

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 finds 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/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.