

## Post Qualitative Inquiry

Keynote Lecture

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Abstract: *Post qualitative inquiry* offers a critique of conventional humanist qualitative methodology and marks a turn toward poststructural and posthuman inquiry. It also takes account of the new empiricisms emerging with the ontological and material turns in the humanities and social sciences. This inquiry is not methods-driven but informed by concepts like Karen Barad's *entanglement* and Gilles Deleuze and Felix Guattari's *assemblage* and by conceptual practices those concepts make possible, practices that will be different in different projects. Post qualitative inquiry is an invitation to think and do educational inquiry outside normalized structures of humanist epistemology, ontology, and methodology.

I'm very happy to be with you today, and I especially want to thank the conference organizers for their hard work in getting us together here so we can help each other think.

The title of my paper this morning is "Post Qualitative Inquiry," which is also the title of a chapter I wrote for the 2011 4<sup>th</sup> edition of the *Sage Handbook of Qualitative Research*. I deliberately used the rather large and ambiguous term "post qualitative" for two reasons. The first was to mark what I believe is the impossibility of an intersection between what I've been calling "conventional humanist qualitative methodology" or "1980s qualitative methodology" (e.g., Denzin, 1989; Erickson, 1986; Lincoln & Guba, 1985) and postmodernism, poststructuralism, posthumanism, and other post theories I refer to together as the "posts." The second was to announce that that impossibility can help clear the way for what I hope will be a multitude of different approaches to social science inquiry that might come *after* humanist

qualitative methodology if one puts the poststructural critiques of humanist ontologies and epistemologies to work.

When I wrote that chapter, I was well aware of the slippery politics of any critique of qualitative methodology given the current neo-positivist status of educational research, policy, and practice in the U.S. In addition, conventional humanist 1980s qualitative methodology continues to be radical in U.S. social sciences disciplines like psychology, political science, and economics. Further, I have certainly championed qualitative methodology for decades in my teaching and writing. My point is that I don't take lightly my critique of this methodology that has done much good work in educational research.

But my critique comes from my own experience in teaching qualitative methodology for the last 20 years. At the University of Georgia, we offer a five-course sequence in qualitative research methodology, and doctoral students who complete the sequence are awarded a certificate, which is supposed to qualify them to teach qualitative methodology once they enter the academy. I joined the faculty at the University of Georgia in 1995, and, since then, I've taught our introductory course in qualitative methodology 17 times. But I also taught courses on postmodern theory, on Foucault and Derrida; and I sent our education students to our Comparative Literature Department to take courses with Ronald Bogue, who is an internationally renowned Deleuzian scholar. Over the years, students who had diligently studied poststructural and postmodern theories struggled and failed to reconcile those theories with humanist qualitative methodology. So in 2003 I developed a counter-course, Post Qualitative Research, that is not grounded in humanist qualitative methodology to support them, and the content of that course has changed over the years as we all got smarter about how one might inquire using the "posts."

I completely understood my students' dilemma in not being able to reconcile humanist qualitative methodology with the "posts" because I had also experienced that disconnect as a doctoral student, a disconnect I believe occurs because our educational research curriculum generally separates epistemology and ontology from methodology. Like my students, I had studied two bodies of knowledge simultaneously but separately: poststructural and postmodern theories on the one hand and humanist qualitative methodology on the other. And like my students, I had been unable plug my poststructural dissertation study into qualitative methodology because the epistemology and ontology of the "posts" simply do not align with a humanist methodology. For example, when I wrote the methods section of my dissertation, I first presented my study as a conventional humanist qualitative study and then immediately deconstructed that description using Deleuze's lovely concept, the *fold*.

As is often the case for me, it was in the thinking that writing produces that I first understood that the concepts that structure humanist qualitative methodology—concepts like the human subject, the interview, the observation, the voices of participants, the field, data, data analysis, member check, validity, systematicity and so on—all those concepts were not thinkable in the "posts." As Foucault (1976/1978) might say, those concepts exist in a humanist "grid of intelligibility" (p. 93) but not in poststructuralism. So, for me, humanist qualitative methodology failed the first time I tried to use it, in my poststructural dissertation study; and in the first two papers I published in 1997 I used Deleuze's concept the *fold* to deconstruct *data* (St.Pierre, 1997a) and his concepts the *nomad* and *smooth space* to deconstruct the *field* (St.Pierre, 1997b). I told someone not long ago that I read Deleuze and Guattari's transcendental empiricism, their experimental ontology, *too soon*, so early in my doctoral studies that conventional humanist qualitative methodology was ruined from the start. Of course, other researchers who had taken

up the “posts” deconstructed other concepts of qualitative methodology. For example, Patti Lather (1993) deconstructed validity, Jim Scheurich (1995) deconstructed the interview, and Wanda Pillow (2003) deconstructed reflexivity. Many of you at this conference were doing the same. We said we were using the “posts” to work the ruins (St.Pierre & Pillow, 2000) of humanist epistemology, ontology, and methodology.

