TRANSPORTATION RESEARCH RECORD 829

Research Programming and the Value of Research

TRANSPORTATION RESEARCH BOARD

COMMISSION ON SOCIOTECHNICAL SYSTEMS
NATIONAL RESEARCH COUNCIL

NATIONAL ACADEMY OF SCIENCES
WASHINGTON, D.C. 1981

Transportation Research Record 829
Price $4.20
Edited for TRB by Susan Singer-Bart

modes
all

subject area
11 administration

Library of Congress Cataloging in Publication Data
National Research Council (U.S.). Transportation Research Board. Meeting.
Research programming and the value of research.

(Transportation research record; 829)
Reports presented at the 60th annual meeting of the Transportation Research Board.
1. Highway research–United States–Addresses, essays, lectures. I. Title. II. Series
TE7.H5 no. 829 [TE192] 380.5s [625.7'072] 82-6360
ISBN 0-309-03306-3 ISSN 0361-1981 AACR2

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Long-Range Versus Short-Range Research Program Planning

CHARLES PINNELL

This paper discusses the difference between short-range research (1-5 year implementation period) and long-range research (10-20 years into the future) and points out the critical lack of long-range transportation research. Past eras of urban transportation are identified and discussed, including (a) "introduction of the automobile" era, (b) "getting out of the mud" era, (c) "freeway" era, and (d) "transportation system management" era. The trend to a "regulation" era due to a lack of long-range research and specific directions for our national transportation program is emphasized. The need to develop a new methodology for long-range transportation research is pointed out along with the need for the development of transportation futurists. A suggested framework for the needed methodology is presented and a plea is made for the critical national need to embark on an effective long-range research program in transportation.

The opportunity to discuss the subject of long-range versus short-range research presents a very interesting and challenging problem. Before getting down to specifics, I wish to first develop the framework within which this paper was developed and to present some basic definitions.

First, the background. The title of this session is "Research Program Planning to Maximize Effectiveness", and the paper is developed within this context. It further assumes that the research program that we are planning is a rather substantial program such as would be found at a state or national level. A state program might be one for the transportation department of the state, and a national program might, for example, be that of the U.S. Department of Transportation.

I suspect that the terms short-range and long-range research register a variety of meanings for anyone who encounters them. Thus, I will define these terms according to my concept of what they mean.

SHORT-RANGE RESEARCH

Short-range research is research that addresses a specific, well-defined problem and that can normally be completed in a one-to-five-year period. The results of short-range research can be evaluated [i.e., workable solutions to the problem were obtained (or not obtained)]. The results can usually be transferred into specific actions and implemented rather rapidly. An example of a short-range research project might be the elimination of fixed-object hazards such as large sign structures or illumination poles. This problem surfaced after a substantial amount of the Interstate system was opened to traffic. The problem could be defined very specifically as, for example, we need sign structures and illumination poles, but an unacceptable number of fatalities are occurring when vehicles hit these fixed objects. Research projects addressed this problem, and in two-three years the development of breakaway and attenuation devices provided a satisfactory solution. The solution was verified in the field and implemented all over the United States.

LONG-RANGE RESEARCH

Long-range research addresses a perceived, but not clearly defined, problem that is expected to exist 10-20 years into the future. It will be impossible to verify the desirability of the solution or recommendations that may emerge from the research. The results will impact on policy and strategy as opposed to specific, day-to-day activities. The research may be multimodal and certainly will be multidisciplinary. An example of this type of research would be a study of the transportation system for a major metropolitan area in the year 2000.

One's first reaction may be that long-range research, as previously defined, is simply long-range planning. I disagree and will attempt to show that much more complex and comprehensive study approaches are needed than are now being used in long-range planning.

I do not have specific facts and figures to quote, but my impression is that we are conducting only a miniscule amount of long-range research in transportation at the present time. However, we have reached a point in time when it has become imperative that we implement a rather large and effective long-range research program.

PERSONAL TRANSPORTATION ERAS

In order to justify this critical need, let me briefly focus on the status of personal transportation as I see the situation. I feel this will make an effective case for the research needs I have emphasized.

The personal transportation area has evolved through a number of eras. These could be identified as follows:

1. Early 1900s--"introduction of the automobile" era;
2. 1920-1940s--"getting out of the mud" era;
3. 1940s-1960s--"freeway" era, development of the Interstate system and urban freeway networks; and
4. 1960 to present--"transportation system management (TSM)" era.

The private automobile came into use in the early 1900s, and its popularity grew very rapidly. During the period 1920-1940, we were mainly concerned with the development of a minimum national system of streets and highways. The end of World War II ushered in the freeway era. This era brought the trend to suburban living, the two- and three-car family, and the rapid upward acceleration of the vehicle ownership curve. A 45 000-mile Interstate system was constructed, and almost all our cities built record mileages of streets and freeways.

The freeway era continued into the late 1960s, when a significant change began to occur. Inflation caused road construction costs to double and triple, and the national concern for growing environmental problems combined with these increased costs to reduce construction programs to almost a standstill. We thus entered the TSM era, where we have concentrated efforts toward making the best use of the existing facilities.

The TSM approach has merit. We have been wasteful and somewhat narrow-minded in our views, and it is well to concentrate on the means to make more effective use of existing facilities. This approach requires one to consider the impact of economics, urban forms, human behavior, and other factors. The transportation professional was forced to broaden his or her skills and introduce interdisciplinary efforts into problem solving.

One must recognize, however, that the TSM approach is not an end in itself but only a holding action. We must look for long-range solutions to our transportation problems. We have all seen artistic renditions of the city of the future. These renditions are typically characterized by an absence of automobiles as we now know them. The movement of people depicted is usually by rapid transit, sky cars, and other exotic people-mover systems. The question that must concern all transportation professionals is, How do we get from the present to the future relative to personal transportation?

We need to embark on a new era in personal transportation, but the basic problem is that we simply are unable to determine what this new era should be. Never have we faced such a variable future that presents so many critical but unanswerable questions, such as the following:

1. What is the future of energy relative to transportation?
2. What is the future of the private automobile?
3. Should we develop rail rapid transit systems in our major urban areas?
4. Should we decentralize our population?
5. Could we decentralize our population?
6. What is the future of railroad passenger transportation? and
7. Could we fund and implement major new national public works projects in transportation similar to the Interstate program?

Because of the lack of answers to these and other critical questions, we cannot determine a clear direction for our national transportation efforts. In the absence of this direction, we are, in effect, entering a new era in transportation that I will label the "regulation era". In most of our large urban areas, no substantial changes are being made in transportation facilities, even though urban growth and vehicle travel increase continually at a rapid rate. Serious levels of congestion already exist, and this congestion grows daily.

When one has a scarce commodity and a great demand, the natural response (and the only response when no steps are being taken to increase supply or reduce demand) is to ration the scarce commodity. Thus, we may be forced to use traffic regulation to ration scarce transportation facilities in the face of tremendous demands. These restrictions could take many forms, ranging from existing parking restrictions and freeway controls to the total banning of automobiles at certain time periods, in certain areas, or on certain facilities. Such regulations could become very confusing and frustrating to the American public.

What is the answer to this dilemma? Obviously, no one has a ready answer, which points to the need for long-range research to help find the answer. There is a need for a new breed of transportation researcher, one I will term a transportation futurist for the lack of a better term at this time. The transportation futurist will be concerned with conducting long-range research.

The transportation research field has already used most of the basic tools of long-range research. These are information technology, systems analysis, operations research, forecasting, and scenario writing. The main problem lies in the translation of the results of long-range studies into specific policies and actions.

Edward Cornish, president of the World Future Society, in a presentation to the First International Future Research Conference held in Oslo in 1967 (1), outlined the framework of a methodology that could be used by the transportation futurist. Cornish points out that one cannot study the future because one cannot study what does not exist but rather, one must study futuribles (or alternative futures), which are statements of what may come to pass in an unknown future.

A basic methodology for long-range research was outlined by Cornish as follows:

1. Generation of futuribles--Asking and answering the question, What might happen in the future? This task could range from fantasy to conservative projections of past trends.
2. Assessment of futuribles--Once a futurible has been generated, it can be studied. A critical step would be to estimate the probability that a given futurible would happen. The probability of the futurible occuring might range from 1 to 99 percent.
3. Evaluation of futuribles--Once pertinent futuribles are defined that have a reasonable probability of occurring, then one can ask the question, How will this futurible affect us if it occurs?

