

# The Role of Basic Research

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I suppose that I should begin with a definition of what I mean by the expression “basic science”. The science of semantics resembles a sort of non-physical theory of relativity which exposes to our view the frames of reference underlying our words, whose meaning we assume, often unwittingly, is understood. Certainly our attitude toward the expression “basic science” depends on our educational and scientific frame of reference. Person A who speaks of himself as a scientist with an inflection indicating his deep pride in being so regarded would, no doubt, define whatever he is doing as basic science and anything with more recognizable utility as applied science or, even worse, as engineering. Another and purer scientist B might regard what Mr. A is doing as the crassest commercial prostitution of a scientific education.

Scientists A and B belong to the halo school of definition of basic science, as shown by their frequent substitution of the expression pure science. The undercurrents of suggestion here of unadulterated, dedicated, uncontaminated, unfettered, unrestrained, unsullied search for truth provides a real lift to many an underpaid scientist and a real substitute for money — provided he has married the right kind of woman.\*

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\*In 1955, we assumed a scientist was male. Please accept my remarks in the context of the times.

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But the world consists of many types of people and many scientists do not look at the term, basic science, with glasses of the same color. Some actually use the expression as an epithet. The distinction between a Ph. D. and an M. D. is sometimes made that a Ph. D. is the kind of doctor who doesn't do anybody any good. Or we can recall that a prominent director of one of our largest industrial research laboratories is supposed to have said that basic science is that scientific activity which cannot possibly provide any financial return to the people paying the cost of doing it.

Most of us here today probably associate ourselves more closely with the halo school of definition of basic science than we do with the epithet school. I know that I personally do. Yet most of us have been closely enough associated with practical problems in wartime military applications, in industrial research, in the development of new chemo-therapeutic agents, or with other matters so that our purity has been badly sullied. For most of us it is symptomatic of our real attitude that when we try to defend basic research to visiting state legislators or federal budget review officials or boards of directors, we almost universally point to the most outstandingly practical applications we can single out and swear that these could not possibly have happened without the basic research of past years. Clearly, one of the major peacetime uses of the atomic bomb has been to blast loose purse strings to obtain money for basic research in physics. Even so, it is surprising how many equivalent megatons of persuasion this takes.

Hence we might think that in a sense basic research is not really different from applied research in its real purpose — it is merely applied research with a time delay. How happy a person is in doing basic research is frequently a matter of how long a time delay he is adjusted to or is willing to tolerate. Willard Gibbs, perhaps the greatest scientist America has produced, was able to see very little practical influence of his monumental contributions to thermodynamics during his lifetime, but it is doubtful that this ever caused him any concern or deterred him from the most vigorous prosecution of his theoretical studies. His time delay was set at infinity. We all know persons among our personal acquaintances who spend about a decade in basic research pursuing studies which appealed to them for their intellectual stimulation and with no distinct utilitarian purpose in mind and then reoriented their careers to exploit some promising discovery of their basic science days. Frequently such a person is quite happy in both phases of his scientific career and would have become

distinctly unhappy had he attempted to stay oriented in basic science for his entire life. The time delay for such a person is about ten years. At the end of the scale there must be industrial divisions of "Research" where the new Ph. D. staff member may be allowed a couple of months to brood on and attack some fundamental problem of wide scientific interest provided he comes up with some real moneymaker by the end of this time. The time delay here falls in the range of months down to microseconds.

I hasten to state that I have no intention of trying to name a time delay that distinguishes between basic and applied science. For I was not really serious in suggesting that there was no real distinction between basic and applied science other than the provision that we be more tolerant of the basic scientist and allow him more time to produce some useful results. Actually utility is not a real basis for talking about basic science. There is no reason why basic research results cannot have immediate, important, financially rewarding application. X-rays were in use in a Vienna hospital within 3 months of their discovery, in 1895, but this does not mean that Röntgen was not doing basic research. Nor is there any reason why they need ever have clearly discernible practical results. The distinction, I think between basic and applied research lies in the motivation behind the research and the criteria that are applied to determine what work shall be undertaken and what changes shall be made in the lines of investigation as the study develops. In basic research the motivating force is not utilitarian goals, but a search for a deeper understanding of the universe and of the living and inorganic phenomena within it. Its keynote is intellectual curiosity.

Now I realize that a great amount of flowery nonsense has been written on this point. The scientist has been portrayed as a dedicated seeker after truth, bending his every effort to extract the last ounce of understanding from reluctant Nature, scornful of utilitarian applications of his work, completely altruistic, and receiving a bountiful reward of inner satisfaction quite sufficient to his needs, scornful of monetary gain, fame or outward rewards of any kind.

