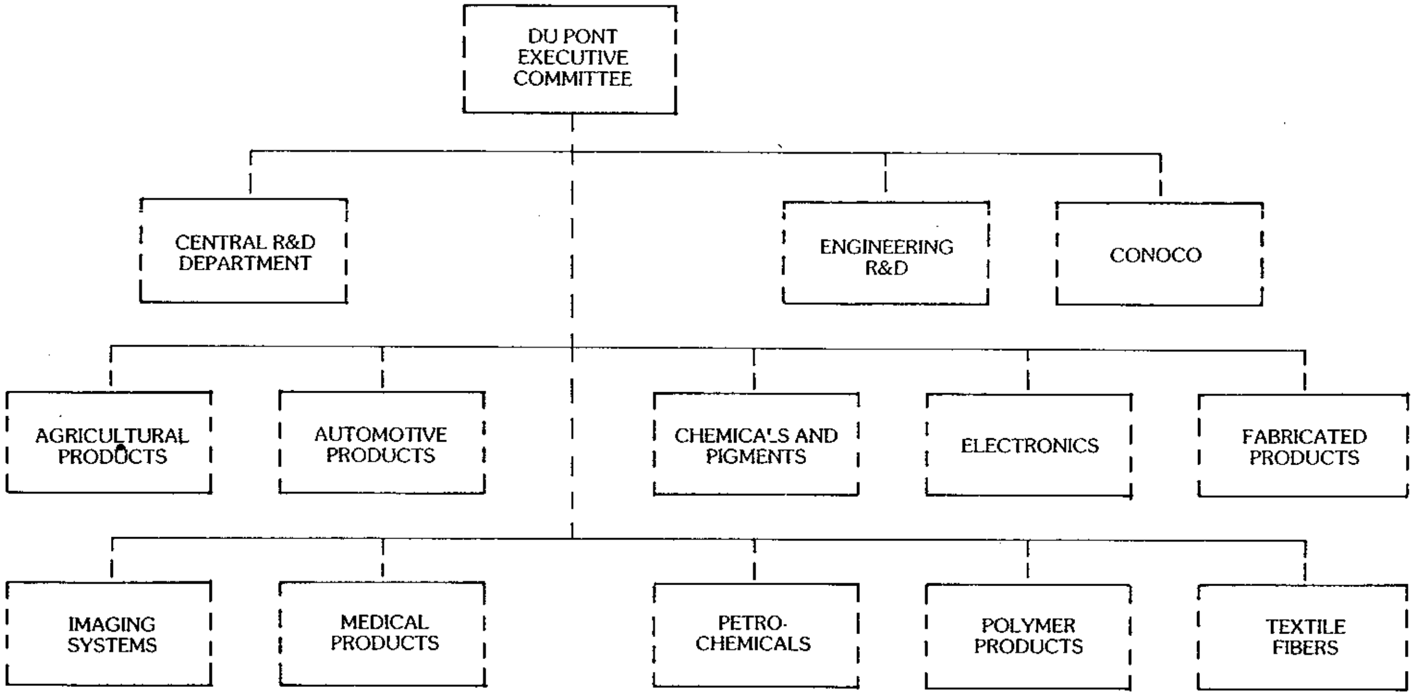


Fig. 2 Du Pont's R&D Organization



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DEVELOPING A MARKETING PROGRAM TO INITIATE SALES OF A NEW TECHNOLOGY PRODUCT
Six Purchase Decision Influences and Corresponding Marketing Strategies

M. G. Frantz

New technology products create a double-edged challenge for the marketing professional. First, the buying influences may not be as clearly understandable as they are when market research is conducted for a new version of an existing product. Second, while all good marketing strategies are creative, those developed for new technology products must be especially creative if they are to result in those critical first sales.

How do you prepare a marketing plan that will convince a potential buyer to purchase a product that doesn't appeal to a readily identifiable perceived need? How is the buyer convinced of the value of a product for which there are no comparative yardsticks? How do you overcome buyers' uneasiness (and perhaps even fear) about an unfamiliar technology intruding into their lives?

The first part of this article will address these questions and will specifically identify those factors that may be influencing the potential purchaser of your new product. In the second section, suggestions will be given for specific responses to these buying influences that will help generate those tough first sales orders. This discussion will not include a review of product design, pricing, distribution channels, or market research techniques.

In order to illustrate the points presented below, I will give examples related to the introduction of the first whole-body CT scanner in the mid-1970's. I was closely involved in the initial marketing and sales of these devices at the time and I believe the lessons we learned are applicable to many recently developed technologies in the consumer and industrial fields. The CT scanner is now a familiar medical diagnostic tool, but less than 10 years ago, it was a grossly oversized, donut-shaped mass of electronics intruding into the radiologist's black-and-white world of film-based cameras.

In brief, the CT scanner produces electronically reconstructed images of cross-sectional planes of the body. The patient is placed within the scanner's gantry opening, where an array of pinpoint radiographic beams sweep transverse planes of the body as the gantry rotates 360 degrees around the patient. A computer analyzes the variations in absorption of each beam as the gantry rotates, making possible the definition of a particular body part (such as the lungs) while eliminating the interference of a masking structure (such as the rib cage).

The first step in converting potential buyers for any product area is understanding those factors that will influence the purchase decision-making process. I have classified six influences as critical to the introduction of a new technology product: perceived need, technology gap, perceived value, ease of assimilation, fear, and outside influence.

Perceived Need

The first buying influence is the actual perception of need by the potential purchaser. Apparent or conscious needs are easily addressed, but more often in a new market/new technology situation we must deal with latent needs. The potential buyer may have some unfulfilled desires, but these are deeply submerged, often due to preconceived ideas about the limitations of technology.

For example, before the introduction of the CT scanner, radiologists were limited to picture-taking via a photographic emulsion system that was activated by a radiation beam. New product development centered on more sensitive films and user-friendly black boxes, but this system could not address the basic limitation of structure masking. Therefore, physicians were not consciously looking for a device that would allow visualization of the internal organs because it did not seem that this was possible.

