Applied Science and Technological Progress

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In institutions whose mission includes the application of research results to products or operations, the categorization of research into basic and applied has little operational value. Industrial and government researchers feel particularly strongly on this point, because from the standpoint of research management the basic-applied dichotomy tends to focus attention on the wrong issues. In fact, all research in a "mission-oriented" organization contributes or should contribute, however remotely in time, to the general objectives of the organization. On the other hand, there is clearly a spectrum of activities ranging from pure research on the one hand to technological development on the other, and to some extent one can locate research activities within this spectrum, according to their "appliedness."

This relates to two factors, the time scale on which the research is likely to find an application, and the specificity with which the domain of application can be foreseen or the work committed at the time the research is undertaken. The shorter the time horizon and the more evident the area of potential application, the more "applied" the research. Furthermore, there can be a perfectly viable difference in viewpoint between the research worker and his sponsor. Research that may be viewed as quite fundamental by the performing scientist may be seen as definitely applied and may fit into a coherent pattern of related work from the standpoint of the sponsoring organization or agency. The scientist may see his own work in an entirely different matrix of interconnections.

Furthermore, in a well-coordinated group of scientists, success in a particular line of applied research may greatly expand the possibilities for basic research. For example, when a new area of development opens up, an important benefit of intensified basic research related to this area is the indirect one of maintenance of technical standards, and the introduction or perfection of new intellectual and experimental tools that might not otherwise be justified. It is not necessary to control the direction of the efforts of the individual research man in order to realize these benefits.

Research is best regarded as a continuing process involving a series of contingent choices by the researcher. Each time he decides between alternative courses of action, the factors that influence his choice determine the degree to which the research is basic or applied. If each choice is influenced almost entirely by the conceptual structure of the subject rather than by the ultimate utility of the results, then the research is generally said to be basic or fundamental, even though the general subject may relate to possible applications and may be funded with this in mind. The fact that research is basic does not mean that the results lack utility, but only that utility is not the primary factor in the choice of direction for each successive step. The general field in which a scientist chooses or is assigned to work may be influenced by possible or probable applicability, even though the detailed choices of direction may be governed wholly by internal scientific criteria. Research of this type is sometimes referred to as "oriented basic research." Much biomedical research is of this character, since almost any new knowledge in the life sciences has a fairly high probability of being applicable.

As another example, once the transistor was discovered, and germanium became technologically important, almost any research on the properties of group IV semiconducting materials could be considered to be potentially applicable, and this has indeed proved to be the case in practice. On the other hand, research into the theory of zone-refining single crystals was of such obvious immediate application to the control of transistor materials that it could be legitimately called applied rather than merely applicable. Prior to the discovery of the transistor, both of these types of research would have been of equal interest and importance from the scientific viewpoint, but they would have been classified as quite fundamental or "pure." Indeed the same two types of research carried out in a university might be regarded as fairly "pure," while in the Bell Laboratories they would be regarded as "applied" simply because potential customers for the research results existed in the immediate environment.

The essential point is that the categorization of research depends on the existing situation in technology and also on the environment in which it is conducted. As definite categories, basic and applied tend to be meaningless, but as positions on a scale within a given environment they probably do have some significance.

Although basic or fundamental research tends on the average to be less applicable in the sense defined above, the terms *basic* and *applied* are, in another sense, not opposites. Work directed toward applied goals can be highly fundamental in character in that it has an important impact on the conceptual structure or outlook of a field. Moreover, the fact that research is of such a nature that it can be applied does not mean that it is not also basic. Almost all of Pasteur's work, from the fermentation of beet sugar and the disease of silkworms to the anthrax disease of sheep and the cure of rabies, was on quite practical problems; yet it led to the formulation of new biological principles and the destruction of false ones, which revolutionized the conceptual structure of biology. As another example, studies of semiconductor devices have opened up whole new areas of basic solid-state research that would probably never have been conceived of if the problem or phenomenon hadn't first shown up in a practical device.

