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Ethnographic Responsibility Without the “Real”

Over a decade ago, Harry Wolcott (1990) wrote about qualitative researchers' preoccupation with persuading readers of the veracity of a study's description and interpretation as distracting researchers from the more important task of communicating understandings. He rejected qualitative inquiry's quest for certainty, explaining that "our efforts at understanding are neither underwritten with, nor guaranteed by, the accumulation of some predetermined level of verified facts" (p. 147). More compelling than discovering a verifiably "found world," he argued, is a question of what one does with what one "finds" there: "I do not go about trying to discover a ready-made world; rather, I seek to understand a social world we are continuously in the process of constructing. . . . Validity stands to lure me from my purpose by inviting me to attend to facts capable of verification, ignoring the fact that for the most part the facts are already in" (p. 147). Wolcott positioned himself, and qualitative inquiry in general, as participants in interpreting and constructing social worlds rather than as recorders of verified data. In this essay, I take up Wolcott's discussion of the quest for certainty, a quest to represent a "real" by assuring readers of the veracity of the "real" and one's interpretation of it. I argue that this search for a "real"

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limits qualitative inquiry's potential to break with predictability and to offer new readings of its subjects of inquiry. I call for and elaborate on the possibilities engendered by ethnographic and qualitative inquiry that places itself "in pursuit of a less comfortable social science" (Lather, 1993, p. 673) by acknowledging uncertainty and venturing new interpretations.¹

I share Wolcott's skepticism of the scientism often underlying preoccupations with verifying that what one represents is indeed "real." However, I also understand a need for researchers and readers of research alike to be attentive to the vicissitudes of fieldwork, concerned with consistencies and inconsistencies within and across forms of data, and mindful of the implications of researcher perspectives and relations with participants in inquiry—all of which constitute the "facts" and meanings that researchers and readers alike must grapple with in the textualization. In this regard, what is often most interesting are textual and lived moments of doubt, uncertainty, and irresolvability in the practice of research. These moments highlight the idea that, like life, qualitative inquiry is fiction, in the sense that it is made or constructed, but not in the sense that it is pure invention, lies, or imaginings. In other words, qualitative inquiry has a grounding in "real" events and "real" lives, but learning about and representing events and lives is a process of constructing others' constructions of the constructions of the world. Geertz (1973) called this "explicating explications. Winks upon winks upon winks" (p. 9). These winks, either the "original" or researchers' descriptions and interpretations of them, are never fully representable in language or verifiable as faithful copies of a "real." We gesture to them as best we can.

In this essay, I consider the goals and uses of ethnography and qualitative inquiry in higher educational research as they are limited by commitments to verifying data and representing the "real" and argue for broader understandings of the practice and uses of such research. In particular, I am concerned that our field's practical orientation creates pressures for verification of data, presuming that research can represent a "real" and that this "real" is essential to useful research. I then examine elements of verifying a "real," including member checks, triangulation, and transferability, and relate them to research purposes. I argue that if we limit ourselves to foreseeable utility (as we presently define and understand problems) and to representing a "real," we limit our research to repeating itself and to repeating the status quo. Finally, I advocate the creation of speculative research that concerns itself less with verifying the "real" it represents and more with opening new paths for thought.

By a "real," I refer primarily to a knowable world that can be described and explained. Drawing from van Maanen's (1988) work, Patti

Lather (1991) has described “realist tales” in research as “those stories which assume a found world, an empirical world knowable through adequate method and theory” (p. 128). Ethnography that subscribes to realism conceals researcher doubt and often displays “interpretive omniscience” (p. 133) as authors authoritatively organize quotes and footnotes that would represent and explain what they “find.” As Belsey (1980) describes, belief in a “real” and textual devices of realism are aligned with liberal humanism, which “assumes a world of non-contradictory (and therefore fundamentally unalterable) individuals whose unfettered consciousness is the origin of meaning, knowledge and action. It is in the interest of this ideology . . . to present the individual as a free, unified, autonomous subjectivity” (p. 67). To present the “real” elides the ways in which subjectivity, experience, and the meanings of actions and events are contradictorily constructed in ways often not accessible to researchers, or to participants themselves. Moreover, the representation of a “real” takes place in a declarative mode, which would instruct or impart knowledge of stable subjects to a reader whose own position is stabilized (Belsey, 1980, p. 91).

A fundamental assumption underlying my discussion is that the degree to which researchers worry about verifying and representing a “real” is related to how they conceptualize the purposes of and audiences for their research. For example, if one wishes to produce useful knowledge, or knowledge that might be applied in practice in multiple contexts, verification may take on a degree of importance. Canonized ideas of rigorous fieldwork, thick description, triangulation, and so on, become means of persuading readers of the veracity and potential applicability of one’s research. But if one wishes to evoke scenes in order to challenge readers’ sensibilities, or question commonsense, one might worry less about verifying data or describing thickly and attend instead to what Rosaldo (1989) has called interpretive force. In this sense, to talk about a “real” is to talk about the uses one envisions for research and about the rhetorical devices researchers use as they address imagined or intended audiences. Thus, I begin my exploration of uses of verifying a “real” in the context of purposes to which research is put, highlighting what I perceive to be the practical emphasis of higher educational research.

Research Purposes/Interesting Research

In a typology that has held interest for researchers and educational theorists alike, Habermas (1971) outlined three purposes, what he called interests, underlying knowledge production. The first, technical interests,

which he aligned with empirical-analytic sciences that deduce hypotheses from the empirical world, produce knowledge that expands technical control to produce predicted effects. The second, practical interests, are embedded in a historical-hermeneutic tradition that seeks to interpret and understand meanings in order to orient action within common traditions. This type of research is less concerned with producing predictable effects than with developing deep understandings to guide action within a community or context. Habermas argued that both technical and practical-hermeneutic interests can participate in perpetuating the status quo, simply adding to efficient administration or action within a system rather than questioning its assumptions. In contrast, the last, emancipatory interests, have a critical orientation that seeks to “free consciousness from its dependence on hypostatized powers” (p. 313) and to cultivate “enlightened action” (p. 316) for social change. Following Habermas, many critical theorists would advocate emancipatory knowledge as an ideal for research. Weis and Fine (2000), for example, offer a broad statement that a “purpose of social inquiry at the turn of the century is not only to generate new knowledge but to reform ‘common sense’ and critically inform public policies, existent social movements, and daily community life” (p. 60). Emancipatory research has an appeal in that it invites those research affects to think and act differently. Yet it is (pre)defined by its own sense of its usefulness, albeit a usefulness that seeks change rather than stasis. In this essay, I suggest that usefulness places demands on researchers to verify a “real” and thus limits open speculation, the depiction of uncertainty, and the creation of new concepts.