Technology Gap

The second buying influence is the technology gap that may exist between your potential buyers' understanding of the technology that you are going to ask them to use and the actual technology itself. This factor is often overlooked - even ignored - in many nonindustrial fields, the rationale being that the buyer really doesn't have to know how something works, he just needs to know what it does. However, this influence, which is closely related to the first, must be dealt with head-on, and not lost in the marketing approach to other issues.

The technology gap between the state-of-the-art and the buyer's perception of current technology can often represent a stumbling block to sales, particularly if the new technology is based on a discipline outside of the experience or on the educational background of the buying group.

With the CT scanner, we were faced with a significant technology gap. We were going to ask physicians, who understood radiography as being analogous to photography, to radically alter their perceptions of this method of diagnosis. Now, instead of photographs, we were expecting them to grasp the concept of computerized reconstruction. This was a technological development that with some explanation, would be comprehensible to mathematicians and computer analysts. However, most physicians had no such preparation for the notion of computerized reconstruction.

Perceived Value

The perception of true value is the nuts-and-bolts influence that determines whether or not somebody actually signs on the dotted line and spends money to buy your product. He must perceive a meaningful return benefit for the cost and aggravation of accepting a new product.

When the product is a variation of one that is already being used by potential purchasers, they have a basis upon which to compare its inherent value and relative cost. But with new technology, the potential buyer often cannot perform that comparative exercise easily, and the burden of providing value is on the marketer. If the product is based on totally unfamiliar technology, this burden becomes even greater, because in this situation, the buyer must be induced to accept a value for a need that has not been defined before. Moreover, the buyer must accept this proof so completely that the acceptance overcomes the natural inertia of a content body.

With the CT scanner, we had to find ways to make sure that radiologists perceived its true value because we knew we couldn't expect them to spend $500,000 on a product they couldn't completely justify in their own minds. This exercise was further complicated by a "chicken-and-egg" problem: it would take substantial clinical usage of the CT scanner to demonstrate all of the practical applications of this new technology, and this, in turn, was evidence of its value.

Ease of Assimilation

The fourth buying influence, ease of assimilation, is another factor that I believe is too often ignored, but it is crucial to effective new technology marketing.

When any new technology enters someone's life, there is a certain amount of displacement of the environment that inevitably occurs. Also, behavior - the familiar and comfortable old ways of doing things - must be changed. This issue is obviously more important when the new devices are expensive and complex, or when the new product must be used interactively with devices already in place. However, this principle holds true even for something relatively inexpensive and simple to use - as a microwave oven coming into the kitchen.

When we began talking to radiologists about the CT scanner, we were acutely aware that its purchase would mean major environmental changes. Installation of a scanner requires space accommodations that, in many hospitals, means the breaking down of several rooms to accommodate the scanner itself, as well as, the earmarking of several other rooms for the computer and the personnel who are needed to run the whole set-up.

Use of the CT scanner also requires retraining of the radiologist and the education of a whole new breed of technicians. In fact, new text-books had to be prepared to pictorialize transverse (or cross-sectional) anatomy.

Fear

The fifth buying influence is fear - of the unknown, of change, of making a costly mistake. Everybody is afraid of making a mistake by buying an expensive piece of new equipment - such as purchasing a computer for which no new software will be written. On a more fundamental level, humans are wary creatures. We have an innate psychological aversion to that which is completely unknown. And most people have a definite reluctance to change for its own sake.

The CT scanner frightened potential buyers on all of these fronts. This was an unknown and - to them - unproven technology that raised several important questions we had to address. This costs half a million dollars; what if it doesn't work? If it does work, how can I be sure the device won't undergo further development and become obsolete in a year or two? Will it change my life? If I'm not as good at interpreting these new color pictures as I am at interpreting good old black-and-white x-ray film, will my livelihood be in danger?

Outside Influence

The final factor is outside influence. This can be of no importance in some fields, and of great importance in others.

Outside influences can come from virtually anywhere, including the buyer's associates and family, the political climate, and the prevailing public opinion on a certain topic. No matter where the influences come from, the greater the new technology departs from the norm, the greater the marketing problems they can create.

At the time of the initial marketing of the whole body CT scanner, Senator Ted Kennedy was campaigning as a presidential candidate. As head of the Senate's health subcommittee, he had hit upon a platform issue that would gain him a great deal of publicity - the soaring costs of health care in this country. It was easy to take political pot-shots at the new CT scanner business because of the device's cost and the public's unfamiliarity with the cost/benefit ratio. As a result, we had Congress literally fighting against us as we were trying to sell scanners.

Other outside influences to be dealt with came from the hospitals. Aside from the cost of the scanner itself, the hospital was going to have to spend another $200,000 or so to create the necessary 2,000 square feet of space for the device. The administrators were also going to have to deal with an understandable reluctance on the part of the various medical department heads to give up precious space from their already tight quarters.

The radiologists' surgical colleagues were also an adverse influence. Where radiologists were concerned about whether the CT scanner would work well enough, some surgeons were worried that it would work too well, thereby reducing the need for exploratory surgery.

The Solutions

The marketing professional must understand each one of these buying influences and attack them head-on in order to convert the potential buyer into an innovator. Once these influences are defined and understood as they relate to the product in question, then creative marketing strategies must be developed that respond to each purchasing decision factor.

The following points may overlap at times, but I find it helpful to consider them separately, even if the actual implementation encompasses several messages. In this discussion, the communication techniques, form, and media are not reviewed, as these are unique to each industry.

Define the Need for the Buyer

Clearly, the first marketing goal is to establish the need for the new product in the mind of the prospective buyer.

Show the buyer how this product can change his life and benefit him. Use specific examples, and don't assume that the buyer will be able to draw his own conclusions about his need for the product. From my own experiences, I have learned not to assume that consumers have a sense of imagination

- even those who are highly educated. Therefore, don't be reluctant to spell out the need and corresponding benefit.

In order to reinforce this message, pre-seed several opinion leaders, or representative buyers. Then get the message out about how these leaders feel about the product and the benefits they've derived from specific applications. The computer industry has used this technique quite successfully. For example, they convinced leaders in the field to use their new software product, and then specifically state their experience: "I saved my company a great deal of money by using this program that helped me track my inventory."

