FRICTION, FICTION, AND FAILURE:
SCIENTIFICALLY BASED RESEARCH AND THE RURAL

A Thesis in
Curriculum and Instruction

by
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Abstract

The Educational Science Reform Act of 2002 and No Child Left Behind define experimental studies as the preferred method to produce knowledge about schools, learning, and teaching. Situated in rural Pennsylvania, *Friction, Fiction, and Failure: Scientifically Based Research and the Rural*, is a challenge to the certainties and assumptions upon which this legislation is based. Using conversations between an Old Order Amish teacher and a public school teacher, the book challenges legislated scientific preference from three angles: objectivity, evidence, and transparency. The participant’s religious convictions precluded the use of standard data collection techniques, thereby excluding her from representation within sanctioned educational science under current federal policy. Using poststructural theories in order to rescue her from scientific and educational irrelevance, the study details how the researcher and participant's shared rural identity impacted objectivity, the possibilities of realist tales within educational research, and how the metaphor of friction can be engaged within discussions of the transparency of representation of others. Concerns about the consequences of legislated scientism for rural citizens are explored, questioning some members’ exclusion from sanctioned educational research. The study positions the certainties promised by educational research as tenuous and considers the implications of uncertainty for teachers, educational researchers, and their relationship to the public good.
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Preface

We need to put our critical theory to work in this moment of our now.
- Patti Lather (2004, p.22)

In the post Education Sciences Reform Act (ESRA) of 2002, there has been much debate around what constitutes science in educational research. The question, What is scientific research? is an important one; developing new understandings of what constitutes scientific research in an effort to influence policy is indeed urgent work for researchers. While we need to consider broader conceptualizations about what constitutes educational research, this work explores questions around objectivity, evidence, and transparency in educational research. What does it mean that “correct,” scientifically based research (SBR) makes unabashed claims to truth and why might this be problematic? What would an educational research methodology look like that did not claim truth of representation? What assumptions are implicit in the legislated version of evidence-based science? Whose voice will not be heard?

Theoretical Basis

I draw on poststructuralist work of Judith Butler, Bronwyn Davies, Kamala Visweswaran, Marnina Gonick, Elizabeth St. Pierre, Chris Weedon, and Margery Wolf in varying degrees. With these theoretical works as my base, I use poststructural theory to question scientifically based research. I engage poststructural concepts of the other, subjectivity, discourse, agency, and transparency of language to consider notions of truth claims and representation implicit in scientifically based research as legislated in Education Sciences Reform Act. Poststructuralism, says Bronwyn Davies, “open(s) up discourses and practices to questioning, and provide strategies for questioning that run against the grain of common sense and of domination (and dominating) discourses and practices” (2000, p. 169). I put poststructural

1 See glossary
concepts to work in an effort to question the common sense assumptions of scientifically based research by way of consideration of its consequences as ESRA of 2002 and I offer historical contextualization as well. My effort to consider the consequences, particularly for those in rural communities with whom I identify, is offered as an ethnography comprising the core chapters of the study. The ethnography highlights my engagements with key concepts in poststructural theory while also offering a model of research that is firmly excluded under ESRA. While my questions and strategies are poststructural, my consideration of consequences positions this study as a pragmatic inquiry. The two approaches are not contradictory. Pragmatists look to the consequences of our findings (Cherryholmes, 1993). Pragmatism is a discourse on thinking, an attempt to bridge where we are with where we might end up, while knowing that we cannot do so with certainty (Cherryholmes, 1999). This is in direct conflict with empirical researchers, says Cherryholmes, who “often seem to be searching for and attempting to produce texts that ‘get things right,’ express truth, or ground meaning once and for all…(but) truth (capital T) and grounded meaning in any final or transcendental sense are not within our grasp” (Cherryholmes, 1993, p. 3). In chapter one, drawing on published critiques as well as affirmations of scientifically based research, I outline where we are in terms of legislated scientism. Chapter two is a historical look at the domination of positivist epistemologies. The remaining chapters are exercises in the consequences (or more correctly, exclusions) of chapters one and two. They are the bridge to where we might end up if situations were different. Within this analysis, as Cherryholmes warns of pragmatic work, there is little certainty to be found. Cherryholmes claims that the poststructuralist does not look to the consequences of her analysis (1999), but work reviewed below of Bloch (2004) and especially St. Pierre (2002) and Lather (2004) offer a serious challenge to this characterization of poststructuralist theory. This study itself is a

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2 See glossary
challenge to the positioning of “poststructural and postmodern investigations” as “not forward-looking” (Cherryholmes, 1999, p. 4). My study is based on a forward-looking concern with the consequences of ESRA: what it means to embrace untroubled notions of truth, representation, and evidence, who is excluded by this vision and what it might mean to legislate science for policy.

**Intentions**

The questions for this study arose during an investigation in which I conversed with an Amish school teacher over the period of several years. My original intentions were to explore the rural literacies that might be at work in her community. I was interested in how this distinctly rural group drew on specific literacies that may (or may not) conflict with literacies at work around them. I approached the study from the premise that literacy, like reading, has many forms and that teachers often fail to recognize and teach to the multiple literacies that children bring to classrooms. My goal was to contribute to understanding of rural literacies (see Edmondson, 2003) with the hope of affecting classroom practice. The Amish teacher with whom I met to discuss these questions, however, insisted that I refrain from either audio recording or note-taking as a condition of her participation. Already clearly outside the boundaries of legislated definitions of scientifically based research, the absence of these data collection tools presented additional challenges to the assumptions of research methods and steered the study in an entirely different direction. How could I address questions around rural literacies within the boundaries of scientifically based research? Clearly there is no room for this line of inquiry; I did not ask a viable question that could be framed within what is deemed scientific. Further, my participant’s conditions for participation precluded the possibility of her contribution to the

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3 See Glossary
4 See appendix A for legislative summaries
literacy research base. Both because of the questions I intended to ask and my participant’s membership in a religious group that is equally cautious about science and technology\(^5\), this study and others like it are discarded by legislated understandings of what counts as research. How then can it be said that this representation of scientifically based research represents the view of “the American people” (Feurer, 2002, p. 4)? Which American people? Of legislated scientism Patti Lather asks, “Where are we going with democracy with this project?” (Lather, 2004, p.23). Lather, along with pragmatists, ask ‘where are we going,’ but poststructuralism is deeply suspicious of an emancipatory outcome based on “getting it right” (St. Pierre and Pillow, 2000, p.6). While this work is clearly a response to the positivist\(^6\) ideals of objectivity and the potential of the scientific method to discover truth, the value of the study does not lie in its delineation of alternatives to the outcomes of the current structure. My intent, instead, is to “see poststructurally,” making visible both the “systemic practices and moment-by-moment work through which relations of power and powerlessness are played out” (Davies, 2000, p. 166). It is this process of making visible, says Davies, that increases the will to act (2000). It invites openness to the unexpected, turns a critical gaze towards oppressive patterns of power, and engages a strong will to action (Davies, 2000).

In order to make explicit how this qualitative study conflicts with legislated notions of science that are decidedly positivist, I begin with the contemporary context of the Education Sciences Reform Act of 2002. I present a recent legislative history of the ESRA and explain its relevance to educational research in chapter one and follow in chapter two with a historical contextualization. Each of the core chapters of this work represents a challenge to the legislated idea of scientifically based research. It is my intent to demonstrate both what may be lost as a

\(^5\) See appendix B for background on the Old Order Amish
\(^6\) See glossary
result of its assumptions and drawing on the critiques of others, why the principles themselves are problematic. Chapter two places the study (as well as ESRA) in the larger historical context of educational research. It is largely a story of the domination of positivism with interpretativist interruptions, demonstrating not only that the study is a critique of the assumptions legislated in ESRA, but also critiques assumptions that have buttressed educational research for more than a century.
Chapter One

How did we get here?

The legislated version of science that I challenge throughout this study, the Education Sciences Reform Act of 2002, is not a single piece of legislation that appeared out of nowhere. Its historical context is deep. The influence of similar goals and ideologies in classrooms date back to the 1920’s when science was first used to standardize reading instruction (Shannon, 2007) and much earlier if one looks outside classroom walls. My primary interest abuts this work at the point where specific notions of science became legislated, first in the Reading Excellence Act of 1999, to the current definitions of science in No Child Left Behind of 2002. I begin with a legislative history of scientifically based research in order to better understand its most recent form and illustrate its evolution so that it might be better understood as a concept in flux that can be understood and challenged and why it is necessary to do so. I offer a review of critiques in order to make space for my own challenges detailed over the remaining chapters.

The No Child Left Behind Act of 2002 includes 111 references to Scientifically Based Research (SBR), but legislated definitions of educational research first appeared with the passage of the Reading Excellence Act of 1999 and the original Castle Bill (H.R. 4875, 2000) (Eisenhart and Towne, 2003, p. 32), cumulating in the Education Sciences Reform Act (ESRA) of 2002. In the original Castle Bill, grant-eligible research was defined as “scientifically valid research,” which meant it met the delineated requirements of either “scientifically based qualitative” or “scientifically based quantitative” research standards (Eisenhart, and Towne, 2003, p.32). This quantitative/qualitative distinction was collapsed in the Scientific Research in Education report.

7This bill is otherwise known as The Scientifically Based Education Research, Statistics, Evaluation, and Information Act of 2000. The full text can be found at: http://thomas.loc.gov.
8 See also the 1993 Educational Researcher article by Carl Kaestle: “The Awful Reputation of Educational Research”
Since 1999, there have been multiple definitions of SBR in play over the legislative history of No Child Left Behind (see Eisenhart and Towne, 2003 and Feuer et. al., 2002 for a detailed genealogy). The defining of SBR in the *Scientific Research in Education* report of the National Research Council (NRC) was a move of critical importance because the SRE definition of scientifically based research is currently used to affect the direction of education programs in classrooms and educational research. As NRC members claim, with the sudden and dramatic need for schools to show measurable outcomes, came new incentives for schools to seek guidance from educational research (Feuer et al, 2002, p. 6) as such, what constitutes an acceptable research base from which to draw had to be assured. Under NCLB, the most narrow definition of SBR say Eisenhart and Towne, “Grantees” (commonly known as schools) “are required to develop, select, or implement reading programs grounded in the best science,” ensuring that federal funds for reading education were used in accord with “the best scientific evidence” which required legislators to identify the standard of research that would satisfy the funding requirements (2003, p. 32). The National Research Council also authored *Preventing Reading Difficulties in Young Children* (1998) and *Teaching Children to Read* (National Reading Panel, 2000). The *Scientific Research in Education* (SRE) report will be the focus of this review as it was legislated via the ESRA of 2002, but a follow up to the SRE report titled *Advancing Scientific Research in Education* was published in 2004 (Towne, Wise, & Winters, 2004) that I will overview briefly before proceeding to the original SRE report.

*Advancing Scientific Research in Education* is a users’ guide to SBR as defined in the SRE report; the recommended actions to implement SBR are clearly delineated. Nothing is left to chance or misinterpretation by the key players identified. The SRE report described SBR and the 2004 document tells us how to implement it; its purpose is to: “spur actions that will advance
scientific research in education” (pg. 1). By way of a brief overview, it contains thirteen specific recommendations “for future work in advancing scientific research in education,” divided into three “target audience(s):

1. federal research agencies: “take the lead role in implementing recommendations related to promoting quality” (pg. 74)
2. professional associations and publishers: “develop a knowledge base of education research that accumulates over time” (pg. 74)
3. schools of education and universities: “we target (emphasis added) schools of education and university leaders to reinforce their central role in promoting the development of a highly competent field of education researchers” (pg. 76).

Mention and/or analysis of this 2004 document is absent from the research literature, indicating, perhaps, its unremarkable position in relation to the 2002 work of the National Research Council, the Scientific Research in Education report.

The Scientific Research in Education Report (SRE)

The National Research Council’s (NRC) controversial publication Scientific Research in Education (SRE), which became the basis of the Education Sciences Reform Act of 2002 (ESRA) and NCLB, has been critiqued by qualitative researchers from a variety of theoretical angles. As two of its authors point out, “other researchers” are troubled by the privileging of positivist, experimental designs in NCLB and ESRA (Eisenhart and Towne, 2003, p.31). The SRE report re-defined scientifically based research as a set of six norms, a change from its previous Castle Bill definition with its separate qualitative and quantitative standards. The SRE definition, where science is science (or not), would become the basis of SBR in No Child Left Behind (see figure one), although SBR in NCLB specifically impacts what counts as evidence for schools to earn federal funds, not what research will be federally funded (Eisenhart and Towne, 2003, p. 34). While the SRE claims an inclusive attitude toward qualitative research, others disagree and point out a multitude of contradictions between larger claims and specifics.
within the publication (see Lather, 2004, Bloch, 2004, Moss, 2005, St. Pierre, 2002). Lather claims: “…objectivity is enshrined and prediction, explanation, and verification override description, interpretation, and discovery” (Lather, 2004, p.19). Central to many critiques of the SRE are the six underlying principles that underlie scientific inquiry as delineated in the document. From the report:

The scientific enterprise depends on a healthy community of researchers and is guided by a set of fundamental principles. These principles are not a set of rigid standards for conducting and evaluating individual studies, but rather are a set of norms enforced by the community of researchers that shape scientific understanding. We conclude that six guiding principles underlie all scientific inquiry, including education research (p. 2):

1. Pose Significant Questions That Can Be Investigated Empirically (p. 3)
2. Link Research to Relevant Theory (p. 3)
3. Use Methods That Permit Direct Investigation of the Question (p. 3)
4. Provide a Coherent and Explicit Chain of Reasoning (p. 4)
5. Replicate and Generalize Across Studies (p. 4)
6. Disclose Research to Encourage Professional Scrutiny and Critique (p. 5)

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<th>Document</th>
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<tr>
<td><strong>Advancing Scientific Research in Education of 2004</strong></td>
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<td><strong>Relevant Characteristics</strong></td>
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<tr>
<td>● user’s guide to scientifically based research</td>
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<td>● 13 recommended actions to advance scientifically based research divided by three target audiences</td>
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<td><strong>No Child Left Behind of 2002 (NCLB)</strong></td>
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<tr>
<td>● most restrictive notion of scientifically based research (Eisenhart and Towne, 2003, p. 34)</td>
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<td>● basis for reading program adoption in grant-seeking schools</td>
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<td>● clinical trials are the gold standard for assessment of programs (Eisenhart and Towne, 2003, p.34) leading to increase in standardized test scores</td>
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<td><strong>Education Sciences Reform Act of 2002 (ESRA)</strong></td>
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<td>● focuses on defining educational research, not classroom programming</td>
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<td>● definition of scientifically based research heavily influenced by SRE report</td>
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<td><strong>Scientific Research in Education report of 2002 (SRE)</strong></td>
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<td>● authored by a committee of the National Research Council</td>
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<td>● rejected Castle’s separate definitions of scientifically based research for qualitative and quantitative research</td>
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<td><strong>The Scientifically Based Education Research, Statistics,</strong></td>
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<td>● offered two standards for scientifically based research: qualitative and quantitative</td>
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● education research eligible for federal funding had to be “scientifically valid” (Eisenhart and Towne, 2003, p. 32).
● definitions of scientifically based research drew heavily on the REA (Eisenhart and Towne, 2003, p.32).

The Reading Excellence Act of 1999 (REA)

● required “grantees to develop, select, or implement reading programs grounded in the best science” (Eisenhart and Towne, 2003, p. 32).
● REA definition would be the basis for the definitions of scientifically based research that followed (Eisenhart and Towne, 2003, p.32).

Table 1. overview of scientifically based research legislative history

Critiques of scientifically based research

The SRE report is (pre)policy created by certain people in a specific time and place with certain goals in mind. “Policies always begin with their authors’ ideal image of society and they are intended to be procedural and regulative statements to normatively⁹ realize that ideal” (Edmondson, 2004, p.419). I offer my own critique on the basis of objectivity, evidence, and transparency that challenges the SRE report authors’ ideal image of society that appears to be based on certainties of who can participate or benefit from educational research and the precise regulation of that participation. The following arguments critique scientifically based research from the same concerns with objectivity, evidence, and transparency. Though I separate them for clarity, the concepts resist. Here and throughout the rest of this study, it is difficult to note concerns with objectivity apart from evidence and transparency and perhaps more awkward still to not consider representation explicitly. Some of the authors whose work I review below see their concerns in isolation more than others, but none weigh in on the specifics of how their critiques relate to representation.

⁹ Judith Butler clarifies this term: the “norm” is what binds us together, but it also creates unity though exclusion (Butler, 2003). They are “precepts,” commonly held “presuppositions by which we are oriented and which give direction to our actions” (Butler, 2003, p. 3). Norms “constrain and enable life,” designating in advance what “will and will not be a livable existence” (Butler, 2003, p. 3). She explains that there is a double nature to norms. We cannot do without them and don’t have to assume they are given or fixed, but we cannot accept them as they are (Butler, 2003).
Objectivity

The first group of critics write in challenge to assumptions of objectivity in scientifically based research. Marianne Bloch, identifying her critique only as “somewhat postmodern” (p.105), focuses her concerns on the normative, limiting definitions of SBR and the positioning of other research as messy and poor (2004, p.97). Her concerns lie with the leftovers of objectivity. She notes that ‘appropriate’ or ‘good’ research is narrowed into a constrained notion of what the report writers define as scientific while what’s left: inappropriate research, poor research, poor policy and practice, become defined by elimination as anything that doesn’t fit the NRC description of rigorous science (Bloch, 2004, p.100). She questions the report’s meanings of quality, truth and reality (regimes of truth) and the corresponding space for some visions of education but not others (Bloch, 2004). The SRE’s version of “Rigorous Science” is dangerously defined, she contends, as acultural, apolitical, and asocial (Bloch, 2004, p.100). She concludes: “Because the definition represents universal truths for research that are to be broadly applicable to all or everyone, equity, justice, and difference are often of concern, but only as a way to homogenize everyone to a universality that is not present” (Bloch, 2004, p.107). Elizabeth St. Pierre (2002) points to problems when other theories are dismissed that challenge objectivity. Specifically, she addresses the exclusion of postmodernism exhibited in this passage from SRE:

We assume that it is possible to describe the physical and social world scientifically so that, for example, multiple observers can agree on what they see. Consequently, we reject the postmodernist school of thought when it posits that social science research can never generate objective or trustworthy knowledge2 (pg. 25). 2This description applies to an extreme epistemological perspective that questions the rationality of the scientific enterprise altogether, and instead believes that all knowledge is based on sociological factors like power, influence, and economic factors (Phillips and Burbules, 2000) (pg. 25).

and counters:
Of course, postmodernists and others might say that the idea that multiple observers can agree on what they see is also an extreme epistemological and ontological perspective and that a scientific researcher is pretty naïve not to believe that power, influence, and money affect what counts as knowledge (St. Pierre, 2002, p.25).

She clarifies that postmodernism doesn’t reject objectivity, rationality, or reality, but within postmodernism these terms are situated rather than universal, making the category elastic, diverse, and difficult to reject, noting that the postmodernist studies the limits, usefulness, and dangers of any theoretical position- including postmodernism itself (St. Pierre, 2002, p.26). She speaks not only to the disciplinary work of methodology and theory within objectivity, but also notes the disciplinary power of language used in the SRE report such as:

- a cohesive community with self-regulating norms (pg. 22)
- to foster objectivity through the enforcement of the rules of its ‘forms of life’ (pg. 53)
- training scientists in certain habits of mind (pg. 53)
- research policed by “the watchfulness of the community as a whole” (pg. 53)

She observes that the report seems to be written in a time warp, when many believed that science was objective and existed outside relations of power- “when we actually believed that science would set us free” (St. Pierre, 2002, p.26).

**Evidence**

Maxwell’s critique of SBR considers the methodologies he identifies as experimental, randomized experiments in causal investigations (2004). He is concerned with priorities about what counts as evidence. His critique of SBR as defined in the SRE report centers on its “regularity” approach to causation. The authors, Maxwell claims, present their regularity view of causality with emphasis on establishing whether X caused Y rather than how (2004, p.4). He notes this manifestation in the report: “In all of this work, more knowledge is needed about
causal relationships. In estimating the effects of programs, we urge the expanded use of random assignment” (Shavelson and Towne, 2002, p. 125). Maxwell offers a fundamentally different view of causality based on process theory that challenges the privileged position that SBR gives to randomized controlled experiments, a view of causality he sees as being compatible with qualitative research methodologies (2004, p.3): “Process theory deals with events and the processes that connect them; it is based on analysis of the causal processes by which some events influence others” and is a fundamentally different way of thinking about scientific explanation (Maxwell, 2004, p.5) than advanced by the SRE report, the difference between if and how.

**Transparency**

This last group of work by Lather (2004 and 1996) and Gee (2005) are connected in their concerns around the effects of language in legislated science. Lather’s 2004 critique of SBR is perhaps the most difficult to categorize. She considers broader meanings behind governmental legislation of scientific method, “the structure of the situation,” (2004, p.16) as she puts it. She rejects the document’s constricted notion of accepted methodologies by way of Foucauldian, Feminist, and Postcolonial readings of the “continuities of dominant narratives” (2004, p. 23). This is similar to St. Pierre’s (2002) work, but Lather is not critiquing the postmodern theoretical exclusions as a result of the privileging of objectivity in the SRE report, though she would surely concur with St. Pierre’s position. Instead she uses the rejected postmodern theories themselves to understand scientifically based research through a wider lens. Each of her three theoretical angles rejects the transparent, it-is-what-it-is thinking on which scientifically based research is dependent. For example, her Foucauldian reading of SBR:

Policy is to regulate behavior and render populations productive via a “biopolitics” that entails state intervention in and regulation of the everyday lives of citizens in a “liberal” enough manner to minimize resistance and maximize

For Lather, there is much more at stake than the definition itself. She sees SBR not only as a backlash against the proliferation of research approaches (such as feminist, post-colonial, radical environmentalist) over the past 20 years, but also governmental intrusion into scientific method (Lather, 2004). She questions: “Where are we going with democracy with this project?” (Lather, 2004, p.23). She also weighs in on the status of the social sciences in general, using Foucault to position them not as superior or inferior, but epistemically different: “Here the ‘inexact knowledges’ become ‘a field of strategic possibilities’ (Foucault, 1998, p. 320 cited in Lather, 2004, p.24).

Gee (2005) explicitly takes up the transparency of language in a different approach with his analysis of the six doctrines that determine what is to be scientific according to the SRE. He does not engage in definition critique of what scientifically based research should or shouldn’t be. Rather, he contends that specifying what counts as scientific research is a futile task because claims are always couched in the language of theory and language is given meaning through the theory in which it is read (Gee, 2005). He offers what he characterizes as the “morals” of his analysis of the SRE’s task:

It makes little sense to ask whether a given “result” is “scientific” or “scientifically derived” or whether a given model is “scientific.” These are all relative to given theories of specific domains. It does not make much sense either to ask whether a given theory of a domain is scientific or not- because the theory itself incorporates its own criteria of what counts as evidence, coherence, explicitness, method, significance, and rigor (p.16).

For example, in Gee’s (2005) consideration of principle three, ‘Scientific inquiry must use methods that permit direct investigation of the question,’ he concedes that he doesn’t know what “direct” as used this way means, but reiterates that no scientific inquiry engages in direct
investigation of any question because the only way to investigate is to formulate a question through language couched in theory and employ the theory’s chosen methods of inquiry (Gee, 2005). In response to SRE claim one, ‘Scientific inquiry must pose significant questions that can be investigated empirically,’ Gee asks, “Who determines what counts as significant?” (2005, p.11). Similarly, of the SRE’s second principle ‘Scientific inquiry must link research to relevant theory,’ he implores, “Who says what counts as relevant theory?” (p.12). To illustrate, he offers the example of one sentence analyzed by researchers from different schools of grammar theory across time. Here the “relevant” theory in linguistics (and educational research) means using the right theory to pose one’s questions and claims; what constitutes the right theory is determined by particular researchers working from their specific areas of expertise (Gee, 2005) not governmental oversight. In critique of doctrine four, ‘scientific inquiry must provide a coherent and explicit chain of reasoning,’ predictably he counters, “Who is to say what counts as coherent and explicit?” (p.13) and the understated: “Proponents of one side of the issue find the reasoning of the other side less than fully coherent” (Gee, 2005, p.13).

Writing in 1996 before the beginning of the political onslaught that would be scientifically based research, Patti Lather addressed the tension in feminist academic writing between accessible and complex language (1996). Unlike Gee, she asks not who is to say what counts as coherent and explicit, but instead considers what it means to demand coherent and explicit research: transparency. She provides fitting commentary on SBR’s principle of coherency and explicitness, re-stating that the transparent use of language is not innocent, (Lather, 1996) the basis of many pieces reviewed here. She troubles the common sense rightness, the ideology, of coherency and explicitness evoked by the authors of the SRE report. Claiming that speaking “so as to be understood immediately” means speaking through the “production of
the transparent signifier,” maps easily onto “taken-for-granted regimes of meaning” (Lather, 1996, p.528). The language of the six principles of SRB is to be understood immediately. It is plain and transparent. The irony in her use of the word regime cannot be missed, as the establishment of SBR expands the reach and power of the regime from which it emanates, a regime that has frequently used the language of common sense to advance its policies, selling a “regime of truth” (Foucault, 1980). Finally, speaking to accessible language Lather claims, “Sometimes we need a density that fits the thought being expressed,” (Lather, 1996, p. 528) a wry statement when considered in light of the ideology and its corresponding vision of science being advanced through the six principles of SBR generally, principle four specifically, an ideology that rejects complexity at every turn.

This eclectic sampling of critiques (see figure two) address a variety of issues around scientifically based research as codified in the SRE report. The critics offer an assortment of concerns: the relationship between objectivity and the normative, exclusionary nature of the definition of scientifically based research (Bloch, 2004 and St. Pierre, 2002), specific methodological critiques about what counts as evidence (Maxwell, 2004) and the work of language in defining scientifically based research (Gee, 2005, Lather, 1996 and Lather, 2004). The critiques are useful in understanding some of the most important problems with SBR. They name assumptions, definitions, and restrictions within the SRE report. What the critics do not do, however, is offer explorations in what might be lost if researchers accept the values within the SRE report. They do not offer specific examples of work that falls outside the boundaries they name and they do not consider how the other could be represented outside the principles they critique. It is my intent to raise similar concerns with transparency, objectivity, and evidence
while staying busy with the doing of ethnography that Patti Lather characterizes as a “kind of self-wounding laboratory for discovering the rules by which truth is produced” (Lather, 2001).

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<th>Author</th>
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<th>Salient Points about scientifically based research as defined in the SRE report</th>
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<tbody>
<tr>
<td>Gee (2005)</td>
<td>• transparency</td>
<td>• SRE principles are meaningless outside specific theories used to read them.</td>
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<td></td>
<td>• act of defining SBR</td>
<td>• SRE principles are vague and dangerous because they can be applied based on political interest.</td>
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<td></td>
<td></td>
<td>• the conversation instead should be about goals and theory.</td>
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<tr>
<td>Lather (2004)</td>
<td>• transparency</td>
<td>• SBR excludes qualitative research despite claims to the contrary.</td>
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<td></td>
<td>• act of defining SBR</td>
<td>• the definition of SBR is governmental intrusion: “science for policy” (p.19)</td>
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<tr>
<td>Bloch (2004)</td>
<td>• objectivity</td>
<td>• SBR definition is normalizing, limiting, and exclusionary. Other is messy and poor.</td>
</tr>
<tr>
<td></td>
<td>• definition itself</td>
<td>• questions reports meanings of quality, truth, reality, and assumptions of universality</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• claims there is room for some visions of education, but not others within set parameters</td>
</tr>
<tr>
<td>St. Pierre (2002)</td>
<td>• objectivity</td>
<td>• objects to the SRE report’s explicit rejection of postmodernism</td>
</tr>
<tr>
<td></td>
<td>• definition itself</td>
<td>• critiques disciplinary language of the SRE</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• notes modernist faith in science</td>
</tr>
<tr>
<td>Lather (1996)</td>
<td>• transparency of language used to describe SBR</td>
<td>• pre-scientifically based critique of the ideology behind calls for “plain speaking” (p.525)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>• transparent use of language is not innocent and maps easily onto “taken-for-granted regimes of meaning” (p.528)</td>
</tr>
<tr>
<td>Maxwell (2004)</td>
<td>• evidence</td>
<td>• critiques regularity view of causation: asking if instead of how</td>
</tr>
<tr>
<td></td>
<td>• specific methodological definitions</td>
<td>• offers alternate view of causation, process theory, that is not incompatible with qualitative research</td>
</tr>
</tbody>
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Table 2. summary of critiques cited

SRE report authors Eisenhart and Towne have called for the “shape(ing)” of scientifically based research through “public participation” that would “draw on the strengths of a diverse array of investigators and investigatory techniques” (Eisenhart and Towne, 2003, p.32). Even the brief assortment of critiques offered here would preclude this possibility. To answer the collective critiques, even most of the individual critiques, would require moves far exceeding a
re-shaping of scientifically based research. To define is to limit and normalize and determining
the acceptability of thought through legislation is disciplinary. The privileging of positivist
epistemologies to which Eisenhart and Towne (2003) refer not only means exclusion of research
on methodological grounds, but determines who can and cannot contribute to knowledge about
literacy education. My own study could not fall within a re-shaped version (whatever exactly that
means) of scientifically based research. In addition to the SRE report cited throughout this
chapter, I also offer review of additional published work by NRC so that I might further clarify
the positions from which the SRE report authors work while simultaneously highlighting the
mismatch between our respective stances to inform the reading of the rest of this study.

National Research Council Member Publications

The work of Feuer, Towne, and Shavelson (2002), Eisenhart and Towne (2003) and
Shavelson, Phillips, Towne, and Feuer (2003) all National Research Council members and co-
authors of the SRE report advance, extend, and defend ideas advanced in the document. For
example, the November 2002 work of Feuer, Towne, and Shavelson is self-described as a select
summary and elaboration of the NRC report, specifically the report’s emphasis on the need to
develop a scientific culture governed by norms (2002) defined by individual, institutional, and
organizational compliance with the doctrines in the report. The authors reiterate this necessity
and detail a number of factors that impede this process, citing the “eclectic mix of professionals,”
(p. 7) the “diversity,” (p. 7) and the “proliferation of outcome measures” which present “a
formidable obstacle to replicability” (Feuer, 2002, p.9). Rather, the goal is sameness: “to bring
diverse communities- scientific and otherwise- together to integrate theories and empirical
findings across domains, cultures, and methods” (Feuer, 2002, p.7). Ultimately, “Researchers
must have a clear, commonly held understanding of how scientific claims are warranted” (Feuer,
2002, p.9). The piece takes a noticeably defensive tone at times, opening with an acknowledgment of the “rub” that the legislation “inches dangerously between a prescription of methods and a rigid definition of research quality,” but immediately claiming “the good news” that legislators and “the American people” are manifesting their faith in science (Feuer, 2002, p.4). The authors explain in detail the federal government’s long history of soliciting opinion from the NRC, taking a ‘they asked; we simply delivered tone,’ and describe the legislative historical trend toward defining SBR in quantitative terms implying ‘we weren’t the first folks to think this way; don’t blame us.’ While continuing to assert somewhat repetitively that the SBR as codified in the SRE report is not incompatible with methodologies that fall outside of the strictest interpretation of quantitative research, for example when, “a problem is poorly understood and plausible hypotheses are scant,” or “when clarifying the conditions and context that shape causal connections” (Feuer, 2002, p.8), the intent of the piece is to call for scientifically based researchers to function as a “community” within the legislated norms of evidence-based research. While the authors lament the difficulty of meeting this goal, they do not question the nature of the goal itself. There is no room for the community, as they envision it, to change or even allowance for the discussion of such. If research falls outside of the codified norms it is a hindrance to the goal of coherency across the research community in addition to being non-scientific. Science is science and researchers are researchers under the regime of scientifically based research.

The same team who authored the scientific culture article, (Feuer et al., 2002) with the addition of Denis Phillips, published a second Educational Researcher piece almost immediately following the first. Continuing with the argument that the SRE report allows for research other than randomized field trials (albeit with important caveats), the authors consider the role of the
narrative as evidence within design experiments. Specifically, they argue that design studies can provide, “adequate warrants for their knowledge claims” in spite of the narrative component (Shavelson et al., 2003). In order to understand student learning and document how this develops over the course of the design study, explain the authors, the narrative functions to, “take into account the desires, beliefs, goals, reasoning processes, and so forth over time and that this is best done in the form of a narrative” (Shavelson et al., 2003, p.27). This contribution is deemed valid if it contains “reasonableness of argument as judged by expert practitioners and researchers” 10 (Shavelson et al, 2003, p.27). Maybe. Even if the “community of practice” concurs, the authors follow with a laundry list of concerns that threaten the warrant of knowledge claims in narratives: rival narrative accounts, replicability, generalizability, inclusivity, and accuracy. In summary, “(But) narratives lacking more traditional controls on extraneous variables will not be able to warrant the causal claim” (Shavelson et al., 2003, p.27). This is the so-called integration of methods? Denzin and Lincoln (1998) citing Erickson (1986), reiterate that use of qualitative techniques does not necessarily make for qualitative research: “What makes such work interpretive or qualitative is a matter of substantive focus and intent, rather than of procedure in data collection, that is, a research technique does not constitute a research method” (Erickson, 1986 cited in Denzin and Lincoln, 1998, p.44). Shavelson et al. make claims to methodological inclusivity within design studies (2003). Their characterization of the role of narrative strategies as preliminary and supplementary at best, and most curiously, written with controls on extraneous variables (as if this were possible, particularly within the narrative) belies the authors’ unflinching positivist orientation toward methodology and evidence.

