The Connection Between Research and Practice

MARY M. KENNEDY

This article reviews four hypotheses that have been put forward to account for a perceived lack of connection between research and practice: (a) research needs to be more authoritative, (b) research needs to be more relevant, (c) research needs to be more accessible, and (d) the education system itself is inherently too stable or too unstable and therefore unable to respond coherently to research findings. A brief history of thought within each of these hypotheses is offered, and the place of education researchers in the larger education context is discussed.

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In the past few decades, members of AERA seem to have become increasingly skeptical about the potential for research to improve practice. My "baseline" for this observation is roughly the late 1960s and early 1970s, when both researchers and research funding agencies were relatively optimistic about the role of research in improving education: National labs and research centers were legislated in 1963, federal funding for educational research rose some 2000% during the 1960s (Bloom, 1966), and a new National Institute of Education was formed in 1972. In 1978, Patrick Suppes published a book celebrating the impact of educational research (Suppes, 1978). Throughout this period, members of AERA seemed convinced that research would soon produce universal truths about teaching and learning that teachers would, could, and should implement. In fact, along with federal funding for research came federal funding for dissemination as well to ensure that teachers learned about the latest findings from research.

Now, if I can borrow an old saw from G. K. Chesterton, the historian, it is tempting to say that the main thing we have learned from educational research is that we have not learned much from educational research. Witness the article Carl Kaestle published in 1993 titled "The Awful Reputation of Educational Research."

But this trend toward pessimism extends well back from my baseline of the 1960s and 1970s. There was another period of intense optimism at the beginning of the century, documented by Geraldine Clifford (1973) in the Second Handbook of Research on Teaching. That wave of optimism was also accompanied by a stream of disappointments—Clifford quoting Thorndike as expecting his research to reach practice in some 30-50 years, William Bagley in 1934 expressing disappointment at how little influence research had had, W. W. Charters worrying in 1948 about the lack of influence research had had on practice, Stephen Corey in 1954, and Julian Stanley in 1957. Viewed from that vantage point, Carl Kaestle's 1993 article is simply extending a century-long tradition.

One difference between the more recent and the earlier phases of optimism and pessimism is that this time we have studied the problem more systematically. During its heyday in the 1970s, the National Institute of Education created an entire division devoted to dissemination efforts, and that division sponsored a great deal of research on research use. Researchers studied the dissemination and use of research as a topic in its own right and also studied the implementation of innovations derived from research.

From these efforts, we now have a virtual catalogue of reasons for this perceived lack of usefulness of educational research. The reasons hypothesized for the apparent failure of research to influence teaching can be grouped into four general hypotheses: (a) The research itself is not sufficiently persuasive or authoritative; the quality of educational studies has not been high enough to provide compelling, unambiguous, or authoritative results to practitioners. (b) The research has not been relevant to practice. It has not been sufficiently practical, it has not addressed teachers' questions, nor has it adequately acknowledged their constraints. (c) Ideas from research have not been accessible to teachers. Findings have not been expressed in ways that are comprehensible to teachers. (d) The education system itself is intractable and unable to change, or it is conversely inherently unstable, overly susceptible to fads, and consequently unable to engage in systematic change. Either of these characteristics—excessive instability and excessive stability—render it incapable of responding reliably to educational research.

The Persuasiveness and Authority of Research

Much of the discussion about the merits of research from the 1940s through the 1960s was focused on research design. The goal was to isolate variables and define their relationships with other variables. Causal relationships were especially important, and researchers designed studies in the hope that they could control for all possible rival interpretations. The need for control was one of the main reasons researchers wanted to conduct their research in the laboratory. Design considerations were so central to the thinking of educational researchers that a chapter on research design that appeared in the first Handbook of Research on Teaching was reprinted as an independent monograph and became required reading for graduate students for at least a decade. This was, of course, Campbell and Stanley's (1963/1969) "Experimental and Quasi-Experimental Designs in Research on Teaching." Campbell and Stanley brought to our attention the notion of rival hypotheses and listed several of them that we needed to control for. Their admonitions made comparison groups important and made random assignment to groups one of

Mary M. Kennedy is a professor at Michigan State University, 116F Erickson Hall, East Lansing, MI 48824. Her specialties are teacher learning, research use, and education policy.
the central criteria for sound research design. Early on in their article, they define their premise as follows:

This chapter is committed to the experiment: as the only means for settling disputes regarding educational practice, as the only way of verifying educational improvements, and as the only way of establishing a cumulative tradition in which improvements can be introduced without the danger of a faddish discard of old wisdom in favor of inferior novelties. (Campbell & Stanley, 1963, pp. 2)

It wasn't just designs that preoccupied researchers. We argued about the use of covariates, the reliability of change scores, the appropriate unit of analysis in nested school designs, ways of teasing out interaction effects, and every other conceivable methodological issue you could imagine.

