Government, Industry, the University, and Basic Research

The following three articles—by Paul E. Klopsteg, Monroe E. Spaght, and Kenneth S. Pitzer—are based on papers given by the authors in the symposium Roles of Government, Industry, and the University in Basic Research *held in Berkeley, California, 30 Dec. 1954, as part of the annual AAAS meeting.*

Role of Government in Basic Research

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TWO conferences that considered government's role in scientific research have been held within the past 2 years. The first, the 7th Conference on the Administration of Research, met late in the summer of 1953 under the sponsorship of the University of California and held a symposium at Berkeley. The general theme of the symposium was the common interests and relationships in research among industry, universities, and government. The second conference, also held at Berkeley, was the meeting of the Industrial Science Section of the AAAS, the theme of which was the roles of government, industry, and the university in basic research. The two conferences discussed similar subjects but the second limited its concern to basic research.

Of basic research there are many definitions. I venture to suggest still another, better suited to the present article* than any I have found. The concept "basic research" may comprise the systematic endeavor, without preconception, to increase our knowledge and understanding of nature. It is the kind of research that some of our colleagues characterize as "pure science." If it is indeed pure, it derives that quality from uncompromising objectivity, unconcern over specific aims, and absence of intent to exploit results. It is intellectual adventure: a hunting expedition in unexplored domains where the weapons are the experimental devices and aids to observation by which data are gathered, processed, and made ready for interpretation. The trophies of the hunt are new concepts and principles. They are freely shared, through publication, with all who are interested in them.

For the most part, basic research is conducted by scientists on faculties of colleges and universities. Much research that is sometimes called basic is carried on within government-owned and government-operated as well as industrial laboratories. Such research may lack the afore-mentioned purity because it gets an occasional nudge, or at times even a strong push, in the direction of the practical interests of the supporting agency or industry. But it is difficult to separate the kind of research under my definition from all that appears under the category "basic," for example, in the National Science Foundation's study *Federal Funds for Science*. This study shows that government funds in amounts of $116 million, $120 million, and $131 million have been obligated in fiscal years 1953, 1954, and 1955, respectively. The figures are valid in that each reporting agency had its own interpretation of the term *basic research* and submitted its figures accordingly. If the suggested definition could be applied precisely, the quoted amounts would undoubtedly undergo a drastic downward revision.

Whatever the extent of such reduction, the annual amount applied by the Government to basic research is now many times greater than it was prior to World War II. Much progress has evidently been made in persuading those individuals who act for government in providing or denying funds to agencies that basic research is important in the national interest, and that it is a proper function of government to support such research. It is the "defense in depth" for both our economic development and the national defense.

As we consider and discuss basic research, we should be aware of the fact that basic research and education are inseparable, especially research and education in the graduate schools of our universities and colleges. The importance of increasing the numbers of scientists and engineers has been the subject of much discussion in recent months and of intensive study within the executive branch of the Government. Hence the importance of the teaching function of our institutions of higher learning can hardly be overappraised. The competent research professor probably contributes as much as or more, in the long run, through the education of his graduate students than he does by his own research. Under his direction the students make appreciable contributions to knowledge in their research. Moreover, the total amount of good research that can be conducted is limited by people,

* The opinions expressed are my own. They do not reflect the official views of the National Science Foundation.

not by dollars. It is clear also that applied research and development depend for further progress upon essentially the same kind of education that prepares students for basic research. There are many examples of physicists and chemists who in an emergency of war became excellent engineers; and there are instances of industrial scientists and engineers who have successfully reentered the academic community.

It is not my intention to dwell at length on the responsibilities of the National Science Foundation, an executive agency of government, for the support of basic research and education in the sciences. The NSF Act of 1950 assigned such responsibilities, and others. The provisions of the act constitute national policy with respect to basic research. They have been further clarified in Executive Order No. 10521, which is also an expression of national policy. One section of the order states:

> As now or hereafter authorized or permitted by law, the [National Science] Foundation shall be increasingly responsible for providing support by the Federal Government for general-purpose basic research through contracts and grants. The conduct and support by other Federal agencies of basic research in areas which are closely related to their missions is recognized as important and desirable, especially in response to current national needs, and shall continue.

There is ample provision in the law and the executive order to justify strong and increasing support by the Government of basic research and to make certain that no single agency becomes monopolistic in such support. No doubt some of the research carried on by the service agencies, in conformity with the executive order, is basic in the sense of our definition. No one, I am sure, disagrees in the present circumstances with the policy of increasing the nation's basic research under government subsidy, for we recognize the need for greater effort in research. Until something better may appear, the policy statement quoted may be generally approved.

Clearly, it is quite impossible within the scope of a short article to deal with the many aspects of government's role in basic research. A simple way to present a summary of sorts would be to give many more figures showing the extent and direction of government involvement in basic research. Such a presentation could be misleading; for dollar figures, as a measure of research in the various sciences, are not an accurate or comparable measure of the work that is going on. In different fields, research costs differ widely. A much better measure would be scientist-months, and an effort ought to be made to develop a suitable way of basing comparisons on time devoted by competent scientists to research in their respective fields.

Since government is already deeply involved in basic research, it would be academic at this point to argue that it should or should not be so involved. But we may properly discuss whether more or less government money should go into basic research. Is it possible to devise a better policy? Can the role of government be altered in a desirable way? This is a large question. It deserves examination. In response to the question "Why is government involved in basic research?" I have an answer from one of the speakers in the 1953 conference. He said, "One can say without reservation that the underlying motivation of the Government in science is the utilization of science." If this appears to be in conflict with our definition, we may take note that if basic research requires justification, it is justified by the experience that new knowledge of science has great potential value to society. Such value comes from eventual utilization. At any rate, this is the argument that must be made to bureaus and legislative committees to justify budgets and appropriations.

The arguments are familiar. When we consider science and technology during the past 150 years, we have conclusive evidence that progress in practical utilization depends on ever-increasing knowledge and understanding. We cannot utilize what we do not have. The risk capital that has supported basic research through the years has paid off handsomely. In the long run, the results benefit every citizen; they are therefore in the national interest, and public funds should pay the cost.