But I believe we worked those ruins for too long. Those of us using poststructural theories seemed unable to just leave the ruined structure of humanist qualitative methodology behind and do something different from the beginning. But why did we continue to try to make humanist qualitative methodology work for our poststructural studies? Looking back, I think we in the U.S., at least, had been so well-trained in conventional humanist qualitative methodology that we could not think outside it. For that reason, I am much more aware now of how very difficult it is to escape our training, how difficult it is to leave behind sacred concepts like *method* and *data*, how difficult it is to de-formalize, de-scientize everyday practices like talking with and observing people, and how difficult it is to make these new turns some of us are trying to make—the ontological turn, the new empirical turn, the new material turn.

Another reason I think those of us who used poststructural theories were stuck in humanist qualitative methodology for so long is that we believed our methodological choices were between an interpretive qualitative methodology and a positivist quantitative methodology, so we chose qualitative methodology thinking we might be able to deconstruct it, to open it up enough to make it work with the “posts.” I think that was not only wishful thinking but also a fatal mistake because by hanging on to a prescribed methodology, we ignored the very serious and sustained poststructural critique of method. Continuing to embrace qualitative methodology prevented us from imagining different kinds of inquiry that were not grounded in humanism’s

ontology and epistemology. I'll return to this problem later, but first I want to set the stage for my call for post qualitative methodology by briefly reviewing the history of qualitative methodology in the U.S. I'm not sure how this maps onto your experiences, and I'd enjoy talking with you about that later.

It's important to remember that 1980s qualitative methodology in the U.S. was part of the larger interpretive turn in our social sciences that drew heavily from interpretive anthropology introduced in 1973 with Clifford Geertz's still stunning book, *The Interpretation of Cultures*. Geertz told us that in interpretive anthropology we don't first find and then describe or represent culture in our research reports; rather, we *inscribe* culture as we write it; we make it in our texts. That understanding ushered in what Geertz (1988) called the "burden of authorship" (p. 138), what Marcus and Fischer (1986) called "the crisis of representation" (p. 7), and what Habermas (1975) called the legitimation crisis.

After the interpretive turn, our research reports could no longer be naïve, innocent, transparent reflections of what *really is* but were always already partial, contingent, and potentially dangerous *fictios*, fictions—our interpretations of research participants' interpretations of interpretations they found in their cultures and used in living. What this means for interpretive social scientists is that even if we invent five or ten or twenty different "research designs"; even if we follow a trusted, well-defined "research process"; even if we improve our research methods so we can go deeper and be more accurate; even if we interview the same participant five or ten or fifteen times hoping to reconcile discrepancies; even if we observe in the same research setting for one or two or five years to uncover its authentic culture; even if we do all that, we can never get to the bedrock of truth because there is no final truth, no *brute datum*, out there to be found. What we have at the end of fieldwork and analysis is not a solid

“finding” but rather interpretation piled on interpretation piled on interpretation. All we can ever get in interpretive social science is what a participant thinks today, what we observe today. What we get is today’s story— which might well change in six weeks or six months—and we have to be content with that. Such is the nature of interpretive social science. And why should we expect more? Isn’t life like that—messy and unpredictable? Don’t we change our minds about what we think from one day to the next, from week to week, from year to year? Aren’t we constantly reinterpreting our lived experiences as we tell them to different people throughout our lives? And don’t all those re-tellings change us as well? I surely hope so. As Foucault (1982/1997) wrote, “to be the same is really boring” (p. 166).

Interpretive social science in the U.S. was radical in the 1970s. Many claimed it wasn’t science at all. But it provided a powerful critique of the positivist social science that had thrived in our universities in disciplines like psychology, sociology, political science, and economics ever since the Vienna Circle logical positivists or logical empiricists brought it to the U.S. when they fled Europe in the run-up to World War II. Invented between the two world wars in the 1920s and 1930s by European economists, mathematicians, physicists, philosophers, and linguists, logical positivism was, and still is, attractive because it claims that the rigorous, scientific methods of the natural sciences can find rules and laws in the social world as they have in the natural world. In social science, positivism’s desire is to predict what people will do and then control them.

There were many disagreements among the Vienna Circle logical positivists, but they shared some basic assumptions that continue to structure the social sciences and education in the U.S. in the 21<sup>st</sup> century. The most important assumption, I believe, is the separation of Science from Philosophy. The logical positivists rejected philosophy, metaphysics, whatever is non-

observable, non-measurable, and speculative. In doing so, they rejected ideology critique like Marxism and theories that destabilize human reason, like those of Freud. Their methodologies, then, separated scientific reasoning from what they determined was non-scientific. In general, the logical positivists believed in the verifiability principle of meaning—the idea that only that which can be seen and measured is valuable. Their empiricism claims that there is, indeed, such a thing as a *brute datum*, a sense datum, that is not subject to further interpretation, judgment, or contingency and that truth can be found through the right use of rational methods.

The Vienna Circle logical positivists also supported the following ideas: the belief that language could be clear and unambiguous; the use of prescribed, formal, exact methods, preferably mathematical; a belief in a unified theory of science that rejects a division between the natural and social sciences; that observability entails objective, re-producible experiments; and a belief in incrementalism, the idea that knowledge steadily “accumulates” and the purpose of science is to fill “gaps” in the edifice of knowledge. Positivism maintains the Cartesian binaries of mind/body, Self/Other, rational/irrational, objective/subjective, human/nonhuman, and so on. To me, these ideas represent an age-old desire to get below the messy, contingent surface of human existence to a pristine, originary foundation, the bedrock of certitude. A belief in the power of scientific knowledge to “cure” the problems of human existence is, I would argue, a belief and not the truth. And, as many scholars have noted, a Science presumed to be separate from Philosophy is simply another narrative in the service of power.