10 This conceptualization of validity coincides with Denzin and Lincoln’s description of validity in qualitative research as “whether or not a given explanation fits a given description. In other words, is the explanation credible” (1998)? They continue with suggestions for evoking validity such as member checking and encouraging access to data by an outsider, not at all unlike what Shavelson et al. almost suggest for the narrative component of design experiments.
I conclude with a brief review of a final piece in which SRE report authors Eisenhart and Towne (2003) outline the chronology of legislative efforts to define SBR and disentangle the various definitions of SBR that appear in NCLB of 2001, ESRA of 2002, and the SRE report. The necessity of this task, stated but limply supported is to, “make clear that public participation can shape a more robust education research agenda that draws on the strengths of a diverse array of investigators and investigatory techniques” (Eisenhart and Towne, 2003, p.32). They caution that if the debate around these definitions does not occur, “political forces will foreclose on a narrow definition of national educational research and make it the standard for federal support” (Eisenhart and Towne, 2003, p.32). To support this point, they offer as evidence the changes made at the What Works Clearing House and the changes in the meaning and scope of SBR from the original Castle bill to the ESRA of 2002, although acquiescing that the legislative changes may seem, “minor to many in the education research community” (p.35) and acknowledge that “many are likely to be disappointed that the changes anticipated by the WWC changes are not more extensive” (Eisenhart and Towne, 2003, p.36). I am not encouraged. I don’t conclude my reading of this last article by NRC members filled with confidence that my public participation can shape a “more robust education research agenda that draws on the strengths of a diverse array of investigators and investigatory techniques” (Eisenhart and Towne, 2003, p.32) so that there may be room for research that falls outside the positivist norm in its various legislated forms. Instead, I take away from this piece the message that the best I can hope for is minor changes in federal policy with which I am supposed to be pleased because they are, essentially, better than nothing.
Conclusion

Unfortunately, I’m not pleased. In fact, in many ways the impetus of this work is my displeasure with (and incomprehension of) the assumptions about science inherent in ESRA and NCLB. Incomprehension because my work highlights the problematics (impossibility?) of key assumptions that frame the documents. I’ve discovered, but didn’t set out to find, complexities about definitions, validity, representation, truth, and evidence, complexities within this work that would unquestionably be deemed un-scientific, that prompt consideration of the tenets being advanced as scientifically based research. My identification as a qualitative researcher requires that I write from the margins, not a bad place to be, but I am an outsider to those on the side of power. Perhaps, too, I am an outsider to those on the side of uniformity, simplicity, and modernist ideals of science as objective and emancipatory. I’ve painted a picture of this ideology with a review of literature that frames the political climate in which I write and have positioned myself with critics of the legislative and theoretical moves of its power players, meaning the NRC and political representatives that claim to represent “the American people.”

Using data from my own research, chapters three, four, and five are models of challenge to the legislated vision of scientifically based research. Each of these three chapters opens with the story of the investigation separate from the more explicitly theoretical work. In chapter three, I use my own positioning within my study to consider the conceptualization of the researcher as either insider or outsider relative to the research participant as a challenge to objectivity. I confront the omnipotent, yet silent voice of the researcher common to positivist work codified as SBR. In chapter four, I continue the analysis of self and other in my own research as I consider evidence and validity. I engage data in a piece of fiction, the writing of which was first prompted by a methodological problem but evolved into a theoretical exploration
of representation, validity, and evidence. In this chapter I push not only against positivist epistemologies, but also against assumptions of traditional or realist ethnographic representation. Chapter five is my last consideration of self and other in research. To consider the construct of transparency, I use the metaphor of friction to understand my own rural subjectivity. In this piece, not only is the use of metaphor itself positioned as a theoretical move against the political norm of simplified language, but my experience with the difficulties of defining a concept that first appeared to be straightforward are detailed. As part of this process, I include a mini-study in which my participant and I dialogue around photographs. Both the photographs themselves and our conversation are offered as evidence and led me not to generalizable and replicable findings, but discoveries about ideologies that surround popular representations of the rural other.

Chapter six concludes the study with reflection on how the original intentions of the study are a mismatch with scientifically based research and what kind of thinking is excluded by the scientism of ESRA. The ambiguities and challenges I encountered along the way are fronted.

In each of these three chapters, I offer the reader an account of practice, a hallmark of validity (Freeman, 2007). Here it takes the form of a narrative component, told across the three core chapters, the intent of which is to make explicit the struggles, questions, and wanderings that impacted the research. This is a poststructural, experimental move that is both a rejection of objectivity and voiceless narration. You will hear me in this work, my bias and my growth. I will not offer “findings” or recommendations on what scientifically based research should be. In my participant’s refusal to concede to the idea of me capturing her through data, the study was cast further astray from the principles of scientifically based research. I found opportunity in the margins:

Instead of emulating the natural or, in Foucauldian terms, “exact” sciences, the goal is getting people to no longer know what to do so that things might be done
differently. This is the yes of the setting-to-work mode of postfoundational theory that faces unanswerable questions, the necessary experience of the impossible, in an effort to foster understanding, reflection, and action instead of a narrow translation of research into practice (Lather, 2004, p.23).

My goal is to trouble scientifically based research in the Education Sciences Reform Act by demonstrating the potential value of research that is “messy” (Bloch, 2004, p.97) and unsettling. In the setting-to-work mode of postfoundational\textsuperscript{11} theory, I will ask the reader to consider a troubling of where we are now as a means of thinking about how things might be different with understanding, reflection, and action. The questions that guide this inquiry are:

What does it mean that “correct,” scientifically based research (SBR) makes unabashed claims to truth and why might this be problematic? What would an educational research methodology look like that did not claim truth of representation? What assumptions are implicit in the legislated version of evidence-based science? Whose voice will not be heard? And- where are we going with democracy with this project?

In order to consider these questions in their broader historical context and resist the ahistoricism that plagues scientifically based research, in chapter two I begin with a history of the role of positivism in educational research.

\textsuperscript{11} See glossary
Chapter Two

Positivism’s history in educational research

While I have positioned the ESRA in its immediate context, this legislation is the outcome of assumptions that have driven educational science for over a century. Early aspirations to establish a science of education were the outcome of the newly institutionalized study of education at the turn of the century (Lagemann, 1997). Considering a history of positivism that spawned ESRA offers “big picture” relevance. I name people, events, and conditions that contributed to its development while also noting impediments of varying effects. If interpretivist and positivist epistemologies are positioned as dueling entities, interpretivist enjoyed only brief moments of influence, while positivism is the clear victor. Historically, it seems that science is positivist. By the end of the nineteenth century, the role of science was to assist in the Enlightenment project of individual and collective freedom by the objective discovery of scientific laws (Shannon, 2007). Accordingly, during the 1890s on university campuses, innovation and progress were increasingly associated with specialization and experimentation (Lagemann, 2000). One result of this was the disciplinary distinction between philosophy and psychology: psychology’s experimental, objective science was preferable for educational research over the abstract values or theological beliefs1 that influenced philosophy (Lagemann, 2000). The public schools of the 1890s, like today’s schools, were charged with solving a variety of social problems. When they failed in this mission, the failure was attributed to low student achievement and inefficiency, a charge that opened the doors to the guidance of outside experts (Lagemann, 1997) and made the time

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1 Nearly a century later, this rejection of values and theology would become an irony in light of the role that the religious right would assume in control of educational policy and research.
right for the influence of Thorndike and Taylor. In the continuing (secular) climate of disciplinary specialization and experimentation of the early 1900s, E.L. Thorndike applied objective scientific research to education, where science, “…knows or should know no favorites and cares for nothing in its conclusions but the truth” (Thorndike, 1906, p. 265, cited in Shannon, 2007, p. 22). According to Thorndike, the truth could be observed and explained by correct observation of simple stimulus and response relationships and consequently translated into “formulas for educational practice” (Lagemann, 2000, p. 57) that teachers of supposed limited intellect could implement.

Thorndike’s behaviorist research agenda was disseminated nation wide by his graduating students, his lectures, and writings until his retirement in 1941 (Lagemann, 2000). Lagemann credits him with three of the most fundamental orientations of early educational (positivist) research:

1. the grounding of educational psychology as narrowly behaviorist in focus, learning was only stimulus and response
2. an extreme emphasis on quantification: “whatever exists at all exists in some amount”
3. a belief in “genetic determinism” or “human nature,” resulting in an emphasis on testing and tracking in education and psychometrics in education research (2000, p. 235)

Thus between 1890 and 1920, education research was firmly established in behaviorism and the accompanying, “techniques and ideologies of quantitative measurements” (Lagemann, 2000, p. 16). During the 1920s skepticism about whether education could be a science led early researchers to place great emphasis on quantification: science would lead to consensus whereas philosophy led to conflict (Lagemann, 2000). The meaning of the science of education, once its existence was established, was contested at times, but always based on the controlled
experiment. By the mid 1920s quantitative measurement was the established standard (Lagemann, 2000).

**Interruption: Dewey**

Dewey’s New Education and the research agenda that might have emerged from Chicago Lab School already had a formidable opponent in scientific management at the time of his resignation in 1904. Because of his abrupt departure from Chicago, he was never able develop a continuing research agenda based on the Lab School (Lagemann, 2000). Too, the “social structure” of knowledge, meaning the trend of specialization and professionalization, was a much better fit for Thorndike’s science than for Dewey’s; thus, his conceptualization of a science of education failed to mature (Lagemann, 2000). While Dewey’s fundamental concerns about children, beliefs about how they learned, and his opinion of their teachers differed dramatically from Thorndike’s, Dewey and Thorndike shared a commitment to experimentation. The location of Dewey’s experimentation, however, was not the university laboratory, but the Lab School. The school as a research setting was understood by Dewey as “under control” and “simplified” socially (Lagemann, 2000). He likened classroom research settings to the labs of hard sciences, but very much unlike Thorndike, his research was grounded by goals of discovering pedagogies that would “socialize and liberate the potential of all children” (Lagemann, 2000, p. 49). The purpose of inquiry for Dewey was “not to discover timeless truths,” but to arrive at temporary, “warranted assertions that aid in transforming the environment so that humanity may maintain and enjoy its precarious existence” (Garrison, 1997, p. 90-91 cited in Cherryholmes, 1999, p. 34). Studies of efficiency meant research inquiries that ask how learning could be extended into the social context outside of school, benefiting both the child and her community (Lagemann, 2000). Dewey’s science was a way of thinking about social, ethical, and moral questions and a
means to foster a participatory democracy (Lather, 2007). Lather notes, too, that Dewey’s “value-laden science” has been reborn, but the emphasis on democracy has been replaced with “management for purposes of governmentality” (Lather, 2007, p. 67). By way of a variety of historical and more recent circumstances, the legislation of scientism in order to serve the greater good, variously defined across time and purpose, is now a legitimate function of government.

Dewey’s science contrasted Thorndike’s behaviorist, decontextualized view of learning where the mind consisted of countless, separate connections, each of which need to be “triggered and trained” individually (1997, p. 8). The direction of educational scholarship during the tenure of the Lab School made for an environment less than hospitable for Dewey’s ideas, and even more than his unfinished work at Chicago, says Lagemann it was, “Thorndike’s rise to prominence that made it unlikely that educational scholarship would develop along the lines Dewey had advocated” (2000, p. 57). Still, Dewey’s New Education was prevalent in a significant number of schools into the 1920s and 1930s and its advocates competed actively with advocates of scientific management from 1910 to the 1930s (Shannon, 2000).

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Frederick Winslow Taylor’s model of production efficiency, scientific management, was a good fit for the emphasis on quantification and efficiency in education. Developed as a means of factory management, but embraced by schools, scientific management was based on the following convictions:

1. science, in the form of the experiment, trumps personal judgment
2. science, in the form of the experiment, can find the “best” (most efficient) way
3. tasks can be best studied and performed if broken into components
4. the worker’s job is to follow scientifically based instructions generated by those in authority who have access to science
Accordingly, the scientific management educational research agenda consisted of three steps: analyze the learning environment to identify the best instructional methods, measure the effects of the methods, and adopt the best methods (Shannon, 2000). “Input-output studies” remained a “staple” of educational research well into the 1960s (Lagemann, 2000, p. 79).

The coherence of the field deteriorated during the 1920s to 1930s in response to the Great Depression and immigration; curriculum research was the center of the debate about the purpose of education (Lagemann, 2000). Child-centered researchers advocated project-based learning not unrelated to Dewey’s New Education, while the visions of those who were society-centered played out as either: “social efficiency,” Bobbitt’s system of employing scientific management to discern the best curriculum to prepare children for employment, or “social reconstructionist,” Counts and Rugg’s effort to enlist schools in social change (Lagemann, 1997, 2000). “Life adjustment” or progressive education was a curricular response to the Great Depression, in which curriculum was diversified and schooling emphasized “practical activities of adult life” (Lagemann, 2000, p. 102). Critics viewed the movement as the beginning of a decline of academic standards that has yet to be entirely reversed (Lagemann, 2000). Lagemann characterizes educational research (curriculum study at the time) during the years between the two world wars as constant struggle and fleeting reform efforts that resulted in little permanent change (2000). Despite intermittent, new considerations of different purposes of educational research between the 1920s and the beginning of the second world war, the principles embodied by Taylor’s scientific management and Thorndike’s emphasis on measurement remained the status quo for educational research.

Neither was qualitative research immune to the thinking of the time. During the same period of time discussed above, from the 1900s to beginning of World War II, ethnographers
wrote “objective,” colonializing accounts of field experiences reflective of the positivist paradigm. Denzin and Lincoln (1998) characterize this as the first moment in qualitative research, the traditional period. The classic ethnographies from the time written by Malinowski and Margaret Mead for example, demonstrate distinct concern with valid, reliable, and objective interpretations (Denzin and Lincoln, 1998, p. 13). While this view has been largely discredited today, relics remain in the form of the realist tale. (see the following discussion of Andrea Fishman’s work) The beginning of the second moment in qualitative research, the modernist phase, began after World War II.

The end of the second World War also marks a shift in the story of positivist research. Any progressive thinking about education and its science that occurred in the interwar years was decisively rejected, as was any positive public sentiment toward educational research. The concern was no longer for education for the masses, but rigor, excellence, and the education of the most able (Lagemann, 2000). During the 1950s and 1960s, Brown, the cold war, and the war on poverty, spurred an unprecedented pool of federal funds made available to support this goal (Lagemann, 1997). The hard sciences were celebrated for the invention of the atomic bomb and thus public concern focused on the public school’s ability to produce “scientific manpower” in the name of national defense (Lagemann, 2000, p. 160). Educational research of the 1950s and 1960s was still firmly aligned with “the scientific method,” a vision of science that not only had the potential to solve the problems of education (Lagemann, 1997) but also could lead to the United States’ world dominance in math and science simultaneously advancing continued military domination in addition to solving domestic issues. But, “Antieducationism” was a persistent theme of the times; the status of the hard sciences rose, while the status of education and its researchers declined. Schools were seen as mediocre, says Lagemann, and “educationists”
were seen as “intellectually vapid” (Lagemann, 2000, p. 161). As a fix for the vapidity and a means to meet military and domestic goals, discipline-based scholars from philosophy, sociology, political science, economics, and history were recruited by schools of education to address curricular reform, thus widening the previously narrow disciplinary focus of the “educationists” (Lagemann, 2000). The result of this research was “course-improvement” projects: courses that were designed by researchers, based in their discipline, and disseminated to teachers via materials and trainings (Lagemann, 1997). This top-down, Research and Development, model would meet significant challenge with the passage of Title I of ESEA of 1965, but not before research out of psychology challenged the Thorndike-tenent of learning as stimulus-response that translated into a drill and practice model of teaching (Lagemann, 1997). Also during the 1960s there was increasing (but temporary) federal support for ethnographic methods in educational research, first as an add-on to address the problems of process-product research (Lagemann, 2000). Later ethnographic research of the 1970s and 1980s challenged the narrowness of process-product research, establishing the role of context and cultural difference in schooling (Lagemann, 2000).

The passage of Title I of the Elementary and Secondary Education Act of 1965 verified through policy a multi-decade long trend toward quantification. Its passage initiated the relationship between federal funding, educational policy, and schools that would continue until present day, while simultaneously setting the stage for federal incursion into what constitutes educational research. At Senator Robert F. Kennedy’s insistence, Title I program evaluation was

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2 Process-product research is described by Lagemann as a methodology where predetermined teacher and student behaviors are observed in attempt to correlate behavior with achievement as measured by test scores. The goal was to identify effective behaviors that could be applied as generalizable treatments. Discovering that generalizable treatments were elusive, researchers used ethnographic methodologies in attempt to “capture some of the local and particular circumstances” that accounted for the differences that interfered with generalizability (Lagemann, 2000, p. 222).
tied to funding in an effort to ensure that federal monies reached the poor children for which they were intended. The program evaluation was to take the form of standardized reading tests that measured speed and accuracy, the results of which were to be made public (Shannon, 2007). Public reporting, it was argued for the first time, would stimulate comparisons between schools, districts, and states, enabling competition and making schools accountable to parents (Shannon, 2007). Officials responsible for the program’s implementation viewed Title I as a renewed experiment in scientific management by which the most cost-effective way of educating the poor could be discovered (Shannon, 2007). Thus, relative worthiness for federal support was quantified and decision making for utilization of monies received was determined by individuals far removed from schools. Both of these developments occurred for the first time with the implementation of Title I in 1965 and are the basis of federal educational policy to the present day. Title I of ESEA of 1965 was re-authorized by President Clinton in 1994 with a commitment to the original spirit of equal education for “all children,” but with the addition of mandatory standards, state testing, and recording of yearly progress for Title I students (Shannon, 2007). Continued lack of (quantifiable) success meant an eventual withdrawal of funding (Shannon, 2007). In a proposed 1998 re-authorization, Clinton envisioned a more potent ESEA that required high stakes testing of all students with aggravated reporting of adequate yearly progress by race, class, and English proficiency (Shannon, 2007). In 2001 ESEA was re-authorized yet again by George W. Bush as No Child Left Behind. The 2001 version retains the core intentions of the original legislation in addition to both the successful 1994 reauthorization and the failed 1998 reauthorization. Most pertinent to the story of the historical domination of positivism is NCLB’s legislative continuation of an increasingly narrow vision of scientific research and its
relationship to quantification, a vision both enforced through funding and enacted in classrooms-science for policy.

The compensatory model of education on which the 1965 ESEA was based met serious challenge with the findings of the Coleman Report in 1966, mandated by the Civil Rights Act of 1964. Coleman surveyed school personnel in an effort to measure equality of opportunity, expecting to find significant differences between predominately white and black schools within the same region (Lagemann, 2000). Instead, he concluded, says Lagemann, that schools are “relatively powerless in overcoming disadvantage,” a finding that constituted a direct challenge to the intent of Title I (2000, p. 196). While the report had little influence on policy, it stimulated research about factors that could support equal educational opportunity (Lagemann, 2000) if school-based compensatory programs such as Title I could not. For educational research, the report suggested that educational research thus far had done little to “increase the instructional power of schools” (Lagemann, 1997, p. 13). Additionally, unfavorable evaluations of Head Start and Title I beginning in the 1970s spurred the growth of educational policy research, “a greater appreciation of the centrality of culture in education, and a new openness to qualitative approaches to educational study, (while) federal support for it dwindled drastically” (Lagemann, 2000, p. 164).

In qualitative research the years from the end of WWII to 1969 are characterized by Denzin and Lincoln as the modernist phase (1998). Denzin and Lincoln describe a “creative ferment” that took the form of a litany of new theories such as ethnomethodology,\(^3\)

\(^3\) Ethnomethodology is associated with Harold Garfinkle and combines phenomenological thinking with a pressing concern for how people construct and organize everyday life (Denzin and Lincoln, 1998). There is no judgment of members or their activities (Denzin and Lincoln, 1998). Behavior is seen as not simply rule governed, but as rational responses given “rules, values, motives, and the like” (Denzin and Lincoln, 1998, p. 142). Traditional ethnographies, based on the premise that language is a “neutral conduit for description” do not fit within ethnomethodology because the approach considers objective reality as interactional, discursive, and constitutive (Denzin and Lincoln, 1998, p. 33).
phomenology, critical theory, and feminism (1998). Research agendas included rigorous qualitative studies intended to give voice to society’s “underclass” (Denzin and Lincoln, 1998) a goal perfectly aligned with the legislative trend toward compensatory educational programs of the period. Rigor, however, was still defined quantitatively. The qualitative researcher in the modernist phase may have combined participant observation or open-ended interviewing with standardized, statistical analysis (Denzin and Lincoln, 1998). The qualitative researcher looked for, “probabilities or support for arguments concerning the likelihood that, or frequency with which, a conclusion in fact applies in a specific situation” (Denzin and Lincoln, 1998, p. 17). Thus, during the modernist period, qualitative research grew in terms of the theories from which research questions were posed, but maintained close allegiance to the positivist confidence in objectivity and numerical representation, a confidence denounced in the third moment of qualitative research.

It was Clifford Geertz, say Denzin and Lincoln, who defined the third moment of qualitative research: blurred genres (1973, 1983 as cited in Denzin and Lincoln, 1998). During this “moment” from 1970 to 1986, Geertz argued that positivist research approaches to the “human disciplines” were being replaced with more interpretative perspectives (Denzin and

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4 Social phenomenology is associated with Alfred Schutz and is concerned with the ways the taken for granted world is both produced and experienced by members. This contrast to a view of the world as existing “out there,” separate from perception or interpretation (Denzin and Lincoln, 1998, p. 139). The concern with subjectivity is a contrast to the “non-existing world created by the scientific observer” (Denzin and Lincoln, 1998, p. 138).

5 Critical theory is marked by its emancipatory agenda and its objection to positivist science (Bernstein, 1995). Generally, critical theory is concerned with the conditions and ideologies that reproduce class structures (Denzin and Lincoln, 1998, p. xi). Thus, it is always concerned with power. Early thinkers include Hegel, Marx, and Horkheimer (Bernstein, 1995). The role of the researcher is to analyze existing social conditions, seek unity with the oppressed, look at the historical constitution of the oppression, and stimulate, but not dictate change.

6 Feminism in its variety of forms fundamentally takes as its starting point the patriarchal organization of society. Feminism was born of the Women’s Liberation Movement of the 1960s. It is both a politics and a theory (Weedon, 1997). It is a politics because its goal is to change existing gender-based power relations (Weedon, 1997). Feminism is constituted by a wide variety of theories because of the wide variety of angles from which to address the patriarchal domination of society.
Researchers engaged with a wide range of theories and methodologies, including feminism and poststructuralism and Geertz proposed that the boundaries between the social sciences and humanities (semiotics, hermeneutics) were blurred (Denzin and Lincoln, 1998). He called for thick description and argued that anthropological writings were “interpretations of interpretations,” denying a privileged voice for the observer (Denzin and Lincoln, 1998). Lack of firm rules about text, standards of evaluation, or subject matter troubled the author’s presence in interpretative text (Geertz, 1998 cited in Denzin and Lincoln, 1998) leading up to the crisis of representation that is the fourth moment of qualitative research and a second setback in the historical domination of positivism during the 1980s.

**Interruption: The 1980s**

In the 1980s, researchers working from interpretative perspectives highlighted weaknesses of process-product research, contributing to new understandings about the relationship between context and schooling (Lagemann, 2000) and marking the beginning of a period of legitimization of qualitative research. Critical and postmodern scholarship “flourished” during the 1980s and there was a resurgence of teacher research (Lagemann, 2000, p. 223) not unrelated to the principles advanced by the whole language movement. Generally, there was growing acceptance of qualitative research: new questions and new methodologies coming into play during a time of “severe retrenchment in the federal role in education research” (Lagemann, 2000, p. 230)

Acknowledgement of factors at work in the classroom such as the importance of context, inadvertently highlighted by process-product research, and consideration of the individualities of learners and teachers do not lend themselves well to experimental methodologies. As such, advocates of whole language questioned the authority of experimental science and drew on
interpretative methodology (Shannon, 2007). They embraced Geertz’s validation of blurred boundaries between the social sciences and humanities. Whole language proponents drew on research from disciplines such as linguistics, sociology, anthropology, and semiotics (Shannon, 2007) in their thinking about how children become literate and in their efforts to influence educational policy. This decade-long project would be seriously undermined by its own success. The research by which it was informed, neither classroom nor university based, was enough to save it from the distortion that resulted from attempts at generalizability, the rise of neoliberalism, and the political ascension of the religious right. When the success of whole-language grew to the point where it became a “program,” packaged, sold, and mandated, it lost both its power as a teacher-student generated approach to literacy and teacher research base to experts. The Religious Right objected to whole-language’s emphasis on textual interpretation (Shannon, 2007). Under whole language principles, meaning is not textually bound, but is created by the reader. This asserted a serious challenge to the authority of religious texts.

The end of the “severe retrenchment” of the federal role in educational research (Lagemann, 2000, p. 230) would also mean the end of this interruption of positivism’s domination of educational research. Methodological growth and limited federal involvement interacted in an inverse relationship foreshadowing a dramatic reversal beginning in the 1990s and continuing to the present day. Challenges to positivist ways of knowing that began in the 1980’s (and before) would be both ignored and discredited in the Education Science Reform Act (Shannon, 2007) and No Child Left Behind. The gains made during this period are often characterized as on-going and frequently referred to in critique of recent legislation.

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In qualitative research of the mid-1980s, still in favor in the mainstream, the work of the third moment is extended into the fourth, the crisis of representation. Qualitative research was reflexive and questioned gender, class, and race (Denzin and Lincoln, 1998). Experimental writing such as fiction, memoir, and drama became common, while at the same time concerns about the author’s unstated claims to validity, reliability, and objectivity were problematized (Denzin and Lincoln, 1998). The crisis of representation, which is a questioning of the ability of the qualitative researcher to capture, versus create, lived experience is joined in this period by a questioning of traditional criteria for assessing qualitative research by traditional means of validity, generalizability, and reliability (Denzin and Lincoln, 1998). The fifth moment, the present, is defined by the questioning of the assumptions of representation and rethinking of the evaluation of qualitative research initiated in the mid-1980s (Denzin and Lincoln, 1998). Qualitative researchers have been expressly reflective and self-evaluative of their theories and methodologies since the end of the modernist phase in 1969. Some of the core questions that qualitative researchers have asked of their own work, specifically about methodology and representation, have also been posed to the legislation of scientism that began with the Reading Excellence Act of 1998 and culminated with No Child Left Behind.

Thus far I have drawn heavily from Ellen Condliffe Lagemann’s (2000) work on the history of educational research to account for the historical domination of the positivist epistemology. I note the merger of psychology and education in the 1890s that produced the behaviorist origins of educational research. I describe how by the 1920s Thorndike and Taylor had established educational research as decidedly quantitative and objective, creating a research climate inhospitable for Dewey’s agenda. World War II produced a surge of faith in the methods of the hard sciences, with the public placing great stock in the ability of science to create bombs.
that win wars. In 1965 ESEA was born. The legislation spawned Title I, creating a partnership of sorts between legislation, school finance, and quantification. Finally, two ESEA reauthorizations, with the Reading Excellence Act sandwiched in between, produced neatly legislated definitions of science that swiftly and decisively made positivism the official victor in the struggle between interpretivism and positivism for legitimacy in educational research. Next I turn to a closer examination of some contemporary conditions that enabled positivism’s current domination.

Who Owns Science?

“The best way to cut through political debate is to measure.”

The one best way of doing educational research has been established by way of legislative control of methodology. Most of the critics reviewed in the previous chapter (Gee 2005, Lather 2004, Bloch 2004, & St. Pierre 2002) have argued against the legislation of methodology, but have stated concerns that transcend the specifics of methodology. Bloch’s (2004) concerns center on the homogeneity and exclusionary implications of a legislated definition of science. St. Pierre (2002) objects to the specific theoretical exclusion of postmodernism. Gee (2005) comments on the futility of defining SBR in terms that can only be understood through theory. Like Gee, Lather (2004) is concerned with the assumptions inherent in the language defining scientifically based research specifically, as well as the governmental act of defining science. Together, most critics argue that “the problem” of SBR cannot be resolved by advocating for changes in methodological requirements that deem research either scientific or not. They see bigger issues. SBR, however, is predicated on the control of research through methodology. Cherryholmes comments on the relationship between theory and methodology by reiterating that “research construes the world” and that one’s selected “research
“Embedded in different research traditions...are visions of what is beautiful and desirable. It does not seem possible to disentangle these visions from the skills required to produce them” (Cherryholmes, 1999, p. 34). Based on this thinking, methodology is inseparable from theory, but more specifically methodology is the tool with which to construe that which one finds beautiful and desirable. Patti Lather clarifies that the basis of critiques of positivist science is neither anti-science nor a methodological debate, but rather a challenge to the traditional philosophy of science with unquestioned rationality, faith in progress, growth, and accumulation of knowledge (2007). Thus, resistance to legislated scientism cannot (should not) be limited to a methodological discussion. Methodology, she claims, “diverts attention from more fundamental issues of epistemology” (2007, p. 38). At issue is the conflict between what interpretivists deem beautiful and desirable and what positivists deem beautiful and desirable. Below, I summarize Cherryholmes’ differentiation between empiricist and interpretivist aesthetic goals:

1. Empiricist research (including quantitative, experimental, quasi-experimental) and its subsequent policy and other implementations values (or finds beautiful) control, efficiency, productivity, and rationality. Reliability is highly prized for its ability to diminish the likelihood of the unexpected and the unwanted and its promise to increase the likelihood of desired outcomes.

2. Interpretivist research (qualitative, ethnographic) seeks to produce understanding, not causality. Interpretivist researchers seek understanding of how participants see the world, not the confirmation of a hypothesis. This requires communication. Interpretive knowledge “increases our ability to imagine ourselves from the perspectives of others.” Interpretivist values (that are beautiful) include self and mutual understanding, communication, affiliation, and connection (1999, p. 35).

Qualitative research’s vision of what is beautiful and desirable, that which reveals and supports diverse, complex understandings of the world, is a threat to many. It has the potential to offer direct challenge to regimes of truth, including the belief in science itself (Lincoln and Cannella, 2004). Lincoln and Cannella offer four reasons why qualitative research has been
demonized and treated as a threat to the Religious Right’s model of “Western civilization” first challenged by the liberal agenda of the 1960s.

1. Qualitative research opposes the science that has enjoyed historical domination, a science that is objective, experimental, and quantitative. The impossibility of objectivity is understood.

2. If qualitative research is not objective, so say its critics, it is subjective and therefore without rigor. Subjectivity in qualitative research challenges the objective/subjective binary on which positivism is based. Rigor, within positivism, can only mean strict adherence to quantitative methods. Researcher biases and assumptions are fronted in qualitative research, whereas they are subsumed under methodology in quantitative studies.

3. Qualitative research often tells stories of oppression, challenging the status-quo and destabilizing opinion of social policy and taken for granted understandings of American life.


These four threats presented by qualitative research, say Lincoln and Cannella, spurred the religious Right’s efforts to affirm a patriarchal, truth-oriented status quo that would ensure their power and control over difference, redressing the “moral decay” in Washington that they regarded as a threat to their way of life (2004, p. 182). The Right’s view of text is one indication of the fear, real or imagined, of loss of control. When text—whether scripture, children’s literature, research data, or a published research article—is open to interpretation, power shifts from author to reader. Now meaning is not guaranteed, but instead is negotiated between text and reader. This is in contrast to Scholes’ conceptualization of “textual fundamentalism,” the belief that “texts always say just what they mean, so that any honest or decent person ought to be able to understand this perfectly clear meaning without making any fuss about it” (1989, p. 52, cited in Cherryholmes, 1993, p. 2). When texts are open to interpretation, it is a particular threat to the
religious Right because the authority of sacred texts and those who use them to various ends is now problematic.\(^7\) So too are the claims of positivism (Cherryholmes, 1993). Yet questioning of science, truth, and textual authority is the basis of qualitative research in its fifth moment, thus positioning the science of the religious right and interpretative epistemologies at odds.

