Science and the National Welfare

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SOCIETY IS AT THIS MOMENT at the threshold of an undreamed-of mastery of our material environment, for science, which provides that mastery, is in its Golden Age.

In particular, achievements in nuclear physics promise incredible advances in the years ahead. Energy from atomic power plants has been much talked about, but even more important are the tools provided by nuclear physics for research in other fields. Radioactive isotopes, for example, will permit us to explore the structures and constitution of molecular aggregates, for such isotopes can be introduced into a system as scientific detectives. They will behave as the usual atoms of the particular element behave, but they can be traced and studied by means of the radiation they emit. Tracer studies of this kind will unravel secrets in biology, physiology, medicine, chemistry, and metallurgy.

The combined effect of tracer studies, of a variety of sources of radiation, of various sources of high-intensity, highly-accelerated subatomic particles, and fundamental knowledge of the nucleus means that spectacular advances in many fields are at hand. The problem of curing fatal diseases will be successfully attacked; fundamental biological and physiological processes will be understood; new types of therapy will be developed in medicine; better control of intricate chemical manufacturing processes will be feasible; new products, like petroleum fuels and metals with unusual properties, will be possible; and even new forms of plant life can be created. The speed with which these possibilities are realized depends primarily on how much effort we put into such activities. For there is no question that the impetus of the new knowledge in nuclear physics, in conjunction with steady advances in other fields of science during the last 50 years, means a general efflorescence of the physical and life sciences.

But if we are to profit from this happy situation, there are major problems to be solved, and their solution will not wait. From one point of view life today is a race—a race between knowledge in the physical sciences, which gives material mastery, and general ignorance, which retards or rejects mastery of our environment. Rejection means no more and no less than destruction of civilization as we know and cherish it.

The problems confronting us, approaching them from the standpoint of the sciences, exist on several planes and two in particular: the specific problems of science as science, and the question of these sciences in relation to the other activities of man.

Problems of Science

The problems arising within the sciences themselves are extremely practical ones, and, on the whole, they are not complex. Several axioms are at once apparent. First, science is universal. Second, science is unlimited in its material. Third, the rate of scientific progress depends on the amount of effort put into science. These axioms are important: they mean that no individual and no nation has a monopoly in science, that science affords an inexhaustible mine of valuable knowledge and discoveries, and that we must be willing to support science appreciably if we expect to gain heavily and to maintain leadership.

The Steelman Report

A comprehensive and cogent analysis of the problems of science is to be found in John R. Steelman's report to the President, *Science and public policy.* Taking into account the three major groups engaged in research and development activities—the universities, the industrial laboratories, and the Federal research agencies—Dr. Steelman points out that each of these groups is "especially adapted to the performance of a particular type of research and each can make a unique contribution to our total research and development effort," with university emphasis on basic research, industry on development, and government laboratories engaged in both.

As a "basis for our progress against poverty and disease" and as the basis of national security, the Steelman report analyzes the present scope of our scientific effort, the deficiencies now present, and the needs in terms of a broad program. The main recommendations of the report are 8 in number, and I would like to discuss them briefly.

(1) It is recommended that expenditures for research and development be expanded as rapidly as facilities and trained manpower can be provided. A suggested goal is that, by 1957, 1% of the national income should be expended in research and develop-

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ment in university, industry, and government laboratories.

The report shows that a little over $1,100,000,000 is being spent this year for research and development, excluding the social sciences. With a national income of $200,000,000,000, this is an expenditure of little more than .5%. Only about $110,000,000, or less than 10% of the total, is spent for basic research. Almost half—that is, $460,000,000—enters into the development of military weapons and needs, not including the amount spent for atomic bomb development now considered to be a civilian activity.

(2) It is recommended that heavier emphasis be placed in the future on basic and medical research. More specifically, it is recommended that the total research and development budget be doubled, coincidentally quadrupling basic research activity and tripling research on health and medicine.

(3) It is recommended that support for basic research be provided by the Federal Government at a progressively increasing rate, reaching an annual rate of $250,000,000 by 1957. The present rate of total expenditures for basic research is $110,000,000, while quadrupling would require $440,000,000. This proposal, therefore, leaves ample scope for large-scale and expanding support of basic research by private groups and state governments.

(4) It is recommended that a National Science Foundation be established with a Director appointed by, and responsible to, the President to administer the program of grants in support of basic research. It is also recommended that the Director have a board of advisers, half of whom should come from government laboratories in order to provide for proper correlation of the work with that of the government laboratories.