To repeat, positivist social science dominated U.S. social science during the early decades of the 20<sup>th</sup> century until the identity politics and the social movements of the 1960s and 1970s began—the women’s movement, the civil rights movement, the gay and lesbian movement, and other resistance movements that rejected the rational social scientist of

positivism who claimed to be neutral and untouched by race, class, gender, sexuality, age, and so on. As the feminist, Donna Haraway (1988), put it, there is no privileged God's eye view from nowhere. Haraway and others argued that knowledge is always situated, local, partial, and contingent and cannot be replicated, generalized, and scaled up, as we like to say in education these days. Further, identity politics argued that, contrary to positivist claims, all science, both social and natural, is contaminated by human values and desires and can never be "objective." In other words, the researcher cannot not be there, and science is always a very human enterprise.

So interpretive social science, and interpretive qualitative methodology, was invented in the U.S. in the 1970s and 1980s to counter the positivist social science that had been dominant for decades, and it thrived until the beginning of the 21<sup>st</sup> century. For a number of years, in fact, more qualitative than quantitative research studies were presented at the American Educational Research Association, and qualitative methodology became a powerful methodological machine in educational research.

But a powerful backlash to interpretive social science and qualitative methodology began in the U.S. in 2000 with the passage of the No Child Left Behind Act, which introduced the concept, "scientifically based research in education," a description of research that is essentially positivist. Interestingly, the person who wrote the definition of scientifically based research in the federal law was neither a researcher nor an educator, but we should not be surprised at that. As Foucault explained, politics and not rationality is often found at the beginning of things. Nonetheless, this political maneuver took everyone by surprise because it was the first time in our history that the federal government had mandated research methodology in federal law. The argument was that, because educational researchers, especially qualitative researchers, had failed to produce knowledge that could tell us "what works" in schools and correct educational



problems, the government had to intervene and use the force of law to make us use rigorous, scientific methodologies. In fact, scientizing everything about education soon became the norm. The president of our National Academy of Sciences said in his 2001 presidential address that his goal was, in fact, to make education a science.

To accomplish that, Grover Whitehurst, an educational psychologist, was appointed as the first Director of the new U.S. Institute of Educational Sciences that was created by NCLB. The Institute of Education Sciences was the new funding agency for educational research. Whitehurst immediately determined that causal research was the only kind of research that would help us learn “what works” in schools. Furthermore, he determined that the gold standard of causal research was the randomized, controlled trial. Qualitative research was dismissed across the board as unscientific because, as some (Shavelson, Phillips, Towne, & Feuer, 2003) claimed, it’s based on narratives and not on facts. During the decade after NCLB, the first decade of the 21<sup>st</sup> century, we qualitative researchers in education often heard patronizing, dismissive comments like, “Qualitative research can be interesting, but it’s not science.” Another comment was, “We want to know what will work in any fifth grade classroom in the country; don’t keep telling us ‘it’s complicated.’”

The upshot of NCLB was that U.S. federal government would not fund qualitative research because it claimed its findings could not be scientifically-based or evidence-based. Qualitative research findings could not be generalized. Qualitative studies described but did not measure, and rigorous science is supposedly based on numbers, not on words. What happened is that in order to get federal funding for educational research, many qualitative researchers proceeded to make their interpretive studies positivist by introducing positivist concepts and “practices of formalization” (Pascale, 2011) like bias, subjectivity statements, systematicity,

audit trails, inter-rater reliability, coding data, and so on. Interpretive qualitative methodology, whose methods are emergent, became methods-driven, linear, and systematic, following a pre-determined “research process” using pre-determined “research designs” like case studies and focus-group research. In this way, much interpretive qualitative methodology became positivist.

But the federal government wasn't the only force that was positivizing qualitative methodology. During that first decade of the 21<sup>st</sup> century, the U.S. publishing industry structured and normalized qualitative methodology by churning out hundreds of textbooks, handbooks, and journal articles; and universities produced research curriculum that described exactly what qualitative methodology is and exactly how to do it. Qualitative research conferences became popular. In our books and journal articles, we reduced qualitative methodology to a recipe. Just think of the chapter headings of most introductory qualitative textbooks: identify a research question whose answer will fill a gap in the research literature, choose a methodology that aligns with the question, design the study choosing among five or six established research designs, carefully follow the research process, collect data, code data to identify themes and patterns that somehow miraculously “emerge” from the data, and then write up the study using the conventional research report format.

The problem is that studies that follow that research process typically do not acknowledge their epistemological and ontological assumptions, they seldom use theory in analysis, and they often produce low-level, pedestrian, insignificant findings. By adopting the positivist stance of ignoring theory—by separating science and philosophy—these researchers seem unaware, for example, that it's not just that our research questions determine our methodology, it's that our onto-epistemologies determine the very questions we can ask and, more radically, *whether we even believe in method*. What is very clear now is that the positivist

qualitative methodology that emerged in the U.S. after the 2000 No Child Left Behind Act that mandated scientifically based and evidence based everything is *not* the interpretive qualitative methodology that was invented in the 1980s.

