



Comments on Bulterman-Bos

Education Research as a Distributed Activity Across Universities

Ellen Condliffe Lagemann

In response to Bulterman-Bos (2008), this article discusses three kinds of research needed in education: problem-finding research, which helps frame good research questions; problem-solving research, which helps illuminate educational problems; and translational work, which transforms the findings of research into tools that practitioners and policy makers need. Clinical research is most important as a form of problem-finding study. Although it is best carried on in “ed schools,” other kinds of education research are best done in other faculties. For this reason, education research should be a distributed activity, encouraged across all the faculties of research universities.

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If education is a complex, multifaceted social phenomenon that takes place in a variety of institutions and settings, it should go without saying that education needs to be studied in many different ways. Sadly, however, the history of education research could easily be presented as a quest for “the one best way.” As I have suggested in *An Elusive Science: The Troubling History of Education Research* (Lagemann, 2000), I believe this view has been a source for a good deal of mischief. It has made for many battles between the advocates and detractors of this or that approach, and it has not yielded the wide and deep knowledge we need to support the wisest policies for education and the most effective practices. With that in mind, I would like to sketch a spectrum of research, the range of studies that I believe we need in education, and then place Dr. Bulterman-Bos’s discussion of clinical research within that. I will also argue that in developing the range of studies that are needed in education, education research should be viewed as a distributed activity in which many, if not all, the faculties of a university engage.

Problem-Finding, Problem-Solving, and Translational Research

All research begins with questions, and good research begins with good questions. Those may come from personal experience; observation; debate with colleagues; reaction to a new idea, article, or

book—pretty much anything. Once one identifies something one wants to learn about, one may turn to what has already been learned about that topic by conducting a literature search. The search may satisfy one’s curiosity or disclose a vacuum and with that an opportunity for further research. Alternatively, one may turn to whatever it is that stirred one’s interest in the first place and attempt to learn more through observation, experiment, or discussion. Either way, one is engaging in problem-finding research.

Problem-finding research is too little appreciated in education. But it is essential, if one is to ask significant questions. What is or is not a significant question differs across fields. In education, I believe that significant questions are ones that pertain to purposive growth or changes in the self. I once described education as a “process of interaction by which individual potential (instincts, propensities, talents) is activated, shaped, or channeled and a change (an observable or consciously felt difference) thereby produced in the self” (Lagemann, 1979, p. 6). “Is Jorge reading?” is not an educational question. But “Is Jorge learning to read?” is an educational question because it focuses not simply on the act of reading but on the process by which the capacity to read is acquired. Educational questions may take causative form: “Is Jorge learning to read because he has an effective teacher?” They may be comparative: “Is Jorge learning to read more easily than other children in his class?” Or one might want to know how one knows whether Jorge is learning to read, a question that focuses on assessments of education. If so, then one asks, “Is Jorge learning to read, and how would one know whether that is occurring?” Regardless of what one wants to know about education, significant questions in education are ones that focus on some aspect of intentional growth.

Problem-finding research may occur anywhere and may take a great variety of forms: observation and description, case study, portraits of institutions or learners, anecdotes, or even stories. It may be formal or informal. It may be a part of what a teacher does in the classroom or the first step taken by a researcher. Its purpose may be to advance scholarship or to inform one’s own practice. It may be scientific or not.

In addition to problem-finding research, we, of course, need problem-solving research. Problem-finding research is no less important than problem-solving research, but it is different. Problem solving has to do with analysis and explanation. It seeks to evaluate the power and significance of different variables. It

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attempts to illuminate the causes for and the consequences of the kind of purposive change that makes education different from, say, physical growth and development, which are more inevitable and inherent than education. It also attempts to explain why education does not occur in situations in which one might expect to see purposive growth, or why different people respond differently to similar potential sources of growth. Of course, it is something of a misnomer to speak of “problem-solving” research. Problems in education are rarely really solved, although they may be investigated and ameliorated. However that may be, in my view, it is in connection with problem solving that disciplinary research comes in.