Indeed, it is not impertinent to ask why, in a country that takes great and justifiable pride in private enterprise, more of the financial support for basic research should not come from private sources and correspondingly less from government.

Many of us, I am sure, are somewhat unhappy at the possibility that government funds without foreseeable limit must be provided for basic research. Inseparable from basic research is the kind of higher education that produces research scientists. I should be much more content to see large annual funds from private sources carrying the major costs of research and of the training of scientists. If the policy of constantly increasing government subsidy were unassailable, we should soon be heading for a welfare state in science and education, with all basic research and most educational costs subsidized by the Government.

To travel this road without limit must be not only questioned but opposed on at least two grounds. First, when funds come easily and in large amounts, as they usually or frequently do from government, the concomitant is likely to be free and easy spending and finally deterioration in the quality of the research. More important is the looming, disquieting vision of annual funds requiring nine figures to express, supplied and administered by the Government, in the indefinite future. Does such a vision make you uneasy? It should; for it requires only scant imagination to picture therein a bureaucratic operation that would irresistibly and inevitably take a hand in the affairs of our institutions of higher learning. If this should happen, how could freedom from domination by "empire builders" in government be maintained?

There appears to be only one possible method of blocking such a trend. Here is an important role in basic research for enlightened government: to devise ways and means by which vastly larger funds are made to flow to our institutions of higher learning from a great diversity of sources. Funds should come not only from corporations but from the great number of private citizens who are potential donors to such causes.

No one would take issue with the statement that our universities and colleges suffer a chronic form of anemia which consists of a deficiency of dollars in the financial bloodstream by which budgets are maintained in balance. Much thought has been devoted to the financial needs of higher education, and many words have been written on this question. Efforts are being made to obtain more money from corporations and, to a lesser extent, from individuals, with a measure of success. It is heartening to note that corporate directors increasingly see the dependence of our economic progress on the output of our colleges and universities. At best, however, the total of such funds can, in the foreseeable future, cover only a small fraction of what the institutions need. Moreover, the maintenance of a steady flow of funds from industry demands a great effort in annual solicitation.

The rapid increase in college enrollment and its prospective doubling within a dozen years deeply concerns the college and university administrators. They recognize that now is the time to prepare for the deluge.

A great potential source of funds, aside from the corporations, is the private citizen—every citizen whose income would logically make him a contributor to worthy causes, such as higher education. It is here, in the tapping of this source, that government can play an exceedingly important role in the support of basic research and education. Only government can bring about a large yield of funds from this source. It can do this by making it possible for the individual to give, at nominal or no cost of giving.

Let me make clear what I have in mind. There are some millions of income-taxpayers whose tax computation involves the surtax brackets. Those in the highest brackets are able to contribute at the lowest cost. For the relatively few in the maximum bracket, the cost is $90 per $1000 given, since the tax rate is 91 percent. Thus, if one of these select few donates a deductible $1000, it saves him the $910 in tax that he would pay if he did not make the donation, hence the $90 cost. As we move downward in the surtax brackets into the lesser incomes, where the number of taxpayers is vastly greater, the cost of giving steadily increases. For example, the cost in the 20 percent maximum bracket would be $800 per $1000. This is incentive in reverse, a deterrent. That it has proved so is clear. Individuals have not been rushing forward with their gifts. Total deductions for gifts are only a small fraction of permissible amounts, for both individuals and corporations.

Obviously it would be fairer and more realistic if a positive incentive could be established, and if it could be made sufficiently compelling to induce adequate giving. Incentive would be maximum, obviously, if the cost of giving were entirely eliminated. It would still be great if the cost were kept small. It points to the necessity for revising those portions of the income-tax law that pertain to charitable donations. They should provide either that cost of giving be the same for all or that cost be related to ability to give. In either case, costs should be substantially reduced below present levels.

Several methods come to mind for accomplishing this desirable end. Details would have to be worked out by tax experts. In making the suggestions, I recognize that objections to them can and will be raised. The way around them must be found; for the fact cannot be overemphasized that we have the choice of only two courses. Either we take the simple and easy way and let the Government pay the cost, accepting with it the threat of government domination of research and education; or we insist on the maintenance of freedom and initiative for our institutions of learning. If, in trying to achieve the second choice, there is an alternative to obtaining the funds from a great diversity and number of private sources, I am unaware of it.

Of the three methods that I have to suggest, the simplest would probably be that of permitting the taxpayer to make his deductions not from taxable income but from the computed tax. Limits would have to be set for the amount given, and these would require occasional study and revision to bring about the desired results. The method would put all taxpayers on the same basis with regard to cost of giving: there would be no cost.

A second method, following established practices more closely in form, would comprise adding a new surtax bracket to the existing ones—an arbitrary "highest" bracket into which donations that are now deductible would fall. If in this bracket the surtax were 100 percent, and the "spread" of the bracket were made great enough to include all deductible items, the cost of giving would be zero. Limits would have to be set for the amounts that might come within such a bracket, and a percentage figure would have to be chosen between, say, 95 and 100 for the applicable surtax rate.

A third method—possibly the most promising with respect to the possibility of revising the tax statutes—would be to continue the present rules for computing the tax but to make the allowable deduction from the tax base a multiple of the donation. The multiplying factor would have to be determined with the stated objective of providing strong incentives to giving, and with ability to give as one of the factors.

It may be supposed that with sufficient incentives for corporate and individual givers, our institutions would be quickly provided with all the money they need, for both educational and research functions. This would, of course, depend on the details of the tax provisions. One would anticipate, however, that these would be meticulously and conservatively drawn with a view to careful experimentation with the method chosen and with the idea of modifying them in the direction of increasing the incentive if this should appear necessary.

One other important fact should be kept in mind. Probably the Government will have to continue to support certain areas of basic research, apart from the support that adequately financed institutions would provide for the researchers on their faculties. As we all recognize, a great change in the pattern of basic research occurred during World War II. Team research, the new phenomenon, was largely unknown before 1940. Most of the work financed by OSRD was applied research, but it was carried on primarily under contract with educational institutions by their scientists. Funds were ample to finance the most intensive creative activity of this sort that had ever been known. Among other things, it demonstrated how a sense of urgency among research workers can produce results otherwise impossible.