The problem with this line of thinking was that it discouraged research on complex approaches to teaching, for such studies could not be mounted with sufficient control over external influences on learning. In fact, it limited researchers' abilities even to think about such teaching. Consider, for instance, an educational reform called discovery learning that was popular in the 1960s. Like the current reform movement, that one aimed to foster more self-reliant learners and a greater command over the fundamental ideas in the various academic disciplines. In a critique of the research evidence supporting discovery learning, Lee Cronbach (1966) said we needed more persuasive evidence of its effectiveness. Yet even as she described what would count as good research, he conceded that "if all my recommendations were followed, research would become impossibly elaborate" (Cronbach, 1966, p. 77).

In fact, researchers who studied educational phenomena in the field rarely did manage to control for all the extraneous variables. Each major study of that period became mired in methodological debates over whether the inferences its authors made were justified. From Rosenthal and Jacobson's *Pygmalion in the Classroom* (1968) to Coleman's *Equality of Educational Opportunity* (Coleman, 1966) to *Findings From the Follow Through Planned Variation Study* (Kennedy, 1978), methodologists quibbled over confounding variables, measurement problems, methodological biases, and competing interpretations of the data. While such arguments are a necessary part of the process of developing knowledge, they also have side effects that may only become apparent later. In this case, these debates had several side effects. First, they became so esoteric that only the most sophisticated methodologists could follow the debates. Less-qualified researchers and practitioners couldn't possibly track the nuances of reasons for why findings should or should not be believed. Second, although the debaters believed that more carefully designed studies would settle these issues once and for all, the main thing their audiences learned from the debates was that the issues were hopelessly complicated and might never be resolved. Finally, the debates diverted attention from other educational issues. I recall sitting on a proposal review panel for the National Institute of Education in which the strongest proposal from a design point of view was an offer to conduct further analyses of Coleman's EEO data. But a practitioner on our review panel strongly objected to funding that project, saying she was tired of seeing all the research money being spent on increasingly esoteric arguments over one study when there were so many other pressing problems facing the schools that were not being addressed.

Most of the arguments about design began to exhaust themselves by the late 1970s without ever producing the ideal, definitive research designs that had been hoped for. Even Lee Cronbach threw in the towel in 1975, when he conceded not only that it was impossible to design studies that could take into account all the relevant variables, but that even if we succeeded at that, the findings from social research would not accumulate over time but instead would "decay" because the social contexts were constantly changing (Cronbach, 1975). We will never, he concluded, be able to generate stable findings that can provide the basis for theories about social phenomena, including the phenomenon of teaching and learning.

But we still argue about design, still worry about how to know if our designs are yielding reliable knowledge. Contemporary discussions about design have taken a different turn, however, as Ann Brown's (1992) article on "Design Experiments" illustrates. Brown grants that we cannot eliminate all possible rival hypotheses and argues that our goal should be to accommodate these other variables rather than control them. She argues that, if research is to produce important knowledge, it has to occur within the natural constraints of real classrooms and must accommodate as best it can the multiple confounding influences that are there.

The Relevance of Research

The second line of thinking about research and practice focuses on the kinds of problems teachers actually have and the kinds of problems researchers are trying to solve. Concerns about relevance frequently motivate the U.S. Department of Education's Office of Educational Research and Improvement to require research grantees to involve teachers in the design of their work, on the assumption that such involvement will force researchers to attend more to teachers' questions and concerns. They have also led to recommendations for collaborative research (Bennett & Desforges, 1985; Huberman, 1989).