During the last 15 years or so, I've learned to my dismay that one can do a qualitative study without reading much theory at all. All you have to do is interview some people (the more the better), transcribe the interviews word for word as if the words are somehow sacred, code the interview data using a computer program (counting the frequency of codes makes the study more scientific), sort the coded data into categories or findings, and then write a research report following as closely as possible the standard research report format used in the natural sciences. We have, in the last 15 years or so, created, formalized, and sanctioned this kind of positivist qualitative research in the U.S. We also invented mixed-methods research in order to, as my students tell me, "sneak" a small qualitative component, perhaps a few interviews, into a larger quantitative study. In sum, this new stripped-down, methods-driven, atheoretical positivist qualitative research can be interpretive. This methodology does not make the interpretive turn away from positivism.

And they certainly cannot accommodate poststructural theories. Nonetheless, I continue to see many new scholars struggle to make poststructuralism and humanist qualitative methodology work together. For example, I read research proposals in which a doctoral student proposes some awkward combination of an interview study and a Foucauldian genealogy, although Foucault was very clear in his genealogies and archaeologies that he was interested not in the speaking subject but in the conditions of discourse that enable what we can think and say.

I review manuscripts in which researchers describe a methods-driven, quasi-positivist study but introduce a Deleuzian concept like *assemblage* or *Bodies without Organs* in their

literature reviews and then can't really use the concept because Deleuze's transcendental empiricism just bounces off the logical empiricism of their methodology. I read studies in which researchers claim to be using Derrida's deconstruction but then code data. It seems that these researchers who want to use poststructural theories don't know how to do empirical educational research without doing a qualitative, quantitative, or mixed methods study. No matter what their onto-epistemological commitments are, they believe their studies must fit into one of those methodological containers.

Why does this happen? I've thought a great deal about this, and I don't think it's a failure to learn but a failure to teach. It's much easier, after all, to teach and learn ahistorical, atheoretical methods-driven research—a recipe you can count on—than it is to teach and learn epistemology and ontology. It's much easier to instruct students to choose among quantitative, qualitative, and mixed methods research methodologies than it is to teach the history of science and the history of empiricism so they understand the history and politics of the methodological distinctions we've made. It's much easier to teach, say, interviewing and coding data than to teach what Alecia Jackson and Lisa Mazzei (2012) call “thinking with theory.” Our failure to teach and our students' failure to read means they have little to think with except the given, the self-evident. Our students who are theoretically impoverished are ill-equipped to critique dominant, normalized structures. Instead, they join what Lyotard (1989) called the “maintenance crews for the big explanatory machines” (p. 182), including normalized research methodology machines.

For that reason, I argue that all researchers, in both the social and the natural sciences, should begin their studies, not with the methodology machine, but with the history of the onto-epistemologies that enable us to think such a thing as “science,” a history that allowed us to

separate Science from Philosophy, a history based not on rational decision-making and scientific evidence but shot through with power and politics. And I argue that beginning with that history is a *requirement* for those who want to do what I've called post qualitative inquiry.

This is because the “post” systems of thought do not have the same descriptions of reality, human being, language, knowledge, power, reason, agency, method, and other concepts that structure humanist systems of thought. When we do our reading and understand that the humanist description of those concepts is only one set of descriptions among others, when we de-naturalize and de-normalize the assumptions of humanism—which is what the “long poststructuralist revolution in the critical humanities” (Merchant, 2014, p. 2014) and social sciences has been doing for over half a century—then we understand why we could work the ruins of humanist qualitative methodology for a hundred years without escaping it. As Derrida (1967/1974) advised, we have to overthrow the structure so that something different can happen.

Humanist qualitative methodology, then, cannot accommodate research that uses poststructural, postmodern theories. Nor can humanist qualitative methodology accommodate the “new” work coming out of the recent ontological and material turns, work that has organized itself differently as, for example, affect theory (Gregg & Seigworth, 2010), thing theory (Brown, 2001), actor network theory (Latour, 2005), assemblage theory (De Landa, 2006; Deleuze & Guattari, 1980/1987), the new materialism (Coole & Frost, 2010), the new empiricism (Clough, 2009), and the posthuman (Braidotti, 2013).

Much of the new empirical, new material, posthuman, post qualitative work uses Deleuze and Guattari's flattened ontology, an ontological geography of surfaces and a constellation of imbricated concepts such as *rhizome*, *assemblage*, *plane of consistency*, *line of flight*, *abstract machine*, and *diagram*, that enable their transcendental empiricism. Working with

DeleuzoGuattarian concepts is not easy. For example, one can't understand a concept like *diagram* without understanding others it works with like *assemblage*, *abstract machine*, *plane of consistency*, and *Body without Organs*. It's like studying a foreign language. Just as you think you might understand a few concepts, Deleuze and Guattari throw more concepts at you. And a concept that's primary in one text, like *sense* in Deleuze's book, *The Logic of Sense* (1969/1990), may never be used again.

