EXAMINING CENTRAL ISSUES IN LITERACY RESEARCH, THEORY, AND PRACTICE

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RESEARCHING THE LITERAL: OF MUTED VOICES, SECOND TEXTS, AND CULTURAL REPRESENTATIONS*

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During the past two years I have read broadly in literacy-related fields in anticipation of today, doubting from time to time that I would ever settle on a topic for this address. In retrospect, I suppose that ideas for this talk have been with me for quite some time, but it took months and months of reading and reflecting on other people's thinking to bring my own ideas into focus.[1] In choosing to research the literal of muted voices, second texts, and cultural representations, I set my sights on discovering ways to keep scholarly writing from masking more than it discloses. I am personally interested in this topic because of risks I have been unwilling to take in the past and opportunities I have missed in writing up my own research.

According to the *Oxford English Dictionary* (1987), *literal* is derived from the Latin term *litteralis* meaning to represent the very words of an original translation, version, or transcript. In this paper I will argue that as researchers we need to represent our literal selves by unmasking and laying bare the influences that shape our writing. It is an argument that has its risks, but also its rewards. The risks involve questioning privileged ways of writing and knowing, sharing interpretive authority, and coming to terms with the voyeuristic aspects of research. The rewards include being heard and understood more fully, but more importantly, I believe, they include finding a richer and more textured side to literacy research.

Of Muted Voices

Silencing the discourse of others is one meaning associated with muted voices. It is not the meaning intended here, however. The muted voices that concern me are our own, and they are of our own making. They are muted in the sense that all too often what we want to communicate through scholarly writing is suppressed or largely unintelligible to all but a select few.

In a society that favors symbolic thinking, it is often difficult to get anyone to admit to being a literal thinker, much less a writer of plain English. Yet, clear writing in the professions is essential (Gottlieb, 1992). Increasingly, state legislatures are requiring that medical consent forms, business documents, and legal contracts be writ-

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[1]I would like to thank the following individuals for reading and reacting to an earlier draft of this paper: JoBeth Allen, Patricia Anders, Michelle Commeyras, Sandra Hollingsworth, Lorri Neilsen, and David Reinking.

ten in straightforward language. If such is the case for medicine, business, and law, can we in education be any less concerned? As researchers engaged in the study of literacy, our work has the potential to make significant differences in people's social, economic, and political well-being. To deny public access to this work is insensitive at best and irresponsible and elitist at worst.

But perhaps this is too harsh an indictment. Perhaps we merely believe we have the luxury, or even a mandate, to write for a relatively small circle of like-minded academics. Recently, Yvonna Lincoln (1991) attempted to locate the origins of this kind of thinking. Speaking before a group of her peers at the 1991 annual meeting of the American Educational Research Association in Chicago, Lincoln quoted from Marianna Torgovnik's work at Duke University. According to Torgovnik (1991):

> All of us who write . . . today wrote dissertations yesterday. And our teachers often tried, and succeeded in, handing on what they perceived as the correct—that is, the careful, the judicious, the fair—way to write. But the styles we were taught can't work now in the same way as they worked fifty or even fifteen years ago. No one who gets around to writing a book, or even an essay, ever reads everything that has been written about a subject. Yet we cling to the fiction of completeness and coverage that the academic style preserves. This style protects us, we fondly believe, from being careless or subjective or unfair. It prescribes certain moves to ensure that the writer will stay within the bounds that the academy has drawn. . . .
>
> If all we want to do is to write for professional advancement, to write for a fairly narrow circle of critics who exist within the same disciplinary boundaries as we do, there is nothing really wrong with the traditional academic style. . . .
>
> But when [researchers] want to be read, and especially when they want to be read by a large audience . . . [the process] means writing as persons with feelings, histories, and desires—as well as information and knowledge. (p. 33)

Staying within the bounds that the academy has drawn is safe. It also takes less time and effort, as Sandra Hollingsworth of Michigan State University discovered when she sought permission to deviate from the guidelines set forth in the *Publication Manual of the American Psychological Association* (American Psychological Association, 1983). Sandra, or Sam as she is known to most of her NRC colleagues, wanted to use authors' first names in place of their initials in the reference list of her article (Hollingsworth, 1992) that appears in the Summer 1992 issue of the *American Educational Research Journal* (*AERJ*). In keeping with feminist research practice, Sam wished to identify the gender of the authors whose works had enabled the women in her study to examine their personal experiences as literacy teachers and to claim ownership of their education through their participation in the research process.

