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The Role of Basic Research in Universities

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At a time when government and society are reassessing national priorities, and support for basic research is uncertain, a clear definition of the role of basic research in universities is of vital concern. It is therefore appropriate that we examine university goals and the contribution of basic research toward attaining those goals. Arguments are put forth below to the effect that education is the only proper endeavor for a university, and the prime role for research in a university is as the principal tool for graduate education. It is the education of men and women, who through research learn to think creatively and imaginatively, that justifies a significant university involvement in basic research.

Basic research has often been defended on the ground that it leads to or supports practical developments that benefit society. This case has been stated strongly and clearly by DuBridge [1967] but the findings and implications of Project Hindsight [Shemin and Henson, 1967] appears to many to be at least as persuasive. The feeling that research expenditures should be justified in terms of identifiable benefits to society is well put by Congressman Craig Hosmer [Hosmer, 1968] who states: 'The science community should take greater pains to make clear that its efforts contribute directly and indirectly to progress benefiting every man, woman, and child in the country. The public will not buy science for science's sake-so sell it to them for their own sake. Public interest is in the human sciences, man as a living being and man in his environment. That is where the money will be. Therefore, adjust research priorities to the public's priorities to the extent possible. The public does not ask for a money-back guarantee if an idea fails, but it wants reasonable assurance of some visible benefits if it succeeds.'

It is not hard to provide such assurances for applied or 'relevant' research. However, providing reasonable assurance of some visible benefits to society from most basic research projects is difficult at best. How do we relate basic research to the problems that pervade today's public interest? What visible benefits can most basic research projects provide toward solutions of problems such as air and water pollution, poverty, integration, garbage disposal, growing urban crime rate, the war in Vietnam, etc.? When one realizes that the expenditure for basic research in a typical university with a strong science-engineering graduate program is of the order of ten million dollars per year, it becomes clear that there is a lot of justifying to be done. As anyone who has tried can testify, the link between a specific basic-research project and a projected practical application is tortuous and often unconvincing. It is true that some undirected basic research does occasionally payoff in a practical way. However, most of the individual research activities conducted on a university campus do not.

The obvious truth is that people in a university normally undertake a specific basic research project because it interests them, not because it may prove to be of practical value. It is then difficult to provide, after the fact, a link with matters of practical interest.

We should question whether it is sound policy to continue justifying expenditures for basic research in terms of direct practical benefits. For example, Hoyle [1968] asks 'whether justifying ourselves with gadgets is really the way we should look on our relation with society. I think the policy is unsatisfactory because it is basically dishonest; we are not what we pretend to be; we are not in business as widget manufacturers.'

If not with gadgets, with what can the academic community best defend and justify the expenditure of public funds for basic research? The answer, I believe, is to look at research, both basic and applied, as means through which we achieve some valued goal. That is, even basic research should be considered as being applicable to a specific purpose. In a university, this purpose is the support of the educational objectives of the institution. With applied (or relevant) research, the practical benefit is usually easily identified. However, the assumption that the results of basic research, which have often turned out to be of practical value in the past, will continue to be so in the indefinite future is no longer widely accepted. Furthermore. This assumption may no longer be valid. The natures of contemporary society and of science and technology have changed markedly.

Basic and Applied Research

Basic research began to affect technology significantly in the period beginning around 1900 with the introduction of the results from basic chemistry research. Electromagnetism, solid-state physics, and nuclear physics followed with dramatic impacts that have been documented repeatedly and convincingly. Thus, starting around 1900, we sec that basic research did indeed produce a rapid series of benefits for society. It is fair to state that nearly all of the technological achievements of our society rest solidly on a foundation of basic research. However, applied research has evolved in a sophisticated way since the end of World War II and has taken the lead in providing material benefits to society. By applied research, I mean only that the research is directed toward some practical objective, even though that objective may not be clearly seen or immediately accessible.

An example of applied (or directed) research is the development of the transistor at Bell Telephone Laboratories. In this case, the research management committed funds and manpower to the study of semiconductors with the thought that a better understanding of semiconductors would lead to new or improved devices for communications. The research was directed in the sense that the program had as its ultimate objective the development or improvement of communication devices. The techniques and the physical laws used in this research program were the same as those that would be used in a similar program of basic research that had no practical objectives in mind.