Concerns about relevance developed partly in response to the heavy emphasis on authority that permeated the 1960s. Shulman, for instance, argued strongly in 1970 that research needed to move out of the laboratory and into the classroom because the precision gained in the laboratory was purchased at the expense of classroom relevance. At that time, laboratory studies of learning focused mainly on individual learning, whereas teachers worked with groups of learners. Laboratory studies focused mainly on learning nonsense syllables, while teachers were concerned about teaching subject matter. In the intervening decades, educational research has indeed moved to the field. Yet it is still not uncommon for teachers to dismiss research because the classrooms involved in the research differ from their own classrooms—different socioeconomic groups, for instance, or different cultural settings.

Though we may not have solved the relevance dilemma, we have certainly progressed toward understanding it. Indeed, in an article published in the Second Handbook of Research on Teaching, Dan Lortie (1973) mentioned what he called an "odd gap" in the literature on teaching: There was no research on teachers' beliefs and teachers' conceptions of their work. Lortie found it odd that this gap
existed, but I think it is safe to say that the gap has been filled in the intervening 25 years.

Moreover, the research that has been done in the past two decades has actually been quite fruitful for researchers, if not for teachers, in that it has given us vivid descriptions of classroom life, shown us how teachers manage that life, and shown us how teachers think about their practice. Very few naturalistic studies of schools and teaching were done in the first half of the century, but the last two decades have been filled with such studies.

One of the earliest of these studies was Jackson’s (1968) Life in Classrooms. Jackson claimed that classroom life was characterized by crowds, praise, and power. The fact that students are always grouped with 20 or 30 others means that they must wait in line, wait to be called on, wait for help, and tolerate interruptions and disruptions in their work. That’s the crowd part. Teachers control most actions and events and decide what the group will do and how much deviation from the plan will be tolerated. That’s the power part. Teachers also give and withhold praise and do so publicly so that students always know which students are favored or not favored by the teacher. That is both praise and power.

Another early and important study was Dan Lortie’s (1975) Schoolteacher, which didn’t involve observing classrooms, but instead focused on teachers and what they thought about their work. One of many important concepts that Lortie introduced was that of uncertainty. The presence of 20 to 30 children in a single classroom means there are 20 to 30 possibilities for an interruption in one’s plans. Even apart from these routine disturbances, though, students may get into fights with one another, get sick, or simply ask a question that is difficult to answer. All of these actions, from major to minor, are disruptions to the scenarios that teachers have planned and make it difficult for teachers to predict with any certainty how a lesson will proceed, how long it will take, whether students will find it engaging, or whether they will “get it.”

Nor can teachers often say with certainty at the close of a lesson whether it went well or whether students “got it.” A teacher may think that students were engaged, attentive, and learning on one day, only to discover the next day that they don’t recall much at all from the day before. These experiences accumulate to give teachers the feeling that learning is a mysterious process, one that they don’t understand and certainly don’t control (Huberman, 1983).

A third important insight into teaching and teachers comes from several studies of student adaptations to school life. Students, for the most part, would rather be elsewhere and tend, therefore, to be either disengaged or overtly defiant of teachers’ efforts, so that teachers often find themselves negotiating and bargaining with students who, for their part, strive to simplify their tasks, postpone the deadlines for assignments, minimize their workload, and maximize the grades they are given (Sedlak, Wheeler, Pullin, & Cusick, 1986).

This portrait of classroom life puts teachers in quite a different world from that of the researchers I described earlier. When these two communities are characterized in this way, it is hard to imagine research being helpful to teachers. It is too pristine, too fussy, too concerned with justification.

On the other hand, both researchers and teachers aim to increase certainty. That should give them something in common. Perhaps the problem, then, has more to do with how each group tries to cope with uncertainty. For researchers, it is a matter of improving study designs, checking and verifying, and replicating. Certainty comes about through intellectual processes. For teachers, certainty is often achieved by creating predictability within the classroom. Many teachers respond to classroom life by defining clear and unambiguous classroom routines. That way, students know what to expect from the teacher and the teacher knows what to expect from the students. Keeping students busy also is a way of avoiding confrontation and reducing the possibilities for disruption. Both teachers and students prefer routine, familiar tasks to more novel and open-ended tasks. Routines increase predictability and decrease anxiety for both teachers and students.