After V-E Day the urgency diminished, and after V-J Day it disappeared. The thousands of engineers and scientists engaged in this strenuous work were eager to return to the quiet of their laboratories, to resume the academic life and the more leisurely pursuit of their interests. But life for them could not again be the same. Many had seen team research on a large scale and had participated in it. They saw its applicability to many basic problems as well as to applied research and development. To them the "kilobuck" had become the monetary unit in expenditures for research. The team research they saw ahead is exemplified in the great accelerators of ever-increasing energy, which have annual costs for equipment, supplies and maintenance, and which require budgets for operating and scientific personnel greater than the total prewar budgets of many institutions. In such undertakings it seems impracticable, if not impossible, for a single institution or even a group to finance the operation out of regular current funds, even if these were greatly increased by means of the afore-mentioned revisions of income-tax laws. As science advances, there will be more and greater need for team research, in the biological as well as the physical sciences. There seems little doubt that the costs must be paid out of government grants or contracts. Moreover, to the extent that government agencies carry on basic research in their own laboratories, there will be government financing.

In summary, it is clear that the Government has a present important role in supporting basic research by providing funds for the conduct of such research in the laboratories of its own agencies and, in emergencies, for the procurement of research and development services from nongovernment agencies. It must also make grants and contracts under which educational institutions may support the work of research scientists on their faculties. Government also has a present responsibility with respect to increasing the numbers of qualified research scientists. Prospectively, government will have to finance indefinitely the large projects of team research that involve annual budgets quite in excess of the financial resources of any institution.

Beyond this, government has a role affecting basic research that goes beyond current practices and procedures. Serious threats of government domination arise from unlimited increases in the flow of government money to educational institutions. The trend can be stopped and reversed only if the government devises ways and means, through changes in the income-tax laws, that will result in the direct major support of institutions through gifts from large numbers of corporate and private donors. This would keep research free and improve its quality; and it would substantially reduce the severity of, or indeed eliminate, many problems that have their origin in the inadequacy of funds for higher education.

Basic Research in Industry

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THE total area of research is broad. Its boundaries, as well as those of what we call basic research, are not very distinct. Even within the central regions of basic research there is less than perfect agreement on what should be done, and how, and why. I can best define the subject in fairly general terms; and I can best describe it by pointing to certain broad questions that are common to most programs of basic research in industry.

In the interest of clarity, let us consider basic research in industry from three points of view: (i) its place—that is, its relationship to the larger scene of scientific inquiry; (ii) its problems—not a category of projects, but a review of some general questions that may help to delineate the main features of basic research in industry; and (iii) its promise—a few comments on the past achievements of basic research and its possible contributions in the future.

Place. Research has been called the systematic, intelligent treatment of problems for which the data or methods needed for solution are either unsatisfactory or lacking. Such a systematic, intelligent inquiry can be carried on in any field from science to human relations. There are many unsolved problems in all fields. A great deal of research is being conducted, some by colleges and universities, some by research institutes, some by government laboratories, and some by industry. All told, perhaps 450,000 researchers are working in the United States, and more than a third of them are professionals; that is, they have at least a bachelor's degree in one of the pertinent curriculums.

I hardly need to add that investigation on this scale is costly. The rate of expenditure for research by industry, government, and universities is more than $4 billion a year. Industry accounts for a large part of this money. Indeed, it has been said that the general use of technically trained people as organized teams for solving scientific and engineering problems is the most distinctive feature of our industrial development in the last 25 years. One recent study indicates that of the total research expenditures in the United States, about 65 percent, or $2.5 billion, is spent in industrial research laboratories. However, not all this came out of industry's pocket: $1.4 billion came from company funds and $1.1 billion came from government sources. The industry programs employ more than one-third of all the people engaged in research.

Such figures indicate that a substantial effort is going into various kinds of research, but how big is the effort compared with some of the other parameters of the business supporting it? A survey of 1450 companies in 26 lines of business showed that company-financed research ranged from 0.1 to more than 5 percent of gross sales income. Leather, lumber, and food industries are on the low side of the group. Drug, instrument, and chemical industries are high. But percentages based on net sales can be misleading, because of the great difference in profit margins among the various industries. In the drug industry the research expenditure, which is only 4.4 percent of sales income, is 38 percent of net profits. In the food industry, which has a lower profit margin, research expenditures amounting to only 0.2 percent of sales income are 10.6 percent of net profits.

It appears, then, that it is not easy to "place" research, even with respect to something as concrete as company financing. Low-profit margins make even low research budgets look big. High profits after taxes make big research budgets seem smaller.

Probably the best way to pin down the cost of research in any given situation is to look at the cost per man, and here we find rather striking uniformity from company to company and from industry to industry. Research costs per man range from about $7000 a year to roughly $10,000 a year.

In considering costs per man, we must remember that these figures are for total manpower and that the ratio of professional to nonprofessional workers varies considerably. In terms of professional workers, research costs per man are much higher; the middle range runs from about $15,000 to $25,000 a year.

So much for the big picture. To see where basic research fits into the scene, we should consider the main kinds of research being carried out in industry. I use the term *main kinds* because there is little agreement on the precise division of effort or on the names for the various divisions. One widely accepted description allows for four categories. A new book lists and describes in considerable detail some 23 categories of activity bearing the name *research.* For my requirements, I suggest three categories. We have pure research, which I define as the inquiry after knowledge for its own sake, without consideration or hope of practical gain. We also have applied research, the investigation carried out in response to immediate, direct, and obvious needs. Basic research is in between.

By basic research, then, I mean the scientific inquiry carried on, not under pressure of immediate needs or in hope of quick profit, but with reasonable hope of some eventual payout. It is research conducted to broaden the base of knowledge in any field that interests the companies supporting the research. It may use the knowledge, tools, and methods of pure research, but it rests on a different philosophy. It knows generally what it wants to learn, and it stops at the edge of its chosen field—where pure research would run on.