Applied to the CT example, we trained several respected radiologists in the use of the scanner, had them use the device on a number of patients, then publicized their reports.

Close the Technology Gap

While I agree with those who say that a housewife doesn't have to know a great deal about printed circuit boards to use a microwave oven, I don't believe that the technology gap should be ignored, or even considered to be merely a secondary issue. In my opinion, that house- wife has to understand enough about microwave technology to be comfortable with using this device to prepare food for her family. As far as possible, relate this new information to the target customer's current knowledge base.

In marketing the CT scanners, we began our explanation of the new technology by comparing and contrasting the principles involved with those of conventional x-rays. We used models, and simplified reconstruction formulas to explain how the scanners operated.

Clearly Define Value

The third marketing goal is to clearly define the value of the new product for the buyer. The best advice I can give here is to be completely candid. Provide him with complete information regarding the alternative technologies and the comparative prices - including any hidden costs.

Make the presentation credible. If the product has any drawbacks, be up-front with this information. Don't let it come as a surprise. In order to reinforce the value, use the opinion leader testimonials described above. In addition, try to position the final arguments to appeal to the buyer's strongest motivational factor (greed or prestige, for example).

In approaching radiologists, since there were no previously existing CT scanners, we used the standard x-ray machine as a basis for comparison. Though the costs were vastly different, we stated these differences, and then explained what the scanner could mean to the radiologist in terms of increased revenue by displacing exploratory surgery, and in terms of the overall cost savings to the medical system.

Provide Comfort Level Assurance

To improve the comfort level to the buyer, develop ideas regarding how he might ease product assimilation into his world. This is an obvious strategy, but you need to address it - and address it well.

Deal with training and educational issues, and make sure that the buyer believes you are going to provide the services necessary to keep that product working, and that there will be continuity of service. Another strategy is to provide some hints on how the purchaser can minimize or avoid disruptions to the environment.

For example, in marketing the CT scanner, we worked closely with the hospital administration on the issue of environment. To deal with the inevitable objections about space limitations, we went into the hospital with designers we hired for this purpose and did site drawings, working out ways to fit the CT scanner into the designated area without taking too much room away from other departments. We found that the more we did, the easier it was for the radiologists to convince other members of the purchasing committee that the CT scanner could be accommodated. This strategy paid off in sales.

Help the Buyer Overcome Fear

The best way I know to get maximum input to the potential buyer as well as helping him overcome his fear of innovation is to provide a demonstration, allowing ample opportunity for some actual hands-on experience.

This is a costly prospect, however, and some companies simply can't afford to engage in such a program. If this is the case, the technique of pre-seeding that is so useful in helping to establish need can be used effectively in this area as well. Place your product with opinion leaders, and get them comfortable with it. The comfort level will trickle down.

The pricing policy may also be designed to allay the fear of obsolescence. Lease/purchase options soften the nightmare image of making an irrevocable, expensive, and unsound purchasing decision.

A creative strategy that has been used in the CT scanner business may be applicable to other high-cost products. One company put CT scanners on tractor-trailers and sold them to several hospitals, which shared the cost. In some instances, the hospitals' radiologists owned the equipment. This approach, which afforded the physicians many tax advantages, provided an obvious motivational push. Finally, firm assurances of after-sales service and general technical support will quiet many fears.

Provide Assistance With Outside Influences

Good public relations techniques can go a long way in giving everyone involved in a major purchase a feeling of comfort. We used public relations with scanners, reasoning that a hospital administrator who has been reading about the great things that CT scanners can do would not only heighten his level of understanding of the product, but would also lower his level of anxiety. Thus, when radiologists presented the idea of a half-million-dollar purchase, some of the objections that might have been raised had already been dealt with.

Another means of dealing with outside influences is to provide the potential buyer with arguments to deal with the people who are going to argue the most against the purchase. Identify those people, and try to understand the problems **they** will perceive in relation to this new technology. This support will comfort the potential buyer, and it will arm him with some creative - and effective - rebuttals when he's discussing the potential purchase with those who object to it.

THE CONVOLUTED PATH FROM DISCOVERY TO MARKETPLACE: SOME EXAMPLES

H.N. Friedlander

Introduction

One advantage of being an "elder statesman" in the practical world of new product development is that one is likely to be humored while playing the historian. It is rare that the historian can be both a participant and an observer. But, I have that privilege. Let me fill in some of the background relevant to the examples of several successful new product developments which I have chosen to discuss to exemplify the convoluted path from discovery to marketplace.

In the late forties, Monsanto Chemical Company carried out extensive research on the polymerization of acrylonitrile seeking practical new applications for this basic organic chemical[1]. One of the copolymers of acrylonitrile proved to be an excellent acrylic fiber candidate. Unlike dry spun acrylic fibers already in the marketplace, the new material was best made by wet spinning, a process that allowed large bundles of fiber to be produced at low cost. In addition, heavier denier filaments resembling wool used for carpet yarns could readily be made. To exploit this new technology in an unfamiliar marketplace, Monsanto wisely sought out a partner with marketing skills in the synthetic fiber business and with technical experience in wet spinning as well, namely, American Viscose Corporation.

In 1949, the Chemstrand Company was established as a fifty-fifty joint venture of Monsanto and American Viscose[2]. While the development of **Acrilan***, the new acrylic fiber, proceeded, the new company was afforded the opportunity, in 1951, to buy nylon technology from duPont. This opportunity got Chemstrand off to a running start. By the end of the fifties, Chemstrand was profitable and a leader in marketing synthetic fibers.

To maintain corporate leadership in technology as well as marketing, the Chemstrand Company decided to build and staff a world class research center for basic and applied research in synthetic fibers and the underlying polymer science. In 1960, the Chemstrand Research Center was opened as the first industrial laboratory in the fledgling Research Triangle Park, in the Piedmont region of North Carolina. Within the next decade, Chemstrand Research Center earned a world reputation as a leading center for polymer research. From fundamental research in polymer morphology, polymer characterization, fiber structure, fiber spinning dynamics, and polymer synthesis flowed a steady stream of publications worthy of an academic environment. At the same time, applied research in chemically and mechanically modified fibers, high performance fibers, flame retardant and soil resistant fibers, bicomponent fibers, polymer and fiber blends, hollow fibers, and fiber assemblies and composites yielded a steady stream of patent applications.