But Doyle (1986) showed us that an unfortunate result of routines is that everyone’s attention gets diverted from the substantive meaning of academic work to procedures, routines, schedules, and products. The need to manage the group focuses teachers’ attention on getting tasks done rather than on the quality of the work that is done (Carter & Doyle, 1987), and the need to assign grades focuses teachers’ attention on product standards and completion deadlines rather than on the intellectual content or merits of the work. As teachers and students focus on these tasks, meaning gets lost (Doyle, 1986). Academic content is transformed from issues to lists (McNeil, 1988). Rules and routines give teachers a way to respond to student bargaining, a set of criteria for grading student work, a way to increase predictability, and a way of ascertaining whether the class as a whole has “progressed.”

These insights into classroom life help us understand teachers’ situations much better than we ever did in the past and give us some suspicions for why research might not be perceived as particularly relevant to teachers. Whatever knowledge research may provide, it can rarely guide teachers toward concrete strategies or routines that can accommodate all of the constraints they are trying to manage. There is quite a bit of research, for instance, that has contributed to the current reform movement advocating conceptual or constructivist approaches to teaching, and much of it suggests that the approaches to teaching advocated by reformers might actually benefit students more than traditional teaching strategies do. But how can such research persuade teachers to do things differently when the strategies they currently use solve so many problems and satisfy so many constraints?

In fact, research on teacher thinking also led to a new version of the relevance hypothesis. In the 1960s, the relevance hypothesis usually meant that researchers were not addressing the questions that teachers had about their practice or the research was not done in classroom contexts that represented most teachers’ classrooms. The contemporary version of the relevance hypothesis is that research findings need to be in an epistemological form that better matches the realities of classroom life. In 1983, for instance, Bolster said

I believe the fundamental requirement of any inquiry which hopes to be consonant with the teachers’ perspective on teaching is that it must view human behavior as reflexive . . . . Significant knowledge of any social situation, therefore, consists of an awareness of the emerging meanings that participants are developing and the specific ways that these meanings are functioning to shape
their endeavors and thus the characteristics of the situation itself. (Bolster, 1983, pp. 303)

Since then, numerous researchers have advocated case studies, ethnographies, and, more recently, narratives (Carter, 1993; Connelly & Clandinin, 1990) as forms of research that might better approximate classroom reality and, therefore, be more relevant to teachers. So the current version of the relevance hypothesis has extended the concept of focusing on teachers’ questions to incorporate the idea of actually viewing the classroom as teachers view it.

The Accessibility of Research

The third hypothesis accounting for the apparent lack of connection between research and practice focuses on teachers’ lack of access to research findings. This has always been Congress’s favorite hypothesis, and it has led to sponsorship of regional labs, ERIC clearinghouses, a National Diffusion Network, and many other endeavors whose essential purpose is to somehow carry research findings from the ivory tower into the classroom.

At the same time that these efforts were being expanded, though, researchers were studying their influence on teachers. Their work has led to new understandings about what makes research accessible to teachers and how teachers use research that is accessible. In the 1960s, when people talked about an “impact” from research, they meant that teachers were applying specific techniques that had been found by researchers to be effective. But studies of research use showed that research was more likely to be used conceptually than instrumentally. That is, practitioners did not take from research tools that could be directly applied in their classrooms, but instead took ideas: concepts that could, especially when combined with other ideas and with their own experiences, help them understand their situations or help them invent specific responses to local situations (Kennedy, 1983). Even when teachers were trying to implement specific classroom innovations, we discovered that they did not adopt innovations, but instead adapted them (Berman & McLaughlin, 1975, 1978).

A second finding from this line of work was that teachers’ own prior beliefs and values were important influences on their practice (Kagan, 1992), not just in the direct sense of dictating what was important to accomplish, but also in the indirect sense of influencing how receptive teachers were to ideas they might encounter from research or from colleagues (Hollingsworth, 1989). The presence of such prior beliefs and values creates a dilemma for researchers—and for reformers, more generally—for it suggests that changing practice cannot occur simply by informing teachers. If teachers are sympathetic with a reform agenda, they will be receptive to its research findings, but if they are not at least sympathetic, they will probably not be persuaded by the research no matter how authoritative or relevant it is.