As an example, consider a long-range study to evaluate personal transportation in the year 2000. One futurible that could be generated would be one that envisions a technological breakthrough on a new source of power for the small automobile. Assume that a 75 percent probability of this happening was estimated. Then one could evaluate a future urban scenario (with a high probability of occurrence) that provides for continued availability of the private automobile.

SUMMARY AND CONCLUSIONS

The process of generating, assessing, and evaluating futuribles is but a part of the overall long-range research needed in transportation. We must also be able to take the results of the process and translate them into specific policies and goals. Without this last step, we are no better off than we are at the present. Thus, basic considerations of the social, economic, and political environment must also be taken into account.

A significant parallel exists between the present energy situation and urban transportation. It was possible in the late 1950s or early 1960s for professionals in the energy field to project and evaluate a futurible that would closely match our present situation. We can, in fact, find papers and reports from the past that rather clearly illuminate the present energy situation.

The sad fact, however, is that the energy professionals did not have the capability to influence public policy and cause national goals to be set that could have made major impacts on avoiding the crisis in 1979. From the urban transportation viewpoint, we have the same problem, opportunity, and challenge. We must be able to go beyond just long-range planning and forecasting. We must develop a methodology and a means for implementing our research findings. The long-range research must be done in such a manner that it can impact public policy and overcome the social, political, and economical forces that are retarding progress so effectively today.

In summary, let me define some specific steps that I think those of us in the field of transportation research must recognize and support. These are as follows:

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.

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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.

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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.

The department believes that the establishment of a specified mission, and goals and objectives at all levels, will lend a framework for more effective planning of our work and allocation of our resources. The implementation of this system will obviously be of benefit to the research program. Research on materials and methods that is not easily implemented or assimilated into existing specifications or operations, for whatever reason, is counterproductive toward the department's mission, and time and resources are wasted. For example, the new technique for pothole patching discussed earlier may require (a) more people in the road crew to implement than is feasible, (b) a strategy for handling materials that is so foreign to standard practices as to be disruptive rather than beneficial to the maintenance effort, or (c) materials that are difficult to obtain or handle, that are too expensive or dangerous to use, or that cause excessive pollution to the surrounding area.

As unlikely as it sounds, the department's maintenance scheme may not call for pothole patching to the extent that implementation of the research results is cost effective. A serious pothole problem may, because of unusual subgrade, traffic, or moisture conditions, be better treated by excavation of the deteriorated area of pavement and relaying of the pavement. Again, the implementation of the results of another study may require the collection and analysis of vast amounts of data, which may be difficult, expensive, impractical, or impossible.

APPLICATION OF THE SCIENTIFIC METHOD

It is obvious, then, that the definition of a problem must include a consideration of the implementation of its solution within the framework of departmental policy. This leads to the second topic of this paper: Is the problem researchable? That is, Can a program of study that uses the principles of the scientific method be applied to the solution of the problem?

In order to determine whether the scientific method can be applied one must first ask what data are needed, and are they measurable? A properly constructed problem statement should be a good guide to the nature and extent of the needed data. The definition of the problem will suggest the data to be taken; the scope of the problem will indicate how much data and the precision needed. Whether or not the data can be taken, for whatever reason, is critical to whether or not the problem can be researched. Since measurable data are the basis of all scientific endeavors, the lack of it, because it is inaccessible to instruments, too voluminous, or too costly, will kill a project before it even begins.

Given that the data can be measured with the desired precision, and in the needed amounts, the next consideration is, Can the researcher formulate a hypothesis based on the measured data that can be verified by experiment? There are few areas where data are available in which the subsequent steps of the scientific method cannot be applied. However, they do exist, especially if the data are subjective in nature or its interpretation is subjective. A good example of this is an opinion survey that, if properly conducted, yields good data on public sentiment. These data hardly lend themselves to problem solving in the realm of engineering. The researchability of a problem, then, relies heavily on the availability of objective data and the ability to formulate the data into a sound hypothesis that will lend itself to experiment.

OTHER IMPORTANT ISSUES

Other issues that are important in determining a problem's researchability are timeliness, people, and resources.

The timeliness of a research project may not determine ultimately whether the project will be attempted, but it is an important issue that must be addressed prior to the initiation of any project. Leonardo da Vinci's helicopter was a good idea that was eventually pursued several hundred years later. The market for a helicopter in da Vinci's time, however, was not good. The timeliness of a problem, then, is concerned not with the merits of the research or its possible resultant material or technology but rather with the availability of contemporary mechanisms to implement the results.

People, naturally, are an issue in the researchability of a project. Of primary importance is how the people who will use the results of a study view the problem. Do they agree that a problem even exists? Ideally, these are the people who initiate, or who contribute to, the formulation of the original problem statement. However, if the problem statement does not originate at the problem source, then there is a question as to the existence of the problem. Often problem statements submitted by research scientists without benefit of field experience or coordination with field engineers may turn out to be irrelevant to real needs. On the other hand, a complex problem submitted from the field may never be studied if trained research scientists are not available. For a problem to be researchable, people knowledgeable at both ends of the engineering spectrum, from field engineers to research engineers, are needed.

Finally, the ability to solve a problem may hinge on the availability of adequate resources, usually money but quite often people, as discussed before, as well as on technology, raw products, and facilities. In Texas, our research program exists because these resources have been available. Committed funds were available because of federal regulations that allow for research and planning 1.5 percent of federal-aid highway dollars. Personnel are available because we have, in recent years, become more competitive in pay with the private sector, which enables us to attract and hold competent and experienced technical and engineering personnel. Research scientists are available from the University of Texas at Austin and Texas A&M University because of far-sighted legislative action that decreed that the University of Texas' Center for Transportation Research and Texas A&M's Texas Transportation Institute (TTI) would be the official research arms of the department. Facilities are available at the above institutions that make possible a wide spectrum of research that would be difficult, if not impossible, to perform within the department. TTI's huge annex outside of Bryan, Texas, makes possible full-scale testing of road-side safety devices and lighting strategies under realistic conditions, and the University of Texas annex contains facilities for scaled-down and full-scale testing of structures.

The research program in Texas is organized and run on a structured, business-like basis: a research section was formed within the department to administer the program and to maintain a liaison with the research agencies and other departmental entities; a research and development committee, composed of the chief engineers of each engineering division, exists to formulate and guide each year's activities. Communication with the needs of the people in the field is open by means of the research area committees, which identify, define, and recommend problems for investigation to the research and development committee.

The defining of a problem, by means of a problem statement, involves

1. Identification of the problem,
2. Discussion of the problem in the context of desired objectives of the process or material, and
3. Setting of the scope of the problem, which must not be too wide or too narrow.

The problem statement is analyzed in terms of

1. Adequate definition of the problem,
2. Adequate setting of the scope,
3. Availability of information that may solve the problem without further study,
4. Contribution of the solution to the furthering of the department's objectives and mission, and
5. Implementation potential of the desired solution.

Two final questions must be answered, Are trained personnel and technology available for the research and implementation of the problem and its solution? and, Are adequate resources available for support of the research, such as funding, personnel, facilities, and organization and support?

ACKNOWLEDGMENT

This work was performed in cooperation with the Federal Highway Administration.

Prioritization of Researchable Problems and Planning Future Program

CHARLES F. SCHEFFEY

Research and development is, by nature, a future-oriented activity, with the result that it requires careful planning and a commitment of resources beyond the current year's budget if major problems are to be solved. It is also primarily a service function to the operating side of the organization it serves and must be responsive to the requirement if it is to receive support. It should, however, also provide for a continuous review of technical policy in the light of new technology, be a base for future improvements, and conduct special investigations. Both operating personnel and research personnel must participate in the development of the program because the latter may miss important current problems, and the former may fail to exploit technological opportunities. An extensive network of problem solicitation exists in the Federal Highway Administration to ensure participation of all its elements and to obtain input from the states. In addition, important informal channels exist through the committee structures of national organizations. In the final analysis, budget construction involves a careful balance of this wide range of views as to what is important. The Federally Coordinated Program seeks to reflect this consensus and to concentrate resources on the most urgent task. The primary task of management, once objectives have been set, is to ensure provision of adequate resources and effective coordination of the work.

I have interpreted the invitation to present a paper on the topic of the problems of constructing a responsive research program to be an opportunity not only to discuss how we do it in the Federal Highway Administration (FHWA) at the present time but also to indicate what we are doing to improve the process. Since there are other papers in the Record that are intended to present the point of view of state research managers, I will concentrate on the federal perspective although there are many aspects of this problem that are common to any organization.

