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On Viewing Educational Research as a Textual Enterprise

This article explores the history of North American educational research in an attempt to offer readers a historically informed view of the field as a whole. The first part of the article situates many of the major research programs in their original social settings, showing how they emerged in response to various administrative and academic pressures. The second part acknowledges the textual underpinnings of educational research by underscoring the practical reality that researchers produce patterned forms of text, which are used in patterned social situations to accomplish anticipated goals.

Cet article porte sur l'histoire de la recherche en éducation en Amérique du Nord et se veut un aperçu historique du domaine dans son intégralité. Dans un premier temps, l'article situe plusieurs des programmes de recherche les plus importants dans leur contexte social original et démontre leur évolution en réaction à diverses pressions de nature administrative et académique. L'article évoque ensuite les fondements textuels qui sous-tendent la recherche en éducation en soulignant que les éléments pratiques de la réalité font en sorte que les chercheurs produisent des textes selon des schémas donnés et qui sont par la suite employés dans des situations sociales structurées pour réaliser des objectifs anticipés.

The ways of doing educational research continue to grow in complexity and number, posing serious challenges to scholars, particularly graduate students, attempting to make sense of the epistemic terrain of the field as a whole (Callahan, 1962; Cronbach, 1957; Eisner, 1985; Lagemann, 2000). Unfortunately, institutional support for teaching encompassing perspectives of the field is sparse. According to Pallas (2001), graduate students in education at large universities usually gain their training in local communities of practice made up of a small number of faculty and students, all of whom implicitly share roughly the same epistemic paradigm and therefore find little reason to examine it critically in the light of alternative options. As a result, silos form—different sequences of methods courses, different brown-bag lunches, different professional affiliations—leaving students and faculty of one paradigm largely insulated from those of another.

This article proceeds from the assumption that a more direct addressing of the epistemic landscape of North American educational research will in the long run benefit everyone involved—from the graduate student struggling to formulate a research prospectus to the committee in charge of evaluating it, to the policymakers influenced by the findings. To this end, I begin this article by exploring some of the major research methods that have shaped North

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American education over the past century. In the second half of this article I offer an approach to educational research that emphasizes the practical reality that, as researchers, we are called to produce certain kinds of texts for certain kinds of audiences in order to accomplish our professional goals. I then situate some of the research methods in their traditional social settings and highlight some of the interesting features that are revealed in the process.

This is, of course, a discussion in broad strokes, one that simplifies the rich history of North American educational research and necessarily neglects many alternative readings of that history in order to offer a conceptual framework of potential value to the reader. My hope is that readers will find in the first half of this article a useful overview of many of the major research methods used in North American education, and in the second half a useful theoretical framework for making sense of those methods and effectively choosing among them in future situations. This is also an article of encouragement for those who see the role of writing in research as more than one of simply “writing up the findings”—for those who have entertained the idea that it is the writing itself, the anticipated communicative act, that should shape the choice of research method in the first place.

*Surveying the Landscape of North American Educational Research
Research Programs: 1900-1960*

Modern educational research was born out of the need for school superintendents to defend their decisions against mounting public criticism led by businessmen and politicians whose business-industrial ideology was sweeping North America at the turn of the 20th century (Callahan, 1962). Facing a public weary of increased taxes resulting from the “guesswork” of educators, many superintendents adopted an efficiency model based on the widely hailed principles of Frederick Taylor’s scientific management. This model relied on efficiency experts and other researchers to lend a scientific basis to administrative decisions by accurately measuring and analyzing quantifiable features of school operation. Some accepted the systematic observations of in-house researchers; others relied more heavily on surveys, popularized by Hall’s recent development of the modern questionnaire (de Landsheere, 1997; Lagemann, 2000). In either case, correlational and descriptive statistical studies flourished, and with Fisher’s (1925) development of the analysis of variance, efficiency experts and researchers began to replace the laborious description of “school facts” with reliable statistical inferences drawn from educational data sets (Howie, 1999; Rucci & Tweney, 1980; Skipper, Guenther, & Nass, 1970). The experiential judgment of the teacher was replaced in large part by the large-scale quantitative practices that had given the United States its economic edge, and decisions ranging from curricular changes to classroom sizes were frequently made on the newly justified grounds of fiscal solvency (Callahan, 1962).

With the 1930s came the passing of the efficiency movement, and the initial enthusiasm for the experimentation of the 1920s was tempered by difficulties in harvesting meaningful quantitative data from all but the most sterile settings. Campbell and Stanley (1963) noted “defections from experimentation to essay writing” during this time (p. 2): Dismayed by the state of experimental research in controlled settings, many academic psychologists turned their at-

tention to the mental life of school children *in situ* and adopted naturalistic methods such as introspection, observation, and narrative reporting. According to Kendler (1987), some researchers combined the practices of phenomenology and experimentation by writing detailed narrative reports of simple laboratory demonstrations (Kohler, 1929; Wertheimer, 1945). Others remained committed to controlled experimentation as the only route to reliable educational knowledge despite the initially disappointing results (Monroe, 1938), others turned to correlational studies (Cronbach, 1957), and still others began to reject the possibility of a scientific basis for educational inquiry altogether (Johanningmeier, 1969).