This description does not fit me. Probably it *does* fit you, but you will undoubtedly agree with me that it is not an accurate description of the person on your left or on your right here today. Scientists are by and large quite ordinary people. Within their specialties their natural intellectual capacities are greater than the average man's and their trained competence

is certainly greater, but as human beings they are subject to the same shortcomings, the same wants, desires and drives as anyone else. Nevertheless, I think we are correct in saying that the underlying motivating force in basic research is intellectual curiosity, and that this curiosity is to be rated with the highest qualities of mankind. The investigator may be a thoroughly disreputable character in many ways, the intellectual curiosity may belong to his boss or his professor rather than to him, but somewhere behind the work he does is the desire to find out *why* or *how* or *what*.

We can use some clues to determine how basic a research program is. If the final goal is very precisely stated, the program is probably not too basic. If the investigator is not free to make radical changes in his program, and to pursue some unexpected question which has arisen in his work and which excites his curiosity as to *why* or *how*, the program is probably not basic.

With these comments by way of outlining what I have in mind when I talk of basic science, let us get on to consideration of the role of basic science.

At the risk of appearing inconsistent, let me state that the arguments we present to state legislators or federal budget reviewers or boards of directors are undoubtedly valid and that a good reason for supporting basic research is because of the practical results such support ensures. This is a ticklish argument because it has to be made clear that these results may be a long time coming, that most of the projects for which support is requested should not be expected to be fruitful but that a broad-gauge support toward goals defined in only a very broad sense are required if major discoveries are to be expected.

Insofar as an appreciation of this rather subtle point is produced in the government administrators, university officials and industrial managers, a climate is created in which basic science can thrive. Certainly, great strides have been made during the past few decades, particularly in the post-war years, and mutual understanding of scientists and administrators has been good. Much remains to be done, of course. A poor appreciation of this point by administrators of research funds can mean the stifling of basic studies by the requirement of detailed program goals, project justifications, etc. which suggest to the investigator that unless he achieves the stated goal by the end of his contract period a renewal of his contract is not likely to be forthcoming. The result can be programs representing rather

unimaginative extrapolation of known results which waste the scientific powers of the applicant.

The university by its very nature and purpose is and will remain the natural center for basic research. In our major universities the administration of funds supplied from the general university budget has been generally favorable to the fostering of fundamental studies. The rising costs of research studies, particularly in the physical sciences, and the growth of the participation of private corporations, private foundations, the National Science Foundation and State and Federal Government agencies [particularly the ONR (Office of Naval Research) and the AEC (Atomic Energy Commission)] have created a new problem. The sizeable amounts of money these agencies have put into research and development have unquestionably been a very strong positive influence on the growth of basic research. Furthermore, the men who have administered these funds have had a fine appreciation of the role of basic studies. However, in their dual responsibility to the scientist on the one hand and to the taxpayer or stockholder on the other, it has not yet been possible to work out mechanisms for the disbursement of research funds which avoid the tendency of the small-project system, with all its red tape for reviewing and referring and project justification, to take away the freedom of the talented investigator to wander off into really uncharted regions of intellectual and scientific endeavor. It should be possible to say to more scientists of demonstrated competence and responsibility, "Here is some money to keep you going. Run along now and do whatever you want. Don't bother us anymore about it. If you find out anything worthwhile send it into a scientific journal. All we ask is that you work hard. In fact, don't even do that if you can get more accomplished in another way." Some grants at the present time are in effect administered in this way with very good results. In the end, putting money in basic research is betting on the person who is doing the work, not on the worth of the program which he foresaw before he began it.

There is little doubt in my mind that we would achieve more in the long run per million dollars expended in research if this system were more widely used. I feel that basic research conducted by universities would be more efficiently done and more likely to reach beyond limited objective studies if lump sums of money were provided by external agencies to the general university budget or to general departmental budgets for

administration in the manner traditional to university research rather than disbursed piecemeal to small projects bearing specific titles. I am certain we will make improvements in our present financial arrangements. The whole business of wide scale support of university research by industrial and governmental agencies has grown so fast that we have been forced to use improvised contractual arrangements which in many cases are more suited to the limited objective developmental project than to the fostering of the fundamental research.

I should hasten to add that a generous supply of funds for basic research is not in itself sufficient. Money does not directly beget new ideas. We need to attract increasing numbers of people who are capable of creative thinking. Traditionally, the occupation of unfettered research has been centered in the university professor who also has the dual role of a teacher. The union of these two activities has in my estimation been a happy one because, on the one hand, advanced instruction is surely more inspiring if given by someone who has first-hand knowledge of the subject irrespective of his abilities as an orator, and on the other hand, there is nothing more stimulating to uninhibited thinking than the uninhibited questioning of successive generations of students. We are now faced with the delicate dilemma of being able to foresee the availability of funds for creative activity without the likelihood of the increased number of teaching professors to make the best use of them. It seems to me that the universities must have the support for creating more positions which are comparable to that of the professor in freedom, prestige and tenure. Whether these "research professorships" also entail formal teaching or not is not quite as important as is the contact with graduate students and the general university atmosphere. In any case, the point I wish to emphasize is that a healthy sustained increase in basic research in a university cannot be built upon people hired on the basis of one-year contracts, generous as these might be. There must be a general increase in the permanent staff, even though this might mean its size goes beyond current teaching needs.