The centrality of beliefs and values has also become apparent outside of teaching, in other contexts where researchers have examined the stability and malleability of beliefs. Certain types of beliefs are found to be much more resistant to change, and they tend to be the beliefs teachers rely on (Pajares, 1992; Rokeach, 1968). Beliefs that are most resistant to change are

- Those that are formed during childhood. Most teachers’ beliefs are formed while they themselves are students in school observing their teachers and envisioning the kind of teachers they themselves would be.
- Those that are closely associated with our identities. Teachers’ beliefs about teaching are important to their definitions of themselves as teachers.
- Those that are part of interlocking networks of beliefs. Teachers’ beliefs about how students learn are associated with their beliefs about the appropriate role for teachers to play in classrooms, the nature of school subjects, and so on.

This third line of reasoning suggests that the problem of accessibility is not merely one of placing research knowledge within physical reach of teachers, but rather one of placing research knowledge within the conceptual reach of teachers, for if research encouraged teachers to reconsider their prior assumptions, it might ultimately pave the way for change. It also suggests that persuasiveness may require more than simply strong research design and that relevance may require more than a similar context or a relevant question. Instead—or, perhaps, in addition—the potential for research to contribute to practice depends on its ability to influence teachers’ thinking.

The Stability (and Instability) of the Education System

Each of the hypotheses I described above—the persuasiveness and authority of educational research, the relevance of educational research, and the accessibility of educational research—represent an attempt to explain why research has not had more influence on practice and, concomitantly, to offer suggestions for changes in the character of research that would increase its value to teachers. In so doing, all of these arguments assume that research could influence practice if we improved our methods, our questions, our dissemination, or some other feature of our work. The fourth hypothesis for the apparent lack of connection between research and practice suggests that the problem lies not in research but in the education system itself. Many features of education mitigate against the potential for research, some because they stabilize the system and some because they destabilize it.

One important contributing factor here in the United States is our decentralized governance. Education governance in the United States is more decentralized than governance in almost any other country in the world. We have no centralized curriculum, no centralized examination system, no centralized textbooks, no centralized standards for teachers, and no centralized curriculum for teacher education. Some of these matters are determined by states; many are determined locally. Even when states define policies in these areas, they tend to define them loosely. And even when defined locally, our local school systems are not very tightly coupled (Weick, 1976), and teachers have a fair degree of autonomy in their daily practice.

Another contributing factor is that the population we serve is remarkably heterogeneous, perhaps more so than any other country in the world. Not only are we racially and ethnically diverse, but families have come here from numerous other countries and have brought their own customs and values with them. Such diversity increases the
difficulty of creating a coherent curriculum that can adequately accommodate our many interests. As early as 1938, Willard Waller observed that our cultural heterogeneity constituted a conservative influence on the curriculum, for the points where agreement could be reached among various groups tended to be mundane points.

There are also arguments that our system is inherently unstable. Meyer (1983), for instance, argued that decentralized decisionmaking and community control of schools make our system especially susceptible to fads as local teachers, parents, and other education constituents search for new ways to improve their education. These fads come and go so rapidly that it is rarely possible to generate a real knowledge base about any of them. Consequently, innovations are justified by exaggerated claims, theoretical virtues, and anecdotes. Proposals for change are justified as moral imperatives rather than as proven ideas, and persuasion occurs through publicity rather than through reasoned argument. His observation is not new: Woodring (1964) evaluated the reforms that were popular in the early sixties—the use of teaching machines, for instance, and the use of team teaching—in search of some underlying psychological principles and concluded that they mainly derived from social and political pressures rather than from clearly stated psychological principles. Similarly, Callahan (1962) concluded after examining the history of education administration that American schools are extremely vulnerable to public pressures and that this vulnerability is built into our pattern of local support and local control. Larry Cuban’s (1990) take on the problem is that the combination of decentralized decision-making and multiple constituencies requires schools to simultaneously hold multiple and conflicting goals, many of which are not very well defined. At the same time, the loose coupling of local school districts protects teachers from the various waves of reform and counter-reform that circle their classrooms.

