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Crossing anxious borders: teaching across the quantitative–qualitative 'divide'

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This paper is about teaching and learning across the so-called quantitative–qualitative divide in light of current debates in the US about the definition and quality of educational research. It draws on the author's research and teaching experiences, her role in the redesign of qualitative methods coursework and participation in a school-wide effort to improve doctoral training at Harvard Graduate School of Education. The paper explores institutional, cultural and psychological reasons for why the quantitative–qualitative divide persists, including researchers' own anxieties. It is argued that epistemological tensions in educational research should be sustained and embraced rather than resolved in favor of one side over the other, as is too often the case. The author identifies how qualitative research departs from the quantitative model, placing distinct demands upon students who are learning the craft, and offers suggestions for what might help the process.

Introduction

This paper is about teaching and learning across the so-called divide between quantitative–qualitative research within the field of education. Current debates about the definition of educational research, driven by federal legislation such as the No Child Left Behind Act of 2001 and the Education Sciences Reform Act of 2002 have spotlighted this divide. In this context of increased federal regulation of educational research and the heightened role of the government as a broker of what counts as evidence and 'quality', a renewed concern about research training for educational scholars has taken hold. These concerns are reflected in reports written by the Carnegie Foundation (2003), the Institute for Educational Sciences (IES) (2004) and the National Research Council (NRC) (2002, 2004), and have provoked a range of responses (Erickson & Guteirrez, 2002; Eisenhart & Towne, 2003; Lather, 2003;

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Erickson, 2005; Eisenhart, 2005). This is a renewed concern because US graduate schools of education have long wrestled over the definition and aims of educational research, which Ellen Lagemann (2000) has described as an 'elusive science' personified by the conflict between and eventual triumph of measurement-oriented Edward Thorndike over meaning-oriented John Dewey. It would seem that this conflict between measurement and meaning persists but has taken on a new shape.

My view of the current debate is that it focuses too much attention on what research 'methods' we should be using and not enough attention on what problems we should be solving and what research 'questions' we should be asking. I believe there is a reason behind this fixation on methods that has to do with researcher anxieties, which I shall describe. I worry that the now-popular call for 'mixed methods research' keeps people from confronting the inevitable and healthy tensions that lie at the heart of educational research that matters. That said, I also agree with Jennifer Greene (in this volume) about the generative potential in the call for mixed methods research—if broadened and redefined.

I wish to advocate for a *multi-methodological* approach rather than the current *additive* (quantitative + qualitative = *mixed methods*) approach. I distinguish between methods and methodology; methods are discrete 'tools' for collecting and analyzing data that answer specific questions whereas methodologies are the theoretically- and conceptually-driven, philosophically-grounded knowledge 'kits' that hold the tools.

In this paper I draw on my own research, on my experiences as the designer of and instructor in a sequence of qualitative research methods courses and on my participation in an effort to redesign and improve doctoral training at my home institution, Harvard Graduate School of Education. It is in these three capacities that I have come to understand just how anxiety-ridden is the border between qualitative and quantitative research. I offer the metaphor of *border crossings* as a way for us to consider the multiple, rather than dualistic, either/or conception of research methodology and to argue that methodological borders would be easier to cross if they were not policed so ardently by (a) governmental, institutional and disciplinary border patrols and (b) the researcher's own self regulation.

I shall use mixed methods—drawing on institutional ethnography[1] and my own teaching and research experience as evidence (Smith 1987, 1990a, b, 1998; Campbell, 1998) in light of psychoanalytic and Foucauldian theories—to make my case. I say this knowing full well that this is not the sort of mixed methods research people imagine when they hear the phrase. But those of us who are ethnographers understand that our research strategies have always been quite 'mixed', in the sense that they are disparate—ranging from large-scale surveys, descriptive statistics, content analysis, projective testing (all of which use quantitative analysis), as well as in-depth formal and informal interviewing and observations that are often analyzed using multiple analytic strategies such as grounded theory, life history, narrative and discourse analysis. Ethnographic research strategies are also 'mixed' in that they are meant to be responsive to the setting, circumstances and subjects of our research. In order to immerse ourselves in the social worlds and to tap the understandings of those we study, ethnographers must be prepared to tolerate uncertainty and to accept the

