The 1988 Presidential Address
Contributions of Educational Research to Policy and Practice:
Constructing, Challenging, Changing Cognition

RICHARD J. SHAVELSON

To put it simply, our labors haven’t produced enough findings that Americans can use or even see the use of. . . . Education research has not fulfilled its role in the effort to improve our schools. . . .(1988, p. 5)

Chester E. Finn, Jr., Assistant Secretary of Education
Office of Educational Research and Improvement
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The Assistant Secretary echoes the sentiments of many policymakers and practitioners, and much of the public as well. Perhaps more telling, he reminds us of responsibilities to produce knowledge useful to policymakers and practitioners while still holding true to the tenets of our sciences with their strengths and limitations. And he taps uncertainties about our own research contributions. If it helps any, we are not alone. Feelings of uncertainty and frustration about the contributions of social science research are not uncommon (Goodwin, 1975; Shavelson, 1987; Weiss & Bucuvalas, 1980a). As one disillusioned sociologist put it, “only rarely have I witnessed serious governmental attention being given to serious social science research” (Wilson, 1978, p. 82).

In this talk, I look at disillusionment with research in a different way. First, I want to encourage some of us to bring our science to bear on the mind frames of practitioners and policymakers, in an attempt to better understand how education research might shape the policy debate or reconceptualize the nature of problems confronting practitioners.

Sum and Substance
To this end, I argue three points. First, the perception that educational research does not significantly contribute to practice is inaccurate. It grows out of a confusion of models, of science versus social science, a confusion, historically, of people outside our research community. For example, policymakers and practitioners who hold that education research should directly and immediately influence policy or practice, much in the same way that physics directly impacts on the calculation for a lunar orbital flight, are disappointed when their unrealistic expectations are not met.

Second, I argue from personal experience that education research does contribute to policy and practice. It does so by helping construct, by challenging, and by changing the way policymakers and practitioners view particular problems. From personal experience, I have seen how, for example, research on teaching has changed the way practitioners and policymakers view teaching, how research on military recruiting in community colleges has challenged policymakers’ notions about current recruiting efforts and their potential payoff, and how school and teacher effectiveness research has changed the way policymakers think about, collect, and interpret statistics (indicators) on the “health” of education.

Third, although education research influences how policymakers and practitioners think about a problem or represent an issue, and how they subsequently act, our impact is not frequent enough or as effective as it might be. To realize the potential contribution of research, it would behoove the education research community to better understand policymakers’ and practitioners’ cognition. An increasing number of us need to use our science to study and

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understand the action mind frames of policymakers and practitioners. A better understanding might enable us to more effectively translate research findings, or it might even lead us to changes in the way we typically design and conduct our research. To increase the contribution of research to policy and practice, then, we need to become fluent in more than just our research language.

Faulty Assumptions About Educational Research

Two faulty assumptions underlie most perceptions of the relation between educational research and policy and practice (cf. Weiss & Bucuvalas, 1980a). The first holds that education research should directly and immediately apply to a particular issue, problem, or decision (Shavelson & Berliner, 1988; Weiss & Bucuvalas, 1980a). The second faulty assumption holds that “just as the natural sciences lead to technology that makes society wealthy” (Weiss & Bucuvalas, 1980a, p. 13), education research can lead to practices that will make society happy, wise, and well educated (to paraphrase Weiss & Bucuvalas, 1980a, p. 13).

Direct and Immediate Application Assumption

The assumption that educational research should have direct and immediate application to policy or practice rests on many unrealistic conditions. Let’s consider them: Research would have to be relevant to a particular issue and available before a decision needed to be made; research would address the issue within the parameters of feasible action and provide clear, simple, and unambiguous results; research would be known and understood by the policymaker or practitioner and not cross entrenched interests; recommendations from research would be implemented within existing resources; and research findings would lead to choices different from those that decisionmakers would have otherwise made (Weiss & Bucuvalas, 1980a). The probability that any single study or series of studies could possibly meet some combination of these conditions must be quite close to zero.

