Comments on Bulterman-Bos

The Dysfunctional Pursuit of Relevance in Education Research

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Responding to Bulterman-Bos (2008), the author argues that the effort to make education research more relevant is counterproductive. Teachers and researchers have different orientations toward education that arise from different institutional settings, occupational constraints, daily work demands, and professional incentives. These are not dysfunctional differences to be resolved by creating the proposed composite role of the clinical researcher but useful alternative perspectives, with each providing what the other is lacking. Relevance is a tricky quality to define because it is easier to recognize in retrospect than in prospect, and efforts to make research more relevant can make it useless or misleading. Scholarly work that neither arises from a quest for relevance nor demonstrates any particular utility at the time it is carried out may turn out to be highly useful at a later time and in a different place.

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In the title of her article, Jacquelien A. Bulterman-Bos (2008) asks, "Will a Clinical Approach Make Education Research More Relevant for Practice?" (this issue of *Educational Researcher*, 412–420), and by the end she comes to the answer, "Yes." Overall this is an engaging effort to sort out the nature of education research and to understand its relationship to the practice of teaching. During the course of this discussion, the author draws on my analysis of the transition that teachers undergo when they enter doctoral programs on the road to becoming researchers—a shift in worldview from the normative to the analytical, from the personal to the intellectual, from the particular to the universal, and from the experiential to the theoretical (Labaree, 2003). She argues that this depiction of the differences between teaching and doing research is useful only to a point, because it is grounded in a discredited Cartesian conception of the split between body and mind. Drawing on Michael Polanyi, she argues that knowledge is not full bodied and useful unless it remains connected to the real world of education, where knowledge necessarily retains within it elements that are normative, personal, particularistic, and experiential. Therefore, she asserts, education research needs to be grounded in teaching practice if it is going to be able to represent the context of practice effectively. This means that education researchers need to spend years as teachers before becoming researchers, and they need to remain active as classroom teachers during their time doing research. In her view such a clinical approach to education research will produce a form of knowledge about education that is both more valid as a representation of education and more useful to practitioners.

Bulterman-Bos raises the kinds of issues about the aims and meanings of research that scholars in any field should be discussing, and such a discussion is particularly pertinent in a professional field like education, where policy makers, practitioners, funding agencies, and citizens routinely ask how research can help improve the profession. I welcome her effort to clarify the issues surrounding the relevance of education research, and I appreciate the opportunity to join in this discussion; but I find that I profoundly disagree with her analysis. My disagreement operates at three levels of abstraction, starting with a quibble about her interpretation of my own earlier argument about the differences in how teachers and researchers view the educational enterprise and then moving on to much more basic concerns about the nature of scholarly research and the meaning of relevance.

First, although her article in general represents the argument in my earlier article accurately, it does introduce a subtle distortion of my account of the teacher–researcher split. In my article, I acknowledge that, by laying out the polar differences in the orientations of teacher and researcher so starkly, I risk overstating the difference between the two modes of work. I note that researchers in their own practice also demonstrate elements of the normative, personal, particularistic, and experiential; likewise, teachers show elements of the contrasting orientations. The difference, I argue, is not the function of a mind–body dichotomy but a matter of emphasis, the result of a division of educational labor structured by the institutional settings, occupational constraints, daily work demands, and professional incentives of each realm of practice. Teachers are engaged primarily in a practice of social improvement, grounded in personal relationships with particular students embedded in time and place, and the professional knowledge they build is largely an accumulation of clinical experiences. Education researchers are engaged primarily in a practice of social analysis, grounded in intellectual conceptions of education generalized across contexts, and the professional knowledge they build is largely a web of theories. The different conditions of work lead to different modes of professional practice and different ways of thinking about education.

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This leads to a second broader point, that the way to deal with these differences in orientation is not to combine the two in a single role—clinical researcher—that perfectly balances these elements, but to acknowledge and honor the different zones of expertise and to promote a fruitful dialogue between practitioners in the two zones. It seems neither practical nor necessary for all researchers to split their time between school teaching and education research in order to establish the power and credibility of the research knowledge they produce. A differential allocation of functions between teachers and researchers seems fruitful for both professions, as long as the barrier is relatively low and the conversation across the barrier is ongoing. Universities are not eager to pay scholars to teach school, and school districts are not eager to pay teachers to do research, so it is hard to see how such a hybrid occupational role can become institutionalized. And the skills involved in being an expert researcher or teacher are so strikingly different that it would be difficult for individuals to achieve mastery in both at the same time. Each requires immersion in a particular institutional setting, fluency in a distinctive professional culture, and full engagement in a complex set of professional practices.

According to Bulterman-Bos, these differences in perspective between researchers and teachers are highly dysfunctional, leading to invalid research and ineffective teaching; but I see the differences as carrying great potential value. To teachers—immersed in a web of pedagogical goals, social contexts, and instructional relationships—research can offer a way to gain perspective on their realm of practice, holding up a theoretical mirror that allows them to see what is unique and what is commonplace in their classroom setting. To researchers—afloat in the intellectualized and decontextualized realm of educational theory—the classroom can offer a way to gain grounding in the personal and particular world of teaching and learning in schools, providing professional problems to spur theory development and providing clinical settings for testing theory. Both stand to gain from the interaction, because each side provides what the other is lacking. The answer, I suggest, is not to resolve the tension by merging the two roles but to take advantage of the tension by using it to enrich both modes of professional practice.