We've been slow to take up Deleuze's and Deleuze and Guattari's ontology in the U.S. social sciences not only because, as Lyotard (1979/1984) noted, "we are stuck in the positivism of this or that discipline" (p. 41) but also, I suspect, because their work was translated into English later than other 20<sup>th</sup> century French theorists. In addition, Deleuze visited the U.S. only once, whereas Foucault and Derrida, for example, were frequent, popular lecturers, and translations of their work were quickly available. Most importantly, Deleuze and Guattari's work is deliberately ontological and difficult for those of us who've been so obsessed with methodology and epistemology that we've neglected to study ontology.

My sense of things right now is that we're *trying* to make the ontological turn and the material turn in educational research but we can't quite do it. We don't know *how* to do it. So we fall back on humanist research methodologies like qualitative methodology and try to plug Deleuze and Guattari's transcendental empiricism into those containers. But it just doesn't work. It never can. What we really want is a textbook with four or five handy research designs for new empirical, new material research, post qualitative research. Of course, if such a book were written, it would be contrary to the very image of thought Deleuze and Guattari created.

I completely agree with Maggie MacLure (2013) that the "shock" of working "within a materialist ontology has not yet been fully felt" (p. 663). But I am convinced we won't be able

to make the ontological turn and to make the “new” in the new empiricism, *until we just leave humanist qualitative methodology behind*. We must try to forget it, and, as I said earlier, that will be especially difficult for those of us who’ve been very well-trained as qualitative methodologists. But we must remember that we invented qualitative methodology as an interpretive research methodology to counter positivist social science almost 30 years ago. We invented it. We made it up. It’s not sacred. The sky won’t fall if we just put it aside and try something different.

First, we must understand that the ontologies of the new empiricisms and the “posts” demand that we think differently about *method*. In 1979 Lyotard (1984/1979) wrote that he found postmodernism in America, and he defined postmodernism as an “incredulity toward metanarratives” (p. xxiv), especially the privileged metanarrative of the positive value of modern science. Lyotard was critical of positivist social science and its obsession with methods-driven inquiry, as were other scholars we label postmodern, and he argued that, in the postmodern, method exists only in the future, so we should use the future anterior verb tense to talk about it. For this reason, method cannot exist *before* we begin a study; instead, method is always what “will have been done.” I repeat, method is what will have been done. So method exists only at the end of our studies when we might try to explain what we did.

Deleuze and Guattari (1980/1987) did not believe in pre-existing method either. They wrote that “a ‘method’ is the striated space of the *cogitatio universalis* and draws a path that must be followed from one point to another” (p. 377). In other words, the very idea of method forces one into a prescribed, linear, systematic order of thought and practices that prohibits the experimental nature of their transcendental empiricism and of post qualitative inquiry more generally. Method proscribes and prohibits. It controls and disciplines. Further, method always

comes too late, is immediately out-of-date, and so is inadequate to the task at hand. *But method not only can't keep up with events; more seriously, it prevents them from coming into existence.* Again, "method," as we think of it in conventional humanist qualitative methodology, *cannot be thought or done in new empirical, new material post qualitative inquiry.*

At this point, one might well ask, "If this new work doesn't use existing, accepted scientific research methodologies, how do we know it's science?" I would respond by saying that science exists only in a relation of power, when one group who claims to be scientists draws a line to exclude others they claim are not scientists. We certainly learned how this works in recent U.S. history when someone who was neither an educator nor a researcher wrote a definition of scientifically based research in education for the No Child Left Behind Act that drew the line between science and not-science. We also learned first-hand that drawing that line was an act of power, politics, desire, and values and not an act of clear-headed rational deliberation. But the history of science tells us this, and perhaps we should begin educational research courses by reading books like Thomas Kuhn's (1962) *The Structure of Scientific Revolutions*. Kuhn's history illustrates that scientists who support "normal science" are often suspicious of the "new" and "different," and that may not be a bad thing.

But I think it's too late to worry about whether this new work is science. The cat's out of the bag, so to speak. Educational researchers have already begun to study the new empiricisms, the new materialisms, and the posthuman and are putting them to work in their projects. As I explained earlier, I introduced the concept *post qualitative inquiry* in 2011 to encourage researchers to move past 1980s interpretive qualitative methodology and the more recent positivist qualitative methodology. In 2013, Patti Lather and I edited a special issue of the *International Journal of Qualitative Studies in Education* on post qualitative inquiry. In 2014,



Alecia Jackson and I edited a special issue of *Qualitative Inquiry* on qualitative data analysis after coding. Alecia Jackson, Lisa Mazzei, and I are currently editing a special issue of *Cultural Studies-Critical Methodologies* on the new empiricisms and new materialisms. And Hillevi Lenz-Taguchi and I are editing a special issue of *Qualitative Inquiry* on “concept as method.” Other special issues of journals as well as recent edited and authored books announce that scholars are taking up the challenges of new inquiry.

This “new” work is both exciting and difficult, and whenever I’m interested in something new—to me, at least—I teach a course about it so my smart students can help me think. Two years ago the wonderful Bronwyn Davies, who was with us at UGA for a semester, and I taught a new doctoral seminar, the “New Empiricisms and New Materialisms,” in which we all struggled to think about how to “do” social science inquiry differently if one thinks with those scholars we call poststructuralists as well as those writing more recently about the ontological and material turns. What would be new and different about that work? And where would one *begin* to do this “new” kind of inquiry? It’s all well and good to follow Deleuze and Guattari and say “begin in the middle”—even if that’s probably the best advice—but what does that look like? And how does a *new* researcher even *know* whether something is “new” and “different”? And how different do you have to be to be “new”? More practically, how can you increase your odds of doing something “new”?