This assumption is problematic for two reasons. It sets up expectations that virtually any piece of educational research would be unable to meet. And it ignores certain ways in which research has significantly influenced policy and practice. For example, research can alert policymakers and practitioners to problems, increase their commitment to working on a problem area, support a position held, legitimate decisions already made, or be used to persuade others about a position held. More on this later.

“Good News Outcomes” Assumption

The faulty “good news outcomes” assumption goes something like this (Figure 1): Education research should produce knowledge. Policy or practice based on knowledge is more “rational” than action based on experience, judgment, and intuition. Rational action by policymakers or practitioners will lead to “good” education outcomes. And the benefits of such outcomes will be uniformly and equally shared by all groups in society (Figure 2; Weiss & Bucuvalas, 1980b, p. 13). This faulty assumption is based on unreasonable and sometimes misleading expectations.

First, it promises too much. There are clearly methodological limits to what education research can do. For example, with the recent educational reform movement following on the heels of the 1983 commission reports came a call to monitor the reform and evaluate its progress. To this end, policymakers came to view education indicators as a
means of monitoring the status of education, providing a causal model of the effects of state-level policy on the nation’s achievement test scores, providing a means for evaluating the effects of particular programs, and holding school personnel accountable. At best, indicators can paint a broad picture of the condition of education and stimulate thinking about potentially effective policies. To promise more would be to build in disappointment (cf. Shavelson, 1988a).

Second, the assumption seems to rest on the expectation that research produces timeless truth. But much of what education research produces is, and probably will always be, time and context bound. As Cronbach (1975) pointed out, generalizations decay. “At one time a conclusion describes the existing situation well, at a later time it accounts for rather little variance, and ultimately, at a later time it accounts for rather little variance, and ultimately it is valid only as history” (pp. 122–123).

Third, the assumption implies that the scientific knowledge base should prescribe action. But this knowledge is inadequate, in and of itself, to prescribe policy or practice. As Schon (1987) pointed out:

In the terrain of professional practice, applied science and research-based technique occupy a critically important though limited territory, bounded on several sides by artist- ry. There [is] an art of problem framing, an art of implementation, and an art of improvisation—all necessary to mediate the use in practice of applied science and technique. (p. 13)

Schon and I agree that professional competence lies in the translation of scientific theories and “facts” into practical, goal-directed actions. Because the set of potential applications is indefinitely large—and therefore so is the set of “novel” situations encountered by the practitioner—practice must involve the art of translating scientific knowledge into actions applied to novel situations, even constructing new knowledge on the spot from experience when surprising situations arise (cf. Schon, 1983, 1987; Shavelson, 1973, 1976, 1984).

Fourth, this faulty assumption implies that the knowledge base is what policymakers and practitioners would act on, if available. Yet education research cannot depoliticize education decisionmaking (Shavelson, 1988a). One dramatic illustration is the issue of education vouchers. Education vouchers are like coupons that can be redeemed at the school of the holder’s choice. A decision, such as whether or not to implement a system of education vouchers, inevitably involves value judgments and political pressures, as well as a scientific knowledge base. Research suggests that for most people, especially for low-income and minority parents, education choice is based more on convenience than anything else (e.g., Bridge & Blackman, 1978; Darling-Hammond & Kirby, 1985; Snider, 1988). Regardless, federal policymakers, presidential candidates, blue-ribbon commissions, and some educators and citizens continue to hail education vouchers as the route to education reform.

Educational Research’s Contribution: Constructing, Challenging, Changing Cognitions

If education research does not directly and immediately lead to improvements in education policymaking and practice, where, then, does its value and influence lie? I believe that the contribution of educational research most often lies in constructing, challenging, and changing how policymakers and practitioners think. Research on effective schools changed the ways policymakers and practitioners envisioned school reform, although the implementation of the tenets of effective schools varies greatly in specific instances. Research on cooperative learning changed the ways some teachers organize their classes, although teachers do so in a variety of ways that might surprise researchers. And item-response theory (e.g., Lord, 1980) has changed the way the government monitors national achievement in the subject matters, even though its strong assumptions limit the array of assessment techniques that might be used to collect achievement indicators (Shavelson, 1988b). To better understand some of the ways research contributes to policymakers’ and practitioners’ cognitions, I reflect on how some of my research has influenced the way policymakers carry out educational activities and policymakers formulate policy options.