(5) It is recommended that a program of Federal scholarship aid to university students be developed in order to provide for the proper training of the increased number of scientists needed and that this program be a part of a general program of assistance to university students in all fields of interest.

(6) It is recommended that suitable Federal assistance be given to colleges and universities in developing their scientific research facilities, and that this should be administered as part of a broad program of aid to universities in all fields.

(7) It is recommended that the work of the several Federal research establishments be better coordinated by the establishment of an Interdepartmental Science Committee, by a coordination of all scientific research programs through the Bureau of the Budget, and by the assignment of a number of the White House staff to devote himself to problems of liaison at the top policy level of the Federal Government.

(8) Lastly, it is recommended that aid to the reconstruction of European scientific research be made part of our European Recovery Program. This recognizes, first, that science is universal in that its truths are part of the universe accessible to all investigators; second, that we gain as much by original discoveries made elsewhere as by those which we make; and, third, that the progress of other nations in science and technology is necessary if they are to become self-sufficient again.

The program outlined in the Steelman report is splendidly conceived, and every point is vital if we are to live up to the responsibilities with which we are confronted by our good fortune in natural resources and freedom from war devastation.

One of the great obstacles in the way of a major program of expenditures on basic research is the difficulty of explaining to an appropriations committee—and even to management in private business—precisely what the program will accomplish with that degree of definiteness expected and demanded in other fields. It is necessary to entrust funds for research programs on faith, on the competence of the leaders of such programs, and the trust must be maintained for a sustained period of time. It is characteristic of most fundamental research that several years are required for the completion of any work of importance and that the end result may be difficult to evaluate by anyone except specialists. What, for example, is the cash value of Einstein's discovery of the relation, $E = mc^2$? No doubt it is an astronomically large value now. But what was its worth at the time of its formulation, and who was qualified to make the evaluation? The point simply is this: pure knowledge cannot be evaluated in cold cash, and pure knowledge is independent of such evaluations.

Unfortunately, appreciation of this fact is not as widespread as it should be, which suggests the story of two partners who had long operated a chemical manufacturing business. They finally decided to employ a research chemist. Along about 11 A.M. of the first day of his employment, one partner said to the other, "Shall we go see whether that research chap has discovered anything?" "No," replied his partner, "It's a little too soon. Let's wait until after lunch."

Zones of Danger and Weakness

One of the dangers facing us in the present situation is overconfidence. The United States has led the world in technological progressiveness and in the techniques of mass production. We are, without question, the most powerful nation in the world. In these very facts lies the essential danger, for overconfidence is a product of precisely this set of circumstances. Illustrations of pride preceding fall fill the pages of history, and civilization after civilization has perished in this fashion. We need glance backward no farther than the recent war to see a once scientifically sophisticated power lose leadership and initiative—Germany. For many years, during the latter half of the 19th century and the early 20th, science in Germany was in a position of international prominence, and yet we now know how misguided and superficial were their efforts in the direction of atomic energy. I believe that two factors were at play here: First, the Nazi leaders eliminated the truly first-rate scientific leaders and installed second-rate party-men in positions of scientific leadership. Second, there are obvious evidences of overconfidence on the part of the scientists as well as the nation in their scientific ability and achievement. Thus, after the revelation of our work in atomic energy, we had the spectacle of, first, the German refusal to believe that accomplishment, and second, childish attempts to pretend that they had not wanted to develop an atomic bomb but that they really had progressed in atomic research and that their researches were to be devoted to peacetime uses. The rationalizations would be merely amusing were they not also sardonic.

Again, we have the spectacle of England's dilemma in this century. Prior to the 20th Century, the English had led the world in technology, one of the consequences of their early industrialization. This leadership had lulled the British into accepting this pre-eminence almost as a law of nature, and progress in modernization of facilities and in mass-production technique was not pursued vigorously. The result was that England fell behind Germany and the United States. A reluctance to accept scientific advances, in the face of obsolescence, is thus dangerous.

The obvious lessons of the past, as far as science is concerned, indicate that competent leadership must be fostered in science (remember that for every thousand scientists adequate to contribute in a rather routine way there is only one with great and inspiring creative ability), and we must never take for granted future achievements on the basis of past performances. This thought leads to another danger confronting us: as a nation we have been outstanding in applying science; we have not been outstanding in basic scientific discoveries or theory. If we are to attain our goals, it is imperative that basic research be supported on a large scale.