Just as the new research center was getting underway, American Viscose, in 1961, sold its interest in Chemstrand to Monsanto. Later in 1967, Monsanto decided to accelerate product development based on this stream of technology and placed the laboratory under the control of its New Enterprise Division. In 1975, the laboratory's name was changed to Monsanto Triangle Park Development Center to reflect its changed mission.

From among the products that were explored at the Triangle Park facility, I have chosen two to exemplify the complexities of the innovation process, namely: the work on spunbonded non-woven fabrics that led to **Cerex*** and on recreational surfaces that supported the development of **AstroTurf***. As a footnote on the **AstroTurf*** development, I will also discuss the work leading to **AstroTurf*** doormats.

Spunbonded Non-Woven Fabrics

With the firm establishment of synthetic fibers in the textiles marketplace during the sixties, it became clear to all synthetic fiber manufacturers that further expansion into new markets would require bypassing expensive conventional fabric preparation techniques. Extensive research was begun on all aspects of web production from synthetic fibers for conversion to non-woven fabrics.

Major technical problems lay ahead. Webs made from staple fibers bonded with adhesives were like paper. They had poor drape and tear strength. Webs bonded by needlepunching were thick like felt. Some progress was being made in laying down continuous filament webs. Bonding these webs proved difficult. Bonding could be achieved by using fiber blends where some of the fibers reacted selectively to adhesives or had varying melting points that allowed selective heat bonding. These techniques added costly technical complexities as well as increased fabric weight.

At the Chemstrand laboratory similar studies were undertaken. However, completely unrelated studies were also underway to change the surface characteristics of fibers. Friedlander and Menikheim[3] were conducting fundamental studies of surface limited free-radical modification of virgin nylon surfaces. They used Cl and ultra-violet light and were successful in changing the wettability and static electrification of nylon fibers. One of their co-workers, Henry E. Harris, became interested in whether by-product HCl might be affecting their results. He found that gaseous HCl is reversibly absorbed on nylon and can be removed with water. However, the nylon surface becomes sticky. Two touching filaments will adhere. This discovery led to a basic patent on autogenous bonding[4] in which no additional bonding material is introduced.

The bonding mechanism is now well understood. Reyerson and Peterson had shown earlier[5] that at -78 degrees, two molecules of HCl are adsorbed by each amide carbonyl group in nylon while at 0 degrees and 20 degrees one molecule of HCl was adsorbed per amide group at very low pressure. The absorption process which is surface limited by diffusion into crystalline nylon disrupts the molecular bonding and plasticizes the nylon. Upon desorption, the nylon recrystallizes. The recrystallized nylon constitutes the bond between filaments.

Combining the new bonding discovery with other work in the laboratory on light weight web formation led to a new spunbonded non-woven nylon fabric process[6]. In the process, a curtain of nylon fibers is formed by a multiplicity of conventional polymer melting and fiber spinning heads or through extruders feeding multiple spinnerettes. To develop strength, the filaments after formation are stretched by air attenuators and laid down on a moving screen to form a uniform web. The web is passed through a treatment box containing HCl gas for bonding, quenched in water, washed, dried, edges trimmed, and wound up on rolls.

Besides the innovative bonding process, many other innovations were needed to lay down uniform webs, control the bonding process, and integrate the system from polymer to finished fabric. To control such a complex process requires accurate understanding of the effects of humidity on static electrification which affects laydown uniformity and on the role of water in the chemistry of bonding. Knowledge of the factors affecting nylon polymer and fiber morphology and fiber and web hydrodynamics were needed as well as many innovative mechanical developments.

The resulting spunbonded non-woven fabric named **Cerex*** was a light weight nylon fabric with outstanding tear strength but only fair drape characteristics. Finding uses for such a fabric was the next development task. Early product development selected three applications: trouser pocket fabric, interliners for stiffening coat lapels and shirt collars, and backing for vinyl wallpaper. None of these applications were successful. The fabric did not have good enough abrasion resistance for pocketing. It was not uniform enough for interlining. Nylon expanded too much when wet relative to vinyl for use as wallpaper backing.

The key major application for **Cerex*** proved to be as the support for carpet underlay. A product patent was obtained for this application[7]. Low cost carpet underpad was being made from rubber or urethane foamed directly in an oven on an open weave jute fabric. Woven jute proved to be expensive and non-uniform. Carpet underpad manufacturers began to use synthetic non- woven fabrics to control costs. Spunbonded non-woven polyester fabrics were initially used but the polyester fabrics incorporated low melting binders that limited oven temperature and relatively heavy weights were needed to overcome poor tear strength. **Cerex*** proved advantageous in this application because its higher strength allowed use of lighter weight and therefore lower cost fabric grades. Also, having no additional binder it could withstand higher oven temperatures which allowed greater oven productivity. In addifion, carpet installers liked the smooth surface that the nylon fabric afforded because it made adjustment of the carpet during installation easier.

Once production was underway, extensive applications research found new uses for fabrics of various weights, tear strengths, and flexibility. High tear strength fabrics proved useful for filtration. A unique application was coolant and cutting oil filtering for machine shops and steel and aluminum mills where resistance to tearing by metal fragments proved important. Heavier weight fabrics are useful for air filtration and subterranean drainage applications. The dimensional stability and conformability of upholstery fabrics is enhanced by bonding lighter weight upholstery fabrics with **Cerex*** which adds little to weight or cost while expanding the use of the face fabric. Reinforcement of quilted products and batts use similar properties to advantage. Low cost sleeping bags are one result of this application. High tear strength and low weight also makes **Cerex*** especially useful for laminating with paper tissues for such disposables as hospital gowns and operating room drapes. Other unique applications were found. Controlled air permeability coupled with high strength makes a nylon non-woven especially useful for cargo parachutes and military flares while coated light weight fabric makes excellent washable labels for garments. **Cerex*** has also found applicability as a coating substrate in those uses where strength in all directions is important but where differential environmental stretching found in the wallpaper trial is unimportant.