Constructing Cognition

Sometimes research generates a new way of conceiving some central component of education. For example, in the late 1960s and early 1970s, I was at the mecca of research on teaching: Stanford University. Nate Gage, in his seminal chapter in the 1963 Handbook of Research on Teaching, had just ushered in a “paradigm shift.” He moved the field away from a trait conception of teaching and its quest to identify the traits that make up a “good” teacher, to a behavioral conception with a research agenda to determine the basic or technical skills of teaching. Fred McDonald and Dwight Allen created “microteaching” to see if they could train teachers in these skills. Eventually researchers sought a link between teaching skills and student achievement, inaugurating the era of process-product research, the outcome of which has had a profound influence on education policy and practice today.

Process-product research on teaching is an excellent example of how research constructed a new conception. Replacing the trait conception that teachers are born with certain traits that produce student achievement, this behavioral conception has significantly shaped how policymakers and practitioners think about teaching, teacher education, and teacher evaluation. It portrayed teachers as technicians who could be trained, using behavioral science methods, to acquire the basic skills of teaching and apply them to their trade.

But my story doesn’t end here. I was a misfit at Mecca. I couldn’t see how studying the relation between discrete teaching actions (such as question-asking) and student achievement would improve teaching. Moreover, I couldn’t see how training teachers to perform those discrete actions would improve teaching either. Rather, we needed to help teachers formulate some notion of when, more than how, to act; when, for example, to ask questions and when not to. It seemed to me that teachers’ decisions about what to do, and when and how to do it, constituted the basis of skilled teaching in a time- and context-bound classroom (Shavelson, 1973, 1976). Because teachers meet up with a multitude of unique situations for which they couldn’t possibly be trained—and in which asking more questions would not “work”—I preferred to conceive of teaching as the exercise of professional decisionmaking that is, deciding how to adapt their substantive and pedagogical knowledge to...
We found, however, that the assumption that the armed services had not made a concerted effort to recruit in the community colleges was incorrect. Not only had they tried, but they had tried and largely failed!

Our research, then, challenged and subsequently changed the policy question from one of whether the military should recruit in an untapped market, to one of how might the military recruit more effectively than it had in the community college market.

To address this question, we used data from the Census Bureau and the Department of Defense—controlling for regional economics, propensity to enlist, and the like—and identified recruiting "hot spots" in the country: areas in the continental United States in which recruiters had been unusually successful in recruiting men with college backgrounds. We then pinpointed the specific recruiters who accounted for a lion’s share of the college enlistees. Based on extensive interviews with these recruiters, we were able to formulate recommendations for improving recruiting in the community colleges. Indeed, the Army modeled much of its community college recruiting program on our recommendations.

Challenging Cognition
In addition to constructing cognition, sometimes we challenge the way policymakers or practitioners conceive a problem. One problem that Gus Haggstrom, Tom Blaschke, John Winkler, and I confronted was providing by military policymakers: Because of the projected decrease in the number of 18-year-olds by 1990, the military would have to recruit almost 20% of its volunteer force from male high school graduates (Shavelson, Haggstrom, & Blaschke, 1984; Shavelson, Haggstrom, & Winkler, 1983). This heightened demand for graduates would take place in a strong economy. Consequently, the reasoning went, the military, higher education, and employers would be in competition for fewer and fewer high school graduates. With increased competition, the possibility of recruiting shortfalls was on policymakers’ minds. New recruiting markets had to be sought.

One new market, according to some policymakers, was the nation’s system of community colleges. Weren’t community college students of somewhat higher quality than high school dropouts or graduates entering the civilian labor force? Weren’t they higher in mental ability with fewer run-ins with the law? Weren’t these the students who were uncertain about their educational and occupational aspirations (cf. Baird, 1971; Clark, 1960; Pincus, 1980)? Might not the military provide an attractive alternative to some community college students? Might not the community colleges be the untapped market most likely to provide insurance for possible recruiting shortfalls?