It is a kind of inspired curiosity, maintained by staffs totaling 10,000 men and women, of whom 4000 are professionals. It has reached such stature that top management in American industry is betting more than $100 million a year that this curiosity will yield something worth while. This sum is roughly one-tenth of the total expenditure by industry for research in the United States. It is spent in support of work both in industry-operated laboratories and in other laboratories, such as those in colleges and universities.

Altogether, industry spends about $10 million a year in support of research in universities and other nonprofit institutions. This sum is all spent for pure and basic research and does not include instructed research, graduate fellowships, or other grants of money for programs directed more toward the training of future scientists than toward the discovery of new knowledge. I stress this to show that the selfish interest that industry must take in advancing science is tempered. Shell Oil Company is a good example. We are now maintaining a program of 20 research grants, costing about $200,000 a year, which we give to departments in leading universities throughout the United States. The research grants are awarded for the support of current work in fields of science and technology, but there are no restrictions on the direction of work or on the publication of results, and the individuals who receive the awards are under no obligation to Shell. In addition to these, we are supporting specific research being carried out by professors of chemistry and chemical engineering in three universities. These men are working on problems of their own selection; to us it is basic, to them it is pure. All of these aids to university research are separate from our current program of 50 graduate fellowships in engineering and the physical sciences.

Another example of the support of university research by industry is the $730,000-a-year program for research on petroleum sponsored by the American Petroleum Institute and conducted in various university and government laboratories. The information produced by such studies is too general to answer any immediate needs or to give anyone an edge in competition, but all the information is of long-range value to the petroleum industry, and the member companies willingly support it.

It is, perhaps, not too extreme to describe the industrial scientist in basic research as a man imbued with a chaste spirit of scientific inquiry and a good sense of double-entry bookkeeping. The basic researcher is no more in business for his health than the sales manager, although the pressure on him to show a quick profit is not quite the same.

Yet, despite the lack of pressure, the movement in industry toward more elaborate programs of basic research has been remarkable. Even though we knew 20 years ago that the great technologic advance in industry would require a great deal of research, we could hardly have predicted then that industrial concerns would today be sponsoring a program as large as the one I have just outlined—and my own opinion is that we have seen only the beginning.

This has happened for a very good reason. It has paid off. It will continue only so long as it continues to pay off. Although altruistic reasons may be involved in some programs of basic research, and although industry is increasing its support of study aimed at the long-term social benefit of all mankind, it must be understood that when we talk about basic research in industry today we talk about an undertaking that is made primarily for the economic advantage of the sponsoring agency.

Problems. The first concern of anyone planning to conduct basic research is to decide as nearly as he can what he wants to do and how he will do it. This leads to a consideration of certain general questions, and in these we can discover—perhaps better than anywhere else—some of the essential features of basic research in industry.

Why does industry want new information? To us who have grown up in a society whose philosophy is constant change, this question seems easy to answer. If we were vintners in Bordeaux we would probably be reluctant to make an overt effort to find new things, but if we are marketing gasoline in California we can be sure that what is enough knowledge today will be inadequate tomorrow. The new knowledge may ultimately manifest itself as an improved product, a better process for making today's product, or, what is perhaps the most likely, a new item on the company's list of products. It may result then in keeping the company competitive in a changing technical environment, in increasing profit through improved operations, or in expanding the scope of the corporate activities through new ventures.

Are the problems of an industry ones that research can solve? Although it is true that research must begin with a problem, it is also true that not all problems are suitable for research. Some problems can be answered only by individual taste or prejudice. And some problems lead to solutions that are indeterminate or not reproducible. For example, a businessman trying to decide whether to apply for accelerated depreciation on a new plant has a problem. But he cannot solve it by research. The answer, which depends on conditions that do not yet exist and which cannot be predicted on the basis of past experience, is indeterminate.

What are the scientific possibilities for successful research in an industrial field? In the organic chemical industry, for example, there are so many fields eligible for further study that a large company that has no unbreakable ties to any raw material or product could support a very large amount of research, which on the average might pay off. Yet, the mere existence of a problem that seems eligible for solution through research is still not justification for work. Methane might be made to yield higher hydrocarbons, but I doubt that one would underwrite research in methane behavior unless one had some completely new ideas about methane. Here, it seems to me, is one of the hardest tasks of the industrial research scientist—to assess the chances of obtaining useful new information through exploration of an enticing area of ignorance. It may be an area never before trespassed by the brain of man or it may be an area trampled over countless times. There may be new weapons at hand, but they may prove to be poorly adapted to the terrain of the conquests. Even after the campaign has been decided upon and the march is begun, probably nowhere in man's activity does he come to so many choices of path where there is such a premium on alertness and even on intuition. Industry's demands on the brains of its scientists are not light.

I have cited the chemical industry as one that presents many areas seemingly eligible for basic research; here a critical program of selection must exist. In many industries, however, the limit is set low by the number of ideas worth looking into.

This limited situation must be distinguished from another that sometimes besets the research scientist in an industry that actually has good opportunities for research. This is the case in which only the scientist can see the opportunities. His trouble lies in convincing management to allocate funds for study. There is no pat solution for this problem. The natural divergence of interests between research and management is part of the play of forces that gives vitality and a certain air of democracy to most successful organizations. Success goes to the group whose management is best able to find and maintain a dynamic balance. One scheme for reaching this healthy tension is to reach into the research group, pluck out an experienced scientist, and put him on management's side of the conference table. His subconscious then works for research, but his conscious thought, forged hard by the endless blows of reports on sales and overhead, tempers his eagerness for new horizons through research.

I should point out also that having the opportunity to find new knowledge and fulfilling these rigorous qualifications still are not enough justification for industrial research. The research should not be conducted if the findings do not have some chance of paying off on the investment. However, our scientists generally have intelligent hunches about their chances of finding something that will ultimately be worth while. Let me illustrate with an example or two from the area of applied research. If a chemical process gives undesirable yields of side-products, the trained chemist can predict quite well whether the side-products might be reduced under other processing conditions or whether the operation could be improved only through some very difficult or costly procedure. A good engineer can predict quite well his chance of success in slowing the rate of deterioration in a certain piece of equipment or whether, by using new and better instrumentation in a certain operation, he could reduce the labor required to do the job. Although my examples come from the area of applied research, I believe that, after all other tests have been passed, there are still bases for judging the value of the new information that can come from the basic research.