Over time, many educators turned to the intellectual leaders of their field for an educational vision that would repair their own epistemological and ideological fractures and offer a sustainable research program commensurate with the relatively scarce research funds of the time (de Landsheere, 1997). With notable exceptions, two paradigmatic camps stabilized: Those who were enticed by the prospect of an objective educational science remained committed to the educational psychology of Thorndike; those who resonated with the idea of reconstructing a new social order through progressive schooling tended toward the educational philosophy of John Dewey (Lagemann, 2000).

A Columbia-trained psychologist, Thorndike (1914/1921) conceived of the mind as a massive collection of differentiated "neurones" organized into stimulus-response bonds, not unlike the wires of a telephone switchboard. This materialist, mechanical metaphor lent a directness to the interaction between stimuli and responses that displaced intentionality from the purview of education (Joncich, 1962). Thus original nature was nothing more than the product of a common collection of stimulus-response bonds, which when left on their own developed into socially undesirable behavior: "what is is not what ought to be," lamented Thorndike (1962) in his *Principles of Teaching* (p. 55). Closing the gap required learning, which meant forming or reinforcing socially appropriate stimulus-response bonds and dissolving the less desirable ones. The control of behavioral change through modification of neurochemical bonds: This was the business of education, and the charge of educational research was the discovery of the universal laws by which such modifications took place. As such, the fundamental phenomena of education were construed as material, quantifiable, and thus directly amenable to the highly successful statistical research methods then emanating from British agronomy and biology.

Thorndike's (1962) epistemological framework transferred the basis for decision-making from the experiences of the teacher to the experiments of the researcher. "A child's mind is never a witch's pot to be set in action by educational incantations" (Joncich, 1962, p. 26), remarked Thorndike in opposition to the experiential lore passed on from teacher to teacher. Rather, teachers should receive the curriculum and pedagogy from the educational administrator and researcher respectively whose expertise in statistical methods, psychology, and neuroanatomy far outweighed the subjective experiences gained from working with children *in situ*.

Dewey (1997) advocated a science of education as well, although its epistemic basis fundamentally conflicted with that of Thorndike. Trained as a philosopher, Dewey saw the mind as a complex product of social adaptation

and rejected Thorndike's stimulus-response psychology because it neglected the role of intentionality in human life (Tomlinson, 1997). He agreed with Thorndike that the mind when left on its own did not develop naturally into a socially productive organ. However, it was scientific thinking itself, thought Dewey (1900/1990), that offered children and adults alike entrance into the kind of informed decision-making and conscientious social action that would ensure civilized living. It began with reflective thinking, a sort of active inquiry beyond the face value of an alleged truth or belief, and progressed to the processes of analysis and synthesis, of breaking apart and rebuilding into personally meaningful systems, followed by the continual testing of such systems against personal experience (Dewey, 1910/1997). In doing so, people began to create systematic knowledge provisionally reconciled against personal experience, which inevitably led to new areas of confusion for further scientific consideration. Educational researchers may have had specialized training in statistical practices, argued Dewey (1929), but it was teachers who had experiential access to the motives, intents, personalities, and abilities of children learning *in situ*. It was not guesswork that the experienced teacher had to offer, but systematic, practical knowledge, provisionally held and continually reconciled against actual classroom experience, and that, for Dewey, constituted the first truly scientific basis for educational decision-making in North America.

By the 1940s, a generation of the US's top behavioral scientists had advanced inferential statistics in the direction of education, owing in large part to Hotelling (1931), Johnson and Neyman (1936), and Snedecor's (1934, 1937) reframing of Fisher's variance program in social scientific terms a decade earlier (Rucci & Tweney, 1980). A student of Hotelling at Columbia, Wilks (1932) developed the multivariate generalization of Fisher's (1925) analysis of variance in 1932, one year after Hotelling's own multivariate generalization of Student's *t*. Johnson, after having studied with Fisher in London, returned to Minnesota, where he developed with Neyman a covariance technique for testing linear hypotheses in educational research. According to Rucci and Tweney (1980), Snedecor's influence on education took the form of two statistics textbooks, which were "nearly always cited by early psychological research using variance analysis" (p. 178) until Lindquist's (1940) *Statistical Analysis in Educational Research* emerged as the standard in the field.

Further advances in educational statistics were so plentiful that by that mid-1940s few researchers cited Fisher, Snedecor, or Lindquist in conjunction with introductory variance analysis (Little, 2003). Fisher's initial program, barely two decades old, had become one of the methodological approaches of choice in educational research, gaining significant support from the 1947 founding of the Educational Testing Service and the increased demand for rigorous admissions practices that followed the 1944 passage of the G.I. Bill (Lagemann, 2000). By the 1950s, courses in variance analysis were prevalent in graduate programs across the US (Rucci & Tweney, 1980), which served to further bolster experimental approaches to education (Campbell, 1957; Cronbach, 1957).