We see in this case a good example of the main characteristics of the convoluted path of innovation between invention and market place. First, the discovery is likely to come from a collateral search for something else. Second, detailed knowledge of the fundamentals are required for a controllable economic production process. Third, useful applications must take advantage of the unique properties of the product and be economical in the overall system.

Recreational Surfaces

The path from discovery to marketplace for **AstroTurf*** recreational surfaces was different from that just described for **Cerex*** spunbonded non-woven fabrics. Yet the path was convoluted as is common to most technological developments. The initial innovations that made **AstroTurf*** possible came from exploratory work at Chemstrand's nylon production facilities in Pensacola, Florida and applications research on carpet production at Chemstrand's **Acrilan*** production facilities in Decatur, Alabama[8].

As part of work aimed at product diversification in Pensacola, extensive study was made of means to produce heavy denier nylon filaments, especially ribbon-like fibers. Fibers that resembled straw were sought for application in fashion accessories[9]. The exploratory work demonstrated that heavy denier monofilaments could be spun if the fibers were quenched in water rather than air and monofilament spinning processes based on this principle were developed. Ribbon yarns could be made from properly designed spinnerettes. A key patent was obtained for a spinnerette with a rectangular grooved slot which produced a ribbon filament with molded-in ridges that reflected light like blades of grass rather than like shiny plastic[10]. The ribbon yarns, pigmented green, were incorporated into grass-like woven carpets after suitable modification of carpet looms.

The initial applications for the grass-like carpet were thought to be in playgrounds and field houses with additional decorative applications around swimming pools, on patios, and in floral displays and cemeteries. The first indoor test installation, in 1964, was in the field house of the Moses Brown School in Providence, Rhode Island(8) and about the same time outdoor tests were conducted in school playgrounds in New York City. Meanwhile, Judge Roy M. Hofheinz of Houston, Texas, had built the first indoor athletic stadium, the Astrodome. Because he was unable to grow grass indoors and he was aware of Monsanto's installation in the Moses Brown field house, he turned to Monsanto to give him an indoor surface suitable for baseball and football[11]. The Astrodome installation utilized very expensive woven fabric, rolls of which were held in place with gigantic zippers. Special handling equipment and underpads had to be developed to make the field useable.

The Astrodome installation was a giant test ground demonstrating that considerable additional technology would be required to meet the demand in outdoor stadia for all-weather playing fields. For outdoor applications, ultraviolet light resistant yarns had to be developed. Color stability and uniformity became a must. Utilizing technical background in stabilizing nylon tire yarns, this problem was solved at the Pensacola facility with pigments that also acted as stabilizers, namely copper phthalocyanine and lead chromate[12]. Alternate techniques for converting ribbon yarns to carpet pile besides weaving were urgently needed to reduce fabric costs. This problem was solved at the Decatur facility by knitting high strength fabrics for football fields and tufting less durable fabrics for other applications[13].

Problems remained in how a ball bounced from the surface, the effect of falls on players, and maintaining traction for runners in various sports. The solution to these problems was undertaken at Triangle Park by careful study of the rebound characteristics of the recreational surface system, i.e. the combination of fabric and underlay. Special foam underlayments were developed to control ball bounce characteristics and reduce injuries from player contact with the surface. Triangle Park also undertook quantitative accelerated weathering tests to put outdoor exposure on a scientific basis. Frictional characteristics of the surface were also quantified because they affected ball bounce, abrasion injuries, and foot traction. Shoes for each sport were evaluated. While cleated football shoes are unsatisfactory, soccer shoes are satisfactory even for American football. Finally, a team of civil and mechanical engineers was assembled to solve installation problems. Special machines for handling large rolls of carpet, installing underpads, making seams in the field, and anchoring the ends of the fabric were developed.

Besides the heavy duty fields for baseball and football, work was carried out to develop fabrics suitable for golf. Satisfactory putting surfaces were developed but they could not be used for golf greens because, despite the development of energy absorbing underpads, golf balls pitched onto the green would not stick as they do on natural grass. Other applications for the high quality **Astro Turf*** recreational surface did not develop because the nylon surface could not compete with resilient surfaces for running tracks and tennis courts and with lower cost polypropylene outdoor pile carpets for decorative applications.

The market for recreational surfaces became highly fragmented with artificial surfaces tailored for specific sports and with each application having its own cost structure[14,15]. The original research was aimed at diversification of nylon fiber production to heavier denier yarns. However, even after successful development of the yarn process, much additional technology was needed to achieve a recreational surface system suitable for stadia and the nylon product specifically developed for this application proved to be too expensive to compete against low cost imitations that filled the light duty applications such as landscaping and around swimming pools and patios. Although one football field requires 15000 kilograms of fabric, 15000 to 30000 kilograms of shock absorbing underpad, 5000 to 10000 kilograms of adhesives and sealing materials, and many tons of concrete, asphalt, and rock, the amount of nylon utilized is almost trivial. Yes, the path from innovation to marketplace is indeed convoluted.

Doormats

As a footnote to the **AstroTurf*** development was the development of **AstroTurf*** doormats[16]. It became clear that a heavy denier nylon pile carpet would be much too expensive to be used to replace natural grass for heavy traffic areas such as playgrounds or areas hard to water such as superhighway medium strips and lawns in arid climates. A grass-like surface made in continuous rolls from a low cost material was needed.

We began exploring, at the Pensacola facility, whether low cost grades of polyethylene could be utilized. A continuous molding machine was designed by innovative mechanical engineers that has cavities in which blades resembling grass could be molded in a cylindrical rotary mold[17]. It was necessary to develop means of filling the mold, chilling it, and stripping off the molded surface continuously. These problems were solved. Pigments and stabilizers were developed next and and backings for stability were continuously applied[18]. When the finished grass-like plastic product was installed out of doors, its initial appearance was excellent. But, when the sun warmed the grass, it expanded and pulled out its ground fasteners. On cold nights it cracked. These problems were inherent in polyethylene and could not be solved without adding to cost beyond what the application could support.

