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The Scientific Approach To Research

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THE SCIENTIFIC APPROACH TO RESEARCH

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INTRODUCTION

The conduct of research, whether it be at the national, state, or local level, must follow a scientific approach. This approach should encompass the identification of research needs and associated levels of funding, formulation and conduct of the research experiment, and finally, marketing and implementation of research findings.

Federal, state, and local partnerships have recently been established to provide direction for implementation of these techniques. Even though the private sector is not under public sector mandates, there exists a degree of consistency between the public and private sectors of the transportation communities.

The TRB Conduct of Research Committee sponsored a session at the TRB Annual Meeting in January, 1994 to discuss the techniques used in conducting research at various levels within the public and private sectors. The presentations described the Federal guidelines and a state's interpretation of those guidelines as they apply to that agency's program. Other presentations related the application of the conduct of research practices by a major research institute and an independent research consultant. The three presentations by researchers at the state, institute, and consultant levels demonstrated a significant degree of commonality in their scientific approach to research. The local transportation assistance center presentation stressed the need to identify the customer or research user in order to provide an implementable product. The final presentation illustrated the need to consider the cost-effectiveness of the research program.

Because of the high level of interest in the presentations, the committee decided to publish this TRB Circular. This circular will not only provide valuable insight for those unable to attend the Annual Meeting but will also provide the basis for future considerations in promoting the "Scientific Approach to the Conduct of Research."

TRANSPORTATION RESEARCH NEEDS IDENTIFICATION

Khani Sahebjam and Robert J. Benke
Minnesota Department of Transportation

EXECUTIVE SUMMARY

Transportation research and the utilization and development of new technology is an essential cornerstone of effective transportation system management efforts. The Minnesota Department of Transportation has a tradition of commitment toward research and development of transportation technology. A vital aspect of the transportation research and experimentation process, as well as in the development of new technology, is the identification of research needs.

Mn/DOT has recently experimented with and adopted a transportation research needs-identification process that helped Mn/DOT develop approximately 80 top priority research projects for near-term start-up. The functional subject areas were Traffic, Environment, Bridge, Local Roads, Materials, Construction, Freight Movement, People Movement, Transportation & The Economy, and Transportation Finance.

The new process shifted Mn/DOT's direction of research program development from a reactive role conducting research projects that were "researcher-driven", to a more proactive role with the involvement of all elements of the transportation work force.

This process identified the immediate practical research needs in Minnesota using minimal resources and staff. For the purpose of identifying transportation research needs, the new process conducted by Mn/DOT proved to be efficient and effective.

INTRODUCTION

Transportation research, utilization of existing technology, and development of new and applied technology are essential for the improvement of transportation systems in Minnesota and the United States. The Minnesota Department of Transportation (Mn/DOT), with the cooperation of outside partners, is committed to research and development and application of new technologies in terms of resource allocation and support for new innovations. Mn/DOT is also committed to promoting internal risk taking, innovative thinking, education about the importance of research, strategic expansion of resource dedication, and formation of partnerships with the private sector to share resources such as people, facilities, funds, and information.

Mn/DOT's initiative in transportation research is a broad-based, multidisciplinary effort that encompasses a wide range of research programs. Mn/DOT has a strong track record in Materials and Pavement Research. Since 1986 Mn/DOT has been engaged in the planning, design and construction of the Minnesota Road Research Project (Mn/ROAD), a pavement technology research facility. Other major research related ventures at Mn/DOT include: MINNESOTA GUIDESTAR, Minnesota's Intelligent Vehicle Highway Systems partnership, and the Maintenance Operations Research Program that focuses on applied research and development of roadway maintenance activities.

RESEARCH MANAGEMENT

The Office of Research Administration (ORA) is under the direction of the Research Management Council (RMC) of Mn/DOT. In conjunction with the aforementioned research programs, ORA manages and coordinates a diverse program of transportation research and research implementation.

A challenging aspect of transportation research and development is the identification of transportation research needs. There are various means of developing a research program. Mn/DOT has recently shifted the direction of the program development and transportation needs identification process.

In the past Mn/DOT's research program focused primarily on materials and pavement issues. The program tended to be driven primarily by researchers and academia, who often expressed a special interest in research that did not always meet Mn/DOT's immediate need. More recently, Mn/DOT has developed a much broader program through increased resource allocation and commitment to research and experimentation in "non-traditional' subject areas.

A more proactive role was embraced by Mn/DOT through developing 1) A Research Services Section that concentrates on program development and contract administration; and 2) A Technology Transfer and New Technology Development Section with a primary focus on research implementation and technology transfer.

RESEARCH BRAINSTORMING PROCESS

In the Fall of 1992, ORA hosted 6 brainstorming sessions to develop research ideas. Each of the sessions represented a particular element of the transportation business: Local Roads, Materials, Traffic, Bridges, Environmental and Construction. Each session had from 30 to 60 participants representing a cross section of transportation service suppliers including: Mn/DOT, city, county, and federal staff, consultants, contractors, and other agencies. Key to the process was the involvement of staff from each area in the identification of the list of invitees.

The sessions were run by trained facilitators and ORA staff. After the opening general session and introductions, the group split up into subgroups of 5 to 8 people where the brainstorming actually occurred. After identifying and categorizing the topics, the groups prioritized the subjects into high, medium, and low priority groupings and reported their results back to a general session. There was a total of 1804 ideas developed from these 6 sessions.

Following the brainstorming sessions, ORA staff began the processing task. The ideas generated at each session were consolidated to eliminate any duplication of ideas. After the consolidation of the individual group ideas, they were merged to develop a master list of high, medium, and low priority topics. Once again, the ideas were consolidated and duplications were eliminated. The master list was then sorted by category, high priority, and origin of idea so that the research ideas could be evaluated by a team of about ten technical experts from each original group.

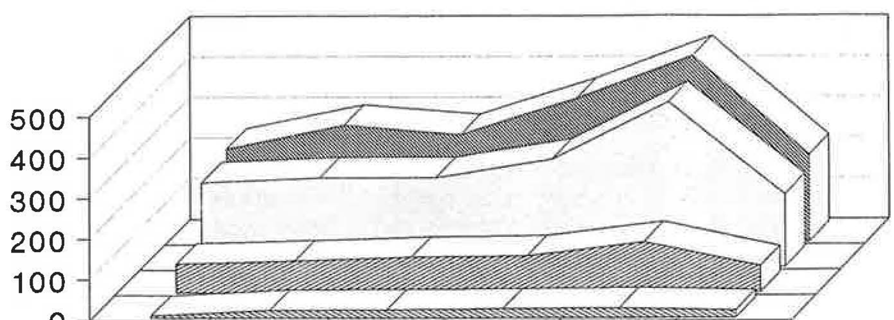
The experts were then asked to select their "top 20" from the resulting list of about 100. The results of this polling method were then provided to the experts for use in a half-day session where they debated the merits or shortcomings of the "top 20" and developed a "final top 20" list. They also identified a contact person for each idea. Figure 1 illustrates the reduction process.

A subjective evaluation of the selected high priority ideas ("Top 20") was performed by the technical experts. Each research idea was given a high or low rating based on its risk and ultimate payoff. Risk was defined as exposure to failure and probability of success in obtaining some form of a conclusion. Payoff was defined in terms of economics, safety and social benefit of the research project. Figure 2 illustrates the results. Approximately half of the ideas had the ideal rating of high payoff with low risk. Approximately a quarter of the high priority ideas were rated as high risk/high payoff. The remainder were judged to be less productive, i.e., low payoff and/or high risk without adequate payoff.

Another evaluation process that was conducted considered the long-term/short-term payback. Payback is defined as the time it takes from the commencement of the project to the implementation process (acceptance of the results). It is ideal to have a mixture of both short-term and long-term projects to achieve a balanced program. Approximately two-thirds of the high priority ideas should have a short term payback (short term is defined as less than five [5] years).

Upon completion of this process, a literature search using the TRIS data base was performed on the top priority ideas. The contact persons and research originators then reviewed the literature searches and recommended either further research or identified the idea as a technology transfer project indicating that

Research Idea Reduction



	LRRB	MTL	ENV	BDG	TRF	CST
All Ideas (1804)	229	288	264	353	457	213
Condensed (H,M,L)	209	221	221	265	406	179
High (condensed)	72	79	88	91	125	65
"TOP 20" (114)	6	20	21	23	25	19

"TOP 20" (114) — High (condensed)

Condensed (H,M,L) — All Ideas (1804)

Figure 1

RISK VS PAYOFF

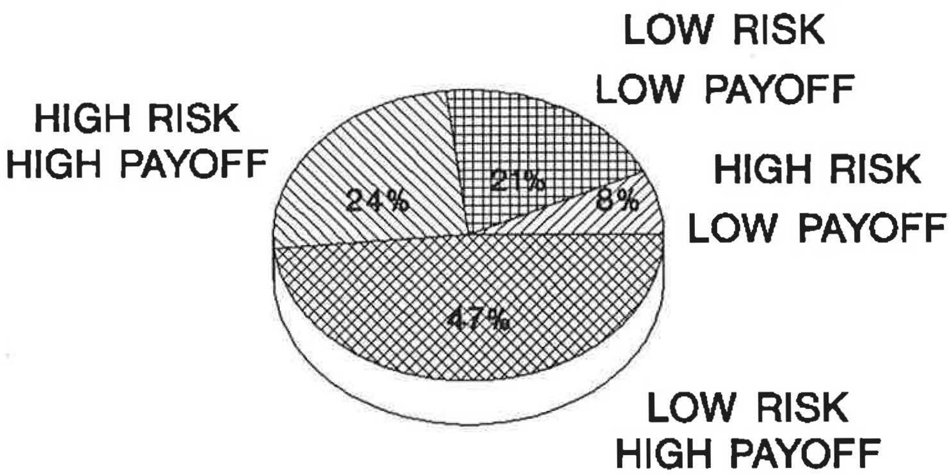


Figure 2

TABLE 1 BRAINSTORMING RESULTS

	LR	MAT"L	ENV	BDG	TRF	CNST	TOTAL
SELECTED HIGHS	7	20	21	23	25	10	106
CONTINUE RESEARCH	6	15	13	18	16	6	74
TECH. TRANS	1	5	8	5	9	4	32
POLICY HIGHS	27	7	21	25	13	17	110
# SUBGROUPS	5	7	4	5	8	4	33
IDEAS/SUBGRP	46	41	66	70	57	54	55
IDEAS/PERSON	6.0	5.2	8.5	7.7	7.6	6.5	6.9
HGH P/PERSON	2.0	1.8	3.4	2.3	2.4	2.4	2.3

LR = Local Road
MAT'L = Materials
ENV = Environmental
DG = Bridge
TRF = Traffic
CNST = Construction

sufficient research has been done or is on-going on that specific topic. (Table 1 shows these data). Of the 106 top priority projects, 74 were recommended for further research and 32 were recommended for the technology transfer process. Also in Table 1, the non-research (policy) ideas are shown. There were a total of approximately 110 high priority policy ideas that were forwarded to the appropriate agency(s) for their information and action. Other pertinent brainstorming session information shown in Table 1 is the number of subgroups per session, ideas per subgroup, ideas per person, and high priority ideas per participant. There were between 2-3 high priority ideas per person that had an influence on the design of the revised research needs-identification process.