Higher educational research spans and combines all three orientations to research, but has primarily committed itself to “useful” research that has technical and interpretive interests.² Underlying much, if not most, research in the field is a belief, or hope, that if inquiry verifies its data as accurately representing something “real,” it can inform readers (or participants in research) about a concrete problem or situation, even provide answers or solutions for application in similar contexts. This hope perpetuates and is perpetuated by what I call a “theory of the accretion of knowledge”—the idea that a body of research can progressively cohere to produce answers to difficult questions of policy or practice.³ The *zeitgeist* to cure through research asks inquiry to create correct knowledge by representing verifiable stories and subjects in the hope that a glimpse of the “real” will guide readers’ thought and action. Richardson (1997), for example, has written of the temporality of the very structure of much research: “Just listen to us: theory (literature review) is the past or the (researcher’s) cause for the present study (hypothesis being tested), which will lead to the future—findings and implications (for the

researcher, researched, and science)” (p. 77). According to this thinking, research is supposed to create verifiable knowledge in order to point or lead to some sort of change. I question these linear, teleological understandings of inquiry’s purposes based on a contention that the “real” about which research would produce knowledge is not necessarily desirable and is necessarily elusive to qualitative inquiry.⁴ What if higher education understood more of its inquiry as part of a conversation that cannot conclude with certainty?

Interests in Verifying Data

Researchers establish their depictions and interpretations of the “real” through processes of verification, a term closely linked to validity, which originated in the field of psychometrics (Kvale, 1995, p. 22).⁵ Indeed, Kvale (1995) defines verification as “the concrete analyses of validity in the knowledge produced, a reflected judgment as to what forms of validation are relevant in a specific study, and a decision on what is the relevant community for a dialogue on validity” (pp. 27–28). A well-respected methods textbook, LeCompte and Preissle’s (1993) *Ethnography and Qualitative Design in Educational Research*, offers a place to begin a discussion of verification and validity as they relate to the purposes of research. Following a general definition that “validity is a category of truth. It refers to the kinds of qualified and tentative truths that scientists seek” (LeCompte & Preissle, 1993, p. 321), the authors offer a range of definitions:

Validity is first a term used in everyday language to convey a number of common-sensical meanings. Sometimes people mean accurate when they use the term *valid*. . . . A second, common-sensical meaning for the term *valid* is justifiable, warrantable, and hence believable. . . . Other times people mean that something is logically correct when they refer to it as valid. . . . Valid is also used as a synonym for sanctioned or authorized. . . . Finally sometimes people mean something is effective, for whatever reasons, when they label it as valid. (p. 322)

Typically, verifying data has relied on two forms of validity: “Internal validity is the extent to which scientific observations and measurements are authentic representations of some reality. External validity is the degree to which such representations may be compared legitimately across groups” (LeCompte & Preissle, 1993, p. 323). As I will discuss, each of these meanings comes into play in considering verification of a “real,” whether of data or of interpretations of data.

Eisner and Peshkin (1990) connected concerns with verifying data to realist epistemologies and correspondence theories of truth:

One of the fundamental beliefs of researchers is that efforts to describe the world become increasingly valid as descriptions correspond to the world described. Validity, in a basic sense, pertains to the congruence of the researcher's claims to the reality his or her claims seek to represent. Valid interpretations and conclusions function as surrogates through which readers of research reports can know a situation they have not experienced directly. Thus a valid description or interpretation of a state of affairs is closely aligned to matters of truth. Truth, in turn, is related to matters of correspondence. Correspondence, in turn, is related to a distinction between a subjective self and an objective world. What we want, in traditional terms, is an account that tells the truth about some objectively described state of affairs. Such an account would be a valid one. (p. 97)

Their discussion of correspondence, truth, and objectivity gets at a fundamental tenet of "realist" research—that there is a world that can be known and described through appropriate use of appropriate procedures. Research can get it "right." It can create a window onto, or mirror of, some reality. Yet Eisner and Peshkin's definition problematically conflates description and interpretation. While many rightly argue that description is inherently interpretation, these two might be usefully separated for the purposes of discussion. Does research wish to verify data ("facts") or interpretations? To verify "facts" is to take one path and to verify interpretations is to take another. And if research wishes to verify interpretations, whose (participants' or researchers') count? To reframe the curricular question, "What knowledge is of most worth?," one might ask of the goals of research, "Whose interpretations are of most worth?"

Because a strength of qualitative inquiry is said to be its search for meaning, I focus on its interpretive aspects, which some, such as John Smith (1993), place as "the central focus of social and educational inquiry" (p. 184). Distinguished from description, interpretation consciously includes "meanings, intentions, motivations, and reasons that stand behind the expressions and actions of human beings" (p. 184). While these elements of interpretation can span the interests Habermas delineated, it is important to be explicit about the purposes of interpretation. Smith distinguishes among three interpretive projects that bear on the purposes of qualitative research. The first, validation hermeneutics, seeks to establish an author's (or interviewee's) meanings. These meanings have an independent existence and become an external referent for assessing an interpretation's accuracy. While an interpretation of meanings can not be verified, it can be falsified by studying standards of the moment and intentions in context. The second, critical hermeneutics, seeks to understand speakers' and authors' meanings as they are constructed through ideology in historical conditions. It assumes that the inquirer can assess meaning better than the speaker and can clarify the