Despite this basic-applied feedback in research, if the criterion used is that of the individual choices of the investigator *after* his initial choice of general field of work, then I think a fairly meaningful distinction can be made between basic and applied research.

What industrial researchers are most skeptical of is such questions as, What proportion of our research should be

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basic and what proportion applied? or indeed, What should be the proportions between research and development? They would rather argue from the point of view of business objectives: research, development, production, and marketing are part of a continuous process of two-way information flow, and any distinctions that tend to place barriers at particular stages in this process also tend to reduce the effectiveness of all its individual components. On the other hand, if the researcher at the most basic end of the spectrum is continually having to change the direction of his efforts at the behest of market and production needs, his effectiveness is largely destroyed. Thus science, to be effective in the whole process, needs both isolation and communication. The research and development process may be thought of as a long chain, the two ends of which are well separated but nevertheless connected firmly through the intervening length. The man at the application end of the chain must be able to obtain information directly from the scientist, but the feedback along the chain to the scientist must not be so strong as to interfere with the conceptual integrity of what he does.

Although scientists like to emphasize that fundamental research is "free," it is actually, in another sense, a highly disciplined activity. The discipline is provided by the scientific "community" to which the researcher is related. His choice of problem and direction is heavily conditioned by the social sanctions of this community, the requirements of originality, and scrupulous reference to related and contributing work of others. The scientist takes these external constraints so much for granted that he does not consciously view them as constraints, but his description of his own activities as "free" may be quite misleading to the layman, who takes the description unquestioningly. In applied research the individual is subject to somewhat different constraints, but not necessarily more severe. They are a variable mixture of constraints arising out of science and constraints arising from the institutional environment in which the research is done. Although scientists are strongly self-motivated, they are also sensitive to their audience. The audience of the academic scientist is the worldwide community of his professional colleagues or peers in his own specialty, communicating through the official scientific literature, through scientific meetings, through "invisible colleges" of preprint circulation and correspondence, and through personal contact. To the scientist in a mission-oriented organization, his audience is mixed. It consists partly of his professional community, but also to a great extent of the colleagues and superiors within his own organization.

There is increasing concern with the need for better understanding of the research process itself. Several of our correspondents have deplored the lack of systematic scholarship on the research and development process—of research about research. Recently there has been an upsurge of interest in this area, but there is still an absence of solid generalizations based on reliable empirical studies. Much knowledge of the research process comes either from the observations of social scientists with minimal knowledge of the substance of the research area they are investigating or from the anecdotal evidence of scientists and technologists having little appreciation of the standards of historical evidence and often inadequate appreciation of the economic, social, and cultural factors that influence the rate of adoption and application of research results. There is a need for greater involvement of scientists and technologists themselves in the introspective study of the research process, but subject to the critical scrutiny of social scientists or historians. Many scientists and engineers tend to be unwilling to search for consistent patterns of success in research because they realize the importance of fortuitous interconnections and intellectual spontaneity, and they worry lest dissection of the research process squeeze out this spontaneous element and destroy the environment of successful applied research through premature policy application of untested or overgeneralized findings. The very fact that the natural sciences appear to have a mystique, impenetrable to the uninitiated, often tends to generate an unconscious resentment in students of the scientific process who are not themselves scientists. This creates hazards for the management and support of science, both basic and applied, which increase as the total effort grows larger and more visible.

It is important that some case histories, originally prepared by scientists or technologists themselves, be studied and evaluated by trained historians. The case for the utility of research is usually made on the basis of history, especially in the case of basic research. This is really the only solid ground we have, since basic research in general precedes its applications by 10 years or more.