Assuming that there are areas of interest suitable for research and that there is reasonable chance of obtaining new knowledge that will be useful, can a company executive assure the board of directors that something good will result from the research? The answer is *No*. No matter how able the scientists may be, how well equipped the facilities, how diligent the staff, one can never guarantee results. If there is anything certain about research it is this—and I include pure, basic, and applied research in this confident generalization—not all efforts will succeed, some successes will never make a profit, and nothing is sure until the work has been done.

How does industry decide how much to spend on a program of basic research? In the case of applied research, there are some well-accepted yardsticks to use in answering this question. These yardsticks generally relate to the ability of the sponsoring company to use the results of research. For example, there is no point in developing more processes than a company can commercialize. Money to build the plants, the technical ability to operate them, and the management talent to control them are needed—and do not underestimate the last two, for they are definite limitations.

With basic research, however, one is inclined to say that these limitations do not apply and that we should study only those things that have fulfilled the requirements of being eligible and subject to reasonable prospects of utility and will make the corporate entity feel that it is being a good citizen in its industry and its society.

As I have said, the limitations on the amount of basic research are most likely to be set by the number of worth-while ideas that the scientists of an organization can propose. If, however, the number of eligible areas is unlimited or very large, then one can apply the same kinds of limits that prevail with applied research—there is no business reason for following a program of research beyond what can be consumed and utilized by the sponsoring organization. Just as with applied research, there is a relationship between the effort and the amount of capital and management attention that must be dedicated to its utilization. Thus a large chemical company would not devote to basic research more than it could hope to utilize by the application of an appropriate development and capital-expenditure program. Beyond such consideration of the relationship of basic research to development and application, any further investment in search for new knowledge would have to be based on altruism or some longer range program of contribution to society that falls beyond the justification usually sought in a business enterprise. An attempt to relate investment in basic research to the specific parameters of the development and capital abilities of an isolated company requires that the investment be large enough to play on the laws of probability. It is obvious that a single venture in basic research can yield nothing; alternatively, it could by chance yield such a wealth of new ideas that the corporate facility for its application would be completely inadequate. This problem becomes less important, the larger the organization, and it disappears completely in a venture the size of the American economy.

I presume that some expect me to explain away the much-discussed imbalance in America between basic research and application. I support the general contention that Americans have excelled as utilizers of, rather than contributors to, knowledge—and there are reasons for this that demand no apology—and perhaps I must agree also that a greater investment in search for new knowledge is necessary if our present rate of technologic advance is to continue. Let me point out again that industry is supplying an increasing amount of support to this search, particularly to universities and their affiliated research institutes. I believe that this support from industry will continue to increase.

Promise. The rewards for research can be very great indeed, and it is in them that we find our hope of promise for other rewards to come. The way so far is marked with some remarkable achievements, some famous, others obscure. But all are important.

The immediate results of basic research are seldom very spectacular. They do not get into the headlines, they do not directly change the lives of millions or result in enormous savings or gains or victories or defeats—not in themselves. But in their long-term effects they may do all these things. The history of science is studded with examples. I shall mention only a few.

Certainly one is the story of Langmuir's work in the General Electric laboratory years ago—the first industrial research unit of its kind in the United States. Langmuir, who is responsible for the modern incandescent lamp with its tungsten filament and gas filling, laid the foundation for this invention by means of a series of experiments made for the purpose of studying atomic hydrogen. The subject was related to a field in which G.E. was interested, but Langmuir had no thought of improving lighting when he began his work. Indeed, he said: "At the time I made these experiments, they would have seemed to me useless if my prime object had been to improve the tungsten lamp." Yet as a result of this venture in basic research, the replacement of the old carbon filament lamp with the tungsten filament, gas-filled lamp was estimated to be saving the public more than $2 billion a year as early as 1930.

Another story, of intermediate age, but of similar stature in industry, concerns a young instructor in chemistry at Harvard, W. H. Carothers, who was hired by Du Pont to begin a program of basic research in organic chemistry. He chose as his subject the study of polymerization by condensation and the structure of substances of high molecular weight. In 1928 he began the study of condensations in which linear polymers are produced—and this led eventually to nylon.

More recently the Solid State Research Group at Bell Telephone Laboratories conducted some basic research on the electric properties of semiconductors. This work, which was really an outgrowth of some contract research on crystal rectifiers that Bell and other industrial and academic laboratories had carried out during World War II, led the research group to the transistor—a revolutionary development in electronics.

These examples show us the potential rewards of basic research. Other, quiet results, known only to those in the field concerned, can also be important and can, in their way, contribute just as much to the advancement of science and the betterment of standards of living.

I mention briefly, in this connection, some work in free radical chemistry done a few years ago by Vaughan and Rust and their colleagues at Shell Development Company. These men wanted to know more about the high-temperature substitutive chlorination of propylene to yield allyl chloride, a reaction that had been discovered by others in their group. The discovery of this reaction, which was contrary to the teachings of classical organic chemistry, had been made in the course of a basic exploration of the field of hydrocarbon chlorination; it was the basis for the world's first synthetic glycerin plant. However, when this plant began operation, a totally unexpected side-reaction began to occur at a totally unexpected place in the process; it threatened for many days not only to shut down the plant but perhaps to require some redesign with attendant capital cost and lost operating time. But this group of scientists had learned enough about the basic mechanics of the reactions involved to come up with a means of controlling the formation of the undesirable side-product. Few people know about this contribution, yet it has made possible a steady supply and a stabilized price structure for one of the most important chemical building blocks in industry.