PROCESS OF RESEARCH NEEDS IDENTIFICATION

After approximately 6 months of processing and filtering the 1804 research ideas developed using the original brainstorming process, the ORA revised the process for identifying Minnesota's transportation research needs. The reasons for this shift are as follow:

1. The original process resulted in a large and difficult-to-manage number of research ideas that required an enormous amount of effort to prioritize, consolidate, and define. It also resulted in many non-research (policy) ideas, which was not the immediate goal of the research needs-identification process.
2. The ideas in the final list were in the form of a single statement and, in some cases, were too general and vague. A better defined and expanded problem statement was later needed for the researcher to develop a research proposal and cost.

The revised focus group process was implemented for the Intermodal Programs brainstorming session. The following concepts were used in the new process:

1. Each selected participant was given the opportunity to think about, develop, and submit 2-3 ideas prior to the meeting.
2. The Intermodal Division subjects were divided into four functional groups: freight movement, people movement, transportation finance, and transportation and the economy. Each group reviewed and discussed their related research ideas and selected the top five ideas from each functional group creating approximately 20 intermodal research ideas.
3. The participants then developed the problem statements describing the specific goal of the research for those 20 ideas and suggested potential researchers and research project reviewers.

Upon completion of this process, both the original and the revised process were evaluated and the advantages and disadvantages were summarized as follows:

Advantages of new process:

1. Group discussion and consensus of each idea.
2. Participants were better prepared and more informed at the session.
3. Chance to build on ideas and develop more topics at the session.
4. Much less work for ORA staff due to the condensed process.
5. Elimination of non-research (Policy) ideas.
6. Identification of interested parties for review of the research project.
7. Identification of potential researchers.
8. Literature search is responsibility of researchers and not the ORA.

Disadvantages of new process:

1. Takes away from a true brainstorming process
2. Does not identify as many technology transfer research topics (research that has already been completed but users are unaware of the results).

While the brainstorming and focus group processes appear to be effective means of identifying critical research needs, Mn/DOT staff retain some discomfort in subject areas where they have minimal experience and expertise. The first specific example addressed was the subject of transportation, land use, and economic development interrelationships. To address this issue, Mn/DOT has contracted with the H.H. Humphrey Institute at the University of Minnesota for the preparation of a strategic plan for research in this topic. The plan will be developed following an assessment of current research, evolving public policy, and current funding scenarios (e.g. ISTEA). A panel of experts from public agencies, academia, and the private sector has been recruited to assist in this task. Hopefully, a report will be available by the '95 TRB meeting to share results of this additional means of identifying strategic research needs.

PROGRAMMING & FUNDING PROCESS FOLLOW-UP

A total of 82 Problem Statements (Similar to Stage-1 NCHRP) from the original brainstorming process and the revised needs-identification process were submitted to the Center for Transportation Studies (CTS) at the University of Minnesota (UM). The center distributed the problem statements to potential interested researchers who in-turn will develop a project proposal with a defined work plan and cost estimates. Mn/DOT's technical experts, contact persons, and research originators will then review and evaluate the proposals. ORA will then select the highest rated projects and match the available and appropriate funding to these projects for the following fiscal year.

Other universities and research consultants will be asked to respond to proposals in areas where the University of Minnesota faculty are not available. In addition, some of the research ideas have been included in the NCHRP process.

CONCLUSIONS/RECOMMENDATIONS

Based on our experience with the true brainstorming process and the focus group process used most recently, Mn/DOT will rely primarily on the revised focus group approach. Since each session is designed in partnership with key technical staff, we will be encouraging and supporting the use of "pre-focus group" brainstorming opportunities that provide for "grass roots' organizational involvement without the extensive reliance on ORA staff time. In addition, we will continue our use of general solicitations to catch ideas not included in the focus process as well as the consideration of needs identified by the Department's managers, the Legislature and other partners.

A research needs-identification process can assist states or other agencies to determine their top priority research needs. The process can be successful with minimal resource allocation and staff requirements. Repeating this process for each functional area every 2-3 years is recommended.

ACKNOWLEDGMENTS

This report was prepared in conjunction with the efforts of William Bunde of the Minnesota Department of Transportation, who assisted with the needs-identification process and development of statistical data.

CONDUCT OF RESEARCH PROCESS AT THE RESEARCH INSTITUTE LEVEL

G. Sadler Bridges
Texas Transportation Institute

Texas Transportation Institute (TTI) is the largest university-related transportation research program in the world by several measures — dollar volume, number of staff, and scope of research performed. Our current budget is $25 million. We have offices in Houston, Arlington, San Antonio, and Dallas in addition to our headquarters at College Station, Texas.

We have almost 500 people, including 105 graduate students but not the 110 or so undergraduate students who are employed in our research program. Among the approximately 200 professional staff are 60 who are also faculty members at Texas A&M University. Some 40 are faculty in Civil Engineering, the remaining 20 are in other engineering, and non-engineering departments. The engineers are from departments of Industrial, Electrical, and Chemical. Other faculty include Computer Science, Landscape Architecture, and Range Science, just to name a few.

TTI's largest single sponsor is the Texas Department of Transportation. Also, we were informed in a recent GAO audit that we were the largest single receptor of research from both the U.S. DOT and NCHRP. TTI personnel generally present about 50 papers at TRB, and about 100 persons attend including 35 graduate students from Texas A&M who work in our research program.

This background is not for bragging purposes, but to tell you that as an academic research program TTI is large and complex. Staff includes all kinds of people—prima donnas and hard workers; theorist and practitioners; visionaries and problem-solvers; engineers and non-engineers, to identify a few. Our engineers use poor statistical design (according to our statisticians), and do not know economics (according to our economists). Our materials engineers look on operations as voodoo art, and, of course, our operational engineers are convinced that entirely too many research dollars are splurged on materials and structural research. The engineers agree on only one thing—the economists and planners are totally out of touch with the real world.

What are the principles that should be followed in managing an academic related transportation research program? There are at least four factors that are important: (1) recognize the motivational factors of faculty, (2) employ good people, (3) communicate continuously, and (4) manage by supporting not by supervising.

First, one must realize what factors motivate a faculty member. Anyone who tries to manage research in an academic environment without fully understanding that motivation will be very frustrated. A close friend left the Federal government after a long career managing really large research programs to join a university, that was beginning a large transportation research program. He has since retired. He found out quickly that money is not the motivation for faculty. He has told me many times, "Sadler, I can't get these guys to work. I bring them the problems and the money — they don't want to work for me."

There are three measures that a faculty member is judged by within the academic community: Research, Teaching, and Public Service. However, most of them would say that the three measures are Research, Research, and Research. But research does not mean just any research; the research must lead to publication, and the publication must be in a peer-reviewed journal. Success in research is not in how valuable the results may be to transportation, to the sponsor, or even the traveling public or the monetary value of the contract. Success is measured by how valuable other learned people feel that knowledge has been advanced. Of course, nothing is more valued than a visionary faculty member who can identify opportunities for advances in knowledge in what may otherwise be viewed as a practical problem.

In the academic community, research that advances knowledge is valued — research that only leads to improved practice is not. The dollar volume of the research is of much lower priority. I heard our Dean of Engineering explain this by saying that 40 years ago or so engineering was completely an applied topic. Most engineering principles were in the form of handbooks, nomographs, etc. Today, he continued, universities are teaching engineering science. Scientific principles apply equally in engineering as in science. Today's engineer is expected to create a good part of the knowledge base that is used in his profession.

Research is also important to faculty if graduate students can use the topic as a basis of their thesis or dissertation. Graduate students are valuable as a resource, and the number of graduate students supervised is a part of the faculty's work load measures. Graduate students are also valuable to the research projects. It allows the use of some of the brightest young minds available at a low cost. Graduate students tend to work very hard because their degree is dependent on the research. Having a proven record of work makes for more marketable degree holders.

One thing that I have become increasingly aware of is giving young faculty both opportunity and encouragement to do those things that will be used to judge their effectiveness by fellow academics. Neither TTI nor our major sponsors, whether they be Texas DOT, U.S. DOT, or a transit authority, require that research results be published in academic journals, but it is important to the faculty. If a young faculty member does not accomplish an adequate number of peer-reviewed journal publications within a specific period of time (usually six years) tenure will be denied and then has only one more year to find a new position at another institution. Thus, the penalty for not providing an opportunity for a young faculty to write those articles is that at tenure time, the individual will not receive tenure and will be lost to the research program. Thus, faculty must do more than non-faculty researchers in that they must write journal articles in addition to maintaining the research quality required to meet the sponsors needs.