With those questions in mind, I first review how two theorists who critiqued humanist ontology, Foucault and Deleuze, described the “new.” Then, given their descriptions, I offer a few practices I think might be useful in getting us unstuck from conventional humanist qualitative methodology whose structure traps us and prevents us from making that ontological turn and moving toward the “new.”

I begin with Foucault (1971/1972) who wrote early in his career in the *Archaeology of Knowledge* that “it is not easy to say something new.” Nonetheless, we know Foucault did say quite a few new things. He also said in an interview two years before he died at the age of 54, “I worked like a dog all my life” because his project was *his own transformation*. He said, “Do you think I have worked like that all those years to say the same thing and not to be changed” (Foucault, 1982, p. 131)? This is interesting, isn’t it? Foucault’s warning us not only that doing something new is very hard work but that scholarly work is personal and not just academic. I agree and tell my students that if they’re working hard to read Foucault and Deleuze and Guattari and Derrida and Barad and Baudrillard and Latour and Bennett and Bergson and Spinoza, they *will be changed*, and they can’t go back. So I tell them that if they’re especially fond of themselves as they are, they’d best avoid reading this literature on the “posts” and the new ontologies and go to the movies or mow their yards instead.

John Rajchman (2008), discussing Deleuze’s (1986/1988) book on Foucault, explained that, for Foucault, the new is “not at all what is in fashion, but rather what we cannot yet see or say in what is happening in us just because it is not already contained in the ... given [structures] that govern what we can think” (p. 89). We can’t see the new because we’re limited by the structures of the present, and we have no language yet to say it. Almost a hundred years ago, Whitehead (1925) wrote that pre-existing structures normalize our thinking and produce “minds in a groove” (p. 197), but experimentation can help us move out of the grooves of the normal and self-evident. For Foucault, Rajchman (2008) explained, the new is “a pragmatic experimental matter, something we must actually do for which there precedes no determination, no model, no ‘we,’ not even an ‘I’” (p. 89). Again, there is no model (no method, no research design, nor an “I”) that exists ahead of experimental work that pushes toward the new and different.

Deleuze also believed that it is only in a practical and experimental engagement with the world that we can create something new because “the new is an outside that exists within this world, and as such it must be constructed” (O’Sullivan & Zepke, 2008, p. 2). For Deleuze (1968/1994), “the new, with its power of beginning and beginning again, remains forever new, just as the established was always established from the outset, even if a certain amount of empirical time was necessary for this to be recognized” (p. 136). So it may take some time to distinguish the new that is always becoming from the established that *is*. For this reason, what might be new can’t easily be *recognized* because it is outside our grids of intelligibility.

Most importantly, using language from major, structuring discourses—language like systematic science, methodological positivism, the solipsism of the Cartesian knowledge project—is dangerous because the language of those discourses does not work after the ontological turn (e.g., see MacLure, 2013; St.Pierre, in press). Major, dominant language describes what is recognizable, already captured, disciplined, and normalized—what *is*—not what is immanent but not yet, what could be becoming if we were able to resist the present and think and do it. In fact, that is Deleuze and Guattari’s (1991/1994) famous challenge: “We lack creation. *We lack resistance to the present*” (p. 108). Foucault’s (1982) challenge in regard to the present and, especially, to subjectivity is similar. He said that we are obligated “to refuse what we are” (p. 216). In both cases, we must refuse what Deleuze and Guattari called *order-words* that enforce the present, in this case, methodological order-words like method, systematicity, transparency, representation, validity, objectivity, and so on. These words force us into *is*, lock us into what is. It is unlikely they will help us think and do the “not yet.”

So if we cannot design our research projects before we begin them using the methods or the language and practices of conventional social science research methodologies, what do we

do? Where do we begin? My students certainly ask me this question. And I have begun to recommend some specific practices that might increase our odds of accomplishing something “new” in new empirical, new material, posthuman, post qualitative inquiry. These practices are not new and radical but are, I believe, age-old practices of solid scholarship.

My first “practice for the new” in new empirical, new material, posthuman, post qualitative inquiry is, as I mentioned earlier, to leave conventional humanist qualitative methodology behind, to refuse it. Those of us who’ve learned it too well will just have to try to forget it. It’s simply one approach among others, and we can’t take it too seriously. The ontology of this methodology retains the human/nonhuman, word/thing, representation/real distinctions, which are unintelligible in an ontology in which “the separation between subject and object, thought and matter, words and things, is an illusion of language” (Lecercle, 2002, p. 27).

The second “practice for the new” I recommend to my students is to read a lot, to read more than they ever thought they could read. I tell them it’s OK to read a few of the many, many, too many texts that introduce and elaborate qualitative, quantitative, and mixed methods research in the social sciences. But lest their minds get stuck in the groove of methodology, I encourage them to spend their time reading not methodology but scholars who have written about onto-epistemology for centuries. This historical reading, after all, is the true pleasure of our lives as academics, and I believe inquiry begins by reading the history of onto-epistemology, the history of science, the history of empiricism so we understand how very differently we’ve imagined the world at different moments in history and how some ideas have been take up and normalized and others refused. In that reading, we begin to understand the fragile history of the present.