The Religious Right’s intent to influence policy was (and is) accomplished by way of solidarity and organization. Beginning with the Moral Majority in 1979, then the Christian Coalition in the 1980s, the Right- in concert with business, the media, think tanks, foundations, and current patriarch George W. Bush have all worked together to counter attacks not only on “traditional values,” but also diversity in academic research (Lincoln and Cannella, 2004). To counter diversity in academic research, the Right engages a discourse of “mono-intellectualism” (Lincoln and Cannella, 2004, p. 184). Mono-intellectualism, “privileges one view of truth, rigor, standards, and linearity,” in part to counter research methodologies “that challenge their narrow, righteous, truth-oriented view of the world” (Lincoln and Cannella, 2004, p. 184), methodologies excluded under definitions of scientifically based research. Through legislation and funding, the Right’s one best way has been firmly established in an effort to silence voices that may offer alternate views and interpretations that pose a threat to the comforts of mono-intellectualism. Scientifically based research represents a return to modernist conceptualizations of science (Hatch, 2006) in direct conflict with qualitative research. When one considers the four threats posed by qualitative research to the religious Right (Lincoln and Cannella, 2004) in addition to the conflicting aesthetic goals of empiricist and interpretivist researchers (Cherryholmes, 1999), the utility of positivist science to advance conservative ideology is clear.

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While the religious Right, beginning with Regan and culminating with George W. Bush, has triumphed in legislating a constrained definition (and methodology) of educational research, they did not act alone. According to Patti Lather, “The design and application of educational research has become a partisan tool” (Lather, 2004, p. 18) and there is no denying that she is correct. The Right has had a heavy hand in the legislation of positivism and it is indeed gratifying to have a direction in which to point one’s finger. Positivism and the religious Right are obvious partners: they currently “own science.” Perhaps what makes recent developments in the historical trend toward positivism “partisan” is the disavowal, “via nakedly political and self-aggrandizing moves,” of the argument against science-is-science (Lather, 2004, p. 17). The recent uses of educational science are “nakedly political and self-aggrandizing” as the political use of science always is. It is applied (or ignored) to construe what is beautiful and desirable to those who own it. Thus, the “historical amnesia” of decades of critique of positivism (Lather, 2004, p. 27) is a calculated use of science that suits the agenda of the religious Right as much as qualitative research is a threat to its agenda. The two go hand in hand forming a two-pronged rationale for ensuring their outcome of choice, an outcome if not certain, then clearly foreseeable in light of events, people, and circumstances of the last century and before.

However, the legislating of what does and does not constitute educational research has had too long a history to be characterized as anything but a bi-partisan project. After all, it was Clinton’s version of ESEA that became law in 2001: a member of the Religious Right he most certainly is not, but science was his tool as well. Because he “owned science,” he was able to use it to advance his own ideologies, neoliberal ideologies exemplified by the Nation at Risk report released by President Regan in 1983. Nation at Risk called for a complete reform of public schools in order to ensure the country’s ability to compete in the global marketplace (Shannon,
The report claimed that due to the emphasis on equity over excellence in the 1960s and 1970s, American students were no longer skilled enough to meet new global economic demands (Shannon, 2007). *Nation at Risk* is an expression of neoliberal ideology because it concludes that strong economic conditions are the solution to the perceived social problem of under-skilled graduates. The schools that produced the under-skilled students are to blame for compromising the country’s global competitiveness and science is the anecdote to the uncertainty of this situation. Through positivist science, we could (and can) learn what needs to be done to fix schools and alleviate uncertainty. Lather names “unified science” as an ideal neoliberal tool of “crisis management” (Lather, 2004, p. 762) which provides a readily accepted solution to the supposed crisis of our inability to economically dominate the world. The same science that met Clinton’s neoliberal goals in the first reauthorization of ESEA and the Reading Excellence Act of 1998 met both G. W. Bush’s neoconservative ideology and the needs of the religious right. While I have argued that the religious Right has much to gain from the science legislated as ESRA and NCLB and much to fear from alternatives, the Right’s influence also needs to be understood in context of the forces of history and political convenience responsible for the current legislated scientism.

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8 The Striving Readers Act, supported by NCTE, was introduced on March 22, 2007 as a neoliberal solution to “low literacy skills.” The Striving Readers Act provides funding for middle and high school literacy initiatives. The lacking skills that the bill will seek to rectify has been quantified at $16.6 billion yearly, a cost absorbed by the economy to remediate high school graduates. From Jeff Sessions’ Senate Floor Statement introducing the Act: “America’s declining competitiveness in the global economy is due in part to sub-par literacy skills...Our high school graduates continue to lag, as employers move jobs overseas, not for the low-cost of labor alone, but also to take into the highly literate, motivated, and technologically skilled workers that other nations can offer them.” The benefits of cutting the dropout rate are quantified in terms of improved wages ($13.2 billion per year), remediation ($3.7 billion per year), and healthcare costs ($17 billion per year). There is no mention in Sessions’ statement that the evaluation of the initiatives funded by the original legislation required research with a “strong experimental component,” but this language has been replaced with “scientifically valid” evaluative research that may include qualitative methods. Nonetheless, Sessions is proposing that the Act is authorized as part of NCLB. As of July 17, 2007, the Act had been referred to the House subcommittee on higher education, lifelong learning, and competitiveness.

9 Neoconservatives acknowledge inequities, but reject “structural” explanations (Shannon, 2007, p. 116). Neoconservatives will produce limited funding to address the inequities provided that the disadvantaged be taught to make better choices so that they may be independent in the future (Shannon, 2007).
Conclusion

While interpretivist research, in spite of the current political climate, is alive and well as evidenced by the increasing number of faculty positions, conference presentations, journals, books, and courses (Lincoln and Cannella, 2004), the political hostility specifically toward qualitative research is tangible. “Knowledge and how it is created” (Hatch, 2006, p. 406) is indeed in the hand of those who hold political power, as it has been since the inauguration of the federal role in education. What is a qualitative researcher to do? Lather suggests operating from the premise that a satisfactory solution is impossible (2004). She recommends preparing, “to meet the obduracy of the problems and obstacles as the very way toward producing different knowledge and producing knowledge differently” (Lather, 2004, p. 28). Meeting the “obduracy” of the problem in this study means understanding the methodology that I work in opposition to, while employing excluded methodology to produce excluded knowledge. I find the subversion and resistance in this study beautiful and desirable. Historically, interruptions in the domination of positivism have provided opportunities for work like mine that rejects grand narratives in favor of local, tentative understandings. I join the several decade long project of questioning positivist assumptions of objectivity, evidence, transparency, and validity. The immediate contextualization of this study in the previous chapter in addition to the broader contextualization above demonstrate the fortitude of the assumptions that I resist, and perhaps, the necessity of doing so.

The following chapter is the first of the core chapters of the study which challenge research as codified in ESRA. Here I confront the impossibility of objectivity. I analyze my own positioning within my study to consider the conceptualization of the researcher as either insider
or outsider relative to the research participant and speculate on what I learn might mean for interpretation.
Chapter Three

In the first of three vignettes, I use data from my study to illustrate the impossibility of objectivity as manifested in understandings of identity. In the course of my research, I found my own identity as “rural” and how I related to my participant from this position and others to be far more complex than I anticipated. The story of my coming-to-know points to the possibility that the complications within my ethnography mirror those of any methodology. I offer a conceptualization of insider/outsider researcher positioning that resists objectivity. My identity in relation to that of my participant can be understood as existing on a continuum in contrast to a fixed, binary relationship. Implicit in this talk is my assumption that consideration of self and other is vital in educational research, but excluded in ESRA science. I explain how issues of self and other impacted my study and survey existing research to trace the development of insider/outsider research orientations in an effort to move these understandings forward with a different way of considering the relationship between the researcher and the researched, while simultaneously challenging the possibility of objectivity.

“And I see we have a visitor!”

My participant-to-be boomed out the school house door. She didn’t come out. She didn’t walk or march. She boomed. Other than this, I will spare you the standard and tired description of the ethnographer’s arrival on the scene and first meeting of her participants. I came to know her through two different friends. The first friend knew her family through business and facilitated our first contact. The second friend, a fellow teacher, was a mutual friend that we didn’t realize we shared in common until just after we started our work, but I mention her because I soon realized she provided me with important credibility in my participant’s eyes. I
certainly was not Amish, and had no direct connection to anyone who was Amish, but I was a
teacher and that meant something.

In our early work together, it wasn’t our shared subject position of teacher that I expected
to be the basis of our rapport, but rather our shared subject position of “rural woman” or perhaps
“rural mother” or even, most naively, “Christian.” I expected my Amish participant to have deep
religious conviction and I imagined her refusing to associate with someone who didn’t. I was
careful to let her know that I was a church-goer. This attempt to establish this credibility was as
dishonest and unnecessary as the dark, solid colored clothes I wore to our early meetings. My
religious conviction is complicated by matters of selective family history, identity, and my
children. As it turns out, my participant could care less whether I go to church or not, but we
have talked at length about the differences in the level of control our respective churches have
over members’ daily lives. In this way my religious identity was shared discourse, an honest one,
far from what I anticipated.

The subject position “rural woman” that I expected to draw on the most in our
relationship was far less useful than I imagined. To be rural means one has developed a deep
connection with a rural place; the rural place is much more than a backdrop to one’s life. There is
a difference here between residency and inhabitance (Orr, 1992). I’ve defined my identity, in
part, through connection to a rural place (Bushnell, 1999) and census bureau designation as rural
or not matters little in how I see myself. Because my Amish participant lives near my growing up
place, it was easy for me to assume that we shared a meaningful rural subjectivity. Who could be
more rural than she? But clearly, her rural is not my rural. I can talk with Marv, our farming
neighbor, about the finer points of hay. I know how to drive a tractor and use the manure
spreader. I learned to drive a stick shift on gravel roads at thirteen years old. I know the
etiquettes of rural birth, death and disaster, and estate auctions. Perhaps this knowledge might have been useful if this were a study of farming or rural rituals. As it stands now, it was useless as anything but an illustration of how little I understood about what it means to be Amish and the complexities about what it means to be rural.

Over the course of the study, I quickly found truth in Elizabeth St. Pierre’s assertion that notions of refuge, identity, and solace cannot be paired with place. She writes that there is no refuge in identity, that there is no solace in place and we attach ourselves to places because they have a more lasting identity than we do and we unswervingly tend to attach ourselves to identity (St. Pierre, 2000, p. 259). As I’m busy living the identity maintenance work of rural resident, I notice that this fails to provide much in the way of refuge and solace. If anything, the more I engage the question, what does it mean to be rural? the messier my identity becomes. Yet I continue. For rural people like me who live in places adjacent to more urban areas, perhaps the work of maintaining a rural identity is taken on more deliberately in order to quiet uncomfortable challenges to the role we’ve convinced ourselves is important to play. As shopping centers and housing developments advance acre by acre into spaces previously occupied by woodland or field, the development projects mean more than just Low Prices Always; they challenge rural identity. Ten years ago my identity as rural and local would have been self-evident. Today my rural identity exists based only on how I see myself as different from those more urban. Whether the differences are real or imagined, a rural identity is an important part of what I know about myself and how I connect to others. Therefore, I argue not that the category rural should be eliminated but interrogated such that this “common narrative” (Davies, 2000, p. 146) is regarded within discourse as understandable, complex, and changeable. Initially I expected to be able to draw an imaginary line around my participant and me, in effect saying: we are rural. Perhaps I
can to some extent. The imaginary identity line is the short answer to the inevitable question *why study the Amish?*

The longer answer begins with childhood drives through local Amish settlements, with my mother pointing out tidy Amish farms with lush gardens, laundry hanging, and Belgian teams working the fields. From the car it appeared to my mother that the Amish were living her version of the American Dream, the ultimate rural life. Home, family, and community were the center of life. The appeal was replete with rural nostalgia that my mother had a taste of as a small child: making do with what you had, sewing, canning, living off the land. I grew up holding the Amish in the highest regard. During my undergraduate work, this background prompted me to read Andrea Fishman’s *Amish Literacy: What and how it means* (1998). Even then I was much more interested in the story of Fishman’s research than I was in learning what reading material was common in Amish homes. I was largely unmoved by the text and again set my interest aside. As an adult watching development transform my local area, I began to follow controversy about local farmland and open space preservation while simultaneously re-considering the Amish. By this time, in my graduate studies I was reading texts like *Prairie Town: Redefining rural life in the age of globalization* (Edmondson, 2003) that prompted me to ask questions about the multiple literacies at work in Amish communities and those literacies’ relationship to the group’s survival as a whole. This is what I set out to study, but soon became entangled in methodological issues that raised bigger questions having much less to do with the Amish than my mother would prefer.

Even after a childhood of Sunday drives and quilt collecting, I never believed that I had any substantial understanding of Amish culture to bring to the study. I did imagine that our shared knowledge base of rural life, even as different as it was, would facilitate our connections.
After many months of working together it finally occurred to me to ask my participant directly if my being rural had anything to do with our rapport; she was sure that it did not. When I persisted, she told me that we talk about our differences and if I were “from the city” we would simply have more differences to talk about. While I’m not sure I can dismiss the notion as summarily as she, even so early in the study, but her observation has prompted a change in how I view our relationship. Instead of a simplistic notion of a shared rural identity, I eventually came to see a complex web of competing and intersecting discourses and subjectivities. Though both of us are rural women, other more contradictory and powerful positions tend to, but not always, dominate our understandings- or lack thereof. I not only overestimated the importance of my rurality as I understood it, but underestimated the ability of my participant to connect with someone different from herself in important ways.

I planned on working hard to develop a rapport with my participant. So hard, in fact, that I baked: apple crisp, apple cake, banana bread. This is no small detail because for me baking is certainly not something I do regularly nor particularly enjoy. I don’t consider myself capable in the kitchen and avoid it whenever possible. Nonetheless, surmised perhaps from childhood trips to a local Amish bakery, in my mind it was polite to show up with a dessert when visiting with the Amish. (She was still “the Amish” to me then.) I didn’t prepare just any dessert, no chocolate mousse or cannolis ever made the trip, not that I could have pulled that off anyway. The foods I would bring would show my Pennsylvania Dutch heritage and they had to be homemade. My first offering on the day she boomed out the schoolhouse door was banana bread muffins not only because I was relatively sure they would be edible, but I was certain this offering would establish some sort of rural connection that I hoped would eventually lead to the rapport necessary for the kind of research I envisioned. Somehow I had determined that banana bread
was rural food. As it turned out, I held the bag awkwardly as we talked rapid fire well past when she should have called the children in from recess about the differences and similarities between her class and mine. We parted that day with plans to attend each other’s school Christmas programs. I was ecstatic, never imagining her eagerness to talk. I would make consistent and bumbling attempts at connections over the course of our work together, but the rapport was instant and continuous. Having forgotten about the banana bread, I was reminded many months later when she shared with me that her ten year old daughter was beginning to cook independently: simple things like banana bread. While I intended to demonstrate my rural sensibilities and penchant for down-home food that first day, instead I established my cooking skills as being on par with those of her ten year old daughter. As it is, my participant has no adoration of those who cook well. Like me, she avoids the kitchen whenever she can. Here, we connect.

Her visit to my school’s Christmas program was first. When I pulled in the drive to pick her up, my uncomplicated imaginings of how I thought the Amish lived proved inaccurate again when she came out of a small ranch house, identifiable as Amish-owned only by the glow of the orange triangle reflector on the buggy in the garage. Not set on a multi-acre plot, there was no barn, no cows, no long country driveway: it was a decidedly common home. In fact, for a short time I had lived in one almost identical. On the way to the school, I shared my nervousness about coming to her program. I told her how Andrea Fishman’s work in Lancaster County came to an end after she was invited to a Christmas program (1998). She brushed this off, reassuring me that she has “English” friends and that I would not be the only “English” there. I wondered, too, about how she will introduce me to others, but I did not ask.

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1 The Amish refer to anyone who is not Amish, regardless of religion, as “English.”
I was so self-consuming with my own anxieties around my upcoming visit to her school, that I did not consider hers. It wasn’t until we were pulling into the school and she stated matter-of-factly, “I’ll probably be the only Amish here” that I considered the situation I was about to bring her into. All at once she looked nervous and I doubted that bringing her was a good idea. The program was held not in the smaller elementary school all purpose room, but in the high school auditorium. It was a huge space packed to and beyond capacity with children and audience members. We sat and I tried to explain how the show would be organized. She asked no questions. I desperately wanted the show to begin because I didn’t think things were going well and it was too late to do anything about it. We were both uncomfortable. The show began and my discomfort got worse. Children in front of us talked throughout the program in spite of my telling them several times to stop. The fifth grade song included borderline inappropriate hip movements and gestures. This was followed by a wedding-style locomotive line that featured yelling, running kids lapping the stage, one of which pulled the curtain back to expose the next class getting positioned on the stage. I desperately hoped no one would fall and get hurt.

Finally it ended, and we made our way as quickly as possible through the crush of people and out of the auditorium. In the safety of the car, I asked her what she thought. She responded with “cool” and “nice,” but because I was so sure she was horrified by the performance, instead of either listening or prompting her further, I immediately launched into conversation about the issues surrounding the program. I told her how some teachers, myself included, feel the program has gotten too elaborate and time consuming; how some parents complain if their child doesn’t get a big enough speaking role; how some children clearly don’t want to participate at all, but have to. These were all honest critiques of the program and she seemed politely interested, but I found that the part of the program that I really wanted to distance myself from difficult to
describe. I tried: “Some of the dancing is borderline-inappropriate.” She asked what I meant. I feebly tried again, saying what I didn’t really mean: “Some of the running and yelling on stage is not appropriate for school.” I was unable to use the word “sexualized” with her, but that is what I wanted to express. Even though I didn’t say what I wanted to, she said that she understood, though it seems unlikely that she did. Then, remarkably, she said: “Maybe I didn’t notice because I’m an outsider. I thought that’s just the way it was.”

I spent days thinking about this comment. It was so early in our work together, occurring right after our meeting in her school where we connected so easily. At this point, I still felt not only that I knew something about what it means to be Amish, but I was presumptuous enough to think that I knew how she would judge this expression of my culture. I assumed that she would read the events of the evening the same as I did because I was so busy trying to read the night like she. Whatever made me think that this was possible? My effort to connect revealed at once my presumptuous ideas about how I could anticipate her understandings and revealed a place where we could not connect. I couldn’t stand outside myself long enough to consider that she may simply have no judgment of an expression of my culture of which I disapproved. It wasn’t a possibility in my mind that she wouldn’t judge me based on what she saw at my school. We were both outsiders that night. My reaction to her anticipated disapproval was a misguided attempt to manipulate our positions to manage the discomfort I imagined she felt with the content of the program when the only real discomfort was my own. She was perfectly comfortable with being an outsider, but I was intent on minimizing our differences. In my attempts to do so, I revealed troubling assumptions about my own discomfort with our differences and what I thought I knew about her as other.
**Representing the Other**

My concerns and actions related to my position in relation to my participant throughout my research make for, perhaps, mildly entertaining anecdotes that highlight my struggles with this aspect of my research. But my concerns with how I would represent her were a most serious matter. Feminist theory, starting in the 1960’s, has addressed researcher positioning in regards to the problematics of speaking for the oppressed other. Alcoff identifies two claims that have widespread acceptance in feminist research: “Where an individual speaks from affects both the meaning and truth of what she says and thus she cannot assume an ability to transcend her location…(it has) a significant effect on the content of what is said” (1995, p. 98). And: “not only is location epistemically salient but certain privileged locations are discursively dangerous” (Alcoff, 1995, p. 99). By this Alcoff refers to unintended reinforcement of an oppressed group’s powerlessness. The speaker retains the power, doing nothing to disrupt the discursive hierarchies that disempower (Alcoff, 1995). Alcoff purposively conflates the speaking for and speaking about and representation, pointing out that in all practices the researcher represents the other’s, “needs, goals, situations, and in fact, who they are, based on my own situated interpretation. In poststructuralist terms, I am participating in the construction of their subject positions rather than simply discovering their true selves” (Alcoff, 1995, p. 101). While my research is an experiment in representing the other differently (see chapter 4), my role in the construction of the other cannot be overstated. My positionality in the field in relation to my participant is the origin of how I will create her in text and how I will create myself. My representation will be mediated in complex ways by discourse, power, and location (Alcoff, 1995).
A brief outline of the history leading to insider/outsider concerns places the current concerns in a social and historical context.² Pre-dating postmodern and feminist critiques that troubled ethnographic assumptions about representation of the other was the realist tale epitomized by Malinowski’s 1922 Argonauts of the Western Pacific. Referring to Malinowski’s work, Clifford summarizes this mode of modern fieldwork authority: ‘You are there, because I was there’ (1983). In the realist tale, there is no consideration of how the researcher’s orientation as a cultural outsider impacts the work in any way let alone any consideration of the complexities inherent in the insider/outsider categories themselves. Van Maanen characterizes the realist tale as exhibiting four characteristics: experiential authority, native point of view, typical form, and interpretive omnipotence (Van Maanen, 1983). Experiential authority is accomplished via the passing of more-or-less objective data from a silent, sterile, and impersonal narrator: the representation stands for itself (‘The X do this.’ not ‘I saw the X do this’) (Van Maanen, 1983). The native point of view, Van Maanen describes, is characterized as a “swallowing” of the field worker, where the text focuses exclusively on the remarks of the participants in the form of closely edited quotations. Readers expect to learn what the native does and what she thinks (Van Maanen, 1983). The typical form of the realist tale is, “a documentary style focused on minute, sometimes precious, but thoroughly mundane details of everyday life among the people studied….the actions and words of singular persons are minimized in realist tales in favor of what typical natives typically do, say, and think” (Van Maanen, 1983). Van Maanen describes the tone as flat, dry, and unbearably dull where the concrete is favored over the abstract (Van Maanen, 1983). In the realist tale the ethnographer maintains interpretative omnipotence with no

self-reflection or doubt, questioning of interpretation or methodology (Van Maanen, 1983). While Malinowski’s work can be considered a classic example of the realist tale, Van Maanen is careful to point out that the genre survives in spite of “recent” (circa 1983) challenges to its assumptions outlined above. After considering some of the theory that preceded thinking about insider/outsider researcher orientations and offering my own conceptualization of researcher positioning, I will argue that Andrea Fishman’s 1988 study *Amish literacy: What and how it means* is a realist tale that offers glimpses into Fishman’s ideas about insider/outsider positioning.

On the forefront of challenges to the realist tale were postmodernists such as James Clifford. In the mid-1980s, he questioned the possibility of ethnographic conclusions written by outsiders, the ethics of presenting findings as facts, and problematized the concept of objectivity (Wolf, 1992). He claimed in a 1983 piece on ethnographic authority, that ethnography cannot fully escape the reductionist use of dichotomies and essences, but needs to attempt to avoid portraying the abstract, a-historical “other.” In clear contrast to the assumptions of the realist tale, he continues:

> It is more than ever crucial for different peoples to form complex concrete images of one another as well as the relationships of knowledge and power that connect them. But no sovereign scientific method or ethical stance can guarantee the truth of such images. They are constituted- the critique of colonial modes of representation has shown at least this much- in specific historical relations of dominance and dialog (Clifford, 1983).

Further, Clifford and George Marcus called for the authority of the researcher’s voice to be decentered and the researcher’s bias, limitations, and methodologies to be fronted in the text in
addition to concerns about representation of the other by the invisible anthropological self (Wolf, p. 130, 1992).³

**Insider/Outsider**

Clifford challenged the assumptions of the realist tale, but held firmly to notions of an insider/outsider dichotomy. Malinowski never questioned the feasibility of the researcher’s ability to adopt insider thinking and Clifford too describes an untroubled insider orientation to ethnography. He laments that pre-Malinowskian ethnographers did not live in the field for a year or more, and failing to receive this initiation, “they did not speak as cultural insiders, but retained the natural scientists documentary, observational stance” (Clifford, 1983). He calls for more complex representation of the other, but does not consider how the relationship between the researcher and researched is a necessary point of consideration to achieve the analysis he describes.

Although conversations among social science researchers examining the researcher-participant dyad first emerged in the 1970's acknowledging the qualitative difference between work done from insider versus outsider position, it still lacked critical analysis of how the researcher shapes the research (De Andrade, 2000) and how the researcher-researched relationship might be more disorderly than dichotomous. Insider and outsider research orientations are social, historical constructions whose meanings are in flux. Emic and etic, terms coined in 1967, correspond, but are not identical to contemporary understanding of the terms insider and outsider (Young, 2005, p. 152). Young defines emic as having, "personal experience of a culture/society," while etic is described as, "the perspective of a person who has not had a

³ 1960’s feminist thought, however, pre-dates the work of male-dominated postmodernist thinking about representation of the other, power, and methodology, but it was only after this body of work was established that feminist work could be taken seriously (Wolf, 1992). Women, Ruth Behar reminds, are the original Other (Behar, 1995).
personal or 'lived' experience of a particular culture/society" (Young, 2005, p.152). Emic and etic refer to narrower notions of insider/outsider; they do not consider the social contexts acting on a research situation nor the consequences of researcher/participant positioning, which are now increasingly understood to be central to ethnographic interpretation. Two ethnographies by Del Casino (2001) and De Andrade (2000) illustrate this development in the field of ethnography. De Andrade analyzes not only how her orientation as insider studying racial and ethnic identity in the Cape Verdean American community of southeastern New England shaped the research process, but also how her participants affected the process. Del Casino (2001), an "outsider" studying outreach programs for HIV and AIDS in Thailand, discusses how his decision making in the field affected that nature of his participant observation and how the participants themselves swayed both the direction and content of his research.

Like many contemporary ethnographers, both De Andrade and Del Casino, who self-identify as insider or outsider, place their research meta-analysis appropriately at the center of their interpretative work. Their respective insider and outsider identities are changeable and constructed simultaneously via their own notions of self and their participants' view of them as researcher, friend, student, co-worker or group member. Neither insider nor outsider orientation is a taken-for-granted, fixed identity. De Andrade explains the interdependent, constructivist nature of what she conceptualizes as insider status: "Through them, I understood that insider status is not simply granted or achieved. It is created through an ongoing process of evaluation that is dependent on the performance of group membership by researchers and participants at multiple levels" (De Andrade 2000, p.283). Clearly, for De Andrade, birth right is not assurance of insider status. Feminist researcher Linda Alcoff (1995) interrogates how researcher-participant social orientation writes the representation of subjects and controls the totality of the
ethnographic research situation. Both researchers take seriously Alcoff’s view. Insider/outsider positions are complex, socially constructed and the positionality between the researcher and the researched is highly influential on the doing and writing of the work.

The complexities around the terms ‘insider’ and ‘outsider’ in qualitative research are well illustrated in the variety of theories used to make sense of these terms. Clearly representing the most rustic view Brown and Dobrin’s text, *Ethnography unbound: From theory shock to critical praxis* (2004) includes a chapter by Mary Jo Reiff in which she characterizes insider research as a research typology. She claims: “Studying genres within the actual contexts of their use- within real human groups- requires ‘insider’ research, a type of research that can be accomplished through a particular genre, the genre of ethnography” (Reiff, 2004). In this curious passage, she seems to claim that ethnography *requires* insider positioning in order for the work to be considered ethnography in any sense and further that insider research can only happen in ethnography. I take issue with this notion of "insider" (or outsider) as a research typology. How can an insider perspective be transparent and untroubled, something that a researcher can "accomplished" definitively? Robert Merton would likely reject Reiff’s stance. His 1972 characterization of insider/outsider orientation challenges the notion that insider research is privileged in regards to access to social truth; he maintains that both insider and outsider perspectives are needed (Merton, 1972 as cited in Banks, 1998). Patricia Hill Collins, an important feminist theorist, moves Merton’s position in her work on the interaction between gender and race. She conceptualizes the black woman researcher not as insider/outsider, but as outsider-within (Hill Collins, 1990 as cited in Banks, 1998). Banks identifies his “typology of crosscultural researchers” as finer distinctions of both Merton and Hill Collins (1998). He describes his categories as changeable over the course of one’s lifetime: indigenous-insider,
indigenous-outsider, external-insider, external-outsider and acknowledges the impact of the researcher’s “biographical journeys” on the process and product of their research (Banks, 1998).

- The *indigenous-insider* - a crosscultural researcher who not only shares the views of the community being studied, but is recognized as an insider by the members.

- The *indigenous-outsider*, too, was “born-and-raised,” but later adopted conflicting views resulting from a significant connection with an outside culture and is consequently seen as an outsider by the community of origin.

- The *external-insider* was not “born-and-raised.” She rejects the views of her community of origin and adopts the views of the community of study. She is subsequently recognized as an insider.

- The *external-outsider* has no ties to the studies community; she claims objectivity. She has only partial understanding and a lack of empathy that cause her to misunderstand and misinterpret. She sees the community as “Other” and as “pathological or deviant” when compared to her own views. She is criticized by community members and her work may violate her subjects’ integrity.

However, Sandra Acker (2000) offers an example of the application of Banks’ positionality-typologies to categorize the multiple positions she recognized in her team qualitative research project concerning women in academia. Although she identified research relationships in all four categories operating in her study, in her analysis she did not find one category or another to be conducive to greater insight (Acker, 2000). In fact, she considers briefly, “These examples reinforce, once again, the argument that we are none of us always and forever either insiders or outsiders. Our multiple subjectivities allow us to be both insiders and outsiders simultaneously, and to shift back and forth, not quite at will, but with some degree of agency” (Acker, 2000). Like Acker, I don’t see my work aligning neatly with any of these categories. As she sets up her piece, she poses a variety of questions about the implications of insider and outsider research orientation. She also asks, but does not elaborate on nor return to the question: “Is insider-outsider status more a continuum than a clearly delineated affiliation?” (Acker, 2000).
Acker drops this question and moves on in her piece, but I will use examples from my ethnography and work of anthropologist John Hostetler to argue that an insider-outsider continuum is a useful conceptualization of the relationship between the researcher and the researched. I will address the high level of fluidity of my position as researcher over the course of my study and also seek to challenge the conceptualization of insider or outsider as a dualism as embraced by Banks (1998) and others (see De Andrade, 2000, Del Casino, 2001, Giusto, 1997).

Judith Butler identifies the inner/outer body binary as a stabilizing and consolidating distinction (2004). Insider/outsider researcher positioning may be read similarly. When researchers self-position as "insider" or "outsider," we attempt to stabilize, perhaps by looking for labels such as those Banks offers to describe our relationship with our participant. There is an undeniable humanistic need to make the relationship coherent and my work is illustrative of this drive. I didn’t seek to make the relationship as a whole coherent, but rather my strategy was to identify components of our lives where I imagined we had commonalities. I attempted to control a relationship which is tenuous and discursively constituted. We attempt to say in varying degrees, "I am not one of them" or "I am one of them." What does this (mis)construction of coherence conceal? My efforts to make my own research position stable, not as Amish woman, but as rural woman are evident in the story of my research. Oddly, desserts were my tool of choice in my efforts to find some stability in our wildly different positions. I knew I wasn’t an outsider in every way; I wasn’t an external-outsider. Yet I certainly didn’t have the idea that I was conducting insider-research. We were both rural women and teachers. What I consistently failed to do early in our work together was to identify the mismatch between the importance I placed on my own (select) subject positions and the importance with which she regarded these
subject positions. Rural woman, as it turned out, was not a position of much importance, but teacher was. As teachers, we connected to a much greater degree than we did as rural women, particularly in the beginning of our work together. Recall the first meeting in her classroom where we talked easily about our classrooms and students. Even though we were teachers in vastly different settings, we talked easily as teachers about the differences and similarities in our work. I was an outsider to her as an Amish woman, even as an acquaintance, but an insider as a teacher. While this was a significant connection, shouldn’t a researcher seek some shared subject position (or at least shared experience) that precludes deeming oneself absolute outsider to her participants? Acker claims that an interview requires the researcher, “to find common ground and emphasize whatever “side” of oneself will make the best match to the other” (2000). I discovered this is easier said than done. In order to do so, the researcher must make presuppositions about what “side” of oneself will indeed make the best match at the best time and put that side into play accordingly, a process of manipulation, perhaps, but yet has an undeniable role in establishing rapport with new acquaintances. I found this to be difficult and risky work. Learning from the blunders I’ve described in my efforts to control my relative insideness with premature decisions about food and modesty, I question the practicality of Acker’s advice though not her intent. Researchers must look for and work from the match she describes. Are we not compelled to believe that we could connect in some small way with anyone by virtue of shared humanity, let alone those with whom we do research? How can researchers look for connection if they’ve predetermined their definitive outside status? Beyond methodology, how does objective research that does not value connections with participants differ from research that does? What does objective research look like that does not value connections with participants and how does it differ from research that does? Banks posits that
the external-outsider with her claims to objectivity may write research that violates the integrity of the participants and contributes to their disempowerment and oppression (Banks, 1998). Is there more room for latitude, the potential for dangerous representation, when researchers identify as an outsider or do not self-identify at all?

If an outsider orientation to research is problematic, an insider orientation must be equally so. Foucault, writing about genealogy, claims we can never understand the other. Judith Butler quotes: "Nothing in man—not even his body—is sufficiently stable to serve as the basis for self-recognition or for understanding other men" (2004, p. 105). If we indeed cannot understand the other in totality by any means, how might a researcher’s assumed understanding of participants’ lives impact research? What might be left unsaid in analysis? What might be left unsaid between researcher and participant? What might be understood? What presumptions might be accurate or imaginary- and how would the researcher know? I’ve identified a few key instances where my presumptions were inaccurate; likely many more remain unchallenged. Isn’t there a necessary stepping back or distancing in varying degrees in the act of studying regardless of ones relationship to her participants? Can there be interpretation without some degree of othering?