Fortuitously, however, the people working with the molded product discovered that it was comfortable to stand on and that it cleaned dirty shoes effectively. The continuous fabric could be cut into rectangular pieces which worked very well as doormats. Until that time, doormats were woven from tropical coconut fibers or assembled from waste rubber pieces from tire manufacture. Market research showed that the molded doormat was cost competitive, looked better, lasted longer, and was easier to clean than the older types.

In the United States with many individual homes and with the universally recognized **AstroTurf*** trademark, the **AstroTurf*** doormat became a profitable product. Here indeed is a thumbnail example of the tortuous path between innovation and the marketplace. After developing a continuous process to make molded grass which proved useless, an entirely different application for the process was found, namely as individual doormats.

Conclusion

Although the examples of discovery leading to new products which are discussed here may seem unsophisticated compared to so called high-tech products, each required considerable technical knowledge to make them successful. In each case, a scientific principle or technique was sought to fill an apparent market need. In each case, the knowledge led to a different product than that initially sought. And in each case, additional scientific knowledge or techniques

* Trademarks of **Monsanto Company**

were required to introduce the product successfully into the marketplace. Finally, in each case the product proved to be limited to specific market niches by inherent physical or chemical properties or cost. I know of no cases of innovation, sophisticated or otherwise, that do not follow this common convoluted path from discovery to marketplace.

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THE ROLES OF GOVERNMENT, INDUSTRY, AND UNIVERSITY IN THE PROMOTION OF SCIENCE AND TECHNOLOGY IN SWITZERLAND

H. Ursprung

I'll speak to three points. First, I'll present the needs of science policy illustrated by a case-study: the development of biology and biotechnology in Switzerland. I'll then mention, very briefly, other cases of successful science policy, but also failures, and work out some take-home lessons, and shall conclude with a critical evaluation of today's science and technology policy in Switzerland.

1. The Needs of Science Policy: the Case of Biology and Biotechnology in Switzerland

Above all, science policy needs scholars with vision and courage. For my case study, let me mention four giants first: Watson, Crick, Jacob, and Monod. Their epochal discoveries had an enormous impact on biology, which became a real science. New Biology! Which is the informational content of a cell? Is it uniquely located in the chromosomes ? Is transcription of genetic information controlled cell-specifically? How? Are perhaps chromosomal proteins the real controlling elements ? These questions were in the air in the early sixties, and brought fresh air into biology. Many of us biologists felt that this fresh air was badly needed, since quite a few of the so-called classical domains of the life sciences - comparative anatomy, systematic botany, systematic zoology, to mention three - had become rather stale.

Fortunately for the progress of science, the list of scholars with vision by no means ends with the category of the four names I mentioned. For the moment, let me add only one name to it, Eduard Kellenberger's, whose clairvoyance was crucial for the development of New Biology in Switzerland. Physicist by training, then at the University of Geneva, Kellenberger in my view was the first scientist in Switzerland who clearly recognized the epochal importance of molecular biology and who transformed his insight into action. By action in this context I do not mean scientific contributions, but contributions of a science policy nature. In 1961, Kellenberger initiated a series of workshops with participation of foreign scholars, held in Zurich, Geneva, Lausanne, and Bern, financed by the Swiss Government through the channel of the Swiss National Science Foundation. 1963 already followed the first of a series of phage laboratory courses held in Naples and Geneva. Beginning in the same year, Kellenberger helped found the European Molecular Biology Organization (EMBO), whose activities focussed on advanced training programs, fellowships, and the creation of the European Molecular Biology Laboratory. Another pioneering role followed in 1967, when Kellenberger, together with several distinguished colleagues that meanwhile had accepted (or were about to accept) calls to Universities in Switzerland, founded the Swiss Committee of Molecular Biology (SKMB). This Committee played an important catalytic role in Switzerland. Its laboratory and lecture courses, workshops, and fellowship programs soon led to the recognition of New Biology in most Universities in our country, and laid a solid basis for the development of this science.

Vision, courage and perseverance !

Mind you - such pioneering efforts for Kellenberger constituted an up-hill fight. Conservative circles in academia regarded New Biology as a threat to descriptive botany and zoology. Rather than recognizing that descriptive botany and zoology had transformed from a goal into a means, they tried to equate them with ecology. Describing the fate of individual plants and animals, or populations, is an important indicator for environmental changes, and for that reason botany and zoology remain important areas of biology. But of course it has been widely recognized that a purely phenomenological, descriptive approach to ecological problems is not sufficient for understanding environmental processes. Quantitative measurements of the physics and chemistry of the atmosphere, water, and soil, and their dynamics, are necessary prerequisites for understanding ecology. The same conservative circles also built up resistance when their colleagues in New Biology planned their own buildings, departments, and started to build up staff. Territorial thinking is deeply rooted in the behaviour of many animals, including man !

Science policy thus needs scientists with vision and courage, and of course responsive governments with money.

Interestingly enough, it was the same Eduard Kellenberger who in 1971 in a report to the Swiss Science Council (the advisory council of the government) pointed out the growing importance of biotechnology. He had good reasons for this second act of science policy of his. He knew that as early as 1950, the famous chemical company CIBA had installed a series of fermentors in a laboratory at ETH Zurich in search of a biological way to produce antibiotics. He had witnessed the pioneering efforts, at Ciba-Geigy, by Jakob Nuesch, who developed antibiotics by fermentation technology. He was keenly aware that biology was about to enter the phase of industrial application, and that science policy acquired a new dimension: that of technology policy.