In most cases, research quality is not an issue when dealing with both faculty and graduate students. Unfortunately, far too many state DOTs have had the experience of funding a university to solve a practical problem only to find later that the funds had been expended on developing material for journal articles of little value to the state. Some others have had the experience of graduate students being given problems with little supervision and insufficient experience to develop practical solutions. Faculty, like all other researchers, need to keep the needs of sponsors in mind. The sponsor comes first, and it is an additional responsibility of the faculty to identify journal quality research out of what may be very practical research.

In this respect good faculty researchers are no different than good non-faculty researchers. Good ones are good and poor ones are not. Which leads me to the next factor employing good people. Beyond the basic objective of retaining people with the prerequisite research skills, we must recognize not everyone is suited to working in certain types of organizations. Not everyone is suited to working in an environment in which future support for their job is not known until contracting agencies made their annual funding decisions. These same people can work very successfully in a different environment. For example, I know a person who left the research field due to stress over future funding and is now the successful manager of computer facilities at another university.

I cannot over emphasize the importance of communication. In cases where the quality of research was not up to our standards, most often the researcher had felt isolated from support. I did not say the researcher was isolated, I said they felt they were isolated. It is so very easy for any research manager to get so involved with day-to-day activities that one fails to communicate enough with key personnel. Everyone in the organization must realize that they are not isolated, that support is available, that resources and personnel can be committed to helping with their problems. This is important for everyone, but it is particularly important for less-experienced researchers. Everyone must understand that the organization is a team. Research is a team sport; it is much more like football or basketball than tennis or boxing. If it is your job to block Lawrence Taylor you better know exactly what the rest of your team is doing and where you can get help. If you do not know your team will lose. It is exactly the same case in research.

There is a big difference between talking to people and communicating with them. Communication is two way. Communication means you not only hear but understand the other person. Management text books talk about MBWA or management by walking around. I submit that if one walks without also communicating that you do not know what MBWA means.

The fourth factor is to manage by supporting not by supervising. This is actually just another way of emphasizing communications. For an individual who has the innovation and originality to be a good researcher, the worst thing is to supervise. Researchers are good because they do not "stay within the lines." In a real sense a research manager works for the researcher not the other way around. It is management's job to find those tasks that researchers are not good at, do not want to perform, or are better doing other tasks. The research manager should provide for these tasks to be completed for the researcher.

If a researcher is a poor writer supply an editor. But do not force one—make sure it is the researcher's idea, not yours. For the researcher who is a poor manager, do not force them into managerial situations. If the researcher is also on tenure track, make sure that they do not take on so much research that they have no time to develop scholarly articles. If a piece of equipment is required, see that it is purchased. These are just a few examples of things a good research manager can do to manage by supporting not by supervising. Researchers do better when they do research and not other tasks.

The four factors discussed here are important to the conduct of research in a university environment. Again, the factors are: (1) recognize the motivational factors of faculty, (2) employ good people, (3) communicate continuously, and (4) manage by supporting not by supervising. These are obviously not the only things one has to watch for, however, I feel they are the most important.

CONSULTANT'S APPROACH TO THE RESEARCH METHODOLOGY

Barbara Thomas Harder

Generally, the decision of who conducts the research (in-house or outside, for-profit, not-for-profit) begins with quality researchers foremost, and then the decision is made based on a multitude of variables, including funding, timeframe for performance, facilities...and others items. Therefore, analyzing distinctions among the types of researchers is a bit of a red herring, but I do believe there are some similarities among all researchers. *Credible researchers seeking new knowledge or answers to problems will produce credible results.* The integrity of the researcher should not change with the type of organization for which the researcher works. Nor should the fact that the research is being performed for one's own organization or for an organization other than one's own change the quality of the research. Essentially, one's *best effort*, in whatever context one find's oneself, *is* the bottom line... Let me emphasize, BEST EFFORT. The quality of the conduct of research is the unchanging variable.

Researchers must follow the basic tenets of scientific inquiry, I will first review some of these major steps, and then I will discuss a few perspectives I have from a consultant's viewpoint.

A brief review of the major methodological steps of scientific inquiry are:

- Problem Statement Development
  - accompanied by an assessment of viability, risk, usefulness, and potential for implementation
- Literature Search
  - what has already been done on the topic
- Research Work Plan Development
  - to do this step one must consider where the following will fit into the research:
    - observation and description, cause and effects, analysis and synthesis, hypothesis and its testing,
    - deduction, models, fallacies, to name a few;
- Design of the Research/Experiment
  - a few items that must be addressed:
    - purpose for the experiment, variables, comparative versus absolute measures, samples, controls and standards, replication, bias of experiment, and more;
- Design of Apparatus (if required)
  - specifications, calibration, standards, impedances;
- Execute the Experiment
  - test facilities, controls, sampling, estimates, measurement, bias of researcher
- Analysis of Data
  - testing hypotheses, deductions, conclusions, and recommendations
- Report of the Results
- Implementation Preparedness
- Evaluation

This paper does not specifically address the design of the experiment, design of the apparatus, execution of the experiment, or analysis of the data, since there are many books written on these subjects. Several are *An Introduction to Scientific Research* by Wilson; *The Art of Science* by Carr; *Scientific Method: Optimizing Applied Research Decisions* by Ackoff, Gupta, and Minas; *Handbook for Scientific Research* by Beech. These can be found in the reference section of a engineering or science library. (NOTE The above outline was taken from Wilson only up to and not including implementation preparedness and evaluation, which I added).

Also, assessment/evaluation will be discussed in one of the other papers and not addressed here.

The three items I want to address directly, as related to applied research, are:

Developing the Problem Statement
Literature Searches
Implementation Preparedness

Developing the Problem Statement

There are five points to highlight in this area:

problem definition
assessment of problem viability and associated risks
usefulness of anticipated results
priority of producing a solution
potential for implementation

These are some of the major steps that I go through when determining if a problem should be researched. Generally the answers to these five points are determined cooperatively with the client, or those wanting the research performed.

One of the most critical items of any research project is to properly *define the problem and understand the context* of the problem to be solved. The importance of this step cannot be overestimated. It is the foundation of determining whether the research should be done. This is all quite obvious, yet in my experience, it is an area that all too often does not receive the appropriate amount of expertise applied to it. The lack of sufficient attention for definition may come from those who require the research to be done *as well as* (if different people) those who will be conducting the research.

Producing less-than-optimal problem statement definitions can happen in situations when research problems are "grass roots" generated, in other words, where those experiencing the problem are responsible to write the problem statement. These individuals are experts in their field, but generally are not research professionals, (and usually aren't economists, statisticians, or risk analysts either). A team approach to defining the problem would be more satisfactory. The expert having the problem needs to discuss the problem with other experts, researchers, and additional people in the organization to spread the vision for why the project is appropriate and to gain an understanding of the larger context in which the problem will be solved (and results used). Open interchange among this group must be done so all possess a good *understanding* of the problem and the associated impacts of performing or not performing the research.

Associated with the definition of the problem comes an assessment of the viability of the problem and the risks associated with it, initial determination of the usefulness of the results, determining the priority or importance for having a solution, plus a view into the means of implementing these results, if they are indeed as useful as is projected.

Problem Viability--is the problem workable, practical, and is research on it feasible? What risks (exposure) are associated with the research and what risks are there if the research is not performed? -- Are there consequences for not having a solution to the problem, and is there a time or funding factor involved? Answers to these questions need to be made in the light of best judgement at the time, from technical experts/researchers as well as those particularly familiar with systematic risk assessment. Assessments must not be superficial, bases for conclusions must be sound.

Usefulness of Anticipated Results--to what extent will the anticipated results improve the organization's operations or function? Will the anticipated results contribute to the strategic goals of the organization or of the broader industry environment?

Priority of Producing a Solution--How pervasive is the problem? If the problem is viable and the results can be used, yet the solution addresses a nominally important problem...reviews should be made regarding stewardship of resources. Today we are not particularly looking for innovations in the proverbial buggy whip. Additionally, are there political overtones in the priority?

Reviewing the *Potential for Implementation*--this is different from making preliminary assessments regarding the viability or usefulness of a research result. A new process, method, or product may indeed be useful, and it may also be practical, but can the solution be put into practice? Is there a vision for implementation, a sense of fitting the innovation into the way business is conducted at the present time; or a means to handle change as a result of innovation--in an appropriate and effective manner? Are there sufficient champions among the ultimate users to get over the initial hurdle of using something new? Related to this topic is planning for implementation, which will be covered later.

A major warning flag must be raised at this point in the research statement development. The definitions of a problem can be so tightly made that the applied research turns into a study with the anticipated results simply needing technical verification. This often happens when risk averse organizations perform research. The risk of not producing results, that are practicable and implementable, are so high that problems for research virtually guarantee an expected result. My concern is that there be sufficient flexibility in the problem statement that unexpected results are encouraged and even welcomed.

There is a significant place for such technical assistance studies in the research community. However, the severely risk averse environment may not be as conducive to producing true innovation as one that allows a manageable amount of risk for successes and failures alike.

Literature Searches

My approach to performing research is to know as much as I possibly can about the state-of-the-art of the problem/topic, and have that information as soon as I can get it. The avoidance of unnecessarily duplicating research is essential in order to use scarce resources most effectively.

A literature search should be done when writing the problem statement. An even more extensive search should be done as soon as possible after the problem statement is completed. But in our industry--transportation, and let me use highways as an example, a truly thorough search is not easily done. Today we have electronic search capabilities, but we have not maximized the potential benefits of the available technology.

Within our industry we have serious deficiencies in the ability to communicate what has been done or what is currently being done. We have private industry doing research, associations of private industry, a number of federal agencies, state departments of transportation, larger municipalities, academia, and research institutes--a remarkable array of sources of highway-related research findings.

Outside our immediate industry, there is an even more startling assortment of sources of research findings that may be eminently applicable to highways. Now also with defense cutbacks, there are technologies that could be useful to highways.

We are not sufficiently coordinated within our industry, and we are not familiar with what is available outside our own area. We risk duplication of effort and wasted research dollars every time we do not do a sufficiently thorough literature search (obviously there is a place for some duplication of effort in research).