As I understand them, disciplines provide systematic ways of thinking and investigating. Historians approach all problems with chronology in mind because their discipline teaches them always to think about change over time. Sociologists approach problems looking for patterns between and among institutions or groups of people because the discipline of sociology pertains to social groupings and patterns of organization. Economists think in terms of markets and incentives, always trying to identify and gauge how rational actors would seek to maximize their returns.

If one is a historian, sociologist, or economist studying educational phenomena, I believe one is more likely to be able to uncover causative relationships than if one is trained apart from a discipline as an education researcher. That is because one’s discipline will guide one’s search for evidence and make that search consistent with the ways in which one scrutinizes evidence and the kinds of inferences one believes one may validly draw. Disciplines shape how one frames a problem; they guide the ways in which one makes inferences; and they align one’s approach to evidence, methods, and findings. If one is attempting to understand relationships among variables, being sure that one’s approach is systematic, aligned, and coherent is extremely important. This helps ensure the validity of one’s findings or results. To portray a classroom, one may rely on a rich array of adjectives, or tell a story, or refer to a poem. But to discover whether teachers who possess a master’s degree are more likely to help their students learn to read than teachers who possess a bachelor’s degree, one would do better to rely on tested rules for evaluating evidence, relating data sets, and discerning what the relationships among them demonstrate. Doing so will help ensure a consistent logic, and it will enable other scholars to replicate one’s work.

Disciplinary work is more likely to meet the standards of science than work that proceeds outside disciplines. Whether it is applied to physics, biology, or education, science, as I understand it, is an approach to inquiry that requires posing significant questions that may be studied empirically, linking data and theory, employing appropriate research methods as well as clear and explicit patterns of logic and reasoning, and continuously verifying and validating findings (Shavelson & Towne, 2002, pp. 3–4). Because disciplines offer distinctive theories and logics and are susceptible to replication, they are more easily aligned with the principles of science than are nondisciplinary approaches.

Although I believe that disciplinary work is more likely to yield good science than nondisciplinary work is, I concur in the discussion offered in *Scientific Research in Education* (Shavelson & Towne, 2002) pertaining to the design of scientific work in

education. As that report stated (and I should note that I served on the National Research Council committee that wrote the report), a wide variety of designs can be scientific. What differentiates science from nonscience is that

the design must allow direct, empirical investigation of an important question, account for the context in which the study is carried out, align with a conceptual framework, reflect careful and thorough reasoning, and disclose results to encourage debate in the scientific community. (Shavelson & Towne, 2002, p. 6)

Finally, in addition to problem-finding and problem-solving research, I believe we need translational research that can yield usable knowledge. The findings of science may be useful to other scientists and may be tested and refined in further research. But the findings of scientific research are not in themselves useful to teachers or other frontline practitioners of education, and they often are of little direct use to policy makers. To be useful in practice or policy, the findings of scientific research in education must be transformed into tools that link knowledge to actual circumstances. To be useful in practice or policy, knowledge that was generated to produce understanding must be reorganized so that it will enable people to do things—to promote learning or to make a persuasive case for a policy proposal.

Educational problems are, by definition, multifaceted and complex. To be usable in education, then, knowledge must be developed from insights drawn from various disciplines. This is necessary so that the knowledge can account for all the factors (or as many as possible) that may affect a particular educational situation. After knowledge is built from various disciplines, to be usable, it must then be built into tools that can actually be utilized in practice or policy. Tests, texts, and toys may be examples of usable knowledge relevant to practice; policy briefs may be the equivalent for policy.

Recognizing the Importance of Problem-Finding and Translational Research

In education, just as there has been too little recognition of the need for problem-finding research, so, too, has there been too little recognition of the need for translational research. Despite the popularity of Donald E. Stokes’s (1997) wonderful book about “use-inspired” research, *Pasteur’s Quadrant: Basic Science and Technological Innovations*, there has been little effort directed toward studying how usable knowledge can and should be created. In consequence, we still know less about how to do translational research than about how to do disciplinary research. Despite that, it is widely recognized that teachers and policy makers ignore research. On occasion, they are even angered by the complexities that research reveals. That is understandable given that teachers and policy makers must act in the moment and have little time to mull things over. However incisive, disciplinary research does not help them meet the immediate challenges they face. It cannot directly improve educational practice or inform decisions among policy makers. If knowledge is actually to be of use in education, then it is urgent that we learn how to move from disciplinary knowledge to actual tools embodying such knowledge—usable knowledge.