The lack of coherent direction is particularly apparent in our textbooks. Both the Second International Mathematics Study (McKnight et al., 1989) and the Third International Mathematics and Sciences Study (Schmidt et al., 1996) found American textbooks to be more fragmented and superficial than texts in most other countries. They were longer, covered more topics, and devoted more space to review and repetition, so that individual topics were repeated often but were treated with little depth. Similar conclusions have been drawn from examinations of mathematics curricula at other grade levels (e.g., Porter, 1989) and from examinations of texts in other subjects (Brophy, 1990; Gagnon, 1987). Tyson-Bernstein and Woodward (1986) concluded from their review that textbook publishers, in an effort to please everyone, try to include as many topics as possible rather than taking the time to develop a few central ideas.

Even the contemporary reform is not coherent. Some of us think the reform is aimed at clearing up confusions by aligning texts, tests, teacher preparation, and standards so that the system as a whole is unified. Some of us think the reform is aimed at changing the character of teaching and learning toward more constructivistic approaches, authentic learning tasks, or “bigger” ideas so that students are more intellectually engaged and academic content is more rigorous and more conceptual. Still others think the reform is aimed at decentralizing the system even more by introducing charter schools and/or vouchers and giving parents more power, through market systems, over their children’s education. Like past reforms, none of these ideas has a sufficient knowledge base to justify massive expenditures, and, like past reforms, these ideas will probably fade away before such a knowledge base is developed.

So we have a system that can be characterized by a lack of agreed-on goals, a lack of shared guiding principles, no central authority to settle disputes, decentralized decision-making, a continual stream of new fads and fancies, limited evidence to support or refute any particular idea, textbooks that manage the conflicts by including all possible ideas and giving no serious attention to any of them, and reforms that are running at cross-purposes to each other.

Ironically, at the same time we decentralize our educational decisions more than most other countries, and permit numerous fads and conflicting ideas to constantly compete, we also give our teachers less time than most other countries do to formulate their curricula and to develop their daily lessons. According to OECD’s Centre for Educational Research and Innovation (1995), teachers in the United States spend more hours per year directly teaching students than teachers in other nations do. The extensive amount of time they spend in class leaves them painfully little time to think about any aspect of their teaching (Fullan, 1994). It should not be surprising, then, to learn that many teachers do not see any underlying order to classroom events (Huberman, 1983), for they haven’t the time to examine their experiences to find underlying order.

These two views of the education system—as large, cumbersome, and unchanging on one hand and as disorganized and driven by fads on the other—are not incompatible. Most of our fads and fancies are more observable in rhetoric than they are in practice, as Cuban’s (1984) history has shown us, and those that do influence practice tend to alter its more superficial features rather than altering the fundamental character of teaching and learning (Applebee, 1991; Cohen, 1990; Peterson, 1990). Teachers may adopt new devices such as math manipulatives, for instance, and not adopt the conceptual underpinnings that justify these devices.

Not only are these two portraits of our system compatible; they are related. The multiplicity and ambiguity of goals that press against local educators, combined with their lack of time to actually think through any new ideas that present themselves, severely limit the capacity of all actors in the system to think hard about their practices and to pursue a steady course in any one new direction. These pressures encourage a kind of defensive rigidity in which practitioners protect themselves from substantial changes by making slight adjustments at the margins, by claiming they have already made the changes that reformers propose, or by claiming that research justifies their existing practices.

But the problem of multiple, conflicting, and ill-defined goals is not unique to the United States and does not derive solely from our unique system of governance. Indeed, most other countries have far more centralized systems than we have, but do not have any more evidence of research use. Many of them are susceptible to fads—often emigrating from the United States—and all are susceptible
to multiple and conflicting goals for their education systems and to shifting policy climates and public sentiments as perceptions of national strengths and weaknesses change over time.

All are also susceptible to certain universal sources of stability. One such source, first noticed by Dan Lortie (1975), is that teachers learn their practice through an extended apprenticeship of observation. Unlike practitioners in virtually all other professions, teachers observe practitioners for 13 years before they even begin their formal preparation for their work. Many of their deepest beliefs about teaching and learning derive from this apprenticeship of observation.

Lortie's observation was reinforced by Cuban's (1984) history of teaching, a study aimed at trying to understand why some features of the system change while others don't. Though there have been numerous changes in our education system over time—including the expansion of the system to include all children, the gradually raising of education requirements for teachers, the introduction of graded classrooms, and so forth—none of these changes touches the most basic feature of the system—the character of interactions between teachers and students within classrooms. Cuban concluded that teaching practices have remained relatively unchanged in part because of structural constraints on practice and in part because of the stability of teachers' beliefs about what should occur in their classrooms.