Let me emphasize, the sources we have currently are very good, but more needs to be done. As many of you know, there is a high level group, the Research and Technology Coordinating Committee, now advising FHWA in that general area. Also, the AASHTO

Research Advisory Committee is collecting data for research-in-progress at state DOTs. These are excellent types of efforts, and the information they produce is vital. Other organizations within the highway community must also see the usefulness of this kind of data availability (high level coordination, research-in-progress). Yet so much is done "in-house," and little documentation is registered with nationally available data sources.

The problem is it is a lot of work to maintain accurate data in a form that the data can be effectively disseminated. We as an industry must bite the bullet and get over this hurdle.

Implementation Preparedness

Including this item as an integral part of the research methodology for *applied* research might be considered quite unorthodox by many researchers. Yet, as a consultant, or maybe its just my professional pride or ego, I want to see what I do make a difference. It also benefits me if I can say the results of my work truly changed things for the better. With my own business, I use past successes to generate future business. In positions I held in the past within large organizations, the implementation successes for the organization and the ultimate client brought similar credibility to the research group, and enabled us to perform even more challenging assignments.

Research findings are useless if they are never implemented. If the research findings show the solution is not better than what is currently being done, then implementation is *not* putting the results into practice and may involve going back to the "drawing board."

One asks if I really want flexibility in the problem statement so that unexpected results are also welcomed, and I don't know what the results will be, then how can planning for implementation be done? Essentially, forcing those defining the problem and/or the researcher to consider how the results will be put into practice, is an *awareness* exercise. The implementation plan may be preliminary, but it forces the research project team to acknowledge, upfront, that where possible, implementation strategies must be considered during the conduct of the research.

Just developing an innovation does not guarantee its use. *The process of implementation is tremendously people-dependent, and it is a direct antithesis of the scientific research process, which seeks to eliminate personal influence.* Applied research (as any research) must not only fend off personal influences/biases that skew the results, but must also incorporate in a *personal* way, the ultimate users of the results.

Therefore, the challenge of any (applied) research is not only to find the answer but to present the answer in such a way that implementation can progress.

Some areas that might be included in an implementation plan are generally well known, but the institutionalization of implementation related activities within the actual research effort may not be as familiar a concept. Several areas to consider are:

- upfront involvement of the ultimate user (the mantra of many concerned with implementation today), in defining the problem, in championing the need for the solution, and the ability to implement it;
- similar upfront involvement of the fabricator or manufacturer of the innovation, who may be different than the user;
- regular feedback from researchers to these interested parties should be built into the research process (not just feedback to research management and administration); adjustments to the research based on the user/fabricator input should also be institutionalized into the process;
- marketing and communications techniques and methods must be provided for within the body of the research effort--updating of the plan for ultimate implementation, visual records--pictures or video, preliminary results for field testing, and additional vehicles (based on the installation environment) to explain the research, other than the detailed research report; and lastly,
- accountability for implementation should also be addressed. Who will do the implementation, how does the baton get passed from researcher to implementer?

For applied research, *more implementation preparedness must be done within the traditional context of the research project*. This gives an added role to the research team, which implies having additional skills related to implementation as well as expertise in technical issues and research. Does this mean that an implementation professional is on the research team, yes, maybe. Does it mean that there will be an incremental increase in cost of the research, yes, that very well may occur. Is spending these additional costs justified in order to have the results put into practice or put into practice more quickly? That depends on the individual research project...but certainly if the results might never have been implemented, then yes, the costs are justified.

In summary, the following three major points are not new items, but greater attention must be paid to them.

1. the need for well developed problem statements including not just a technical description of the problem to be solved, but incorporating
   - assessment of problem viability, associated risks
   - usefulness of anticipated results
   - priority for/urgency of producing a solution, and
   - potential for implementation
2. the importance of thorough literature searches and data availability, and
3. implementation preparedness as an integral part of the research performance.

TRANSFERRING RESEARCH FINDINGS TO THE LOCAL HIGHWAY AGENCIES

Lynne H. Irwin
Cornell University Local Roads Program

In 1983 ten Technology Transfer Centers were established in various states by the Federal Highway Administration under what is now known as the Local Technical Assistance Program (LTAP). Ten years later there are 55 technology transfer (T^2) centers, serving all 50 states plus Puerto Rico, and an additional four centers serving the Native-American populations. In addition, with the assistance of the FHWA, new T^2 centers are being formed in Europe and Latin America.

This paper describes a few of the successes experienced by the T^2 Centers, and offer some insight into why they are effective. It will cover some of the new directions that are being taken by the T^2 centers due to the 1991 ISTEA legislation. It is also intended to stimulate some discussion regarding a major problem that is faced by all technology transfer providers.

WHO PROVIDES T^2?

Among the 55 LTAP centers, 43 are housed at and administered by colleges and universities, 11 by state departments of transportation, and one jointly by a university and a DOT. Their activities have generally focussed on providing the following services to local (eg., county, city, town, village, borough, etc.) highway agencies:

- Develop and maintain a mailing list
- Publish a quarterly newsletter
- Conduct training seminars and workshops
- Provide technical assistance
- Provide an information service

Technical assistance usually is provided by answering questions through letters, telephone calls, and in some states, through an electronic bulletin board. Information is supplied by distributing research publications and technical articles, through a lending library, and in some states, through a videotape lending library. Certainly the most visible activity, and perhaps the most effective as well as being the most expensive, is the offering of training seminars and workshops.

Traditionally the audience has mostly been rural local highway agencies, although that is changing due to the 1991 ISTEA. The 1991 legislation provided that the program name be changed from a *Rural* to a *Local* Technical Assistance Program (LTAP), with an increased emphasis placed on extending the T^2 services to urban public works agencies in communities having a population up to one million people. In addition, the 1991 legislation encouraged new activities in the areas of tourism and economic development, and it provided for the creation of four new T^2 programs directed at Native American population areas.

Funding for the T^2 centers initially began at the level of $125,000 per year, with 100 percent coming from the Federal Highway Administration. Beginning in 1988, the formula was changed to 50 percent FHWA and 50 percent local. In many cases the states provided the local match out of Highway Planning and Research funds, which also came from the FHWA.

In 1988 the funding level was raised to $200,000 per center, and in 1993, under ISTEA, the base level was raised again to $220,000. Supplemental funds for the urban program were added in 1992 and 1993, having an 80-20 federal-local matching requirement, with the amount varying from state to state depending on the number of MPOs and similar large municipal areas.

In 1994 the additional funds for urban programming have disappeared, but the responsibility to minister to the urban areas remains. This is likely to create some significant pressures on the T^2 centers fairly soon.

WHAT HAS THE LTAP PROGRAM ACCOMPLISHED?

In 1991 the 47 T^2 Centers then in operation conducted 1,597 training courses, with a total attendance of 55,613. In addition, the centers offered 631 "roadshow" programs, informally taught by a circuit rider, with a total attendance of 10,449. The vast majority of the programs were offered in a one-day format, and taught close to home around the state rather than at a central location. Safety was a principal topic in 493 of the courses.

In one state the program participants reported that for every dollar spent on training (for program registration meals, travel, and worker salary) their municipalities saved $94.

In some cases the training has a hands-on emphasis, as in a program that trains grader operators. One highway supervisor reported:

> *From the town's point of view, the results of the session far exceeded our hopes. By its end, several miles of our roads, that beforehand were flat, constricted and virtually without drainage, were converted to widened, crowned ways with good drainage on both sides.*

Not all local highway agencies can afford the time or the minimal expense to send their employees to the training programs. In this case the newsletters and the technical assistance outreach programs can still provide some help. In 1991 115,191 technical publications were distributed by the T^2 Centers. Collectively the centers maintain libraries with a total of 8,128 videotapes, and 10,237 individual loans were made. Many local highway agencies utilize the tapes to conduct their own in-house training programs. Quarterly newsletters were mailed to 140,539 contacts.

Each center must evaluate its program on an annual basis and submit a report to its state DOT and to the FHWA. In so doing, many of the successes of the program are identified. For instance, the following was reported by one client:

> *We dramatically improved construction techniques by following highway rehabilitation and construction information related to the use of geotextiles, drainage facilities, and soil testing. Consequently, we eliminated problem areas that have existed for decades.*

WHY ARE THESE PROGRAMS EFFECTIVE?

There are a multitude of reasons why the T^2 Center programs are effective. They seem to fall into a relatively small number of categories, however.

1. *The FHWA LTAP program allows each T^2 program to tailor its activities to meet local needs.* Unlike many federally funded programs, this one has managed to avoid the "one size fits all" way of thinking. In some states local government is comprised of a small number of units, often predominantly or extensively run by professional engineers. In other states local government is run by non-engineers, either appointed or elected. Often these latter governments are very small, with a low base of property values, few miles of road to maintain, and few employees. The types of technology transfer programs that work effectively with such different kinds of local government are quite different.

2. *The T^2 programs are run by people who honestly care about their client agencies and want to make a difference.* In many instances the T^2 centers are lead by or employ active or retired local highway department officials, who have broad experience in construction and who have considerable knowledge to share. A great deal of effort is going into understanding the needs of the audience and arranging suitable programs to meet those needs.

3. *The training programs that are taught are relevant to the needs of the audience.* A decade ago a popular buzzword was "appropriate technology." Perhaps for technology transfer in the 1990's the corresponding buzzword should become "appropriate training." It is not sufficient to preach the gospel of technology, it is necessary that the preaching be relevant to the listeners. Much of the success of the T^2 movement has been in its ability to recognize the right information to provide at the right time.

It is not useful to try to train a highway employee to use methods that a limited budget will not permit the agency to utilize. Thus in some cases it is better to train a highway employees to do a better, longer-lasting job of blading a road than it is to teach them to pave the road. Traffic counts and economic analysis might indicate that the road should be paved, but the practical realities of the situation dictate that the road remain with an aggregate surface.