Alcoff claims, "Location and positionality should not be conceived as one-dimensional or static but as multiple and with varying degrees of mobility" (1995, p.106). Like Alcoff, I argue that researchers cannot (and should not) declare themselves consummate insider/outsider in relation to another individual. Instead, I see the orientation of the researcher to the researched as falling on a continuum. The role of the researcher is highly changeable throughout the course of one study, one field experience, or even one conversation. The work of Merton (1972), Hill Collins (1990), and Banks (1998), like Malinowski and Clifford before them, all accept the
premise of the insider/outsider binary, whereas the insider/outsider continuum (Eppley, 2006) rejects stable, binary positioning. Elizabeth St. Pierre (2000) asks:

How do we, rather, escape that binary to negotiate in praxis and represent in text the never ending contradictions that stymie, the looping folds that shift us in some different pause from which we try to make a more tentative sense or the last interpretation that is always presumptuous and often violent?

The insider/outsider continuum is a conceptualization that embraces the contradictions and folds that can move researchers toward a last interpretation that is never the last at all and acknowledges the complexities of representing the Other. Researchers, then, can be neither insider nor outsider; they are instead temporarily and precariously positioned on a continuum, regardless of methodology.

**Insider/Outsider Positioning in Published Work**

*Writing the Amish: the worlds of John A. Hostetler* by David Weaver-Zercher (2005) is a text that challenges stable insider/outsider researcher positioning. It is both a biography of John Hostetler and the story of his ethnographic research with the Old Order Amish of Pennsylvania. John Hostetler was born Amish, but left the church to pursue formal study in anthropology; the Old Order Amish became his ethnographic subjects. The book offers lessons about the interplay between self, audience and participants. The complexity of his positioning illustrates the nature of his researcher positioning as dynamic and tentative, best represented as movement on a continuum. His story illustrates that researchers cannot declare themselves consummate insider/outsider in relation to another individual. Hostetler, in his role as scholar-mediator between his Amish subjects and his varying audiences, was influenced both by his Amish discourse and its accompanying degrees of insider identity and his academic discourse and its disaccord with his Amish participants. Though he positioned himself at differing ends of the
continuum, his membership of both discourse groups persists in his work. Weaver-Zercher (2005) offers several notable examples.

In 1984 Hostetler, by now the preeminent authority on the Old Order Amish, worked actively, but unsuccessfully to block the production of the film *Witness*. This example is particularly useful for illustrating Hostetler’s fluid positioning, as ethics surrounding Hostetler’s own work and the Amish subjects he sought to protect were both called into question (Weaver-Zercher, 2004). His criticism of the film’s intent to profit off the Amish was deemed hypocritical by his adversaries when it was pointed out that Hostetler himself has made his living (as an outsider) “off the Amish” (Weaver-Zercher, 2004). Hostetler, like many ethnographers, engaged his outsider positioning in his efforts to defend and protect his subjects from what he viewed as exploitation. In his 1984 piece, “Marketing the Amish Soul,” about the production of *Witness*, he describes Amish people as “vulnerable,” “easily exploited” and “harmless” (Hostetler, 1984 in Weaver-Zercher, 2005). Hostetler uses his insider-authority language intended to manipulate his English audience into accepting this characterization of the Amish as weak and naïve in his efforts to stop production of Witness. Yet, elsewhere in his writings describing the Amish’s struggle to gain the rights to have their own schools, “The Amish and the Law” (Hostetler, 1984 in Weaver-Zercher, 2005) he characterizes the Amish as able and effective organizers who tap outsider expertise and negotiate the judicial system effectively. Hostetler’s contradictory and purposeful use of language is not taken up by Weaver-Zercher, but offers another example of the inadequacy of conceptualizing insider/outsider as a binary. As a last example, Hostetler not only worked to influence his audience’s perception of the Amish as he deemed necessary, but also worked to influence Amish society itself. In Hostetler’s 1944 piece, “Letter to Amish Bishops Concerning Shunning” (Hostetler in Weaver-Zercher, 2004) he
assumes an insider status in writing a letter to Amish bishops condemning the practice of strict shunning based on theological and psychological objections (Weaver-Zercher, 2004). Writing from the insider end of the continuum on a matter that would only have relevance to Amish people themselves, the authority he evokes is not academic-outsider, but religious-insider; he signs his letter, “an unworthy servant,” and he explains his motivation as, “a sense of duty to warn all who are ignorantly or willfully sinning” (Hostetler, 1944 in Weaver-Zercher 2005). Hostetler’s early, theological writings posit him close to his Amish origins, squarely on the insider end of the continuum.

As a former-Amish male, Hostetler shared specific and important subject positions with his participants, but he could not share the important subject position of current Amish and thus could not declare himself insider, even though he did so when necessary to meet his goals. As a former Amish church member, he was as close to being an insider as any ethnographer studying the Old Order Amish will ever be, but even he could never reach complete insider status. Hostetler’s shifting position on the insider/outsider continuum over the course of three decades of ethnographic work with the Amish has evolved simultaneously with the very notion of what it means to do ethnographic work from an insider’s orientation.

Andrea Fishman’s 1988 ethnography Amish Literacy: What and how it means is an ethnography that illustrates the difficulty, perhaps impossibility, of considering insider/outsider researcher positioning within the assumptions of a realist tale. Undoubtedly, when one reads the bulk of Fishman’s work there is the clear, realist sense of ‘You are there, because I was there’ (Van Maanen, 1983). She takes this tone early in the work, most effectively by including a two and a half page description of the Stoltzfus home and extended descriptions of each family member. No detail is too small:
The bathroom is equally modern and efficient. A long cabinetlike vanity with a white-and-gold flecked formica top and white fiberglass sink faces a white fiberglass bathtub and shower combination and a modern toilet. Two large built-in wooden closets hold linens and toiletries, while a large, sliding-door closet holds hanging clothes. Beneath the single window covered with its white lace curtain sits a traditional silver-painted radiator (Fishman, 1988).

In answer to answer the question she poses: “Who are the seven people living in this house?” she offers description of the participants including: “Eli’s hands may be his most revealing feature. Thick, heavily calloused, and permanently lined with grime, they attest to work done with few labor-saving devices” (Fishman, 1988). While she fails to explain what descriptions of bathrooms and hands have to do with her description of Amish literacy, she most effectively claims her experiential authority: *The X do this.* not ‘*I saw the X do this*’ (Van Maanen, 1983).

The form through which Fishman’s communicates her data and findings is also indicative of the realist tale. The form of the realist tale, Van Maanen claims, employs a documentary style narrative that focuses on “minute, sometimes precious, but thoroughly mundane details of everyday life” (Van Maanen, 1983). Fishman demonstrates this in various places in the text through references to her own participant-observation in the Stoltzfus family’s day to day life. She observes, “…though I had a “formal” set of questions to ask, I could elicit better answers while doing dishes, washing the blackboard, or driving the car than by sitting still attending to pencil and paper (Fishman, 1988). The details of her participants’ life, in addition to their reading and writing behaviors, are reported with detail: leisure and church activities, homemaking, and farm work, giving the text as a whole a decidedly documentary, realist feel. A modern realist tale

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4 In Fishman’s concluding chapter she claims: “Describing the house lead to describing the evidence of literacy in the house, which led to describing the literacy activities I had seen going on in the house (Fishman, 1988). This thinking is not referenced in any way in the chapter from which the passage was pulled. I maintain that this type of description accomplishes more than useful textual organization. Further, there is no explanation of the why a description of Eli’s body constituted data.
must not only, Van Maanen claims, present the natives’ point of view, but also must offer participant perspective. Practices of the group as well as their perspectives are both necessary (1983). Fishman, drawing on Heath and Smith (1982, 1983) and Geertz (1976), illustrates this view when she claims that, “a good ethnography answers two primary questions: “What is going on here?” and “What the devil do they think they’re up to” (Fishman, 1988). The figuring out of “what the devil they think they’re up to” is left to the researcher’s interpretative omnipotence. The ethnographer has the final word on how the data is interpreted and presented to the reader; self-reflection and doubt are not central matters (Van Maanen, 1983). While Fishman talks at length about the messy process of imposing categories on her data and writing her text, she never offers readers the possibility that her interpretations themselves are situated (beyond brief consideration of her role as friend to her primary informant) and may or may not be accurate or complete. She states,

I realized that only through participant observation and subsequent “thick description” of these people in the context of their culture (Geertz, 1973) could I begin to discover and disclose both the basic humanity and cultural complexity central to understanding Amish society (Fishman, 1988).

I argue that social science research is far more complex than a process of discovering and disclosing. Insider/outsider concerns are an important factor in the complexity of human-to-human research. However, there can be no consideration of how the researchers’ outsider status impacts the work in the realist tale (Van Maanen, 1983) or in experimental or quasi-experimental work. One cannot write “even” from qualitative epistemologies where interpretations are presented as final if “findings” are considered in light of how the researcher’s discourses and methodology affect interpretations. What experiential authority can be claimed and communicated through a documentary tone if the researcher is making sense of data based on
what she thinks she is seeing based on her positioning? Most importantly, how can researchers generally (or ethnographers specifically) make untroubled, simplified claims to understanding and representation of another’s point of view? There is a world of difference between reporting, ‘The X do this’ and ‘I saw the X do this.’

While I have argued that there is no room for complex insider/outsider considerations in realist tales, Fishman does not ignore her positioning in her study. She does consider her inside/outside orientation periodically in the set-up of the study and conclusion, but she does not consider how her orientation impacts her analysis. In the first of two examples that demonstrate her outside status, she describes her abrupt termination of access to a local school. Speaking of the school’s teacher she states, “In the last analysis, I was still a colleague but still an outsider, too.” And of her relationship with the parents: “Perhaps if I had gone more slowly, perhaps if I had gotten to know individual families separately and shared more meals and more stories with the other women, my outsider status would have become background to more acceptable, personal qualities” (Fishman, 1988). Here she clearly recognizes that her role as an outsider prevented her from connecting in ways that would facilitate her on-going research. Further, she realizes that her outsider status is provisional and can be negotiated with her participants as other connections develop. While she identified herself as an outsider to the teacher and school families in the opening of the study, this is not reflected in her analysis of her work in Meadowbrook School. Her reader is left to assume, simply, that because she was there, we are there. The “hugging incident” is a second place where Fishman describes her struggle with her outside status. She recounts an incident where she exuberantly kissed and hugged Eli Stoltzfus, her main informant’s husband, in front of a group of his friends. She reflects,

In order to understand, however, I had to see Eli not just as a friend I had missed for months but as an Amishman surrounded by his Amish friends in their rural
Lancaster County context where behavior appropriate to my upbringing did not work at all. In other words, I had to see the world and myself as others did at that moment, not as my usual unexamined ethnocentric perspective would have it (Fishman, 1988).

With the exception of these few outsider examples, overall Fishman writes her research as more of an insider than outsider in relation to the Stoltzfus family, her main informants: “I was a trusted friend with outside interests rather than a friendly, interested outsider…” (Fishman, 1988). In the conclusion of her introductory chapter, she quite clearly designates herself as insider: “Both Anna and I welcome you to our world, to enjoy and perhaps to learn from it as we have” (Fishman, 1988). While the intent and meaning behind this large statement cannot be known for sure, it seems to reflect her belief that by the end of the study she was writing as an insider. She is silent, however, in her realist tale about how her periodic naming of her insider/outsider status had any bearing on how she came to understand Amish literacy. She labels her status from time to time, but does not engage in discussion about the dynamic impact her positioning had on her research.

I have shared some of my own versions of Fishman’s stories from my research and will continue to do so. My intent is not to critique Fishman’s work for what I think it does well or where it fails. It is first and foremost a text located in a specific historical and social context. Published in 1988, it remains the only study of Amish literacy that draws on an extended research relationship with a very limited number of participants. It is the work to which my study will be compared, no matter how different the two pieces are in intent. It is thus important for me to clearly delineate theoretical and methodological differences and similarities between the two studies across the body of this work.

**Asking Better Questions**
Like in Fishman’s and Hostetler’s work, my relative inside/outsideness was ever-present. The dynamic of our relationship impacted my research at every turn. Clearly my lack of association with an Amish church positioned me toward the outsider end of the insider/outsider continuum. When we talk about her church, this is evident; however, we are not as far apart as we would be if I were Muslim, Jewish, or had no background in an organized religion of any kind. Throughout our work together, when we talked as teachers, we connected and I was more of an insider in relation to my participant. Over the course of our work we commiserated about isolation, difficult parents, and unengaged students. We reveled in the feelings we have every first day of school, of being important in the life of a child, and seeing children grow in their abilities. Though we shared these understandings, other powerful discourses disallowed an untroubled connection. She cannot understand my struggles with high stakes testing in Pennsylvania public schools and I cannot understand her rote learning and recitation methodologies that dominate Amish schooling. As compared to my positionality during our talk about religion, my subject position as teacher locates me more on the insider end of the continuum than outsider, but insider/outsider positionality is always complex and tentative depending on the direction of our conversation. Although I failed miserably in identifying my insider movements on the continuum in advance and was unwise, at best, in my efforts to control them, I was right to seek connection with my participant. What I didn’t understand was that the connections were there at times and when they were not, there was no need to attempt to engineer them. I privileged an unambiguous notion of insider research and failed to recognize an alternative model that better represents intersecting and conflicting discourses that impacted the research situation.
Poststructural theories about discourse and identity preclude the possibility of any neat, self-contained discourses or identities. Discourse theory is one way to understand ideas about insider/outsider orientations. In the Foucauldian sense discourses are more than ways of thinking. They are relations in the ways of constituting knowledge, social practices, and forms of subjectivity and power (Weedon, 1997). Identities are constituted by discourses and no meaning (or relationship) exists outside of discourse. “…the “I” has no story of its own that is not also the story of a relation- or set of relations- to a set of norms” (Butler, p. 8, 2005). The failure of my attempts to control and predict discourses that my participant and I may or may not have shared illustrate the impossibility of humanist notions of identities for either of us. Though I began from essentialist notions of “Amishness,” I soon found both of our identities to be constituted by our membership in many discourses, a variety of which swirl around each interactions. We might tap mother in one conversation, teacher in another. When she is an entrepreneur or when I am a graduate student, our discourses conflict. In our work together, I learned that she has membership in a variety of discourse groups that I didn’t anticipate. I chose to focus instead on seemingly random discourses to which she assigned little or no importance. My movement on the insider/outsider continuum is reflective of the discourses in play and has little to do with my attempts to control my movement. Treating the emergence of a new identity (such as questioner) as a discursive event is to refuse a separation between experience and language and to insist instead on the productive quality of discourse (Scott, 1992).

How might work approached from a conceptualization of insider/outsider positioning as existing on a continuum be different from that conceptualized as a binary? Would researchers listen, speak and think about interpretations differently? Elizabeth St. Pierre (2000) questions how the complexity of positioning might be represented in the ethnographic narrative:
Abiding by that inside/outside binary is bound to produce failure. How do we, rather, escape that binary to negotiate in praxis and represent in text the never ending contradictions that stymie, the looping folds that shift us in some different pause from which we try to make a more tentative sense or the last interpretation that is always presumptuous and often violent?

I have been clear that it is not that the categories lack utility, for they clearly need to be engaged at every turn in quality ethnographic work. Positionality needs to be regarded as fluid and wholly dependent on the context of the research situation and central to the research itself. Researchers must constantly consider their elusive and unpredictable positioning and how it writes their work. Conversations with research participants in ethnography have the potential to create development of alternative discourses with which to understand the participants' subjectivity, not completely, but differently from where we started. These conversations, enabled and fettered by shared and conflicting discourses, allow the researcher to imagine an understanding of that positioning at times, but they do not allow complete and untroubled understanding of participants' social locations any more than our insider/outsider status is complete and untroubled.

I did not set out to challenge the insider/outsider binary, but feminist readings such as Alcoff (1995) and Butler (2004) juxtaposed against Weaver-Zercher’s (2005) John Hostetler biography moved my thinking about the researcher/participant relationship. Essentially, I gave myself permission to be an outsider, insider- or most often somewhere between. I realized that holding on to the binary would mean certain failure. I certainly couldn’t write as an insider, but how could I understand anything as an absolute outsider? Letting go of the binary meant changing the focus of my thinking from how I might be more of an insider (which was an exercise in futility anyway) to what it means for my research that I’m neither insider nor outsider. I could now ask better questions that extended beyond the purview of my own study. Reconceptualizing my relationship to my participant also precluded the possibility of crafting a
realist tale for there is no room for questions of representation or identity. This in turn opened up methodological opportunities. The asking of better questions meant questioning the researcher/participant relationship, not only how this dynamic operated in the creation of data but also in its interpretation and reporting.

I claim that consideration of my relationship to my participant enables me to ask “better questions” and chronicle the story of my work to understand our relative positioning. Thus, I work from the assumption that considerations of self and other that I raise here are a valued research enterprise essential within any human-to-human study. Here I make the assumption, an unfounded assumption, that researchers do indeed see the utility of working to understand their participants beyond the data they offer. I’ve described my journey in coming to understand the interdependency of self, other, and data. Yet there is no room for this within replicability and generalizability- perhaps that is just the point in the “mask of methodology.” The constructs encourage researchers to ignore the realities of self and other within claims of objectivity. This first vignette describes my struggles with getting to know my participant outside the methodological boundaries of scientifically based research and how my concerns about our relationship affect not only how I will represent her, but how our identities affect what I think I have learned from and about her. In place of objectivity, I have a relationship with my participant that fosters a far different level of investment and caution than I might have if I engaged in ESRA science. This is what research might look like that shuns objectivity and the mask of methodology. The next vignette details my struggles with the tension between my non-objective representation and the validity of the study.
Chapter Four

Validity

When I first began pitching this project to my committee members and shared my suspicion that I would not be able to tape record data, I was told, “Then you’ve got problems.” Indeed, I did have problems—validity problems. At that time I saw my methodological conundrum as needing to be solved. How could I capture reality without technology? How could my claims be presented as legitimate knowledge? Undeterred that I could not tape record, I considered field notes, letters, and even participant journaling as alternatives. In the end none of these methods were feasible. The tools of the traditional ethnographer would not be used in this work. I wish I could say that this move was purposeful, that I recognized the problematics of reality-capturing from the onset of the project. Instead my response was a panicked belligerence. I was unwilling to abandon the project and replace it with a more traditional line of research, but had no idea how I would resolve the serious questions of methodology that were of concern to my committee. Eventually I understood that the questions could not be resolved. This realization led not to the demise of the project, but to the questions and theoretical framework that drove the research. Instead of asking how I might evoke validity in my work, I considered possibilities outside the limits of the normative framings of validity (Lather, 1993) codified as scientifically based research. I re-conceptualized validity not as a technical problem, but as a theoretical one (Mishler, 1990 cited in Lather, 1993). I never dismiss validity (How could I dismiss something Patti Lather claims to be “obsessed” with?), but consider validity as a theoretical question in my study. My problems with validity became an “apparatus for observing the poses of methodology” a means to see the “unthought in our thought” (Lather, 1993). In this chapter I ask if my unconventional methodology precludes the possibility of the work being seen as valid and
by whom. While emphasizing the timeliness of discussions about standards of evidence in qualitative research, Freeman and her co-authors caution against defining validity:

Our premise is that it is neither desirable nor possible to reach consensus about or prescribe standards of evidence in this diverse field. Such prescriptions, we believe, amount to disciplinary action (Foucault, 1975/1979) that constrain the generation of knowledge rather than improve it (2007, p. 25).

Rather, they define validity as the preferred term for the overall merit of a study, dependent upon the community of practice from which the research emanates such as ethnography, action research, feminist, postmodern, etc. (Freeman, 2007). Generally, they claim validity in qualitative research can be assessed by investigating the match between the status of conventions in the field (such as standards of evidence) against researcher practice (Freeman, 2007).

The structure of theory and the researcher’s account of practice are offered as two hallmarks of exemplary qualitative research (Freeman, 2007). The validity of this work can be judged within the structure of poststructural theory and against legislated positivist epistemologies. Poststructural theory works “the ruins” of humanist conceptualizations of regimes of truth, faith that reason and science will produce true knowledge (St. Pierre, 2000). It is a humanist impulse to try to “get it right,” but we have never gotten it right; one regime of truth simply replaces another (St. Pierre, 2000). (see chapter one for poststructural critiques of scientifically based research.) Poststructural theory works in the margins of, but not in opposition to, humanist notions of the transparency of language (people are how they are represented in print) and the ability to capture and reproduce language (objectively and independent of discourse). Poststructural questions about representation of the self and other and evidence form the core of this work. Account of practice, the second of Freeman’s (2007) hallmarks of exemplary qualitative research, is evident throughout this research. Specifically, the narrative
component of each core chapter tells “the story” of the research and is intended to make explicit my on-going struggles and decisions, while offering the reader insight into my relationship with my participant. The story of my own research, an account of practice, is both the aim and the subject of this work, a means to challenge objectivity, evidence, validity and representation.

While my rejection of validity as a technical issue is based on established postmodern (especially poststructural) assumptions, my university’s social science Institutional Review Board’s Application for use of Human Participants reflects an unwavering confidence in methodology. In addition to sections on the application to determine informed consent and reveal conflicts of interest, questions under “Materials/Equipment” prompt the investigator to describe their data collection tools. The researcher is offered a choice of questionnaires, interviews, surveys, and/or equipment. When I claimed that no pre-set interview questions exist, the IRB administrator claimed that she understood the “fluid nature of ethnographic research,” but needed “interview topics,” a request made in spite of the fact that I supplied broad research questions as required. According to William Tierney, approval of specific questions is an increasingly common request of IRBs (2007). In my experience, the IRB expectations were clear. It’s ok to not do natural science research, but the appropriate natural science tools must be employed: in this case, pre-set interview topics. Tierney summarizes the IRB thinking as: “IRB has given you approval to ask “X” questions, and you cannot deviate from asking these “X” questions without an amendment to your IRB submission” (2007, p.395). Reality must be captured; validity is preemptive. Further, Tierney claims that this stance frames the researcher as an “instrument” (Tierney, 2007, p.395). The approval is given to the questions, not to the breathing, thinking, feeling (qualitative) researcher. Tierney cautions against the temptation to see the problem as an “us” (social science, qualitative researchers) versus “them” (behavioral and
quantitative researchers) conflict. If this were the case, he observes, the solution would be simply a matter of recruiting more of “us” to serve on IRBs (Tierney, 2007). Instead, the problem is bigger. My university’s IRB challenged the very notion of what my research is (and isn’t) and what it means to do educational research in the 21st century.

In the end I satisfied all the IRB’s requests, choosing obedience (or perhaps subversion) over dissent. The “observation instruments” that I reported were Carolyn Frank’s notetaking/notemaking structure (see below) and Seidman’s (1991) three part interview format, which satisfied the IRB’s need for me to identify my “interview topics” in advance. The impossibility of doing ethnography that is something other than extractive was not only clearly communicated, but insisted upon: I would need materials/equipment and an observation instrument in order to proceed. An ethnographic research project without interview questions was not a possibility. At the time of the application, I didn’t know exactly how far afield from the IRB’s vision of social research my work would lead me. I can only imagine the conversation that would have occurred had I indicated that I didn’t plan to write notes in the field (they didn’t ask…) and further, that I planned to engage the data I created in a work of fiction in order to challenge the assumptions of the vision of social research they champion.

**Validity and Data**

In this chapter, I use both my own work and the work of Andrea Fishman to question the nature of representation in ethnographic research and critique its dominant assumptions. Some feminist poststructural theorists have questioned the validity of the “masks of methodology” (Lather, 1993) and further, claim that representations of reality are fictions. I examine these claims and position myself within, specifically considering my difficulties in separating myself from my participant in my representations and explicit construction in fiction.
I claim this work as social science research using narrative strategies and poststructural
theory in spite of my university IRB’s clear restrictions on my methodology. While I produced
the requisite documents and responses in order to earn validation from the board, how will my
lack of hard data influence how this work will be received by its audience? Some may hesitate to
label me as “researcher” at all. I’ve introduced my participant, an Old Order Amish woman who
enthusiastically participated in my research, but will not allow me to tape record and is
uncomfortable with my note taking. As such, the data is born (or created) after our visits when I
write about what I remember hearing and saying using Carolyn Frank’s (1999) note taking/ note
making data collection structure. The data is unruly and coalesces into a mosaic of what was
remembered, what was said and my wonderings, “note making,” about methodology, religion,
community, and about what was remembered and what was said. Ethnographic field work
without a tape recorder is not without precedence. Fishman also sought to record conversations
with her research participants to no avail and also received the same type of vague explanations
from her participants about why the tape recorder was problematic. They identified it only as an
“interruption” (Fishman, 1988). Although I never went so far as to actually bring a tape recorder
to a meeting with my participant, she would likely agree with this characterization. Fishman is
short in her explanation of how she created her data. She states only that she “happily
discovered” the ability to record verbatim can be learned and “data collection meant field notes
written unobtrusively in the field and/or rewritten voluminously as soon as possible” (Fishman,
1988). I take the or in this statement to mean that field notes were written in the field or rewritten
afterwards. Fishman is silent on not only the process of taking notes in the field, but also her
creation of notes from memory when notes were not written on the spot. Further, I notice her
confidence in her ability to take notes verbatim. She doesn’t reveal how frequently she created notes from memory or how this might have affected the research.¹

In my work, I made notes only two or three times (from phone conversations) and work from the premise that data creation demands exploration and analysis. I make the distinction between *taking* notes and *making* notes in accord with Carolyn Frank’s (1999) structure. Data in the note taking column consisted of “cut and dry” text, what I remembered happening without commentary or question. The note making column is where I questioned whether I heard or remembered correctly, made connections, questioned, inserted opinions, and voiced frustration or doubt. Essentially, it is the space where I talked back to the cut and dry notes I “took” For example, this exert is from my notes about our buggy ride.

<table>
<thead>
<tr>
<th><strong>Note Taking</strong></th>
<th><strong>Note Making</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td>She tacks up Sassy in under 5 minutes. Her daughter helps some.</td>
<td>It is clear that S. has done this many times and does not need help. The mare was very quiet. I remember the story she told me last time about the horse taking off with the kids in the buggy with no adult. (no one was holding the horse and the horse thought it was time to go) I wonder briefly if I’m crazy letting my kids do this.</td>
</tr>
<tr>
<td>We pile (literally) in the buggy. We talk about the pace of the horse. S. says she goes “about like this” as long as she is alone but that her husband has to hold her when they go to church because of the other horses on the road.</td>
<td></td>
</tr>
</tbody>
</table>

Although I used Carolyn Frank’s note taking/note making categories as I understood them, I question the premise that one can “take” notes. Text is always created through discourse. What I observe as an undeniable and objective not to mention a reportable happening is only undeniable, objective and reportable through my own discursive lens. This is not a new assertion. In a 1986 article that seeks to define critical ethnography, Simon and Dippo admonish:

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¹ I attempted to contact her via email for clarification, but was unsuccessful.
We must come to grips with the recognition that most ethnographic data is "produced" and not found." In our research work we engage in considerable social interaction with those whom our data reference. We need to recognize our own implication in the production of data and thus must begin to include ourselves (our own practices and their social and historical basis) in our analyses of the situations we study (1986).

While Frank’s (1999) format helped me separate my commentary from my notes, both columns of the text were made not taken. Not having technology to capture data, I was forced to consider the nature of the data that I was creating. Due to the circumstances of this project, I was not able to satisfy myself that I “got it right” with a verbatim record of reality as I chose to capture it, whether by tape recorder, note pad, or letter. I could not consume her life as I saw it and produce the resultant realist description or replicable result. I did not record her life; I created it in my text. What started out first as a technical problem became a theoretical one which prompted me to ask questions about representation and construction of the other within my own work and research legislated as ESRA. The reality that my text offers is a reflection of my participant, the data and me. All three are ethnographic subjects constituted not by transcripts, video tapes and field notes, but by my memory.

My failure to produce data in the form of transcripts denies both my readers and me the satisfaction of telling ourselves that the participants are speaking for themselves on our pages. Transcripts and “verbatim” notes are not transparent. Can participants ever speak for themselves in someone else’s text? Data is constructed by the researcher, who selected and did not select, who analyzed in particular ways or did not analyze, choice lines and exchanges to invite the reader to make specific meanings while knowing all the while that they cannot guarantee the meaning readers will make. Ethnographers contextualize and analyze the transcripts, using language to constitute social reality (Weedon, 1997, p. 22). It is this for which readers eagerly
await: the unfolding of the story the transcripts tell. Though it is the contextualization and analysis from the researcher that gives an ethnography its value and appeal, the hard data remains master of the tale, “for the assumption is that ‘better’ methodology will mean better accounts” (Visweswaran 1994, p. 98). We worry that if it cannot be recorded or verified, measured or counted that it cannot be so or at least cannot be valued. I argue, like others (Gonick, 2003, Wolf, 1992, Viswesaran, 1994) that ethnography by its nature tells an incomplete tale regardless of methodology. I place my methodological failures at the center of my work, recasting them as questions of theory, as invitations to engage data differently and disrupt its very connotation. My representation of data offers “productive ambiguity;” it is more evocative than demonstrative (Eisner, 1997, p. 8).

When I attempted to take notes while talking to my participant on the phone, our dialog was clearly different than when we had more “normal” conversation. When I tried to write down what she said there were unnatural pauses as I wrote and she seemed to talk with uncharacteristic self-consciousness. Because I was so busy writing and listening to her words, it was difficult to listen to her meaning. I listened less to understand than to reproduce; I talked much less than I normally would and the interaction felt much more like an interview than a conversation. In our usual visits without the note pad or recorder, perhaps I listened differently than I might if I knew I had a tape recording to verify our words. During our time together my attention was on her and our conversation. There was no half-listening as I attempted to write notes as she spoke. Removing the obvious tools of data collection from the table, I am obliged (and free) to create a relationship that fosters the free flow of information between researcher and participant, methodology that is a tenet of feminist research (Gonick, 2003, p. 97).
Without data collection tools, I was an explicitly and unconditionally unreliable narrator 
(Visweswaran, 1994) telling a tale based on data created from memory; this was as troublesome 
as it was exciting. How might ethnography like this be different than others? What could be 
gained by relinquishing the note pad and tape recorder and rethinking what counts as data? I 
question what it means to represent the relationship between the educational researcher, the 
participant and the data as intersubjective rather than representative. To what degree can an 
experimental ethnographic project disrupt the construction of the Other? How can projects like 
this one ask questions of experimental work? Does experimental ethnography address the crisis 
of representation: the questioning of established anthropological norms of validity, reliability, 
and most importantly, objectivity (Denzin and Lincoln, 1998)? Can we value a research product 
that does not claim to represent the other, but instead generates a composite based on the 
intersubjectivity of the researcher and participant? I consider these questions in light of thoughts 
about the role of fiction in ethnography and its relationship to representation and construction of 
the other.

Fiction and Representation

Visweswaran makes claims about presumptions of fiction and ethnography. She argues 
that fiction and ethnography both set out to create believable worlds, but the fictional world is 
rejected as factual, whereas the ethnography is accepted as factual (Visweswaran, 2003). But 
this, too, fails to distinguish the two because ethnography also “constructs existing or possible 
worlds, all the while retaining the idea of an alternative “made” world” (Visweswaran, 2003). 
She claims ethnography, like fiction, no matter its pretense to present a self-contained narrative 
or cultural whole, remains incomplete and detached from the realms to which it points 
(Visweswaran, 2003). In this way, she does not distinguish creative fiction from ethnography.
James Clifford also fails to make a clear distinction, noting that his assertion may raise “empiricist hackles.” He states,

> Ethnographic writings can properly be called fictions in the sense of “something made or fashioned,” the principle burden of the word’s Latin root, *fingere*…Interpretive social scientists have recently come to view good ethnographies as “true fictions,” but usually at the cost of weakening the oxymoron, reducing it to the banal claim that all truths are constructed (Clifford, 1986).

Visweswaran resists separation of the novel from anthropology, rejecting both as prototypes of fiction and science respectively, claiming instead that we have yet to understand the relationship of both the novel and autobiography to anthropology (Visweswaran, 2003). Zora Neale Hurston, known predominantly as a novelist, but was a student of anthropologist Franz Boas, produced work in the margins of genre demarcation: novel, auto-biography, ethnography. The same genre ensnarement can be seen in anthropology. Visweswaran reports that Malinowski himself struggled against his urge to read “trashy novels and sought to maintain separation between anthropology and the work of amateur travel writers” (Visweswaran, 2003). His *Diary in the Strict Sense of the Term* (1967) contains a record of his reading material, revelations of guilty feelings about reading fiction, as well as considerations of writing a novel himself. It does not, Visweswaran claims, correspond to the tradition of the anthropological field diary genre for which he is known (2003).