The difference between basic and applied research then seems largely to be whether a practical or a purely intellectual result is the conscious goal. Discovery of new laws of nature, which ultimately benefit mankind, is more likely to arise from basic than from applied research. However, fundamental discoveries that change basic physical laws are so rare that these events are hardly useful as either a distinction between basic and applied research or as a justification for support of basic research. Indeed, according to Wheeler [1968], 'Not since the quantum idea flowered into wave mechanics in 1925 has there been a change in fundamental principle.... Regularities, yes; beautiful symmetries, yes; but new laws, no. Not for 44 years!' He continues, 'No one in chemistry or biology feels himself cheated because the relevant physical laws are already known. There is challenge enough, and to spare, in unraveling fresh regularities and in finding new ways to put together old building blocks. So too in physics. And with each passing decade we understand the principles better because we have applied them to more issues. We believe in them all the more firmly because they have never let us down ... not one single effect has been discovered which has led to a new law of physics, and not one single finding has ever been obtained which is generally recognized to be incompatible with existing law.' This is not to say that no new law will ever be discovered again; it is just that the interval between discoveries is long. The foundation of knowledge gained from both basic and applied research is necessary in order that fundamental discoveries can be made. Meanwhile, nearly all research is performed with heavy reliance on the laws of nature as we presently know them. Inconsistencies between the research results and these laws are, almost without exception, ascribed to error in the research. The difference between 'applied research' in industry and 'basic research' in universities is principally defined by the goals of the research, rather than by techniques or methods.

Urban Problems

Support for basic research has been weakened by the apparently sudden public realization that scientific and technological progress has not been all for the good. Problems of air and water pollution, the population explosion, the invasion of privacy, and the difficulties in urban transportation are examples of social ills that can be attributed to undesirable side-effects of both basic and applied research.

Because the role of basic research in universities is not well defined and because the difference between basic and applied research is often slight, a new pressure on universities is developing. That pressure is for the universities to drop or reduce basic research and to engage in research relevant to the social problems of our times: pollution, civic disorder, poverty, transportation, integration, etc. It is very unlikely that universities can be organized to work effectively with city, state, and federal governments for the solution of these operational problems. Universities are traditionally slow to react in an organizational sense to change, and, if universities were somehow restructured to handle such operational problems, their creative educational function would be seriously damaged. In order for universities to become directly and meaningfully involved in urban problems, they would undoubtedly have to organize interdisciplinary or interdepartmental research programs that would have to be directed to achieve the desired objectives on a set schedule and within a framework dictated by the operational requirements of government. There would be little room for basic, undirected research. If we go a step farther and ask what organizational and management structure would have to be placed on a university if it were to accept line-responsibility for operational problems, we can see that the least we should expect is the destruction of academic freedom as we now know it. Note that I am not arguing against university research, either basic or applied, that is relevant to social needs. The point I wish to make is that active participation of universities in the operational problems of government would be harmful to the educational function of the universities.

A close student-faculty relationship is essential to superior education. A professor distracted by extra-university matters cannot contribute effectively to the demanding task of education. Already there may be cases of so great an involvement of university faculty in governmental and industrial problems that the educational environment on campus has suffered. If we consider the distractions, operational priorities, and necessary management activity that would be required to meet the operational needs of an interdisciplinary program of urban research, we can see that conditions on campus could become so extreme that students would be regarded as a hindrance rather than as a primary responsibility. This would be a tragedy, for while there are many varied organizations ready and able to work on problems of society, there exists no organization other than the university to fill its educational needs. (The research activities themselves are not a distraction -- research is the primary tool for graduate education, and in addition, research provides a form of self-education for the individual faculty member and a more stimulating environment for the student.)

Research and Graduate Education

If we accept the thesis that the university today is not organizationally equipped to become involved in operational problems and that it would be harmful if it were to become so, we should then ask, What is the function of the university in modern society? The answer is, I believe, an obvious one: the proper function of the university is education. This answer need not be qualified or modified by the inclusion of other functions such as 'community service' or 'acquisition of new knowledge.' Education is, after all, a vital community service and acquisition of new knowledge is a necessary by-product of graduate education. Although it may not be possible to restrict university activities exclusively to education, those activities not supporting educational objectives should he kept to a minimum.