4. *The T^2 Center programs are meeting a previously unfulfilled need.* In 1982, before the advent of the FHWA-sponsored RTAP program, there were very few active T^2 programs ministering to the needs of local government. Back in 1895, when the Office of Road Inquiry was first formed (the precursor to today's FHWA), until the start of World War II, there was a great, national involvement in providing training for highway departments at both the state and local levels. Road Shows went from city to city and state to state, espousing new and better methods of road building. Extension agents from the universities taught workshops on "scientific" road construction methods. And industry provided a large amount of training and direct technical assistance through associations such as the Asphalt Institute, the Portland Cement Association, the American Road Builders Association, and others.

Gradually over the three decades following World War II many of these activities died and were forgotten. When the T^2 movement came along in the 1980's, there was a great need for such programs, particularly on the part of local government.

MAINTAINING FUTURE EFFECTIVENESS

Over the past eleven years since the first ten T^2 Centers were established, there has been a notable maturation of the movement. The first decade was also a period when each state had to decide whether and when to get on board. The first decade was a period when the transition from having 100 percent federal funding to finding a source for a local 50 percent match had to be dealt with. This decade was a period when the T^2 Centers had to meet their audience and identify their role.

While some might argue that we are not yet out of the woods, much of the preliminary maneuvering is behind us, and now is an excellent time to look ahead. We need to identify clearly what is needed in order to do the job that is in front of us. Each individual T^2 Center might have a slightly different perspective, but there are a few broad principles that should apply to all of the centers.

- Get to know the audience even better. After a few years of serving a given set of constituents there is a danger that the servers may feel they know the constituents well. But in public works everything is changing all of the time. There may be no other field of endeavor that has a higher rate of turnover of personnel. Each year's budget is very dependent on the national and local economic climate, and that changes yearly. Thus both the audience and the needs of the audience are continuously changing.

• Be sure that you know how to approach your audience. With the addition of new responsibilities under ISTEA to provide training for urban municipalities and related matters, there is a danger in assuming that the urban problems are the same as those of rural local government. There may be a world of difference, beginning with a less enthusiastic willingness to partake of the traditional T^2 program offerings. Before doing things that are not effective, and then trying to recover from it afterward, it would be better to engage in a careful diagnostic of the new audience.

• Maintain a positive relationship with the FHWA. The modern interpretation of the Golden Rule is "he who has the gold makes all the rules." The FHWA provides the initiative from which funding for all T^2 centers flows, and for this reason alone it would be wise to work closely with the FHWA people at the Division and Regional levels. But beyond that, in recent years the importance within FHWA of being involved in the success of the T^2 movement has grown immensely.

Now is an especially excellent time to build on the relationship with the FHWA, because there are no immediate crises. Find out how you can help them to be successful, and your T^2 Center will be successful along with them. You may find that the people at FHWA are willing to help nudge matters along within the state DOT on behalf of the T^2 Center.

• Build on the relationship with the state DOT. If things are going well right now, they probably will change. If they are going poorly, they need to change. In either case, there is a need to build a climate of understanding with the state DOT. Officially, the FHWA works through the state DOTs in communicating with the Technology Transfer Centers. Thus the state DOTs are now and will remain on the critical path to success for the T^2 Centers.

• Integrate into the University environment. While not *all* T^2 centers are housed at universities, nearly three-fourths of them are. It is particularly important for the university-affiliated centers to avoid becoming too isolated from the surrounding academic environment. While some centers are "on the fringe" of the university, in separate centers, institutes, cooperative extension and/or continuing education programs, many of the most successful centers are located in academic departments.

This proximity provides an entree to researchers and other teaching professionals that is typically not available to the more remote programs. The possible exception to this "rule" is in the case of an affiliation with a large transportation institute, where faculty and other transportation professionals are drawn together, perhaps from many academic departments and even several campuses. If there must be a trade-off between having greater autonomy and having more extensive affiliation, due to the importance of networking in the success of T^2 efforts, having greater access to colleagues should be given a higher priority.

• Look beyond the borders of your own state. One characteristic of most successful and effective T^2 programs is that they are run by thieves. Well, not thieves in the legal sense of the word, but T^2 people are definitely willing to adopt good ideas that they see being used elsewhere.

Now is a good time to start to think regionally and to build liaisons with the T^2 Centers in the states that border on yours. In September 1993 the *LTAP Training Exchange* published a list of 55 training programs that the LTAP Centers were willing to share with each other. Course development consumes a large proportion of the funds available for training, and using a good course developed by another T^2 center is a good way to save money all around.

In summary, the effective manager of a technology transfer program needs to be able to be simultaneously introspective and visionary, aware of what is happening with the audience, the funding agencies, and all of the other T^2 Centers. Such a person has to be willing to develop new programs, steal the best ideas from his or her colleagues, and share the best ideas and programs with other centers!

WE HAVE ONE LITTLE PROBLEM

While this is supposed to be a report on the effectiveness of the T^2 centers and how we transfer research findings into practice, there is one great concern, that needs to be addressed.

The problem faced by all of the technology transfer centers is *how to measure whether the technology has been transferred?* This may not seem like much of a problem to those who are not involved, but some of us face the problem on a daily basis. It cuts to the quick as far as evaluating the effectiveness of technology transfer is concerned.

This is not a problem that can be researched according to the customary scientific methods. We cannot, for instance, establish a "control group" by selectively keeping part of the audience ignorant and unaware of a new road construction method, while others are being trained. After a particular point in the training supposedly both groups could be watched to see if one group uses the new method or if their roads last longer, or something.

In the absence of such an impossible scenario, it is difficult to establish whether the training that is given has any effect. Commonly questionnaires are used. And sometimes field visits or telephone interviews are tried. But there is always the uncomfortable suspicion that those interviewed may be telling the interviewers what they want to hear. "Oh yes! Your training program was very beneficial!" "Oh yes! Our municipality saved millions of dollars as a result of that training program!" "Oh yes! We use those materials daily. Why only last night I read them to my children while they were going to sleep!"

How can we know, really, whether the training has any effect on the behavior or the decisions of the agency that received it? How can we know that their procedures did not change because of reading an advertisement in a magazine, or due to a visit of a salesman, or because the highway crew in an adjacent town was using the new method?

This is a problem that would benefit from some good, clear thinking.

APPRAISING TRANSPORTATION RESEARCH

Linda R. Cohen, Gordon J. Fielding, James F. Nolan and George C. Smith[1]

Abstract

Public sector involvement in research is examined, with emphasis on the creation of incentives by government. In transportation, government investment in research is often a spark for improvements in overall economic productivity. This connection has been dubbed "the Virtuous Circle" and helps justify basic and applied research.

Is research always justifiable from a societal perspective? The answer to this question is best approached with the application of cost-benefit analysis, or more specifically net present value (NPV). While NPV appears to be simple calculation yielding transparent solutions, a proper cost-benefit analysis requires careful construction of a base case as well as decisions on discount rates and indirect impacts. Sensitivity analysis is used to check the validity of the chosen assumptions. A case study of new, high speed rail technology illustrates how some of the concepts can be applied.

APPRAISING TRANSPORTATION RESEARCH

Research in transportation changed dramatically during the 1980s. Interest in transportation waned at the beginning of the decade: funding declined in real terms, enrollment in university courses dropped, and private research firms were diversifying. Deen (1) observed that research expenditures had fallen between one-tenth to two-tenths of a percent of total spending in surface transportation modes and that technological improvements were being neglected. Astute lobbying reversed this decline by the end of the decade. Beginning with the Strategic Highway Research Program (1987), a series of new federal research and development initiatives were authorized. This has included, among others, the Program to Automate the Highway (1987), University Research Centers (1987), the Highway Safety Act of 1987, funding for Intelligent Vehicle-Highway Systems (1991) and increased funding for both the cooperative highway and transit research programs.

The recent fortunes of transportation research have changed dramatically. Between 1989 and 1992, federal funding for transportation research has increased by 14.9 percent in real terms. Funding by state agencies has also increased although the magnitude is unknown. Possibly the best indicator of the change is to be seen in the Highway Planning and Research Funds available to states and federal agencies, which doubled from $150 million in 1987 to $300 million in 1992.

A new dilemma now faces managers of transportation agencies; how to allocate the funds efficiently. This article, based upon research requested by the California Department of Transportation (Caltrans), considers research as an investment. A rationale for government investment is presented together with various approaches to research sponsorship. Techniques for developing and appraising R&D (Research and Development) proposals are considered with the recommendation that the Net Present Value method for investment evaluation be used. An example for rail technology is used to illustrate the approach. Additional examples as well as a methodology for appraising a portfolio of R&D proposals are included in the final report (2).

THE CASE FOR PUBLIC SUPPORT OF R&D

Economists justify government support for research with market failure arguments. Firms are not sufficiently rewarded for undertaking research activities because their profits may be substantially less than the social value of innovations. This argument rests on two attributes of research: first, a successful project results in information about new products or processes, and second, substantial uncertainty exists about the commercial prospects of a research enterprise.

Information, once it becomes public, can be used freely by people other than its discoverers. Sometimes just the knowledge that a product is feasible gives an advantage to potential competitors. The first characteristic of research implies that an innovation can be copied at much less expense than the original research or development work, so that competing firms can gain profits from the invention at a lower cost than the original innovator. This is known as the "appropriability problem"; researchers may be unable to appropriate the full returns from an invention. Indeed, it may be in everyone's interest to be a copier rather than an innovator. As a result, research will receive less attention than it should, and in some cases it might not be performed at all.

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The second attribute, uncertainty, is more subtle. The problem is not just that uncertainty over profits exist, but that risks to individual investors may be much greater than for society as a whole. When private risk exceeds social risk, firms underinvest in research activities.

Use of public resources to subsidize research is a common response to these market failures. However, while lack of appropriability and uncertainty are characteristics common to all research activities, they are particularly problematic in the supply of government services like highways and public transit where public and private investments are commingled.