The role of basic research in the training of scientists and engineers is a very important one. Beyond question the university graduate school is the most effective device we have for the cultivation of the intellectual powers of a potential scientific investigator. The paramount business of the university graduate school is to make learning possible. In principle there are no boundaries on the questions to be asked and the answers to

be sought. An atmosphere of this nature is ideally suited for the inspiration of the superior human intellect and for challenging this intellect to its greatest exertion. We can cite many examples from our own experience of very promising men who did poorly through the grade school and high school system because the educational system was geared to the mediocre student and failed to strike a spark of interest in them. Some of these failed to see this inspiration in college because of dull and uninspired professors and teaching methods and wandered off into life to leave their potential intellectual power dormant for ever. Others of this kind happily survive to go on to graduate school and there catch fire when they learn the true meaning of scientific research. There is no human intellect, however great in genius, which cannot find a challenge worthy of its powers in contemplating the structure of the atomic nucleus, the nature of the chemical bond, the physiology of a living organism, the meaning of life, or the origin and the future of the universe. The more a university or any other agency engaged in basic research can stick to the goal of unrestricted human thought, the less likely the great intellects are to be deterred from direct contact with the great unanswered questions of science.

Whether or not we follow our ideals closely in graduate school training, we do try to orient our research toward basic problems and feel that the student has a better development of his scientific abilities and intellectual curiosity because of it. The companies and laboratories to which we send our graduates may not always share our enthusiasm for this system and for its sometimes almost complete divorcement from economic and engineering problems. Frequently, when a fresh Ph. D. goes to his first job an agonizing reappraisal takes place. Whether it is more agonizing for the Ph. D. or for his new employer is sometimes hard to say.

There is a basic difference between undergraduate and graduate because of the fact that the undergraduate is primarily concerned with learning what others have done while the graduate student is trying to obtain new knowledge. His later activities convince him how much there is yet to be learned and give him confidence that he can help in unveiling the unknown. The point is that without a first-hand, rolled-up-sleeves contact with the frontier dividing the known and the unknown, a student is likely to have a gross misconception of the lush lands beyond the frontier which restrains him from trying to get there. Our scientific predecessors have been very diligent. Our libraries are so crammed with their findings and

our daily lives so surrounded by the marvelous tools, machines, gadgets and conveniences based on their work that it is hard to avoid the feeling that most everything has been learned. Our textbook system which tends to present science in an overly organized and dogmatic manner reinforces this view. We strive by books, lectures, TV shows and demonstrations to show how incorrect this is, but the belief is deeply ingrained in our subconscious and strenuous efforts are needed to overcome it. Research in pure science is a most effective antidote for this. Let me give you an example. No one doing research today on the new subatomic particles in physics can have the slightest doubt that what we know about atomic nuclei is but a tiny speck compared to our ignorance. We used to think we had reached our ultimate building blocks of atomic nuclei in the proton and the neutron, and for a few years we rested thinking that a little theoretical work would clean up the problem and send us looking for new problem to solve. But look what happened. We now have pi mesons and mu mesons and tau mesons and kappa mesons and theta mesons and V particles and many more, some of which come in three varieties, positive, negative and neutral. In the last few years the number of fundamental particles — and here I use the term very loosely — has grown to more than twenty. These days it has become almost as important to “undiscover” a fundamental particle as it is to discover one.

The activity in this area can only be described as feverish. Here is basic science at its best. Here is a problem with no foreseeable answer which is making an irresistible appeal to the best efforts of physicists all over the world. Bristol, Milan, Brussels, Ecole Polytechnique, in Paris, Padua, Manchester, Bombay, Columbia, Brookhaven, Cornell, MIT, California, Chicago. These are exciting meaningful names to the workers in this field. Most of these men are paid to do this work. But no pay could account for the eagerness, the industry, the ingenuity with which these men attack their illusory goal. Intellectual curiosity and the thrill of helping to unravel secrets which cannot help but be of the most profound importance for our understanding of the universe in which we live are what drive them on. Here also we see exemplified the teaching role of basic science, for at each one of these international laboratories you will find an eager group of young graduate students participating fully in the program. No need to lecture to them on the scientific method, to urge them to master the mathematical tools or the experimental tools of their trade, no need to

tell them that the physical world has not been completely explored and explained. Discarded hypotheses lie scattered all around them. Mathematical descriptions of great complexity enter into their daily talk, the fast electronic circuits, the emulsion techniques, the cloud chambers, the bubble chambers are the apparatus without which the data could not be collected. The challenge to their skill is great and the development of their manual and mental powers will stand them in good stead when their graduate school training is complete, even though their careers may take them far from the world of subatomic physics. In this connection, I am tempted to tell you the story I heard somewhere of the physicist who was working on the design of the reactor to be placed in the submarine Nautilus. He complained that he had gotten his training in subatomic physics but had had to switch over into atomic sub physics.