conditions that create (mis)understandings. Interrogation of the workings of ideology ideally leads to emancipatory praxis. The third, philosophical hermeneutics, critiques assumptions underlying validation and critical hermeneutics that “knowledge is a matter of accurate representation and that we can somehow extract ourselves from our own historical traditions to criticize those traditions from this ‘outside’ standpoint” (p. 194). Following Gadamer’s (1989) contention that meaning is not to be discovered, but is created in the act of understanding, it positions inquiry as “a practical and moral activity, not an epistemological or technical activity” (p. 197). Inquiry becomes a form of conversation in which competing interpretations can not be adjudicated by appeal to an external referent. Rather, potential meanings are added to the deliberations of a community of discourse. Even as the first two are not necessarily predicated on verification per se (falsification may be more appropriate), they presume some sort of accurate representation and interpretation of reality. They are based in interpretive closure. Philosophical hermeneutics, on the other hand, recognizes that verifying meanings (whether a researcher’s or a participant’s) may be less desirable than encouraging the creation of new meanings. This stance admittedly leaves open numerous questions, for all meanings are certainly not necessarily interesting or desirable, but it points to a direction where utility (beyond the creation of some meaning) is not presumed beforehand and a verified real is not the central aim. Moreover, it opens the locus of meaning from the authority of the speaker (as in validation hermeneutics) or the inquirer (as in critical hermeneutics) to a relational, even contradictory, space that is less certain.

Limiting Interests and Purposes

To demonstrate some of the implications of limiting research to representation of the “real,” I examine three tenets of “naturalistic inquiry” that have come to constitute something of accepted practice in higher educational research that is qualitative, ethnographic, or naturalistic: negotiation of outcomes (through member checks), triangulation, and transferability. Member checks and triangulation generally establish something akin to internal validity, while transferability substitutes for external validity. While each has participated to varying extents in legitimating qualitative research by offering new criteria for its evaluation, each is similarly complicit in limiting its potential by perpetuating the ideology of the accretion of knowledge through representation of a verifiable “real.” All three are advocated by many texts on research methodology, but my discussion draws from Lincoln and Guba’s (1985) pivotal

text, *Naturalistic Inquiry*. I do so out of no disrespect for these authors' work. On the contrary, I choose this text out of profound respect for its impact on educational research in general and its crucial role in making possible the rise of naturalistic, ethnographic, and qualitative research in higher education.⁶ At the same time, however, I am concerned that the field of higher education, perhaps in its own quest for legitimacy, has reified the concepts Lincoln and Guba offered and turned them into recipes for action rather than suggestions to be thoughtfully put to use and reworked according to context.⁷

In *Naturalistic Inquiry*, Lincoln and Guba developed epistemological and ontological justifications for a "naturalistic paradigm" of inquiry that outlined the incommensurability of positivist and naturalistic assumptions. They offered five axioms underlying naturalistic inquiry: "Realities are multiple, constructed, and holistic," "Knower and known are interactive, inseparable," "Only time- and context-bound working hypotheses . . . are possible," "All entities are in a state of mutual simultaneous shaping, so that it is impossible to distinguish causes from effects," and "Inquiry is value-bound" (p. 37). These axioms challenge verification of a fully knowable, found, or non-contradictory "real" even as the specific methods the authors describe as naturalistic hint at verifying a "real." Member checks, triangulation, and transferability in particular have been put to uses that would produce singular readings and answers to problems of practice rather than opening inquiry to multiple uses that are consonant with the very uncertainty and multiplicity naturalistic inquiry acknowledges.

Negotiating Outcomes or Complicating Readings

In a break with research practices that position a lone author as omniscient and unrelated to the shape of inquiry, Lincoln and Guba (1985) advocated negotiating the interpretation of data with participants. They explained:

Because it is their constructions of reality that the inquirer seeks to reconstruct; because inquiry outcomes depend upon the nature and quality of the interaction between the knower and the known, epitomized in negotiations about the meaning of data; because the specific working hypotheses that might apply in a given context are best verified and confirmed by the people who inhabit that context; because respondents are in a better position to interpret the complex mutual interactions—shapings—that enter into what is observed; and because respondents can best understand and interpret the influence of local value patterns. (p. 41)

Their call to consult with participants not only encourages egalitarian and non-exploitive research practices (consonant with those embraced

by many feminists) but also reminds researchers to honor the ways in which participants story their lives. However, the negotiation of outcomes through “member checks,” in which researchers share interview transcripts and/or analyses with participants (see p. 236), promises a true story through participant verification. Like validation hermeneutics, it does so primarily by privileging as a goal of research the representation of the emic, or insider, views that participants offer. This stance positions research participants as origins of meaning by granting them interpretive authority over a context. Respect for participants becomes conflated with verification through member checks, which allow “the investigator . . . to purport that his or her reconstructions are recognizable to audience members as adequate representations of their own (and multiple) realities” (p. 314). This verification of data is integral to producing “trustworthy” research, which must be so “in the eyes of the information sources, for without such credibility the findings and conclusions as a whole cannot be found credible by the consumer of the inquiry report” (p. 213). Rhetorically, credible data are produced, yet this is a verified representation in which the author cedes authority to participants and limits inquiry purposes to mirroring what its participants can know.

The member check can allow for clarification, explanation, or extension of questions and ideas, just as it can offer important insights into participants’ understandings of self and context. However, is the only goal of research to represent accurately the views and perspectives of those who participate in our studies? Must researchers and participants reach interpretive consensus? Or should researchers, as Britzman (1995) has said, “abandon the impossible desire to portray the study’s subjects as they would portray themselves” (p. 233)? Can researchers risk interpreting participants’ worlds both with and against them? In tracking her shift from studying experience to studying the costs of experience in her research into student teaching, Britzman recounts, “I wanted to move beyond the impulse to represent ‘the real story of learning to teach’ and attempt to get at how ‘the real’ of teaching is produced as ‘the real story’” (p. 232). Such a shift entails “acknowledg[ing] the differences within and among the stories of experience, how they are told, and what it is that structures the telling and retelling” (p. 232). This is a difficult project that must inquire into the workings of discourses that intersect and collide in specific locations to produce the “real” that inquiry would study. It is also a project that runs the danger of constructing an all-knowing critical researcher whose “true consciousness” enables critique that is invisible to research participants. Yet the interrogation of the production of experience can signal new purposes for member checks that

neither locate authority in research participants nor demand a seamless interpretation by the researcher.