In reflecting upon this experience, Sam wrote in a letter to me dated September 14, 1992 (and I quote with her permission):

> The publication of the article itself, *Learning to teach through collaborative conversation: A feminist approach,* with its narrative style and critical stance on teacher education in literacy, was a challenging proposition. It took three rounds of reviews before it was a "go." The publication of first names to display and celebrate gendered contributions was even a more challenging effort. I simply don't think naming had been questioned before—and therefore became precedent setting.

Sam ended her letter by noting how appreciative she is of Hilda Borko, who was the editor of *AERJ* at the time, and Jeanne Spaeth, *AERJ*'s production editor, for their willingness to assume the risks that allowed her to use the authors' first names in her list of references.

I would like to pursue the issue of naming further because I believe it illustrates how certain privileged conventions of scholarly writing can threaten the integrity of our research by masking who we really are. Although I realize the argument can be made that by *not* divulging an author's gender, we allow others to evaluate the work more objectively, I agree with Sam Hollingsworth who wrote in her letter to me:

> Including first names so that gender (and sometimes ethnicity) becomes visible is not only an obvious feminist issue; it is also an epistemological issue. Sandra Harding, in her 1991 book, *Whose science, whose knowledge: Thinking from women's lives,* argues that the dominant concepts of science, objectivity and methodology, never written to take into account the experiences of women and minorities, are problematic in themselves. Therefore, publication guidelines supporting the same paradigms by hiding gendered opinions are not only discriminatory, but questionable in terms of scientific scholarship. (Personal communication, September 14, 1992)

I expect the issue of naming will become a concern of other editorial teams and publication committees as they come face to face with authors who wish to disclose the influence of gender, race, and ethnicity in their professional writings. Or, as Jay David Bolter (1991) has pointed out in his book, *Writing Space: The Computer, Hypertext, and the History of Writing,* similar challenges to publication guidelines will likely occur when the electronic medium begins to question the values of stability and authority as traditionally defined by print technology.

But for me the larger issue is why we typically choose to mute our own voices by not disclosing in the *body* of our scholarly writings the kind of information that would enable readers to discover our social histories, biases, beliefs—in short, who *we,* as academics, really are. For as JoBeth Allen (personal communication, November 24, 1992) commented after reading an earlier draft of this paper, we seldom have qualms about making clear (through various instruments, journals, and interview data) the histories, biases, and beliefs of the teachers and students we study.

Why do we choose to hide behind a facade of objectivity in matters like: Which research questions get asked and which do not? Which populations are studied extensively and which are ignored for the most part? or, which methodologies are embraced and which are discarded—and why? As Jerry Harste (1992) reminds us, "To say 'I will select my methodology based on my research question' is to fail to realize that methodologies have in them theories of language, learning, and knowing. Paint-by-number research is neither innocent nor neutral" (p. x). Or, as Sandra Harding (1987) puts it, "[those] who identify and define scientific problems leave their social fingerprints on the problems and their favored solutions to them" (p. 184).

Sometimes these "social fingerprints" are so permanently fixed in a researcher's past that the only way to deal with them is to first acknowledge their presence and then account for their effects. This is what Valerie Walkerdine (1990) of the University of London has done in her book *Schoolgirl Fictions.* In a section of the book called "Working-Class Rooms," Walkerdine describes how for years as an academic she managed to conceal her working-class origins. But in 1983, while participating in a research project that required her to be present for long periods of time in the home

of a white working-class family, Walkerdine came face to face with her past:

> Where and who was I: the working-class child of my fantasies or the middle-class researcher who was part of an attempt to tell a truth about "The Working Class"? . . . I was struck by the . . . anxieties and pain triggered in me by being perceived as a middle-class academic confronting a working-class family. Although I invested considerable desire into wanting to "be one of them" at the same time as "being different," no amount of humanistic seeking . . . would get them to see in me a working-class girl 'like them.' Rather than disavow that dynamic, therefore, it became necessary to work *with* it and to acknowledge . . . their reading of me as middle-class in the data I collected. (pp. 195-196)

One year later, in 1984, Walkerdine wrote what she describes as one of the most painful essays of her life. According to her, that essay, *Dreams from an ordinary childhood,* "was a 'coming out' as working-class, a realization that for years I had been frightened to reveal my past in academic circles and had engaged in a kind of masquerade in which I had . . . felt ashamed of my background" (Walkerdine, 1990, p. 157).

In the summer of 1990 while attending a conference at Mt. Holyoke College, I had the privilege of meeting and talking with Valerie Walkerdine. She was a captivating speaker and an equally good listener. Our conversation took several turns, as I recall, but what I remember most vividly was the time we spent comparing our lives growing up "working-class" in the 1940s and 1950s—she, in an area outside of London, and I, in a small town in upstate New York. Like her, I had been influenced greatly by a maternal grandmother who lived close by and fed my dreams of becoming a teacher. Unlike Walkerdine, however, I have never tried to hide my blue-collar origins, though for sure a few unpleasant childhood memories linger—like the day a friend, Carol, in third grade told me that her father, a wealthy lawyer in our hometown, would not let her come to my house to play after school because my father worked in a factory.

