

ment 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?

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Prioritization of Researchable Problems and Planning Future Program

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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.