There is another side to this story of basic research, the part about those who do not conduct any. So long as the competition conducts none, nothing much happens. But if your competitor studies his science and broadens the base of knowledge, sooner or later he will know some things you do not know. And then you are in trouble. For a long time, virtually all the methanol used in this country came from the wood-distilling industry, a tidy little operation that yielded a nice profit and required no great effort. Management cooked wood, sold the product, and failed to conduct research. Meanwhile some German chemists were working hard on the chemistry of carbon monoxide and some other related matters in organic chemistry. One day they announced that they had found a way to make synthetic methanol. An American chemical company heard about this and decided to put the German company to the test. They ordered some of the product, not just a little quantity for analysis, but enough to challenge the German company's productive capacity. They ordered a tanker of methanol. It was delivered. The wood-distilling business was dead.

Then there is the poignant and rather common case of the scientist who conducts basic research or is aware of it and yet does not appreciate what the studies disclose. This situation points up the necessity of carrying out enough basic research of your own—and of knowing about the research efforts of others—so that opportunities or challenges will be recognized as they come along. Bichowsky, in a diverting little book entitled *Industrial Research,* tells of the time he attended a lecture by William Ramsey, the discoverer of argon, neon, krypton, and xenon. Ramsey said that these gases were so inert that they would not combine with anything and hence were useless scientific curiosities. Then, to show that the gases were pure substances, he displayed a series of gas-filled glass tubes arranged so that a charge of electricity could be passed through them. "All pure substances," he said, "are characterized by the fact that they give out, under an electrical discharge, their own special kind of light." He turned on the current. The five tubes lighted up with a pale glow, each tube a different color. "Under different conditions of discharge," he said, "these colors can be intensified." He then switched a condenser into the line, and the tube containing neon flashed up a brilliant orange-red. It was very striking. Everybody applauded and went home. Not one of the 500 or so persons who saw the demonstration realized that he had seen the first neon sign.

It is easy to look at the recent and very rapid growth of research in industry and to ask whether this may not be a fair-weather operation, conducted at relatively low cost to the stockholder in a time of high profits and high taxes. One might be tempted to suggest that if business sags a bit, research may be sharply curtailed. But the handling of research budgets in recent years suggests that most companies will be more intelligent in their administration. After all, many large research programs can be curtailed only at a serious cost in lost momentum. Further, as competition stiffens, it becomes more important to maintain or increase the advantage that comes with more thorough knowledge. Therefore it seems quite safe to assume that industry's expenditures for basic research will continue to climb. If there is any delay in the development of research, one of the most likely causes will be the scarcity of technically trained people. They are not coming out of colleges and universities as fast as our society needs them. Industry and education can do the nation a service by encouraging young people to study science, to ground themselves well in fundamentals, and to make careers in scientific work. Few endeavors are more satisfying, and few are more needed in the world today.

The opportunity and necessity for basic research continue as research increases knowledge. There can be no turning back. Man's best hope of gaining wisdom, the better to manage his knowledge, lies in gaining still more knowledge of himself, of the world he lives in, of the materials he works with, and of the processes by which he changes them.

Companies spend money on research with hope of eventual profit. This hope rests on the certainty that profit comes only from service to society. Basic research, which broadens industry's knowledge far beyond its immediate goals, has served us well in the past and is serving us magnificently now. We have no choice but to foster its growth for still greater service in the future.

Role of the University in Basic Research

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IF we take the usual meaning of basic research as contrasted with applied research, then we can say at once that the role of the universities is to perform a large portion of the basic research and to train virtually all the men engaged in research. Most of the basic research has been conducted in the universities in the past, and I believe that this should and will continue to be the case. The mission of the university is to create and transmit knowledge. This aim is parallel to the aim of basic research; consequently there is no possible conflict of purpose when basic research is carried out at a university.

Other organizations usually have very different principal aims, such as making and selling gasoline, generating atomic power, or making atomic weapons. In many cases such organizations conduct effective basic research related broadly to their other aims; this is to be encouraged. However, there always comes a point at which the basic knowledge seems to have too little relationship to the principal business. Thus it is best if a very active basic research activity exists under auspices that set no artificial boundaries.

From another aspect the university offers the ideal setting for research. The strongest human driving force in basic research is curiosity. H. L. Mencken wrote:

> The value the world sets upon motives is often grossly unjust and inaccurate. Consider, for example, two of them: mere insatiable curiosity and the desire to do good. The latter is put high above the former, and yet it is the former that moves one of the most useful men the human race has yet produced: the scientific investigator. What actually urges him on is not some brummagem idea of Service, but a boundless, almost pathological thirst to penetrate the unknown, to uncover the secret, to find out what has not been found out before. His prototype is not the liberator releasing slaves, the good Samaritan lifting up the fallen, but a dog sniffing tremendously at an infinite series of rat-holes.

This story illustrates very well the driving force, the human driving force, that leads the scientists, in their search for basic knowledge and explanation of basic phenomena, to work long hours and with great enthusiasm. Students have this curiosity and aid effectively in the search. And one reason why much basic research is best handled in the university framework is because the focus on curiosity is the primary aim of the institution as well as of the individual. At the same time the university certainly does not ask for a monopoly on basic research. There are many nonteaching basic research units that have their place and that have made remarkable contributions—indeed contributions that would not have been feasible in universities. Examples are the Carnegie Institution of Washington and the Mount Wilson Observatory.

I think those of us in universities would agree that it is desirable for various sorts of laboratories with applied aims, whether government or industrial, to be active in basic research to the extent that seems wise within their general framework of operation. I am sure it contributes to the effectiveness of the industrial laboratories and applied government laboratories if a certain component of their work is of a strictly basic character, and if it is made clear to the individuals concerned that they may follow their investigations almost without restriction wherever they lead. Nevertheless, it seems clear that the major responsibility for a flourishing program of basic research lies with the universities; it is from this point of view that I wish to continue my discussion.

I am not going to comment at length on the importance of basic research. I think that most scientists are in agreement on this question. But I am concerned with the question of how the effectiveness of the university in basic research can be maintained, improved, and extended. As is not uncommon, the subject turns in considerable measure to money. One of the big problems of universities today, and one of the problems we expect to be even more stringent about 10 years hence, when student enrollments are expected to be about twice those of the present, is the provision of adequate funds and facilities for university activities. These include in very large measure research of a basic nature as well as the teaching or the transmitting of knowledge.

