Long-Range Versus Short-Range Research Program Planning

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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.

REFERENCES

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Basic Versus Applied Research: How to Maximize Effectiveness

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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