However, it is important to bear in mind that history may be an inadequate guide, since the boundaries between science and technology are becoming increasingly blurred. The decreased interval between scientific discovery and widespread application in recent years has been well documented. Furthermore, a number of social factors are progressively altering the nature of the whole technical enterprise—the growth in numbers of technically trained people as a fraction of the work force, particularly in management positions; the growth of higher education, especially the relative growth of graduate and postdoctoral training; the apparent increasing pace of adaptation of social and political institutions to technical change, at least on a small scale; the institutionalization of research and development as an economic activity; the appearance of a scientific equipment industry. In short, there appears to be a strong positive feedback inherent in the growth of science that increases the receptivity of society to the application of scientific findings and methods in almost every aspect of life (*1*). Historical studies have generally pointed up the fact that the development of technology has been surprisingly independent of the development of science, at least in detail. Yet most of the studies on which this conclusion is based come from the 19th and early 20th centuries, and there is evidence that the detailed interconnection of science and technology is becoming much closer, so that many of the most scholarly and solidly based historical studies may have the least relevance to the contemporary scene.

The relative role of science and technology in the early history of the Industrial Revolution is well expressed in the following quotation from the German engineer Ferdinand Redtenbacher in about 1850 (*2*): "The manifold mechanical movements needed for the arrangement of machinery need not always be invented anew. . . . A very exact and complete knowledge of mechanisms already invented is therefore most important in the arrangement of machines. Scientific knowledge is actually of little help, for complex mechanisms are evolved not through general powers of thought but by quite special powers of understanding of form, of disposition and of assembly of parts. Whoever is gifted with these powers and has developed them by varied practice will therefore be able to produce many and very ingenious inventions even though almost totally lacking in previous intellectual education; while he who lacks these powers, even though he have other most remarkable diverse gifts, will not yet be in a position to devise even the most insignificant mechanism."

The general approach to industrial innovation described in the foregoing paragraph is applicable to much of the period of rapid industrial growth in the United States in the 19th and early 20th centuries. It is not without importance today, but it is no longer the central style of innovation. The dominance of this outlook and style in the 19th century is illustrated by the fact that even Josiah Willard Gibbs, the greatest theoretical scientist produced by the United States prior to the 20th century, was awarded his Ph.D. from Yale in 1863 for a thesis on the design of gear trains, a thesis that relied heavily on geometrical visualization of the type described by Redtenbacher (*3*).

It seems clear today, however, that a new pattern is emerging in which the "general powers of thought" are replacing the "special powers of understanding of form" as primary generators of industrial innovation. This seems to happen less by a general uniform evolution than by the appearance and rapid growth of new industries with a new style of thought, beginning with the German chemical industry in the late 19th century and culminating in the modern computer, electronics, and communcations industries. These industries were the first to develop a science base because their underlying technologies could be treated on a laboratory scale.

There is a very high correlation between the rate of growth of an industry and its investment in science and technology. This does not necessarily mean that the research investment is the *cause* of growth; the reverse could well be true. But, as this difference in growth rate continues, and as new science-based industries nucleate and develop almost explosively, it seems clear that research-intensive industry will become an increasingly important segment of our economy. And further, these dynamic industries have a tendency to invade the older industries, as illustrated by the invasion of the textile industry by synthetic fibers produced by the chemical industry, or the invasion of electronics and computers into the machine-tool industry and, more recently, into publishing and educational supplies.

The point might be also made that, as technology becomes more sophisticated, it is created to an increasing degree by highly trained people who have a strong bias toward the abstract and the scientific. These people are increasingly penetrating all levels of management, and it seems likely that their viewpoint concerning the relation of science and technology will itself determine the future of this relationship, regardless of what the experience of an earlier era may have indicated about its nature. Each generation has its characteristic intellectual style, and in our own time abstract thought is quite clearly the dominant mode. Within the universities today, this is the mode that attracts the brightest students and the best minds, and there is evidence that the students are considerably in advance of the faculty in their adoption of this style.