fact that the research strategies we proposed initially no longer fit the fieldwork situation. To those who don't do this kind of in-depth, immersive, field-sensitive research, it can appear that the sort of 'mixed' methods that ethnographers employ are less than 'scientific'. This is an unfortunate consequence of the science wars, and one that qualitative researchers-in-training need to be made more aware of and be readied to account for. Perhaps staking a claim to doing 'mixed methods' would help, but there are other social relations at work with which educational researchers must contend.

Institutional and epistemological divides

What drew me to the field of education is that it is a discipline *and* a profession, requiring thoughtful relationship between research and practice. I have noticed that this relationship is often framed as a *divide*, as expressed in the enduring concern about 'bridging the theory–practice gap'[2] rather than an *articulation* between two sorts of practice. A slightly different version of this divide can be seen among sociologists and anthropologists, as in the distinction between those who do 'applied' versus 'academic' scholarship.[3] Interestingly, in recent years, each discipline has made strides to renegotiate the terms of the divide by calling for a 'public' sociology and anthropology (The American Sociological Association Ninety-Ninth Annual Meeting, held in San Francisco in 2004, was entitled 'Public sociologies').

But, unlike sociology and anthropology, in the field of education, the divide is institutionalized as a split between the nature, content and process of the Ph.D. and Ed.D. degrees and the subtle, but ever-present valuing of one qualification (Ph.D.) over the other (Ed.D.). Some argue that, until this split is resolved, the field as a whole will continue to suffer, a point made by speakers at a previous American Educational Research Association Panel entitled 'A new vision for the doctorate in education: creating stewards of the discipline through the Carnegie initiative on the doctorate' (2004). I tend to agree, and want to suggest that the institutional split reflects deeper divisions than have been acknowledged.

Added to this institutional separation of doctoral degrees, are the split (and opposing) notions of evidence in which one type of scholarship—that which is associated with quantitative research and defined as 'empirical', 'hard' and seen as 'good' is pitted against the other type—that which is associated with qualitative research, defined as 'interpretive', 'soft' and seen as lacking or 'bad'. The gendered dynamics of this methodological divide have been noted by feminist scholars who argue that beliefs about what counts as evidence and science both reflect and reproduce gender difference and inequality (Keller, 1985; Harding, 1987; Smith, 1987, 1990a&b; Haraway, 1988; Collins, 1990; Bhavnani, 1993; Stanley & Wise, 1993). Despite acknowledgement by research scholars that the quantitative–qualitative divide should not be viewed in binary terms, the dichotomy persists. As recently as last year, a National Science Foundation report prepared by Charles Ragin *et al.* (2004) argued: 'A qualitative–quantitative divide permeates much of social science, *but this should be seen as a continuum rather than as a dichotomy*' (2004, p. 9, my emphasis).

If this is the case, why does the divide persist? I suggest that these institutional and methodological divides can also be understood as defenses that help scholars bind their anxieties, what in psychodynamic terms is called *splitting*. The psychological process of splitting is itself not the problem. In fact, it helps us resolve conflicting feelings—the most common of which are love–hate, gratitude–jealousy, attachment–autonomy—that can be difficult for us to abide. So, to protect ourselves from facing feelings that might be unacceptable or unbearable (like hate or jealousy or autonomy), we project those feelings onto someone or something outside ourselves, which we denigrate or devalue. This keeps the 'good' feelings pure and intact, allowing us to continue about our business. But this defense can also keep us from accepting (some would say celebrating) difference, including the ability to see different parts of ourselves as one whole. According to a Kleinian perspective (Klein, 1975), one goal of development is the integration of mixed feelings so that we are better able to see others in their own light (rather than in terms of our projections), which then allows us to do the same for ourselves.