If the member check became another source of data, inquiry might find another resource for complicating data and interpretations by studying participants' constructions of their own and researchers' constructions. When sharing interview transcripts or interpretations with participants, researchers might ask: What interpretations do participants take comfort in? Which do they take issue with? What do I as researcher take comfort in or issue with? How? What can be learned from these responses, from this dialogue about the "real?" To honor participants' narrations while reading beyond them is no easy ethical task, but the points of consensus and dissensus that a dialogic rendering of member checks can offer creates a more polyphonic text than one that verifies the accuracy of data and interpretations. Such a text asks readers to navigate among the perspectives and contradictions represented.

(Mis-)Aligning Evidence

The credibility of thorough research has long been predicated on triangulation, which constitutes part of both fieldwork and analysis. Lincoln and Guba (1985) described triangulation as an ongoing means of verifying what one learns: "As the study unfolds and particular pieces of information come to light, steps should be taken to validate each against at least one other source (for example, a second interview) and/or a second method (for example, an observation in addition to an interview). No single item of information . . . should ever be given serious consideration unless it can be triangulated" (p. 283). Triangulation acknowledges the potential for variability in a participant's narratives across interviews, in the relations of a participant's words to his or her actions, across multiple participants' words and actions, or between what is said and done and artifacts and documents. In this sense, triangulation asks researchers to be attuned—both in the field and as they interpret data—to the multiple ways participants construct and act in the world. Triangulation, then, could be understood as a means of highlighting contradictions and puzzlements, attending to nuances in the multiple contexts that comprise the inquiry, and representing that which does and does not fit. Instead, triangulation has often been taken to suggest a search for consensus among multiple sources of data that verify each other, while single, anomalous sources are to be cast aside. Such verification, or alignment of evidence, in a search for the places in which sources converge to demonstrate a point, limits research to the fixity and certainty of verifiable "facts." Despite its potential for urging attention to variability, triangulation can instead fix what is confirmed as knowable. There are

times that a search for convergence is useful in revealing repetitions and habits at work in contexts. However, to limit inquiry to the representation of singularity (and this includes the search for “confirming” and “disconfirming” evidence that some methods texts advise) is to present non-contradictory moments and unified subjects “who say what they mean and mean what they say” (Britzman, 1995, p. 230).

Together with member checks, triangulation as verification constitutes a form of persuasion of the researcher’s interpretation through representational realism. Stronach and MacLure (1997) characterize realist writing as “aim[ing] to resolve contradictions, smooth over inconsistencies and achieve a sense of closure. In order to be revelatory, therefore, realist texts have to conceal: they have to iron out inconsistencies, establish coherence and insinuate a shared point of view between reader and writer that convinces the former that both see the world in much the same way” (p. 53). Triangulation would create a credible and unchanging “real” that elides the ways in which the meanings of actions and events are contradictorily constructed in ways often not accessible to researchers or participants. In fact, in a comment that many researchers seem to have ignored, Guba and Lincoln (1989) later retracted their emphasis on triangulation, suggesting that it “carries too positivist an implication, to wit, that there exist unchanging phenomena so that triangulation can logically *be* a check” (p. 240).⁸

What if, in cases in which the evidence doesn’t line up, researchers used triangulation to complicate? Like the complication of the member check, such analysis would point, however speculatively, to the contradictions that constitute a scene, a subjectivity, an event. Equally importantly, it would point to the very real messiness that confronts researchers in the construction of meaning and would admit to the impossibility of verifying data or interpretation as “real.” Rather than searching for the triangular point at which three lines meet, and thus creating an interpretation that represents a seemingly coherent and verifiable world, researchers might look for multiple convergences and divergences in their data—and admit to their own interpretive uncertainty. Placing perspectives and actions into relations that aren’t always neat highlights the contradictions, movement, and change subjects live out within and across contexts. To study the ways in which scenes and subjectivities are dynamic, fluid, and inconsistent offers access to the workings of the discourses of social and institutional worlds, how they act on and are acted on by subjects. Inquiry that juxtaposed contradictions, consistencies, and inconsistencies would not offer readers the authority or coherence of the “real” but multiple “perspectives [that] cannot necessarily be added together into a unified summation” (Rosaldo, 1989,

p. 93). Readers would then encounter texts that call on them to engage with conflicts, the dynamics of construction, and participants' and researchers' incomplete interpretations.

Unfitting Contexts

In a move designed to relieve qualitative inquiry of positivism's criterion of the generalizability of research, Lincoln and Guba (1985) proposed "transferability," a construct parallel to external validity but one that is sensitive to the specificities of context. Transferability refers to the degree to which what is learned about one context may apply to another. It is, they wrote, "a direct function of the *similarity* between two contexts, what we shall call '*fittingness*.' Fittingness is defined as the degree of congruence between sending and receiving contexts" (p. 124). Transferability assumes that researchers can not make claims beyond their specific cases and instead must represent the specific through thick description. Thick description offers readers who may wish to transfer "working hypotheses" (p. 124) to other contexts "a base of information appropriate to the judgment" (pp. 124–125) of the fit between contexts. Writers, then, have a responsibility to offer detailed description, while readers have a responsibility to attend to the uniqueness of two contexts in order to find similarities that allow for application.

Transferability has served several key functions for research. It has offered an alternative to claims that the only valid research is that which can claim to generalize and relieves writers of the burden of verifying the representativeness of that which they study. By placing responsibility on writers and readers of research to attend to specificity, it has encouraged the writing of rich texts. At the same time, however, transferability's presumption of application in like contexts attaches it to a belief in knowledge as progress and to a search for a descriptive, if not interpretive, "real." Although Lincoln and Guba referred to the construction of "working hypotheses" (p. 124)—what I take to mean no more than tentative ideas drawn from one context to another, in a practical epoch that seeks to cure, transferability promises a sort of generalization to similar contexts.

