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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?

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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,