Splitting also happens at a cultural level, where groups of people, behaviors and ideas get pitted against each other and either idealized or denigrated in ways that keep us from having to do the hard psychic and social work of coping with uncertainties, fears and forbidden pleasures that we wish to avoid. This is a brief and admittedly sketchy description of splitting. I have written elsewhere about how it operates in school contexts so that some forms of knowledge and types of people are valued over others in ways that reproduce gender, race and class inequities (Luttrell, 1989, 1997) and as a way to understand near-universal gender belief systems about women's subordination (Quinn & Luttrell, 2004). But, my point here is that considering the psychodynamics of splitting might help us understand why these methodological divides are so tenacious. Rather than embracing the inherent tensions that are embedded in the continuum between research and practice, quantitative and qualitative (and we could add other cultural binaries here, like reason and emotion; mind and body; male and female; homosexuality and heterosexuality), people resolve their conflicts/tensions by picking one side over the other. But this need not be so, and we could imagine a community of educational scholars where these tensions were *sustained* and *embraced* rather than resolved.

I am not one to draw on sports analogies to make a point, but in this case, I think that the game of baseball has something to teach us about reaching beyond the current binaries, especially those concerning evidence. It is a joke that I was told a few years ago by one of my doctoral advisees and with which I have been regaling students in my qualitative research course as a way to talk about epistemological tensions.[5] The joke goes something like this: There are three umpires discussing their job of calling balls and strikes, and their philosophy about what they do. The first umpire says, with great assurance, 'I call 'em as they are'. The second umpire says, 'Well, I'm not so sure, but I call 'em as I see 'em'. The third umpire looks steadily at both of them and says, 'They ain't nothing 'til I call 'em'. What I like about this joke is that it challenges the usual dualism about the nature of evidence—one type is not pitted against the other. Moreover, it illustrates what I believe constitutes part of the thrill of the

game (for lovers of baseball). By extension, for practitioners of educational scholarship there is creative tension between these three philosophies, and analyses that hold all three in view offer the greatest possibilities.

Ethnography and anxiety

My views about the connection between researcher anxiety and methods also draw upon the work of anthropologist George Devereux (1968). His views, and then Ruth Behar's (1996) reference to them, helped me to understand the demands of ethnographic knowing, including my own discomforts and defenses as a someone trying to learn about places I had not been, people I did not know and circumstances in which I had not lived. I found certain aspects of this learning agonizing, especially being in settings where I witnessed stark inequalities and abuses of power that wounded people's sense of self and identity. On many occasions, after a day of listening to disturbing stories about the injuries sustained while growing up Black in the racially-segregated south and doing domestic work (Luttrell, 1997) or seeing pregnant girls cope with being treated as objects of shame and blame in their school setting (Luttrell, 2003), writing up my fieldnotes seemed fruitless, at best. As Ruth Behar, author of *The vulnerable observer*, quoting Devereux, writes:

> Because there is no clear and easy route for confronting the self who observes, most professional observers' defenses, namely 'methods' that 'reduce anxiety and enable us to function efficiently' … [in] situations in which we feel complicitous with structures of power, or helpless to release another from suffering, or at a loss as to whether to act or to observe. (Behar, 1996, p. 6)

For Behar, participant observation is 'oxymoronic', 'split at the root' (1996: 5) in ways that can lead to an anthropology that is hollow at the heart. She splits apart (and casts as opposing) two ways of ethnographic knowing—emotional participation and detachment. I prefer to view the tensions in ethnographic knowing as necessary and productive, and have found that I needed to be emotionally present, able to acknowledge painful and at times unbearable feelings (my own, and those of my research participants) *and* to exercise some analytic distance in order to sort out what I learned and would write in the service of social and educational reform.