To gain recognition and support for problem-finding and translational research, we will need to overcome the barriers that have limited understanding of the value of such study in the past and privileged what I am calling problem-solving research. It is commonly believed that problems are self-evident. Try to talk a politician into developing a budget for problem-finding research! I suspect that politician would quickly break into laughter. Nevertheless, not only must problems be observed; they also must be formulated into opportunities for research—into possibly answerable questions—if they are to be systematically investigated. This need is poorly understood.

At the other end of the spectrum, it is widely believed that if the results of research are known, it will be self-evident how they should be applied. Once again, what is commonly thought to be obvious is, in fact, in need of research. Beyond that, even if usable knowledge is critical to the improvement of educational practice and policy, it is enormously challenging to figure out how to translate the findings of research into tools for use and much easier to focus on the development and dissemination of disciplinary knowledge. Finally, of course, the research universities where much of the nation's research is carried out were built around commitments to science. Although most make a bow to public service in their rhetoric, the kinds of scholarship that are most revered and rewarded are those that are the most abstract and theoretical and the farthest from practice. Although this may be changing, publishing original research wins credit toward tenure, but not writing a textbook or supervising students in the field.

In research universities, schools of education have tended to be at the margins, often either starved for resources or treated as “cash cows” expected to support other faculties through the tuition they charge students enrolled in large classes. Often isolated from their colleagues in other departments, education school faculties have tended to be demeaned and scorned by their colleagues located elsewhere. Although there have been many different reactions to the social situation in which most scholars of education have found themselves within academe, a common one has been to try to be highly scientific. In this way, scholars have quite naturally hoped they might finally win the respect of their noneducation peers.

Searching for status and all that high status can bring, scholars of education have repeatedly tried to make their work more scientific. As I tried to suggest in *An Elusive Science: The Troubling History of Education Research* (Lagemann, 2000), throughout the 20th century, there have been many different efforts to improve knowledge about curriculum, instruction, and administration (or, now, leadership) via the introduction of more rigorous, more scientific methods. Today, once again, there are calls all around for more science in education. The problem with all this is that although it is true that we need more good science, we also need work that is not science. If we continue to try to meet all the needs that research in education should fill—problem finding, problem solving, and translation—through scientific research, we will continue to distort what science really is and, even more ironically, unwittingly contribute to the continuing demeaning of so much that is so important in education.

Making a good curriculum is vital to good education, but it is not science. A curriculum may embody usable knowledge, but

the actual process of making it, at best, involves translation, not science. School leadership is not science. It may be informed by science, and it may be studied in scientific ways, but it is foolish, I believe, to try to make it a science. Instead of trying to say that all that we do when we study education is scientific, we should be clear that when we do “scientific research in education” we are building on knowledge that may not have been generated by science and adding to knowledge that may not be science. Science is a vital part of the knowledge we need in education, but it is only a part. Science can offer vital insight into education, but it is not a panacea. To improve education, in addition to science, we need social will, determination, and a willingness to think, talk, and debate our goals, aims, and values as well as our most deeply held beliefs about equity, human capacity, and the significance of individual and group differences. Clarifying such matters is not a matter for science, but it is crucial to effective education. The current romance with “scientific research in education” has led us to privilege science—perhaps to overprivilege science—at the expense of knowledge, which comes in many different forms.

Clinical Research—Is It All That Is Needed in Education?