Before one discusses the process by which research programs are constructed it is necessary to understand some basic characteristics of the research and development process itself. I made a rather discouraging analysis of research activities in both the FHWA contract program and a sampling of federal-aid research studies conducted by the states about five years ago to determine the actual time span between the first budget request to obtain resources for pursuit of an identified problem until a usable solution was at least partly deployed in the operating system. Except for some rather short-range fire-fighting-type studies, such as those aimed at determining the cause of stripping of a particular asphaltic concrete mix, the average time for major problems was about six years. Even the more successful research efforts require about four years for the sequence of steps, which includes conception, budgeting, development of work plans, analysis and experimentation, report preparation, implementation efforts, and policy changes required to obtain operational deployment. Research and development is, therefore, by its nature a future-oriented activity. The implication is that careful long-range planning is necessary in order to obtain the lead times that are required to permit adequate examination of alternatives and the development of effective solutions. The four-year minimum time frame has a familiar ring for those of us in the federal establishment. It implies that one of the tasks of a research manager is to persuade top management people to think in terms of objectives beyond their own term of office.

Research and development can never be pursued with a 100 percent probability of success. It is essential that research managers be willing to take risks; in effect, they must be willing to bet on the ability of their organization and their people to resolve difficult problems and not play it safe by undertaking only those studies for which the methodology is completely clear at the outset. We must examine the achievements of our programs in terms of the degree of success measured against the difficulty of the problems being attacked. The research manager must be prepared to convince the administration that the risks of investing in a program are smaller than the risks that will continue to occur in the operating system in the absence of solutions to the problems that are under study.

With both conviction and some trepidation, two additional characteristics of research and development are suggested. Research has the potential to be disruptive and contains the seeds of self-deception. When it produces significant results, research can be disruptive because it usually requires major changes in the way in which we do business. Such changes, even in an enterprise where there is an atmosphere of acceptance of the need for change, are nonetheless certain to produce tensions and opposition. A major policy change requires, first of all, the admission that the existing policy is inadequate. Second, it requires managers to develop new procedures or criteria for controlling their operations; and third, it may require extensive, and sometimes painful, retreading of the technical specialists responsible for implementing the output. The self-deception nature of research is a subtle process by which individuals involved in research sometimes narrow their vision to a single line of approach to solve a problem. This can result in both a stagnation of the research process itself and also a tendency to self-preservation of a line of data collection and experimentation that has already exhausted its ability to provide further refinement of the solution.

These rather negative statements have been injected at the beginning of this paper in order to make clear that the designing of an effective research program is not immune to the hazards of human behavior that occur in any other branch of human activity.

ROLE OF RESEARCH AND DEVELOPMENT

It is also necessary before discussing program building to be clear about the role that is expected of the research and development organization. The functional statements of most research organizations that I have inspected seem to assume that everybody knows what research is supposed to do and it is therefore enough to say that they will conduct and manage research and proceed to fill the rest of the statement with a definition of the technical scope of the intended program. One can hardly decide whether a research program does or does not carry out the essential role of research and development unless one first designs that role.

Research and development is, first and foremost, a service to the operating and policy arms of the agency of which it is a part. It is responsible for solving the problems that the organization is currently facing and it fails that organization if it does not perform this primary function. However, it must also identify future problems in time to initiate appropriate activity to produce solutions before these problems become critical. This implies that a certain fraction of the resources available must be applied to obtain a better understanding of how the system for which the organization is responsible functions. In most organizations, research and development must assume the additional role of a continuing inspection of current technical policies in order that these policies will not become obsolete and will exploit the best technology available. The research organization may also be given responsibility for certain urgent special studies in case of emergencies.

Research fails the enterprise it serves if it does not make a concerted, knowledgeable, and patient effort to translate its findings into understanding, acceptance, and practical application. A mental review of the above roles for the research program should make it clear that neither research personnel nor operating personnel alone can develop an effective total research program. Research personnel may not be sufficiently aware of current critical operating problems and operating people may not be sensitive to the opportunities for exploitation of the latest technology.

PROGRAM BUILDING AND PRIORITY SETTING AS PRACTICED IN FHWA

FHWA reorganized its research and development activities in 1970 and launched the present Federally Coordinated Program (FCP) in 1971. FCP is a framework for coordinating the efforts of the state highway agencies that are financed in part with federal funds from the contract program of FHWA, National Cooperative Highway Research Program (NCHRP), and the FHWA staff program. Except for the broad categories of research designed at the initiation of the program, the specific context in terms of major projects is not rigid but shifts with time as new problems become urgent and old problems are either solved or abandoned for one reason or another. An article published recently in Public Roads indicated which of the original 50 projects have been completed with workable solutions, abandoned because they proved unachievable, or were combined with new projects. Only about one-third of the original projects are still active, and these deal with such long-term problems as the improvement of the resistance of highway structures to natural hazards, the continuing analysis of highway accidents, and the refinement of environmental assessment methodology. This implies that approximately 35 new projects in the program have been initiated as a result of the current procedures for new program direction within FHWA.

This process consists of several parts:

1. Input mechanisms for wide involvement of the highway community in problem identification,
2. Analysis and disposition of this input,
3. Budget construction and priorities, and
4. Continuing program management and review.

A considerable effort has been made to ensure the widest possible participation in the initial process of problem identification. These procedures are both formal and informal. On the formal side there are three. Under current arrangements, the Office of Highway Safety of FHWA conducts a biannual solicitation for the identification of problems in the safety area. This is intended to reach all FHWA field offices, state highway agencies, governors' safety representatives, and local government officials interested in traffic safety. Problem statements submitted in response to this solicitation are first screened by the Office of Highway Safety and ranked according to its view of their importance. They are then forwarded to the Offices of Research and Development for consideration. A second formal solicitation is conducted by the associate administrator for engineering and traffic operations and is also intended to reach all the field offices of FHWA and the state highway agencies. Its scope is quite broad and is intended to include everything except highway safety.

The third formal process results from the involvement of the staff of the Offices of Research and Development in the review of the research problem statements submitted under the NCHRP program. After initial screening and Highway Research Information Service (HRIS) search by the NCHRP staff, both the first- and second-stage NCHRP problem statements come to our staff for review and identification of current related studies in progress in the federal or federal-aid components of our programs. Although these are not construed as a formal request for incorporation into the FCP, they are considered by our staff as an expression of concern on the part of the states with respect to various technical problem areas.

The informal inputs to our program come to our staff from many sources. The professional staff of the Offices of Research and Development hold memberships in major national committees of many groups concerned with the highway program, including TRB, American Association of State Highway and Transportation Officials (AASHTO), American Society for Testing and Materials (ASTM), American Concrete Institute (ACI), American Society of Civil Engineers (ASCE), Institute of Traffic Engineers (ITE), American Psychological Association, Institute of Illumination Engineers, and many others. Most of these organizations have subcommittees that are almost continuously engaged in the identification of research needs. A second informal process occurs by the involvement of our staff in the review of the research studies being conducted by the individual states under FHWA's Highway Planning and Research Program (HPR), part 2. There is perhaps no stronger indication of how important a state believes a problem to be than its willingness to invest its own resources in its pursuit. There is also a continuous contact between our research and development staff and their professional counterparts in the various operating elements of the FHWA Washington office. Frequently memos from the directors of these offices to the Office of Research or to the Associate Administrator for Research and Development request assistance in solving specific problems. These informal contacts interact with the basic inventiveness of our staff expertise and frequently result in the internal generation of both problem identification and project proposals.

In addition, other components in the U.S. Department of Transportation, and especially the policy-level offices of the Secretary, do not hesitate to insist that we should work on certain problems that influence these policies. In the safety area we have an external watch dog called the National Transportation Safety Board that frequently urges FHWA to pursue certain investigations. Also, the National Highway Safety Advisory Committee reports advice and criticism to the Secretary of Transportation. I mention these to remind us all that a research organization has a complex constituency. We ignore the insights provided by these diverse elements at our peril, particularly if they have been given a formal role in the policymaking process.

The analysis and disposition of this varied input require considerable effort. The formal submissions in the process alone produce on the order of 300 problem statements per year, and every problem suggested deserves a formal reply. Our experience has been that these suggestions follow into three roughly equal groups. The first two groups consist of those problems for which research has already been completed or is in progress and those for which the need for research has already been recognized and incorporated into budget planning. We do not consider it a negative aspect of these procedures that such a large portion of the suggestions do not really create new problem recognition. The responses to these suggestions provide an opportunity to generate more awareness of the availability of useful completed research and the satisfaction on the part of the submitter that the need for research activity has been recognized and programmed. The third group provides the identification of previously unrecognized problems or increases awareness of the urgency or problems already recognized but not as yet programmed.