In my recent book, *Pregnant bodies, fertile minds: race, gender and the schooling of pregnant teens* (2003), I describe ethnographic research as a 'social art form' (a phrase borrowed from anthropologist Karen McCarthy Brown, 1991) that is achieved through an abiding curiosity, careful listening, painstaking observation, sustained attention, emotional engagement, and a rigorous tacking back and forth between the problems confronted by research subjects in their everyday lives and the conceptual/theoretical problem or puzzle one hopes to answer as a consequence of the research. Most crucially, I believe that educational research should enable researchers and those who are the subjects of research to change how they see themselves and are seen by others, to exert what could be called cultural agency. This is not easy when audiences (including school officials and policy-makers) may have strong presumptions

and images about the setting or people being studied (as is often the case in research about urban education).[6]

In my work with pregnant schoolgirls, I found it necessary to develop some innovative ethnographic and image-based methods (including verbal and visual narrative analysis) in order to tap into their insider perspectives and follow-up some early leads about how the girls were thinking about being identified as a 'problem', their dilemmas and what sort of conditions shaped their passage from adolescence into motherhood. But I also came to realize that these strategies did more than help me elicit hard-to-access data; they also served to reduce my own worries about trying to bridge several divides, including those between research and practice, art and science, woman and girl, White and Black, middle class and wage-poor life worlds, to name a few. The research activities/methods served as a means through which I could cross anxiety-ridden borders, permitting me to be present with and more fully attentive to the girls, and freeing me from a certain rigidity I held about research protocol and staying 'on task'. Similarly, the research activities (collage-making and theater) provided even enough distance for some girls to be able to take another view of themselves and others. I marveled at the girls' curiosity and playful engagement in the research activities, their willingness to open up and express themselves (and to identify what gets in the way of that agency), which in the end, is the thrust of the story I tell, and the way that my research articulates with practice.

Lessons learned in the field

In 1999, a recent Ph.D. sociologist and I were hired at the Harvard Graduate School of Education to, among other things, redesign the qualitative research coursework for doctoral students. Doctoral students flooded my office, asking me to serve on their doctoral committees whose research they thought needed a 'qualitative methodologist'. It seemed that overnight I had become a 'qualitative methodologist'—and I could sense an impending identity crisis because I had never viewed myself as such. Rather, I saw myself using qualitative methods to study American schools as contexts for, and windows onto, the formation of self-understandings and social identities and as sites where beliefs about worth, value, knowledge and power are acquired and resisted. That I used qualitative methods stemmed from the *questions* I asked about subjectivity and social, cultural and psychological processes, and the *theories* with in which I was in dialogue. I used qualitative methods because I sought detailed, in-depth knowledge about specific cases, with the goal of finding out how things work rather than predicting outcomes.

I began to feel a gap between how I saw myself and how I was being positioned by others as the 'qualitative methodologist'. I grew reluctant to take up the position of qualitative 'expert' for many reasons, not the least of which is the huge range of student research that falls potentially under this rubric. Nonetheless, as a consequence of my teaching experience, my role in the redesign of the qualitative coursework sequence at the School, and my participation in a school-wide effort to improve doctoral training, I have learned at least three things that I think are worth sharing:

1. *Teaching qualitative methods as if they are a set of generic skills, separate from a theoretical/methodological framework born out of a disciplinary set of enduring questions does a disservice to students of education.* Students need to be able to identify the driving concepts behind their research questions, or put somewhat differently, they need to know in what ways their questions are sociological, anthropological, or developmental (and so on). And, in this spirit, they need more knowledge about the enduring debates, which drive a disciplinary perspective, for example, the culture concept for anthropologists, the structure–agency dialectic for sociologists, or developmental theory for psychologists. Without a grounding in explicit theoretical frameworks, I think students come to view qualitative methods as a smorgasbord, where it is easy to pile one's plate full of this or that special technique or tool without thinking about the meal as a whole. I am speaking personally here, not as a representative of my institution where this debate remains unresolved. Some people are arguing for a more generic approach and others for a more disciplinary approach. At this point in time, I believe both are necessary. Students need numerous opportunities for coursework where they can learn about theoretical concepts, methodological kits, data collection methods/tools, data analysis strategies, as well as access to research apprenticeships where they can learn the 'tricks of the trade'

