1. Recognition of the need for long-range research--As professionals, we must bring the long-range future of transportation into clearer focus and recognize the critical need to establish some clear direction and positive policies and goals.
2. Develop methodology for long-range research--We must interest some capable transportation researchers in conducting long-range research and have them work with futurists and other disciplines to evolve a methodology for long-range transportation research that is responsible and productive.
3. Develop means of using long-range research--We must also recognize the past failures of long-range researchers and develop the means for implementing our long-range findings. This means involving and informing our political and economic leaders and convincing them of the critical need to adopt and implement long-range policies and goals.
4. Commit funds for long-range research--The first three steps cannot be accomplished without funds. Thus, we must find a way to convince those who allocate funds for transportation research that there is a critical need for long-range research and that research funds must be provided to support the needed studies. This will be very difficult because there is an abundance of short-range research needs and insufficient funds. It will not be easy to convince those who allocate research funds to commit substantial amounts of funds to what may be considered pie-in-the-sky studies. We must overcome this image with sound, practical research methodology and good communication.

The noted economist Garrett Hardin has said (2), "Ruin is the destination toward which all men rush, each pursuing his own best interest". There is a multitude of best interests involved with the problem of providing for urban transportation. These include those of the politician, the land owner, individual citizen groups, and the transportation professional, to name just a few.

Lester Thurow (3), has termed the United States a zero-sum society. He indicates that on any national effort there must be winners and losers. If we build new transportation facilities, there will be a multitude of persons who will have increased mobility and will be winners. There will be losers also, such as social programmers, environmentalists, home owners (who may have to move to provide rights-of-way), and others. Our system is such today that the losers (even though the losses may be minimal), if they so desire, can effectively block almost any program. This problem has become so serious that one can rightfully ask, Is it possible to implement any large long-range-development program in our present society? If I were pressed for an answer to that question today, I would probably have to answer "no."

Thus, if we wish to call ourselves transportation professionals, we must recognize the very critical situation into which we are drifting. We must clearly define our long-range problems, determine what must be done to address them, and embark on a well planned effort that will achieve desired future objectives. To accomplish this, we need an effective program of long-range research in transportation.

REFERENCES

1. E.S. Cornish. The Professional Futurist. In Mankind 2000, 2nd ed. (R. Jungk and J. Galtung, eds.), Columbia University Press, New York, 1969.
2. G. Hardin and J. Baden. Managing the Commons. W.H. Freeman and Company, San Francisco, 1977.
3. L.C. Thurow. The Zero-Sum Society. Penguin Books, New York, 1981.

Basic Versus Applied Research: How to Maximize Effectiveness

JAMES D. PALMER

A dilemma has existed regarding basic and applied research. The characterization of this dilemma is noted by the competitive environment in which these modes of research are carried out. Basic and applied research are important and must take place in a supportive environment. Adequate funding for each is necessary to maximize effectiveness and to facilitate growth in knowledge and in application of knowledge. The nature and development of research are traced to modern times and conclusions are drawn as to the need to maximize research effectiveness for basic and applied research.

Traditionally, research has been contrasted by two approaches; fundamental or basic research, which is carried on without regard to the immediate utility of the outcomes, and applied or industrial research, which is directed toward the solution of specific problems. These definitions carefully delineate the environment in which a particular research thrust is developed and provide an implicit statement about the funding for research.

In this paper, the thesis will be advanced that, in order to maximize the effectiveness of research, whether basic or applied, it will be necessary to understand the issues that surround the topic, review policy perspectives for future research work, understand the political funding environment, and develop national priorities related to the utility of research outcomes.

The support of research, basic and applied, is essential to the development and advancement of concepts and ideas. Creativity is enhanced by research. Conceptualization, idea development, and creativity are essential for the continued positive evolution of mankind. In our time, the nature of research and the uneven record of benefit to mankind has come under minute scrutiny (1). To maximize the effectiveness of research efforts, we must restore public confidence.

HISTORICAL OVERVIEW

Research, as we know it today, is a relatively modern development that comes as late as the onset of the Industrial Revolution period. Activities in fundamental research have been ongoing in universities in some form or another since universities have existed. The earliest forms of research were of the kind related to manuscript preparation and records of observation of physical phenomena. These activities have never existed in a benign environment.