Cohen (1988), too, suggests that it may be wrong-headed to expect substantial change in teaching practices. He suggests that the stability of teaching practices derives from the nature of teaching itself. Teaching is analogous to psychotherapy or social work in that its aim is to improve other human beings—a task for which clear strategies remain elusive and in which practitioners depend on clients' motivation and ability for their success. Cohen argues that the uncertainty of the work interacts with the practitioners' dependency on clients. For instance, if teachers attempt more difficult goals, they automatically decrease the certainty that they will succeed and increase their dependence on their students' abilities and motivations. They become more vulnerable. Lortie (1975) also found that the uncertainties of teaching encourage more conservative practices, and March (1991) also argued that the exploration of new ideas introduces more risk of failure than does the exploitation of existing ideas. When trying new ideas, returns are less certain and more remote in time. When exploiting existing ideas, on the other hand, returns seem more certain, quick, and predictable.

Add to the long apprenticeship of observation and the ambiguity of teaching itself a multiplicity of educational goals—and, in fact, an increasing proliferation of goals as more and different cultural groups press for acknowledgment of their own values—and the enterprise becomes too complex to manage unless some limitations are self-imposed. These self-imposed limitations contribute to the stability of the enterprise and foster a reluctance to seriously entertain the many laudable ideas that reformers and researchers so frequently ask for.

**Educational Research As a Part of the Education System**

Now here's the rub: These inherent characteristics of education also limit the possibilities for researchers to produce a stable or coherent body of knowledge that could be useful to practitioners. The constant conflict over goals and directions for education spills over into research agendas. Educational research, or funding for education research, has been characterized as fragmented, unstable, and subject to repeated shifts in foci (Chall, 1967; Dersheimer, 1976; Dersheimer & Iannaccone, 1973) in part because of disputes over what the terrain consists of and who is in charge (Lagemann, 1996). The central federal agency for educational research, the U.S. Department of Education's Office of Educational Research and Improvement, has been found on more than one occasion to be lacking both focus and continuity (Atkinson & Jackson, 1992; Bick & Jackson, 1994), and federal funding for educational research has been constantly threatened since the federal-funding heyday of the 1960s. Disputes among researchers, combined with political and public disputes about education itself, make it difficult for the sponsors of educational research to forge and sustain focused research agendas.

Particularly telling is the overwhelming attention we give to arguments about how we should conduct our work. We sometimes seem to agree more about how to acquire new knowledge than about what knowledge we have already managed to acquire, in part because we disagree about what counts as new knowledge. We argue about research methodology, and we argue about the role we should play in our interactions with education practitioners. In the fifties and sixties, we argued about basic versus applied research; today we argue about quantitative and qualitative research. We envision roles for ourselves that range from the committed action researcher originally advocated by Kurt Lewin (Lewin, 1946, 1948) to the dispassionate social experimenter advocated by Donald Campbell (1969, 1973). Some of us even argue that research has had too great an influence and is responsible for some the problems that now ail schools (Coleman, 1975; Richardson-Koehler, 1987). These arguments about methods and roles are not unrelated to more broad arguments about the goals of education, for different approaches to research often entail different assumptions about the nature of educational practice and about how research can or should contribute to it.

It might be tempting to think that we would make more progress if we concentrated on conceptual contributions to practice rather than on discrete innovations, perhaps giving people new ways to think about old problems or perhaps focusing on ideas that are large enough to encompass many aspects of practice (Anderson & Burns, 1990; Fenstermacher, 1982; Shavelson, 1988). Historically, though, we have tended to shift our central concepts almost as often as we shift our attention to specific practices. We have embraced behaviorism, task analysis, cognitive development, and, most recently, social constructivism, each in the hope of finding a single guiding metaphor that captures the essence of teaching and learning. But as theoretical ideas gain popularity, they also lose their precise meaning and consequently lose their explanatory power (Cronbach & Suppes, 1969). Before educational ideas have time to be systematically developed and refined, their critics become so numerous that the ideas are replaced by other ideas.