This whole process was not created because we lack ideas for worthwhile research programs. On the contrary, we have a considerable backlog of problems for which we lack adequate resources to initiate research. The purpose, rather, is to reevaluate priorities and to ensure that the available resources are committed to the most urgent problems for which the greatest possible benefit can be obtained with the least risk. Since the two FHWA-sponsored solicitations are conducted by the operating elements of FHWA and not by research and development, these problems come to us with indications as to ranking as to national significance from an operating point of view.

One of the essential concepts in the FCP is that of major projects that guarantee the continuity of funding over a period of years to ensure concentration of adequate resources to solve critical problems. Budgeting each year is therefore not an annual popularity contest but a process of continuous refinement of the previous year's judgment about what major areas are most important, what old problems have been adequately solved, and what new problems warrant the establishment of major projects.

The current list of active FCP projects, therefore, provides a framework but not a limit for the consideration of new problems at any level--whether they be large enough for an individual study, a task, or a major new project. If the problem statement is closely related to one of the existing projects, it is considered as a possible new study under an existing task. If it is large enough to require several studies, a new task may be considered. In some cases the problem warrants an entire new project, although this is more likely when a whole group of related problem statements comes in at the same time, as occurred when the present project 1Y was established to tackle the wide range of questions associated with providing greater traffic and worker safety in construction and maintenance zones.

The screening process thus results in four possible outcomes for really new problem statements:

1. Recommendation for a study under an existing FCP project task;
2. Creation of a new task under an existing project to incorporate one or more such problems;
3. Recommendation for creation of a new project, with preparation of documentation, which includes a cost/benefit analysis and a preliminary project plan and schedule; and
4. A finding that the problem is too low in priority to reach it with current levels of resources.

This leads in a natural way to the next step in the process, which is the actual budget formulation. By the nature of the structure of FCP as a coordinating framework, management in research must make an assessment of the degree to which states are likely to be willing to participate in certain research activities with their own or federal-aid (HPR part 2) resources. From a federal perspective, we believe it is our proper role to fund more strongly with federal contract money those activities that are urgent but are not likely to generate adequate activity in the state research program. This tends to make the contract component of the FCP more heavily oriented toward new technology development, policy issues, and longer-range research. This balance, however, is a continually changing picture, and frequently projects funded almost entirely from federal funds in the early stages generate substantial state participation or NCHRP program participation in later stages.

As of January 1981, our federal budget process follows the zero-based budgeting concept, which requires that we identify our priorities in substantial detail approximately 18 months before the fiscal year to which the budget pertains. Although the administration may provide some general guidance at the time of such initial budget submission, this is generally only with respect to overall funding levels that are likely to be supported. Similar long lead times and uncertainties no doubt exist in most agencies with respect to research budgeting. It is therefore quite important that the budget arrangement and proposal be sufficiently flexible so that, when the inevitable cuts are made, there is still some chance of preserving a reasonable balance between completing continuing commitments, a vigorous attack on the most urgent current operational problems, and the building of a solid base for future technology. When top management permits, such flexibility is obtained by setting the budget line-item level of detail such that it includes several currently related major projects. This provides at least some limited latitude for research program managers when budget adjustments become necessary.

I am not prepared to provide a neat and detailed formula and set of criteria by which priorities can be established in the budgeting process. To reduce this critical function to a computerized automatic process is, in my opinion, both impractical and undesirable. This is not to say that the logic patterns involved in such well-developed recent disciplines as decision analysis cannot be profitably applied to the problem. Budgeting for research and development, however, is more than an ordinary process for making decisions in the face of uncertainties. It is also a process of subjective judgment as to the likely success of selected approaches and the fine art of relating to a diverse constituency that must use the eventual outputs. Exercise of these subjective judgments involves not only an intimate knowledge of the existing program and capabilities of our own staff and the research community that may become involved through contract resources but also an awareness of the levels of sophistication in the outputs that can be implemented. These are the judgments that most administrators expect their research managers to exercise. It is perhaps a philosophical point, but an important one, that, as long as the administrator holds the research manager responsible for the productivity of the program, primary weight should be given to the recommendation of research managers as to the best use of the available resources. The administrator will of course demand that these managers communicate with the operating elements. He or she will also seek the advice and counsel of these operating elements as to the adequacy of the proposed program.

This brings us to the fourth step in the process, namely, the necessity for a continuing program management and review. The Offices of Research and Development have established two arrangements by which FCP projects are subjected to external review. Approximately one-third to one-half of the projects receive such reviews each year at the annual FCP conference. The review parties in this case include the research investigators involved. This includes selected individuals who represent state, local, and federal operating elements who are the intended customers for the outputs. For the past three or four years we have also asked TRB to organize ad hoc task groups to make independent reviews of these projects.

In addition to these arrangements, there is a system of internal reviews conducted in cooperation with the operating offices of the FHWA Washington headquarters, such as the Office of Highway Safety, the Office of Engineering, and the Office of Environmental Policy. There is an effort to keep these projects responsive to operating needs, but I would be less than honest if I claimed that we always did exactly everything that our customers advised. In some cases I find it appropriate to back the position of my project managers and to resist suggested changes. We view this as part of our proper function as managers who must stand up and be counted when it is time to deliver results.

PENDING IMPROVEMENTS IN FHWA's RESEARCH BUDGETING AND PRIORITY DECISION PROCESS

The present arrangements within FHWA have been described in sufficient detail to indicate that they are comprehensive and systematic. No claim has been made, however, that they are completely effective nor do we claim that they cannot be improved. As indicated, we are convinced that the voices of both research and development and operating personnel should be heard when the top executive makes final decisions on the allocation of research resources. The research manager who bears primary responsibility for the success of the program should initiate the budget proposal and must have sufficient latitude in management to make necessary adjustments as budget changes occur. The chief executive, however, must know the position of the intended customer elements before he or she can make a proper decision with regard to the research managers' recommendations. We have recently made proposals within FHWA to ensure that the top management officials of the operating elements of FHWA and appropriate field representation will provide this review and appraisal to the administrator. It is not yet certain that this arrangement will be adopted but we are hopeful that in the near future some type of formal research requirements review board will be established.

Research managers must be keenly aware of their position as stewards of a public trust with respect to the resources placed under their direction. There is no one pattern of research priority decisions or budgeting that will ensure success. The research manager must work with the chief executive to ensure that a process appropriate to the current status of research in the organization will be established. While it may differ considerably from the arrangements that have been discussed and are now in use in the FHWA, it should probably contain at least parallel elements; namely, (a) a process to identify current operating problems, (b) an orderly procedure for the analysis of these suggestions, (c) an arrangement to ensure that the allocation of resources addresses the critical operating problems as well as long-range problems and the exploitation of new technology, and (d) an arrangement for continuous management review. It is important to establish a process that will ensure that appropriate elements of the organization will participate.

Setting State Research Priorities

J.W. LYON, JR.

This paper presents a generalization of the interaction of research management and the priority-setting process. Several concepts of research management and their effects on problem prioritization are evaluated. Several purposes for establishing priorities are discussed, along with the order of priority of basic research and direct problem research. The role of research and the research unit within the agency and the organization for setting priorities are discussed. Two corollaries are presented concerning the degree of formalization for setting the priority of basic research and contract research. Priority-assessing steps are given that augment a general research-management cycle. Future planning of research programs is discussed and a comparison made with planning long-range research.

State transportation departments all have different research units to assist in solving problems. Presumably, these research groups have defined missions and a working chain of command. But do these departments and research units do an adequate job of relating their problems to the needs of, and impact on, the transportation department? In other words, Do they adequately set the priority of their needs? Are these problems properly evaluated with reference to available resources? Are needs and resources adequately combined into a program whose priorities are properly established?

The missions and methods of the research organizations are as varied as the number of state transportation and highway agencies. Research management methods vary from highly formal to casual. Certainly, no single research management plan will work for all state research agencies. Our problems and resources dictate individuality in research programs. Even with this acknowledged individuality, research must provide an acceptable product. The measure of success or effectiveness of this product is the recognition of the degree to which research meets its commitments and requirements within constraints of budget and personnel resources. This, then, is the purpose of setting the priority of research.