With that as background, let me turn to Jacquelin A. Bulterman-Bos's (2008) thought-provoking article about the theory–practice divide in education (this issue of *Educational Researcher*, 412–420). Relying heavily on David Labaree's description of the perspectives that teachers bring to their doctoral studies as they prepare to become education researchers, she calls for a new, unified work role. In Bulterman-Bos's view, differences in perspective are based on the different roles that teachers, on one hand, and researchers, on the other, fulfill. These different roles result in differences in perspective that create barriers to the development of the kinds of knowledge we need in education. Hence, building on Michael Polanyi's seminal work on tacit knowledge, Bulterman-Bos maintains that a synthesis of roles will result in a unified and more powerful approach to research.

I agree with Bulterman-Bos about the importance of clinical research. Clinical research is extremely difficult to do well, but when it is done well, it can yield powerful insights. John Dewey, among many others, understood this, which is why he placed such emphasis on grounding education study within a laboratory school.

As I have said, I also believe that clinical research is essential for problem finding. Watching children in a classroom working with the same teacher, but responding in very different ways, may lead one to ask why it is that good teaching has such different results with different students. That, in turn, can lead to a host of questions about the power of social situations, aptitude, and much more. There can be no doubt that observation can generate the grist from which research problems are drawn.

Nevertheless, I am troubled by Bulterman-Bos's argument for several reasons. First, however much I admire Labaree's work, I am not sure that all practitioners and all researchers hold such different “worldviews” and perspectives on research. In my experience, some teachers becoming researchers are entirely capable of seeing the world in ways that are “analytical,” “intellectual,” “universal,” and “theoretical.” Many researchers I know are just as

committed as teachers are to “the question of what is good for students.” Finally, even the most scientific researcher can, at times, focus on questions that are “normative,” “personal,” “particular,” and “experiential.” To put my point very directly, although I admire Bulterman-Bos’s stance toward clinical work, I believe her argument rests on a dichotomy that is not universal.

In addition, I do not believe the cause of education research will be advanced by efforts to synthesize the characteristics that Bulterman-Bos attributes to researchers, on one hand, and practitioners, on the other. I simply do not believe that disciplinary research can accommodate the normative, personal, particular, and experiential characteristics that Bulterman-Bos associates with students of education research who are or have been teachers. Bulterman-Bos maintains that in clinical research one can “feel” what good research questions are” as well as what methods to use, what data to marshal, and how to present research in ways “that acknowledge the dynamics of practice” (pp. 418–419). If that is true in clinical research, it is certainly not true in the disciplines. Even acknowledging that with experience one gains a tacit “feel” for matters of method, disciplines by definition offer rule-governed ways to identify patterns and meaning across multiple situations. That is both their strength and their limitation.

Education Research as a Distributed Activity

In my view, then, clinical research of the kind that Bulterman-Bos recommends is a necessary, but not alone sufficient, kind of education research. Rather than focusing merely on the kind of research that sits most comfortably in schools of education, I believe we should focus on supporting education as a more distributed activity across the full range of faculties found in universities.

Bulterman-Bos’s focus on the importance of clinical work in schools of education makes great good sense. For all sorts of historic reasons, schools of education in the United States have grown up as highly varied congeries of departments and programs, many offering degrees in nursing, nutrition, library study, communications, and the arts, along with education. Many are also home to an array of programs in educational history and philosophy, as well as to cognitive science and statistics. Whatever the precise combination at a particular institution, variation has been the leading characteristic among U.S. schools of education.

Beyond programmatic variation, across the entire range of schools of education, some exclusively graduate, others both undergraduate and graduate, there is also considerable variation in clinical arrangements. In some institutions where the education of practitioners is central, there are affiliated laboratory or professional development schools and long-standing relationships with local school districts. In other institutions where the education of practitioners is more marginal, clinical relationships tend to be more ad hoc and attenuated. I believe it is unfortunate that some schools of education (and, generally, the most elite ones) have grown up quite removed from the worlds of educational practice.