In atomic energy, for example, we were essentially dependent upon the work of European scientists for our basic knowledge, and European scientists in this country contributed heavily to our success, in particular Fermi and Szilard. Again, during the first half of the war, we were dependent on British research and development in radar for our own program, and it was not until the latter portion of the war that we contributed in a basic way to this field. Then our contributions, particularly in microwaves, were significant.

Research in Rubber

Still another field, vital to our economy, in which we have been dependent on European research is rubber, representing in the recent conflict a vast Federal investment second only to atomic energy and radar. The need for synthetic rubber during the war, as a result of the unavailability of natural rubber, is well known. What is not so well known is that the synthetic rubbers we used were developed largely by the Germans. The four types of synthetic rubber which we produced during the war were GR-S, Neoprene, Butyl, and the Nitrile rubbers. Of these, only Neoprene is purely American, a development of the Du Pont Company. Butyl is partially an American development, for it constitutes a radical improvement of the German material, polyisobutylene; yet it was based on this German work. Fundamental patents were taken by the Germans on the remaining two types—the Nitriles (under the German name Buna-N) and GR-S (under the German name Buna-S)—in the early 1930s. Of all these rubbers, GR-S is the most important: more than 80% of our total production was of this type because it is not only cheaper but best for tires.

Now that natural rubbers are again available, the problem of what to do with the synthetic industry, which involved a Federal investment of more than $700,000,000, is acute. This industry will be called on for only limited production, primarily to insure plant potentialities in the event of any future emergency and to provide the synthetic product for certain applications. The magnitude of the investment, the size and scope of the plants, and the relations between the synthetic and natural commodity are major commercial problems. For this very reason, the need for continued research and development is obscured.

The National Bureau of Standards has long been active in the research and development phases as they pertain to both synthetic rubbers and natural rubbers. From the standpoint of the national economy and security, it is necessary that a major and coordinated program of research and development be maintained in this field. Basic research is necessary if new types of synthetic rubbers are to be developed; developmental research is needed to develop desirable characteristics in the rubbers now available, to determine their properties. Much also remains to be done in measurements and instrumentation associated with the synthetic rubbers.

In the future, this country must have a vigorous program of rubber research to maintain "a technologically advanced and rapidly expandible domestic rubber-producing industry" as part of our national policy outlined in the Crawford Act (Public Law 24, 80th Congress). The cost of such a program would involve an annual expenditure of about $4,000,000, which is less than 1% of the amount spent for the 1,000,000 tons of rubber that this country consumes annually. Industry should expend a corresponding amount for the development of new rubbers, in addition to its expenditures for research on end-products.

The cost of such a program is actually relatively small in terms of the value of the commodity and in terms of its national importance. Merely to maintain the present synthetic plants in a stand-by condition involves an annual expenditure of over $8,000,000, and these plants may well be obsolete at the time of another emergency. Therefore, a Federal expenditure of half this, to insure our future in this field, is, from any practical point of view, trifling.

Research in Optical Glass

A comprehensive and broad program of research in the field of synthetic rubber is a matter of national wisdom, and similar programs are needed in other fields, many of them not of such vital concern on the surface. For example, a national program of basic research on optical glass is a primary desideratum, and yet the thought of the importance of optical glass is not likely to occur to those not engaged directly in military problems, because the annual requirements of this country for precision optical instruments for civilian purposes during a period of peace are almost negligible when compared with the demands made upon our industry by our military agencies during war.

Here is a field in which we were long dependent on European developments. Prior to World War I, all optical glass used in this country was imported from abroad. It was during this period, under the sponsorship of the Navy, that the Bureau started experiments on the production of optical glass and succeeded also in fulfilling military requirements during that conflict; but this was possible only because the United States did not enter the war until the fighting in Europe had been going on for over two years. In the years between World War I and World War II, experimental work was supported at the Bureau by the Navy Department as a hedge against any future emergency, and the foresight of the Navy Department was amply rewarded in the recent conflict, for not only were satisfactory types of glass available as a result of prior experimentation, but actual production in this emergency period was necessary by the Bureau, attaining a peak of 236,000 pounds in 1943. Moreover, the Bureau was able to train industrial engineers and technicians so that their plants could enter into the production of this specialized kind of glass, and assistance was rendered to other branches of the military establishment.