Viewing research as a part of a larger system that contains multiple, competing, and often ill-defined goals, the connection between research and practice is not one in
which research influences practice, as many researchers might hope, nor one in which practice influences research, as many might hope, but rather one in which both research and practice are influenced by, and are perhaps even victims of, the same shifting social and political context.

Discussion

Each of the hypotheses described in this article rests on assumptions about what the relationship should be between research and practice. Much of the work invested in developing persuasive research designs, for instance, was premised on the notion that research should provide generalizable statements because such generalizations would yield what Kliebard (1993) calls “rules of action.” Early versions of the relevance hypothesis and the accessibility hypothesis also assumed this role, and researchers thought that if only research addressed teachers’ questions and if only teachers knew about the findings, they would indeed use some of the generalizations that had been found.

But later versions of these two hypotheses were based on different assumptions. As the relevance hypothesis extended from studying teachers’ questions to viewing the classroom in the way teachers view it and as the accessibility hypothesis moved from physical accessibility to conceptual accessibility, these two hypotheses seemed to rest on an assumption that research should provide new and better understanding of the dynamics of teaching and learning, new perspectives rather than new rules of action (Shavelson, 1988). This view of the contribution of research leaves teachers with considerable professional judgment as to how they might draw on these insights to make their moment-to-moment decisions. Lampert (1985), for instance, argued that we need to view teachers not as technical production managers, but instead as dilemma managers who routinely balance among competing goals as they design their courses of action. I suspect most researchers now acknowledge this view of teachers and aim to provide guidance and insights more than rules of action.

The accessibility hypothesis, however, introduces a new dilemma for us, for research that is conceptually accessible to teachers may be research that does not challenge assumptions or introduce new possibilities. Chinn and Brewer (1993) showed the many ways in which all of us—from scientists to children—can reject research findings that are incongruous with our prior beliefs. If that is the case, then, conceptually accessible research could be research that further reinforces the stability of the education enterprise rather than research that challenges assumptions or offers new insights. To the extent that research that introduces new ideas is inherently less conceptually accessible to teachers, then we researchers also become dilemma managers.

The fourth hypothesis, which identifies the ambiguity and conflict inherent in the education enterprise as the culprit for the apparently modest role of research, follows naturally from the third, for as researchers have adopted the goals of providing new insights and of challenging old ideas, they have discovered the intransigence of prior beliefs, the frequent popularity of untested fads, and the frequent lack of receptivity to tested ideas. All of these phenomena lead us to realize that the ideas that are conceptually accessible to policymakers or teachers are not necessarily the ideas that have been most carefully developed or argued.

This fourth hypothesis, then, suggests that researchers have no particular authoritative advantage in the public arena. It views our role as on par with that of reformers and other education advocates. We become simply another group of players in the continuing debate about education. We use different methods and often adopt a posture of neutrality, but our influence in the ongoing debate sometimes seems to depend more on our advocacy than on our evidence.

But the fourth hypothesis also suggests that researchers are susceptible to the same varying waves of fads and reforms as teachers are. If this is so, then our own shifting paradigms and research agendas reflect social moods and public sentiments as much as new developments in theory or new empirical findings.

Conclusion

The disillusionment many of us feel, and that many of our audiences feel, probably stems from false expectations. Our audiences continue to believe that research should provide reliable and relevant rules for action, rules that can be put to immediate use (Shavelson, 1988). Many of us still seek such results. But many others are beginning to search for other contributions.

Is this progress? Despite our numerous changes in paradigms, research interests, and assumptions about how we contribute to practice and despite our debates over method, we have learned a tremendous amount about our enterprise in the past several decades. We now have a much deeper understanding both about what we are capable of doing and about how we can contribute to practice. We also are more aware that we cannot avoid the political processes, though as Donmoyer (1995) pointed out, we are still uncertain of our political position.

This improved understanding of the system in which we work and the system we aim to assist leads to new and different research questions and new and different research designs. And to the extent that our work reflects more adequately the ambivalent and ambiguous character of education, it may become more persuasive and more relevant, and perhaps as it does, it may also become more conceptually accessible.

Note

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Dr. David Hough, Editor
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Associate Dean, College of Education
Southwest Missouri State University
Springfield, MO 65804-0095
417-836-5286 FAX 417-836-4884
E-mail: dah315f@vma.smsu.edu

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