With the 1950s also came growing disappointment in some circles with the relatively uncoordinated body of knowledge that had amassed from decades of

research under Taylor's and Fisher's programs. For years hypotheses had been empirically tested, often in sophisticated ways, and forthcoming claims rightly given the status of scientific knowledge. But the body of work as a whole generally failed to aggregate into something larger than the sum of its parts. Under the banner of the Theory Movement, write Evers and Lakomski (1996), many scholars worked to replace the "naïve empiricism" of these efforts with a more rigorous, theoretical approach that would yield a systematic body of educational knowledge relevant to administration. Enamored of the logical empiricism of Feigl (1951, 1953), scholars of the Theory Movement advanced a hypothetico-deductive method of theory-building that involved deducing specific, observable hypotheses from more general theoretical statements, testing those hypotheses empirically, and either rejecting or accepting them on the basis of those tests. This web of general theoretical statements formed the higher-level theory that would unify educational research and direct administration, while remaining firmly grounded in empiricism through the hierarchy of deductive claims that led all the way down to observable, testable hypotheses.

No one was more central to this line of work than Griffiths (1959), yet according to Evers and Lakomski (1991), even Griffiths failed to meet the challenging epistemological standards of logical empiricism, though the attempt was valuable in its own right.

Griffiths places great emphasis on identifying and articulating the core assumptions of his theory; for from these will be derived "a larger set of empirical laws".... The empirical laws are not quite forthcoming, but in their place Griffiths does specify a set of testable propositions, some major, some minor, which all taken together represent a serious attempt to create a hypothetico-deductive structure.... True, there are no purely logico-mathematical derivations as Feigl's framework [logical empiricism] requires, but then administrative theory, then and now, is not a quantitatively formalized theory. (pp. 56-57)

Another limitation of the Theory Movement was its exclusion of values from the purview of administrative theory owing to the demand of logical empiricism that key theoretical terms be empirically definable, that is, measurable. Evers and Lakomski (1996) offer a clear illustration of the logical empiricist's position.

One can test and perhaps confirm hypotheses about school vandalism, noting patterns of correlation among defined variables. It is, however, something else again to justify the claim that such vandalism is wrong. The idea here is that one can observe destruction, or damage to property, or even the suffering behaviour of others without observing something called wrongness. No matter how carefully we observe, it is only facts that are ever seen and never values. (p. 4)

This exclusion of values may have bolstered the image and activity of the natural sciences, especially physics and chemistry; however, it effectively alienated administrative theory from the practical concerns of school administrators, profoundly diminishing their basis for making and defending value-based decisions in schools—a limitation that became increasingly problematic

for school superintendents as the US entered the tumultuous years of the 1960s. (For other criticisms of logical empiricism, see Feyerabend, 1975; Kuhn, 1962.)

The mid-century era also saw the emergence of more socially oriented perspectives in educational research owing in large part to the founding of the Committee on Human Development at the University of Chicago (Lagemann, 2000). Among the Committee faculty was social anthropologist W. Lloyd Warner, whose interest in relationships among social groups led to his development of the concept of social class as an integral factor for understanding human behavior (Warner & Lunt, 1941, 1942; Warner & Srole, 1945). This aligned in many ways not only with Dewey's progressive pedagogy, but also with the insistence by Caroline Zachery of the Progressive Education Association that the personal lives of students must be understood through an investigation of their relationships with others (Pulliam & Van Patten, 1999). Although, as Lagemann (2000) points out, Warner's concept of social class was later criticized on several grounds, his work not only provided an extremely fruitful analytic framework for socially minded educational researchers interested in equity issues, but also highlighted the relevance of fieldwork to education. Warner, Havighurst, and Loeb's (1944) *Who Shall be Educated?* and Davis's (1951) *Social-Class Influences Upon Learning* demarcate this early awareness of sociocultural factors in education.

Research Programs: 1960-2000

From a cumulative epistemological perspective of the sort advocated by Campbell (1974), we might expect educational research to have stabilized over the 20th century into a set of orthodox practices, leaving only a dwindling fate for those less enduring "fads and fashions" (p. 416) commonplace in theology and the humanities. Yet the second half of the 20th century experienced a complex branching of research methods at least on par with the variety of the first half of the century.

As early as 1923 McCall lamented the difficulty involved in harvesting meaningful, statistically amenable data from educational settings. Not only were educational researchers finding the statistical methods themselves troublesome to master, they were finding the reconceptualization of their phenomena in categories amenable to such methods an even more daunting task. It was an ontological and epistemological problem that remained largely unaddressed until 1963 when Campbell and Stanley published *Experimental and Quasi-Experimental Designs for Research*.

Campbell and Stanley's (1963) text offered a full-scale endorsement of experimentation as "the only available route to cumulative progress" (p. 3). The aim of the educational experiment was to determine through quantitative comparison whether and to what extent a particular "intervention" affected student performance. Research methods offered an increasingly rigorous way to make the quantitative comparisons, but it was the design of the experiment, argued Campbell and Stanley, that determined the degree to which those comparisons represented the effect of the intervention rather than the effect of confounding variables on student performance. Such was the rationale for the 16 research designs explored in detail in their text, to which I now briefly turn.