MANAGEMENT AND THE PRIORITY ORDER

Principal attention for research management has been focused on such activities as problem definition, development of the research work plan, proper accomplishment of the research, and implementation. A typical research-management cycle is shown in Figure 1.

The setting of priorities is not a prominent facet of research management within the framework of the management cycle shown. Does this imply a lack of knowledge, ability, or interest in the priority order of research problems? Probably not. However, it may imply a lack of formality in setting the priority of research in the management cycle, certainly as compared with problem definition, study design, and implementation of the findings.

If, in fact, we in state research do establish a priority order, what are we trying to accomplish by this? Our objective is to define those studies that will address the greatest needs and offer the greatest returns for the state agency. To accomplish this objective our priority-setting effort has two basic thrusts. One is the establishment of a priority order of research problems to develop a program, then the setting up of the budget resources to accomplish this defined program. This activity might then be entitled "program development." The other thrust is to make optimum use of limited resources to get the greatest possible return from available funds and staff. Both of these directions acknowledge the identification of greatest possible returns to the state agency, but they differ in the way that resource availability is considered. One identifies needs and establishes resources to meet the needs; the other acknowledges resources and identifies needs that stay within those resources.

If we recognize the close interaction between identification of needs and resources, then those who have a responsibility for setting the priority of research should also have input or control capabilities of the agency resources available to the research program. Without these capabilities the research program will probably be a continuous effort of fitting needs into constrained resources. The identification of high-benefit or high-need problems should have management recognition of resource availability for these problems.

THE RESEARCH PROGRAM

The type of research program should be identified in order to better develop a priority-setting process. Basic research and long-range research lend themselves to the process of setting priorities for program development. However, direct problem research tends to be directed toward this process to a lesser degree. A direct-problem research program is generally priority ordered within available resource allocations. Problems defined as immediate needs cannot wait for a program to be developed. In order to start this type of study in a timely manner the general question is, "Can we do this within our immediately available resources?"

A weakness of this direct problem research type of priority ordering is that it may not have an ability to allocate resources reasonably. In the event that a problem develops that has a higher priority than others that are under way and available resources are committed, several options are available:

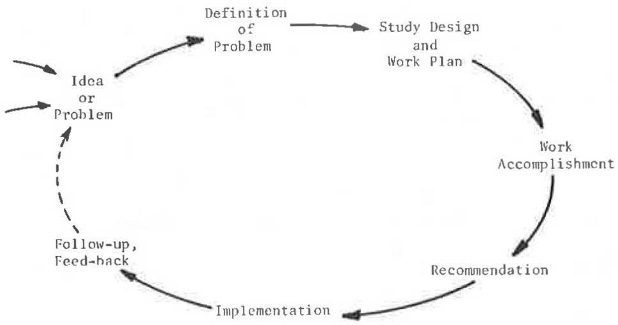
1. Additional resources are developed, which is generally a very difficult task;
2. On-going effort is curtailed so that higher-priority work may start; and
3. We wait, which is not looked on with favor, particularly by those who have the problem.

Yet, a continuous program of direct problem research has a built-in, renewable resource in that the completion of studies generates the availability of resources to undertake additional studies. There is a quicker turnaround of resources available for a continuous, direct problem research program that, in effect, minimizes delays of starting a high-priority study. Setting priorities for this type of program is generally an on-going review of problems and resources and tends to be done informally.

Research Role

The role of research within a state transportation department affects the attitudes and needs for establishing priorities. The organizational position of the research unit and its mission will strongly influence research management or be strongly influenced by management, and subsequently influence

Figure 1. Typical research-management cycle.



the priority order of research problems. In some cases, the research unit may have the primary mission of coordinating contract research, whether that contract research is directed toward basic research or direct problem research. In this case the probable role of the research unit would be to implement a program structured by a recognized group (council or committee) that had reviewed the state agency's problem statements and priority order of a program.

In other cases, the research unit may have the primary mission of conducting in-house research to meet the needs of the agency. Within this role, the research unit may

1. Informally set priorities itself with the advice and counsel of the chain of command of those operating units that have specific problems that might be in competition for research resources;
2. Have the chain of command set the priorities with that supervisor directly responsible for those several operating units that have competitive problems in making the priority decision;
3. Make recommendations to a high-level staff person who interacts with operating personnel and determines priorities; or
4. Make recommendations to a council or committee that makes priority decisions.

In all of these four cases, the probability is that the prioritizing process of an in-house research program will be less formal and more of a continuous activity than that process for contract research. However, if the state agency is principally concerned with long-range, in-house research, the probability of formalization is increased.

In summation, two general corollaries are presented:

1. Research prioritization is formalized to a degree directly proportional to the amount of basic research undertaken and
2. Research prioritization is formalized to a degree directly proportional to the amount of contract research undertaken.

If these two corollaries are correct, then a state agency that has a contract, basic research program would have a greater chance to be more formal in its priority-setting program than a state agency that has a contract, direct-problem-research program. Those state agencies that have an in-house, direct-problem-research program would have a strong tendency for informal setting of priorities. The tone would be low keyed and priority decisions probably would be made at a lower level than for the more formal programs.

Organizational Concepts for Setting Priorities

A state agency involved in research will have some recognized organization for research. This organization would generally consist of the research unit and an overview and control group. This group may be the chain of command for research, a separate council or committee, or a designated staff position. In any case, the responsibilities of this overview and control group should be clearly defined. It is very important that the agency give some clearly identified group the responsibility of openly receiving problems and needs from personnel of all levels of responsibility, the various disciplines and technical units, and even the geographical areas of the state. Each person must have confidence that the problem (which is very real to him or her, although sometimes not to someone else) will receive sincere consideration.

A research operation or group cannot, and should not, be expected to address all of the problems of an agency. Problems are handled continuously by capable technical personnel in the agency without the involvement or awareness of research. However, some mechanism must exist to allow the person who has a problem that he or she feels cannot get resolved, to bring this problem forth for research and consideration. If a state agency has a good mechanism for receiving and recognizing problems, then it probably has a good mechanism for identifying and setting the priority of the needs to address those problems, regardless of the methods used. Any weakness within an organization that will restrict the recognition of a problem will probably inherently restrict proper ordering of priorities. Problem recognition and priority decisions suffer from inabilities of the research management to identify the problem because of unfamiliarity or even a fixation of their own problems to the detriment of others.

Another major consideration for the agency's organization for setting the priority of research must be the recognition of all available resources that can be brought into play to solve the problems that are competing for these resources. A research-overview group can review the problems and stratify them into an order of benefit-returns and importance to the agency. This ordered array then would be a form of problem prioritization. But, there is a major abrogation of responsibility to then say to the group that is to get the research done, "Take this list of problems in priority order and go as far down it as you can." The complete priority-setting activity must be done with knowledge of resource availability and must include not only development of the list but also the recognition of how to implement the list, considering resources.

Organizational concepts for setting priorities must be directed to openly allowing and even aggressively pursuing the solicitation of problems not just the review of these problems and determination of a listing of benefits and needs. These concepts must incorporate a broad spectrum of knowledge of resources that are available, or can be developed, to solve these problems. A research overview group that has as a mission to set the priority order should be able to produce a program with confidence that they are aware of all of the problems and all of the resources.

Research Problem and Setting Priorities

The research management activities of a problem are shown in Figure 1 and have been discussed to some extent previously. This process is important because the priority ordering of research starts at the inception of the problem and is influenced by all steps that precede the actual priority decision point.

Obviously, the priority-setting process requires evalution of at least several problems. A single problem can be incorporated into a previously developed list. This single problem can only be evaluated comparatively if it is in competition for resources with other considerations. An expanded listing of research management activities that expressly leads to priority decisions is listed below.

1. Identify the problems,
2. Define the problems,
3. Define the agency needs concerning the problems,
4. Determine the scope of the problems,
5. Define resources needed to solve each problem,
6. Evaluate the accumulative resource needs for the various problems,
7. Set the priority of the problems under consideration, and
8. Develop the study design and work plan.

The process of setting priorities begins with the identification of a problem. The effectiveness of the solution of that problem depends on the thoroughness of each of the steps taken to develop that problem into a research study. The priority-setting process is also obviously affected by these steps. If the problem is not properly identified, its priority will not be properly ordered. This may be a built-in safety factor for a program in that, if a problem is not understood, it is not going to receive major support or a high priority. That problem will be undertaken only if there are surplus resources available and the review-priority-approval authority in effect says, "We do not understand it, but we will do it anyway." This situation probably does not occur very often.