What does it mean for research that the fences between ethnography and fiction are rickety? I do believe the genres are separate- usually, but also think that there is space for reading and writing across genres- as the work of Zora Neale Hurston exemplifies. While my larger work, even with the narrative thread, lies closer to Malinowski than Neale Hurston, my creation of the composite figure works the margins between fiction and ethnography. While the
composite character is fictional, she is based on data that I created. Yet there is much to be gained from reading ethnography as fiction. Being mindful of fiction writing strategies at work in educational research of all kinds fosters critique of the representation readers are asked to believe, challenging fundamental beliefs in research such as validity, reliability, and objectivity (Banks, 1998). How can the author be so certain? Could this claim really hold true elsewhere? Can the author truly step outside her biases and discourses? What techniques does she use to ask me to believe her truth? Explicitly engaging data in fiction in the writing of ethnography, on the other hand, allowed me to consider the relationship between fiction and objectivity and representation. Where I overtly engage in fiction, ‘The Visit,’ it is labeled as such. Otherwise, I consider the bulk of this work as true fiction. Perhaps considering ethnography as “true fiction” is as clear a designation of the relationship between ethnography and fiction as possible. I never intended to deliberately mislead, but at the same time I wrote with awareness of the inherently partial and situated truth that I created.

I’ve considered how I represent my participant both in fiction and by name. In Kamala Visweswaran’s feminist ethnographic work with women in India, she uses only the first initial of the participant’s name as a means to disrupt the authenticity represented by the use of a pseudonym (2003). The initial signifies a move away from the declarative or official historiography founded on transparent “realist” narrative: “the initial stands for a person who shall not be exposed, an identity that will not be elaborated on” (Visweswaran, 2003, p. 61). Thus, my use of “S.” to represent my participant represents a theoretical and methodological statement about the limitations of the claims I will make about her. Picart claims, “A name defines, identifies, embodies. Through it, one gains a face, a body, a voice. But your name also grants others the power to gain access to you; to construct, deconstruct, and reconstruct your
face, body, and voice to suit what they think your name must mean” (Picart, 2002, p. 266). I work against the tidy representation of the other that the pseudonym symbolizes. The pseudonym gives explicit permission for readers to construct participants to suit their pre-existing image. Would my readers accept a name such as Shiba? How would this East African name affect the reading of this work? Would it distract from the mental picture readers make of the white Amish school teacher? 2 I deny readers the satisfaction of an Amish pseudonym such as Rebecca or Mary and in so doing remind them that the access they have to the participant is incomplete and tainted. S. will not be an untroubled representation of an Amish woman.

The assertion that ethnography tells an incomplete tale is not a new one (see Bochner & Ellis, 2002, Gonick, 2003, Wolf, 1992, Viswesaran, 1994) and while I draw on the work of these theorists, my work will not make a substantial contribution to this literature. Instead, I seek to consider the composite figure within the conversation about the use of fiction in ethnography and consider the relationship between fiction and methodology. Haarsager (1998) offers a useful conceptualization of the role of fiction in the truth-telling genre of journalism in her article on fiction techniques in journalism. She identifies “literary journalism” as a descriptive journalistic style that goes beyond standard fact-based, who, what, when, why, where, how accounts (1998). She notes that this means of presenting “truth” (original emphasis) works by “better connecting readers through the power and devices of narrative styles more common to fiction than fact” (Haarsager, 1998). Characteristics of fiction operate both within journalism and ethnography; both are generally regarded as truth-telling genres. Journalism is reliable and balanced (Haarsager, 1998) and seeks to tell the truth, not unlike the realist tale. In my work, the composite figure is a rejection of the claim to truth as much as it is a rejection of the silent or objective narrator, whether the narrator is a reporter or ethnographer. Unlike journalism, post-

2 There are small number African-American Amish who have joined the church through marriage or adoption.
modernism generally, poststructuralism and experimental ethnography more specifically, question the notion that there is an untroubled truth to present regardless of genre:

The opposite of fact isn’t fiction but something like error. The opposite of fiction isn’t truth but something like objectivity or actuality. Any genre or piece of writing that claims to be objective, to represent the actual, is a writing that denies its own existence, as David Locke said. No text is free of self-conscious constructions; no text can act as a mirror to the actual (Banks, 1998, p. 13).

Bochner and Ellis’ collection: *Ethnographically speaking: Authethnography, literature, and aesthetics* (2002) offers two examples of fiction writing in ethnography. One piece ‘The Griot’s many burdens,’ is presented essentially as story alone with only three sentences of concluding commentary. Margery Wolf’s early work, ‘The Hot Spell’ is couched more typically within theoretical work about the use of fiction in ethnography (1992). I see my own work with fiction, the short story ‘The visit’ as distinct from the pieces I cite here. The main character I created is an intentionally created composite of my research participant and me. While there is an established body of work on fiction in ethnography, it asks questions of methodology, whereas my work explicitly engages questions of representation of the other and faith in the truth claims of legislated scientism.

While Visweswaran argues that fiction is ethnography (1994) “The Visit” represents only a part of the whole ethnographic tale that I tell. Writing in 1998 before federal defining of scientifically based research, Anna Banks explicitly challenged “objectivist realism” in social research and claimed, “Academia appears to be the only place where a belief in the possibility of the one absolute truth, and a firm belief in objective facticity, still lingers” (Banks, 1998, p.167). There is no objective facticity here. This ethnography is not a product that neatly represents the subjects of the story while I stay silent. Instead the data, my participant and I interact with methodological wanderings to produce partial knowledge (and fictions). Recognition of this is
accountability to my subject; partial knowledge is not so much choice as necessity (Visweswaran, 1994, p. 50) because of the complexities involved in understanding the other. My methodological situation led me to use fiction in my work and consider the construction of the other. My writing of “The Visit” is an attempt to question the kind of representation I might have constructed if I had the typical data collection tools. Instead of writing a realist tale, I considered representation in both pragmatic and theoretical terms. This led me in turn to a conceptualization of the composite figure and to consideration of issues of representation.

While “The Visit” is fiction, its subjects are familiar and can be found in countless ethnographies: a research participant from a different culture than the researcher, the data, and much less often, the researcher herself. Often these subjects interact in ways both comfortable and predictable to the reader, but here I tell a “disruptive story” (Gonick, 2003, p. 92) that I hope will prompt dialog about our construction of its familiar subjects, initiating and continuing conversation about how we understand them. I share this fictional tale based on my ethnographic data to disrupt notions of the researcher, data and an exotic other. “The Visit” has a distinctly voyeuristic tone characteristic of the realist tale (Van Maanen, 1983). Indeed, I was a voyeur in some regards. Had I not had curiosity about Amish culture, I would not have pursued this project. Striving to not represent my participant as the ultimate rural other, but a composite, is one way that I sought to manage my voyeurism. Another is my methodology. In our talk together, I am just as likely to be the subject of our conversations as she; I name both of us as subjects of the research.

My most compelling goal in writing “The Visit” was the purposeful creation of the character Sarah as a composite of my research participant and me. The use of the composite figure in fiction is a means to experiment with what the differences might be between
consumption of life to produce reality versus recording of life and that reflects reality. The composite is about the other and self and how they are connected in representation. In the process of creating my data after our meetings, I became increasingly aware of how difficult it was at times to separate what I remembered saying myself, from what I remembered hearing my participant say. As I wrote, I constantly asked myself if what I recalled were my own thoughts and words or my participant’s. I wondered what it meant for my work that it was difficult at times to categorize what I recalled. Soon, I became aware of the impossibility of representing her. Our shared experience and hours of talk about adjusting to life outside the elementary school classroom was where I was most aware of tangling of the data. Instead of working harder to separate our thoughts in order to reproduce a more truthful account, our banter over questions around changes in our identities, how others would perceive our decisions and our responsibilities to our students and families became “The Visit.” While the length of this particular relationship spanned several years, I remain unsure how the closeness of researcher-participant relationship matters in terms of difficulties of representation. I suspect that if I had adopted a different methodological approach that would have fostered a different relationship such as engaging in field work with multiple participants, designing a more structured approach for our time together, or had fewer opportunities to meet with my participant, I'd have the same difficulties with how I created data.

I make no attempt in “The Visit” to construct my participant as a representation of the other; she is a fusion of other and self. Can there be validity here? Sarah is presented to you as fiction (though I doubt you will regard her as such). Sarah and her story are explicitly fiction. She does not exist. She is a composite character, an engagement of data that does not claim to represent the other, but yet, she is not a fantasy. The complexities of her character personify the
complexities of the ethnography. Sarah is not constituted by neatly divided binaries: me/not me, Amish/English, fact/fiction, agentic/passive. I intended to direct my readers’ understanding of her, but know that I cannot guarantee the meaning that will be made. She was my intentional creation, yet still autonomous. Sarah helped me raise the question of whether research participants can be cast not as others, but as composite figures. She was an experimentation intended to represent a composite of my participant, the data and me cast in a fictionalized tale. She is fiction; yet I claim that she’s based on data that I created rather than captured. In light of this, can you believe me? What does it mean that you are more likely to question the validity of this work as compared to others that employ more comfortable methodologies and theoretical bases? Taking Freeman’s hallmarks of exemplary qualitative research, structure of theory and the researcher’s account of practice (2007) to heart, I suggest that this work is valid within the paradigm it will be judged, but clearly falls outside the boundaries of scientifically based research. To that end, I continue the narrative thread that offers my account of practice. In this installment and others, I ask for your trust by making explicit my confusions, discomforts, and biases. This narrative segment will help you read The Visit (that immediately follows) so that you might understand the background and constitution of the composite character I created.

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Our next visit was less eventful than the holiday program at the high school. I attended her school’s Christmas program and as promised I was not the only English person there. Admittedly I would have felt very uncomfortable if I had been, though perhaps not as uncomfortable as she felt at mine. The few other English guests were older women, drivers for the school families. I watched the program with a hint of how S. might have felt at my program. I imagined the eyes of the parents on me wondering who I was and who had sanctioned my visit.
As an outsider I watched but did not compare to my school’s program like I was sure that S. did when she attended mine. I judged it only to be interesting, different and appropriate to the context as I understood it. I could not help but recognize that I did not extend the same degree of flexibility to S.

I invited her back to my school, this time to see my fifth graders’ social studies play. It was safe and traditional. Afterward we spent some time in my classroom. Again I was embarrassed- this time because of the math word wall. She commented that the material is what her eighth graders do and that my students were way ahead of hers. Again I found myself uncomfortable with our differences like I was with my school’s Christmas program. Again, I tried to both defend and distance myself. In an effort to limit my responsibility for the content, I explained that the content of the word wall is derived from the state’s high stakes tests. I did not try to explain my complicated feelings about the math curriculum; instead I said only that I’m not sure whether the pushing and pace of the curriculum is good for kids.

I did not explain the conflicted feelings I dealt with as a fifth grade teacher in Pennsylvania. I pushed my students hard, some thrived many didn’t; I let myself be overwhelmed by the power of system. I wanted each child to achieve the highest score she could because scores in the “basic” and “below basic” range led to real consequences for the children when they got to middle school. I felt, too, a need to justify my teaching to others given that I was a vocal opponent of the methods the district was adopting in efforts to raise test scores and standardize instruction. I told myself, and maybe it’s true, that the good teaching and classroom environment of the majority of the year made up for the pace and intensity of math instruction in the six weeks leading up to the test. I knew it was wrong, but I didn’t have the gumption to relinquish the pre-test preparations. I told myself I had to do it, yet I had no problems ignoring a
variety of other mandates. I let my fear of the system overshadow my judgment and confidence in my day-to-day teaching. Still attempting to manage my discomfort with S.’s opinion that her students are behind, I told her that I didn’t have the same level of content when I was in school. She said that she too is teaching content she herself was never expected to learn in school, but the push for her students to do more is parent-driven. Not having systemic pressure, I found it ironic that S. saw her students as being “behind.” Rather, I suspect it is the Pennsylvania public school students who are behind in their schooling: in the relevance of the content and the sense they make of their experience in a high stakes testing environment.

Over time I became increasingly uncomfortable with the philosophical direction in which my district was heading and the philosophy’s intrusion into the classroom. Even though I was vocal in my opposition and subverted at every turn, I began to recognize my complicity in a system that I felt was harmful to children. In the span of a few months, teachers were told that read-alouds must be limited to an approved list and a basal reading series was adopted for the following school year in efforts toward “consistency.” While bigger issues such as pulling out targeted students for PSSA drilling and the disintegration of the school environment were likely more responsible for my dissatisfaction, it was the smaller details, the symptoms of the bigger problems, that prompted me to consider full time graduate study. While making the decision to leave the classroom was not difficult, I found the actual leaving to be significantly more difficult that I anticipated.

S. and I had many hours of talk about leaving. From our very first meeting she told me she was considering leaving the classroom. In many subsequent talks, I heard her describe her dissatisfaction as restlessness, as a discontent with the monotony of the classroom. For her the answer clearly was not to simply resign, but rather to launch a small business on her property.
She felt a small business had the potential to provide income for retirement as well as provide the personal challenge that she felt was missing in her work as a teacher. For both of us the decision to leave was complex, perhaps even more so for her. Both of our decisions were wrapped up in issues of family finances and at first this seemed paramount. For her it meant that her two income family would live on one income while also absorbing the start up costs of the Amish school supply business she intended to start. For me it meant a spouse accepting unappealing but fulltime work that would provide a steady income and benefits for our young family. These issues were resolved over the course of a year for her, more quickly for me, but we soon realized that this was not the most difficult part of leaving the classroom. S. decided that she would not return the following year, told her students, and her school board, but eventually decided to return for another year. She described her (and her husband’s) doubt that she could be happy at home every day even with the store and mentioned that a school board member had spoken to her husband about her resignation. This was enough to put the plan on hold for another year.

Even though I was the only one not returning to the classroom, we talked most often about our identities as teachers and what it would mean to lose them. In both of our communities, being a teacher is held in high regard. Although no one in my family could understand why I would quit a well-paying classroom teaching job, S. describes feelings associated with guilt in wanting to resign. She expressed frustration about why she couldn’t simply be satisfied with what she had. I could relate. Though we had our complaints, overall neither of us was miserable. In some respects I was envious of teachers in my building,

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3 The Amish do not pay into the social security system and thus do not draw funds from the system upon retirement. Contrary to popular belief, they are not supported financially by their larger Amish community except in unusual cases of illness, injury, or disaster. Individuals are largely responsible for providing for their own retirement needs though many will eventually live in an addition built onto an adult child’s home. S. is the mother of just two children, neither of whom is assured to join the church.
especially those to whom I was close in age, who were able to do their job in relative peace even in spite of having many of the same objections as I did about the changes coming into their classrooms. While I was abandoning a sinking ship as I saw it, and enjoyed the support of my colleagues, S. had neither the support nor the luxury of explanation. For her resignation was much more difficult to explain and took more than one try to complete. There were many breakdowns my first semester as I grieved the loss of my teacher identity and adjusted to the demands of graduate school. I missed the sense of connection I had with my school family. I missed feeling like I was part of a group and had an important role to play. I knew that no one worked harder than a classroom teacher and I unexpectedly felt like I was copping out. I deserted the sinking ship instead of staying on to fight the losing battle.\(^4\) S. and I talked about the self-indulgence of leaving the classroom, of not having outside responsibility seven hours a day, of being able to fill our time largely as we chose, but there was another side to the coin. In the classroom I enjoyed a steady flow of affirmations, at least in the non-PSSA months, that my actions had purpose and utility. Here, I was competent. While in graduate school I was as unsure that I made the right decision to move to full time study as I was that I could finish the program. I struggled.

\[\text{The following year when it was S.’s turn to leave the classroom, she seemed to adjust more easily. She didn’t share feelings of loss, but the decision itself had difficult consequences. She described her husband as being supportive of her decision, but doubtful that she would be happy at home full time. She was unsure enough about this aspect that she changed her mind after deciding to resign the first time. Near the conclusion of this extra year of service and after}\]

\(^4\) My district adopted a basal reading series the year following my resignation and they continue to push for better test scores. The last year in the classroom, my PSSA scores were the lowest of the five years I had administered the exams. Predictably, this bothered me even though by the time the scores came back I was no longer employed by the district. I found it very difficult to let go.
she had again informed the board that she would not return, a school board member paid her husband a visit to talk with him about her intentions. Although I found it shocking that the school board did not address these concerns at the very least in her presence, if not to her directly, this was not an issue for S. Upon receiving S.’s resignation, the board identified a teacher to replace her but after having agreed to accept the job, she wanted to back out and thus the school board wanted S. to return for another year. S. told me that that her husband had no intentions of volunteering her to teach another year and was not intimidated in the least by the visit. She said that he listened politely and agreed to talk to her. When he talked with her, again he expressed both his concerns that she would be able to fill her time as well as his support if she was sure in her decision. This time she did not change her mind. A few weeks following her final decision, however, after preparations for her business were initiated, the school board *again* pressured her to return to the classroom when they could not identify a teacher for a different school, her children’s school. *Again* she almost returned, but another teacher was found in time. I almost have to laugh when I imagine my district’s school board petitioning my husband to repeal my resignation. The pressure that she faced is equally unimaginable for me.

Understandably, it was vital for her to have a plan in place before she left. I never heard any doubt that she would find a business to take the place of teaching and she was successful. Her initial idea was to expand her participation in Market America. According to the company’s website [www.marketamerica.com](http://www.marketamerica.com):

> Market America's mission centers on revolutionizing the way consumers shop for products and services. The company is committed to providing high-quality, market-driven products to consumers through its unique and innovative UnFranchise® Business Development System - a system any individual can realistically implement. Market America is boldly charting a course toward Mass Customization and One-to-One Marketing to become the new standard by which other businesses are measured (retrieved 3/16/07).
Market America sells a variety of products including lawn fertilizers, household cleaning products, anti-aging creams, and a wide variety of supplements including “performance formulas” for men, vitamins for children, and antioxidants for pets. S. came to my home to use the computer to do further research on the company in order to help determine the feasibility of increasing her participation. She previously purchased a small stock of products to sell after having been recruited by her brother. He receives a percentage of her sales. Likewise, she would receive a percentage of sales from any sellers she would recruit. Her brother would also receive a percentage of her recruit’s sales. This sounded like a pyramid scheme to me and I was open with her about my opinion. I found this to be an uncomfortable situation because I didn’t doubt her intelligence, but recognized her lack of options. She frequently voiced her frustration about not being permitted by the church to have a computer and I sometimes got the sense that her understanding of how she might be able to make money with a computer was exaggerated. Perhaps this stemmed from the rhetoric of the Market America literature. Up until this point, the Market America products she purchased were mostly for her own use; she had not seriously pursued outside sales. Using my home computer, we began by reading the Market America company website and then watched a DVD that she brought. The DVD’s intended audience presumably was potential sales associates and consisted largely of rags to riches testimonials against a background of mansions and tropical locales, providing little explanation of how the program works. I’m not sure exactly what she wanted to find out about the program in order to help her make her decision, but she didn’t seem satisfied. She was reserved about the program to begin with, but seemed even less so after looking at the company’s information- or lack thereof.

Other ventures included selling Amish baby dolls on Ebay and sewing Amish men’s black felt hats. With the intent to resell, she bought the dolls from her sister’s nearby dry goods
store. They were made by a local Amish woman, dressed in authentic Amish clothes complete with hats and undergarments. The same day she came to my house to research Market America, I took digital pictures of the dolls and later sold them on Ebay at the price she set. She also investigated the on-line Amish craft market, but didn’t find anything that she would be interested in making. That hat project was equally short lived. She took a day class to learn how to sew the hats, which are quite expensive, but ended up using what she learned to make hats for her son and husband instead of to sell. When she described this to me she said with a laugh that her husband was skeptical that she would enjoy the work in light of her oft-stated dislike of sewing. She said that she told him, “Hey, I’ll try anything once.”

In the end, she conceptualized, designed, and opened an Amish school supply store in her basement meeting an important need for all the local Amish schools. Before the store opening, teachers would either have to purchase items sight unseen from a catalog or pay a driver, known as a taxi, to transport them to the nearest store located in Lancaster, Pennsylvania at a cost of about $250. Now teachers in the area can access everything needed to operate an Amish school: the standardized text books, paper, pencils, classroom decorations, markers, stickers, grade books, and standardized tests. S. tells me there is nothing a teacher would need that cannot be bought from her store.

Our time together was easy. I felt obligated to state and restate that I was writing about our talk, and was always unsure how to interpret her lack of interest in exactly what I was writing. Occasionally she would ask how my “papers” were coming, but otherwise seemed largely uninterested. She initiated most of the visits: “When are ya comin’ down?” I thought often about her eagerness to see me. I wondered if it was simply the companionship and developing friendship or a more specific pleasure in talking about teaching or some degree of
intellectual satisfaction in the “depth” of our conversations. She told me that she doesn’t talk “like this” with anyone else, meaning not personally revealing conversations, but talk about her world, my world, and our world. After attending a Nebraska Amish\(^5\) wedding in the neighboring valley, for example, she recounted how she was intrigued by the many differences she saw between her church and the Nebraska group: interior floors were unfinished and paint-spattered, nearly all the food served was raised on-site, lawns were unmowed, plumbing was outdoors. She told me that she questioned her husband about the differences she saw every chance she got throughout the day. She told me with a laugh that she said to him, “I’m starting to sound just like Karen, saying why do they do this, why do they do that?” She always seemed to enjoy our talk about the *whys* of a variety of things from our lives. Still I was uncomfortable with the idea that I was benefiting far more than she from our work together.

While I know this was true, my worry was alleviated somewhat when she began to include me in her preparations for her store opening. I was relieved to be able to help with the process of getting the store up and running. She asked me if I would bring my laptop over and type her inventory and I was happy to do so, eager to offer help again after the Market America day. I created the document at her home, printed it from my home, and mailed it back to her. We took two day trips to Lancaster, Pennsylvania to pick up a load of books for the store and do some other shopping and errands, saving her considerable shipping and transportation costs. She offered gas money that I refused. We stopped in Harrisburg and went inside the state capitol,\(^98\)

\(^5\) There are two groups of Old Order Amish: the Lancaster Amish and the Nebraska Amish. The Nebraska Amish first settled in Nebraska after immigration from Europe; while the Lancaster Amish settled in Lancaster, Pennsylvania. The two groups have little interaction. The Nebraska Amish are decidedly more conservative and plain, rejecting many conveniences and technologies, such as indoor plumbing and built-in kitchen cabinetry, which the Lancaster Amish adapted long ago. They are known as the white-toppers for their white roofed buggies or the brown Amish for their brown pants and jackets.
which was a first for her. These are small gestures on my part, but I was grateful that she let me reciprocate in tangible ways.

While I have no way of knowing why she participated so eagerly, I suspect it was a variety of factors. Perhaps in the earliest meetings, it was a combination of rapport and the teacher-to-teacher talk that alleviated her boredom with her classroom. Later my willingness to help with the store might have been a small factor, though because I couldn’t offer anything that was essential or impossible for her to do for herself, it was more likely the offer of help than it was the help I actually provided. Facilitating her access to the internet and the DVD seemed to satisfy a curiosity, likely nothing more. The principal reason I think she participated was the friendship that formed and continue to maintain, developed over years of meetings. I like to think that my willingness to do my research without formal structure, like the three part interview format of Seidman (2003) that I planned to use, allowed us to interact in more authentic, spontaneous ways with which we were both more comfortable. She was eager to both explain and question things that mattered to her. Because I was did not try to interview her nor make our conversation fit in a prescribed structure, I was able to respond genuinely; I think this had a significant positive impact on our relationship and the research.

Although I knew it wouldn’t fit within the relationship that had emerged, I felt obliged to ask her if I could tape record our conversations. She said no. She said that she doesn’t mind “sharing” but that she “would be too careful and nervous” about what she said if I were recording our conversations. Not willing to let go, several months later I asked her if she would consider writing letters to me and she reluctantly agreed to try. I wrote to her but did not receive a response. When we met again, she apologized and said that she tried several times but just “ripped them up.” She said, “That’s just not me.” I explained again my concern about getting her
words right, but by this point I started to understand that this concern was mine alone. She had little interest in the text I created and no interest in assisting my troubled efforts to get it right.

The Visit

“Sarah, are you expecting the ministers?”

*“I felt a short prick of panic when I joined Lara at the window as the buggy pulled in. What did they want with John and me? In my mind I thought back to the visits that Jacob got about his computer and Lara and Jesse about the truck. We haven’t been bothered since the machines were sitting out back. These so called “visits” make me so angry. If I didn’t want to be Amish, I would leave the church; I’m not stupid. I just want to be left alone. Lara and I stood at the window and watched as John greeted the men and all three went back into the garage. Suddenly, I knew what they wanted with John. “No, I’m not expecting them, but I bet they are here to talk about me teaching next year. It’s been two weeks since I gave my notice. They’re here to try to get John to get me to go back next year.”

“So, why aren’t you coming back to Sunny View next year?*” *It was a simple enough question and one I’ve brushed off half a dozen times since April. But now it was coming from Lara, my dearest sister. I’ve never kept anything from her. Besides, the story has been emerging and coming together in my mind lately and I feel as if I might be able to put it into words. Still, I hesitated, reluctant to dredge up the doubts that I turned my back on once the decision was made, but I’m confident I can hold them in check. Lara has been so kind to avoid asking me directly until now. I would never stand for it from her if the tables were turned.

“This probably sounds funny, but at first I thought it was the weather. You know how I dread winter coming every year. I couldn’t believe that only halfway through my second year
back I was already feeling restless. Things were going so well in the classroom, on an even keel; I constantly asked myself why I couldn’t just be satisfied with what I had. When I came back to Sunny View to teach after being home when Levi and Emma were babies, it was a huge challenge for me to take over for Mary with only eight weeks to go in the school year. You remember what a mess the school was, how people talked about that poor woman. I always felt bad about that. I’m glad she goes to another church; it might have been really awkward. Then again, maybe she was simply glad that I agreed to take over for her so late in the year so she could get out. I don’t know her well at all, you don’t either, do you? Anyway, I always hoped that she didn’t resent me for what I was able to do with the school. From day one I could see why she didn’t want to finish the year. Those kids had no respect! They had been running the show for so long that I think some of them didn’t know how to stop. They desperately needed a fresh start and I’m convinced that things had gotten so far off track that Mary couldn’t have gotten them back even if she wanted to. Some people just aren’t cut out to be teachers.

“I absolutely had doubts! The first two weeks were miserable. John thought I was crazy to go back at that point in the school year, but after being married almost ten years, I think he understands me. So he encouraged me to get to the end of the year with the kids until I could have a fresh start in September. I love that about him. You know how supportive he is. Still, I seriously doubted myself. I could not believe what I had gotten myself into, but I think even then, deep down, I was enjoying the challenge: having something to get me out of the house, finding ways to talk to parents about their kids when I needed to, most of all, watching the kids watching me. Seeing them size me up and deciding that they could trust me enough to let me teach them. They didn’t respect Mary and she didn’t respect them so they were getting 50’s and 60’s and they were really unhappy. I told them every day that I knew they could do better. I
really think they wanted to do better, too. Before I knew it, things were so good. When I think of how far they’ve come and how much happier they are... They’re not perfect, but really good. I couldn’t have asked for better kids. Most of the time, I could just look at them and they knew they needed to behave. There was such respect between us.”

“I looked at Lara. It was all I could bear to say and she knew it. The kids. It’s still the hardest part. I don’t know if I’ll ever be completely sure that I made the right decision. How could I simply wake up one morning and not be a teacher? And what reason do I have not to be? I’ve told myself that there must be something more out there for me to do. But what if I’m wrong? What if being there for the children is the “something more” that I’m looking for? With the trouble the board has had finding someone to take over for me at Sunny View, I’ve certainly had reason to believe that maybe this store wasn’t meant to be. Maybe I should be serving the children instead of serving myself? I have no doubt that this is exactly what the ministers are telling John out in the garage, but I’ve made my decision and I want him to stand up to them. He’s certainly done it before. We can’t let them run our lives…

“So, the decision was really hard. To this day, I’m not sure John really understands why I left and I would love to know what is being said out there, but I couldn’t have asked him to be more supportive. I’ll never forget him setting up the shelves in the basement for me when I was first setting up the store; I’m not sure I’ve ever heard him swear like that! He asked me that night if I was really sure that I wanted to do this. I said I absolutely was because at that point I had decided that I made the decision and I wasn’t going to second guess myself any more, not in my head, not out loud to him or anybody else. This was right after I had decided to go back and teach at Deerfield school before they found someone. It was really hard on me to finally agreeing to take on this other school to help them out and then to have them find someone after all. After
that mess I decided that I was definitely not going back no matter what and we pushed to get the
store open. I was mad at myself for agreeing to go back at all and so relieved when they found a
teacher that wanted to take over. I didn’t realize how much I didn’t want to do it until they told
me that Katie agreed to teach.

“Oh, yes, I had been talking to him all along about how I was feeling. I think he just
wanted to give me one more chance to back out before all the inventory came. When I first
started talking to him the winter of my second year back, he thought it was the weather, too. I
couldn’t come up with any other reason for my restlessness either. What in the world did I have
to be unhappy about? I was paid well. The class was no trouble. The parents seemed to like me.
I loved what I was doing. But the restlessness just wouldn’t go away. There were times when I
got so mad at myself! I had all that I ever wanted: good kids, a husband that lets me be me, and a
job that I felt good about. Why in the world would I want to upset all that? I’ve asked myself a
thousand times why I just couldn’t be content with what I have and I think that’s the part that
John doesn’t understand. I don’t understand either. The guilt is the worst part. It seems really
selfish to leave the kids and it seems really selfish for me to need to do something more,
something different. But I need to be challenged; it’s just the way I am. I don’t expect the
ministers out there or anyone else to really understand.

“No, I couldn’t say any of this to anyone at the time. That’s why I didn’t even talk to you
about it. The only thing I could say to John was that I don’t know if I want to keep teaching and I
don’t know why. I can’t do a much better job explaining now either. With the store going, it’s
easier to explain to people. Before I decided to open the store, Kaye helped me look at some
home-based stuff on the computer, but we didn’t find anything that looked like it was for real. It
all sounded way too good to be true. I know you’ll think this is funny and I never told you
because I knew you’d laugh, but I even looked into selling Amish crafts on the internet! Lara, you should do that, you’re the crafty one, not me! A teacher store suits me much better. I don’t have to make anything and I can still be involved with teaching some. John’s big thing is that he thinks I’ll go crazy with nothing to do all day. I’m still a little worried that he might be right. We’ll really know once Emma and Levi go back to school and I’m here alone all day.”

“I don’t know, it’s hard to tell if I will doubt myself again in September. I always loved the first day of school, the way the clean room smells and the floor shines, even the getting ready, stacking the books on their desks, the new pencils, the tablets; I loved using the same stuff in the classroom as when I was in school. The kids all come rumbling in the door in their new school clothes, so excited to start another year. Every September, it was like a fresh start even with the weather and I always loved that feeling. It’s hard to tell how I’ll feel when the time comes. Never in my life have I had time just to do what I want to do, so I’m looking forward to that part. Like I said, I made the decision and now I’m going to be satisfied with it and not doubt myself. Now with the store going so well, most days I know I made the right decision already. I do have to make sure I stay busy though. I want to keep myself on a schedule or I’ll go crazy. I want this to work at least for this year.”

I was so deep in thought that I started when I heard the buggy turn out of the driveway. As John came in the house, Lara rose to leave knowing that John and I would need to talk about the ministers’ visit, whatever it was about, before the Levi and Emma came in. She greeted John, I promised to call, and she was gone.

“I can’t believe this is happening again! I thought Rachel might turn the job down. I remember how it was to be sixteen and being asked to teach. Maybe she’ll change her mind? I
know she would do a good job with those kids. Can’t they talk to her again? Why do they have to keep asking me? Will there be problems for us if I say no?

“I know they need someone, but how would I do the store if I went back now? We’re much further along than when they wanted me at Deerfield; the basement is full now! We already went through this once and I really don’t want to do it again. They’ll find someone for Sunny View; I know they will.

“If they can’t find someone, I guess I’ll have to go back then. I won’t do it because they want me to; they can’t run my life. Can you just hold them off for a few more weeks until we see what Rachel decides? If the store isn’t meant to be, then it’s not. I just want to wait and see.”