Characterized as a minimum point of entry into research, pre-experimental designs lacked random assignment. As such, any conclusion about the effect of

the interaction drawn from pre-experimental observations presupposed an expectation of what the data would have looked like if the intervention had not occurred. In one-shot case studies in which an intervention is followed by a single observation, the researcher had no second group for comparison and therefore could offer no assurance that any feature of the observation was caused by the intervention. By observing before and after the intervention, one-group pretest-posttest designs improved on the isolation of the effect of the intervention by including two observations. However, the researcher was still unable to offer any assurance that the observation was not caused by confounding variables such as history, maturation, or testing. Conclusions drawn about the effect in this case must rest on the assumption that the pre-test and the post-test observations would have been equal if not for the intervention.

True experimental designs solved the problem of comparison group equivalence by introducing random assignment. This required the researcher to assign half the participants randomly to an intervention group and half to a control group. This ensured that any confounding factors would be equally distributed among both groups, which enabled the researcher to attribute more reasonably any difference between observations to the intervention. In other words, randomization provided a logical rather than an empirical warrant for assuming that post-test observations of the control and intervention groups would have been equal if not for the intervention.

In naturalistic social settings, where researchers were often unable to exert "full experimental control" over the setting, Campbell and Stanley (1963) advocated quasi-experimental designs. Although they are not particularly appropriate for the testing of causal laws or the overall "generation of causal understanding" (Cook, 1983, p. 80), quasi-experimental designs are often described as well suited for probing ostensible cause-and-effect relationships in particular situations. In the separate-sample pretest-posttest design, for example, two randomly equivalent groups are observed, one before and one after the intervention. Although conclusions about the effect of the intervention remain susceptible to history, this quasi-experimental design offers a substantial improvement over the one-group pretest-posttest pre-experimental design by eliminating testing as a confounding variable (Campbell & Stanley). Such designs are unified in their attempt to eliminate the possibility of variables other than the intervention influencing their quantitative comparison of observations.

Yet at the very moment that Campbell and Stanley (1963) were reaffirming controlled experimentation as "the only means for settling disputes regarding educational practice," a growing number of educational researchers were turning to ethnography for a means of inquiring into what Spindler (1982) had first called in 1954 "the sociocultural contextualization of schooling" (Lagemann, 2000, p. 220). Returning to the tradition begun by Warner and Davis in Chicago, this next generation of educational researchers embraced participant observation and interviewing as important tools for understanding the lives of school children. At first, the assumption that such lives could be distilled to a common set of formal properties and documented through objective life narratives guided the process of inquiry, and many ethnographers of this phase tried

to operate within experimental and quasi-experimental traditions (Rosaldo, 1989). *Boys in White* (Becker, Geer, Hughes, & Strauss, 1961) well illustrates this early mixed-method approach. "Firmly entrenched in mid-century methodological discourse," write Denzin and Lincoln (1994, p. 8), "this work attempted to make qualitative research as rigorous as its quantitative counterpart.... This multimethod work combined open-ended and quasi-structured interviewing with participant observation and careful analysis of such materials in standardized, statistical form." Yet with the publication of Geertz's (1973) *The Interpretation of Cultures*, ethnography found its interpretive basis. In a chord similar to that of Dewey's, Geertz announced:

The concept of culture I espouse ... is essentially a semiotic one. Believing, with Max Weber, that man [sic] is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning. (p. 5)

For Geertz, culture was not reducible to behavior. Rather, it was the "web of significance" in which particular behaviors became meaningful, for it is only in particular cultural contexts that eye twitches become winks and coughs become heckles, and it was the goal of interpretive ethnography to describe as richly and as intimately as possible the spectrum of those meaningful actions, those "flecks of culture," idiosyncratic as they may be, rather than focus on what Thorndike (1962) might have called their material or behavioral counterparts.

In place of experimentation leading to covering laws, Geertz (1973) proposed participant observation and interviewing leading to "thick description" (p. 312); in place of objective life narratives, he suggested interpretive essays from the situated standpoint of the ethnographer. It was not the promise of objective, statistically reliable knowledge that imbued the educational ethnographer with credibility, but the kind of contoured understanding that comes from prolonged, intimate, and dynamic interaction with children. Thus to the battery of quantitative practices common to educational research were added the relatively exotic cadre of interviewing, participant observation, personal reflection, and literary analysis (Denzin & Lincoln, 1994).