Definition of the agency needs concerning the problem is a direct priority-oriented step. It assists research program managers in asessing the value of the problem solution, which sometimes results in a simple yes or no decision. This type of decision is the most fundamental priority decision possible--either do the work or do not do the work. If the decision is to do the work, then a stratified array in combination with (or competition with) other problems will be necessary. This stratified arrangement is certainly made more meaningful by defining the agency needs and the benefits of the study.

Determination of the scope of the problem is an obvious need in the research-problem-management process. Definition of the resources needed to accomplish the objectives is also a management need that is principally directed toward setting the priority of the problem, in addition to being able to properly evaluate the problem. This management process should require the evaluation of the accumulative needs of the various problems of interest to the agency. This composite resource-needs evaluation gives the agency a general idea of how far it can go to solve all of the problems. This evaluation is considered to be an obvious need of the priority-setting process.

Long-Range Planning

A number of state agencies have research programs that consist of direct, problem-oriented studies. These states identify this type of program as best addressing their needs and fitting their resources. However, it is easy to get so involved in this type of program that the only reaction and thoughts are for now. This outlook easily approaches the "cannot see the forest for the trees" syndrome, where the individual trees (separate, immediate needs and problems) distract from being able to identify the forest (related problems or a family of problems that are not solved efficiently by a series of separate, disjointed studies). Each agency involved in a program that principally consists of direct problem research should carefully analyze that program and ensure that a problem-review and priority-setting process is built into its research management system that will not only allow, but will force, long-range planning.

Long-range planning is not necessarily the planning of long-range studies but is more directed toward establishment of an overview of direction that can act as checkpoints or benchmarks for a number of seemingly disjointed, direct research studies. This is not to suggest that all direct, problem-oriented studies must fit within some overall plan of research. This obviously is not the case. But, without such a planning program, a state's research course could be plotted by a number of short sights without looking into the distance for a major landmark. Such a course tends to be erratic, have a number of unnecessary changes in direction, and is wasteful of time and effort.

The establishment of long-range goals is an inherent part of an overall priority-setting program. Such a program should also encompass a long-range review and planning for the development of resources to be available for research. An occasional review of the forest should be made to keep a program on course.

SUMMARY

There is no single priority-setting method that the various state transportation agencies can use in their research-management program. Each research program is unique and must be evaluated as such in developing a method for setting priorities.

The order of priority is a direct responsibility of research management, and both the needs for research and the availability of resources for research must be considered. Management responsible for the prioritization of a research program should also have authority over the resources necessary to accomplish the program.

The purpose and style of the state agency research program have principal effects on the priority-setting system used by that agency. Two corollaries are presented in this regard:

1. The priority order of research is formalized to a degree directly proportional to the amount of basic research undertaken and
2. The priority order of research is formalized to a degree directly proportional to the amount of contract research undertaken.

There is a strong interaction between identifying the problem and priority-ordering of the problem. A good priority-setting operation begins with a good problem-identification operation. Also, research management must openly allow or aggressively pursue problem recognition in order to best develop a research program of which priority decisions are a part.

In conclusion, Is the setting of priorities of research by the various states a fact or a fantasy? It is certainly thought to be a fact. The research programs of a large number of states are direct, problem oriented, and have little need for highly formalized priority setting. As such, the use of a highly formalized research priority-setting procedure by all states is a fantasy. Too often, the setting of priorities is mentally defined as a highly formalized procedure, which should not be the definition applied to state research programs.

Value of Transportation Research: Federal Perspective

G. D. LOVE

The value of transportation research may often be quantified in terms of direct savings associated with the construction, maintenance, and operating costs for transportation facilities. Such an approach can serve as a useful device in determining the cost-effectiveness of the research effort and as a means of supporting a feasible level of research funding. However, value must also be considered from the perspective of the basic objectives of specific research projects and the scale of the problems being addressed. As the magnitude of the problem increases, a more substantial research effort is justified, even though the probability of solving the problem successfully may be relatively low. Although there frequently are significant transportation research developments to which high values may be attached, research must be viewed as a systematic cumulative procedure wherein individual studies contribute to the final objective as the nature and the multiple facets of the transportation problem become more precisely defined and understood.

In an assessment of the value of investments in research activities, recognize that the research objectives of an agency may dictate differing measurements of value. For example, if the primary mission of an agency is of a regulatory nature, a different value yardstick should be used than that associated with much of the research in the private sector, where the development of marketable products is usually given top priority.

In the broadest sense, transportation research is conducted by the U.S. Department of Transportation for the purpose of bringing about a desirable change in the nation's transportation system. This may be viewed in terms of a desirable change of a near-term nature and, at the other end of the spectrum, as a possible long-term change that, at the extreme, could conceivably involve the deployment of an entirely new surface transportation concept. Because of the basic differences in research targeted at near-term problems as compared with longer-range research, the value yardstick must also reflect these differences. It seems appropriate, therefore, to consider the value question separately, as it relates to the short-term problem-solving research effort and research of a more fundamental nature that has a more futuristic perspective.

By far the largest slice of the federal highway research and development program is of an applied nature allocated to specifically identified problem areas. These problems are either nationwide in scope or at least applicable to a significant segment of highway mileage included as a part of the federal-aid highway system.

Recognize at the outset that any research and development conducted under the auspices of the public sector, whether at the local, state, or federal level, should only be undertaken after determination has been made that it is either not desirable or not feasible for the private sector to undertake the specific research and development being considered. We have been fortunate in the highway industry that the private sector has historically played an active lead role in the development of new highway products and construction and maintenance equipment whenever the projected market can stimulate the needed level of investment to support the research and development activity. However, in those instances where the market is either very limited or no marketable product exists, where there is a vested interest to be served by maintaining the status quo, where the research may support regulatory action, or where the required level of funding exceeds the financial resources of the private sector, the case for involvement of the public sector is supportable.

In addition to determining whether a research and development effort is more appropriate for public in lieu of private-sector financing, it is also essential to evaluate objectively the appropriate public-sector level to undertake the research and development activity. The types of problems addressed by the highway research program vary from those of a local nature, such as the determination of the durability characteristics of a specific local-aggregate source to problems of national scope. It goes without saying that, ideally, the highway agency for the local level of government should be the logical choice to perform the necessary evaluation to determine the suitability of the locally available aggregate since the research results are primarily of only local value. By the application of this same logic, a strong case can also be made that highway research that addresses problems that are statewide in scope be conducted by state highway agencies and problems of national significance be addressed at the federal level. This type of structure obviously enhances the actual adoption and timely implementation of the research output and the resulting societal benefits.

In considering the societal values that accrue from an applied research program targeted at specifically identified problem areas, the most simplistic value assessment is to record direct savings in construction, maintenance, and operating costs attributable to the implementation of a research output. This is a straightforward approach that can serve as a useful device in determining the cost-effectiveness of the research effort. It may also serve as an effective means of quantifying the benefits of research for the purpose of supporting a feasible level of research funding. However, as we move into areas where the research results may be targeted at protecting or enhancing environmental features, for example, it becomes more difficult to quantify the value of the research output. Even though the value yardstick cannot be calibrated in terms of cost savings, the research results may, nevertheless, contribute significantly to the overall benefit and well-being of society as a whole. We must, therefore, recognize that other measures of value must be considered and, in many instances, there are no commonly accepted standards for measuring worth or value.

This is particularly true as we move from the area of applied research, which generally equates to research targeted at short-term problem solving, to longer-term research, where more emphasis is placed on basic research. Basic research is generally undertaken to gain fuller knowledge or understanding of the fundamental aspects of phenomena and of observable facts, without specific applications toward processes or products in mind. Therefore, the probability of developing basic research results that have a high short-term payoff is far less than that associated with most applied research undertakings. Because of a general tendency to at least attempt to identify an estimated near-term savings for all types of research, whether applied or basic, it is extremely difficult to defend requests for basic research in the budget arena. Earmarked funding included in most public agency budgets for basic research is either nonexistent or at best minimal, even though the scientific community recognizes that basic research should be undertaken because of potential long-term societal values. Nevertheless, a decision to fund basic research is extremely difficult to defend, particularly under restricted budget conditions.