I looked at John and could see how much strain this had placed on him. It’s been hard on both of us. I’m not sure he really believes that I’ll be happy being home even with the store. A lot of men would just tell their wives what to do; I’m glad that he’s not like that because I could never be happy with him if he were. He did agree that I won’t go back, but no one likes to be visited. It seems like agreeing to go back if they can’t find a teacher for Sunny View is what I need to do. If I have to do it, I have to do it. The store will have to close and I will have to wait to find my something more.
Some of her words are based on what I remembered hearing her say; some are what I remembered saying myself; I’m unsure altogether about others and some elements are intentionally altered. The key episode in the narrative, the visit from the minister, strays significantly from how my participant described it as actually happening. As I often did with my writing, I shared this piece with her. She responded quite enthusiastically, with more than a hint of amusement. I waited for her response wondering if she was smiling because she recognized herself as Sarah or if she enjoyed the fact that I thought my readers would find her daily life interesting or, maybe, if she was smiling at the sheer absurdity of my efforts. I asked her if I “got it right” and she confirmed that I “basically did,” but told me that the visit was actually from some school board members, not from the ministers. She said that a visit from the ministers would be an entirely different matter. While this was a misremembering on my part (surely one of countless), we both agreed that a visit from the ministers is much more dramatic and the story should not be changed. (This is, after all, a work of fiction.) An author of fiction, I gave the composite life outside of the data I created in support of this story, though in error, in the name of dramatic interest. Isn’t this what readers of ethnographies expect? Are researchers not compelled to make the familiar strange? Does fiction have a role in this process? Should it? While this decision to alter the text was (eventually) intentional, how might similar anomalies be at work in researchers’ creation of the other? Like authors of realist tales, my first intent was to “get it right” in terms of the basic facts of the narrative. There is a tension in the piece between my desire to “get it right” and my desire to engage fiction and experiment with the composite figure. I felt compelled to represent key elements of the narrative accurately, willing to experiment with, but not completely abandon the idea of representative text. My intent was to create a realistic fiction, not a fantasy. My participant, on the other hand, was quite unconcerned
about the mistake. She seemed to be more interested in the creation of a “good” story than an
accurate one and was quite willing to conspire to change the composite’s experience in the text.
Arguably the change was small one, likely having more meaning to my participant than for
anyone else. I’ve wondered since what kind of liberties I could take with data and still enjoy her
support and trust. I’ve told her that it is a work of fiction not a representation of her. What
differences does she see between research and fiction? Throughout the project as it is, she has
been largely unconcerned about how she is represented. The intentional liberties I took with the
text were small. Sarah’s sister, Lara, was not at my participant’s home when the visit occurred.
She exists and is close with my participant, but was inserted into the narrative simply as a vehicle
for hearing Sarah’s thoughts. Similarly, the description of their relationship was also
fictionalized. My participant never described any talk at all between her and her sister about her
decision to resign. I know nothing about the specifics of their relationship.

While I was eager to engage the data in fiction and remain interested in the questions
around the relationship between ethnography and fiction, my more pressing goal was the
experimentation with the composite character, Sarah. The genre was a vehicle for me to
experiment with the composite and I did so with intention. For example, Sara’s thoughts and
words about leaving her teaching position represent a combination of mine and my research
participant’s. Sarah’s thoughts about the first day of school are mine:

I always loved the first day of school, the way the clean room smells and
the floor shines, even the getting ready, stacking the books on their desks, the new
pencils, the tablets; I loved using the same stuff in the classroom as when I was in
school. The kids all come rumbling in the door in their new school clothes, so
excited to start another year. Every September, it was like a fresh start even with
the weather and I always loved that feeling.

Sarah’s talk about the importance of respect in the classroom is based on the talk of my
research participant as I recalled it later:
They didn’t respect Mary and she didn’t respect them so they were getting 50’s and 60’s and they were really unhappy. I told them every day that I knew they could do better. I really think they wanted to do better, too. Before I knew it, things were so good. When I think of how far they’ve come and how much happier they are... They’re not perfect, but really good. I couldn’t have asked for better kids. Most of the time, I could just look at them and they knew they needed to behave. There was such respect between us.

Sarah’s talk of her ambivalences about leaving the classroom and surrendering her teacher identity and her feelings of responsibility to her students cannot be assigned to either my participant or me, but represents ideas we both struggled with and talked about at length:

_The kids. It’s still the hardest part. I don’t know if I’ll ever be completely sure that I made the right decision. How could I simply wake up one morning and not be a teacher? And what reason do I have not to be? I’ve told myself that there must be something more out there for me to do. But what if I’m wrong? What if being there for the children is the “something more” that I’m looking for?_

**Failure (?)**

The ethnography is a product of two very differently positioned women who forged a uniquely open relationship by way of a poststructural research project. When I dutifully reproduce our conversations as best I can, I attempt to impose some degree of order on the data I produce by categorizing it as either note-making or note-taking (Frank, 1999). Here I engage this data in the production of fiction with a composite character. Is this ethnography? Without hard data in the form of transcripts, I recast the project itself as a fiction in an effort to consider all research participants as composite figures, fictions of sorts, regardless of methodology. My participant has read and approved of the story I tell in “The Visit.” My desire to have her concurrence indicates my unwillingness to leap untethered into the realm of fiction, leaving the social sciences behind. Can this project be, like Clifford argues, both fiction and ethnography? Like Sarah, can it refuse a binary distinction as either literature or science? I claim that it is
ethnography, but, too, it is fiction; it is constructed and tells a particular story (Visweswaran, 2003). Geertz argues that the resistance to acknowledging that ethnography, and I would argue research in general, involves story telling, picture making, and use of non-literal language is based on confusion, “…of the imagined with the imaginary, the fictional with the false, making things out without making things up- problematizing the idea if literalism is lost, so is fact” (Geertz, 1988 cited in Visweswaran, 1994, p. 2). Researchers of all stripes take great comfort in evidence, fact, and data. Research that does not claim to be based around these concepts is suspect at best. I move away from “deliberate” fiction elsewhere in my work, but claim that the knowledge produced will still be a composite, a story, a fiction, of my participant, the data and me instead of a representation of her otherness as an Amish woman. Unquestionably, this conceptualization stands in opposition to what has been defined as scientifically based research, but will you see it as science at all? Is my postmodern step away from the claim of representation of the other too great? Can a composite be a valued research project? To whom?

From a participant with whom it seems both effortless and impossible to connect, to failures in methodology that leave me without “hard data,” this project is “faced with its own impossibility” (Visweswaran, 2003, p. 98). It is an experimental text, not only in form, but in intent. There is attention not only to the constitution of subjectivities and power differentials between myself and S., but also a questioning of ethnography’s assumptions (Visweswaran, 2003). Pursuit of the other is problematic. The text is marked by ruptures, incomprehensions and discontentments (Visweswaran 1994, p. 20). Notions of what constitutes fieldwork, power and subject representation are explicitly troubled. S. and I theorize together from our complicated, troublesome and fluid positions as respective insiders and outsiders. Through my failures in data collection, I surrender any imagined authority as researcher. Total understanding and untroubled
representation eludes me. The task is both liberating and frightening; there is no methodology behind which to hide.

My process of coming to know will not be obscured; I will tell you when I know I have failed. Failed not only to collect data in a form that my reader will find acceptable, but also to interpret and analyze in a way that will make you feel satisfied. Both ethnography and fiction, Visweswaran argues, no matter its pretense to present that which is tidy and whole, is incomplete and detached from the realms to which it points (2003). All ethnography, she argues, is inevitably a “failure” in its ability to capture and represent and the recognition of the failure of my own project is not an abnegation of my responsibility as researcher, but rather forms the nucleus of the work.

My wish for the contribution of this work is that it prompts conversation around ways to talk across differences that do not reproduce social hierarchies and otherness, at the same time challenging objective representation in research. Margery Wolf, in *A Thrice-Told Tale* (1992) engages data in three different genres, one being fiction, writes: “The postmodernist goal is, I take it, to encourage the author to present a less tidy picture with more contradictory voices and to encourage the reader to take more responsibility for puzzling out what is really going on in Wiliwililand” (Wolf 1992, p. 53). In the fallout of the Education Sciences Reform Act of 2002 and No Child Left Behind, it is imperative to continue the incessant questioning of what has been defined for us in advance. Under positivist legislated notions of science, the determination of validity is based upon the unadulterated adherence to prescribed methodology (Polkinghorne, 1997). What is lost in the prescription? Assuming a more inclusive treatment of methodology, what role might narrative strategies play in how we come to know what may or may not be going on in Wiliwililand? Do approved methodologies that make claims to validity via various
processes of data capturing, provide a screen behind which we can hide? ("right here on line 63, it says...") Further, who benefits from the asking of these questions and who doesn’t? “Whether to celebrate or lament the felt loss of found worlds depends on how one reads the political possibilities that open up when “truth” is positioned as made by humans via very specific material practices” (Lather, 1993).

In 1997, Elliot Eisner wrote, “The desire to open up new ways of seeing and saying has been motivated by epistemological and political impulses. As a result of these impulses, we find ourselves on the edge of methodological inquiry. Edges can be treacherous, but they can also be exciting” (Eisner, 1997). This second vignette, an exploration of validity, representation, and fiction, may push the edges too close to treacherous for some, but raises productive questions about what we think we are getting from findings of experimental work and what we lose when we can’t ask questions that don’t fit the sanctioned methodology that we can use to investigate. Perhaps decisions based on valid, objective research should be more tentative. How could the findings be so certain and objective? Could this claim really hold true elsewhere? How does she convince me to believe? More dangerous still is the limiting of inquiry and exclusion of the Other from literacy research when research questions and methodology must conform to a specific vision of science. In the next vignette, I continue the exploration of my relationship to my data and participant this time considering transparency and the challenges it presents to certainty.
Chapter Five

Knowing what I know now, how do I proceed? I’ve worked theoretical territory excluded from the ESRA and offered examples of the kind of research lost under legislated versions of science. I’ve understood my inside/outsiderness in relationship to my participant as existing on a continuum, rejecting a definitive characterization of my positioning. I’ve considered validity as a construct and at work in this particular study and interrogated my representation of my participant (or lack thereof) in light of methodological and theoretical questions. Here, I use the metaphor of friction to consider self and other. I (re)consider the question *what does it mean to be rural?* as a personal exploration, an ethnographic inquiry, and a political statement. Michael Bell claimed that one’s “country” identity entails important social and psychological benefits (1992). Legitimate, but complicated. I suggest that rural educational research should include attention to the *ruralities* that children bring to rural schools, how “being rural” changes, and what this could mean for rural schooling. I explore my own discontinuous subjectivity as “rural” and the friction that has both compromised and contributed to this subject position. My efforts to explore these questions work at cross-purposes with the common sense assumptions about research inherent in the Education Sciences Reform Act of 2002, which delineated the qualities of educational research that can be considered “scientifically based” and therefore eligible for federal funding. I engage in a line of questioning firmly excluded under the kind of research considered to be “scientifically based,” positioning my work as a model of challenge to the legislation. Because it cannot conform to the methodological template, this study is not viable under the legislation. Yet how might consideration of the question *what does it mean to be rural?* be important to educational policy and rural residents themselves? I question the implications of the exclusion of this kind of thinking for those in rural schools. Both because of the original study questions and the methodological “restrictions” I encountered with my
Amish participant, this study and others like it are discarded by legislated understandings of what counts as research. How then can it be said that this representation of scientifically based research represents the view of “the American people” (Feurer, 2002, p. 4)? Which American people might be excluded?

**Rural Identity and Subjectivity**

We firmly attach ourselves to place and identity (Welty cited in St. Pierre, 2000, p. 259). I agree, as I chose to do a research project in my own “growing up” place. I am stubbornly attached to this place. So much so that I refuse to consider leaving, refuse to imagine what life might be like elsewhere. It is a fruitless seeking of identity, a humanist impulse, as if my identification with this place provides some sort of stability in how I understand myself. The problem with this notion is that the landscape, in part, has changed almost as fast as I have. Its identity as rural space is contested and changing like my own. Why then do I call myself rural if there is no way, for me or anyone else, to know exactly what this means? Perhaps if I think in terms of rural identity, there is no point in considering what it means to be rural, suburban, urban, a woman, or countless other tags we give ourselves. If it’s only a simple matter of what I’ve chosen to call myself, identity, the analysis is as short as the insight. I know that the process of how I understand myself is far more complex than a label that I choose at will to adopt or reject. Bronwyn Davies makes the distinction between identity and subjectivity. In humanist terms, any sane, adult individual has identity which is continuous, unified, rational, and coherent. While in poststructuralist terms, “the experience of being a person is captured in the notion of subjectivity” and subjectivity is constituted through discourse (Davies, 2000, p. 57). She continues:

The various discourses in which one participates, or in terms of which one gains a voice or becomes a speaking subject, also are the means by which one is spoken
into existence (even prior to one’s birth) as subject. These discourses subject each person to the limitations, the ideologies, the subject positions made available within them. We become, not what we have learned to call our true essential selves, but that which the various discourses in which we participate define as or make thinkable as a self, or a true self (Davies, 2000, p.77).

Identity is a just-the-way-I-am (or was brought up) self-understanding, a fixed label that represents a true self. Challenges to identity are uncomfortable because they challenge how one understands one’s core self. Subjectivity, in contrast, is an understanding of self based on the influence of language in determining how one sees oneself and how one is seen by others. There is no inner core self; who one currently “is” is based on social interactions. Thus, there is enormous potential for change and room for contradiction within oneself (Davies, 2000) because who one “is” is never fixed, but created through language. A self-understanding of rural subjectivity means understanding one’s self and one’s relationship to the world as complex and changeable. I suggest that the question *what does it mean to be rural?* is a question of subjectivity and is thus not answerable within a humanist conceptualization of the stable self (identity).

While I have been “born rural” and the ways in which I understand what that might mean are ceaselessly changing, the backdrop of my living space has stayed relatively constant. There are pockets of geography, ways of living and interactions with other residents that appear unchanged. Davies argues that it is through these discourses I can understand myself with a level of complexity beyond naming myself rural. I still can lay claim to a piece of rural nostalgia, however troubled that conceptualization may be. In the work of considering my relationship to my participant in light of our rural identities and subjectivities, I not only realized the improbability that rurality could be the basis of our connection, but also resisted defining, except in the most general terms, what it means to be rural. (I attest: *If anything, the more I engage the*
question, what does it mean to be rural? the messier my identity becomes. Yet I continue.) I suggest that the ideas of performance, discourse and connection are key in how I understand my own rurality, but abandon a move toward definition and simplified identity by concluding redundantly (and safely) that how I understand myself as different, as rural, is what makes me rural. And this is true. Because I have taken up the position rural, I see the world from the vantage point of that position (Davies, 2000), my subjectivity, whatever that may entail at any given moment. What I suggest is that the question what does it mean to be rural? is not definitively answerable outside of a humanist conceptualization of the stable self because it is a question of subjectivity. It’s a move beyond a that’s-just-the-way-I-am (or just-the-way-I-was-brought-up) self-understanding that opens the potential for change. It includes analysis of one’s ideologies, limitations, subject positions: the vantage point from which one sees the world (Davies, 2000). It is akin to asking, what does it mean to be a woman? Volumes have been written in response to this query, yet when one is interrogating how she might see the world, each take is potentially important and insightful. As such, because I have identified my rural subjectivity as a powerful discursive position in my life and name it as an important factor in this study, this vantage point should be explored and analyzed. I seek to understand it differently and more fully, but with the awareness that “the answer” is elusive and ambiguous and cannot be systematically excavated and examined.

**Friction and Subjectivity**

One way to consider my rural subjectivity is through the metaphor of friction. The metaphor of friction is a way to consider social interactions at work in my research; it is a force of connection and separation. I use the metaphor of friction in three ways: to consider my own rural subjectivity/what does it mean to be rural, to understand my research relationship with my.
participant, and, briefly, to consider social interactions within my participant’s Amish community. In the course of my work with the concept of rural subjectivity, consideration of my research relationship, and the workings of my participant’s community, I build upon early references to the metaphor of friction in rural research. While Don Dillman posited that technology offers great potential for “overcoming the friction of rural space” (1985, p. 3) and Hobbs (1992) alluded to a conceptualization of friction that exists between rural communities and their urban counterparts, I expand this metaphor well beyond its original use as a shorthand way to describe increased opportunity costs in rural areas. Through metaphor we understand one thing in terms of another, even if we are not aware of metaphor’s role in defining our realities (Lakoff, 2003). I use the metaphor of friction to understand my own rurality, the dynamics of my research relationship, and the functioning of my participant’s community.

I first experienced friction associated with my rurality during my early-teen summers spent in suburban Washington, D.C. with my father. Perhaps it was here that I realized that I claimed a rural identity. I was given free rein to move throughout the relatively rough neighborhood and beyond. I negotiated the D.C. Metro and bus systems unaccompanied. Later, I tackled the beltway and city streets by car. To travel alone was exciting, but scary. This was clearly not my “growing up” place and my extended visits there helped me to understand myself differently. I had no connection there to people or space. I’ve maintained that identity is coherent and continuous, but subjectivity is changeable and indefinable. At sixteen, over time, I could have become “urban” as surely as I identify as “rural” now. Not surprisingly, I didn’t reach this conclusion then, instead discovering that I was rural; that is I claimed a rural identity. Perhaps because most of my journeys were made alone without an adult, I decided that while I could manage urban living competently, it would never be the living place of my choice. At a young
age, I made the decision that the friction between how I grew up and the urban life style I
sampled was too great.

Back in Pennsylvania when I went to the local university as an undergraduate, I again
experienced friction associated with my location and identity as rural. My parents valued
education as capital for the job market but lacked the “college-parent discourse” required to
assist financially or otherwise. It was easy for me to imagine that all my peers shared an upper-
middle class suburban upbringing. I reasoned that because they left home, they surely couldn’t
be rural, conveniently not considering my high school peers who left home to attend college or
that the rural setting of the university itself may have appealed to my university peers. During
this time I thought much less about my rural identity than I had when I was making regular visits
to Washington, D.C. though the friction I experienced between my rural discourse and the
suburban discourse of my peers was likely just as great. Later there was friction from my
position as “Centre County” that worked to otherize me, however superficially, from my
graduate school peers. Derogatory remarks about this place and its permanent residents were not
uncommon. Most criticisms centered on the lack of “things to do” here and the conservative
values of residents. I can simultaneously claim membership in these two conflicting discourse
groups: rural and graduate student. Even though these characteristics don’t dominate my
understanding of what it means to live here, I did not speak up, having no desire to be a
cheerleader for a way of life that I find so complex and not wanting to silence critiques that don’t
offend me. I can understand criticisms of my growing up place, while simultaneously claiming
membership in the group being critiqued. It was during graduate school that I began to consider
the complexity of my rurality beyond a simple issue of identity and toward the intricacies of
subjectivity. Still, I knew that to be rural involved more than just connection to a place, but was
still thinking in terms of how to define what it means for me to be rural. Though I was having much difficulty addressing the ever-present question what does it mean to be rural? I did not doubt that eventually I would discover a clear and satisfying definition I could live with. Likely attributable to my increasingly complex understanding of what it means to be rural, the degree of friction I experienced in graduate school was not at all the same as the urban/rural friction I experienced in Washington, D.C. While I saw myself as other at times, the gulf between my university and rural subjectivity wasn’t nearly as wide as the distance between my temporary urban identity and my rural identity.

Both subject positions, rural and graduate student are complex. A member of a rural community, I am set apart from most other members because of my role at Penn State. There is friction. Beginning even before birth, I grew into my subject position of rural and its accompanying discontinuity between the culture of "the college" (as it is referred to locally) and my rural culture. I am the only member of my immediate family to attend college, one of a very few in my extended family. My rural subjectivity was a lens through which I viewed the world, impacted what stories I told myself and what possibilities I envisioned for myself. I grew into a space of friction between my community and the university. Both in my family and in the larger community, there is no sense of the university being “our university” beyond rooting for football on television but rarely in person. There is category maintenance of sorts, due in part to the geographic proximity between my growing up place and campus. “The college” is regarded as the best place to be employed (in housing & foods, in the office of the physical plant, or as a staff assistant), but clear lines are drawn between the intellectual life and the rural life. Upon hearing that I enrolled my daughter in a charter school very near to the university, my mother warned that I might "make her into a nerd." My grandmother once espoused the skill of a seamstress by
telling me that she “does the clothes of the professors’ wives.” Clearly they are not us; they are other, but where does that leave me? From dead deer hanging from a tree in the yard, to laundry hanging on the front porch, I reject my "white trash" rural subjectivity. There is shame and recognition of truth in the critiques from my university discourse group, yet I am fully able to perform "rural" when necessary. Perhaps too I am able to perform "not rural" when necessary. If my university peers had recognized me as rural, it seems unlikely that they would voice their critiques in front of me. So I am here, floating between contexts, not sharing the middle class suburban upbringing of many peers, but not identifying fully with my rural discourse either. My subjectivities intersect and are constituted through the conflicting discourses in which I am positioned (Davies, 2000). How could I be one or the other? It’s a sense of floating identity that only poststructuralism can speak to; I have membership in multiple discourse groups and when they intersect, there is friction. There is friction both between the two discourse groups that I consider here, rural and not rural- or perhaps less rural, but also within myself as I negotiate the constitution and interplay of my identities.

There is debate within feminist scholarship between identity and a Foucauldian social constructivism associated with a postmodern position (McLaren, 2002, p.117) that is useful to further consider the complexities between considering rurality as identity, definable and stable, or subjectivity, discursive and fluid. Feminists who critique the concept of identity politics argue that identity categories such as “woman” are essentialists constructions, subsuming differences among the group, implicitly normalizing and unifying the category woman as white, middle-class, and heterosexual (McLaren, 2002, p.117). Others believe that the category “woman” needs to be retained for political, emancipatory purposes, arguing that political demands cannot be made for the group without the coherent identity “woman” (McLaren, 2002, p.117). I’ve already
described my difficulties in comfortably defining myself as rural. I hold membership in multiple discourse groups some of which stand in direct conflict with my rural discourse that troubles the idea of coherent identity. I identify strongly with the social constructivism position on identity and the critiques of identity politics as exclusionary and essentialist; I’ve troubled the very act of defining rural, yet I still identify as rural. I describe characteristics about me through which I understand myself as rural and perhaps help others to identify me as rural. Though I understand the limitations of the category “rural,” I am unwilling to abandon the label. I’ve confessed that I’m attached to place and identity. What might be productive about this quandary? McLaren explains that Foucault, while suspicious of identity categories, does not reject them: “He thinks the most significant thing about identity is that it is historically, socially, and culturally produced. Examining the production of social identities can help to reveal the historical and political struggles waged in its production” (2002, p.118). This reframes the question what does it mean to be rural as a historical, political and social inquiry rather than an essentialist one.

It is unsettling to discover that there is no humanist, stable core self that I can label. While I continue to associate strongly with my rural identity and self-identify as rural, I do not have to define myself or accept others’ definition of me. Most importantly, I can (and will) adopt new discourses and change my conceptualization of my identity accordingly, in part by way of examination of social identities that Foucault advocates. I’ve considered the self in this opening narrative and will continue to a consideration of the other that illustrates the struggle to understand the production of rural. Along the way I will look more closely at the use of metaphor.
The Metaphor and (of) Friction

The metaphor of friction will be considered to various degrees within considerations of scientific research, insider-outsider researcher positioning, and disruptive research methodologies across this work. In using friction as a metaphor to read and write theory, I draw on Lakoff (2003) to consider the role of metaphor itself in educational research. The story of my research relationship is extended into this chapter to illustrate how friction impacted this study, how it connected and separated my participant and me.

Lakoff argues for the pervasiveness of metaphor in our everyday thinking: our perceptions, our actions and our relations with others. Through metaphor we understand one thing in terms of another. He argues that even though we are not aware of our metaphorically based conceptual system, it plays a central role in defining our realities (Lakoff, 2003). His work is based on the notion that language is the basis for everything we think, say, and do. For example, the act of defining a concept (such as scientifically based research, validity, or reading) is to impose a boundary, to territorialize and situate oneself in relation to and, Lakoff argues, to quantify (2003). There is no separation between the language we use to define a concept and our understanding of the concept. There is nothing “outside” of language, a decidedly poststructural position. As such: “If conceptual metaphors are real, then all literalist and objectivist views of meaning and knowledge are false. We can no longer pretend to build and account of concepts and knowledge on objective, literal foundations” (Lakoff, 2003, p. 273). The Education Sciences Reform Act is based on this very premise of objectivity and other positivist notions of science where consideration of metaphor has no place. The implicit conclusion is that objectivity trumps
the limitations and complexities of language. Yet I have described the pervasiveness of the metaphor of friction in my research.

Eric Johnson, drawing on Lakoff’s work, offers a Critical Discourse Analysis of the promulgation of Arizona’s proposition 203, anti-bilingual education legislation (2005). He claims that the metaphor PROPOSITION 203 AS WAR was used to imply violence and heroism in an effort to degrade bilingual education in public discourse (Johnson, 2005). He offers examples that include the description of bilingual education as coming under fire, schools as battlegrounds, bilingual educators as having little ammunition, and Spanish as being targeted (Johnson, 2005). He notes: “The methods used to promulgate Proposition 203 originate from a more profound desire to shape society through the control of language… These statements pervade the media and embed themselves in the public conscious” (Johnson, 2005, p.623). How a concept is described in language by those with power has direct consequences in the lives of the less powerful.

Feminists are acutely aware of this. Consider the language used to describe quantitative research in masculine terms as hard science, while qualitative research is described in feminine terms as soft science. Rational (male) scientific thought is purported to be at the core of experimental and quasi-experimental methodologies, while qualitative research methodologies as a whole are irrational (feminine) by default. Scientific inquiry is regarded and legislated as hard science based in rationality. Science is known, Flannery argues, through the metaphors that are used to describe it as processes of discovery, exploration, penetration, conquering, and hunting (2001). Take, for example, metaphors to describe the debate around bilingual education and literacy education as wars: the “war” between phonics and whole language and the “war” against
English in California and Arizona public schools. These powerful, male, interconnected metaphors all relate to ideas of domination and control and reify each other making this conceptualization of science seem inevitable and incontrovertible, (Flannery, 2001) forming the basis of ideological understanding of how science is understood and how and by whom it is employed. “The emphasis on analysis, reductionism, and an often sterile form of objectivity, distances the scientist from the object of study, making it more difficult for science to deal with large, complex phenomena” (Flannery, 2001, p.638). She offers a quilting metaphor as an alternative to understand science, where values, complexity, researcher collegiality, closeness between researcher and subject, emphasis on choice and creativity replace the traditional male model (Flannery, 2001).

I’ve used the metaphor of friction to help me to understand some social interactions at work within my own research and I’ve argued that the analysis of metaphor in educational research is a poststructural move, gendered at times, working from the premise that language does not simply represent ideas, but rather it constitutes thinking and understanding while rejecting the objective and literal. Will my metaphor- friction- be considered masculine? When I think of friction I think of motion, machinery, abrasion, resistance, irritation- and physics class. Are these masculine concepts? Why? What other metaphors might I have employed to describe the relationships I consider?

When two objects rub against each other, friction results in slowing of motion. I engage the metaphor of friction to consider the relationship between S.’s Amish church district and its English community, disagreement within her Amish community itself and the relationship between S. as participant and myself as researcher. Friction is a force of connection and

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1 Also noteworthy is language of “The Science Wars” between the social sciences and natural sciences and “The Reading Wars.”
separation. It is at once a literal slowing of ideas, motion or travel and commerce between my participant’s community and the world and also the tension and conflict that result from this contact. For example, the use of computers by Amish construction company owners is currently an issue of contention in S.’s church district. The relative lack of computer technology that Amish business owners posses causes a friction that limits the size of their business. The bishops see this as a positive friction and while they do not explicitly limit the size of one’s business, their ban on computers serves this function in addition to others. The introduction of computer technology also causes friction among church members as they negotiate the role of the computer in their lives. Some business owners are rumored to have computers and resentment grows when church leaders do not confront individuals, some being relatives of leaders, about their reported transgression. In cases where friction cannot be resolved, in spite of time, town hall style meetings about the issue and bringing in bishops from neighboring districts for “outside” opinion, the church will rupture and a new group will emerge as a psalding. This is a relatively rare event. Most issues, my participant tells me, are resolved when a majority of members refuse to accept the decision and church leaders eventually stop discussing the issue. The silence on the part of the leaders indicates the advent of a don’t ask, don’t tell policy regarding the controversial issue. If the group with the grievance is in the minority, they will either, conform, continue the activity in secret if possible, relocate and join another church, or less often, form a psalding if enough families are unhappy enough to leave. This description of friction has a negative connotation of strife, but friction also has an essential role to play in the survival of the community itself.

In his 1985 piece in Rural Sociology about the social implications of increased use of technology in rural communities, Don Dillman posits that technology offers great potential for
“overcoming the friction of rural space” (1985, p. 3). Similarly, Hobbs (1992) alludes to a conceptualization of “friction” that exists between rural communities and their urban counterparts. Kenneth Wilkinson, too, draws on Dillman’s phrase when he posits ruralness as: “a source of disadvantage in access to jobs, income, services, and many other goals in the future as it has been in the past notwithstanding periods of rural growth and notwithstanding developments in technology that could, in theory, overcome ‘the friction of rural space’” (see Dillman, 1985) (1991, p.101). Although I also use the friction metaphor, I offer an alternate view of the friction of rural space. Friction represents the dissimilarities between the Old Order Amish residents in my study and their larger community and world. It results in slowing of ideas, motion/travel, and commerce. While many rural communities seek to reduce the friction, particularly with technology, between themselves and their urban counterparts (see Edmondson, 2003, for example), Amish communities are based on the friction of rural space. The Amish community in this work is based on and enables (or marshals?) the friction of rural space. Without friction the community would be “contaminated” by extraneous factors such as technology faster than could be accommodated by the community. They would cease to maintain their separation from the modern world they reject. In this way friction is essential protection for the social reproduction of the community. Friction is also essential in that it propels the church to selectively adapt to its larger community. In order to maintain viability, the Old Order Amish have embraced change, albeit slowly and with much consideration. For example, particularly in response to escalating land and cost of living prices, it is not uncommon for women with grown children to work off the farm. (See Beth Graybill’s (2007) work on small business ownership by Lancaster county women) Cell phones are common in my participant’s church district. Amish men frequently work off the farm, most for English employers. Increasing numbers of Amish
families do not farm at all, but instead own small home sites where a single horse can be kept for travel by buggy. Household gardens are kept to supply home-canned produce, but the majority of food is purchased at local grocery stores instead of being raised on-site as was typical in years past. The Amish church district is in a constant state of change spurred by friction between their more traditional way of life and the communities in which they prosper. Community members decide collectively how fiction between the English world and the Amish church will affect their daily lives.

A second consideration of friction is a complex triad of tension between the research itself, the participant, and me. I felt friction related to my concerns about how my participant would be represented in the study, friction between how I wanted to be seen by her and how she saw me. I was uncomfortable with the role of authority figure, but at the same time realized that in the writing of this work, I claim authority. Specifically, because this work is based on dialog, not interviews; there is the inevitable speaking for the other that I work against but must face. There are times when S. sees my theories as “right” or “preferred” or will ask me to answer a question that I have posed to her about her; her deferral is troublesome and makes uncomfortably clear that our relationship is not as egalitarian as I might hope. It is clear that that we cannot escape our locations and all the implications therein, but deferral is not the norm of our dialogue. She will say, “yeah, but don’t you think…” or “that does make sense, but we wouldn’t see it that way…” We say to each other simply, “Is that right?” Recognition of the partially understood is an accountability to my participant; the partial knowledge that is produced by this work is not choice, but necessity (Visweswaran, 1994, p. 50). I seek reprieve from my role as authority in the partially understood, and perhaps am most comfortable here, but also must accept the responsibility of speaking for my participant.
Alcoff considers the complications of speaking across differences of race, culture, sexuality, and power:

For in both the practice of speaking for and the practice of speaking about others, I am engaging in the act of representing the other’s needs, goals, situation, and in fact, who they are based on my own situated interpretation. In poststructuralist terms, I am participating in the construction of their subject positions rather than simply discovering their true selves (Alcoff, 1995, p. 100-101).

This construction, Alcoff maintains, is unavoidable unless one accepts the myth of the autonomous self; in fact, there is no neutral place to stand because our words do not emanate independent of our discursive practices. Whether we are speaking for ourselves or speaking for others (Alcoff, 1995), we create realities. With this knowledge to guide my work with S., I make the dialog and our relationship the center of the work, trying diligently to hold in check my impulses to speak instead of listen. I strive to speak with and to rather than for (Alcoff, 1995), all the while knowing the inevitability of speaking for. I analyze my position and how it writes this work, remaining solely accountable for my words, considering their effects and knowing that I cannot hope to control the meaning readers make from them or the unanticipated consequences they might bring about (Alcoff, 1995). I am an unreliable narrator telling an unreliable truth narrated from my own “situated interpretation” (Alcoff, 1995).

There is friction, too, between myself as researcher and my participant manifested by my participant’s literal refusal of the subject in her unwillingness to consent to tape recording. This is not unprecedented in feminist research. Ghazal Read & Bartkowski in their study of Muslin women’s decision making about veiling explain their subjects’ refusal in pragmatic terms: “Because many of the women were forthright about their opposition to participating in a study based on tape-recorded interviews, the tenor, depth, and candor of these interviews would have been seriously inhibited if conversations were tape-recorded” (2000, p. 402) The authors
describe the problem as a technical one and do not consider their participants’ refusal beyond the
cultural mismatch (friction) between the researchers and the subjects. Visweswaran, however,
while still acknowledging her preference for data collection tools, attributes the refusal to an
assertion of agency by her participant. She recounts:

I sat in front of M’s desk, willing myself to accept her narrative on the terms she
had set. I tried not to think of my absent tape recorder, or even the lack of pencil
and paper to jot down notes. Wasn’t M herself all too aware that the tape recorder
wasn’t there to catch her words? Wasn’t that indeed part of her play? (2003, p.
65-66)

In my own research, I, too attribute my participant’s refusal to consent to tape recording to an
assertion of agency, a refusal to be an “official” subject of my research. I don’t see this, however,
as a power play or manipulation as Visweswaran seems to suggest. From my notes:

“S. called today. I asked her about the tape recording and she said no. She said
that she doesn’t mind “sharing” but that she “would be too careful and nervous”
about what she says.