With the founding of the Council on Anthropology and Education in 1970, educational anthropology gained considerable legitimacy in the university (Eddy, 1985), and by the early 1980s at least four distinct varieties had emerged: anthroethnography, socioethnography, psychoethnography, and critical ethnography (LeCompte & Preissle, 1993; Spindler, 1982). But qualitative accounts of classroom life were conducted not only by visiting ethnographers, but also by classroom teachers, who under the emerging rubric of action research were turning to a variety of more accessible researcher methods as a means of improving their own pedagogy (Cornell, 1969). Introduced by Kurt Lewin (Adelman, 1993) and his students as early as the 1930s, action research began as a series of quasi-experiments conducted in factory and close-knit community settings. Lewin was convinced that many of the everyday organizational problems he experienced could be solved through direct, democratic participation among all parties involved. The process began with "reflective thought, discussion, decision, and action by ordinary people participating in collective

research," explained Adelman (p. 8), and the results of the implemented actions were generally measured quantitatively through passive observation or surveys.

No doubt owing partly to the onset of World War II, but also largely to factors unknown, the momentum of Lewin's action research agenda was stalled until the 1980s (Lagemann, 2000). By then, the infusion of qualitative methods loosened action research from its quasi-experimental origins, and the movement became increasingly associated with discussion, observation, interviewing, and journal-writing. Among teachers engaged in classroom action research, Schratz (1993) noted a recurrent transition from a steadfast reliance on what he called the "traditional positivist approach" (p. 120) to research to an acceptance of the interpretive practices of critical inquiry, largely effected by the realization that many of the classroom experiences of interest were unlikely to be adequately represented through traditional survey techniques.

A more recent response to positivism has come in the form of Evers and Lakomski's (1991, 1996, 2000) program in naturalistic coherentism. Central to this program is the notion of coherence as an epistemic value. Following Kuhn's (1962) criticism of logical empiricism, Evers and Lakomski acknowledge that observation does not necessarily lead to the assent of one theory over another—in other words, observation is not, contra Feigl, the independent arbiter of competing theories in science. Observations may, for example, result in a theory being slightly modified but not rejected; observations may even confirm two or more theories. Therefore, the foundation of a science of education cannot be the so-called "brute facts" of objective science on which educators of earlier generations relied so heavily. "The upshot," remark Evers and Lakomski (2001), "is holism, the view that an administrative theory and its purported empirical evidence are part of a whole body of knowledge, or global theory, that is adjudicated by coherence criteria of excellence" (p. 502). From the perspective of naturalistic coherentism, how well a body of knowledge hangs together as a coherent whole—from its generalized theoretical statements to its anecdotal observations—should stand as the primary concern among educational administrators choosing between competing frameworks. Regarding methods, Evers and Lakomski (2001) explain,

We are ... free to start with whatever qualitative or quantitative formulations of patterns, or regularities, or explanations, or understandings, hold the promise of suggesting order where there is apparent disorder. But during the process of theory building, these accounts will be successively winnowed, or developed, in ways that extend the scope of theory, while maintaining coherence with natural science. (p. 507)

In keeping with its naturalistic orientation, a preferred theory of educational administration, then, should accord well with established theories in natural science that speak to human learning and behavior as well as with experiential lore established in the context of daily administration. In this sense, Evers and Lakomski's (2001) program is Deweyan in the sense that it values active inquiry in naturalistic settings, leading to practical knowledge provisionally held and continually reconciled against larger frameworks. It is also Thorndikian in its suggestion that such practical knowledge should be reconciled against generalized scientific laws of human learning and behavior,

neuroscience in particular. Naturalistic coherentism attempts to bring together theory, even in its most generalized forms, with practice in all its situatedness. To borrow from Geertz (1973), it aims to wed experimental science in search of law with interpretive science in search of *in situ* meaning in an attempt to provide administrators with a body of scientific work relevant to their needs.

Situating Research Methods in Social Settings

At the base of all knowledge—all meaning—are humans, positioned in particular social relations, using language, literacy, and other symbolic forms to induce adherence (Perelman & Olbrechts-Tyteca, 1969), agreement (Rorty, 1987), conviction (Grimaldi, 1972), cooperation (Burke, 1990): in a word, to persuade. That humans occupy the focal position in this image represents a departure from the dominant world view created by Descartes over 300 years ago. In that world view, self-evidence provided the foundation for the prize of science: certain knowledge. By divesting himself of all knowledge that was merely plausible or probable, Descartes foregrounded self-evidence as the only reasonable starting point for a system of rational thought, and apodictic proof was touted as the only means of extending the certainty of self-evidence to other statements (Perelman & Olbrechts-Tyteca). Gone were the “prejudices of our childhood,” announced Descartes, “principles of which I allowed myself in youth to be persuaded without having inquired into their truth” (Durant & Durant, 1989, p. 638). All that remained were the clear, distinct statements that could be reduced to immediate perception: the axioms of Euclid’s *Elements*, for example, and the famous *Je pense, donc je suis*. Self-evidence was thought to speak for itself: It was essentially unmediated, certain, and thus beyond the realm of deliberation for Descartes, and on its immunity to interpretation Cartesian foundationalism rested and still rests today.