Role of Government, Industry, Universities in Applied Research

There is a serious need in public policy for a better delineation of the relative roles of the federal government and industry in the support and performance of applied research. The government is acknowledged to have a responsibility to support fundamental science, especially where it is connected closely with higher education. The government also has a responsibility to support science that directly contributes to public purposes, such as defense, public health, weather forecasting, or environmental improvement. The responsibility of government in the field of primary food production—that is, agricultural and fisheries research—is also universally acknowledged. There is a feeling that the government should not support research in areas in which private industry is active or could be induced to be active through suitable devices of public policy, such as tax incentives or the creation of new markets through purchase of products or services by public authorities. This feeling is based on more than a political bias in favor of free enterprise; it has a solid basis in the nature of the research and development process. Applied research is most effective when it is coupled to a "market" that provides an automatic measure of effectiveness of the end product of research. The existence of a market gives a continuous incentive for self-appraisal, which is often lacking for activities performed in the public sector. When the government supports applied research in an environment that is not organizationally coupled to an end use, it is likely to stray from the mark, and this becomes more of a hazard the closer the research is to application. It is probably no accident that, by and large, government-supported research has been most successful in defense, where the government itself is the customer for the end product. An exception to this general statement is agriculture, where a slow evolution has resulted in extremely effective coupling between public research and private development, production, and marketing. Nevertheless, it is important to note that the government role in agriculture extends well beyond the research itself to include extension services, marketing, economic services, and agricultural subsidies. The latter have had the effect of guaranteeing markets and thus to a considerable extent underwriting the economic risks of innovation. Another good illustration of a desirable pattern is provided by nuclear power. When research and development within the Atomic Energy Commission laboratories reached the stage at which successful development of civilian nuclear power plants seemed likely, the Atomic Energy Act was modified to encourage transfer of the new technology to industry as rapidly as possible, and the criterion of success became the willingness of public utilities to purchase nuclear power plants following their own evaluation of the comparative economics of conventional and nuclear plants. Basically, the criterion for transfer was the willingness of private industry to take on the task, again really a market criterion.

The federal government should adopt a more hospitable attitude toward spinoff of new industry from federally supported technology, including its own laboratories. There is still a widespread belief that ideas resulting from work done at taxpayer expense should be put in the public domain. However, this belief overlooks the fact that the innovator who develops an invention into a commercial product or process and tries it in the marketplace contributes as much or more to technological innovation and economic growth than the originator of the idea.

There remains the question of the role of universities in applied research. Historically the universities have been the major centers of applied research in both agriculture and medicine, although in both these cases a large corollary development activity has grown up in industry. The university research activity has been well coupled to the operational use of the results. In the case of agriculture this has occurred through the experiment stations and through the extension service, which have made it possible to demonstrate the economic value of the research results rather directly. In medicine the demonstration activity has occurred through the affiliated teaching hospitals. Thus one may generalize by saying that a fairly effective system of technology transfer has grown up in the life sciences, which has made it possible to couple applied research in universities to the ulmtaite user. Although some universities have developed engineering experiment stations, there is not, for the most part, a strong tradition of applied research in the physical sciences corresponding to that in the life sciences. This results largely from intrinsic differences between the applied life sciences and engineering. Since living systems always exist in many nearly identical exemplifications, a discovery or invention in the life sciences, even when highly specific and applied, also has a high degree of generalizability. A new technique for a surgical operation can be applied immediately in many nearly identical circumstances. A new variety of seed or a new method of cultivation can be disseminated rather readily, and there is often not a large problem of scale-up from the laboratory to operational use. Where there is such scale-up, as in the case of fertilizers, pesticides, drugs, medical instrumentation, or farm machinery, the corresponding development work has been most effectively done by industry. Thus we see that applied research in the academic environment is most effectively done when it is readily generalizable and where problems of scale-up or large-scale production are not of major importance. Such problems usually involve careful timing, scheduling, or programming of research, which tend to be incompatible with the other requirements of the academic environment.

The problem of scale-up involves more than physical size, however. Of equal importance is the problem of scale-up in complexity or "intellectual size." The development of complex systems involves the coordination of many component pieces of a problem and many individual specialities. Often it involves highly sophisticated science or mathematics side by side with rather conventional or mundane design or repetitive analysis. Such a coordinated effort tends to be incompatible with the university environment, with its high turnover of people, with its treatment of research as a part-time activity, and with the high value it places on individual as opposed to team of performance, and on the proposing of new ideas as compared with critical evaluation and comparison of ideas and their execution in all the most mundane detail. In the future we may expect more enterprises in the life sciences to partake of the same complexity that is now characteristic of many engineering systems. Thus the increasing significance of "intellectual size" in these areas may generate greater reliance on mission-oriented institutions only loosely associated with universities or completely separate.