Even though universities may not contribute directly toward solutions to the several urban problems that presently trouble our society, they do indirectly through their primary product -- the educated citizen -- contribute a great deal. Solutions to pressing problems of today (and tomorrow) will be provided by creative, innovative, and educated individuals. While the university will not be the sole source of such individuals, it will certainly be the prime source. Thus, it is the output of the university, not the university itself that should be looked to for the solution to operational problems.

As problems of society that grow from science and technology become more complex, the level of creativity and education required to deal successfully with these problems increases. If the level of creativity required is high, an undergraduate education will, in general, not be enough. A graduate education involving research is one obvious way to provide the additional education that is necessary.

It is useful to state here the difference between a graduate and an undergraduate education, and the relationship of research to graduate education. Following Booker [1963], we define the ideal undergraduate education as one in which the student learns how to understand and apply what is already known. The ideal education for the Ph.D., on the other hand, is one in which the student learns how to solve problems for which there are no known solutions. (It must be acknowledged that these ideals are not always met.)

The value to our society of educated citizens, who, in Booker's words, 'have reasonable confidence in [their] ability to face what is novel and to continue doing so throughout life,' is obvious. The Ph.D. recipient has the flexibility and mental attitude necessary to recognize, attack, and solve problems that are unlike any ever solved before. Because many of the problems of society have their base in science and technology, we should look to the graduate programs in engineering and science for the trained manpower needed to solve these problems of our time.

A recent study by the National Science Board [1969] shows that the size of the nation's graduate education program is smaller than optimum. Their report states quite firmly that 'it is not possible to produce too many highly educated people in the United States as long as appropriate educational standards are not sacrificed.' They further show that for the next few years, it is necessary that graduate education grow proportionally faster than undergraduate education in order to supply the nation's needs.

If we adopt the proposition that education is the only proper business for a university, the role of basic research in universities would be defined by its educational function, and an appropriate funding level would be established. The minimum level of research funding at a university would be set by the size of the graduate education program. In order that the programs be intellectually stimulating and of high quality, the research on which these programs are based should demonstrate such qualities. Research should continue to be judged on national standards by peer groups so that research excellence (and therefore educational excellence) is not sacrificed in order to turn out large numbers of Ph.D.'s at low cost. Thus the character and scope of university research need not change; it only need be recognized that the primary function of basic research in a university is the support of graduate education. Except for special cases, research programs that cannot attract graduate students should either be dropped, supported at a minimum level, or conducted in governmental or industrial laboratories. The precise fields of research need not be defined; it is necessary only that the research be effective in graduate education.

This last point bears on the problem of the relevance of modern education. The problem is an old one. An education that is relevant today may well he old-fashioned tomorrow. It is not possible to foretell the future so accurately that an educational program can be created that will cover the lifetime needs of a given individual. It is far more practical to have a broad educational program that enhances an individual's creative potential. For example, the discipline of a Ph.D. program in high-energy nuclear physics is quite satisfactory in this regard. While the training is not necessarily relevant to problems the Ph.D. recipient will tackle later in life, he has learned from his thesis work how to enter a field about which he knows little and, through diligence and organized effort, make a significant and original contribution. Having done it once, the Ph.D. recipient should feel that he can do it again in a different field. The best research is that which is effective in attracting and intellectually challenging the best minds of the nation. Their natural ability and their training, plus the realities of the market place, will take care of the problem of relevance.

Summary

It is difficult to see how the problems of society and government can be solved by direct university involvement. Rather, the universities should maintain, in a narrow sense, the concept that their only proper business is education. Community service and acquisition of knowledge are valued by-products of this primary mission. Graduate education can be used to provide society with a large number of people who are trained to think creatively, who can solve problems whose solutions cannot be looked up in a book. Universities are the only institutions that can provide this national resource on the scale required. The essential point is that the most creative talent available to the nation will flow from university research programs. The level of federal funding required to establish 'and maintain research programs to be used for graduate education should be tied closely to the number of able students wishing to obtain advanced degrees. Those research programs of high quality that are successful in graduate education should be the ones supported as the minimum program of basic research in a university.

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