Antifoundational perspectives of the sort with which I open this section are, of course, not new. As early as the fifth century BC, the Sophists ardently defended the position that all human knowledge is provisional and inevitably the product of persuasion, Descartes’ self-evidence notwithstanding. Twenty-two centuries later, Italian professor of rhetoric Giambattista Vico returned to this point when he challenged Descartes’ philosophy. In “On the Study Methods of Our Time,” Vico argued that Descartes’s method, like all other human endeavors, rests on probability, belief, argument, and conviction rather than on infallible demonstrations of absolute truth; he thus named rhetoric the most suitable field for studying human knowledge, for, as Bizzell and Herzberg (1990) explain, “rhetoric takes probability seriously, understands the ways in which argument produces belief, and trains young people for responsible civic action, as Cartesianism does not” (p. 11).

Nor are antifoundational perspectives necessarily old. The notion that observations provide a noninterpretive basis for scientific knowledge had endured longstanding criticism (Dewey, 1916/1944; James, 1896/1992), but it was Hanson’s (1958) *Patterns of Discovery* that crystallized the issue. Based on the results of his now-classic psychological experiment in human perception, Hanson argued that observers’ background knowledge influences even their most immediate visual perceptions. Phillips (1985) explained the experiment.

Slides were made from cards selected from a deck, and these cards were projected for very short periods onto a screen in front of various observers. The slides were all correctly identified, except for one that was a trick slide where the card was given the wrong color (for example, a black six of hearts). Most commonly the observers in the experiment saw this trick slide as a blur, or they misidentified the suit of the card. (p. 41)

Hanson (1958) then generalized the results of this experiment to scientific knowledge production as a whole and argued from persuasive, empirical grounds that scientific observations are inevitably theory-laden and interpretive in nature, thus severely undermining “those philosophical positions that suggest knowledge is built up from a neutral or objective observational base” (Phillips, 1985, p. 41).

With the publication of Kuhn’s (1962) *The Structure of Scientific Revolutions* came further support for antifoundational perspectives. By portraying the history of science as a series of incommensurable paradigms connected through periods of crisis and revolution that resembled religious conversions more than a cumulative march toward certain knowledge, Kuhn effectively situated knowledge claims in the theories, assumptions, and practices of their sponsoring paradigms. Central to his thesis was the point that the methods of science drastically underdetermine forthcoming knowledge claims. “Observation and experience can and must drastically restrict the range of admissible scientific belief” wrote Kuhn (1997), “but they cannot alone determine a particular body of such belief” (p. 4).

More recently, Rorty’s (1999) appreciation for the role of persuasion in research has garnered particular attention. Rorty has admitted being surprised by the severe hostility that his ideas have received from popular audiences who have little or no interest in philosophy or rhetoric. “These denunciations [against Rorty] claim,” according to Rorty, “that unless the youth is raised to believe in moral absolutes, and in objective truth, civilization is doomed” (pp. xxviii-xxix). Indeed, few words are held in lower regard than *persuasion*: Replacing talk of proof with that of persuasion in all but the most devalued human endeavors seems generally to lead to the breakdown of constructive communication, especially with audiences—whether popular, scientific, religious, or otherwise—who approach their topic with a sense of reverence. Yet by *persuasion*, explained Grimaldi (1972), “we are simply accepting the inescapable fact that in all areas of human living there are large complexes of pre-existing convictions and assumptions within which we must attempt to speak to the other” (p. 4). Appreciating the role of persuasion in research does not devalue knowledge; it simply acknowledges, to borrow from James (1896/1992), that because “no bell in us tolls to let us know for certain when truth is in our grasp” (p. 478) no claim to knowledge is beyond deliberation. What we consider true, what we consider self-evident, what we consider absurd or natural or logical: None of this comes to us by unmediated means, regardless of how obvious it might seem to a particular arrangement of people of a particular place at a particular time. In fact it does not “come to us” at all. From Grimaldi’s perspective, knowledge production is a process of construing claims to the best of our abilities out of what we consider to be the convincing material at hand, and knowledge transmission is a process of convincing others

of the veracity of such claims. With Cartesian notions of self-evidence, logic, and method out of the way, the longstanding distinction between *episteme* and *doxa* becomes problematic, and the whole of inquiry and knowledge production materializes as the proper province of what Aristotle (1991) had limited to the political, judicial, and ceremonial spheres of Greek life, namely, rhetoric, the art of finding the available means of persuasion in a given case.

From this perspective, educational research becomes a strategic endeavor. Perelman and Olbrechts-Tyteca (1969) explain:

Every speech is addressed to an audience and it is frequently forgotten that this applies to everything written as well. Whereas a speech is conceived in terms of an audience, the physical absence of his [sic] readers can lead a writer to believe that he is alone in the world, though his text is always conditioned, whether consciously or unconsciously, by those persons he wishes to address. (pp. 6-7)

We inquire and produce knowledge while anticipating its function in the social world, also realizing that our claims will be evaluated against the procedures and norms considered relevant by our audience (Bazerman, 1994a). Audiences need not reconstitute the meanings we have intended: Claims meant to support one cause can be used to support another. Evidence that seems strong to an author can be interpreted as weak by a reader or by another author who embeds that evidence in a different intertext altogether (Bazerman, 1993).