It is a matter not only of getting an adequate amount of money into the university's operation but of handling this amount of money in a fashion that does not hamper the basic character of the work. In other words, the funds that finance research should be available in a manner that does not restrict the operation of the investigator, who should not be continually concerned with the question: "If I follow this lead in this direction, will I be getting into an area where the source of financial support will no longer be appropriate?" The funds for university research can best come from what I would simply call normal university budgets, that is, from the same general budgetary framework that includes the salaries of the members of the professorial staff and their nonprofessorial assistants as well as the funds for chemicals, or materials for the machine shop or the glassblowing laboratory, and so forth. In this method of financing nothing has been said about the subject of investigation other than that it is in the botany department or biochemistry department or the physics department. Therefore, obviously, there is no limitation on the subject or method of investigation.

Unfortunately, however, the usual university budgets are inadequate even for the present level of basic research, and they have had to be supplemented by funds from various grants or contracts, which in turn have tended to put one boundary or another on the manner of use. The aim ought to be to increase the amount of money coming through regular channels in university operations rather than continually to multiply the varieties of routes through which these funds arrive.

During World War II, the Federal Government supplemented research funds in the universities to a very large extent, mostly for military work. In the period since the war the problems have become more basic, but the mechanism of government support is still very similar. Let us consider two general sorts of activities. First, there is the research of a professor and several graduate students, equipped with what might be called ordinary instruments and equipment. Here government contributions are not very large. In most circumstances the greater expenses are actually still being carried through usual university budgets. But even in this area, research would be seriously hampered if the relatively moderate supplements coming from government sources were not continued. On the other hand, there are some very expensive types of research going on at the present time that are of great importance to basic science as well as to various applied sciences.

One of the major examples of this second type is the Radiation Laboratory at the University of California. Certainly the advances in the understanding of the properties of atomic nuclei would never have been made at the rate and with the effectiveness that they have been made without some major establishments of this sort. The use of cyclotrons, synchrotrons, linear accelerators, and, in other locations, atomic reactors, and so forth, is obviously impossible unless there is a substantial organization that can handle the construction and operation of these machines and thus make them available for the scientist in his more immediate experiments on the properties of nuclei. I would certainly not raise any questions about the necessity and appropriateness of government financing for these expensive ventures. I think the Government, by and large, has developed methods of financing these operations that are about as satisfactory as one is likely to find.

What means can be used to supplement the regular current university funds for the first type of research activity? As I have indicated, a professor and a few graduate students and possibly a postdoctorate fellow are usually concerned. In some cases there may be two professors collaborating or a slightly larger number of assistants but still no unusually large expenses. At present there are in this area an enormous multitude of small grants and contracts. I think the term *grant* is certainly preferable; it is used within government circles whenever the law permits it. But, in organizations such as the ONR and AEC, contracts are written in a form that, within the general government framework, allow a maximum degree of freedom in the operations in the university. What are the effects of this large number of relatively small grants or contracts and the small sums of money that go along with them on university operations?

Before answering this question, I would like to mention that this is not the system that is being followed in Great Britain. I have had the pleasure of discussing the problem of supplementing the finances for university research with a number of leading British scientists, and they have told me in general about the British University Grants. A certain sum is set aside for university research in the government budget and then the University Grants Committee decides how much of it goes to Oxford, London, Manchester, Cambridge, and so forth. But at the governmental level it is not necessary to decide how much Professor X at Cambridge, Professor Y at Oxford, and Lecturer Z at Manchester, and so forth, are each to receive. The problem is handled on a much broader basis, and the essential effect is that the funds are put into the usual university budgets insofar as the investigators in the various laboratories are concerned.

I have discussed this problem on a number of occasions with men in the National Science Foundation, in particular with Alan Waterman when the foundation was getting into operation. I expressed the hope that the foundation, since it did not have any boundaries in terms of scientific fields of applicability, would find a broader and less restrictive mechanism for assisting university research than the sort of individual contract that had been necessary for the ONR and AEC. The latter agencies have an essentially applied science character and therefore must consider the applicability of a given subject of investigation to their field. I understand the reasons that apparently led the NSF not to establish a broad new route. As I understand it, the foundation believed that it was politically impossible to take a lump sum of money and decide how much each university, college, technical institute, and so forth, ought to receive on a lump-sum basis, whereas the same result could be accomplished by breaking it into 10 times as many pieces and distributing them in terms of the subjects of investigation and the individual investigators. I certainly appreciate this problem and would not want to assert that the decision was wrong, but I do assert that this small-project method is troublesome from the point of view of the university and that we ought to try to replace it in the future.

There are many troublesome restrictions involved in this project type of distribution. The investigator, in preparing a proposal, naturally asks himself, "What sort of proposal is most likely to be approved?" Even though the government agency has no intent to restrict the field of investigation, the investigator will naturally be thinking not only of his scientific interest but also of what is most likely to be approved. More serious, I think, is the tendency of an investigator, after he has presented a detailed proposal and had it approved, to feel obliged to continue it. I am sure most agencies do not intend this to be restrictive. They almost invariably approve requests for a change in the subject of investigation. But there is an element of inertia here, an element of uncertainty on whether the change would meet favor or not, which tends, indeed, to restrict the freedom of the work to a certain extent.

H. W. Dodds, president of Princeton University, has, I think, sensed this difficulty. In an article in *Physics Today* [7, 4 (1954)] he coined the term *projectitis* and defined it as

> . . . an unhappy addiction to limited objectives. Perhaps at the very moment at which the individual should be broadening his own comprehension and deepening the knowledge of his discipline with freedom for roaming speculation in an atmosphere unencumbered by the pressures of problem solving commitments to external agencies.

This is a thing which, as I say, many of us have sensed without any particular complaint at any particular action on the part of any government agency.