I have been deliberately speaking about the role of basic research broadly, rather than solely in relation to atomic energy, which, of course, is of especial interest to you here today. However, the interests of the atomic energy field are so broad that I feel that this is justified. The Atomic Energy Commission, I believe correctly, is supporting basic research over a very wide spectrum, not limited to fields which seem to have foreseeable application to present endeavors. I do not believe that it would be profitable to use some of the limited time at my disposal for any kind of description or summary of these programs. As you know, these cover broad areas of physics, mathematics, chemistry, metallurgy, biology, medicine and related fields. Much of this research is directly related to reactors and weapons, but much of it is completely uncommitted and is basic science in the purest sense. Although the greatest proportion of the research budget is allocated to the Atomic Energy Commission's own large laboratories, a reasonable proportion of the available money is used to support basic research through hundreds of contracts with universities, colleges, industrial and private foundation laboratories. Since the primary interest of most of you here today probably has to do with the progress of the development of the atomic power industry based on nuclear fission, I suppose that this might be considered a good occasion to try to point out some specific areas where increased activity in basic research might be required. High temperature thermodynamics is certainly one such field. A number of areas in the engineering sciences, such as heat transfer, are also in this category (forgetting for the moment our attempted definitions of basic

research). In my opinion, a limit to the extent to which atomic power can be used in the future will be set by the ultimate solution to the waste disposal problem, and here, it seems to me, the path for the ideal solution must lie outside of any ideas presently contemplated.

I would next like to comment more specifically on the overall amount and proportion of basic research and the budgets expended for this purpose in our country. The total research and development expenditure, by government, industry, universities and non-profit institutions, is presently at a rate of about 3 billion dollars a year. Any attempt to estimate the proportion of this devoted to basic research emphasizes our difficulty of definition again. However, even a generously broad definition indicates that no more than 5 percent of this total annual budget is used for the support of basic research. It is my opinion that this proportion and amount of money expended for basic research is much too small! I believe that we need and should work toward an expansion by something like a factor of two as soon as possible. The efficient use of money for basic research at such an increased level is, I believe, easily possible in spite of the shortage of scientists. The results would not go up by a factor of two with a doubled budget but the increase would be so large that this would be the greatest bargain that the American people ever received for their money! For example, the budget of the National Science Foundation should, in my opinion, be rapidly built up to the proportions envisaged in the original Bush report and the amount of money spent by the Atomic Energy Commission on basic research, some 40 million dollars per year, could very profitably be doubled. Correction of the present inadequate ratio of basic to applied work would soon lead to such a broad improvement in technological advance that the return on the overall budget for research and development, on any objective basis for evaluation, would be increased way out of proportion to the small relative additional cost. The problem of a properly balanced research effort is paramount today not only because of the importance of our defense research, upon which our very existence depends, but also because our whole national economy is rapidly becoming tied closely to continued technological advance.

I also want to make a comment or two about our critical shortage of scientists and engineers. Obviously, my comments on the needs for larger budgets for basic research apply with even greater force to the need for more people in this area. Although I do not have the time to discuss this

serious question in any detail, I do want to remind you that practically all studies show that a serious loss of potential scientists occurs in high school and in the step from high school to college or university. We simply must do something to inspire more high school students through the improvement of the quality of science teaching, which can only be done by improving the position of such teachers so as to lure more able people into this profession. In order to entice a larger proportion of high school graduates into scientific fields it will be necessary to improve the status of the scientist to the point where the attraction will compete with that of careers in other professions and in business. Such unfavorable comparisons do not seem particularly to bother the scientists themselves, and a certain proportion of high school graduates will continue to choose scientific careers under present circumstances, but it will probably be impossible to effect the needed increase without some added incentives.

I would like to conclude with another remark on the subject of improvement of education in science, in this case general education in science. I believe that a good case can be made for the thesis that a better education and a greater understanding of science by the general public may very well be a prerequisite to working our way out of the present international dilemma brought about by the amazing weapons which science has produced for both sides of the conflict of political ideologies. This was put in different language recently by the head of one of the large industrial concerns in our country when he said, after partially defining culture as "a particular stage in civilization and the particular features of that stage," and a cultured man therefore as "one who has sufficient knowledge to fit his environment," that science should now be considered one of the humanities and "therefore, there is no reason why all men of culture cannot and should not have a reasonable grounding in basic science and an appreciation of its problems."