Denzin and Lincoln (1994) indirectly acknowledge the textual underpinnings of educational research. In "Entering the Field of Qualitative Research," they remark, "Thus the narratives, or stories, scientists tell are accounts couched and framed within specific storytelling traditions, often defined as paradigms" (p. 3). Their table (p. 13) delineating the major features of five epistemic paradigms in educational research goes so far as to include a column on types of narration, including scientific reports, ethnographic fiction, fables, dramas, and scholarly essays. Less directly, in their discussion of the qualitative researcher as a sort of Jack-of-all-trades, they explain, "The *bricoleur* produces a bricolage, that is, a pieced-together, close-knit set of practices that provide solutions to problems in a concrete *situation*" (p. 2, my emphasis). It is important to realize that these traditions of storytelling and types of narration—genres in short—are sociocultural products of decades or centuries of historical conditioning. To borrow from Miller (1984), genres are stabilized literate responses to recurrent social exigencies, typified literate acts, according to Russell (1997), that "worked once and might work again" (p. 515). They represent not only regularity in textual features "on the page" but also regularity in the social interactions and situations mediated by those texts, regularity "off the page," so to speak, including shared beliefs, assumptions, purposes, motives, reader-writer relations, and configurations of trust and authority (Bazerman, 1988, 1994b, 1997; Little, in press; Miller, 1984; Russell, 1997). In short, texts fall into the same genre based not only on what they look like, but also on how they are used, and the decades-long articulation of that usage by modern scholars of rhetoric—from Bitzer's (1968) early statement on rhetorical situation to recent reformulations of context by Bazerman and Russell (2003) in the light of Soviet activity theory—represents an enormous and

promising resource for further elaborating in practical terms what we as educational researchers mean by *context* and *situation* vis-à-vis epistemic practice.

Given this perspective, several contours in the history of educational research come to the fore. The early-20th century challenge of mass education, for example, included important changes not only in student demographics, but also in the communicative practices required to accomplish the business of education. These in turn had an effect on what were considered appropriate research methods. The routine face-to-face interactions and relatively informal textual exchanges of the cloistered era of education no longer sufficed under the pressure of public scrutiny. By 1900, educational leaders were pressed to defend their decisions to geographically distant and largely distrustful audiences who had little or no expertise in educational matters. As such, what began to accomplish the communicative work of educational administration were formal texts such as scientific articles and cost-benefit reports, the standardized textual elements and explicit norms of interpretation of which assisted in the reliable reconstitution of meaning by the relevant parties. These texts served the important function of communicating evidence of educational quality in terms that their intended political and public audiences found convincing and useful. Accordingly, the research methods that were compatible with the features of these formal texts were legitimized, and those that were not were largely marginalized—in education.

I emphasize *in education* to point out the fact that research methods per se are neither good nor bad: Only by understanding their function in particular communities can we begin to evaluate them and then only in terms of their usefulness in responding to the exigencies at hand. For the academic psychologists who led the “defection” from controlled experimentation in the 1920s, qualitative observation and reporting turned out to be a viable paradigm, given their interest in better understanding the lives of children, the relatively high degree of credibility they enjoyed, the relatively low-stakes nature of their work, and their ability to find or found social networks that valued their work. For the educational administrators of the day, however, whose goals and audiences were essentially fixed by recurrent public scrutiny and criticism that left little room for interpersonal trust, the naturalistic methods of academic psychology offered little promise of success. Understandably, their configuration of legitimized and marginalized genres and attendant research methods differed from that of the academic psychologists; not only were the two groups responding to different demands from anticipated audiences, they were operating at largely different degrees of autonomy.

In this light, it is understandable why Thorndike’s version of educational science was generally preferred to Dewey’s: For despite the marked shifts in education at the turn of the century, Dewey remained committed to a highly contextual form of educational research that framed knowledge consumption and decision-making as local or at times personal affairs. One of the primary purposes of Dewey’s Laboratory School was “to exhibit, test, verify, and criticize theoretical statements; [and] ... to add to the sum of facts and principles in its special line” (Lagemann, 1989, p. 197). Yet in Dewey’s framework, “testing” was accomplished to the satisfaction of the people who worked at the Laboratory School in the spirit of democratic participation. Such an intimate

scope of social interaction encouraged the production of knowledge that was qualitative in nature, highly dependent on the shared expectations and norms of the immediate audience, and thus not easily communicable through formal texts or otherwise to the distant audiences that were becoming increasingly relevant to educational funding.