I witnessed the further development of biotechnology in Switzerland from close-by. A group of microbiologists at ETH Zurich, led by Armin Fiechter, in 1974 proposed the establishment of a center for technical microbiology. It was known that thousands of tons of antibiotics were being produced, worldwide by methods of technical microbiology every year. Methods had been developed for growing animal and plant cells in culture, biocatalysts had been isolated from cells, bound to carrier substances, and used at a preparative scale for complex syntheses. Bioreactor technology was advanced. The molecular biology know-how in Switzerland was well established at several Universities; I need only mention the names Arber, Birnstiel, Schwyzer, Tissieres, and Weissmann. The Science Council of the government had encouraged the establishment of such a center. As President of ETH-Zurich, I was proud that the initiative had come from within our University, and tried hard to support this powerful idea. Even though our University was in a zero-growth phase, we managed to make the necessary infrastructure, funds, and some personnel available. Swiss companies helped us with donations of fellowships, money, and equipment. Teams were sent to Japan, to the Federal Republic of Germany, and to the United States where similar efforts had been made, to compare notes. And before long, we had established postgraduate training programs, a department of biotechnology with three professors, and a graduate program.

The administrative key to this latter series of events consisted in viewing biotechnology not as a novel kind of natural science, which followed conventional botany and zoology on the one hand, New Biology on the other, but rather view it as an engineering science. Why ? The cry that economy needs more and better trained engineers was heard in Switzerland, too. The strong bias in favor of supporting basic science was undergoing a slight shift towards application-oriented research. It was important to recognize that biotechnology was indeed an engineering science. This insight helped shift means from Old Biology into Biotechnology, leaving New Biology intact.

The view is more than just an administrative key for solving a problem of reallocating limited funds. The difference between engineering sciences and natural sciences in my mind is their finality. The finality of biology as a natural science has been, and is, to arrive at a theory of living matter. The finality of biotechnology as an engineering science is to utilize living matter for producing or degrading large quantities of matter, or, in a broader sense yet, to utilize principles of living matter for solving engineering problems. The biologist uses techniques for understanding biological phenomena, the biotechnologist for applying phenomena to economically and energetically advantageous processes. Biotechnology utilizes many methods of scaling up biological processes, including bioreactor technology, genetic engineering (recombinant DNA), immobilization techniques, process control, and the like. Its aim - this may not be stated enough - is to solve engineering problems. In this sense, biotechnologists are engineers.

I consider the case of biology and biotechnology in Switzerland rather as a success. New Biology is in top shape in our country, as witnessed by the academic performance of its leading scholars. Economy, particularly our chemical, but also apparatus and engineering industry, employs vigorous young staff graduated from our biotechnology curricula, and successful research is under way in bioreactor technology, recombinant DNA technology, cell culture, and transgenic plants and animals.

Of course, the effort must go on. Biology is on the way of establishing itself as the fourth pillar of engineering, next to mathematics, physics, and chemistry. A fruitful interaction between neurobiology and electronics is in sight. Some call it bioelectronics, or bionics, and are developing visions ranging from rather straight-forward biosensors, to biochips, neurocomputing, and organic computers. A science policy for these developments is needed, and it needs, and in fact is dependent on, scientists with vision again.

2. Success and Failure in Science Policy; Take-Home Lessons

Fortunately for Switzerland, the recent successes in biotechnology are not the only example of a science and technology policy that makes sense. The case of chemistry is another example, to a large extent based on excellent working relationships with Universities, intelligent feed-back when curricula are changed, research areas chosen, and professors appointed. The development of apparatus for nuclear magnetic resonance in Switzerland is also the result of an intelligent way of sharing responsibilities between industry and University. I will not go into the details of either.

Unfortunately however, there are examples of profound failures, too. A tremendous potential of computer know-how existed at ETH Zurich in the fifties already, when the first electronic calculating machine was constructed there. Swiss industry did not take up this opportunity for innovative development. Who knows: perhaps it would have been successful to develop a computer-industry of its own, rather than, as is the case today, merely possess one of the highest computer densities of the world - all imported.

I will not go into the details of this failure, nor the details of the early failure of our traditional watch industry that so-to-speak slept through the beginning of the semiconductors age even though its own research laboratories and some Universities had obtained remarkable results that would have lent themselves to industrial exploitation. Rather, I'd like to address the question now as to whether anything may be learnt from such case studies. Which role should governments, industry, and academia play in a successful interplay that would be likely to preserve and/or create jobs, maintain or render the country economically competitive in the world wide race of the present and the future ? Forgive me if I stick to the situation in Switzerland in this attempt. Not only would it be politically delicate to draw on too many foreign examples, but inherently difficult. The cultural and ethnic differences between nations are so profound that comparisons and conclusions will always be difficult. I must leave it up to you which, if any, of my thoughts you may want to keep in mind, adapt, or adopt for your own situation.

2.1 The role of the government in science, education, and technology policy: I regard it as the government's duty to establish a framework of conditions which makes sure that

* education and research meet or keep meeting the highest level by international standards;.

* national needs are recognized timely;

* the country's science remains open to that of other countries;

* the country's economy encounters open doors in the world's markets;

* interaction between academia and industry is encouraged facilitated.

2.2 The role of academia: I regard it as academia's duty to make sure that

* teaching are conducted at the highest international levels;

* scientists make a conscious effort to look not only into the present of their research, but into its long-range future, including,if possible, its application, and ,including neighboring disciplines;

* University leaders exert pressure for dynamic adaptation of curricula and the orientation or research programs;

* scientists are open for collaboration with industry. (A note of caution should be stressed at this point: Scientists at universities ought not to perform research that is too close to a product about to be marketed; the University's aim in research, as in teaching, should be long- term, not short-term).

2.3. The role of industry: I regard it as the duty of industry

* early to recognize promising markets for promising products;

* establish and conduct R & D programs accordingly, either in-house, or as research pools, or together with academia;

* to maintain close collaboration with academia, by joint programs (but see caution in 4.2), but more effectively by sponsoring graduate studies and accepting University staff for sabbatical-in-industry stays;

* to examine, with great care, signals given by governments as to developing national needs and international opportunities;

* to create, where necessary, research interfaces between industry and academia;

* to ascertain continuing education of their staff, in-house and/or at Universities.