When engineering close to production has been done in universities, it is usually in separately organized and staffed contract research centers having a quasi-industrial character. The close association of such centers with universities or technical institutes does assist in recruitment and also provides a source of valuable applied experience for faculty and graduate students, though often to a relatively small fraction and on a somewhat haphazard basis. The operation of both contract research centers and engineering experiment stations or institutes has been attacked as competing unfairly with private enterprise, and recently there has been a strong trend of opinion both inside and outside universities against the operation of contract centers for applied research by educational institutions. The responsibility for staffing and administering such centers throws a load on already overburdened university administrations and diverts them from tasks more central to their educational and basic research missions. It often involves the university in direct competition with industry for contracts, and in making evaluative judgments on subcontract performance by industry. If the research is under security classification, or involves dealing with proprietary information, it departs from the academic tradition that all scientific activities that are proper to a university should be open to the free and searching criticism of the entire world scientific community. There is an often-justified fear among university faculties that security classification will be used to cover mediocre, routine, or pedestrian work.

When applied research in universities has led to useful new technologies, it has often been that the research was undertaken to serve a purpose internal to the university, or where the application was a direct extension of basic research. Early computer development was carried out in several universities largely for the purpose of providing a better tool for scientific computation in basic research. The nuclear resonance spectrometer, the atomic clock, the maser, and the laser were all logical extensions of basic research already under way. The high-power klystron was developed for accelerators for nuclear research. Some fundamental technological developments, particularly in materials and in chemistry, have come from applied university work. Here again, this has usually happened in areas in which the problems of scale-up from the laboratory were minimal. The universities continue to be major sources of innovation in computers, especially on the software side, though the center of gravity has probably shifted to industry.

In general, I believe that more applied research in universities is desirable, when it is appropriate. One might state a general principle as follows: When basic research is to be supported, the burden of proof should lie with those who wish to place it outside the university; when applied research is to be supported, the burden of proof should lie with those who wish to place it inside the university. The following criteria favor university performance of applied research.

1) The results are readily generalizable, as in medical research.

2) The research lends itself to involvement of students—that is, it is not programmed or scheduled to meet deadlines.

3) It is unclassified and not subject to publication restrictions, and thus open to full scrutiny by scientific peers everywhere.

4) It is a logical extension or outgrowth of basic research under way or already performed.

5) It is of primary benefit to the public sector, or relates to areas of public responsibility.

6) The inventor is on a university faculty.

7) It relates to the development of a fundamentally new technological capability, involving new principles, and of benefit to more than one company or industry.

It is usually desirable that applied work begun in universities should be transferred to industry, where appropriate, as soon as possible, and certainly prior to manufacturing or operations.

On the other hand, in considering what type of institution is appropriate for what type of applied research, an overriding consideration may be the source of the original idea. Experience indicates that an idea seldom thrives if taken out of the hands of its inventor at too early a stage, and invention does not always follow organization charts or formal definitions of mission.

Even for the 19th century it is easy to exaggerate the independence of science and technology. Although this tended to be true of mechanical invention, it was less true of the applications of electricity and chemistry, even then. For example, a working model of an electric generator was constructed only a year after Faraday's discovery of electromagnetic induction, and an electric motor 2 years later (*4*). That Faraday's discovery did not immediately turn into a major industry was due not to failure to realize its technological potential but rather to the fact that a whole complex of other inventions for the utilization of electricity would be required before an economically viable technology could be created.