If it is to apply to like settings, transferability's "fittingness of context" needs to verify that it has represented a clearly defined and bounded "real." In its efforts to make sense of its objects of study, ethnography has long had a propensity to favor bounded arenas in time and space, such as rituals, classrooms, university events, or academic years, as objects of study. These boundaries make subjects manageable and seemingly comparable—and can promise "useful" research. However, to mark off the limits of a context reduces its complexity, making

for representations that “liberate . . . events from the untidiness of everyday life” (Rosaldo, 1989, p. 12). Just as there is not a single meaning behind a word, a “given” time does not have behind it a simple, linear chronology; a “given” space is not constituted by the self-evidence of what is found there (Visweswaran, 1994, p. 11). Each is populated by traces of past and present discourses. Seemingly bounded points in time and space intersect with multiple times and spaces, yet these intersections can not always be evidenced neatly.⁹ They must be interpreted speculatively. If attached to that which can be found or known in a context, transferability can not take into account the plural and contradictory forces underlying the constitution of subjects of study in overlapping times and spaces. A literal interpretation of the concept of transferability leaves researchers confined to literal readings of confined spaces. It leaves readers confined to an idea that they have gotten the “real” story by reading accurately represented emic views or to an idea that the boundaries researchers draw tell a whole story.

A reworked notion of transferability that did not depend on a thickly and accurately described context would take the potential uses of inquiry beyond like contexts, to contexts whose likenesses will never be fully knowable. Researchers need not define their own understandings of their research or the ways they textualize inquiry according to what they imagine to be its implications for readers in similar settings. Rather than orienting specificity to the use of “findings” in parallel contexts, the value of specificity may lie precisely in speculating about unexpected connections, relations, and complications. These connections, which will be necessarily incomplete, tentative, and unverifiable, are not in themselves transferable as applications; they are transferable, however, as invitations to readers to think differently about altogether different contexts. Inquiry may be most useful by simply offering new ways of thinking and interpreting. In this way, inquiry can understand itself as offering narratives and interpretations that readers engage with in unexpected ways, including “application” to or speculation about contexts that bear no “objectively” apparent similarities. Inquiry and interpretation, then, open themselves to multiple conversations rather than closing themselves off as answers to singular problems.

With this altered view of transferability, which understands narrative and narrative knowing (see Bruner, 1985; Rorty, 1983) as more fluid than literal definitions of transfer allow, inquiry leaves the appropriation of its narratives to its readers to take from them what they will in the contexts they choose. Like the subjects of research, readers of research dwell in multiple communities, times, and spaces—and their knowledges, which they put to use in unpredictable ways, move with them

from context to context to create novel actions. This indeterminate “usefulness” that I argue is a viable goal for inquiry follows de Certeau’s (1984) idea that “stories ‘go in a procession’ ahead of social practices in order to open a field for them” (p. 125). In other words, if inquiry and narrative do not confine themselves to what is presently thought to be possible or thinkable, to the givens of verification, or to looking for answers to immediate questions, they might participate in constituting altogether new realms of the possible.

In suggesting a complication of the uses to which verifying data is put, I am arguing for an unfixing of the subjects of inquiry—participants and their perspectives, verifiable and coherent evidence and interpretation of the “real” that lead to one point, and knowledge confined to defined spaces, times, and subjects. Ultimately, this is also an unfixing of purposes. However, this unfixing of purposes is made difficult by inordinate attention to “legitimate” procedure.

Verifying and Changing Popular Demand

I am interested in encouraging less predictable higher educational inquiry that takes responsibility for engaging complexities whose implications cannot be known in advance and that asks its readers to participate in thinking through those complexities and implications. LeCompte and Preissle (1993) comment, “The goal of research is a search for truth. Ever since human beings began studying themselves and their activities, they also have been judging the results of these inquiries” (p. 315). Unfortunately, researchers have been judging the results on the basis of what is confirmed or verified, what is tied up and closed down, or what might be “useful,” rather than on the basis of what research might open or make possible. As numerous writers have pointed out, researchers’ and readers’ criteria for evaluating the value and validity of research vary across disciplinary socialization and theoretical orientations (Kvale, 1995; LeCompte & Preissle, 1993). Those who value objectivity privilege rigorous procedures of data collection and analysis; others, such as “constructivists,” seek negotiation and dialogue with participants; and some, such as critical theorists, seek the critique of oppressive ideologies, emancipation, and action (see LeCompte & Preissle, 1993, pp. 324–329). Or readers may evaluate credibility and authority through the seeming truthfulness of claims, the plausibility (or warranted assertibility) of claims, or the usefulness of research to theory, practice, or policy. Verification (of data or interpretation), then, can establish credibility and authority in the context of a discursive community.

In his reformulation of “validation as the social construction of knowledge,” and hence as something that changes as part of a conversation, Mishler (2000) argues, “The key issue becomes whether the relevant community of scientists evaluates reported findings as sufficiently trustworthy to rely on them for their own work” (p. 120). As such, he draws on a Kuhnian notion of communities of inquirers who share assumptions and practices that constitute a tradition that defines acceptable research. However, the idea of community standards creates a paradox for research, as Krizek (1998) has noted:

Scholarship, with its emphasis on acceptable forms and formats, is conservative, yet its purpose is to generate the new, the innovative, and the inventive. . . . As we duplicate the accepted procedures of our disciplines, those into which we have been socialized, we correspondingly limit our potential for advancing the personal, the creative, or, ultimately, a truly innovative perspective. (p. 91)

Repetition may circumscribe thought. In particular, Kvale (1995) has remarked that “A strong focus on validity in research may foster an emphasis on testing and verification of knowledge rather than on exploration and creative generation of new knowledge. The issues of control and legitimation come to dominate over and hamper creativity and production of new insights” (p. 36). A fundamentally modernist approach to locating a research question and methods in relation to established scholarship and perceived (if not canonized) problems and procedures confines inquiry to continuity with tradition to legitimate itself. Seidman (1998), for example, has written of a mechanistic proceduralism in many dissertations, which elaborately depict audit trails and detail methods of triangulation. Rejecting such methodological universalism, he says, “What are needed are not formulaic approaches to enhancing either validity or trustworthiness but understanding of and respect for the issues that underlie those terms. We must grapple with them, doing our best to increase our ways of knowing and of avoiding ignorance, realizing that our efforts are quite small in the larger scale of things.”¹⁰ This strength of tradition and its malleability mean that a paper delivered at an annual meeting such as the Association for the Study of Higher Education (ASHE), a sporadic journal article such as this, a panel, or a few conversations are not enough. It means that classes in research methods, dissertation committees, and researchers must encourage and incorporate more complexity into what it means to conduct and represent research. This need not mean abandoning the established, but it does mean extending it by questioning its utility at all times and pointing to alternative methods and purposes—and the possibilities they create.

