Musings

As scholars and teachers in the field of education, we face two constituencies. On the one hand, our research and scholarship is embedded in specific disciplines, representing such fields as rhetoric, linguistics, literature, psychology, anthropology, or communications. On the other hand, we share a common interest in issues and problems of teaching; we expect our research to speak to practice.

There is usually some tension between these twin constituencies—at its best, a creative tension that leads to new questions and better methods of teaching. Sometimes, however, one or the other of our constituencies gets out of balance, demanding more than its share of attention. Of late, the balance has been shifting toward the practical side of what we do—reflected in demands that research be directly relevant to practice, and in the growing interest in action research and the teacher as researcher.

Complex Patterns

While the reasons behind this shift are laudable, I want to argue that they are also short-sighted in the belief that research in education should or can lead to rapid, large-scale changes in practice. The implicit model is drawn from areas like medicine, where the development of a new drug or a new surgical procedure can transform medical practice immediately. But research in education is a different sort of enterprise, one where there are many differing conceptions of what is worth knowing, and where solutions to problems of teaching must be interposed onto complex patterns of human interaction.

To put the case baldly, I don't believe that any of our research in education can be justified in terms of direct and immediate consequences for practice. Take, for example, direct studies of alternative ways of organizing our classrooms and our teaching, from which we can make recommendations about which approaches work and which do not. Such studies have traditionally made up a large proportion of research in education, and the practical implications at first glance seem obvious. A closer examination of major issues within the field, however, should quickly dispel any too-glib assertion that such studies have given us (or will give us) solutions to problems of pedagogy. Think for a moment of the long history of research on formal grammar instruction, on the role of phonics in beginning reading, on class size, or more recently over the effectiveness of alternative versions of Head Start—and of the interpretations and
reinterpretations that have been offered in each case. Had we acted solely on the basis of the initial studies, our recommendations would be much different (and less productive) than those we would make today, after time and thought, and after a series of follow-up studies. The long process of interpretation and debate is necessary—a reflection of a healthy exploration of what particular research results mean in the complex context of classrooms that differ in student populations, in goals, and in conditions of instruction.

Other research in education seeks to sharpen our understanding of individual learners and the cultures in which they live. Such research usually looks at educational issues through another prism—its research questions and data gathering approaches are rooted in the disciplines framing the research we do. Traditionally such research has been based in psychology (hence the domination of Ed Psych in many schools of education), but history, philosophy, linguistics, sociology, anthropology, English, and many other disciplines have their place as well. The practical applications of such discipline-based research are usually less obvious, and rightfully so: the demand that we find immediate practical applications in such studies can distort both research and practice. But by helping us understand complex processes related to learning and schooling, such studies can make (and have made) very powerful contributions to our approaches to instruction.

Research in Writing Processes

We can take as an example recent research into writing processes. These studies began as studies of individual writers, not as studies of instruction. Most have sought to understand basic psychological or linguistic processes. Yet they quickly provided a rationale for putting more emphasis on the writing process and less on the written product. In effect, studies of good and poor writers were used to define what poor writers were missing, and these missing skills and strategies then became the basis of process-oriented curricula. (These curricula were concerned with fostering such things as planning and revision, strategies that poor writers seemed to be missing, and with deemphasizing editing and proofreading, which poor writers seemed to do excessively in comparison with better writers.)

The resulting shift in emphasis has been all to the good, but the solutions that were offered as practical applications of that research have been neither universally adopted nor uniformly successful. But this is not because of problems in the research, but because we are still in the process of understanding its practical implications. We have only recently begun to ask questions about how procedural skills might best be learned, for example, or of what the developmental course of those skills might look like even in good writers. In the end, the implementation of process-oriented curricula has been more difficult than the initial extrapolations from the research had suggested—but without the original, discipline-based research we never would have begun to ask these more directly pedagogical questions.

Thus even in this classic case of research affecting practice, those effects came about not because research was targeted at questions of what to teach, but through extrapolations from the research to classroom contexts. Such extrapolations are a necessary part of the overall process of improving teaching, but they are also matters of context and judgment. What makes sense in one classroom for one teacher may not make sense in another classroom with a different group of students. Research that can illuminate the problems of education comes from many different starting points and represents many different disciplinary orientations, some seemingly far from the classroom. To limit our research to studies that have immediate practical relevance would be to eliminate some of the most educationally useful research that we do.

Relevance to Practice

Rather than a precondition for research, questions about relevance to practice should be a continuing agenda for the profession as a whole. To treat them otherwise is both to treat them superficially and to give researchers a more central role than they deserve in the shaping of teaching practice. As researchers, we are often too close to our own studies to see the practical implications clearly. Our studies usually fit within our own larger professional agendas—they are designed to clarify and reinforce our theories. As such, they may also mean more to us than they mean to others. The problem is analogous to the difficulty that most of us have in editing our own writing: we know so well what we meant to say that it is hard to notice when we have really said something different.

Another problem in drawing practical implications from our research stems from the distance of most researchers from the contexts in which our recommendations will be implemented. The focus of the researchers' interests and attention is usually and properly on the questions and issues generated by our research agendas, not on the day to day problems of classroom teaching. This should lead us to be cautious in the conclusions we draw about what others should do.

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In an ideal world, the relationship between research and teaching would be a symbiotic one. The issues of practice would help shape what counts as interesting and important, honing researchers' sensibilities as they select problems to work on. The findings of research would give practitioners new questions to ask and new ways to look at their own approaches to teaching. Each would need and respect the other, because each would bring expertise to bear on the problems of education.

In the real world, we have blurred these differing expertises by forcing researchers to become imperfect prescribers of teaching practice (and teachers to become imperfect researchers--but that is a musing for another day).

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