It is true that there is inevitably a considerable component of "art" in technology. Technology is essentially a codified way of doing things, and much of this is based on systematic theoretical knowledge, which is science, but some simply on codified experience, which is what I mean by "art." A good technologist must sometimes be willing to accept or search for solutions that work, even if they are not fully understood. In this he is not so far from the experimental pure scientist, who often behaves like a technologist with respect to his own experimental techniques. In fact, each branch of science is based on a characteristic technology, which changes as the science advances. On the other hand, the greatest impact of the scientist in an industrial environment has resulted from his unwillingness to accept rules of thumb or procedures that are not understood.

The technology associated with an experimental science tends to be passed from worker to worker somewhat independently of the conceptual scheme of the science. There is a collection of "tricks of the trade," which lie outside the body of formal scientific literature. Technologies developed for scientific purposes often later grow into technologies useful for industrial or other operational purposes. Research instruments are first commercialized, then used in other sciences, and finally used to control production processes. Laboratory tools and techniques such as high pressures, cryogenics, high vacuum, spectroscopy, vapor-phase chromatography, and so on, begin in a research laboratory but often end up on the production line. One of the most dramatic examples is the cathode-ray tube which, originating as a physics laboratory device, became the basis of the modern television picture tube. These experimental technologies undergo transformation and improvement in the process of being applied, but their origin in experimental pure science is still evident.

It also happens, of course, that technologies developed for applied purposes are later turned to providing new instrumentation for pure science. World War II produced a host of new techniques, especially in connection with microwaves, that have become indispensable laboratory tools of physics research, and more recently of physical chemistry. Within the last 10 years some of the tools of pure science have become major engineering projects in their own right. The most dramatic examples are high-energy accelerators, satellites for instrumented space exploration, modern radio-telescopes, and the Mohole project (until its tragic demise). In addition, government-supported pure research has created a large commercial market for research instrumentation, including moderate-size accelerators for low-energy nuclear physics.

There are cases in which it may be desirable to develop a field of pure science partly for the sake of the by-product technology that it generates. Although, in principle, this technology might be developed for its own sake without the associated science, in practice the scientific end use provides the focus and motivation, which generalized development could not do. In addition, it attracts more dedicated and able people through the intellectual challenge of the science. Already techniques of "pattern recognition" originally developed for automatic scanning of cloud-chamber photographs of nuclear-particle tracks are finding application in other areas, such as automatic letter-sorting.

A particular problem in the interaction between science and technology has been eloquently described by Peter Drucker (*5*). It is the reluctance of technologists to deal with the more mundane and less sophisticated problems, which still may be quite important socially. This is a special difficulty in connection with the transfer or adaptation of technology to underdeveloped countries, but it is also an inhibition against application of technology to the more backward civilian industries in our own country. The inhibitions are undoubtedly associated with the fact that the solutions, even when successful, are seldom elegant or intellectually satisfying. The importance of such problems constitutes insufficient motivation for attention, when there are comparably important problems in more sophisticated areas that give greater intellectual satisfaction.

Applied Research and Basic Issues

As applied research and development are more and more performed by people with original training in basic science, and thus interested in and aware of basic issues, applied research is likely to bring increasing benefits to science itself, as well as to technology. Applied research will continue to turn up important basic issues that the discoverers will increasingly be capable of recognizing and pursuing. This will be recognized also as having benefits for technology itself, for when applied problems are approached with the methods and the generalizing tendencies of basic research, the solutions found tend to be more broadly applicable, or to lead, by "serendipity," to new applications. Applied research must often look beyond the time horizon of the immediate purpose for which it is undertaken. The more sophisticated the field of application the less likely it is that the first version of a new invention will be valuable without much further development. It is in this further development that applied research aimed at deeper understanding of the underlying phenomena is especially important. For example, the first discovery of the gas laser at Bell Laboratories was followed by an intensive period of rather fundamental research in atomic and molecular physics, which eventually led to greatly improved lasers, culminating in the development of the CO_2 laser with a power output several orders of magnitude greater than that of the earliest gas lasers.