Implication and Imperfection

To encourage inquiry to create open texts that invite readers to participate in the creation of meaning is to ask it to let go of its search for certainty and certain purposes. Texts that exceed the boundaries of what can be verified ask readers to take responsibility for thinking through, with, and against research. This indeterminate, relational nature of inquiry is articulated in Stephen Tyler's (1986) imagining of ethnography as refusing functions some would claim for it, such as the accretion of knowledge or critical intervention:

Defined neither by form nor by relation to an external object, it produces no idealizations of form and performance, no fictionalized realities or realities fictionalized. Its transcendence is not that of a meta-language . . . nor that of a unity created by synthesis and sublation, nor of *praxis* and practical application. Transcendent then, neither by theory nor by practice, nor by their synthesis, it *describes* no knowledge and *produces* no action. It transcends instead by *evoking* what cannot be known discursively or performed perfectly. (pp. 122–123)

Evocation, he says, “aims not to foster the growth of knowledge but to restructure experience” (p. 135). Rather than seeking accurate representation, theory-building, or critical change, inquiry becomes an ethical project that implicates its participants in relations of here and there, now and then, reader and writer, writer and subject, and reader and subject. This implication, I want to suggest, comes from complication of our texts and a refusal of interpretive closure.

Some researchers who reject qualitative inquiry's role as offering linear contributions to a “knowledge base” (the accretion of knowledge) argue for dialogic rather than monologic research texts that seek less to persuade than to invite readers to form relations with the text by offering “a balance between engagements with others and self-reflexive considerations of those engagements” (Goodall, 2000, p. 14). Gottschalk (1998) calls for narratives in which participants—and I take him to mean writers, “subjects of research,” and readers—are “invited into our texts to speak and participate, in a manner that differs qualitatively from traditional ethnography” (p. 220). Such multivocal texts are not predicated on verifiability or realism but reflect multiple representations of “private” and “social” worlds, including participants' and authors' views, experiences, and emotions (see Banks & Banks, 1998; Krizek, 1998). A vocal proponent of autoethnography, Carolyn Ellis (1995) draws from reader response theory, which places meaning-making both in individuals and communities, to describe evocation as “a means of knowing” (p. 318). She says, “In evocative storytelling, the story's va-

lidity can be judged by whether it evokes in you, the reader, a feeling that the experience described is authentic, that it is believable and possible; the story's generalizability can be judged by whether it speaks to you, the reader, about your experiences" (p. 318). Ellis's privileging of feeling over thinking and her desire to speak directly to readers' experiences rather than opening readers to new experiences points to areas traditionally ignored in discussions of research methods and purposes yet runs the danger of being overly emotive and solipsistic.¹¹ However, despite these limitations, these writers' search for implication and complication over verifiable realism offers an alternative to canonized research by engaging the topics of their disciplines on terms that are open and do not presume their effects beforehand.

If research is to do more than represent "real" identities and experience, readers are to do more than gain information about and understand these identities and experiences, and new thought is to be cultivated, the sorts of complications I argue for through new uses of member checks, triangulation, and a fluid transferability are a needed first step. The resulting complex representations let go of an idea of progress based in a unidirectional idea of the accretion of knowledge by admitting difficulty, uncertainty, and paradox. To return to the interests of research, technical, practical-hermeneutic, and emancipatory, each is oriented to a particular type of usefulness: control, action within a context, or action to change the conditions of a context. In each, the viability of the knowledge research produces is predicated on the ability of research to verify itself. Each interest demands interpretive closure, whether that interpretation is located in research participants or the researcher.¹²

To respond to the question of "whose interpretation is of most worth?" (a question that could easily but not "usefully" be reduced to a debate over the value of "emic" and "etic" views), I answer that neither is and both are. There will be times that interpretations diverge and times that they converge—and times that resolution will be impossible. These might be moments when research draws on what Patti Lather (1995) called "transgressive validity," which foregrounds the production of "truth as a problem" (p. 54) and does not conceal but reveals "undecidables, limits, paradoxes, and complexities" (p. 57).¹³ Indeed, her co-authored *Troubling the Angels* (1997) acknowledges difficulties, multiplicities, and limits, openly questions and contradicts itself, addresses readers in multiple registers—all while refusing paralysis and claiming multiple potential usefulnesses. To understand "usefulness" as indeterminate and interpretation as multiple rather than singular is to acknowledge the importance of the interpretations of a third figure, the reader. Presenting verifiably "real" subjects and events designed to impart

knowledge creates what Barthes (1974) called a “readerly” text in which the reader becomes a consumer of the text. To refuse verifiable and thus closed interpretations is to construct a “writerly” text (p. 4) that acknowledges the reader as an active producer of the text. The reader as meaning-maker rather than information consumer enters the circle of conversation—one that does not end with him or her but continues with ongoing interpretation and dialogue.