3. Science and Technology Policy in Switzerland

In Switzerland, a good many of these prerequisites for a successful science policy are already given, but much could be improved. Our governments, both at the Federal and State levels, do support, generously, education and research. But there is a striking imbalance between the various academic disciplines in that far too few young people choose engineering curricula, far too many, curricula in the humanities and social sciences. I believe that this is caused by the fact that our basic education programs are incomplete in their coverage. Languages and literature, mathematics, physics, chemistry, biology, earth sciences, philosophy, history, psychology all are important components of general education. But a timely general education would have to include technology (in the Greek sense of the word). Our high-school curricula lack technology, lack engineers as teachers, and therefore are anachronistic in the technological era we live in. Our young, therefore, simply do not know what constitutes an engineer, and therefore choose other curricula. I believe our governments would be well advised to take the necessary steps for engineers to join the teaching staff of our high schools.

Our government makes great efforts for timely recognition of national needs and foreseeable developments in science. But the response by many scientists to this effort thus far has not been sufficiently enthusiastic. Our government tries hard to include Swiss researchers in international research programs, with mixed success. In basic research, at CERN in high-energy physics, at EMBL in molecular biology, collaboration is rather smooth. In the recent, more application-oriented research programs that take place at the European level, Switzerland has to cope with the fact of not being a member of the European Community, and the matter is furthermore complicated by a rather complicated organizational framework within the government. In fact, this is one of the major drawbacks of our research organization. Since there does not exist a Department of Science and Technology in our government, the burden of elaborating a national science and technology policy rests on several Departments, which makes the formulation of a coherent policy all the more difficult. Thus, at the moment there is much uncertainty as to whether and in which manner our government will support interfaces between university and industry. (For reasons of historical truth, I must add here that many industrialists voiced opposition against financial involvement of the government in industry affairs, for example in the government's insuring industry's risks in R&D, because they were afraid of public intervention in private enterprise).

Our government in my mind has been very successful in its efforts to open doors for our economy on international markets.

In academia, the Swiss institutions have a proud record. Both in teaching and research, the levels reached by and large are high. Let us face it: what really counts in science policy, and constitutes its content and result, is the successful enhancement of knowledge by research, and the successful education of our young. The University with which I was last associated, the Swiss Federal Institute of Technology (ETH) Zurich, in the past 50 years has graduated over 30,000 engineers, scientists, mathematicians, and architects at the Master's level, and 7,000 at the doctor's level. These men and women carry all their understanding and skills from the University into the world outside. The same University in this period has published probably hundreds of thousand scientific papers, thus carrying the results of its research efforts to the world outside. And yet, the call for a yet speedier transfer of University knowledge into the industrial world is there, voiced particularly from those uncounted small and medium sized companies that cannot afford research divisions on their own. More readiness of University scientists for collaboration with industry is demanded. Indeed, in my country more readiness for such collaboration would be in order. But correctly in my mind, many colleagues are afraid that too close a collaboration with industry will distort the respective responsibilities: long-range research at the University, versus short-range research in industry. In my judgement, up to now this fear has biased the situation in a direction of too little faculty involvement in industry. One way of improving the situation would be to generate incentives, perhaps financial, for increased collaboration. But this would necessitate changes in our finance household law, which does not allow a university department to keep net gains from research contracts, and in laws pertaining to University personnel's rights to derive income from research contracts.

But industry too is to be blamed for the present-day situation. With the exception of chemical industry, food industry, and the leading companies in the electromechanical domains, companies in Switzerland are for the most part so small that they can hardly afford academic staff. This structure makes it very difficult for University scientists to find appropriate partners in industry. A vicious circle ? The difficulty is true, in particular, in much of our electronics industry.

The engagement of scientists in looking into future developments could be improved. Many of our colleagues are overburdened with teaching and research loads and simply cannot spare the time that is necessary for thinking in longer terms and with a vision towards longer time ranges. Several of our University leaders are inclined to lead their institutions rather than just manage them (I trust you have heard that managers merely do things right, while leaders do the right things). But by and large, potential leaders in our University system have little opportunity to function as leaders, for at least two reasons: their terms of office are too restricted in time and therefore continuity is hard to establish, second, their power is too restricted. In old European University tradition, most of the power is situated at the level of professorial chairs or perhaps departments, and therefore rests with the faculty. For concentration of efforts, for rapid adaptation to burning needs, this scheme of virtual self-coordination and self-control is insufficient.

I trust our industry is constantly in search of new markets for new products, by definition, because this is vital for its survival. What needs improving, however, is the translation of such insights into concomitant action on the side of academia, and/or governments. If the insights are available in industry, it would suffice to improve mutual information. To give you a recent example: the question is in the air as to whether academia should build centers of gravity i the area of bioelectronics by enriching, for example, the faculty of computer sciences by neurobiologists, or vice-versa. It would be highly desirable for me, as a person who is responsible for a substantial segment of our educational system, to know whether Swiss industries will include bioelectronic products in their production lines.

As far as industry's own research goes, I have already mentioned that in many enterprises, this hardly exists, because of their small size. I believe that it would be beneficial for many of these small companies to team up in their research activities by operating pool research, or taking initiatives for creating University/industry interfaces in order to facilitate their access to forefront research.

It is reassuring to note that such efforts are in sight now; the sensitivity of industry to respond to government signals has increased. Many industrialists remember tha t our government as early as in the late fifties had called their attention to the possibility of the mechanical watch being replaced by the electronic watch, and that this could constitute a threat to the famous Swiss watch industry. Unfortunately, in that particular case, as I indicated before, the reaction of our industry had been very slow.

As far as the need for continued education goes, industry much depends on our University system. Detailed studies are necessary before decisions are possible as to how this growing burden should be shared between University and industry in order to make sure that our staffs are kept up-to-date. Exchanging faculty and industrialists by job-rotation during sabbatical leaves in my mind would be an excellent improvement; it is hardly practiced.

So much for the appreciation of the situation in my country. The analytic approach I have chosen does not constitute a recipe for success, but may be used as a checklist. At its top rank scholars with vision. Governments, industry, and academia are well advised to listen to their advice. Governments are well advised to help generate the cultural environment that makes possible their unfolding. Basic scientists are specialists for the unexpected. Their visions therefore should be taken seriously. They should share them with the world around them.