If we are to be again prepared for future eventualities, a program of research and experimentation must be maintained. Stockpiling of optical glasses is not a solution, for stockpiles tend to maintain the status quo, saddling the military services with obsolete instruments and making the introduction of better glasses and instruments difficult. As a general rule, with valid exceptions only in the case of basic raw materials, stockpiling is futile, for it tends to hinder progress.

The only sensible solution is a progressive research program involving the development of new types of optical glass, analysis of the chemistry and physics of such glasses, the development of new and more efficient methods of making and processing optical glass, the investigation of new optical materials for such systems as the ultraviolet and infrared, studies of polished surfaces, and the development of control methods in production of highly precise optical components.

Research in Buildings and Structures

Finally, let me mention a field somewhat removed from pure science and related more to applied science and engineering—building technology. The need for research in this field needs no stressing in this critical period of housing shortages, but it is significant to note the technical reasons behind our apparent backwardness in this field. In almost every field where American science and industry have teamed together to produce spectacular results, production has involved a centralized operation—for example, the production of automobiles, tires, typewriters, and so on. In the building industry, however, no single firm has specialized in the production of a building as such, and practically every material and product known enters into a completed structure. In each of the fields supplying components for a building, research has been done, depending on various conditions too many to outline here, and varying tremendously in extent and scope. No one, on the other hand, has attacked the problem from an integrated point of view, with the single exception, to my knowledge, of the work of the Bureau of Standards in building materials and structures.

Even here, as a result of the extremely limited funds granted for this purpose, the attack has been on a relatively small scale. Recently, all of the sections engaged in this type of work at the Bureau have been unified into a consolidated Building Technology Division, and an accelerated and coordinated program is under way. Groups are engaged simultaneously in investigations of the properties of materials: structural strength; fire resistance; acoustics and sound insulation; heating, ventilating, and air-conditioning; durability and the exclusion of moisture; building and electrical equipment; and other projects.

Unified scientific research in other fields of industry has been responsible for productive results, and it is reasonable to assume that the effect of this approach, applied generally throughout the $10,000,000,000 construction industry, will achieve similar results.

Science and Man's Other Activities

Even these few illustrations indicate that science does not function in a vacuum, divorced from everyday life. It is a pre-eminently practical thing, dealing with crucial problems affecting industry, business, the nation, and the world. It costs money, and it demands the efforts not only of scientists but of every segment of our population. Too often science is pictured as an "ivory tower" affair with no, or little, relation to reality. On the contrary, it is concerned immediately with the nature of the universe. It is the cause of our industrial economy, it operates within the full context of social existence, and it deals with practical problems as much as, if not more than, with theoretical ones. One of the discouraging attitudes widely prevalent in the contemporary world is the high regard placed upon what is called "practical" and the low esteem granted the "theoretical." In point of fact, the two differ only in time, relative to application; and pure, fundamental knowledge precedes applied knowledge.

The operations and progress of science can therefore be understood fully only in terms of the framework of our general society and in relation to the other activities of man. This context is particularly significant when we consider that science has now placed in our hands tools that are equally potent for good or evil. I have been talking, for the most part about the good, but actually the potential evil is more important, because of what value is this growing potential of good if science is used to destroy the civilization from which it has sprung?

It is fashionable to cry down the so-called pessimist who suggests this dangerous possibility, partly because no one loves a pessimist, partly because man is largely a hopeful creature with a belief that, at worst, he will muddle through, and largely because the dangers are difficult to group and appraise as a consequence of the staggering difference in kind and degree of present dangers in the form of scientific warfare. It is sufficient to say for my purposes that science has presented us with several weapons, each of which, unleashed, can mean almost total, if not total, destruction.

The question, then, is how to prevent such a situation. The answer is not to be found in the physical sciences. It is to be found in other realms of man's activity—in economics, in sociology, and in political science. Man's conduct in the physical sciences is rational; in these other fields it is largely arbitrary.

Research in the "Humane" Sciences

It is often said that man's social irrationality is a consequence of the fact that economics, sociology, and political science are not sciences but merely individual judgments and personal opinions. Now this is palpably untrue even at present, for much is known about cause and effect in these fields, and such statements are made only because habit, custom, tradition, and heritage tend to make us cling to whatever we know rather than to re-examine the data, coolly and critically. So far, no readily demonstrable experiments exist in what I shall call the "humane" sciences as exist in the physical sciences.