Throughout recorded history, the place of research has been known and challenged. Until the Industrial Revolution (2) it was possible to categorize research into two entities--university research and industrial research; however, little activity took place as planned industrial research. Generally, basic research has been interpreted to mean undirected efforts in the university and directed efforts in the industrial sector. Through the introduction of engineering in the university (dating from around 1790), the nature of university research broadened to include the application of scientific concepts to practical problems. Thus, engineering school faculties and other application-oriented academic groups initiated research applied to practical problems, and the industrial sector continued two important functions: support of the technical entrepreneur or inventor and developmental research.

The early days of industrial research were dominated by the investigation of individual entrepreneurs who would present ideas to companies in hope that industry would produce the device and all would profit. This compares with early day university research, in which individuals pursued areas of interest with little thought given to the commercial aspect. These processes, although effective and low cost, were generally slow and hesitant. As the university recognized the value of research in fulfillment of the objective to create new knowledge, more emphasis and reward were given to fundamental research. This, in turn, led to a commitment by universities to conduct research in an organized way. The same was to be true for industry after the Industrial Revolution. By the beginning of the 20th century, industrial research activities became organized and an integral part of the industrial complex. Universities expanded their commitment and established research institutes. On the national scene, the National Academy of Sciences was established. By the end of World War I, organized science was a force both in industry and in the university.

In spite of the introduction of the concept of applied research activity, the preponderance of university research was devoted to idea formulation and concept development or basic research with little activity in the applied arena outside of the agricultural work supported at land grant institutions. The experience of World War II pointed to the need to couple theory with application, to bridge the gap between ideas and concepts, and to use these in physical devices. The influx of large numbers of students to engineering programs following World War II provided the opportunity for expansion of faculties in engineering. Many of the faculty additions possessed backgrounds in the physical and mathematical sciences, and it was hoped they would serve to bridge the gap between pure and applied programs.

The distinction between basic and applied research was confused even more as university departments in the pure sciences developed applied arms and sought applied research programs and industries started basic science laboratories. Thus, from a historical perspective, the clarity and distinction between basic and applied research that existed prior to the Industrial Revolution have become diffused and confused.

To summarize, originally, research was the domain of universities and technology application the domain of industry. As universities prepared individuals for industrial careers, the drive for applied research began and a clear distinction between basic and applied research no longer existed. The growth of engineering faculties in the post-World War II period featured the addition of persons prepared in the sciences and mathematics. Curricula changes emphasized the fundamentals of physical sciences and mathematics. The growth of government research and development funding further confused the simplistic notion of the assignment of pure and applied activities between universities and industry. Industrial research and development groups experienced growth paths similar to those of the university and duplicated facilities and environment.

Today, no clear distinction exists that provides for industry to do applied research and forego undirected activities and for the university to remain committed solely to the expansion of knowledge. Both do fundamental and applied research. The balance may tilt to applied activities for industry and pure research in universities; however, no clear line of demarcation exists. (What should occur is recognition of high-quality work regardless of the origin, university or industry.)

CURRENT PERSPECTIVE

No doubt should remain as to the requirement for research at all levels, from pure to applied. As we examine basic versus applied research, a useful purpose is served to connect the two and place them in an appropriate perspective.

Basic research--Activity in the pursuit of knowledge for the sake of knowledge; undirected, except by the individual who performs the activity; activity that does not have as a primary goal a product or products that would be useful for mankind; activity supported truly for altruistic purposes and by philanthropic sources.

Applied research--Activity in pursuit of knowledge for a specific and defined purpose; directed, usually by the group that provides support; activity that has as a primary goal a product or products that are useful for mankind; activity supported by mission-oriented groups.

As may be seen, these definitions lead to separation of the two types of research activities. If this were always the case, clear decisions could be made as to the desire to support basic or applied research without fear of overlap. Since this is the case, it is the overlap zone that gives rise constantly to the concept of basic versus applied research. The overlap area is a highly competitive zone and generally leads to the poor relations apparent between the two camps. Both kinds of research activities are required and each must be supported in some way.

In the current environment basic and applied research are entwined in all aspects of the economy and policy (3). There is direct involvement in economic growth, technological growth, military security, policy objectives, and foreign interactions. There is a current perspective from the public point of view and from government of high expectations from research to resolve broad societal problems. This expectation is much the same as that from an earlier decade (the 1960s), which focused on education to achieve similar results.

Science and technology are viewed as responsible for the particular mess found as individuals, as a nation, or as a world. Advancements from research applied to technology are deemed responsible for the energy problem, the materials problem, the information problem, and on and on. The same public also sees salvation as coming from the same enormous pool of talent, if only it were directed toward proper ends.