In his book *Still Life with Oysters and Lemon*, Mark Doty (2001) reflects on the simplicity of Dutch realist canvases painted some 350 years ago, remarking on their simultaneous realism and the impossibility of complete representation. He says:

The most beautiful still lifes are never pristine, and herein lies one of their secrets. The lemon has been half-peeled, the wine tasted, the bread broken; the oysters have been shucked, part of this great wheel of cheese cut away; the sealed chamber of the pie, held aloft on its raised silver stand, has been opened. Someone has left this knife resting on the edge of the plate, its handle jutting toward us; someone plans, in a moment, to pick it up again. These objects are in use, in dialogue, a part of, implicated. They refuse perfection, or rather they assert that this *is* perfection, this state of being consumed, used up, enjoyed, existing in time. (p. 40)

The time he refers to is multiple, embedded as it is in the actual existence of the things in the moment; in the creation of their depiction; and in their past, present, and future relations to unknown and unseen others. The representations remind viewers that nothing is self-contained even as they seem to contain a moment. Doty describes artists’ work as a gift to others: “A painting of asparagus, a painting of gooseberries, a painting of five shells arranged on a shelf. Exactitude, yes, but don’t these images offer us more than a mirroring report on the world? What is it that such a clear-eyed vision of the particular wishes to convey? A way to live, perhaps; a point of view, a stance toward things” (47). That, I propose, is what research might offer. It can represent a stance that hints at a “real” and claims one’s own and grants others’ interpretive authority while acknowledging the multiplicity and uncertainty underlying and present in the “real,” and consciously undermining closure and perfection. But the offering is exactly that, an offering, for we can never know with certainty what others will do with what we put forth, how they will take it up or reject it, regardless of our realism or evocations, our interpretive closure or openness, or our adherence to canonical methods or our efforts to break with them. Our texts and our talk about research, though, must enact and acknowledge these inevitable imperfections and implications. The renewed member check, “anti-triangular analysis,” and an idea of transferability that recognizes that connections exist

where we might not expect them, keep the purposes and effects of research open rather than closed down, potentially innovative rather than repetitive.

My emphasis on complicating what inquiry depicts is intended as a gesture to help research overcome its will to knowledge and to encourage a rethinking of the subjects who speak in, through, and to ethnography. Ethnographic knowledge refuses illusions of transparent reality and suspends utility even as it engages the real and the purposeful. Its uses are potential rather than given. Because it is relational and social, ethnography offers contingent knowledges that are never self-evident but whose meanings and implications must be constantly reinterpreted. As an enactment of uncertainty and noncorrespondence to a “real,” ethnography does not offer knowledge but demands thought.

Ethnography has had its appeal because it offers real people and real situations—a humanizing endeavor amidst what is often abstract, decontextualized, and dehumanizing research. However, if ethnography is to become interesting, it must cede its authority by admitting into its textualizations speculations about the indeterminate, ambiguous times and places its subjects inhabit—and that inhabit its subjects. These times and spaces remind us that inquiry can not capture a totality or offer neat stories of progress, whether about the building of knowledge, theory, or change in the field. Ethnography should be thought of as offering its readers the responsibility of responding with thought to what they find. Lyotard (1997) tells us, “Response is not to answer, but to address and carry forward” (p. 228). To answer is to conclude. In ethnography, we respond to a person, a situation, or a dilemma without certainty, without an illusion of a neat end. We do so to imagine the real otherwise, to think with others.

Notes

¹Throughout this essay, there is some slippage between the terms ethnography and qualitative. While I draw from writers who claim both traditions, my discussion is intended to address ethnography, qualitative research, and naturalistic inquiry as they have been put to use in (higher) educational research.

²This may not be a wholly fair representation, as the field over the last two decades has expanded to embrace critical, postmodern, and critical postmodern research (for example, see the essays in Shaw, Valadez, & Rhoads, 1999). As put to use in higher educational research, even these, however—and particularly critical postmodernism (e.g., Tierney & Rhoads, 1993)—have an underlying teleology.

³Guba and Lincoln (1989) have described this as a stance that seeks natural laws, presuming that “Each act of inquiry brings us closer to understanding ultimate reality; eventually we will be able to converge on it” (p. 36). On worries about the need for research that is practical and relevant to practitioners and policymakers, see, for example, Altbach (1998), Conrad (1989), Keller (1985), Kezar (2000), and Leslie & Beckham (1986). For

a questioning of the value of orienting research to policy-making concerns, see Birnbaum (2000).

⁴I would argue that this is true of all social science but I confine my discussion to ethnographic, qualitative, and naturalistic inquiry.

⁵It is interesting to note the similarities between validate and verify in the *Oxford English Dictionary*. Validate means “1. To render or declare legally valid; to confirm the validity of (an act, contract, deed, etc.); to legalize; 2. To make valid or of good authority; to confirm or corroborate; to substantiate or support; 3. To examine for incorrectness or bias; to confirm or check the correctness of.” Valid includes: “1. Good or adequate in law; possessing legal authority or force; legally binding or efficacious; 2. Of arguments, proofs, assertions, etc.: Well founded and fully applicable to the particular matter or circumstances; sound and to the point; against which no objection can fairly be brought.” Like validity, reliability is often attached to the verification of data. I do not address it in this paper, however, as it is so far removed from the grounding of qualitative research in the particular.

⁶It is interesting to note that four years after the publication of *Naturalistic Inquiry*, Guba and Lincoln (1989) turned away from the term “naturalistic inquiry” because, they explained, it suggests “a realist ontology that we specifically reject” (p. 19). They chose instead “constructivist” due to their understanding that “the central feature of our paradigm is its ontological assumption that realities, certainly social/behavioral realities, are mental constructions” (p. 19).

