The application of behavioral science research can contribute greatly to organizational and personal productivity. Our belief in that assertion is what motivated this conference. But, while there have been some very successful behavioral science applications including some presented at this conference (Cascio, this volume) there have been nowhere near as many as there could have been and should have been, largely because there is a gap, and a sizable one, between research and implementation.

What I'm going to do is sketch some principles, based on my experience and the experience of others that seem to predict the likelihood of research getting implemented, and illustrate them with some examples, some successes, and some failures.

The first and absolutely essential element that must be present if implementation is to take place is this:

**Principle 1.** The subject of the research must be clearly and directly related to the solution of an organizational problem perceived by the decision makers in the organization.

Simple, yes, and it has been said many times before. I don't care how elegant your theory is, how striking your research results, or how inexpensive they would be to implement. If those in the organization who make the decisions don't perceive a problem in the area that your research addresses, the chances of implementation are absolutely zero. Why should there be implementation if there is, in the eyes of the organization, no problem to solve. As the old saying goes "If it ain't broke, why fix it?"

In fact, in the eyes of the organization, there is no reason why there should even be research conducted in the first place in the absence of a perceived problem. Science for its own sake is simply not viewed by most organizational decision makers as being an acceptable reason for conducting behavioral science research in an organizational setting. Research has to be sold, a fact of life that makes some academic researchers quite uncomfortable.

When it comes to selling research, there are some very clear, but all too often ignored "How to" guidelines such as those spelled out in Making it happen: Designing research with implementation in mind (Hakel, Sorcher, Beer, & Moses, 1982):
1. Concisely describe in nontechnical terms the primary and secondary objectives of the project and the advantages of conducting it with the methodology you suggest.

2. Ask for and listen to management's reaction.

3. Explain how the project findings will benefit the sponsoring managers and their organization, and contrast these benefits with the consequences or implications of not completing the project (p. 105).

Selling research is much the same as selling anything else. First you qualify your prospect, then you establish a need for your product, then you deal with resistance, explaining what is in it for the buyer, and finally you close the deal. The same steps also apply to getting research results implemented: establish the need, then establish the benefit.

Assuming that the research does address a perceived problem, the next principle to consider is:

Principle 2. The solution provided by the research must cost less to implement than it costs to live with the problem.

Not all organizational problems are worth solving, and "costs" are social, psychological, and political, as well as financial. For example, let us say that your research clearly demonstrates that restructuring a training program will result in productivity improvement. The costs of the new training are the same as for the old. Yet, your revised training program does not get implemented. Why? There could be many reasons. Maybe the person who developed the current program has a psychological investment in it and is in a position to block change. Or the person who delivers the training is seen, rightly or wrongly, as unlikely to be effective in delivering the revised training. Perhaps the new training uses techniques that do not mesh well with the organizational culture. As a general rule, if you cannot figure out why your good, solid, problem solving research does not get implemented, it is a pretty good bet that a cost factor, one that you are totally unaware of, is behind the failure. The paper presented by the Sedelows at this conference (Sedelow, & Sedelow, this volume) provided an excellent illustration of this. Automation was not fully implemented in spite of a very high benefit/cost ratio. The manufacturer was looking down the road at future costs, e.g., if a computer built the cars, who could afford to buy them.

When implementation is considered, Kellas, Simpson, & Ferraro's (this volume) extremely interesting and potentially far reaching research on aging is very likely to run into the same problem. While it does cost organizations substantial amounts of money to entice people to retire and to replace the lost expertise, and while Kellas and his colleagues' work indicates that this is often an unnecessary expense, change in the near future is unlikely because of the demographics of the work force. Behind the relatively small generation approaching early retirement age are the baby boomers, millions of them, and the organizational cost of not providing fast-track opportunities for the high performers in the baby boom generation is viewed as higher than that of funding expensive early retirement programs. However, I predict that a generation from now, when the baby boomers themselves begin to near retirement age, findings such as those noted above will be "rediscovered" and used as a vehicle to justify incentives to keep people in the workplace longer, as the following generation of workers is substantially less numerous.