By definition, a profession is made up of a group of people who possess special knowledge that enables them to accomplish important tasks for society. As I have said, professionals do things: They cure disease, write wills, build tunnels, and teach children

to read. It follows that professional schools should have a clear professional focus, which, in the case of education, would mean being single-mindedly intent on improving educational practice and policy. In terms of research, it also follows that schools of education should focus on those kinds of research that either derive directly from “the doing” of education or that seek to directly improve “the doing” of education. That means that they will feature problem-finding and translational research. Doing so would enhance their mission as professional schools and help counter the frequent tendency among “ed schools” either to ape faculties of arts and sciences or to become so diffuse in focus that they lose their professional definition entirely. It would also strengthen the links between schools of education and their clinical settings if both educational programs and research were grounded in those sites.

In line with that, it also makes sense to recognize that most problem-solving research should be centered in colleges of arts and sciences. Because this is where training in the disciplines occurs and because the disciplines are, in my view, most useful for analyzing the relationships among variables that will clarify educational problems and puzzles, this is where most of the causative, analytic research about education should be centered. If someone wants to study education as a historian, sociologist, or economist, that person needs, first, to become a good historian, sociologist, or economist. Being well versed in the literatures of the discipline and in its methods is essential, and so is acquiring the distinctive ways of thinking and presenting that make it possible to join in the conversations of that particular field of scholarship. In all this, disciplinary peers are significant. Subsequently, this person will need to learn a good deal about the special attributes that make studying education different from, say, studying nutrition. And, for that, he or she will need to spend time in the clinical settings maintained by schools of education. The point, however, is that research in education and training for research in education—for the full spectrum of research that is needed in education—should be a distributed function shared, certainly, by two among all of a university’s faculties and, likely, by many more.

Of late, high interest in education has encouraged more scholars, from more different faculties, to become involved in education. Today, more and more schools of business are collaborating with ed schools in the preparation of school leaders; increasingly, too, schools of education are working with schools of public health on problems of childhood obesity and other developmental issues. Schools of public policy and of education share interests in matters of tax policy, state regulation of schools, and much more. Although these cross-school relationships have, in fact, made education a distributed activity, the importance of partnerships has not been fully recognized. More often than not, collaborations have been ad hoc and somewhat random. They are more often established as a result of a gift or a foundation grant or the determination of a small group of faculty members than as a result of a university’s systematic and sustained commitment to deploy all the intellectual resources needed for studying and improving education.

If universities are to put muscle behind their rhetorical commitments concerning the improvement of education, they will do well

to develop structures to coordinate their research in education. These could take the form of university-wide research coordinating committees or an associate provost deputized to work across faculties. They will require significant resources and support from the top. However the effort is organized, if universities define an appropriate set of goals—say, guaranteeing that all children in their geographic areas reach the age of 18 healthy and ready for college or other further education, or guaranteeing that the race achievement gap in their states be eradicated by 2025—and make it a priority to reach those goals, many of the educational problems we face can be effectively addressed. Of course, some institutions have already made significant and effective commitments to their communities. The University of Pennsylvania's work in West Philadelphia readily comes to mind. The challenge is to make meaningful commitments to education more widespread and effective and less a mere matter of talking grandly of leadership and public service.

In conclusion, I would like to return to the point with which I began. Education is a complex, multifaceted social phenomenon that takes place in a variety of institutions and settings. As such, it requires many different people, working from many different perspectives, to study it well. Universities are complex, multifaceted institutions that possess the resources needed to study education, and education must become a distributed university-wide problem if we are to develop all the kinds of knowledge we need. That is *not* a call to disband “ed schools.” Quite the opposite. Schools of education are essential as places where ideas about education meet the realities of practice. Schools of education are places where education is always the focus, even when public interest lags and the interests of schools of business

or colleges of arts and sciences migrate elsewhere. This article is a plea to marshal all the resources necessary to generate the full range of research that education demands. It is a plea to match university rhetoric with the commitment needed to generate all the knowledge that such a complicated, important part of our society needs and deserves.

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AUTHOR

ELLEN CONDLIFFE LAGEMANN is a Bard Center Distinguished Fellow at Bard College at Simon's Rock, 84 Alford Road, Great Barrington, MA 01230; lagemann@bard.edu. She is also the Charles Warren Professor of the History of American Education at Harvard University (on leave). Her research interests are the history of education in the 20th century and matters of contemporary education policy.

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