I tell my doctoral students, “No one can read for you. You have to do it yourself. And those who read a lot can always tell when others don’t.” I tell them that the most voracious

readers know that their beliefs about the world may well be demolished by reading the next book or journal article and that, at some point, that “shock to thought” (Massumi, 2002) is our desire. At some point, we no longer want to read books we understand. We crave, instead, books that are too hard to read, books that seduce us with what Deleuze called an entirely different “image of thought. Foucault (1966/1970) too wrote about “systems of thought” so different, about familiar words put together so differently that we realize the “stark impossibility of thinking that” (p. xv). It’s the impossibilities we find, which hint at others we haven’t found, that lure us into reading the next book and then the next and the next.

I tell my students that if they do keep on reading, if they *read and read and read*, if they let the strange words and concepts wash over them, they will, indeed, begin to think with them and then live with them. They’ll put them to work in their everyday lives. They will understand what Foucault meant when he said we have to give up the “we” and the “I.” They’ll understand why Deleuze refused the personal pronouns. They will no longer believe or live the human/nonhuman binary.

At some point, these students understand the impossibility of “human subjects research” as conceived by conventional humanist qualitative methodology. Instead, they grapple with theory, for example, with Deleuze and Guattari’s (1980/1987) shocking ontological statement, “There is no longer a tripartite division between a field of reality (the world) and a field of representation (the book) and a field of subjectivity (the author)” (p. 23). In this flattened ontology, ordinary distinctions no longer matter, and human bodies, other living bodies, objects, language, representations, concepts like revenge, values like goodness, dreams, the color green, a memory, the weather, five-o’clock in the afternoon, and the not-yet are mixed, entangled on the surface. There is no depth in this ontology as in structuralism and phenomenology. The human

is no longer prior to language, method, and the world; in fact, the human being of humanism is no longer intelligible.

It is impossible to think humanist “human subjects research” when, as Karen Barad (2007) wrote, “existence is not an individual affair” (p. ix), when humans do not exist separate from the nonhuman. It is impossible to think humanist “human subjects research” when, to simplify Deleuze and Guattari, we are always *assemblages* that are not stable entities that can be broken down into distinct component parts and made to mean but, rather something like machines that are constantly connecting, territorializing and deterritorializing—becoming. Importantly, assemblages do not imply interiority but exteriority, so we would not ask what an assemblage *is* or what parts it contains but rather with what it connects, what it plugs into. Again, human being is not independent and self-contained but mixed with everything else on the surface. The point is that we cannot separate out the human subject in posthuman, new empirical, new material, post qualitative inquiry. Our responsibility is no longer to the privileged human but to the assemblage which is always more-than-human and always becoming. In this age of the Anthropocene, responsibility assumes its full meaning.

This brief description gestures toward the ontological image of thought that guides the new empirical, new material, posthuman, and post qualitative inquiry and displaces humanist qualitative inquiry. To repeat, this ontology uses very different understandings of *language* and *human being* that we must account for. Once those two concepts in humanist ontology are shattered, we no doubt flounder; but it is from that *not knowing* that the “new” might emerge. Derrida (1980/1987) helped us understand the value of “knowing how not to be there and how to be strong for not being there right away. Knowing how not to deliver on command, how to

wait and make wait” (p. 191). Perhaps *not knowing* and *waiting* describe the *style* of the new empirical researcher.

I surely did not understand Deleuze’s and Deleuze and Guattari’s work together when I first read it early in my doctoral studies nor do I “understand” it now. Nonetheless, the image of thought enabled by their concepts like *haecceity*, *assemblage*, and *Bodies without Organs* stuck with me, ruining conventional humanist qualitative methodology from the start. I am grateful I read Deleuze and Guattari, Butler, Spivak, Foucault, Baudrillard, Lyotard, Derrida, and others we call “post” before I was over-trained in humanist qualitative methodology, before its methods-driven structure took over my thought and practices.

To repeat, my first “practice for the new” is to put research methodology aside, and my second practice is to read theory and, especially in this ontological turn with its new empiricisms and new materialisms, to study onto-epistemology. And what might a doctoral student do next? She may well panic because her classmates may have begun their studies by following the clear instructions of the plethora of books and journal articles that describe the “research process” in quantitative, qualitative, or mixed methods studies. They have a well-trod path to follow.

The third practice I recommend, then, is to put the theory to work and begin their studies with a theory or a concept or several related theories and concepts they’ve identified in their reading that help them think about whatever they’re interested in thinking about, for example, reluctant readers, the quantified student, gender, or dropouts (see, e.g., Jackson & Mazzei, 2012). If a student is interested in Foucault’s theory of *power*, she should read everything he wrote about power as well as secondary sources and critiques. If she’s interested in Derrida’s *deconstruction*, she should read (almost) everything he wrote about deconstruction as well as secondary sources and critique. The same would hold for Lyotard’s *paralogy*, and Butler’s

*gender*, Barad's *entanglement*, Bennett's *vital matter*, Deleuze and Guattari's *assemblage*, and so on. In Deleuze and Guattari's work, one concept is seldom enough because their concepts are entangled, just like their ontology. The point here is that sustained and what Judith Butler (1995) called *careful reading* is required and brings some measure of confidence and expertise so that students understand why they cannot carelessly mix ontologies.