Our research addressed these concepts. Using data from the National Center for Education Statistics, the Census Bureau, and the Defense Department, we were able to confirm the policymakers’ expectations about the quality of community college students. And we confirmed the findings in the literature about the uncertainty of community college students’ aspirations and educational choices (e.g., Cohen & Brawer, 1982).

We immediately recognized that the Commission’s conception of monitoring was too narrow. The Commission had focused on achievement and participation, but to effectively monitor science education reform, something more comprehensive was required, namely, a system of indicators that could relate outcomes such as achievement and participation rates to inputs and processes. As one piece of the research, then, we constructed a generic model of an education system, a model that portrayed the major components of the system, and the logical and/or empirically verified links among them (e.g., Figure 3). Without an extensive research literature, we wouldn’t have made it to first base.

With the generic model built, we turned, once again, to educational research in order to identify the best set of statistical indicators to reflect each component in the model and the relations among components. One outcome of this part of the research was a sourcebook on education indicators (Shavelson, McDonnell, & Oakes, in press). In draft form, it has been used by the National Science Foundation to provide and interpret education indicators (e.g., Indicators 1985), by the Alexander/James Commission on the redesign of the National Assessment of Education Progress, by the Center for Education Statistics in the redesign of education indicators, and by the Council of Chief State School Officers’ Center for Education Indicators.

A second piece of the research included interviews and discussions with policymakers and practitioners to determine their information needs. Should the indicator system describe the condition of mathematics and...
science education? Provide an accountability mechanism? Provide for education improvement? Should the system focus at the national level? State level? Local level? Who are the primary audiences for this information? How often should the Foundation monitor?

We quickly learned that the expectations NSF and congressional policymakers had far exceeded anything an indicator system could deliver. We literally came face to face with the Good News Outcomes assumption. They variously expected an indicator system to: (a) monitor the state of education, (b) causally model the effects of state-level policy on the nation's achievement, (c) evaluate program effects, and (d) provide an accountability tool. In our recommendations to NSF, we stressed that, at best, indicators could paint a broad picture of the condition of education and stimulate thinking about potentially effective policies (cf. Shavelson, McDonnell, & Oakes, in press; Shavelson et al., 1987).

A third piece of the research identified and costed out alternative versions of an indicator system based on the generic education model and on our interviews (Table 1). The alternatives ranged from 'don't do anything you aren't already doing' ('status quo') to conducting regularly scheduled policy studies ('cyclical studies') at a price ranging from $500,000 to $1 million per year, depending on the topic, to a full-blown monitoring system ('independent') ranging in cost from about $23,000,000 to $35,000,000 per year, depending on whether the Cadillac, the BMW, or the Rolls Royce was desired. We also identified options in between, including a patchwork of currently available statistics, and what we called a 'piggyback,' in which the NSF would piggyback data collection on NAEP. Though I suspect the Foundation began with the price tag and worked backward to its favorite option, the study formed the basis of the Foundation's education indicator budgets for FY87 and FY88.

The study, I believe, changed how policymakers at NSF thought about education indicators in several ways. First, the study shifted their thinking about monitoring mathematics and science education from a time series with single indicators such as achievement trends to a system of indicators that could be linked together. By linking achievement to student demographics and prior achievement, for example, test score changes could be brought to bear on the Foundation's goal of improving the mathematics and science education of all students.

Second, the study changed cognition by expanding the range of options considered. These new options included putting together a patchwork of currently available indicators in combination with the development of new indicators through analyses of currently available data sets (e.g., High School and Beyond, Census Bureau, NAEP, NSF teacher survey); sponsoring policy studies of, for example, mathematics and science achievement, teacher sup-

* Indicates expansion of existing NAEP items.
** Indicates addition of new NAEP items.


FIGURE 3
One Possible Indicator System: Piggyback on Ongoing Data Collection Such as NAEP

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ply and demand, and curriculum reform; piggybacking on NAEP to collect indicators of mathematics and science education, more frequently and in much greater detail than is currently possible; and implementing a complete indicator system of its own. NSF policymakers came to conceive alternatives they had not heretofore envisioned.