Government Goods

By government goods we mean any product whose use is determined, or significantly affected by the public sector. It is important to distinguish government goods from other products in assessing R&D policy for two reasons: first, because the public sector is instrumental in their use, a range of policy options for encouraging R&D through market-pull policies are available to government that are not feasible for other products, and second, market failure problems can be severe.

Uncertainty is compounded for private companies who might be interested in improving the technology of government goods. Public decisions reflect nonprofit oriented goals; in addition, they depend on constraints not present in the private market. Purchasing decisions can reflect political imperatives: maintaining employment in a certain area, for example, or "buy American" requirements. Regulatory requirements that might be critical for establishing a market for products can shift for reasons unrelated to the actions of suppliers. For example, strict environmental requirements are sometimes relaxed during economic downturns. Furthermore, personnel shifts, either administrative or legislative, are frequently accompanied by changes in policies. Different administrations may place different priorities on public goals: for example, the desire to spur economic growth versus avoiding environmental harm caused by development. All of these factors raise uncertainty for firms, so that they become reluctant to invest in research.

Underinvestment in research for government goods arises because government cannot commit to a set of policies over time (3). Market failure is severe when the time horizon of the research project is long, because the resulting innovations are likely to be available only after the government has changed. Thus, in designing strategies to promote research for government goods, it is important to consider the relationship of the product to potential changes in policies, and to discount potential benefits according to this risk.

This section has identified a number of different market failures that can give rise to underinvestment in research. We turn next to an overview of promotional strategies available to a government agency.

PUBLIC STRATEGIES TO PROMOTE R&D

Strategies to promote research fall into two main categories: those designed to lower the cost of research, and those intended to increase the value of results. The latter are usually called "market-pull" or demand strategies, while the former attempt to increase the supply of research. Four alternatives are considered: on the supply side, direct funding of research and conducting research in-house; on the demand side, establishing prizes for innovations and creating market guarantees.

Direct Funding of Research Activities

Grants and contracts to firms and individuals form the main alternative by which government promotes research. The chief advantages of the strategy are: it is relatively easy to institute, it enables specified goals to be addressed with some precision and it allows the government to retain control over the quantity of expenditures devoted to a project. In addition, many federal cost-sharing programs are exclusively for research grants and contracts, so that an agency can only take advantage of federal programs if it institutes this method of encouraging R&D. The strategy has two main disadvantages: it puts the agency in the position of "picking winners" (both projects and contractors), and the agency is still responsible for monitoring progress. This places a substantial informational burden on the governmental agency. And in a new field like automatic vehicle control systems, few firms have track records to support proposals, and new firms may be overlooked.

Subsidies for research are alternatives to grants and contracts. The federal government gives a tax credit to firms for expenditures devoted to R&D. This policy avoids both picking winners and monitoring; alternatively, it does not allow the government to single out those areas that are more prone to underinvestment. Another related strategy is to subsidize loans to firms. The federal Synthetic Fuel Corporation guaranteed loans to selected companies that built energy demonstration programs in the early 1980s; an expanded version of this policy is currently under consideration. Subsidies have also been granted to encourage the manufacture of low-emission automobiles.

In-House Research

Another possibility is for the government agency to conduct research in house. For example, the Division of New Technology, Materials and Research in California provides in-house research and testing of materials and structures for Caltrans. In addition to avoiding monitoring problems associated with contracting out research, the strategy provides an important spillover benefit for the agency. It provides the agency with a cadre of scientists who can evaluate outside proposals and inform the agency about research opportunities. Research contracts with both state university systems and several private research institutions are managed by the Division to examine and develop innovative approaches to transportation.

A similar rationale is used by major firms who conduct basic research. A number of large U.S. firms have world class science laboratories. These companies claim that the expense of their laboratories is justified because the quality of scientific advice that they get from employees on a range of topics would not be available if they didn't provide the scientists with opportunities to conduct research as well as review and evaluate research done elsewhere.

Conducting research in-house is subject to several pitfalls. Civil service rules, and indeed, normal employment practices, make it difficult to either cut back or change employment in a short period of time. Research contracting gives an agency a level of flexibility that is difficult to duplicate when activities are concentrated within the agency. Another problem is that the agency's employees are likely to be proponents for the use of innovations developed within the agency, as opposed to technological alternatives developed elsewhere. Thus, it is probably more appropriate for an agency to undertake activities that overlap only minimally with technologies investigated in the private sector.

Prizes for Innovation

Another alternative to funding research is to give some kind of financial award to successful innovators in particular technology areas. In order for this strategy to establish incentives, the prize needs to be announced in advance. Firms conduct research and then submit the results of the research. The "best" system wins a procurement contract, which is usually lucrative. A second form of prize that government can give to firms is through standard setting. A current example is the high definition television (HDTV) "standards competition" that the Federal Communication Commission (FCC) has undertaken. The FCC has announced that it will establish a standard for HDTV that will support the best design from among several proposals that are being submitted by competing television firms. The standard will yield considerable wealth to the firm or firms that will hold relevant patents, and is thus a form of prize for research activities.

Prizes have been shown to be very effective devices for inducing private firms to expand their research activities. Selection of private consortia to construct and operate the four toll road projects as authorized by the California legislature in 1990 is an example of the prize strategy. Caltrans initiated the process by inviting firms to submit qualifications; 10 were accepted and invited to propose specific projects. Eight proposals were submitted. Although each proposal had cost private companies $1 million or more to prepare, only four were awarded franchises.

The prize strategy avoids many of the problems identified with direct research awards in that the government need not choose a research strategy, nor evaluate the qualifications of potential researchers. However, it too suffers from limitations. First, the strategy is most successful when a number of different firms can compete for the prize. For example, defense department contest results are very sensitive to the extent of competition. When procurement contracts are awarded on a noncompetitive basis (e.g., sole-sourced), they yield no measurable incentive for firms to conduct research in advance of the contract. Second, the government needs to be able to specify the particular product or application in advance. Thus, it is not a feasible strategy for the conduct of basic research. Third, the commitment to provide the prize needs to be firm. If a technology forcing regulation is modified in subsequent years, firms that invested in the original technology would be left stranded. Indeed, firms would probably discount the potential profits to reflect their assessment of the strength of the political commitment. For these reasons, commitments become attenuated over time in the political sector. The policy is probably most effective for innovations that require relatively little lead-time.

Market Guarantees

The government can guarantee a market for categories of innovations, although not for specific firms, through several mechanisms. One is technology-forcing regulations. Such regulations, which are successful in such areas as automobile emission systems, establish a future date by which products must conform to new technological standards. Another option is government procurement; this strategy yields efforts in research when firms have reason to believe that their product will be adopted by the government. As with prizes for innovation, the policy avoids problems with direct research funding, in that government need not identify which firms are likely to be successful in advance. The strategy is only available to goods whose use the government regulates or purchases in significant quantities. Since the policies need to be credibly committed to in advance, the use of this strategy is further limited to cases where the government can make a commitment, either to follow through on purchases or not to modify standards and regulations. However, this is unlikely to be an effective strategy for promoting basic research whose applications are both uncertain and only likely to be available far in the future. Market guarantees are an attractive alternative to encouraging research in areas that are likely to pay off soon (development work, in particular) and whose importance is agreed to by consensus.

Although demand side strategies for encouraging research such as prizes for innovation and market guarantees are attractive, institutional relationships of federal, state and local agencies make it more likely that transportation research will remain supply side oriented. Therefore, strategies are required that will improve the selection of contract funded and in-house research.

RESEARCH AND PRODUCTIVITY GROWTH

Research plays a critical role in productivity growth, because through R&D agencies acquire the knowledge that enables them to utilize capital investments. For any production unit, output is functionally related to the use of inputs which are conventionally categorized as labor, capital equipment, and natural resources. The functional relation between inputs and outputs represents the technology of production, the managerial and technical skill of owners and employees, and the organization of economic activity within the region.

The "Virtuous Circle"

Investment in transportation facilities can trigger productivity growth. By decreasing assembly and distribution costs, the spatial scope of competition is enlarged and management is motivated to explore improved technologies for producing goods and services. This is why R&D is critically important to productivity growth; successful innovations require additional capital and this continues the cycle.

Lewis, Hara, and Revis (4) explain the crucial role of capital investment as a "virtuous circle". They emphasize that capital investment, including the development and maintenance of transportation infrastructure, offers one of the most effective catalysts for productivity growth. Innovations from research spur better use of resources; implementation occurs through new facilities and superior operating modes that can improve productivity and contribute to economic growth. And the investment of additional capital prompts the cycle of new research and improved technology. However, not all transportation investments are necessarily beneficial. Decision makers must evaluate the net benefit of proposals before investing capital.

Lasting benefits from transportation are achieved through increased productivity. Travel time reductions may benefit commuters, and special services may satisfy the travel needs of individual groups, but the sustaining benefits are those which boost productivity by reducing costs or raising the quality of goods and services.

Economic Impacts

Overall benefits of transportation improvement are frequently obscured. They are normally expressed as the number of jobs created or the number of purchases from other sectors, whereas it is through increased productivity that real economic benefits are achieved. In addition, the influence of transportation upon personal and regional income and land use is usually omitted because of the time and cost required for this type of analysis.

Promotional literature associated with transportation improvements boasts about the number of jobs that will be created. If this logic is followed, workers would be unemployed at the conclusion of construction. A counter argument goes as follows: if the taxes had not been collected to pay for the improvement, individuals would have spent their money and created private demands for additional employment. Only in regions of chronic unemployment can a genuine case be made for transportation investment creating jobs (5). A more thorough assessment of overall benefits is made by examining the productivity increases derived from transportation investment. Elimination of congestion reduces travel time and translates into real improvements in productivity, allowing firms to reduce costs. Improved productivity stimulates the economy and encourages the hiring of additional employees. For example, a supermarket chain owning stores and warehouses across the country will benefit from highway improvements through reduced travel time for trucks. This sort of benefit has been described by Quarmby (6) for the Sainsbury supermarket chain in the United Kingdom.