   More than anything else, I believe that students in education need to be inducted into a community of scholarly practice, rather than forced to take up this or that researcher identity. While some suggest that doctoral students in the field of education be inculcated into a 'culture of science' (Eisenhart & DeHaan, 2005), I prefer students becoming part of more broad and inclusive communities of practice where the continuum between quantitative and qualitative methodologies is embraced, and the huge range of hybrid and combined strategies in between the two end points is preserved. At one end, one could find the most linear model of science with clearly delineated theories and hypothesis, and at the other end, one could find that conventional theories are held at bay and the researcher-researched distinction fades away. I believe students need to locate their research questions, strategies, and personal proclivities along the continuum rather than taking sides. This is why I think the recent call for 'mixed methods research' does not adequately resolve the problem, and in fact, serves to mask the stamp of superiority bestowed on quantitative research. Perhaps as a result of the current era of accountability, my office is now filled with students who come to me and say, 'I want to do a mixed method study'.
2. *The teaching and learning of quantitative and qualitative research follow different trajectories*. Quantitative research, and thus, its instruction, has a more *linear* flow (identify your theory, state your hypothesis, collect data, analyze it, draw conclusions), whereas qualitative research, and thus, its instruction has a more iterative flow. Both approaches seek to distinguish inductive and deductive work, and regard the one as feeding the other in a never-ending spiral.[7] But in qualitative research, this feedback loop is more immediate and embedded within a single study, whereas in quantitative research, these steps are more likely to be separated out into different studies. Qualitative research departs from the quantitative

model in ways which can be unsettling (or mysterious) for those who have been led to be believe that 'good' social science research follows the more linear, quantitative template. Researchers using quantitative methods must learn to adhere to a linear model in order to make adequate inferences from their data, but researchers using qualitative methodology must learn to abide an iterative model with constant refinement of data collection and analysis that takes place on the ground. Put slightly differently, what is distinctive about qualitative research is that data collection and analysis take place simultaneously.

Both faculty and students need to be apprised of this different research process, otherwise, there is a tendency (born of anxiety) to view quantitative coursework as the better model, because one can 'master' each progressive step. 'Mastery' is achieved differently in qualitative research and, above all, requires in-the-field experience.

Education faculty could learn this lesson about the instructional needs in research methods by crossing yet another anxiety-ridden border—each other's classrooms. As part of an effort to design a new core methods course, I suggested that John Willett, a statistician who teaches a quantitative course module to incoming doctoral students and Vivian Louie, a sociologist teaching the field-based participant observation module also to incoming doctoral students, might profitably shadow each other for a semester to become familiar with each other's courses. By sitting in on each other's classes and meeting afterwards to talk about the differences and convergences, they could report back to the committee about what might make sense in developing a core, integrated research methods course. Both John and Vivian were amazingly gracious about agreeing to the task, and according to John, the experience was a highlight of his teaching career. Among many things, John and Vivian both came to the conclusion that the initial learning process for quantitative and qualitative skills needs to take place separately and that, once students are more advanced, these skills can better be merged. I think I could make a psychodynamic argument for why this makes sense, but direct observation of students wrestling with accessing their sites, negotiating their entry, immersing themselves in and coping with the vicissitudes of a specific setting, and how this leads to a reformulation of research questions, makes for a more convincing argument.

Perhaps another way to think about the distinction between quantitative and qualitative research, and thus, its instruction, is in terms of where the conceptual complexities are and what one needs to learn to sort these out. For qualitative researchers, these complexities begin with negotiating the site, learning how to look, and how to translate field notes into data that can be analyzed. For quantitative researchers, understanding the difficult concept of inference—for example, p-values—is necessary before being able to master more flexible uses of methods and build a research design.[8] The order in which you apply methods in practice may not be the best order in which to teach them, which may account for why the quantitative research design course at HGSE is taught at the end of the sequence, whereas, it is taught earlier on in the qualitative course sequence.