To repeat, theories and concepts, then, guide the study instead of a pre-determined research methodology. From among many theories and concepts, the researcher chooses those that help her think about whatever she wants to think about. She plugs concepts into the world to see what happens. If she's confused or stuck, good questions to ask might be, depending on the theorist she's using, "What would Foucault do next?" "Would Derrida code data?" "Would Lyotard use the concept triangulation?" "Would Bennett separate people and things?" And the best thing to do when confused is to go back to the texts and re-read the theory, to plunge into the words of the scholars who inspire us. Of course, if the theory or concept one has chosen isn't working, choose others. Some may argue that beginning with a theory or concept is just as restrictive as beginning with method, and I understand that argument, which is, in fact, very close to the positivist rejection of metaphysics. But I believe researchers need something to think with; they need different and even conflicting theories to help them think about the complexity of the world we live in and to imagine other possible worlds.

I am not especially concerned with classifying or labeling this kind of inquiry, because post-method, "after method" (Law, 2004), post qualitative methodology, every study will be different and unclassifiable, which will no doubt make some uneasy. I can imagine researchers asking, "What *is* this study?" We'll have to train ourselves not to look for prescribed method, familiar practices of formalization, and tortured systematicity in this work.



Of practical concern, of course, is how scholars doing this post-qualitative, new empirical, new material, posthuman research work with Institutional Review Boards who monitor human subjects research for possible harm to research participants. My students certainly worry about how to work with our university's IRB. How do we explain our posthuman studies to people in charge of human subjects ethics review? In the U.S., we've been fighting the creeping control of our Institutional Review Boards for decades. Our IRBs have seldom understood interpretive qualitative methodology and so have contributed to positivizing our interpretive studies for some time. For example, one of my colleagues who was doing an autoethnography was required to sign his own consent form. Of course, this is absurd but not so unusual. We have complained so much in the U.S. about IRBs that a National Research Council (2014) committee organized by our prestigious National Academy of Sciences has just proposed revisions to our federal law that, if adopted, will eliminate the need for human subjects review for most conventional humanist qualitative studies. The committee recommends that studies that use methods like interviews and observations that are familiar to most people as well as studies of educational processes and teaching, and learning be exempt from human subjects review. Further these exemptions "would not be limited to adults" (p. 156). These recommendations, if written into federal law, will produce a sea change in social science research in the U.S. and remove our work from the control of Institutional Review Boards. The rest is up to us.

The fourth and final practice I recommend to students, after they've given up qualitative methodology, have read and read and read, and have found theory(ies) and concept(s) to guide their inquiry is to trust themselves. I encourage them to just put to work the practices the theory or the concept enables—we might call these *conceptual practices* (Harding, 2007; Smith, 1990). In other words, I tell them to begin with a concept and conceptual practices will follow. I tell

them to do the next thing experimental ontology enables them to think and do. These practices may be quite familiar, what we do when we want to explore anything. For example, we read, we write, we talk with other people, we observe what's going on around us. We may make a movie, paint a picture, run a marathon— who knows? In the name of methods-driven, positivist social science, we have, as the recommendations for revisions to the U.S. human subjects law acknowledge, overdetermined, over-formalized, systematized, and scientized *some* everyday practices out of all proportion to legitimate them as “scientific,” but we have completely ignored others. For example, in conventional humanist qualitative methodology, we've mostly reduced research practices to two “methods of data collection,” interviews and observations, though I doubt the concepts “data” or “methods of data collection” are thinkable in new empirical inquiry in which the human has never been separate from “data,” outside it, so she could “collect” it..

The point here is that ordinary practices like talking with and observing people don't have to be formalized, scientized, and elevated to the status of “the interview” and “the observation.” I'm interested in all the other conceptual practices, inquiry practices, we neglect to disclose. For example, when I'm deep into a project and stuck, I go for a walk or weed my garden and inevitably get unstuck. I suppose I could call *walking* and *weeding* research practices—but why formalize them? And, surely, we could name *reading* a research practice, but we don't—we call it the “literature review.” I'm very interested in conceptual practices that concepts like *diagram*, *Bodies without Organs*, *entanglement*, and *vital matter* enable. What would one *do* if one were thinking and living with those concepts? I've noticed that some of my students who are doing this new empirical work are especially drawn to music; others to film; and others, like me, just write, write, write, trying to put words together differently, such a simple thing—putting words together differently—that, as we know, can change the world.

To sum up, my strongest recommendation is that we not try to force our new empirical, new material, posthuman, post qualitative studies into the structure of conventional humanist qualitative methodology. I can't imagine how it could fit. Instead of beginning with methodology, I recommend putting the concepts and theories of experimental ontology to work using the conceptual practices that are appropriate for a particular study. If we've done our reading, I wager we cannot *not* put it to work. It will have transformed us—we cannot think and live without it. *We will be living it.*

As Foucault and Deleuze explained, the “new” is already in our lives, but we have to make it. It's in the experimental moment of *not* knowing what to do next because we are *not* driven by method and methodology that we might push through the grooves of the given and the self-evident toward the new and different in our work and lives. Method will always come at the end, too late to help us, when we think back about what we did and why and what we might have done instead and will try next time.

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