COST-BENEFIT ANALYSIS AS A TOOL FOR DECISIONMAKING

For a state to remain competitive in an expanding global economy, research and development must be an integral part of the commitment to economic growth. In this respect, transportation research serves a twofold role: it is a way in which agencies may look into their own future to set a strategic course, and it is a way to improve the efficiency of existing operating systems.

Despite recent increases in the funding available for transportation research, the current financial climate imposes strict constraints upon the allocation of funds. Agencies and firms can no longer invest money in research without clear objectives and knowledge of probable outcomes; therefore, techniques like cost-benefit analysis (CBA) are required to examine the merits of, and guide the choice between, competing proposals. Although widely used in transportation, CBA is seldom employed correctly, and special care is required in order to avoid errors. The following requirements are essential:

- Uniformity in assessments across proposals must be preserved. Cost-benefit analysis relies upon the art of arranging uniform assessment of alternatives. This may entail the sacrifice of information available for only some alternatives.
- Goals must be defined in operational terms together with the rate of return that is expected from transportation investments.
- A base case, using the best available practice, must be defined so that there is a datum against which future improvements can be measured rather than the "do nothing" case.
- Timing of costs and benefits must be estimated and values discounted to current dollars.
- And results should be tested for sensitivity to changes in critical assumptions, such as the rate of discount.

Cost-benefit analysis creates a ranking among competing alternatives that is best used as one input into the complete decisionmaking process. The criterion used for this ranking is that of maximizing monetary return (benefits) for a given amount of money invested (costs). Quantifiable estimates are preferred, but qualitative estimates can be used and the ranking can be integrated with other criteria to create a measure based upon different goals. For example, Gosling and Jackson (7) describe a methodology used by the Wisconsin Department of Transportation to allocate funding among projects. The methodology consists of an equal weighting between cost-benefit results and the goal of political acceptability.

Cost-benefit analysis comes in a number of forms which differ in the way costs or benefits are expressed. The most common forms are cost-effectiveness analysis, benefit-cost ratio, internal rate of return and Net Present Value (NPV). The merits of each are discussed by Mishan (8). We focus here on NPV.

Net Present Value

The recommended method for expressing the relationship between costs and benefits of research is net present value. This criterion is similar to the benefit-cost ratio, but expresses the result in current dollars rather than a ratio. The formula is written as:

$$NPV = \sum_t (B_t - C_t)/(1+r)^t$$

where B are benefits, C are costs, r is the discount rate (or the rate at which money could be invested elsewhere in the economy) and t is the number of time periods considered, usually the projected lifetime of the particular project.

The larger this value, the more a project improves welfare. Expression of the result in current dollars is a real advantage for decision making, and most of the information required to calculate NPV is available from the same data used to calculate cost-effectiveness studies. Lewis (5) illustrates the superiority of NPV by recalculating the results from an UMTA sponsored study that appraised four transit alternatives in reference to the goal of lowering "cost per new transit rider." The results are instructive; whereas the cost effectiveness study appraisal favors the light rail option, NPV shows that no alternative yields a positive net benefit over a specified base case which entailed using existing infrastructure more effectively. However, the results would change if different discount rates were used, or if a longer project life was assumed.

These cautions are appropriate; NPV, like other methods of CBA, is a technique for appraising similar proposals. It is one element to be considered in the complete decisionmaking process. It should not be used to predict the actual financial outcome of a proposal or project.

Advantages of NPV

Although four methods, cost-effectiveness, benefit-cost ratio, internal rate of return and NPV are frequently used in transportation, this does not imply that they yield the same information. Both cost-effectiveness and benefit-cost ratio neglect the magnitude of the benefit component. This omission can be confusing as the following example illustrates. Assume that there are two proposals:

Proposal 1) $5 in benefits: $1 in costs.
Proposal 2) $1200 in benefits : $1000 in costs.

By the benefit-cost ratio criterion, Project 1 is preferable. For every dollar in cost, the small project generates $5 of benefits. But Project 2, which has a benefit-cost ratio of 1.2, yields $200 in net benefits (benefits minus costs), a substantial improvement on the $4 in net benefits generated by Project 1.

Using the NPV criterion, Proposal 2 is preferred. From a societal perspective, $200 in net benefits is superior to $4. NPV takes into account the size differentials between projects and removes biases towards smaller projects. Only when projects have similar benefits and costs do the two methods yield comparable information (9).

COST-BENEFIT ANALYSIS FOR RESEARCH

Cost-benefit analysis is not a flawless tool for evaluating research proposals. The following is a discussion of some of the most common concerns, including: 1) base case specification, 2) incorporation of indirect impacts (general equilibrium), 3) discount rate, and 4) sensitivity analysis.

The Base Case

Perhaps the most important issue facing the decisionmaker in choosing among research proposals is the selection of the base case, a scenario wherein no *new* project is chosen. This does not mean the comparison is made to a situation where the agency does nothing. On the contrary, the base case scenario should include predicted improvements in current managerial practice and physical infrastructure. For example, the base case for the transit comparison in Lewis (5) assumed a traffic management system that would facilitate the use of streets by transit. Comparison of the capital intensive alternatives was based upon the improvements over the best, current managerial practice. Without designation of a base case, which happens frequently, assessment of benefits is exaggerated.

Indirect Impacts

General equilibrium impacts in transportation refer to benefits (or costs) which result as a consequence of increased ease of movement for both goods and people. These impacts include the technical changes in industry which transportation improvements allow that create improved productivity. General equilibrium impacts are seldom consistently accounted for in CBA.

Travel time savings are frequently used as a surrogate for general equilibrium benefits in transportation, but the efficiency gains that travel time savings encourage are normally omitted, resulting in the underestimate of total benefits (10). Quarmby (6) references case studies from the grocery industry in Britain indicating that productivity gains accruing to the industry as a result of travel time savings by commercial vehicles tend to be underestimated by some 30-50 percent. Omitted benefits are those achieved through economies of scale.

Collection of data is the primary pitfall for most analyses of indirect impacts. The necessary data is frequently either costly or impossible to collect, and value judgements have to be made as to what kind of data is sufficient to account for general equilibrium effects or what kind of proxy data will suffice. Rather than requiring that all indirect impacts be included, it is more important for comparison that competing alternative proposals incorporate the same types of data.

Choosing a Proper Discount Rate

Cost-benefit analysis requires that all elements of the calculation be in a common time frame. The way to do this is through discounting. This section examines some issues involved in discounting.

Discount rates should be in real terms, i.e., corrected for inflation. Furthermore, these rates ideally are adjusted for risk, where risk in this case refers to a project's correlation with the overall health of the economy. It would be simpler if a financial risk of this type could be avoided so as to take away any economic biases (such as economic growth or decline) which may occur over the entire duration of a benefit and cost stream.

For federal agencies, Lind (11) reports a ten percent real rate of discount as standard. Ten percent is roughly equal to the return on private capital in the United States economy. A case could be made for a reevaluation of this rationale for discounting, given the nature of international capital markets. An open economy has various implications for private investment returns, the most important of which is that the prevailing rate of return in the home country may not be the highest or best return to private investment. Thus, in a single country, interest rate is no longer applicable as an indicator of the appropriate discount rate. Lind advocates using not only an equilibrium world interest rate as the discount rate, but also the consumer borrowing rate at home to measure investment as well as consumption effects in CBA.

Hartman (12) describes discount practices in the Congressional Budget Office (CBO) and gives a different rationale for the choice of a discount rate. The government is described as viewing its investment projects in terms of opportunity costs, or the cost of the next best (or possible) alternative. With this perspective, the CBO judges the proper rate of discount to be a time adjusted consumption preference rate. Hartman suggests that this can be approximated by government security yields.

An agency should be cautious in choosing the discount rate for project evaluation. The structure of financial markets implies that the national opportunity cost of capital (interest rate) may no longer be a useful guide for making decisions in a regional context. Some measure of time preference, like the consumer borrowing rate, should also be used to discount projects. Since these rates may vary widely, it is essential that the analyst perform sensitivity analysis to examine the effect of rate changes on cost-benefit rankings.

Sensitivity Analysis

Sensitivity analysis analyzes how a project ranking will be affected by changes in assumptions or variables such as the discount rate. Its main strength is its simplicity of implementation and interpretation. Modifying CBA in this manner allows the analyst to note how changes in discount rates affect the choice between risky and non-risky projects.

Preference for present consumption implies that research projects with their longer benefit time horizons are risky investments. Imposing standard capital-budgeting discount rates invariably biases against research. Sensitivity analysis, however, allows research projects to be compared to other non-research projects using different discount rates to see whether research achieves a positive NPV under different assumptions. As research projects with long-term horizons appear to be sound investments only when lower rates of discount are used, shorter term, demonstration proposals, with high payoffs can be used in conjunction with them to constitute a risk-minimizing portfolio of research investments.

CASE STUDY: HIGH-SPEED RAIL

As part of the final report for the California Department of Transportation a case study was completed to determine whether additional research on high-speed trains between Las Vegas, Nevada to Anaheim, California would be worth the cost. For the purposes of comparison, value of travel time saved was used as the societal benefit derived from implementation of this technology. *Caltrans needs to decide whether the speed difference offered by alternative technology is worth the cost of the research required to adapt technology to local requirements.*

After choosing a base case comprising an electric tilt train (called the X2000) used in the Northeast United States, a comparison was made to an existing advanced technology (the TGV or Very High Speed train used exclusively in France). In order to encompass all alternatives, MAGLEV (magnetic levitation) was included as an example of the riskiest technology foreseeable for high speed rail travel. The case study focused exclusively on the net present value of the travel time savings of the three technologies between the years 2000 and 2015. A discount rate of 10 percent was used. MAGLEV and TGV were found to have $392 million and $97 million respectively of time savings benefits over the X2000.

Neither the suitability of these technologies nor the cost of implementation were studied. Rather, the case study assessed the potential value of time savings and whether these were sufficient to justify additional research.

Travel time savings between Anaheim and Las Vegas were calculated based upon the estimated best travel time of seven hours using current available Amtrak schedules. TGV would save 11.82 minutes per trip over the base case, while Maglev would save 33.72 minutes. See Table 1.