**Contributions of Educational Research: Differences in Frames of Mind**

I believe that, in these three examples, the contribution of research to policy and practice was to construct, challenge, or change the way policymakers and practitioners think about their work. Now I would like to characterize the mind frames of policymakers and practitioners, mind frames that reflect their respective work cultures. Before I do so, however, I reflect, more personally, on our culturally shaped mind frames—researchers’ mind frames.

**Researchers’ Mind Frames**

I admit that very little of my research has influenced policymakers or practitioners. My reference group is researchers. I strive to conduct research that warrants their attention, and if I do well, I am rewarded by my peers. To put it simply, we share a common frame of mind or schema for doing and reporting empirical education research. We frame problems in ways consistent with the latest research and use approved methods. We are cautious in reporting and interpreting findings, findings that usually bear on the theory that originally motivated the inquiry. We may, somewhat gratuitously, suggest possible applications, but are hesitant to do so.

The canons of disciplinary inquiry, however, reinforce narrow science. When I do policy work, I am forced to take greater risks, to use a wider variety of research methods, and to make bigger inferential leaps from research literature than my disciplinary training would encourage. But someone must take these risks in order to bring research to bear on the information needs of policymakers and practitioners, and that responsibility should be borne by some of us in the research community, the same community that created the scientific knowledge base itself. In this way, the accumulation of disciplinary research becomes the foundation on which policy and practice research builds and contributes to policy and practice.

Nevertheless, I also firmly believe that much of education research’s potential contributions go wanting because, try as we might, our mind frame does not easily translate into the policymaker’s or practitioner’s. What we have, then, is a mismatch of mind frames. That is, the canons of science dictate, at least implicitly, how problems are posed, how we go about solving them, and how we report our solutions. The pressures on policymakers give rise to a different mind frame, an action schema if you will, that takes the scientific knowledge base as only one datum in a complex decision process. The practitioner has still a different mind frame, albeit just as action-oriented as the policymaker’s. Some of us need to study these differences in mind frames so that, with a better understanding, we can improve communications.

As a small first step in understanding these different mind frames, I would like to characterize in an abstract, normative sense, ignoring individual differences, research-bureaucrats’ and teachers’ mind frames. My intent is to make concrete the assertion that researchers, policymakers, and practitioners have different frames of mind that restrict the potential utility of education research.

**Research-Bureaucrats’ Mind Frames**

Research bureaucrats are people who work in agencies, usually government agencies, and are responsible for commissioning research, overseeing research, translating research into information useful to policymakers, or some combination of these. They can be found in federal agencies such as the U.S. Department of Education and the National Science Foundation; in state agencies such as a department of education or governor’s office; or in a local school district’s office of assessment and evaluation. If research is to have an impact on policy, these are probably the people who would most likely be aware of research and find it useful in their jobs.

Carol Weiss studied the mind frames of 155 federal, state, and local level research bureaucrats in agencies responsible for policy on mental health, alcoholism, and drug abuse. Participants read research studies in their fields of expertise. Then they rated the studies as to their technical quality, objectivity, and applicability to policy concerns. They were also asked how likely they would be to take the study’s results into account if the study addressed a current issue.

Weiss found that the bureaucrats invoked five criteria in judging the usefulness of a study. Research was judged to be useful to the extent that it: (a) was high in technical quality, (b) recommended actions that policymakers could do something about, (c) fit with the bureaucrat’s prior knowledge, (d) challenged accepted truth, and (e) was relevant to an issue.

Of the five criteria, research quality was the most important. If research serves to challenge expectations or set a course of action, quality is essential either to mobilize support for a policy

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**TABLE 1**

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<tr>
<th>Education Indicator System Options and Costs for National Science Foundation</th>
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<tr>
<td><strong>Option</strong></td>
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<tr>
<td>Maintain status quo</td>
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<tr>
<td>Use patchwork of available statistics</td>
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<tr>
<td>Conduct cyclical policy studies</td>
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<tr>
<td>&quot;Piggyback&quot; data collection on NAEP</td>
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<tr>
<td>Use independent monitoring system</td>
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or to change the ways policymakers think about problems.

Next, Weiss predicted bureaucrats’ ratings of the likelihood that their agencies would take a particular piece of research into account from ratings on these five criteria and combinations of criteria (cross-products of ratings). Each of the five criteria contributed to the prediction of the use ratings.