Closely related to the value concept of research is the risk factor. In our desire to quantify value, we often overlook that any research undertaking has a certain degree of risk. As well-known scientist A.B. Thomas, president of the Batelle Memorial Institute, once said, "If success is certain, there is no point in the experiment. Success often means the end of thought; failure may represent a fair beginning." If the problem being addressed by the research effort has a significant societal or economic impact, a more substantial research investment is justified, even though the probability of successfully solving the problem is relatively low. This is well illustrated by the large investment in cancer research, where a significant part of the total research effort is of a high-risk nature but, because of the potentially large benefit to society, the base of financial support is broad. In a similar vein, the potential impact of the world's dwindling petroleum supply on our nation's transportation system is beginning to be recognized by policymakers in both the public and private sectors and has resulted in substantial increases in energy-related research activities.

In summation, research is generally recognized as a valuable asset to federally funded programs administered by the U.S. Department of Transportation. However, since an applied research program can be more-effectively equated to near-term benefits, it is much easier to defend in the budget review and approval process. The result is that long-term research of a more basic nature generally receives only limited support and is usually the first item to be eliminated from an agency's research and development program.

Value is a many-faceted quantity that must be considered from the perspective of the basic objectives of individual research projects and the scale of the problem being addressed in terms of economic and societal impacts. As the magnitude of the problem increases, there is generally sufficient support for a larger research effort, even though the probability of success may be somewhat limited. Although the value of research is irrefutably accepted as a fact of life in the scientific community, this is not the case in the decision-making arena, where limited financial resources must be allocated on a priority basis. To ensure continued financial support for transportation-related research, we must continue to recognize the importance of quantifying its potential value, even though it may not be feasible to do so in absolute terms.

Value of Research to the Researcher, Economy, and Society as Viewed at the Academic Level

HAROLD L. MICHAEL

The great value of research to the individual and to society as viewed by academia is presented as obvious. Further support is provided by factual data on the benefits that result to the Indiana State Highway Commission from 40 years of cooperative research with the Joint Highway Research Project at Purdue University. One of the most significant benefits noted is the production of the leaders and educators of the future from a university research program. The significant contributions of the Indiana experience in this area are reported and the challenge made that the opportunity, really the responsibility, for similar research-education programs exists for all transportation agencies and universities.

The basic question posed by the assigned topic is one that should never have to be asked, Is there value in research? Of course there is value, tremendous value. Research has given us almost everything we have. As an example, consider the next physical items you touch or use and think about how each of them came to be. If all people did that each time they used something, we would no longer take so much of what we enjoy for granted; we would know the value of research. There is great value in research. If there were not, we would not be spending $51 billion/year on research and development in this country.

Another question about research that has been voiced often also bothers me. That question concerns the risk in research and especially that basic research has a higher risk. The fact is that there is no risk in research. Any research done competently produces new knowledge, even if it is not what was desired. That, too, is new knowledge, and any knowledge has value. Since the real purpose of research is to produce new knowledge, research will always be successful. Risk only occurs if from a particular research project one wants a specific result and nothing else.

In the early years of this nation, Thomas Jefferson noted, "An enlightened citizenry is the only safe repository of control over the ultimate processes of society." Americans then, and for many years after, looked to science as the way to progress and strength. In the recent two decades or so, however, our fellow citizens have become skeptical of science, as they have of many other things. They appear to have forgotten that science and technology play an increasing role throughout our society. In business, in government, in the military, and in the professions, science is clearly an important key to success. The computer has revolutionized activity everywhere. Modern communications govern much of what we do. Travel and transportation find clearly that their futures, although heavily affected by resources available and environmental concerns, lie primarily with what we can do through science and technology.

Economists estimate that advances in knowledge have accounted for perhaps three-quarters of the economic growth of this country. But the economic impact is probably not as important as the impact on society, although I find it difficult to separate the two. Albert Einstein once said, "Concern for man himself and his fate must always form the chief interest of all technical endeavors--in order that the creation of our minds shall be a blessing and not a curse." He knew that the pursuit of scientific truth was a good in itself but that the uses of those truths may be either good or bad, depending on the moral and political choices that determine those uses. This is where an enlightened citizenry must exercise its control--not in developing barriers to seeking knowledge but in determining uses of that knowledge.

The major functions of a university are three in number:

1. To distribute knowledge through educational means,
2. To create knowledge through research, and
3. To store knowledge.

All are important and, certainly, the creation of new knowledge is an important one. We in academia do not do research just to get promoted or for the sake of more publications. Educators do research because that is their function, because development of new knowledge is fundamental to development as a good distributor (teacher) of knowledge, because creation of new knowledge is more likely to occur by someone who has a thorough understanding of existing knowledge in related areas and where intellectual freedom for the pursuit of scientific truth exists. Our founding fathers established the United States as a place where scientific endeavor would be encouraged and honored. The wisdom of that decision is not questioned, for as a nation we have prospered.

As President Carter said in a 1980 address to the National Academy of Sciences, "We still look to our scientists and to our engineers, our military researchers and to our doctors, to our inventors, and to our thinkers, to improve our lives and to improve the lives of our children." President John F. Kennedy noted years earlier to another meeting of the Academy, "Progress in technology depends on progress in theory; the most abstract investigations can lead to the most concrete results; the vitality of a scientific community springs from its passion to answer science's most fundamental questions."

I have said that the value of research is viewed at the academic level. The proof, of course, is if such high-sounding expectations have been met over the years through research in the universities. Let me now summarize briefly some major benefits that resulted from the research financed over the last 45 years by the Indiana State Highway Commission through the Joint Highway Research Project at Purdue University, the organization with which I have been associated for 30 years.

During the 45 years, about 600 research studies have been completed. They were a mixture of basic and applied research. The results of these studies have been reported in many technical publications, especially those of the Transportation Research Board. The technical results of these studies have influenced highway development in Indiana, throughout the United States, and in many foreign countries. Our researchers work closely with personnel of the Highway Commission and develop new projects from problems that are encountered by them in the planning, design, construction, maintenance, and operation of the highway system. Our researchers use highway department personnel as advisors on the research as it progresses. They keep them informed of progress, of findings as they occur, of results as quickly as possible, and of the possible impact on highway activities.

The staff of the Joint Highway Research Project and I are confident that the Indiana State Highway Commission can document that the cost savings to the Commission from the research conducted over the years have been greater than the dollars expended. Perhaps the best argument that this is a true statement is that we operate on the basis of mutual respect for the value of research to the Indiana State Highway Commission. The university is provided funds for the research as a free grant each three months. This provides for the freedom of intellectual endeavor so necessary to pursue good research while at the same time using wise management by the university and the highway department of the research conducted--management based on performance, support, appreciation, and respect rather than on legalism and suspicion.

The direct benefits of the research have been sufficient to justify the continued investment of the highway department. But I am certain there have been and continue to be even greater benefits. Obviously, involvement in current activities of the highway department assists our faculty in maintaining technical competence and knowledge of current events. The opportunities of discourse with other experts expand the continued development of our faculty and permit them to be better distributors of their knowledge to students at all levels--undergraduate, graduate, and continuing. The program permits us to attract a highly competent faculty because the opportunities for teaching, research, and national participation are great. This also improves our capability to provide quality educational programs, enhances our reputation as a university, and expands the loyalty of our alumni.

Perhaps the greatest benefit of such a research activity is the number of educated people in transportation that such a program produces. The young men and women who will be the leaders in transportation are attracted to the profession, educated in the profession, and retained in the profession through transportation research in the universities.

Our highway research project at Purdue has graduated more than 500 such young men and women. They were educated at the graduate level in the best possible way, through involvement not only in academic courses but also in a transportation research project. Anyone who conducts such a project becomes an expert on completion in some small area of transport. He or she is probably as good an expert in their area of study as anyone anywhere. Such graduates are confident in their field. They are enthusiastic to continue work where they are the best. It is very likely they will continue their career in an area closely associated with the subject of that initial research. If they cap their formal education by attaining a doctorate, they are likely to teach and do research in transportation or go into the research and development field in transportation.

Of our more than 500 graduates over the last 45 years, about 150 obtained their doctorates. More than 100 of these are teaching transportation in universities throughout the world. Recently I attended a meeting of university professors involved with transportation. Thirty-nine such individuals from 31 universities were present. Ten of those individuals, now at 9 universities, were graduates of our program. All did research in highway transportation funded by the Indiana State Highway Commission through the Joint Highway Research Project. There is no better way to provide value to society, to the economy, and to the researcher than to be involved in the development of a nation's greatest resource, development of human minds. Research in the universities can be of great assistance in such development and in attracting the best men and women to the areas of the research. This development of the leaders and teachers in transportation of the future is the greatest value that can be provided.