TABLE 1 Time Savings in Minutes to Las Vegas

X2000	TGV	Maglev
278.94	290.76	312.66

While these numbers are helpful, they should be converted to monetary form. Assuming a value of time of $6.35/hour in 1987 and a five percent inflation rate, the value of time in the year 2000, the target date to begin operation, is $11.97/hour ($0.1995 per minute). By multiplying the time saved by the value of time and the projected number of passengers over a fifteen year period, we come up with the Present Value of Benefits attributable to the reduction in travel time for each system.

The projected ridership for the year 2000 has been calculated for the TGV and Maglev systems (13). The difference in ridership between the two is approximately 13,286 passengers per year for each mile per hour speed difference. Given this data, ridership for the X2000 in the year 2000 can be established. Since the X2000 runs at an average speed thirty miles per hour slower than the TGV, about 398,571 fewer passengers are expected.

TABLE 2 Projected Ridership for the Year 2000 (one-way trips)

X2000	TGV	Maglev
1,811,429	2,210,000	3,140,000

Annual ridership over the fifteen year period from 2000 to 2015 is estimated by increasing ridership proportional to a 2 percent population growth rate for the state of California.

Using the value of time estimated for the year 2000 ($0.1995 per minute) and the projected ridership of the three technologies over 15 years, the Net Present Value (in year 2000 dollars) of time savings can be calculated (see Table 4). The Present Value of time savings for TGV is about $97 million greater than the time savings for the X2000, and the Maglev exceeds TGV technology in time savings by about $295 million and the X2000 technology by $392 million.

TABLE 3 2000-2015 Ridership of the Los Angeles-Las Vegas Corridor

X2000	TGV	Maglev
22,763,743	41,192,821	58,527,354

Potential savings of this magnitude indicate the desirability of additional research. The magnitude of expenditure should be determined after the sensitivity of potential time savings is examined.

TABLE 4 Present Value of Time Saved (over the base case) from 2000-2015 (year 2000 dollars)

TGV	Maglev
$97,136,379	$392,376,276

Sensitivity Analysis (this Case Study)

By changing the value of benefits the net present value of additional research can be altered. For example, estimates for value of time saved are the least reliable. We chose a 5 percent annual rate for increases in the value of travel time savings. For the period 2000-2015, estimated savings over the base case (X2000) are $97,136,379 for TGV and $392,376,276 for Maglev (Table 4).

If a 3 percent rate for growth in travel time benefits were used, the value of travel time savings would be $75,631,667 and $305,509,347 respectively. Therefore it is appropriate to report a range for the potential travel time savings of Maglev between $305 and $392 million, using the year 2000 as the base year.

Maglev is still in the experimental stages; and a state agency requires guidance on how much should be allocated for additional research and development. Although Maglev offers the highest potential travel savings, uncertainty exists over design requirements and environmental impacts. Guidance on appropriate levels of research expenditure can be obtained by comparisons with other industries operating under similar risk. The motor vehicle industry for instance, spent an average of 3.6 percent of net sales on research during the 1980's. This seems like a reasonable standard. If travel time savings over 15 years are accepted as a proxy for revenue in this period - another reasonable assumption - then research expenditure on high-speed ground transportation of between $11 million and $14 million is warranted.

CONCLUSIONS AND RECOMMENDATIONS

Despite the extensive use of CBA by transportation agencies, many studies are deficient; they fail to comply with the basic requirements for economic analysis. A recent Transportation Research Board report (5) examines 35 case studies and describes only 6 as "adequate". Failure to discount costs and benefits correctly or to use sensitivity analysis to accommodate risk and uncertainty were the most common omissions. Such errors are avoidable because most of the inadequate studies contained data that would have allowed the deficiencies to have been corrected.

Using NPV to create a fair and consistent CBA is a matter of trying to account for items and effects mentioned below:

- *Goals* for research should be preestablished together with the rate of return that decision makers expect from transportation investments.
- *Base case*. Most transportation research is applied; it seeks to make an incremental improvement in current practice. Definition of current practice should include use of the best available practice, otherwise the benefit from the research will be exaggerated.
- *Costs*. All costs should be included and not only those used to finance the research. Relevant costs would include any negative effects on the environment and employment.
- *Benefits*. All benefits should be identified. They should include direct savings as well as indirect impacts (general equilibrium effects) on the economy achieved through any restructuring that may result from the research.
- *Discounting*. All benefits and costs should be projected for the duration of the *longest* proposal under review. And they should be calculated to present-day values by applying the discount rate agreed to as a goal for transportation research.
- *Sensitivity analysis should be conducted to assess the robustness of results*. CBA involves assumptions about likely costs and benefits and probable discount rates. Results should be tested against the most likely range for the critical assumptions. At a minimum, the effects of changes in projected travel demand and cost inflation should be examined as this will expose uncertainty that may be inherent in the proposals.

Recommendations for CBA studies in this report should be viewed as a new outlook on a familiar framework rather than a new methodology. The procedures are well known, though seldom followed. The standard procedure provides an economic basis for any agency wishing to implement efficient and fair research allocations within increasingly limited budgets.

ACKNOWLEDGMENTS

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APPENDIX: FHWA'S PROGRAM APPROACH TO RESEARCH METHODOLOGIES

Robert J. Betsold
Federal Highway Administration

Program Approach to Research Methodologies

Robert J. Betsold
FHWA

Introduction

- Internal and External Coordination
- Program Diversity
- Budget Allocations

DOT Coordination

- R&T Steering Committe
- R&T Coordinating Council
- Dir., Technology Deployment
- Surface Transportation R&D Plan

FHWA R&T Program Development

- Research and Technology Executive Board
- Research and Technology Coordinating Groups
- Working Groups

Coordination with External Programs

- State Planning and Research Program
- National Cooperative Highway Research Program

Coordination with External Programs (cont'd)

- Small Business Innovation Research Program
- Univ. Transportation Ctrs
- Univ. Research Institutes

Coordination with External Organizations

- AASHTO, TRB, CERF, etc.
- R&T Coordinating Committee
- IVHS America, Motor Carriers Advisory Committee, etc.

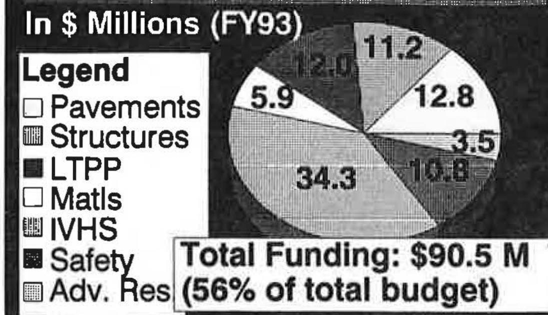
Interagency Coordination

- Nat'l Science and Tech. Council/COMAT
- Interagency Coordinating Committee for Transp. R&D
- Advanced Research Projects

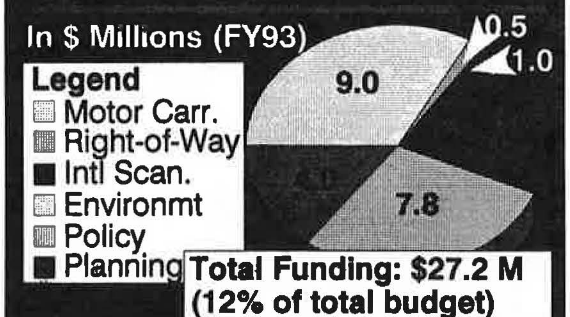
International

- OECD
- PIARC
- Cooperative Research Agreements

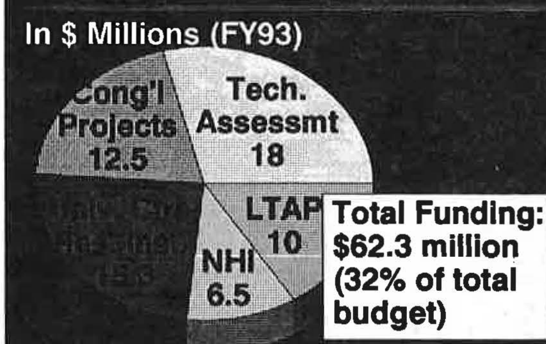
TFHRC Research



Other FHWA Research

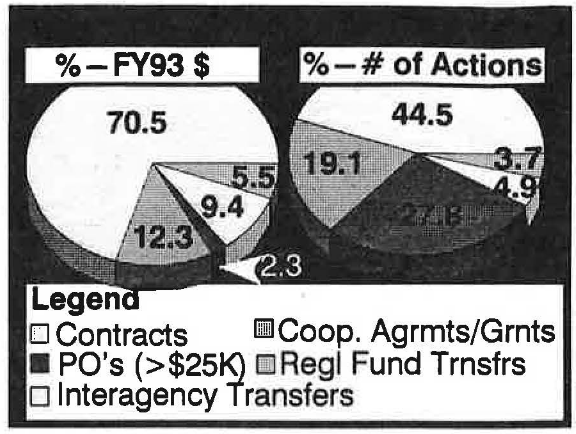


Technology Applications



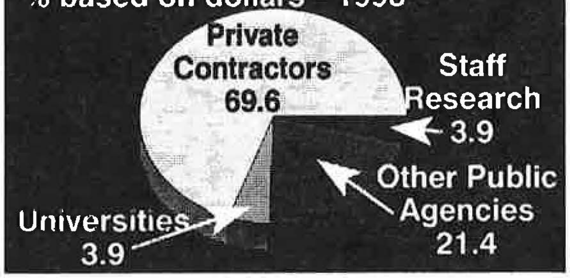
FHWA R&T Budget (FY 1994)

- FY 1994 Appropriations – $103,958,000
- ISTEA Accounts – $41,000,000



TFHRC Program Participants

% based on dollars – 1993



FHWA R&T Future Looks Bright

- Continued Strong Support for R&T
- Defense Conversion Possibilities
- Internal Reorganization