A fundamental problem in the education of the modern applied scientist is how to train him to bring a basic research viewpoint and approach to applied science without creating in him a disdain for, or impatience with, applied problems. A frequent shortcoming of the basic research viewpoint is a tendency to view all problems in the light of the researcher's own specialty.

Enlightened industrial laboratories often adopt the practice of encouraging newly hired Ph.D.'s to tackle problems quite remote from the area of their thesis research. The value of graduate training should lie partly in the confidence it instills in the student to solve new and challenging problems, and to assemble independently the information and tools necessary to do it; yet too many students want to use their first work assignment as an opportunity to extend and improve upon their Ph.D. theses, rather than to broaden their experience and skills.

When the experts disagree on the correct technical course to take, the decision between alternatives is often thrown back on the legislator or nonscientific executive. The question of the proper degree of involvement of the nonexpert in technical judgment is one of continuing controversy. As with all arts, executives and legislators with long experience develop a surprising talent for ferreting out key technical issues, without understanding the technicalities.

Furthermore, many of the types of questions that legislators or executives are required to answer are really questions of political preference, which are only slightly disguised as technical issues. Most commonly, important decisions in applied science depend not on technical feasibility, which is uniquely the province of scientists and technologists, but on social desirability, which must be determined by a multidimensional interaction of scientists, technologists, public servants, and the public. In practice, questions of technical feasibility and cost interact with desirability, and hence the need for a many-sided discussion. Real dangers are involved, however, when the nonscientist attempts to impose his own value system on what should be largely scientific decisions. The public is often tempted to dump large amounts of money into the solution of problems that are perceived to be of social importance, without adequate consideration of feasibility or economic efficiency, and without adequate understanding of the interrelationships within science. The national investment in aircraft nuclear propulsion is probably one of the most striking examples of such misapplied effort. There is a special hazard of misconceived priorities in the field of health, in which the most "visible" diseases tend to receive the greatest research attention.

Scientists and engineers have a much greater obligation than they have assumed in the past to explain their work in terms that are intelligible to the nonexpert and the general public, without being condescending. Too many scientists confuse simplification with condescension. There is good intellectual discipline in explaining oneself to people not committed to one's own specialty. However, it is essential that short-term support decisions should not depend primarily on annual justification to nonscientists. The cycle time for such justifications should usually be several years.

In the last few years, public and political attention has turned toward problems having both a technological component and social components, usually in complex admixtures: transportation, urban reconstruction, pollution, education, and industrial growth in lagging sectors. In health, emphasis is shifting from the understanding and cure of disease somewhat toward the organization and delivery of care, again having a larger social component. For such society-limited problems, a factor of considerable importance is the social acceptability of solutions to many people, something which was of little or no concern in the Apollo program or the Minute-Man weapons system.

The distinction between science-limited and society-limited problems is not invariant with time, and may in fact be radically altered by technological progress. A new technology can overcome social limitations in several ways: by drastically reducing the cost of certain operations or products, by greatly simplifying certain products or operations and thus making them more accessible to the average individual, or by developing a wholly different way of doing things that does not have the same side-effects as existing procedures.

Inventions that permit "designing around" social obstacles require just as much social ingenuity as technical ingenuity, and often the two have to be combined in a single individual. The process of inventing a product for a market is usually one that requires both technical and social invention. The perception of a market possibility consists in seeing what kind of technological invention is needed to overcome a particular social barrier. There are times when stating the need for a particular invention without any knowledge of how it can be done technologically may be a much more important step than the technological solution itself. This was very clearly understood in the 19th century, when many inventions were of this character and required relatively little sophistication in technology.

It is perhaps a hazard in today's highly sophisticated world that preoccupation with technology—a preoccupation made necessary by the high level of education required—may result in too little recognition of the equally important necessity of properly articulating social needs, or, if you prefer, the requirements of the market. With respect to the great modern problems—what I call the four P's of population, pollution, peace, and poverty—it may be that articulating these is the most important part of the problem—that once these needs are formulated in the right way, the technological solutions will become obvious, or will fall into place.