More important, Weiss discovered what she termed truth and utility tests. Bureaucrats’ evaluations of research are not simply based on an additive combination of quality, fit with prior knowledge, and so on. In addition, their evaluation of the applicability of research to a particular issue works in a compensatory way. The truth test balances technical quality with prior knowledge. As technical quality increases, bureaucrats put less weight on the consistency of the study’s outcomes with their prior knowledge, and vice versa.

To mobilize support for a position, or to change the ways policymakers think about issues, the truth test plays a particularly important role: “Is the research trustworthy? Can I rely on it? Will it hold up under attack?” (Weiss & Bucuvalas, 1980b, p. 311). The utility test balances support for current agency policies with challenge to the status quo. This test asks, “Does the research provide direction to the agency, either for current practice or for considering alternative approaches to the problem?” Both provide recommendations for action, but in opposite directions! Research consistent with current policy validates an agency’s agenda and can be used to justify policy recommendations and budget requests. Research that challenges current operating assumptions serves the valuable role of forcing the agency to reflect on its program, perhaps even jarring it out of a period of stagnation.

**Practitioners’ Mind Frames**

Teachers, like policymakers, are action oriented (e.g., Gifford & Gabelko, 1987). Their mind frames are goal driven. They seek information that fits with familiar teaching situations. They seek recommendations for actions that experience tells them will most likely help them realize their goals (cf. Shavelson, 1984, 1986; see also Clark & Peterson, 1986).

Fenstermacher (1986) and Green (1976) argued that teaching practice might be thought of as following the form of an Aristotelian practical argument that leads to action. A practical argument contains at least one instance of each of three types of premises: a situational premise, a value premise, and an empirical premise. A situational premise reflects the current state of affairs as perceived by the practitioner. A value premise either states a goal or may be used to justify a choice of teaching methods. An empirical premise links situational and value premises to actions; it is an if-then or condition-action statement that applies to the situation. Empirical premises, then, constitute goal-oriented rules of thumb that teachers use to make decisions in planning and in carrying out instruction.

The Appendix provides an example of how the notion of a practical argument might be applied in the analysis of teaching. The first premise, a value premise, sets a goal: students’ learning to read. The fifth premise, a situational premise, establishes that the teacher’s students are nonreaders. Premises 2 and 4, empirical premises, link nonreaders to the teaching technique of primers with choral reading and random calling on individual students. The remaining value premise (3), empirically testable, is used to decide on whether to use whole-group, small-group, or individual instruction. Together these premises lead to the observed teaching action.

Research benefits the practice of teaching, according to Fenstermacher (1978, 1986), when it improves the premises (practical arguments) in the minds of teachers. Research does so by providing the knowledge base needed to transform teachers’ experience and value-based premises from being subjectively reasonable to being objectively reasonable by having withstood empirical tests. Put another way, research benefits practice when it establishes links between conditions and actions in teaching, and teachers ground their practice on objectively reasonable premises (cf. Fenstermacher, 1986; Shavelson, Webb, McArthur, & Stasz, in press). The production of knowledge that informs the premises on which teachers base action is a tall task for research (cf. Gifford & Gabelko, 1987). Especially challenging is the translation of findings into terms that bear on teachers’ subjective beliefs about condition-action premises. To provide this translation, we need, first and foremost, to understand teachers’ action mind frames. Some mind frames are captured by the philosophers’ notion of practical argument, or by cognitive scientists’ notion of production functions where, when a set of conditions are met, a particular action in an “intelligent program” is executed. Moreover, when conditions recur often and in a rough temporal order in characteristic surroundings, the sequence of teachers’ actions is often played out in the form of well-sequenced scripts (Shavelson, 1984, 1986). As Morine-Dershimer (1987, in press) and I (Shavelson, 1986) have attempted to demonstrate, however, translating research findings into premises and condition-action statements requires an inferential leap. Consequently, research does not and cannot prescribe the if-then action premise; there are simply too many situational premises that set boundaries on condition-action premises. At best, research provides evidence that may confirm, construct, challenge, or change teachers’ mind frames.