⁷In methodological discussions of qualitative research in higher education, *Naturalistic Inquiry* has occupied a primary location (see, for example, Creswell, Goodchild, & Turner [1996] and Crowson [1987]). Other important texts, which I do not consider explicitly, include Glaser and Strauss (1967), Strauss and Corbin (1994), and Miles and Huberman (1994). A review I conducted of articles published in the *Journal of Higher Education* and *The Review of Higher Education* from 1999–2001 reveals a continuing paucity of ethnographic, qualitative, and naturalistic studies and ongoing reliance on research based on surveys and large database studies (despite what appears impressionistically to be a rise in theoretical and historical scholarship), suggesting that qualitative research continues to struggle for legitimacy. I offer examples of some of these articles’ discussions of research methods to illustrate the ways Lincoln and Guba’s work has been unreflectively invoked. These examples may also suggest that researchers are still trying to educate higher educational audiences about qualitative inquiry and its methods (or justify their uses). Because I do not wish to single out individuals but to discuss a field, I do not include authors or titles. These are but a few examples, all chosen from *The Review of Higher Education*, primarily due to the presence of more complete discussions of methods than in articles in *The Journal of Higher Education*:

(1) “I used three common qualitative techniques to ensure that the data collection and analysis of these case studies met the highest standards of trustworthiness for qualitative research; triangulation, member checking, and creating an audit trail (Lincoln & Guba, 1985). This study employed three types of member checking to ensure credibility. First, I held debriefing sessions with respondents immediately after the interviews to test my initial understandings of the data. . . . Second, I contacted select research participants approximately three months after the interviews to test the evolving analytical categories, interpretations, and conclusions. . . . I sent them a draft of the interpretation of data from their site; these respondents did not offer any substantive changes to the categories and seemed pleased with the representation of their campus in the case-study write-up” (2000). (2) “Our data collection and analysis conformed to the highest standards of qualitative research. Instead of demonstrating constructs appropriate to quantitative research, such as reliability, internal validity, and external validity, we rigorously applied the parallel set of standards appropriate to qualitative research. Qualitative research established the trustworthiness of its findings by demonstrating that the findings are credible, transferable, dependable, and confirmable. We used four techniques to en-

sure trustworthiness: triangulation, member checking, thick description, and keeping an audit trail (Lincoln & Guba, 1985)” (2001). (3) “Methodologically, this paper rests on the assumption that qualitative techniques are particularly well suited to ‘soliciting emic (insider) viewpoints’ and can ‘assist in determining the meanings and purposes that people ascribe to their actions,’ thereby giving those students a voice (Guba & Lincoln, 1994, p. 110)” (2000). (4) “These . . . reports provided triangulation that helped me assess the validity of the interview findings (Glaser, 1978)” (2000). (5) “Authenticity and trustworthiness are always important factors in qualitative research like this study. Miles and Huberman (1994) describe authenticity as ‘truth value’ (p. 278), pointing out the researchers’ responsibility to gain an accurate understanding of what is really happening and their efforts to ensure that the study is credible and understandable. . . . Primary sources of data are typically considered more authentic than other forms of data” (2000). (6) “Trustworthiness refers to the truthfulness or accuracy of the data collected (Miles and Huberman, 1994). . . . Triangulation suggests that data extracted from two or more sources are more like to be trustworthy than information from a single source” (2000).

⁸They offered as an alternative: “Member checking processes ought to be dedicated to verifying that the *constructions* collected are those that have been offered by respondents, while triangulation should be thought of as referring to cross-checking specific data items of a factual nature” (p. 241).

⁹Joanne Martin’s (2002) discussion of the problems of relying on a context or, for example, a job classification and assuming that all people in that context share a culture gets at the limitations of assuming boundaries. She explains: “What if culture is defined in ideational rather than material terms so that culture is seen as consisting of ideas and meanings rather than particular people or jobs? In ideational conceptions of culture, borders become permeable, because ideas or interpersonal contacts can be imported or exported from the larger society or surrounding community into or out of an organizational context. An organizational member can refrain from being a member of its culture or can be less of a member than another employee with the same job assignment. Even boundaries—what is in and out of the culture—can be seen as a subjectively created product of culture; edges can be socially constructed and those social constructions can change” (pp. 26–27). Thus, the contested nature of a bounded space as actors in that space draw on resources in constructing and negotiating a “culture” is not only constantly changing but also not always accessible to researchers.

¹⁰See also John Smith (1996) on the privileging of the technical over the conceptual as creating a context in which “various books of a ‘how to do qualitative inquiry’ variety found very fertile ground. For example, a book by Miles and Huberman (1984) that attempted to standardize qualitative methods was very well received. The number of editions the book has gone through clearly attests to the fact that they struck a chord with the profession. Their message was quite clear: Researchers should leave the philosophical/epistemological issues to those who are most interested in them and get about the business of doing qualitative research—but do it properly. They detailed various procedures for doing qualitative inquiry, with the injunction to their colleagues that if you do not follow these procedures you will not do good research and if you do you will” (p. 163).

¹¹See Ellis’ (1995) *Final Negotiations*, in which she details the process of writing about her relationship with and the illness and death of Gene Weinstein, a fellow sociologist, for an example of deeply personal autobiographical narration.

¹²Even in Guba and Lincoln’s (1989) reworked constructivist paradigm, which disavows verification, a form of closure is a tacit goal of research. They describe negotiating outcomes as a hermeneutic dialectic aimed at constructing “a comparison and contrast of divergent views with a view to achieving a higher-level synthesis of them all, in the Hegelian sense” (p. 149). Either consensus or the revelation of differing views is a potential outcome—yet this revelation is aimed at “the building of an agenda for negotiation” (p. 149). Despite recognition of divergence, closure (through dialectic and synthesis) is sought.

¹³Martin (2002) advocates the use of what she calls a three-perspective theory of culture that includes integration, differentiation, and fragmentation, which respectively understand culture “as a homogeneous entity; as a collection of subcultures; or as a gathering of transient, issue-specific concerns, constantly in flux” (p. 151). As she describes it, data collection and analysis that span these conceptions of collectives and individuals allow more complex interpretations and representations of the world and allow for openness to multiple points of view and constructions of a context (pp. 153–154). Ultimately, Martin’s suggestion may be productive in its encouragement of multivocality and complexity in textual representations. Aware of the potentially generalizing nature of realist texts and their rhetorical claims to authority and objectivity, she offers alternative textual strategies—such as acknowledging differences among cultural members or placing participants and researcher in dialogue that doesn’t necessarily end in consensus (pp. 292–293). While these are useful suggestions that get at complexity, they miss the fundamental uncertainty of all research. Martin’s recognition of inevitable dissensus still presumes unmediated access to a “real,” whether singular or multiple. What is missing is an acknowledgment of uncertainty and advocacy of speculation that hints at unverifiable but potentially productive insights.

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