The basic issues seem to revolve around the perceptions just noted. In a recent conference held by the New York Academy of Sciences (4) the basic factors that influence the public view of the government role in research are discussed.

1. The ever-higher levels of technical sophistication of military capabilities and the truly dramatic acts of space exploration, all through government support and control of research and development and its transfer to use, lead naturally and inevitably to the question of why similar support and control should not be used to solve the more mundane problems of our economy.
2. There is an uneasy sense that economic and social problems are growing in quantity and quality beyond our traditional capabilities for solution, and perhaps even a sense of frustration with the apparent inadequacies of market-oriented democracies to present credible and strong positions regarding current domestic and international difficulties. Thus, the possibility that governmental use of science and technology can address these problems has a great public attraction as a modest effort, without obvious dislocation in our economic or political structures, in situations where other approaches have little promise or require massive diversion of resources.
3. There appears to be an interesting influence, which applies uniquely to the United States, on the opinions of both the public and private sectors that arise from science indicators. These indices are measures that presumably show United States technical dominance relative to that of other industrial countries declining in recent years in such terms as percentage of gross national product devoted to research and development, percentage of the work force devoted to science and technology, number of patents granted, and so on. To the extent that these indices correlate with economic growth and foreign trade, there is then a basis for increasing technical effort on the part of the United States. Since the indices are statistical and apply to the overall macrostructure of U.S. science and technology, the federal government is under pressure to provide the major effort to reverse these trends, for the government has the institutions and the capabilities for providing funds and incentives.

The research environment today is one surrounded by conflict, as shown by the following:

1. A finite reservoir of technical talent to conduct a seemingly infinite appetite for research,
2. Finite resources to fund the pool of finite technical talent to perform the infinite work required,
3. Conflict between research required to satisfy regulatory rules and new product development or basic research, and
4. Investments in research compete with all other investments and time to bring ideas to the point of reduction to practice is a formidable enemy.

In addition to these, a number of other factors (5) within the research community tend to make research a tenuous business (e.g., What do we expect as outcomes from basic and applied research programs? What are the current expectations of most funding groups? What benefits may be attributed to organized research? and, conversely, What benefits may be attributed to entrepreneurs and innovators?).

Historically, basic research is to be carried out by universities with the outcomes being the advancement of knowledge and the production of advanced-degreed individuals available for employment in gainful occupations in areas related to their academic training. Applied research is to be the province of industry with outcomes in the nature of products useful to mankind. These roles are confused and industry is performing basic research and universities are performing applied research, in addition to their traditional roles.

Does it mean that industry has found universities unable to satisfy the need for basic information? Have universities not found adequate funds to meet needs solely within basic research support sources? Do those who fund research, especially the federal government, find inadequate justification for basic research to garner sufficient funds to carry on programs and, because of this, have they channeled more and more funds to directed research?

CONCLUSIONS

The conclusions to be drawn from this review of the nature of basic versus applied research are several. The role of research in society has been examined and it has been determined that research of all types has had a profound impact on mankind. Evolutionary, indeed revolutionary, outcomes of the application of new knowledge have been seen. Humans have moved from the cave through the various ages of man to the present last vestiges of the industrial revolution. There is an apparent conflict caused by new ideas and the innovator, the entrepreneur, and the researcher have experienced severe difficulties throughout the ages. Given the competitive nature of mankind, the proclivity to gravitate toward competing power bases, and the willing use of information and tools of all kinds to build power, it is no surprise that basic versus applied research is the current perception.

In conclusion:

1. Basic research and applied research are essential for the continuing generation and use of knowledge for the common good;
2. Basic research deserves a special place wherein individuals (or groups of individuals) may pursue concepts, ideas, and information for the sheer joy of doing;
3. Basic research deserves support without control;
4. Applied research should be carried on both in close proximity to the basic research activity and in close proximity to usefulness;
5. Applied research deserves support with direction, control, and measurement of outcomes; and
6. Basic research and applied research are not mutually exclusive and should coexist in a noncompetitive support environment.

Finally, so long as support is perceived to be inadequate, fierce competition will occur. The objectives are clear. Increase basic research funds, while increasing applied research funds, and support basic and applied research rather than basic versus applied research.