Assuming that “better” methodology will make a better project (Visweswaran, 1994) as
discussed in the previous chapter, I attempted to collect data in an acceptable form. I asked her if
she would consider writing letters to me and she reluctantly agreed to try. I wrote to her but did
not receive a response. When we met again, she apologized and said that she tried several times
but just “ripped them up.” She said, “That’s just not me.” She would not be the subject. Though
she has given her full consent to participate in this work, has read my work and regularly asks
me how my papers and classes are going, she refuses to be its subject by way of “official”
research that involves written documentation in the form of transcripts or letters. This refusal can
be analyzed as the result of her agency in her attempts to both self-protect and avoid judgment.
Her critiques of her church, if disclosed to church leaders would cause significant problems for
her and her family. Not only is she a woman speaking out against the church, but she does so
with an outsider. If there is no transcript of her words or an even more damning tape with her
voice, there is no proof that the words were said. Does S. take comfort in this? Perhaps her
conscious is clear in that without a tape recorder our conversation is not official and therefore
there is no need to be “nervous” or “too careful” about what is said against her church. By
refusing the formalities of documentation even in light of multiple conversations about the why’s
and how’s of my research, she frames our work as something other than research and her role as
something other than its subject.

The following is from notes written after her husband, P., read a paper of mine:

“S. called saying that P. had read the paper. She said that he had some concerns
that S. could “get in trouble” if someone read this. She said it’s not “keeping him
up at night” but he was nervous about who would be reading this...I explain that I
am completely comfortable with how I’ve protected her, but that I want her not to
just trust me, but understand how it works. She says that she thinks she does
understand.”

We talked later about this conversation and I told her that I was expecting her to
say that P. said we couldn’t meet anymore. She said that he’s “not that kind of person,”
but that he is a “deep thinker.” She said he will say to her, ‘Did you think of this? Did
you think of that?’ I read her phone call, then, as an assertion of her agency in her effort
to self-protect, both from disclosure of her identity to her community and a self-
protection of our relationship from her husband. This last conclusion is problematic. Her
response was that ‘he’s not the kind of person,’ yet I surmise that she needs to protect our
relationship from him. Here is friction between us. Is this judgment based on hours of
conversation during which she shared pieces of their relationship that would lead me to
this conclusion? Or is it indicative of my inability to conceptualize this Amish marriage
relationship as anything other than male-dominated? I have to accept the friction and the
not knowing. Our locations, a university researcher involved in a feminist project and an Amish woman, prevent us from connecting and communicating in the same way we could if were shared more similar discursive positions. Was her response, ‘he’s not that kind of person,’ an expression of her discomfort with the friction between us? What kind of person is she referring to and why did she respond this way? This episode accentuates the friction that results when our English/Amish discourses meet; it is a constant presence and shows itself in ways unexpected and unwelcomed.

Her refusal to be the subject may also be read as assertion of her agency in her efforts to protect herself from judgment by me and my readers. I suspect she is uncomfortable with the idea of my transcribing a conversation or reading a written letter because she doesn’t see herself as being able to represent her thoughts well. At times, she will use phrases like, ‘That probably sounds dumb.’ or ‘this probably doesn’t make sense, but…’ She has referred to herself as ‘not a deep thinker’ in reference to her husband whom she identifies as, ‘a deep thinker.’ When she visited my fifth grade classroom, she said things like, ‘You are way ahead of us’ and ‘I couldn’t do that math.’ I was embarrassed by this assertion of the politics of knowledge. Again the friction recurs. We share the subject position of teacher, but that doesn’t mean the same thing in our classrooms as much as I want it to. Standing in my classroom, where I thought we should be the same, I hated how our differences made me feel. Exchanges like these demand that the power differential between us that I would prefer to deny, be interrogated.

Though I have used the term “agency” to understand S.’s actions, Davies is careful to make a distinction between the humanist, individualistic and rational understanding of agency and the poststructuralist conceptualization of the same term
She characterizes agency in poststructuralist terms as choice-making, but the choices reflect one’s subject constitution within discourse; they are forced-choices a “fundamentally illusory” agency (Davies, 2000, p.60). The only possible action is that which is presented through discourse to the subject. One discursive position that speaks through S. is that of “not smart.” Not smart in comparison to her husband, not smart in comparison to me as an ethnographer or a teacher and not smart as a research participant. I position her differently.

I have described the workings of friction as separating forces, as they so often are, between S. as research participant and myself as researcher, but what follows is an exploration of a friction that does more to connect us as rural women than it does to separate us as researcher and participant.

**Friction and Photography**

Though much work has been done on the representation of women in the popular media, I use feminist poststructuralist theory to analyze the representation of Amish women in photography to illustrate the workings of friction in these common images. Photography naturalizes cultural practices, making it a particularly powerful and insidious instrument of social conformity (Hirsh, 2002). Like works of fiction, the photograph is not the expression of already constituted experience (Weedon, 1997, p. 133). The photographs, especially professionally produced ones, are texts that transmit messages that we read to learn how to perform our subject positions more correctly. The repetitive nature of the published images, read within modernist epistemology as natural and self-evident, tells a coherent story of Amish women that both absorbs the listener and satisfies the teller (Kuhn, 1995). While through a poststructural lens, an image’s meaning cannot be transparent or taken for granted; it is a negotiation between the author and reader, a situated
text reflecting the relative power and discourses of each. Kress notes the mistaken, modernist, belief in the objectivity of the photograph, a belief in its ability to capture reality as it really is, independent of the social context in which it is produced and read (Kress, 2006, p. 163). While these photographs may be offered to viewers as self-evident depictions of their subjects, my participant and I read them as texts weighted with meaning neither self-evident nor fixed. Visual social semiotics stresses that an image is not a distinct creative activity, but is a social process (Harrison, 2003); meaning is a negotiation between the artist and consumer and reflects the discursive positioning of author and reader. Like language, images point to particular interpretations of experience and culturally specific interactions (Kress, 2006, p. 2). As the photographer describes and interprets, she normalizes her views via mass publication. The images she makes are both indicative of her own discourses and constructive or reifying of new ones through which the other is constituted for and by her readers.

Bill Coleman’s images of Amish women prompt questions about the power relationship between photographer and subject. The simplicity of the images belies the complexity of his relationship with his subjects. In the preface of his text, The Gift to be Simple: Life in Amish Country he tells his readers that he has struggled with the ethics of his work and in response has started a monetary fund to which he contributes financially to his subjects (Coleman, 2001). He explains how he at first failed to realize the depth of his subjects’ reluctance to be photographed, but eventually stopped photographing adults- except from a “goodly” distance- and instead focused on Amish children and landscapes (Coleman, 2001, p. 9). On his website, www.amishphoto.com, where one can purchase a large framed photograph $400, he reports that his camera is “tolerated- to a degree.” It seems, then, that his subjects are not willing participants in their subjectivity, but tolerate it. Do they participate for the financial contributions, in interest
of good will or friendship? How do the Amish benefit from this commodification and constitution of their subjectivity? With the use of telescopic lenses do they have means to refuse the subject like my research participant has? Are they automatized: exploited beyond their grasp, made into a spectacle whose appeal increases relative to their awkwardness and helplessness (Chow, 1992)? In Chow’s theory, the automaton is otherized both by the production of class and aesthetic or cognitive difference (1996, pg. 106). The aesthetic is maximized to the fullest extent in the images below. Being “other,” according to Chow, depends on visuality that separates subjects and objects into either intellectual or spectacle, an important source of oppression of women (Chow, 1996, p. 105). Why is the rural other appealing? Is the pleasure of the consumer voyeuristic or a simple yearning for the good old days that never were? Is there a satisfaction for the consumer in gaining visual access to a culture that is based around its separation from the very people who purchase the photographs? It’s noteworthy that both photographs of people analyzed here appear to have been taken from a distance. Anyone with a passing familiarity with Amish cultural beliefs, presumably the very audience for Bill Coleman’s work, knows that the Amish object to having their faces photographed; this adds an element of voyeurism to each piece. Are the photographs a rebuke of those who opt out, in important ways, of modern mainstream American life? Why is it acceptable to construct and market photographs of those who do not wish to be captured? Is our right to observe and purchase more important than another’s right to be separate?

Talking Back

Weedon (1997) re-asserts that every act of reading is a new production of meaning produced within discourse. Inspired by Lorie Novak’s on-line work\footnote{Lorie Novak’s work, multi-media on-line project, \textit{Collected Visions}, can be found at: \url{http://www.cvisions.cat.nyu.edu/}. Her work with family snap shots is based on the notion that the family snap shot is} with family snap shots, S.
and I deconstructed some Bill Coleman photographs in an effort to read the photographs for new productions of meaning as we questioned the representation (or misrepresentation) of both of us. We “talk back” to our interpellations by superimposing our words directly on the photographs. Though the work is presented as representative of all Amish people, the photographs largely are of an Amish group known as the Nebraska Amish. As a Lancaster Amish, S. articulates her disassociation with this undesirable group mainly on the basis of her understanding of their uncleanliness and conservative way of living. She talks back not necessarily because she objects to her interpellation as Nebraska Amish in these texts, but rather as an examination of her identity as Lancaster Amish.

What might be the consequences of this art? As an ethnographer, I am deeply troubled by the story Bill Coleman’s photographs tell of the spectacle (Chow, 1992) of “the Amish” as a unified, uncomplicated, static group, who live a life completely and wholly in opposition to “our” modern way of life. The story he chooses to tell is that of the ultimate rural other. Reading the pictures from my position outside the Amish faith, but from my subject position of rural woman, I feel simultaneously offended by and celebrated in the images. There is a sort of pride of place for me in that I feel a curious satisfaction that someone is celebrating rather than mocking my identification as “rural.” I am pleased to live in a state that does not necessarily celebrate, but at least generally accommodates for the time being, a group of people that choose to live differently in some important respects from the mainstream. I look at the pictures and see the scenic beauty that for me is rural- or at least my personal myth of rural. I see gendered subjectivity that I both can and cannot relate to. As an effect of the ideological work of the photographs, I choose not to read the reality of rural poverty. In his images, the story of what powerfully constitutive of identity. Like any image, the meaning of the family snap shot is not self-evident, fixed nor transparent.
might be read as poverty is told poetically, charmingly, revealed only by looking closer, details of dilapidated school buildings, homes and dirty children.

_**Laundry Picture:**_ ³

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*The Gift to be Simple* (Coleman, 2001, p. 81)

_S_: They spend time, a lot of time on other things like chopping wood. They don’t have the conveniences. Women work in the sawmill. We don’t work hard like they work hard.  

_Karen_: The boy wants to be involved in the lesson. The mother has pulled the bench over so her daughter can help. I love the smell of wash that has been hung out. Gram won’t hang out ratty laundry. When I hung out the kids’ clothes, they smelled so good. I felt like I was taking extra special care of them.

This photograph sells a story of “natural domesticity” (Hirsh, 2002, p. 248) and the stereotypical rural woman that his customers find ultimately desirable; the satisfying and

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³ The green writing on the photographs represents snippets of S’s comments and the black writing represents my own.
lucrative story of the picturesque rural farm: freshly laundered clothes swinging in the breeze, barefooted children playing in the yard, and simple, passive, yet hardworking mothers laboring contentedly in service to their family. Coleman invites the viewer into the yard in this piece. We are so close we hear the snap of the sheet and the whining of the toddler; we smell the scent of her detergent.

Our different readings of this picture are evident on this slide. S. reads against the picture, otherizing herself from the Nebraska Amish. I read myself into the picture, relating to its subjects. I was struck by how I read the activity in the picture, hanging out laundry, as an act of care giving, both from when I was a child and from my own mothering experiences. I also noticed the appearance of the towels and judge them as “ratty” which compliments my participant’s conceptualization of the Nebraska Amish as dirty. She did not comment on the specifics of the photograph at all, referring instead to the task in general as work. Again she defines herself though a rejection of the other, an other that she sees as living quite differently from herself. As busy as I am reading the picture for connections and minimizing the friction between the subjects and me, S. is equally intent on reinforcing the differences between her and the Nebraska Amish. Though we both share the experience of hanging laundry, our very different positions (I can choose to save time and use the dryer) make for dissimilar readings of the photograph.

The next photograph, “Clusters,” shows Lancaster, not Nebraska, Amish women. S. considers her agency related to decisions about her dress and its eternal implications. The picture has a distinctly voyeuristic tone; the women are physically close, clustered, and the camera is clearly at a distance capturing them in a private space. The audience accesses this private moment among young women.
Amish Odyssey (Coleman, 1988, p. 24)

S.: Some live more conservative than others. Plain versus fancy Amish. It’s in God’s eye. How can you judge? People would look at me as not conservative. People think you need to listen to other people to get to heaven.
Karen: Dress- conservative, Lancaster group.
What are they doing? Watching something. What does this mean? (the title, Clusters) Kinship. Unity.

Even though this picture shows Lancaster Amish women, it is interesting that S. once again reads herself out of the picture, deeming these women more conservative than she. Again I read with the ideology of the picture, seeing kinship and unity in what it means to be an Amish woman. I read kinship in the close proximity and relative isolation in which Amish women live; unity in appearance and belief, even knowing that neither is likely true.
Hebdige writes, “Style in subculture is, then, pregnant with significance. Its transformations go ‘against nature’, interrupting the process of ‘normalization’.” (Hebdige, 1996, p. 18). Pregnant, indeed. S. and I have had hours of conversation around the meaning that can be made from reading clothing. She identifies Amish clothing as requisite to being Amish; it works to constitute women’s subject position as an Amish woman. Negotiating its details causes much friction in the church; resistance to established church norms is ever-present. She identifies ruffles on sleeves and heart shaped caps as being “too wild,” though she herself likes both deviations. Aprons, caps, color and sleeves are texts S. reads to interpret a church’s location on the liberal/conservative continuum. Her style as read by Amish inside and outside her church, symbolizes her positionality based on the ongoing historical evolution of her church’s standards of dress. Not only can S. be read herself and likewise make meaning from bodily performances of other Amish, but her appearance is read by the English with whom she interacts as well. Amish style enables friction between the church and its English community. It communicates clearly that she is Other.

Jen’nan Ghazal and Bartkowski discuss cultural forms in a Muslim community, such as gender, religion and ethnicity, as social phenomena that are constructed, contested and intersecting (2000). Like the Muslim veil, the meanings attributed to women’s Amish dress are not inherent in the clothes themselves; they have no meaning outside of discourse. S. describes how Amish dress is contested and intersecting. She tells me how women push boundaries and communicate within their communities by their decision making around fabric choice and sleeve style. She states: “People would not look at me as conservative.” Her discourses conflict, but perhaps are reinforced when she encounters discourses outside of her community. Her body is read differently outside of her community. She believes that outside, her Amish clothes are not
what protect her from the male gaze, as argued by some Muslim women who veil (Del Collins, 2003). S. believes that she is not subject of the male gaze because non-Amish men see Amish women as dirty and therefore not desirable. In the next slide she reads the image as “dirty” as she continues the work of othering herself from the Nebraska Amish.

Here we interrogate the photographer’s intent and S. offers her interpretation of his motives.

*House Picture*

*Amish Odyssey* (Coleman, 1988, p. 101)

*Karen:*
Why does he tell the story this way?

*S:*
So it’s not confusing. People pick up the books to learn about the Amish. It wouldn’t be a satisfying thing. You would wonder what the difference is between the Amish groups. Dirty. Not clean.
Her view that the photographer keeps the story simple for his audience is in accord with poststructuralist theory. An essentialist, humanistic way of understanding ourselves in relation to the world (Weedon, 1997) is a more satisfying narrative than the complexities suggested by feminist poststructural theory. The humanist narrative has certainly been a marketable story for Bill Coleman. How would his work be different if he told a story of extremely diverse church districts who struggle with many of the same individual, family, church and community issues that their English neighbors do? How would his work be different if it celebrated (or critiqued) the constantly evolving relationship between “the Amish” and their larger communities? How would his work be different if he told a story of Amish churches and their struggles and negations with gender and modernity? What might these images look like? Poststructuralist theory asserts that in this artist’s telling of “a particular kind of story” (Kuhn, 1995, p. 20) he reveals much about his own perception of his subjects’ and his customers’ desires. Like a successful mass media advertising executive, he has successfully coupled his customers’ desire for a particular kind of rural story and their willingness to consume.

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Together S. and I engaged with the texts of this work critically. We read, questioned and talked back. Along the way, I attempted to make sense of difficult methodological questions. All of this is important, but there is more: I made discoveries about discourse and the power of the images’ ideology that I was not looking for. As discussed, much of the meaning S. made of the images centered on her perception of the Nebraska Amish as being not just different from herself, an other, but dirty. Her response to the picture of the homes: ‘You would wonder what the difference is between the Amish groups. Dirty. Not clean.’ My representation of her thinking about the male gaze: ‘S. believes that she is not subject of the male gaze because non-Amish men
see Amish women as dirty and therefore not desirable.’ She designated cleanliness as an important difference, a source of friction perhaps, between the Lancaster and Nebraska Amish and differentiated between how she is perceived by English men (dirty) and how she perceives herself. She personally rejected this subjectivity, but spoke through a discourse of uncleanliness in regards to the Nebraska Amish in the photographs. I was quick to recognize her othering strategy. Then I discovered that I wrote the following text:

As an effect of the ideological work of the photographs, I choose not to read the reality of rural poverty. In his images, the story of what might be read as poverty is told poetically, charmingly, revealed only by looking closer, details of dilapidated school buildings, homes and dirty children.

And:

Gram won’t hang out ratty laundry.

My own words shocked me. Immediately, I flashed back to a childhood memory in which my grandmother would lament that surely “they” (the dirty poor) could afford a fifty cent bar of Ivory soap and a washcloth: cruel, but ironic because we were poor, too. We were poor, but we as my grandmother’s words taught me, were not “them;” we were not the dirty poor. My face was clean and the dishtowels on the clothesline were presentable. So it is with S.: she is Amish, but she is not "them" (Nebraska Amish). And it was with me: we were poor, but we weren’t “them” (the dirty poor). Here we are drawn together by friction between us and our less desirable neighbors. In my analysis, I even noted that, ‘I also noticed the appearance of the towels and judge them as “ratty” which compliments S.s’ conceptualization of the Nebraska Amish as dirty.’ S. and I shared this discourse and mine spoke unexpectedly.

As an effect of the ideological work of the photographs, I choose not to read the reality of rural poverty. In his images, the story of what might be read as poverty is told poetically, charmingly, revealed only by looking closer, details of dilapidated school buildings, homes and dirty children.
In fact, the ideological work of the photograph was overwhelmingly effective. My “common sense” understanding of the rural poor was not apparent to me even within the act of analyzing. I claim that the photographs tell the story of rural poverty only in their details, but it seems S. and I both read the message correctly anyway: other poor rural children are dirty. Neither S. nor I are “them.” And these subjects are the ultimate rural other. Could anyone be more rural than the Nebraska Amish? While we analyzed only two photographs that show the Amish subjects as dirty, this depiction is not uncommon in Bill Coleman’s work (see 1998, 2001). His subjects frequently present dirty clothes, dirty faces and bare feet to the reader. What ideologies speak through these texts and to what effect on the lives of the rural poor? How have we learned to read the texts this way? How do the photographs work to keep us separate and why is this ideology appealing to consumers?

Again, I fail. This time I lay bare my bias, only to hope that you will forgive me as I work to re-constitute my identity yet again. In the process of the thinking, reading and writing of this work, I use poststructural theory personally to move between discourses, to subdue some, to use one to counteract, modify, refuse or go beyond the other, to understand how they subject me, while knowing that access to a new discourse does not undo the previous one (Davies, 2000). I recognized the limitations within my particular rural discourse. After self-examination of my socially and historically constructed discourse about what it means to be rural, I changed my rural discourse, not becoming more removed from my rurality, but re-imagining it. I used one discourse, researcher, to critique another for the better: “Much feminist writing has precisely that quality of “bursting open” the holding power of old storylines” (Davies, 2000, pg. 79). I discovered friction at every turn. I did not expect to find friction within my own conceptualization of what it means to be rural. Does being rural mean being poor? I did not
expect to discover my own “othering” tendencies. Within my new understanding of identity/subjectivity as unsettled versus fixed, there is hope for movement. If I had only considered myself “rural” without considering how I learned that this is connected in some way to being dirty, my identity would remain unchanged. Friction is a useful metaphor to think about my changing rural identity; there are degrees of friction that can result from a variety of situations, both expected and unexpected. Stretching the metaphor a bit, I also can consider the abrasion of the two objects in opposition. Not only is friction produced, but the objects themselves change over time, either suddenly in response to friction of a great degree or extended, less dramatic friction. What is certain is that change can occur.

**Implications**

In this last story of my research, I describe both myself and my participant for my readers, offering yet another (re)construction of our identities. I chronicle what it means to be cast as other by forces I describe as friction, I cite the role of friction in a variety of its manifestations, and I explore how friction separates me from others less rural. It is at work in how I see my participant as other; it is evident in her representation as the rural other in art and her understanding of others she sees as more rural than she. Only by considering rurality as a subjectivity could I account for the complexities I encountered within myself, my participant, and our relationship. The concept of a rural identity did not adequately account for the surprises, contradictions, and challenges I encountered in my research as I considered what it might mean to be rural. I found no simplified definition that was adequate; only through subjectivity could I understand what it means to be rural. Contradictions, discourses and connections are all key in how I addressed the question *what does it mean to be rural?* and contrast to a simplified notion of rural identity.
Within scientifically based research as legislated in the Elementary Sciences Reform Act there is room for neither the complexities of identity or research about its effects in the classroom. This work is not cast as legitimate (scientifically based) under the Elementary Sciences Reform Act. Therefore, knowledge developed from this study and others like it is not deemed a relevant contribution to knowledge about educational practices and theories. In the face of legislated notions of objectivity, evidence, and transparency, I consider my own experiences and discourse as data. I join my participant as a subject of the research. I accommodate a participant whose conditions for participation would preclude the possibility of her contribution to the literacy research base. Finally, my use of metaphor as a way to consider data in my study causes insurmountable friction between this study and acceptable ways of knowing under the framing of research in ESRA.

The conditions I describe enabled me to see complexities that changed my questions about rural schools. I began this study hoping to describe rural literacies at work in my participant’s community, a specific point of inquiry largely removed from my own involvement. Due to a litany of intricacies that I was both unable and unwilling to circumvent, my questions were soon replaced with broader concerns. Now I question ruralities at work in rural schools: how they impact schooling, how they change, and what this might mean for rural classrooms.

I argue that the question what does it mean to be rural? is a question of subjectivity. It is a move beyond a that’s-just-the-way-I-am (or just-the-way-I-was-brought-up) self-understanding that opens the potential for change. This understanding is important because the only research that “counts” under ESRA is that which aims to measure children and translates into findings that capture them in representations of their true selves, often in the form of a test score. Yet, in the course of my research, I found the concept of a true (rural) self to be a highly
unstable subjectivity. This prompts me to suggest that the questions of rural educational research should include attention to the ruralities that children bring to rural schools, how “being rural” changes, and what this could mean for rural schooling. These questions are worthy of at least as much research attention as how do we raise test scores? and highlights an example of a line of research are excluded by current legislated definitions of scientifically based research. The boundary breaking in this study demonstrates what might be gained from working in opposition to normalizing legislation while re-opening the question: what does it mean to be rural?
Chapter Six

Completing this study was a process of challenging a variety of certainties. Some, such as objectivity, I intended to challenge from the onset. Most I did not identify in advance. What was most surprising was that I engaged in much of the same thinking that I critiqued in the study. For example, my original intentions were to capture data with a note pad or tape recorder. This would have lead to a final product that looked like I expected an ethnography should and evoke validity as I understood it. Headed toward construction of a realist tale, the impact of my own rural identity was not a consideration in my original conceptualization of this study as an exploration of Amish literacies. Yet this became a key consideration in my analysis of scientifically based research. Initially I was not settled on a definition of rural, but had no doubt that eventually I would come up with a definition that could capture the rural identity across contexts. Bias, too, was revealed as I was forced to confront my common-sense understanding of the rural other as poor. Earlier in the study, in my efforts to establish rapport, I worked from assumptions about my participant that proved to be inaccurate, but eventually had to let go of my desire to manipulate my outsider status.

The certainties on which ESRA science is based eluded me. The near-constant process of understanding and re-understanding the certainties dominated the study. I am the most assertive in offering the study as an example of work that opposes correct science and consideration of the implications of correct science. I challenge its assumptions and the system through which they were legislated. I consider the implications of these critiques for teachers and teacher educators. Though I appreciate poststructuralism’s take on uncertainty, I find it difficult still to be comfortable working outside the boundaries of certainty, all the while knowing that all learning involves conflict. I try to communicate to my pre-service teacher students the necessity and
productivity of uncertainty. I want them to feel uncertain about what they think they know about who their students are and what they have learned from research and from living with them in the classroom. I want them, too, to challenge what they know about themselves as teachers and how they relate to their students, while at the same time being deeply suspicious about the policies that impact their classrooms.

I began this work suspicious of the notion of rural literacy as a generalizable concept. Because I considered myself to be rural, I began my investigation with a group I identified as being most clearly rural, the Amish. I knew rural literacy was a fluid concept; what it means to be rural could not mean the same thing for all who could be considered rural. Thus I began by identifying particular texts read within the Amish community of my participant that I was sure had the most relevance in the constitution and maintenance of this community. I was interested in the role of these texts and the unique literacy of this community. Although I understood that what it means to be rural was as conditional as what it means to be literate, I expected to be able to name and describe literacy in the context I had identified. I started with a consideration of what I had identified in advance as the two texts, texts in the rather conventional sense, most prevalent in the community. My assumptions about the role of these key texts, the Bible and the Ordnung, were challenged immediately by my participants. According to an early participant, neither the Bible nor the Ordnung function as the sort of societal glue that I imagined. Instead, it is the farm that occupies this role. This redirection set me on a second, more complex path: thinking about the farm and the literacies at work around it as key texts. This line of inquiry is still of interest, but was brief in the trajectory of this study. It was soon trumped by bigger

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1 The Ordnung is an oral text. It is the evolving, community-created law that governs each Amish community. Each church district (or community) has an Ordnung that is distinctly its own. The Ordnung governs all aspects of the individual’s life. It is spoken once yearly at a special church service where members reaffirm their commitment to their Amish church.
questions about research itself when my sole participant transformed the focus of the study in its entirety. If not for her intervention, a realist tale based on standard ethnographic methodologies, assumptions, and evidence would have resulted. At the center of the realist tale would have been binders full of transcribed interviews, data I would have collected (not created) in order to support my findings about Amish literacies. I knew this is what it meant to do ethnography: wondering, collection, recounting. As it was, the “collecting” and “recounting” pieces of the process were nothing of the sort. Cut off from access to the ethnographer’s data collection toolkit, video, notepad, and/or tape recorder, I could offer no ethnographic data in the traditional sense on which to base the report of my findings. But I still had wonderings. Now the study was spurred by incredulity of how violating the terms of what could count as data could potentially shut down an entire line of inquiry and how qualitative research could ever be molded into something docile and tidy. I refused to accept the policing of my thoughts and couldn’t accept that my participant’s religious beliefs about the use of technology and personal concerns with documentation could exclude her contribution to what “we” might learn from and with her, deeming her scientifically irrelevant. I questioned the legislation of thought that resulted in a policing of what kind of research can be conducted, which lines of inquiry are valid, with whom and how they can be pursued, and how evidence can be explained to readers. This was and is a more pressing line of questioning than any questions I may have about Amish literacy. Poststructural theory helped me to frame my inquiries into the limitations of research in such a way that the study still included my Amish participant. My work with her became the basis of three challenges to the version of research that posed the initial road block to the study. These challenges represent issues that presented themselves in light of poststructural understandings about self, other, and representation rather than pre-identified areas of critique. Consistent
emphasis on the self enables me to disrupt my own truth (Lather, 2007) “in front of” my reader. I send out these messages knowing that they may not be “caught,” but the sending of possibilities itself is a condition of different thinking about representation (Lather, 1996).

The first complication that evoked inquiry was what it meant to be rural. Across the core chapters of this study, I detail my struggles in working to understand my own rural self-identity and that of my participant as both rural and Amish. At the onset I didn’t recognize identity as an important basis of critique of the research that I was questioning, nor its relationship to claims of objectivity. My rural identity assumed no role in my original questions about rural literacies. I couldn’t have articulated how what I thought I knew about myself and my participant would impact the study. Neither did I understand the task of finding out (capturing?) what it was that I thought I knew about what it meant to be rural. Poststructural theory helped me to reconsider this pursuit altogether and led to some new, but uncomfortable understandings. The complexities can be more productively understood in terms of subjectivity than identity. Identity is a humanistic conceptualization that can be used to normalize and exclude. Some may consider “rural” as an identity, or perhaps, “struggling reader,” “highly qualified,” “English Language Learner,” or “poor.” Subjectivity, on the other hand, is a poststructural construct based on the notion that “what we are” is never a final and fixed matter. Subjectivities are constituted through complimentary and competing discourses. Although one discourse is not discarded for a new one, new discourses come to dominate old ones. This way of understanding people is of the utmost importance in education because the potential of growth is expected and preexisting, essentialist ideas about what students “are” (or will be) can never persist.

Early in the research it became apparent from interactions with my participant that our connection via our shared rural identity, however I understood it at the time, could not be a
given. Both of us identify as rural, though our use of this label is not necessarily self-evident, but this was where assumption would end. My early ideas about how we would connect and how I could facilitate the connections were mistaken. I made many fewer assumptions about what it means to be Amish than I did about what it means to be rural.

As a result of my own considerations of self and other, I can now consider the importance of how researchers understand their own positioning relative to their participants in social science research. What they think they are seeing or hearing objectively is filtered through discourses that may compete with or compliment those of their participants. These competing (outside) and complimenting (inside) discourses cannot be regarded as stable over the course of the study bringing added complexity to the relationship between researcher and researched, making claims to objectivity illusory. Yet this notion of objectivity is alive and well even in ethnographic educational research. A particularly lucid example follows from Naturally Small: Teaching and Learning in the Last One Teacher Schools by Stephen Swidler: “And it was to ensure that I could as accurately as possible capture their words so as to reconstitute them here as authentic indicators of their experience and understanding of life at the schools as the activities were taking place” (Swidler, 2000, p. 12). This excerpt raises a wide variety of concerns, not the least of which is this author’s unwavering faith in his ability to objectively capture and represent his participants. While he is invested heavily in the objectivity of his data collection, inside/outsideness is also manifested in the types of questions researchers pose, the identification of participants (“struggling” or “at-risk” readers, for example) the determination of what is deemed supporting or contradictory evidence, and what characterizes a successful intervention (i.e. fewer children incarcerated or assigned to special education (Bloch, 2004)). We should be cautious of research in which the authors are not forthcoming about how they see themselves in
relation to their participants. No methodology can ever guarantee objectivity and claims to such should be carefully considered.

I could not be concerned with the impact of positionality on objectivity without also considering issues of evidence and validity within my study. Questions of validity asserted themselves loudly in relation to the methodological situation of the study. Without transcripted evidence, my study could not be considered valid under dominant understandings of validity. Understanding that language is neither transparent nor fixed, I turned to poststructural theory for thoughts from the margins about validity itself. If self, other, and objectivity cannot be taken for granted, neither can the evidence that anchors the study, thus troubling the concept of validity itself. I recognized that the concept of validity is as complex as the concept of rural literacy that spurred the study. Neither should be ignored; both should be understood within theory. Most importantly, neither should be defined in such a way that generalizes or disciplines. Under normative understandings, this study does not meet the standards of validity. Innovation and boundary breaking, so said Freeman et al. recently in *Educational Researcher*, is an emphasis of all sciences along with standardization and systemization (2007). The boundary breaking in this study demonstrates what can be gained from working in opposition to normalized understandings of validity while critiquing the standard itself.

Specifically, one must stand outside positivist understandings of validity to question representation of the other in research. Even within the boundaries of traditional ethnography, research already perched precariously on the edge of legislated standards, the representation of the other is untroubled. The assumption is that the other can be represented explicitly by way of correct and systematic analysis of the evidence collected. The objective nature of the data that is the basis of her representation is understood implicitly. In this study, I demonstrate the difficulty
that discourses present to this premise. Instead of making efforts to overcome the workings of competing and complimentary discourses between us, I represent them with the composite figure. This also enabled thinking about the role of fiction in the creation of believable accounts. I asked the reader to accept evidence that I offered as fiction in an effort to prompt thinking about the line between ethnography and fiction. I was explicit about how the difficulties I had in separating self from other led to a minor, unintentional error that I left uncorrected for the sake of dramatic appeal. I suggested that even with the aid of the traditional tools of validity (video, tape recorder, notepad) this move may not be completely atypical. Whether intentional or not, presented as fiction or findings, the point is that no representation can capture the other; she is created and read through discourse. While my methodological situation prompted unusual experimentation with data, I argue that researchers who employ more traditional data collection methods engage in processes similar to those in this study. They, too, create data. Whether data is transcripts, videos, or notes, decisions must be made about what counts as evidence to represent participants, if what way, with what language, and for what reasons. Further, the story of the research must be presented to readers in believable and readable ways, even within accepted textual frames. These processes can never be objective.