Curt P. Richter, of Johns Hopkins University, writing in *Science* [118, 91 (1953)], discussed "Free research versus design research" and noted that there is a tendency to examine too closely the proposals for research grants in terms of the plan of research that was offered. If it is truly basic research, there should be a minimum of emphasis on the plan. The investigator should be free to change his plan from day to day as the subject unfolds, because he has no fixed objective in terms of a particular practical problem to be solved. If he commits himself in developing a detailed design, he is doing something extraneous to his real purpose. A senior investigator with an established reputation can request a broad scope and give little or no plan, but the junior investigator without an established record of past accomplishment finds it difficult to obtain acceptance of an unrestricted proposal.

An event that occurred at a recent meeting of the chemistry panel of the NSF illustrates this point clearly. The question was raised: "Should a chemistry professor in a particular university be allowed to have two NSF grants or projects at the same time?" The panel's reaction was that something was wrong with the breadth of definition of the first project if he wanted a second project. The remedy recommended was to write a broad subject of investigation for this particular project so that the second field of study would be included within the authorization. Then, as a second and distinct action, one might consider whether the total dollar amount of the grant should be increased or not. Here was a case where the subject of investigation in the grant had been written far more narrowly than the research interests of the individual called for. When this is true there is inevitably a restriction upon the character of the scientific investigation. This particular problem can be straightened out, but it indicates the weaknesses and the dangers of the system. It is the system itself that disturbs me, not the administration of the system, which I think has been sincere and effective in practically every government agency.

In addition, these numerous small projects present an administrative burden. Various individuals write proposals; their department chairmen review them; the central administration of the university reviews them; the proposals are then sent to the government agency; the agency has other scientists in other locations referee them; the section chief of the agency makes recommendations; his superior reviews them; eventually the responsible official of the agency gives the final approvals. Next the contract or grant is written; possibly the university business manager objects to it; there is further negotiation, and so forth. In other words, there is a lot of administration for each individual sum of money. I am very happy to see that the NSF has chosen to adopt a relatively standard form of grant with a minimum of business details; nevertheless, the scientific reviewing remains.

Whether it is feasible for the Government to use a different type of distribution I do not know, and I am not recommending any immediate change. I think that the Government should see to it that university research is adequately financed, not necessarily that the

Government finance it directly. If steps can be taken to cause adequate funds to flow into university channels from private sources, this will be far superior to an attempt to modify the government method of support of research in order to overcome these objections. There will still remain the larger projects that need the Government's attention; it might be better to keep the government activities in that sphere. Some industrial funds have been supporting research in a manner that has been very effective; on the other hand, some industrially sponsored research in universities should probably not have been put in a university at all. I do not mean that industrial activities in this area are better or worse than those of government; I mean simply that the best of the industrially sponsored activities are excellent and should serve as models for further expansion.

The foundations, of course, have had considerable experience in handling the support of research with a minimum of restriction. The tradition of a foundation is to say "once we have made you a grant, you go ahead and spend it as you see fit."

On the industrial side the Du Pont Company, to mention one, has been making some general grants in chemistry in recent years that are completely unrestricted with regard to the manner of expenditure. In other words, the funds can be incorporated with other departmental funds and used in whatever fashion seems appropriate and necessary. A relatively moderate addition to the number of general grants of this type would alleviate many of the difficulties involved with government projects and, in fact, would alleviate the need for the smaller government projects.

I hesitate, without more study than I have been able to give, to mention a dollar sum, but it seems to me that this could be estimated relatively easily by looking at the total magnitude of small grants from all such agencies as the NSF. I believe about $20 million per year goes into relatively small grants in the natural sciences. This is a very small proportion of corporate profits. If industry were to distribute this sum in an unrestricted fashion, and if the sum were divided on some reasonable basis among the various universities, the investigators in the smaller, less expensive types of work would be free from the necessity of applying for special funds and from all the concern and possible restrictions that I have mentioned.

This is a challenge that American industry ought to consider very seriously. Does not the solution to this problem lie in this line and at an expense that would not look large from the point of view of the entire scope of American industry. I might also comment that funds that are made available to universities on this broader basis are, I am sure, used very much more efficiently. Once a research grant has been broken down into small units as a matter of direct negotiation with the Government, there is an obligation, either to spend it within the bounds of that original project or to allow it to revert. If the money came to the university on a broader basis, the plants for expenditure could be rearranged if new needs arose and new developments occurred. This would yield a much more flexible basis that would provide much greater efficiency of utilization. I have estimated, as a matter of fact, that the $10,000 per year which the Du Pont Company gives the chemistry department of the University of California is more important for research purposes than $20,000 to $30,000 in the small grants that are tied to specific subjects of investigation and cause various difficulties in their use.

The natural effect of all government aid to research has certainly been helpful. I do not know what the universities would have done in this period had these government funds not been available. I think the administration by various government agencies has been excellent. My critical comment is strictly about the system that breaks down the funds into very small units that are tied closely to particular subjects of investigation. I hope that both government and industry can contribute to improved methods in the near future.

News and Notes

Pacific Division 1955 Meeting

The Pacific Division of the AAAS will hold its 36th annual meeting at the California Institute of Technology, Pasadena, 20–25 June. The Division includes members in California, Oregon, Washington, Idaho, Montana, Utah, Nevada, British Columbia, and Hawaii.

Twenty-six scientific groups will have sessions in the following fields:

Astronomy: *Astronomical Society of the Pacific,* contributed papers, 21, 22 June.

Chemistry: *American Chemical Society,* Southern California Section, symposium on "Free radicals," 21 June.

General: *Federation of American Scientists,* Los Angeles Branch, symposium on "International exchange of scientific personnel and ideas," 23 June. *Southern California Academy of Sciences,* contributed papers, 21 June.

Geography: *Association of Pacific Coast Geographers,* contributed papers, 20, 21 June; field trip, 22 June.

Geology: *Geological Society of America,* Cordilleran Section, symposium in two parts: (i) "Trace elements in igneous and sedimentary rocks and in deep sea sediments," (ii) "Metamorphic problems," 23 June.

Mathematics: *American Statistical Association,* symposia on "Quality control," "Applications of electronic computers," and "California's population: a study in dynamics," 23 June. *National Science Foundation,* symposium on "The theory of numbers," 22, 23, 24 June. *Bio-*