In any case, I believe that qualitative research can't be learned in the abstract, or by examining exemplary qualitative texts (although this too is necessary). It is learned through practice, trial and error, and a constant dialogue with theory. Perhaps, it is because of my dual disciplinary affiliation, as sociologist and anthropologist, that I take this view. I see the benefits and limitations of both an unstructured and open-ended approach that anticipates surprises (the more anthropological model) and a more structured approach that fully elaborates one's theory, set of methods and analytical strategies at the outset (the more sociological model). These two styles are reflected in research courses taught within each discipline. An array of qualitative research methods courses have long been offered within departments of sociology, for example, 'fieldwork', 'case-study', 'historical-comparative', 'content analysis', 'narrative analysis' and so on. But until quite recently, the reigning view in anthropology was that becoming an accomplished ethnographer was a trial-by-fire process. The student would prepare herself for fieldwork (in some far-off land) by reading exemplary ethnographic texts, including everything others had written about the culture she was about to investigate, learning the language (if possible) and then, if she survived after a year or so to tell her story (always partial and less complicated than what she had experienced), she was deemed an anthropologist.[9] The trick was not to let one's research design interfere with one's ability to see the world through insiders' eyes.

3. *Qualitative research proposals are hard to write.* The more *in situ* approach to qualitative research has its own risks and limitations, not the least of which is that it can appear tentative, preliminary or unfocused. I like the metaphor used in the National Science Foundation (NSF) report mentioned earlier, where the authors write that qualitative research 'is a lot like prospecting for precious stones or minerals. Where to look next often depends on what was just uncovered. The researcher-prospector learns the lay of the land by exploring it, one site at a time' (2004, p. 12). This makes writing qualitative research proposals (and getting IRB approval and getting them funded) especially challenging. I try to prepare students for this challenge by emphasizing that while it might be hard to write up a specific data collection and an analysis plan in advance, they can present the *logic* of their study (thus the title of my course on qualitative research design, 'The logic of qualitative research'). I stress the importance of following one's hunches or early leads in fieldwork which is more easily done if one has read extensively and has strong background knowledge. The spirit of this reading is not to show that one has done one's 'homework' and can summarize the literature, what Becker (1986a) refers to as being 'terrorized' by the literature; but, rather to develop her/his own 'take' on the phenomenon of study; to resist the questions usually asked; and to prepare to make decisions about site, case or participant selection, otherwise known as sampling (also see Becker 1986b, 1998). Following a basic principle of feminist theory/method, I ask students to pay attention to what is missing—for instance, to consider who is not part of the site or sample, and why this might be so. Whatever decisions are made about who is and is not

included in the study, it is important to provide adequate detail about who is included. As sociologist Marjorie DeVault (1999) describes the craft of feminist research, the identifications researchers make about the subjects of their research are often telling, including who is 'marked' and 'unmarked', and these identifications jumpstart an important conversation about the partiality of the proposed research. I also ask students to consider what sort of data would prove their leads wrong, to seek confirming as well as disconfirming evidence. But, if students have not been exposed to the nitty-gritty work of qualitative data collection and analysis, it is hard for them to imagine how to collect data that will allow for the evaluation of alternative interpretations—a key sticking point in many student qualitative proposals I read. (This is why a course in participant observation is a pre-requisite for my 'logic' course.)