The Mission-Oriented Laboratory

The characteristic institution for the conduct of applied research in the modern era is the large, multidisciplinary "mission-oriented" research organization. Although this type of organization has not replaced the small specialty company or even the independent inventor as a source of innovation, it is to an increasing degree the source of basic technology both for public purposes and for industrial projects.

What constitutes a "mission"? How is it defined, and how is it used to shape the specific research program? How is success in the performance of a mission to be measured?

The answers to these questions are complex and often subtle. A mission must be neither too vague nor too specific. It must be concrete enough to provide real guidance in the choice of tasks and priorities, and to be understandable by the key people in the organization, but it must be general enough to permit the phasing-out of old tasks and the establishment of new research goals. A mission must be like the shell of a building, within which the interior can be drastically rearranged to carry out constantly changing tasks. A mission, however, should not be simply an umbrella under which almost any high-quality scientific activity can be justified. Not every exciting discovery is convertible into an economically or socially useful product. Unfortunately, the broader the objectives of an institution are, the harder it is to determine what is really relevant to its mission. Very large diversified companies find that almost everything is relevant in principle, but they have to pick and choose, at least in the short run, in order to achieve "critical size" in the efforts they do support. In many cases it may be more important to maintain this critical size than to "cover every bet." One reason for this is that the transfer of information between organizations occurs more rapidly, except under conditions of secrecy, than does the vertical transfer from research or invention to marketable product. In the research part of an institution, it is sometimes more important that the organization be working in a general field than that it be working on a particular project. A company—or for that matter a nation—that has a broad technical capability can quickly exploit the ideas of others, and can catch up on the bets that it misses provided it has the technical sophistication to identify promising ideas at a sufficiently early stage. Just as a company or a nation cannot expect to exploit every promising scientific discovery, so every discovery that it exploits need not be its own.

In considering the "missions" of government laboratories, it is essential to distinguish a "mission" from a "task." A mission is a function assigned to an organization by higher authority or by legislation. A task is a subordinate objective that is best generated from within the research organization and pursued usually by agreement with the sponsoring agency. A research institute that does not generate most of its own tasks, but depends on external direction or "orders from headquarters," is either suffering from inadequate leadership or has a mission which is inadequately defined.

The definition of its mission is one of the most important considerations in establishing a new research organization or reorienting an old one. In evaluating the performance of such an organization in applied research, the emphasis should be on the performance of the organization as a whole rather than on its individual components. Good applied research is of little value if the mechanisms do not exist to translate research results into goods, services, or operations.

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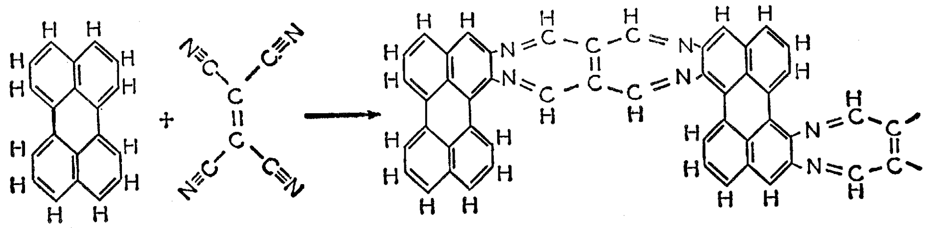
Erratum for Drickamer Article

In the paper "Pi electron systems at high pressure" [*Science* **156**, 1183 (1967)] a mechanism is suggested for the high-pressure reactions of perylene and azulene complexes with tetracyanoethylene (Figs. 10b and 10c) which is clearly impossible as drawn. What I had meant to suggest is the reaction shown below (with Fig. 10b as an example), which is at least conceivable, if not very probable from the chemical viewpoint. A similar modification would apply to Fig. 10c.

As indicated in the paper, any serious study of the reaction requires the synthesis of enough product for more complete analysis, as well as a fairly detailed knowledge of the crystal geometry of the unreacted complex.—H. G. DRICKAMER, *Department of Chemical Engineering, University of Illinois, Urbana*



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