The Indiana State Highway Commission, through its continuing practice of funding research at Purdue University, provides such benefits to the researcher, to the economy, and to society. The opportunity to do likewise exists for every state transportation agency and for all federal transport agencies. In fact, it is a responsibility.

Past and Future Value Systems in Research: An Industrial Perspective

C. F. COOK

The value of research to an industrial researcher is intrinsically related to the researcher's value system and the judgment of the free market system in this nation. The researcher's value system is shaped by his or her formative professional years and the market acceptance or rejection of his or her developments. In the United States, the free enterprise market system has historically provided the judgment of developments and selected only those that successfully benefit society and thus the economy and the researcher.

Although there are many value systems that can be considered, one of the keys is the value system held by the individual researcher. It is not always clear whether one is dealing with value systems or motivational systems when one relates to individual researchers.

If one can obtain just a little insight into the value and motivational systems of a researcher, the first step has been identified to enhance the creative and constructive environment so important in the technological community.

One measure of our nation's strength is the technological value of research done in our nation's three major professional segments--university, government, and industry. If our nation is to move to new thresholds, as measured by any set of values, then the three segments of our research society must extend the cooperative spirit that has brought America to this point in history.

The importance of cooperative spirit to the value of research cannot be overstressed. Other nations may cooperate by edict and may be controlled by monolithic governments, industrial cartels, or university extremists. Our nation cooperates through the free enterprise system. As long as free enterprise and free choice of research remain the cornerstones of our triangular research society, America will continue to provide technological improvements to better mankind and give each of us time in our lives to reach our personal goals.

HISTORICAL VIEW

To talk about the value of research, it is interesting to look at a perspective of mankind. If we look at the short-term history of the United States, we realize that the settlers of this nation worked from sunup to sundown to survive. It was hard, physical, and sometimes death-dealing work to just survive. The selection of what research to do and the value to these individuals of a research improvement are easy to measure in retrospect. In most cases the improvement grew out of urgent needs for a better plow, better ax, or better home and the strong desire to have some free time.

As the frontier of this country moved westward, needs appeared, solutions occurred, industries grew, cities evolved, and man became civilized. Research made contributions of significant value to the individual, to the economy, and to society.

As one traces the American history of industrial research, significant events that have occurred affected the lives of our forefathers and continue to affect our lives today. The most significant areas in this respect are transportation, electricity, communications, medicine, agriculture, and energy. An analysis of these six areas reveals one commonality: The end result of these developments benefited individual man so that life was better and he could reach out to his fellow man. This broad perspective leads one to conclude that the frank judgment of history on research value is intrinsically tied to societal benefits.

The research history of the United States has brought this nation to the threshold of a golden opportunity. This nation can provide the population of the world with significantly better living through the technological developments from our research community.

Whether this nation accepts the challenge or not will define what historians will record about the United States. We live in a different era. For the first time in recorded history a large fraction of mankind has time to think, plan, and spend more than half a lifetime on activities other than those required to survive. What is done in the next few decades will determine whether this nation is the beginning of attempts to reach new heights of civilization.

This time element may be the major underlying factor today that drives many industrial researchers. It is interesting to speculate how our forefathers would look at this phase of America in the stream of history.

The question before the individual industrial researcher encompasses all three subjects discussed in this symposium: What is the value to me, to the economy, and to society?

VALUE TO SOCIETY

The value of industrial research to society could be measured in charts and graphs by comparing gross national product to industrial research expenditures, basic to applied research, ratio of the number of engineers trained, or any number of other solid technical pieces of information. However significant these correlations may turn out to be, they would speak primarily to the technical community and, no doubt, be argumentative. It seems important to step back from the maze of detailed technical correlative data and look for a simpler value measurement.

In the last 60 years at least two significant and easily identifiable contributions to society have resulted from intensive cooperation between the three segments of our professional society. One is the survival of this world, as we know it today, through the research and development effort during World War II. During the perilous years 1942-1945, our nation agreed that the goal of survival was the centroid of all our lives. From those efforts many technical benefits to society resulted in the decades following World War II.

The second event was the national goal to put a man on the moon during the 1960s. From this event, the list of benefits to society is almost denumerable.

Some of us believe a third event is on the nation today--energy. It too is a survival issue. It will require marshalling our technological community to reach an energy solution. From the solution will emerge benefits to society that we cannot foresee, forecast, or enumerate. If energy problems are not resolved, society as we know it will not survive. Since the opportunity is here, and we have the time to think about it, the need, and the technology, then what is missing? There are many answers to this question. As our nation wrestles for the solution, we must remember the past, learn from our failures as well as our successes, and recognize what brought us to this point in history. The key advances in this nation were made in the free enterprise system.

The free enterprise system can provide the best solution. If the free enterprise system is allowed to function, the judgment of the marketplace will select the solution for our nation that will continue to provide the highest value for society through the millions of decisions made in the marketplace daily. This is the thrust that will produce a new societal threshold from which our nation can reach new heights.

VALUE TO THE ECONOMY

Webster provides this definition of economy: thrifty management, frugality in expenditures, an act or means of thrifty savings, management of the resources of a community, and prosperity or earnings of a place. The concept of economy here relates to thrift, frugality, management of affairs, and prosperity. In industrial research, most of the effort is directed toward providing improved products and services to our society at the lowest price and at minimum cost. We call that competition in the marketplace. If the research of company A produces more efficient and economical products and services than that of company B, then A wins and B loses. It is through our failures that research learns most and can regroup and reenter the market in a better competitive position. The competitive market is the only way to measure the economic benefits of research. Companies will come and go into the market with new products and services from research; the value to the economy will be determined by the marketplace.

VALUE TO THE RESEARCHER

The remaining portion of this paper deals with the value to the researcher. The value system of an individual is extremely complex.

Morris Massey of the University of Colorado observed that individual value systems have changed over the last 60 years. An individual's value system is shaped during his or her formative years. This can be seen by looking at the significant emotional events of the decade: in the 1920s--the end of World War I, the close family, and money talks; in the 1930s--the depression and need for security; in the 1940s--mobility prompted by World War II, the onset of family decay, and the drive to win and survive; in the 1950s--permissiveness, television, and affluence; in the 1960s--computerization, moon landings, civil rights, and Vietnam; in the 1970s--the jaded expectation of Watergate; and who knows what in the 1980s!

Massey further concludes that, with the rapid advances in technology today, value systems in the 1980s will probably change every 3-5 years instead of every 10 years as cited. The computer technological revolution of the 1980s will alter society as it is now known.

Among individual researchers, different value systems exist and change continually. The value system of industrial researchers may differ somewhat initially from those of their counterparts in government and universities. However, as time passes, these value systems coalesce to one that has been characterized in a variety of ways.

One basic trait of researchers is that a goal-oriented and accomplishment-seeking activity is a necessary segment of their life. The individual's definition of accomplishment is a key factor and varies from individual to individual. After a researcher has accomplished several technological successes, his or her value system will shift as he or she gains confidence. This maturing of the researcher leads to activities that will reward him or her in their new value system.

The complexity of the value system has been described by Maslow in his famous triangle that shows progression from survival motivation. As Maslow indicates, once needs for survival, food, and shelter are met, needs for social and accomplishment are fulfilled. When these are met, the challenges appear and must be conquered.

In the industrial technological society, the value system of the researcher must be fulfilled, otherwise the individual will seek employment elsewhere or retire from the technical community--in either case a serious loss.

Although any attempt to characterize the value system cannot be all encompassing, certain key factors must be present to satisfy personal goals and accomplishments for a researcher to dedicate his or her life to technological developments. Some of these are

1. Societal-related accomplishments,
2. Peer recognition,
3. Personal financial rewards,
4. Freedom to develop technological solutions,
5. Solutions tested in the marketplace, and
6. Technical society recognition.

Clearly, these factors would relate to most researchers regardless of which leg of the triangle they choose to dedicate their professional careers. As mentioned earlier, maturity brings the value systems closer. Indeed, near the career end point the systems probably become identical.

CONCLUSIONS

Since all research emanates from an individual's effort, the researcher's value system will ultimately determine the arena in which advances and contributions will be made. Value systems are shaped predominantly by the formative decades in one's professional life. In this nation, the free enterprise system has been a major segment of the researcher's value system. It is the penultimate arena in which acceptance or rejection results. Historically, the millions of marketplace decisions, uniquely characteristic of the free enterprise system, select only those developments that benefit the society, the economy, and, hence, the researcher.