Such design efforts notwithstanding, my experience is that the proof is in the pudding—that it is easier to evaluate good qualitative research results, than good qualitative research proposals. I say this with trepidation because I don't want to be mistaken as mystifying the process. Rather, my point is that it is sometimes easier to trace what happened, what decisions were made and why, than it is to present a case for doing it a certain way up front. I have written elsewhere, tracing a series of investigative steps I took in my first study, explaining what was lost and what was gained each step of the way (2000). Others have written similar accounts and I believe we need more.[10] Still, establishing a trail of evidence, charting the path from data to conclusions can seem less transparent in qualitative than in quantitative research which may account for the familiar trope that qualitative work is suggestive rather than conclusive. My problem with this view is that it does not judge qualitative research on its own terms, and instead, judges it against quantitative standards, only to find it lacking. I leave it to readers to ponder ways to overcome this problem. But as I have written elsewhere (Luttrell, 2000), no research is perfect; all research designs and decisions involve something being lost and something being gained. The important point is that the investigator understands what choices and compromises are made and why she/he has made them.

A final piece of ethnographic data to conclude—I arrived in Montreal for the American Educational Research Association (AERA) conference and stood on line at the airport shuttle bus for over an hour. I admit to eavesdropping to the people standing behind me. They were each attending the AERA and began by asking each other what they did. The first speaker, a, woman, said she did quantitative research and was using a new statistical, structural modeling approach. She stopped short of saying what she was using the modeling for (I later learned it was for measuring student motivation) and then asked her conversational partner what he did. He replied saying that he was studying Foucault's concept of meditation. Without missing a beat, the women said, 'oh, you do qualitative research'. And from that point on, the discussion turned to personality characteristics, and that you either love or hate statistics, and either love or hate French theorists. But nowhere in this discussion did either one reveal to the other what they were learning about their subject matter.

What might have happened if these educational scholars had not sought the safe terrain of either side of this unnecessary divide? What insights were within their reach but outside their grasp because of it? One of the most regrettable consequences of not crossing anxious borders is that we miss out on each other's knowledge.

Notes

1. 'Institutional ethnography' is a theoretical/methodological practice identified by sociologist Dorothy Smith (1987, 1990a, b, 1998). It aims to make visible everyday experiences and practices that take place in an institutional structure and to show how these are connected to social relations outside the setting that are not fully visible. It focuses on people's various attempts to navigate what she calls 'ruling regimes' and starts from the viewpoint of those who are active in it. It is tied to, but also critical of the fieldwork tradition in social science. See Griffith and Smith (2005) for an exemplary study of this kind.
2. Indeed, preparing educational leaders to 'bridge the divide between practice and research' was the organizing principle for a proposed 'HGSE (Harvard Graduate School of Education) vision for the doctorate in education' (May 10, 2004), that among other things, proposed a set of core courses and research methods requirements. Our committee spent considerable time debating about which research methods courses would ensure that all doctoral students are 'rigorously prepared with the inquiry and methodological skills needed to be critical consumers of the professional and research literatures and to produce research-based useable knowledge for educators'.
3. I am most familiar with these two disciplines because I was trained as a sociologist and enjoyed a joint appointment in the departments of sociology and cultural anthropology for ten years at Duke University.
4. Panel members included Lee Shulman, Carnegie Foundation, Virginia Richardson, University of Michigan; David Berliner, Arizona State University; Margaret Eisenhart, University of Colorado at Boulder and Chris Golde, Carnegie Foundation. The Carnegie report pointed out that education is the only discipline that does not believe it is producing a *research* scholar.
5. I am indebted to James Holland for this joke, among many other insights that he has provided over the years.
6. The commitment to research and write in the interest of social change is hardly new and carries with it a bequest from turn-of-the-century American sociologists like Jane Addams, W. E. B. DuBois and Robert Park, from whose work educational researchers can surely benefit.
7. I thank John Willet for this metaphor.
8. I thank for John Willett for this insight.
9. Indeed, George Spindler (2000), a founding father of educational ethnography, writes that the concept of 'ethnography' as a methodology within anthropology is relatively recent, and that before the 1970s ethnography was simply what anthropologists did, and not a set of codified practices.
10. I recommend Howard Becker's (1993) article, 'How I learned what a crock was' (available at http://home.earthlink.net/~hsbecker/crocks.html) and of course his classic, *Boys in white* (1976) where the appendix describes the research process.

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