Thorndike's research methods, on the other hand, produced a kind of knowledge that aligned exceedingly well with the procedures and norms considered relevant by the new audience of education. Whereas Dewey gave priority to naturalistic settings like those offered at his Laboratory School, Thorndike's statistical research program required controlled experiments in which all the contextual cues that Dewey had privileged were subdued. Studying animal behavior was one way to reduce human learning to its core processes; experimental designs in controlled laboratory settings that eschewed all but the pivotal stimuli were another. By these practices, researchers could come to know the essential nature of human learning as well as the most efficient ways to form, reinforce, and dissolve the stimulus-response bonds that would literally cause socially appropriate behavior. What Dewey sacrificed in rigor, however, Thorndike sacrificed in educative relevance. His redefinition of the phenomena of education in quantifiable terms required him, like Fechner and Galton before him, to jettison much of what Dewey considered the essential material of education (Hornstein, 1988). Speed became a cardinal index of ability, no doubt influenced by the efficiency movement, but also owing to its amenability to quantitative measurement and objective decision-making: Good readers were those who could read fast; good teachers were those who could complete an educational objective in the least amount of time. However, crucial to Thorndike's success was the fact that these quantitative figures were easily communicable to distant audiences through formal texts, and what is more, generally interpreted by those audiences as intuitively obvious indices of educational quality and therefore seen as palpable decision points for educational administration. This enabled fair, seemingly context-free, though by no means pedagogically meaningful decisions to be made at a time when educational leaders were under severe political pressure to do so. In short, Thorndike's epistemic practices offered precisely the kind of textual artifacts that educational administrators needed to succeed in the increasingly relevant sphere of public deliberation.

Another set of textual contours not addressed in the historical literature surrounds the emergence of interpretive ethnography. Above I noted that with Geertz's (1973) ethnography came the addition of interviewing, participant observation, personal reflection, and literary analysis to the existing battery of descriptive, correlational, and experimental practices generally used in empirical research. Yet it would be a mistake to assume that all these epistemic practices were meant to be used in the same communicative situations, that they implied even roughly the same audiences, or that they placed similar demands on consumers of knowledge. By far the majority of the quantitative literature, from Thorndike through Campbell and Stanley, although steeped in specialized knowledge implies a nearly universal audience of rational people and works to achieve consensus among them: The literature operates from the standpoint that the goal of any scientific article is to encourage all rational

people to come to the same conclusion as the author, to induce mental adherence so to speak—not by trusting the author, but by judging for oneself through rational processes which conclusions are merited and which are not, and toward this end the statistical epistemic practices of educational psychology, coupled with the emergence of conventions to constrain the interpretation of data, worked effectively.

In contrast, Geertz's (1973) *The Interpretation of Cultures* makes no promise of interpretive stability. In fact he considers the wide variety of candidate meanings invocable by thick descriptions as a strong point, as multiple ways of engaging the reader, multiple ways of connecting the audience to the phenomenon under examination. That the product of Geertz's nuanced epistemic practices—a descriptive essay—relies heavily on individuals' scientific imagination and powers of clarity implies a social setting in which the prospect of widely divergent reconstitutions of meaning is of negligible consequence or perhaps even encouraged altogether. This is not a genre that leads to high-stakes decision-making, nor should it given Geertz's professional standing as an academic cultural anthropologist and his vision for interpretive ethnography. Others may have higher-stakes social settings in mind for the enactment of interpretive ethnography; however, this scenario would require a tremendous degree of trust on the part of all people involved: trust not only in an underlying harmony among their individual powers of clarity and scientific imaginations, but also in the impeccable research skills of the ethnographer.

Conclusion

"The roads to knowledge are many," wrote Eisner (1985) in his preface to the *Eighty-Fourth Yearbook of the National Society for the Study of Education*. Indeed, the proliferation of research methods in education is here to stay, and we need not only to bring these practices into the spotlight of formal examination, but also to find improved ways of thinking about and choosing among them: ways that move beyond the longstanding categories of qualitative and quantitative research to bridge researchers rather than isolate them.

One way to complement traditional ways of framing research methods is to recognize the practical reality that as researchers, we are called to write: We are called to produce certain kinds of texts in certain social settings for certain kinds of audiences. "Whenever we notice ourselves as writers," noted Bazerman (1994a), "we find ourselves on a spot, a spot that seems to demand of us that we write" (p. 10). This spot is no less real for the educational researcher than it is for the essayist or novelist. As such, a communicative situation becomes an essential factor in choosing appropriate research methods, which in turn implicates such concepts as audience, style, credibility, and genre as integral to the business of educational research. This is a larger issue than simply choosing a venue for publication. By looking beyond publication to the forms of social action we hope to sponsor by our published writing, and by examining the social settings that typically embed those forms of action, we can improve the potential efficacy of our work and move beyond the limiting notion that our work is complete when it has been accepted by the journal editor. In their *Writing Selves/Writing Societies*, Bazerman and Russell (2003) express this perspective succinctly:

Writing is alive when it is being written, read, remembered, contemplated, followed—when it is part of human activity. Otherwise it is dead on the page, devoid of meaning, devoid of influence, worthless. The signs on the page serve to mediate between people, activate their thoughts, direct their attention, coordinate their actions, provide means of relationship. It is in the context of their activities that people consider texts and give meaning to texts. And it is in the organization of activities that people find the needs, stances, interactions, tasks that orient their attention toward texts they write and read. (p. 1)

By recognizing and appreciating this, we can begin to choose research methods based on the work we intend to accomplish rather than continuing to allow that choice to be made through tacit enculturation into the epistemic paradigms of our mentors.

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