REFERENCES

1. M. Boretsky. Technology Transfer Becomes a Trendy Item. Science and Government Rept., Vol. 9, No. 13. Washington, DC, Aug. 15, 1979.
2. Encyclopedia Britannica. Encyclopedia Britan-

nica, Inc., Chicago, Vol. 19, 1970, pp. 195-200.
3. J.D. Palmer. A National Systems Center for the U.S.A. In 1978 Proc. of the International Conference on Cybernetics and Society, Vol. 1, Nov. 1978.
4. Science and Technology Policy: Perspectives for the 1980's. Annals of the New York Academy of Sciences, Vol. 334, New York, 1979.
5. L.H. Young. To Revive Research and Development. Ideas and Trends, Business Week, Sept. 17, 1979.

Defining Operational Problems

JOHN F. NIXON

This paper discusses various considerations that must go into the identification and definition of an agency's operational problems and the determination of whether or not the problem is researchable. The perspective is that of a state transportation agency concerned with materials, construction, and engineering problems. The paper discusses the crucial difference between identification and definition of a problem. Important elements that should go into a good problem statement are identified, and a discussion is made of the importance of defining a problem in the context of the agency's overall objectives and mission. Finally, important points to be considered in determining whether or not a problem can or should be researched are presented. These include the type and amount of data needed, the timeliness of the project, and the availability of adequate personnel, funds, and resources to undertake the study.

Definition of a problem forces the engineer to set limits to its scope and enables him or her to form the problem into a statement of need that supplies specific questions. These questions form the basis for the objectives of a research project. In Texas, problem identification and definition are contained in a problem statement.

The problem statement should clearly define the problem and specify the scope and objectives of the research. In preparation of the problem statement, the engineer discusses the intended function of the process or material to be studied, identifies the problem, and defines it in terms of actual versus desired performance. The difference between the actual versus the desired results defines the magnitude of the problem and offers objectives that can be quantified.

The problem statement is submitted to a specific research area committee for consideration. There are four area committees, each of which is concerned with a specific area of endeavor (e.g., area 2 handles research into materials, construction, and maintenance; area 4's focus is on structures). The area committee plays an important role not only in prioritizing the recommended program but also in analyzing the problem statement in terms of its relevance in light of contemporary departmental policies, specifications, needs, and objectives. The members of each of the area committees represent the field and division offices of the department and are familiar with all aspects of their specific areas.

PROBLEM DEFINITION

During the consideration process of a specific problem statement, the committee members look at such items as the following:

1. Is the problem properly defined in the context of the desired objectives of the process or material? That is, Does the statement discuss the problem in terms of what performance standards are ultimately expected of the process or material in question? For example, a material for pavement patching should be expected to perform up to a certain standard relative to the pavement itself and other factors. A good problem statement would not only point out that an acceptable patching material is not available but would also indicate what would be an acceptable patching material.

2. Is the problem too wide or too narrow in its scope? The setting of a study's parameters, or its scope, is critical--too narrow a scope may limit the researcher's arena and produce results that are applicable to only specific cases rather than to the general problem. The results may be rendered meaningless when put into real-life situations when more variables may come into play. For example, a patching material applicable to only certain climatic conditions or to specific types of pavements may not be the most effective when used in a general maintenance scheme. A project to develop a more universal material, or several materials for specific climates and pavement types, would be of more benefit. Too wide a scope usually dooms the study to fragmented pursuit of answers to indefinite and generalized problems.

A study objective that begins with "to study, to analyze, or to investigate" is usually symptomatic of a scope set too wide to be meaningful to the researcher. The use of specific terms such as "to develop" or "to measure" assists in adequately limiting the study's scope.

3. Does the solution already exist? Obviously, this is one of the most important questions that can be asked when defining problems of any kind. A search of the literature, for Highway Planning and Research (HPR) studies at least, is mandated by the Federal Highway Administration (FHWA) prior to study approval. This policy is strictly adhered to in Texas because funds are too limited to waste on projects that have already been done elsewhere. Our literature search relies heavily on the Transportation Research Board's Transportation Research Information Service (TRIS). The adequate definition of the problem aids greatly in the computer search for relevant literature. This is a good test of how well a problem has been defined, because a problem in poor focus will be difficult to search in the computer; a similar difficulty will be realized by the researcher in planning a study.

4. Will the solving of the problem contribute to the department's efforts toward meeting its stated objectives and, ultimately, its mission? One may recognize in this last question some of the terminology of a program known as management by objectives (MBO). The department is currently involved in the establishment of an MBO program that will guide our activities all the way from top-level administration down to the flagman on the highway.