I want to advance this argument another notch by making a third, more fundamental point: It is counterproductive to press education research—or, for that matter, any other form of research—to be relevant. One problem is that relevance is a tricky quality to define because it is easier to recognize in retrospect than in prospect. A related problem is that earnest efforts to make research more relevant can paradoxically make it useless or even harmful, by focusing on short-term results that are narrowly measured instead of on consequences with a longer horizon and broader scope.

But to argue against the press for relevance is not to say that education research should be irrelevant. Research in general draws inspiration from real-world problems, and this is particularly true in professional schools, where scholars feel the need to study issues that arise from problems of practice in their professional arenas. In his book exploring the issue of research relevance, Donald Stokes (1997) calls this sector of research activity "Pasteur's quadrant." He categorizes research according to the degree that its aim is to pursue fundamental understanding or to respond to pressing problems, and by this metric he locates Niels Bohr in the first category (pure basic research), Thomas Edison in the second (pure applied research), and Louis Pasteur in both (use-inspired basic research). I argue that scholarly research justifies itself primarily by its contribution to theory, sometimes inspired by immediate social needs (like Pasteur) and sometimes not (like Bohr), and this applies to a professional field like education as much as to a scholarly discipline. If we as education researchers fail to contribute to theory with our research, then we are less scholars than engineers or product developers. Theory development frequently leads to product development, as the relationship between universities and the technology industry has demonstrated so dramatically in the past half century, but the distinctive value of scholarly research dissipates quickly when it segues from being use-inspired to use-driven. And this is what happens with the press for relevance.

Mie Augier and James G. March (2007) have written a persuasive account of how the drive to make research relevant can render it less so. Their focus is on the research produced and education provided by business schools, but their arguments apply with equal validity to the field of education. In both domains, there is a strong professional constituency asking professional schools to prove their usefulness by solving problems and serving the needs of practitioners. The problem is that this urge to be useful often turns counterproductive. Augier and March identify two key factors that undermine the relevance of research trying too hard to be relevant: ambiguity and myopia.

Ambiguity comes from the difficulty in trying to define what constitutes relevance for research. Presumably, education research is relevant if it has some kind of clear connection to issues and problems in the field of educational practice, particularly if it promises to be useful to practitioners who are trying to deal with these issues and resolve these problems. But this begs the question, useful to whom and for what? Think of the wide array of actors involved in education: teachers, students, principals, and parents; test makers, textbook publishers, and educational technology producers; curriculum developers, superintendents, and school board members; policy makers, politicians, and educational bureaucrats; teacher educators and education school deans. What might make research useful for some of these might at the same time make it useless for others. Relevance is in the eye of the beholder. In addition, research may be useful for helping to accomplish some educational ends but not others. Studies that seem relevant to people who are trying to raise test scores may not be at all relevant to those who are trying to improve critical thinking, enhance civic commitment, increase skills in mathematical analysis, promote gender equity, reduce the racial achievement gap, increase graduation rates, or enhance human capital production. These studies may not even seem helpful to people trying to raise student scores on other tests. In fact, what is helpful in attaining one goal may be harmful in attaining another, which, for example, is the argument made by proponents of critical thinking about the effect of efforts to raise test scores. Almost any bit of education research is likely to be seen as more or less useful for some actor concerned about some education-related goal, while simultaneously being seen as useless or harmful by most actors for most purposes. Under these circumstances, it is trivial to call research relevant or irrelevant, because it all depends on the peculiar mix of actors and goals.

But the problem of the inherent ambiguity of relevant research feeds into the more fundamental problem of its genetic predisposition toward myopia. Relevance is a function not only of person and purpose but also of place and time. As Bulterman-Bos and I have both argued, teaching and learning in schools is highly particularized and contextualized. This makes the relevance of research hard to establish across contexts. Studies that may be seen as useful in teaching and learning for one student—or one subject matter or ethnic group or classroom or grade level or school or school district or state or country—may not be seen as useful in other settings. Likewise, what makes research useful at one point in time may make it useless or misleading at another. The knowledge may be so time sensitive that its usefulness expires quickly. And knowledge that is helpful in meeting an educational goal in the present (say, to improve engagement in a science lesson) may undermine a goal whose accomplishment cannot be measured until decades later (say, to improve science understanding in the workforce).

Augier and March (2007) call this tendency *myopia* in order to call attention to a potentially pathological consequence of the effort to make research relevant: It may lead to educational knowledge that is short-sighted. When education researchers seek to make their work relevant, they feel the need to tailor their work to the demands of educational practice in the present time and the local place (or the location of the intended client–consumer). The problem is that this work, even if it is helpful in that particular context, is not likely to be useful in the conditions of educational practice that exist a few miles away or a few months in the future. Conversely, research that seems like an abstract exercise in theory building at one time and place may become highly relevant for practical purposes that were unforeseeable at the point when the work was done. Bohr's work on quantum mechanics is a good case in point.

In this sense, then, applied research may grow stale quickly, whereas basic research may age well. Scholarly work that neither arises from a quest for relevance nor demonstrates any particular utility at the time it is carried out may turn out to be highly useful at a later time and in a different place. Therefore, not only is research relevance hard to define, but the active pursuit of it may produce educational knowledge that is irrelevant.

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