Within legislated positivist understandings of validity, objective evidence is what one gets from measuring. According to the SRE report, when one measures in accord with approved research methodologies, makes correct sense of the numbers collected, and explains the methodology, evidence, and conclusions in a way that someone else could repeat, one’s study is deemed valid. Essentially, the study measured what it claimed to measure. To meet the requirements of validity, one must measure. Thus, research questions that cannot be answered with measurements cannot be the basis of valid research within scientifically based research.
Numerical evidence is the only evidence that will do; it can be explained in a coherent chain of reasoning from framing of the research question in quantifiable terms to the reporting of findings. This science is transparent, self-evident, and ready for packaging by politicians, ready to “daze the public and silence educators” (Shannon, 2007, p.148).

The importance of determining the validity of a study should never be dismissed, but one should also recognize its normative, limiting potential. Asking questions that can only be investigated within positivist understandings of validity is to let approved methodology control inquiry. Instead of a one-size-fits-all conceptualization of validity, the validity of a study should be determined in accord with the theories and methodologies used in the study. The theoretical basis of a study and the researchers’ account of practice are two attributes of valid research (Freeman, 2007) that challenge the privileged role of methodology in determination of validity. This counter-conceptualization of validity makes room for questions about evidence. What counts as data, what it means to “collect” and “capture” data, and the relationship between the data and the representation of study participants are all considerations that challenge normative, positivist understandings of validity under which these questions have no relevancy. Within a reframing of validity, the transparency of representation must also be considered: better (more valid) methodology does not equal better evidence or truth in representation. Researchers, not unlike authors of fiction, use data to actively construct participants and their worlds for readers; truths are always partial and situated. The composite figure in ethnography is a rejection of the silent narrator and untroubled claims to truth, an interaction between researcher, researched, and data. It is a rejection of extractive research. The composite asks one to consider how complimenting and competing discourses affect representation of the other, reflections only possible outside the boundaries of positivist validity.
How the other is represented is complex. Considerations of self and other shake the methodological and theoretical foundation of evidence, objectivity, and validity to the core. These complications thus trouble the result of inquiry, representation. I’ve offered the insider/outsider continuum and the composite figure, both based on the troubling of self and other, as alternatives to transparent representation. The last undertaking is an exploration of representation of the other via the metaphor of friction. The friction metaphor is a theoretical move in opposition to transparency of representation that compliments the complexities of representing the other. Lakoff (2003) writes in great detail about how metaphors structure our understanding of reality. The recognition of metaphor precludes the possibility of objective and literal representations. The metaphor points to the fact that language is never transparent. Objectivity can never trump the limitations of language.

As I’ve demonstrated in this study, the metaphor of friction can be used to understand a variety of social interactions: friction between participant and researcher, friction within communities, and friction between communities. It is a flexible metaphor; its presence is not necessarily something that needs to be overcome. Amish communities, for example, are based on the friction between themselves and their host communities. Friction is both an important force of separation and enables considered change. The friction between my participant and me was productive in that it represents the effects of our conflicting discourses. At the onset of the study, I was intent on minimizing our differences but soon found this to be misguided. It was not until I accepted the friction that I could think about the impact of its presence. Friction must also be implicated in the “othering” of those who are different; I was confronted with my own bias against members of my own rural community within our work with the photographs of Bill Coleman. Awareness of friction in its various manifestations is yet another way for researchers
to consider the complexities of representing the other. Researcher/participant friction resulting from conflicting discourses cannot be overcome through “rigorous” methodology and should not be ignored. To write with this awareness is to evoke validity through disclosing one’s account of practice and careful consideration of claims made from evidence.

There has been much critique of the meanings of methodological exclusions within scientifically based research and considerations of how we “got here” (see chapter one and two), but no explorations in the specificities of what might be lost within its ideologies2 or what its tenets might mean for rural schools and researchers. I offer an example of research that is excluded under ESRA. Whatever might be learned here and from similar studies is excluded from the “vision for the future of education research” (SRE report, pg. 2). The questions I ask cannot be worthwhile because they do not seek stable explanations and generalized patterns, but I share examples of challenges to the certainties on which ESRA is based. In the opening chapter, I ask what educational research that does not make claims to truth might look like. In response I offer three explorations grounded by concerns about self and other that diverge into explorations of the theoretical and methodological certainties of scientifically based research, objectivity, evidence, and transparency, and how all of these concepts impact representation of the other. I wondered about the relationship between the tenets of scientifically based research and the question, “How can we best listen to, work with and represent the people our work is intended to serve?” (Freeman, et al, 2007, p. 30). In other words, what are the implications of ESRA and ESRA-like thinking for rural research?

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2 Ideology names a kind of slippery consensus or “common sense” that a culture, people, or group shares among its members…ideology, understood as a kind of cultural common sense, doesn’t need to be articulated, discussed, or justified (Searls Giroux & Nealon 2003).
Correct Science and Truth Claims

What does this inquiry demonstrate about what it means to have correct science? Science as designated by ESRA is a science of exclusion. Replication is the hallmark of correct science (Lather, 2004) and methodologies dependent upon replicability and generalizability exclude the qualitative emphasis on “context, situated knowledge, and the researcher as instrument” (Lather, 2004, p. 769). As much as correct science may not generalize to rural settings, the findings of rural research likely will fail tests of generalizability and replicability.

For rural qualitative research, methodologies that include attention to context preclude the possibility of generalization as “rural” is a highly unstable category of analysis. Thus, qualitative research attentive to a rural context is unlikely to yield generalities. This suggests a two-fold concern. It is problematic when rural qualitative research makes claims to generalizable truth and equally troublesome that positivist, correct science asserts homogenized truths that (mis)represent the rural. Perhaps this is less of a question of appropriate methodology and more a matter of the possibilities of generalizable truth. Situated, not generalizable knowledge is the product of educational researchers and those who pay heed to matters of context and self offer less sure, but more honest contributions to knowledge about schools, learning, and teaching.

If the truth claims of correct science are impossible, what would educational science that did not claim truth look like? This study offers one example, but is not intended to offer a “best” way. The “best” way must be just this: a way to listen to, work with, and represent those we intend to serve. I’ve demonstrated how the way we are to work now according to ESRA cannot hope to meet this standard. Neither can the best way be scientifically based research as legislated from ESRA to NCLB where evidence of the outcome of objective science applied in the classroom is test scores. I suggest that the “best” way to do educational research cannot be
exclusive and legislated. Instead, the best way must be a way that encourages insight into ourselves, others, the literacies of our participants, and how we represent them. At the onset I suggested that my goal was to trouble scientifically based research in the Education Sciences Reform Act by demonstrating the potential value of research that is “messy” (Bloch, 2004, p.97) and unsettling. I did this in the setting-to-work mode of postfoundational theory not only by critiquing what I find to be problematic in SBR, but also by engaging with the principles in the kind of ethnography that was indeed a “self-wounding laboratory” for discovering the rules that govern truth (Lather, 2001). I conclude now not with a description of the best way, but ask readers to consider my attempts at troubling of where we are now as a means of thinking about how things might be different with understanding, reflection, and action. There are indeed, a thousand things to do.

To operate from a premise of impossibility of satisfactory solutions means to not assume resolve but instead, to be prepared to meet the obduracy of the problems and obstacles as the very way toward producing a different knowledge and producing knowledge differently. Foucault (1981/1991) termed this ‘the absolute optimism’ of ‘a thousand things to do’ (p.174) where our constant task is to struggle against the very rules of reason and practice inscribed in the effects of power of the social sciences (Lather, 2004, p.28).

Voices

Whose voices cannot be heard within ESRA science? Neither participant nor researcher voice has any place within scientism because of its incompatibility with objectivity and generalizability. It is apparent that my attention to voice position me and this study outside of the boundaries of ESRA. My original inquiry into rural literacies as well as the participants I sought to include are deemed irrelevant by ESRA because the study questions could not be molded into the structure of scientifically based research. Under ESRA other questions and the other people they concern are excluded. I suggest that exclusion in the form of scientifically based research is
best understood as the historical outcome of over a century of influence. Within this history, one can note groups and individuals whose collective voices altered the path of educational science because they determined it was not meeting their needs. Those whose voices are silenced by the science of ESRA must consider the implications of their exclusion and advocate for change. Neither my own consideration of my relationship to this research nor insights from my participant could be voiced within the sanctioned conversation of scientifically based research. Would teachers of rural children be interested in considering the literacies children bring to schools and how they might teach to these strengths? Should teachers consider the impact of their own rurality and those of their students’ ruralities on their classroom lives? Why are questions like these considered irrelevant to policy makers?

**Assumptions**

What assumptions in ESRA are unwarranted and harmful? I’ve offered a study that reflects the impossibility of the assumptions of correct science: objectivity, reliability, and generalizability. Self and other cannot be ignored because it is through the discourses that constitute one’s “self” that we understand the other and how the other understands us as well. There can be no objective understanding outside of self. Self and other also must undermine the false authority of evidence and transparency and these constructs’ relationship to representation. Self and other trouble fixed rules about what counts as evidence and its command of methodology and representation. In light of all of this, there is no room for representations presented as transparent and “true.” Yet, the innocence of science is assumed on the grounds of objectivity. “The Real of science has an unchanging and unchangeable existence independent of the knower” (Flax, 1992, p. 450). The rules of reason and practice are also problematic because they discipline which questions can be asked and which cannot. The questions I’ve asked in this
study (in addition to those I intended to ask about rural literacies) aren’t askable within the constraints I’ve described. Whatever insights that could be gleaned are lost. Perhaps this is the main danger to which Foucault refers:

My point is not that everything (constructs, categories, taxonomies, priorities, hierarchies, measurements, practices and discourses) is bad, but that everything is dangerous, which is not exactly the same as bad. If everything is dangerous, then we always have something to do. So my position leads not to apathy, but to a hyper and pessimistic activism. I think that the ethico-political choice we have to make every day is to determine the main danger (Foucault, 1983).

The standards are dangerous like Foucault proposes because they discipline inquiry through the “everything” that he names. At the end of the day, this means that the only research that counts is that which aims to measure children and translates into findings that capture them in representations of their true selves.

**Democracy**

Where are we going with democracy with the science of ESRA? Does the exclusion of our rural children and teachers suggest that other groups may be excluded from educational research? St. Pierre makes the alarming claim that the “voracious desire” by legislators to center and control educational research “marks the end of the democratic politics that inspire our educational system” (St. Pierre, 2004, p.137). Indeed, science has been determined for “us” in advance through mandates instead of dialog. It is parental, perhaps voracious. The ESRA vision of science reflects the world view of its authors and those who mobilized it through legislation. It reflects unwavering faith in empiricist science, its ability to capture reality, and its positive, implicit relationship with societal progress. Science is a club (Shannon, 2007) that dictates who benefits and who loses while claiming innocence. The leveling of power is arbitrated by science (Flax, 1992) where policies made by people are presented as incontrovertible outcomes of a
particular science. This is reassuring for both the legislators and the legislated. There is pleasure and satisfaction in that which is simple, aversion to the complex, and political benefit to solving “problems.” Loss may be methodological and theoretical exclusion, disciplining of thought, exclusion of participants, or counterproductive representations. St. Pierre asks: “whose science is this science, the one that remains when difference is erased? Who was in the room when decisions about the real nature of science were being made?” (2004, p. 135). Foundational claims, about correct methodology for example, sanctioned to improve our “democratic educational system,” (St. Pierre, 2004, p. 136) “lure us away from the lived difficulty of political life” (Butler, 1995, p. 131, cited in St. Pierre, 2004, p. 136), denies democratic politics, ignores those whose realities and knowledges are not mainstream, and labels as “extreme” those who refuse (St. Pierre, 2004).

Conclusion

I began this work with the hope that what I learned might inform teachers of rural children. I suspected that there are rural ways of knowing that differ from the mainstream. I wanted to investigate rural literacies and intended to do so with a group (the Amish) that I can distinctly point to as rural. Already having identified this group as falling outside the norm in important ways, I wanted to call attention to rural ways of knowing and consider how teachers of rural children might teach to these strengths. I hadn’t yet considered that there may not be generalizable “rural ways of knowing” to describe. I hadn’t yet considered that it may be more productive and honest to consider teachers within very specific rural contexts rather than to seek generalizable truths about how to teach rural children. So it seems that while I claimed that the initial direction of the study opposed the principles of scientifically based research because I rejected claims to objectivity, I held fast to the belief in the potential of science to capture truth
that could generalize to “the rural.” It was only because of my participant’s conditions of participation that I was forced to reconsider. While now I can legitimately critique legislators’ penchant for the simple and aversion to the complex by offering this work as a challenge, my initial study, though qualitative from the onset, was not incompatible with many of the same methodological assumptions on which ESRA science is based. Scientism is more than objectivity.

As I’ve described, this more specific agenda about rural literacies quickly became overshadowed by bigger questions. Rural children are an example of a group that is outside the norm in some ways and they are one of many who are excluded in homogenized, objective view of literacy and literacy research. Whether a child is rural, poor, a member of a religious minority, “gifted,” or is proficient in a language other than English, teachers must look away from these differences. This study suggests that teachers should reject the objective and stable representation of the children. Instead teachers should look closely and question the positions to which children have been assigned, consider how the positions inform the children’s literacy development, and how teachers might facilitate literacy in light of the identities that the children have assumed. The teachers themselves might reconsider their own range of identities and how they affect their teaching. I was forced to confront what I “knew” about other rural children: they are poor, dirty, and not at all like me- this being “true” even as I claimed to be rural repeatedly throughout the study. Within the constructs of subjectivity and discourse I find that I cannot only understand myself as discursively positioned, but use these ideas as tools to reconstruct myself (still as rural) and readjust my distorted thinking about the other. I can name and understand my bias and choose to engage another discourse to counteract the harmful one. Perhaps others who work with rural children might engage in the same work of analyzing “common sense”
understandings. Research around these and other related inquiries can be neither objective nor
generalizable. These processes are of no relevance under current policy.

As an instructor of pre-service teachers, I ask my students to consider difference. I ask
them to name their own discourses: teacher, rural, urban, suburban, and a variety of others that
they consider through an array of reading and writing opportunities. My hope is that by
identifying and talking through their strengths and positions, they will recognize and teach from
what their children bring to the classroom, and resist the homogenizing vision of ESRA science.
Perhaps they can identify where they themselves could be excluded or captured in “true”
representations. Although educational researchers have an important role to play, teachers must
lead the way in resisting scientifically based research because they are closest to its affects. They
must be willing to be political, to question and formulate stances, to resist and intervene. Too,
they must be suspicious of certainty, recognizing it as potentially dangerous, and work to accept
the “magnitude of uncertainty in education” (Britzman, 2003, xiii). There is little certainty to be
found when one looks closer at one’s students and self. Teachers must understand that
everything is indeed dangerous and engage in Foucault’s hyper and pessimistic activism.
Postscript: Poststructuralism

What can poststructuralism offer that is an alternative to ESRA science? First, Cherryholmes would remind us that our favored research tradition reflects that which we find beautiful and desirable (Cherryholmes, 1999). While I will make suggestions about the role of poststructuralism in the critique of scientifically based research, it is worth reiterating that what I propose reflects only what I find beautiful and desirable: “Postmodernists must firmly situate themselves as constructs within their own discourses” (Flax, 1992, p. 454).

Early in chapter one, I noted Cherryholmes’ assertion that poststructuralist projects have no interest in consequences and are thus not forward looking. He claims:

“These investigations (poststructuralist), however, do not as a matter of course extend to the consequences of those texts and practices. Poststructural and postmodern investigations tend to be investigatory, interpretive, critical, and analytic. They are not forward-looking. They are oriented to commentary and criticism instead of consequences and action. Poststructuralism and its postmodernist relatives do not have a project that looks to action, nor do they seek one” (Cherryholmes, 1999, p. 4).

I offered reviews of work by Bloch (2004), St. Pierre (2002), and Lather (2004) in addition to my own study that contradict this claim. In this postscript, I respond to Cherryholmes’ critique of poststructuralism by considering the role of truth claims in poststructural theory and reiterate the relevance of the theory to the questions raised in this research.

Perhaps poststructuralism is charged with not being oriented to consequences and action because it is perceived that there is no truth in poststructuralist thought. If there is no truth, how could there be agreed upon action? While poststructuralism challenges the belief in innocent knowledge, meaning knowledge (or truth) that will tell us how to act, a universal, interest-free truth for the good of all (Flax, 1992), it does not reject truth claims out of hand. Instead, truth is understood as discourse-dependent (Flax, 1992). The discourse(s) from which truth emanates,
and thus the truth claims themselves, are contextual, heterogeneous, and often conflicting (Flax, 1992). The resolution of conflict then can happen not from overcoming discourse or relying on an objective truth, but from a “prior agreement on rules” (Flax, 1992, p. 452). If policy is the outcome of discourse reflective of specific worldviews and (true?) resolution of conflict can only be abated with agreement about rules, wherein lies the practicality of resistance? An impossibility or an exercise in futility? “(via poststructural theory) it is what seems impossible from the vantage point of our present regimes of meaning that is the between space of any knowing that will make a difference in the expansion of social justice and the canons of value toward which we aspire” (Lather, 2007, p. 16). Impossibility, then, is repositioned from hopelessness to enabling, an opening up of the potential of limits, action, and the as-yet unthinkable.

Elizabeth St. Pierre (2004) offers an example of work against “the impossible” and the relevance of postmodernist thought in her troubling of truth claims represented in National Research Council’s report, the foundation of ESRA of 2002. The report reads: “It is possible to describe the physical and social world scientifically so that, for example, multiple observers can agree on what they see” (Shavelson & Towne, 2002, p. 25). She notes that clearly, poststructuralism takes issue with the possibility of objective and generalizable truth (and this is presumably why postmodernism is specifically rejected in the NRC report), particularly in light of decades of critique of scientism. Further, with an eye toward consequences, she asks:

What happens to children in classrooms when we assume that the random assignment of subjects will produce true knowledge, that outcomes can be controlled and predicted, and that the reality of one classroom can be generalized to another? Questions such as these illustrate the very value-laden position taken by a supposedly value-free scientific-based research (St. Pierre, 2004, p. 134).
I cannot offer any firm answers to the pragmatic questions St. Pierre poses, but I can destabilize the premises by which the outcomes will occur. I offer this study both as an example of a troubling of research that requires claims to objective truth and as a model of how truth claims may be made under poststructuralism: discourse-dependent, contextual, heterogeneous, and conflicting. This is necessary in order for action (change) to occur. It is a destabilizing and experimentation in alternatives; it is action.
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Glossary of Terms

**Humanism**- a theory or discourse that “has described the truth of things for centuries” (St. Pierre and Pillow, 2000, p.5). Humanism takes as its starting point the belief in the capacity humans to act (and discover knowledge) based on rationality. Humanist thinkers include: John Locke, David Hume, Immanuel Kant, and Edward Said. The themes, or foundations, of humanism relevant to what counts as knowledge and personhood are as follows:

- conceptualization of the self as stable and coherent
- transparency of language
- ability of reason and science to produce objective, reliable, and universal knowledge
- knowledge produced from the correct application of reason will be true
- conflicts of truth, power, and knowledge can be overcome by grounding claims to authority in reason
- freedom results from obedience to laws based on the correct use of reason (St. Pierre and Pillow, 2000, p. 5)

“Humanism has been used by liberals, Nazis, feminists, Marxists, Protestants, Catholics, and others in the production of truth, knowledge, norms, and policies” (St. Pierre and Pillow, 2000, p.5). Bronwyn Davies offers specifics on the concept of the person in humanist terms:

1. Any sane adult has a coherent, unified, and rational identity. Language, a transparent tool, is used not to constitute this self, but to discover the self.
2. The sane adult makes choices based on rationality and the cohesiveness of the inner self. Choices reflect one’s humanness (or lack thereof).
3. Although one is socialized by society, to “follow the crowd” is to fail to establish one’s own identity.
4. Continuity of identity is established by early socialization and internalization of values.
5. Stories can represent true events (2000, p. 57).

**Positivism**- experimental science, one of modernity’s “greatest accomplishments” (Hatch, 2006, p. 404) first theorized by Auguste Comte. It is a label “for the application of the scientific method to the social sciences” (Scheurich, 1997, p. 75). “The positivist temper,” as described by Bernstein recognizes only two models for legitimate knowledge: the empirical or natural sciences and the “formal disciplines” such as logic and mathematics. He characterizes the analysis and clarification of data as secondary, in fact, a “parasite” (Bernstein, 1995, p. 5). Under positivism physical sciences should be the model for social science. It is assumed that “truth” can transcend opinion and personal bias (Carey, 1989, p. 99 cited in Denzin and Lincoln, 1998, p. 7) and can be uncovered by way of experimental research (Hatch, 2006). The only knowledge which is authentic is that which results from the scientific method. Anything which cannot be reformulated through scientific discourse is meaningless (Bernstein, 1995). In summary, Denzin and Lincoln describe positivism as the view that social research should be based on the scientific method and, “this method is exemplified in the work of modern physicists, and
that it consists of the rigorous testing of hypotheses by means of data that take the form

Postfoundational theory- “In the Cartesian tradition of modern epistemology,
Enlightenment thinkers implied that people were capable of being rational subjects who,
after grasping the "foundations" of knowledge, could achieve a relatively unambiguous
understanding of the external world and could use this to transform the social conditions
of their existence” (Antonio & Kellner, http://www.gseis.ucla.edu/faculty/kellner/essays/modsoctheory80s.pdf, p. 3)
It is theoretical work in opposition to humanist “foundations” (see humanism). Judith
Butler characterizes the impetus of postfoundationalism as the need to “interrogate what
the theoretical move that establishes foundations authorizes, and what precisely it
excludes or forecloses (p.7)’” (Butler, 1992 quoted in St. Pierre and Pillow 2000, p. 5).
Patti Lather characterizes postfoundational thought as an epistemological situating of
oneself as “curious and unknowing” in contrast to a participation in a “mastery project”
(2007, p. 9).

Postmodernism- generally, a mode of critique of modernism and enlightenment ideals
about the potential of science to produce truth in the form of the metanarrative. It
includes, for example, some feminist, critical, postcolonial, queer, and poststructural
theories (St. Pierre, 2002). Denzin and Lincoln describe it as a “contemporary sensibility
that privileges no single authority, method, or paradigm” (1998, p. 31). Patti Lather
describes it in more general, social and historical terms as “the material and historical
shifts of the global uprising of the marginalized, the revolution in communication
technology, and the fissures of global multinational hyper-capitalism” (2007, p. 5). To
generalize, postmodern theorist question beliefs still prevalent in American culture
derived from the Enlightenment (Flax, 1987, p. 624). Flax delineates these beliefs as:

1. Existence of a stable, coherent self
2. Reasons and it’s “science”-philosophy-can provide an objective, reliable, and
universal foundation for knowledge
3. The knowledge acquired from the right use of reason will be “True.”
4. Reason is universal and independent of context
5. Freedom exists from obedience to laws based on the correct use of reason. In
obedience, one exercises autonomy and affirms her existence as a free being.
6. By grounding claims of authority in reason, truth can serve power without
distortion. When knowledge serves power, freedom and progress are assured.
Knowledge can be neural and socially beneficial.
7. Science is the way to all true knowledge. Like knowledge, it is neutral and
socially beneficial.
8. Language is transparent, a medium of representation. There is correspondence
between truth and the real. Objects are not socially constructed, but made real

Nealon and Giroux’s list of postmodern characteristics include: a sense of
disjunction or deliberate confusion, irony, playfulness, reflexivity, detachment,
foregrounding of constructedness, a suspicion of neat and easy conclusions, and an
emphasis on process instead of product (2003). Postmodernism is both a mode of critique (or “style” say Nealon and Giroux) and a historical period, but this is not to suggest that the ideals of modernity are prostrate. Rather, “the task (of postmodernism) is to interrogate what the theoretical move that establishes foundations authorizes, and what precisely it excludes or forecloses” (Butler, 1992, p. 7).

Flax’s list of the claims made about postmodernism, conflated with poststructuralism, include:

1. Postmodernists are apolitical (because they refuse the unitary subject)
2. There is no truth. Conflict can only be resolved through power.
3. Because of subjectivity, there is no possibility of agency.
4. If you write clearly, you cannot be a postmodernist. Postmodernist write obscurely on purpose no one outside their “cult” can understand and question them.
5. You either accept or reject postmodernism in its entirety. (Flax, 1992)

Poststructuralism\(^1\) - Differentiating between postmodernism and poststructuralism, Patti Lather notes that poststructuralism “refers more narrowly to a sense of limits of Enlightenment rationality. It particularly foregrounds the limits of consciousness and intentionality and the will to power inscribed in sense-making efforts that aspire to totalizing explanatory frameworks, especially structuralism with its ahistoricism and universalism” (2007, p. 5). It is a theory that offers critiques and methods for examining the functions and effects of any structure or grid of regularity via a concern for power, language, and discourse. Within poststructuralism, structures of meaning are not abstract, fixed, or universal as they are within structuralism (Nealon and Giroux, 2003). Poststructuralism troubles the humanist conceptualization of the subject (as rational, conscious, stable, etc.) in addition to epistemologies that assume the historical progress of man toward knowledge and freedom, the possibility of representing, measuring, predicting, and controlling knowledge of the social world, the possibility of objectivity, and unwavering faith in the scientific method as the path to true knowledge.

However, it is not a corrective to humanism. Poststructuralism offers opportunities for work at the boundaries of humanism instead of offering a successive regime of truth (From: St. Pierre and Pillow, 2000, p. 6). Poststructuralism works in response to humanism; it does not offer a replacement, but offers critiques and methods for examining its assumptions (St. Pierre and Pillow, 2000). Poststructuralism will never claim to have gotten it “right” (St. Pierre and Pillow, 2000). Sandra Harding describes poststructuralism’s relation to humanism with her characterization of the relationship between feminism and the structures or grids of regularity that poststructuralism critiques: “How can feminism radically redefine the relationship between knowledge and power if it creates yet another epistemology, yet another set of rules for the policing of thought?” (1986, p. 656). Bronwyn Davies captures what poststructuralism does do: “(it) open(s) up discourses and practices to questioning, and provide strategies for questioning that run against the grain of common sense and of domination (and dominating) discourses and practices” (2000, p. 169).

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\(^1\) See chapter 6 for a response to the critique of truth claims within poststructuralism.
Qualitative Researcher- “uses methodology to study things in their natural settings, attempting to make sense of, or interpret, phenomena in terms of the meanings people bring to them” (Denzin and Lincoln, 1998, p. 3). Qualitative researchers use a wide variety of empirical materials: case study, personal experience, introspective, life story, interview, observational, historical, interactional, and visual texts (Denzin and Lincoln, 1998). While qualitative researchers write from within their own paradigms, qualitative research has no methodology, paradigm, or discipline of its own (Denzin and Lincoln, 1998).
Appendix A
Description of legislation from http://thomas.loc.gov

Reading Excellence Act of 1999- A bill to provide for reading excellence.
Title I: Reading Grants - Amends the Elementary and Secondary Education Act of 1965 (ESEA) to establish a new title XV Reading Grants program.
The term ‘scientifically based reading research’—
(A) means the application of rigorous, systematic, and objective procedures to obtain valid knowledge relevant to reading development, reading instruction, and reading difficulties; and
(B) shall include research that—
(i) employs systematic, empirical methods that draw on observation or experiment;
(ii) involves rigorous data analyses that are adequate to test the stated hypotheses and justify the general conclusions drawn;
(iii) relies on measurements or observational methods that provide valid data across evaluators and observers and across multiple measurements and observations; and
(iv) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review.

Original Castle Bill- (H.R. 4875, 2000)
scientifically based quantitative research standards:
(A) means the application of rigorous, systemic, and objective procedures to obtain valid knowledge relevant to education activities and programs; and
(B) includes research that--
i) employs systematic, empirical methods that draw on observation or experiment;
(ii) involves rigorous data analyses that are adequate to test the stated hypotheses and justify the general conclusions drawn;
(iii) relies on measurements or observational methods that provide valid data across evaluators and observers and across multiple measurements and observations and across studies by the same or different investigators;
(iv) is evaluated using experimental designs in which individuals, entities, programs, or activities are assigned to different conditions with appropriate controls to evaluate the effects of the condition of interest through random assignment experiments, or other designs to the extent such designs contain within-condition or across-condition controls;
(v) ensure experimental studies are presented in sufficient detail and clarity to allow for replication, or at a minimum offer the opportunity to build systematically on its findings; and
(vi) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review.

Scientifically based qualitative research standards:
(A) means the systematic collection and analysis of data often associated with traditions
of inquiry historically based in the humanities, such as narrative analysis; and
(B) includes research that--
(i) uses some combination of participant observation, in-depth interviewing and
document collection;
(ii) is intended to explore issues and hypotheses whose underlying dynamics and factors
are not sufficiently well refined, understood, or amenable to experimental control to
permit adequate study through quantitative research.
(iii) may include case studies, ethnographies, life histories, multi-site case studies, and
participatory action research;
(iv) uses approaches to assess the experimental knowledge acquired to assure that the
findings are scientifically valid and replicable; and
(v) has been accepted by a peer-review journal or approved by a panel of independent
experts through a comparably rigorous, objective, and scientific review.

No Child Left Behind Act of 2001 - To close the achievement gap with accountability,
flexibility, and choice, so that no child is left behind. Amends the Elementary and
Secondary Education Act of 1965 (ESEA) to revise, reauthorize, and consolidate various
programs. Extends authorizations of appropriations for ESEA programs through FY
The term ‘scientifically based research’—
(i) employs systematic, empirical methods that draw on observation or experiment;
(ii) involves rigorous data analyses that are adequate to test the stated hypotheses and
justify the general conclusions drawn;
(iii) relies on measurements or observational methods that provide valid data across
evaluators and observers and across multiple measurements and observations; and
(iv) is evaluated using experimental or quasieperimental designs in which individuals,
etentities, programs, or activities are assigned to different conditions and with appropriate
controls to evaluate the effects of the condition of interest, with a preference for random-
assignment experiments, or other designs to the extent that those designs contain within-
condition or across-condition controls;
(v) ensures that experimental studies are presented in sufficient detail and clarity to allow
for replication or, at a minimum, offer the opportunity to build systematically on their
findings; and
(vi) has been accepted by a peer-reviewed journal or approved by a panel of independent
experts through a comparably rigorous, objective, and scientific review.

Education Sciences Reform Act of 2002- Became Public Law No: 107-279
To provide for improvement of Federal education research, statistics, evaluation,
information, and dissemination, and for other purposes.
(A) The term ‘scientifically based research standards' means research standards that--
(i) apply rigorous, systematic, and objective methodology to obtain reliable and valid
knowledge relevant to education activities and programs; and
(ii) present findings and make claims that are appropriate to and supported by the
methods that have been employed.
(B) The term includes, appropriate to the research being conducted--
(i) employing systematic, empirical methods that draw on observation or experiment;
(ii) involving data analyses that are adequate to support the general findings;
(iii) relying on measurements or observational methods that provide reliable data;
(iv) making claims of causal relationships only in random assignment experiments or other designs (to the extent such designs substantially eliminate plausible competing explanations for the obtained results);
(v) ensuring that studies and methods are presented in sufficient detail and clarity to allow for replication or, at a minimum, to offer the opportunity to build systematically on the findings of the research;
(vi) obtaining acceptance by a peer-reviewed journal or approval by a panel of independent experts through a comparably rigorous, objective, and scientific review; and
(vii) using research designs and methods appropriate to the research question posed.
Appendix B
Background on the Old Order Amish

The Old Order Amish are one of three Anabaptists groups in the United States: the Mennonites, the Hutterian Brethren and the Swiss Brethren/Amish (Hostetler, 1993, p.25). The Amish are direct descendents of sixteenth century European Anabaptists who fled religious persecution and immigrated to America, starting in the eighteenth century (Hostetler, 1993). Successfully positioning themselves as outsiders, the Old Order Amish have captured the American imagination largely through their use of symbols (Hostetler, 1993, chapter 11) that mark their separation from the larger rural communities in which they live. While there are distinct differences between Old Order Amish churches scattered across the U.S., the Old Order Amish can generally be identified by their distinctive dress, use of horse and buggies and rejection of electricity. They are commanded biblically to be "in the world but not of it" (John 17:14-15, King James Version of the Bible). Thus, they affirm their separation and protect members from outsider influence. These forces of separation in symbol and action have hampered the access of social science researchers, making ethnography a virtual impossibility within this group that defines itself by its separation from the world.

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