**Concluding Comments**

Before drawing the implications of studying action mind frames, I want to make clear that education research can be justified as legitimate inquiry in its own right. We do not have to prove its worth on the basis of improving educational practice (cf. Fenstermacher, 1986, p. 44). We justifiably pursue, for example, the fundamental nature of intelligence, as well as its implications for instruction and testing. Indeed, most of education research bears on theory or a particular line of empirical inquiry, as it should. I see no reason for us to rush out to be relevant! Not only might we fail miserably, we would be caught up in a whirlwind of topical issues.

When we claim, however, that research contributes to policy or practice, the criterion for evaluating this claim changes. Our findings or recommendations should fit within and inform the mind frames of policymakers or practitioners.

At the point where research meets policy and practice, we begin to recognize that institutions and their cultures shape the way researchers, policymakers, and practitioners formulate problems, acquire and recall information pertinent to problems, and act. At a global level, researchers’ problem formulations are usually theory- or dis-
Researchers use information to confirm discipline-driven, whereas policymakers’ and practitioners’ action-driven. Researchers use information to confirm or disconfirm theory, and policymakers and practitioners use information to guide actions with direct and indirect consequences for students and the public.

To be sure, opportunities abound for individual differences. Nevertheless, the mismatch of researchers’, policymakers’, and practitioners’ culturally shaped mind frames probably outweighs the ‘noise’ created by individual variation within those cultures.

Mind frames, then, are abstract mental information structures. These culturally shaped structures summarize or categorize information about many particular cases and interrelate these information categories. Mind frames create expectations for research-produced information. When research findings are consistent with theory, prior knowledge or experience, understanding results. Information that does not fit expectations may be denied, distorted, or ignored. Or, if the technical quality of the research-produced information is impeccable, it may lead to a new way of perceiving the world.

These broad, institutionally shaped mind frames operate in everyday problems. Through careful study of policymakers and practitioners engaged in everyday problem solving, we can begin to unravel the multitude of specific mind frames that researchers, policymakers, and practitioners apply. It behooves more of us to strike out to understand the action mind frames of policymakers and practitioners. A few of us will use the concepts and methods of our science, as Carol Weiss has done, to study policymakers’ and practitioners’ mind frames. More of us should seek a less formal understanding of action mind frames through transactions with policymakers and practitioners. Reflecting on my policy research, I realize that it grew out of an attempt to understand the perceptions of and demands on policymakers. However, I was self-consciously careful to maintain an independent point of view, not buying into untested, hidden assumptions. The indicator research, for example, attempted to take the perspective of the NSF in considering options for monitoring mathematics and science education. We interviewed a number of influential actors within the Foundation, within Congress, and within the Reagan administration in an attempt to understand their information needs and perceptions of NSF’s role in collecting precocle indicators. Inevitably, an increased understanding of action mind frames led to modifications in the way we did the following: posed our research questions; formulated research designs for specific parts of the study; applied, adopted, or created research methods (e.g., statistical, cost accounting, interview) to address these questions; interpreted our findings; and formulated recommendations to the NSF.

By tackling policymakers’ and practitioners’ mind frames, we might better formulate research questions, design studies, and translate our findings into the stuff that constructs, challenges, and changes policymakers’ and practitioners’ cognition. In doing so we may require the common misperceptions held by many of those outside our research community.

APPENDIX

Application of the Practical Argument to the Analysis of Teaching

(1) It is extremely important for children to know how to read. [VALUE PREMISE]
(2) Children who do not know how to read are best begun with primers. [EMPIRICAL PREMISE]
(3) All nonreaders will proceed through the primers at the same rate (the importance of learning to read justifies this standardization). [VALUE PREMISE]
(4) The skills of reading are most likely to be mastered by choral reading of the primers, combined with random calling on individual students. [EMPIRICAL PREMISE]
(5) This is a group of nonreaders for whom I am the designated teacher. [SITUATIONAL PREMISE]

ACTION: (I am distributing primers and preparing the class to respond in unison to me.)


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Shavelson, R. J. (1986, June). Interactive decisionmaking: Some thoughts on teacher cognition. Invited address, 1 Congresso Interna-

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