Admittedly, these "humane" sciences are younger than the physical sciences. Moreover, the variables to be accounted for are vastly greater than those we deal with in the physical sciences. But these are not adequate reasons for belittling the "humane" sciences and denying them support. On the contrary, these are compelling reasons for supporting them, and the present state of civilization demands that this be done. As a matter of fact, since the physical sciences have outstripped man's capacity for using them wisely, sanely, religiously, it is of the utmost urgency that we attempt to forge ahead in the "humane" sciences lest all be lost.

This is the time for intensified activity in these fields, not only because of the urgency of our need but because now the physical sciences have two tremendous tools to contribute to the "humane" sciences, tools that will permit "scientific" analysis of data having a large number of variables.

The first of these tools is statistics, which provides the theoretical, mathematical basis for analysis, the mathematical techniques for handling data, and the criteria for evaluating results. Mathematical statistics is now a substantial and well-developed discipline, and it does, in fact, offer these tools. Automatic electronic computing machines, on which many laboratories and companies are at work, constitute the second tool shortly to be available to the "humane" sciences. These machines will permit the handling and analysis of data, rapidly and comprehensively. Until the present, one of the major problems in fields where vast amounts of data are obtained has been the handling and classifying of the data. Literally thousands of man-days are needed in even relatively simple problems. This means that research is expensive, and the "humane" sciences have not usually been able to afford such luxuries. As an example of the labor involved in handling data of this type, consider a relatively simple problem. At the present time, a typical census problem involving 100,000 pairs of 5-digit numbers, representing statistical data, takes approximately 12 working days, exclusive of card handling and data punching. An electronic digital machine will handle the same sequence in 10 minutes at the most.

The Steelman report does not consider research in the economic, social, and political sciences. The study of the physical sciences in itself was a major effort, requiring 5 volumes of summary findings. It is to be hoped, however, that a similar analysis of the "humane" sciences will be made in the near future and that a program for these sciences will be mapped out and implemented.

Research in the "Mental" Sciences

Just as there is a disparity in the evaluation of research between the physical and the humane sciences, so too there appears to be an analogous disparity in the attitude of most people toward research between the medical and the "mental" sciences. Like the physical sciences, the medical sciences produce what are called "tangible" results—for example, new drugs, new clinical techniques, and so on. Like the "humane" sciences, the "mental" sciences do not appear to produce materialistic results and have suffered similarly in the support granted them for research. This, too, is a situation that needs remedy. Psychology, psychiatry, and psychoanalysis are disciplines pertinent in the solution of current problems. Aside from the statistical fact that 3 out of every 7 beds in the hospitals of the United States are occupied by the mentally ill—a vast drain in terms of lost manpower and cost—and that untold numbers of borderline cases permeate the entire social structure, we need to know more about the workings of the mind. For there is little doubt but that nonevident factors affect human behavior profoundly, factors like frustrations and fears.

These factors affect every activity of man, his personal, social, political, and even scientific life. From the standpoint of science we can say not only that science affects individuals and nations but that these individuals and nations affect science. Even from this restricted approach, then, what has happened or happens to men's minds and spirits is of interest if we have scientific objectives in view. We have seen how entire nations have apparently succumbed to a schizophrenia that has led to the espousing of mad, undemocratic, bestial beliefs. We have seen at least one nation despoil its scientists as a result of such an aberration.

Compartmentalization in the sciences and in other fields is inimical to a coordinated attack on the problems of man. This compartmentalization is actually breaking down in the sciences. The distinction between chemistry and physics, for example, has almost vanished. Competent research in the social sciences now depends on mastery of mathematics and on the utilization of the electronic tools. The complexity of modern life depends on specialization for progress in particular fields but, for over-all progress and for a solution to the dilemma of unbalances, integration and coordination are essential. In short, education of a comprehensive nature, embracing many fields, is needed for the survival of our civilization.

The sciences, like those other truth-seeking activities of man, require a free environment, an environment, above all, free from fear, petty arbitrariness, and tyranny. The pursuit of the sciences is fundamentally nothing more or less than the pursuit of truths. In the last analysis, all of man's activities are subservient to what happens to his spirit—to his spiritual welfare, "For what shall it profit a man, if he shall gain the whole world, and lose his own soul?"