Each of you, I suspect, can recall something from your personal past that continues to influence how you think about life, generally, and how you report your research, specifically. For me, the childhood memory of being judged on the basis of my father's place of work had little bearing on the research I did as long as it was the kind of research that allowed me to collect data in two or three runs at a school, to code the data for computer analysis, and to report what I found in a fairly distanced and prescribed manner.

However, when I became interested in year-long classroom observation studies that required closer and more sustained contact with participants and more interpretive forms of writing, why was it I chose not to observe in white, working-class neighborhoods? Why did I also elect to stay as far away as possible from upper-class professional neighborhoods? I suspect that I made these choices for reasons that similarly influenced the questions I asked (as well as those I avoided), how I couched my findings, and for whom I wrote. The point I am trying to make is that I believe as researchers we need to lay bare our interests in studying a particular population (or individual) and to explain how our own background experiences will likely influence what we find and report in our writings. If we do this, we will be one step closer to discovering ways to keep scholarly writing from masking more than it discloses.

Second Texts

The notion of "silent" *second texts* caught my attention as I was reading Henry Louis Gates's (1987) introduction to *The Classic Slave Narratives.* According to Gates, by the end of the Civil War over 100 former slaves had written book-length accounts detailing the horrors of human bondage. So-called second texts were the prototypes for many of these first-person accounts. For example, the first slave narrative written by Olaudah Equiano in 1789 is believed to be the "silent" second text (i.e., the original model) for Frederick Douglass's well-known 1845 narrative. As Gates noted, "[through] this process of imitation and repetition, the black slave's narrative came to be a communal utterance, a collective tale, rather than merely an individual's autobiography" (p. x).

As communal utterances and collective tales, second texts connote a shared intent among two or more authors. And indeed, this was the case according to Gates (1987):

> Each slave author, in writing about his or her personal life's experiences, simultaneously wrote on behalf of the millions of silent slaves still held captive throughout the South. Each author, then, knew that *all* black slaves would be judged . . . on this published evidence provided by one of their number. The slave authors therefore had to satisfy the dual expectations of shaping the random events of their lives into a meaningful and compelling pattern, while also making the narrative of their odyssey from slavery to freedom an emblem of every black person's potential for higher education and the desire to be free. (p.x)

But what happens when two or more authors do not share the same intent? Can there be a communal utterance or collective tale in such an instance? Or must one author's version of a text take precedence over another's? If so, is the "silent" second text (i.e., the original) the privileged text? These questions give rise to an interesting set of issues that I will explore next through an intergenerational case study involving Katherine Borland and her grandmother, Beatrice Hanson. Borland (1991), who was a doctoral candidate at Indiana University when she wrote *"That's not what I said": Interpretive conflict in oral narrative research,* took the first half of her title from a statement that her grandmother made after reading an initial version of Borland's study. So that the issues become clear, let me try to summarize the study.

On a summer afternoon in 1944, at the harness races in Bangor, Maine, Beatrice Hanson engaged in a lively verbal wrangle with her father as they vied to pick the winning horse. Forty-two years later, Beatrice vividly recalled the events of that day for her granddaughter, Katherine Borland. Because oral narrative research is conducted in a conversational setting, both parties took turns talking, which meant that Beatrice and Katherine alternately shaped and interpreted the narrated event through their own sets of experiences. Later, after Borland (1991) had analyzed their recorded conversation for recurring patterns of talk, she gave a copy of her interpretation of the narrated event to her grandmother, who rejected it out of hand.

Borland, by her own admission, had interpreted the racetrack narrative from a feminist's perspective. Her grandmother, who did not share that perspective, wrote a 14-page letter to Katherine in which she questioned her granddaughter's right to speak for her. In Beatrice's words:

> So your interpretation of the story as a female struggle for autonomy within a hostile male environment is entirely YOUR interpretation. You've read into the story what you wished to—what pleases YOU. That it was never—by any wildest stretch of the imagination—the concern of the originator of the story makes such an interpretation a definite and complete distortion, and in this respect I question its authenticity. The story is no longer MY story at all. The skeleton remains, but it has become your story. (Borland, 1991, p. 70; emphases in the original)

The issue that this conflict raises is one of interpretive authority. In short, who owns the text? Is it Beatrice? As the originator of the narrative, is it her version that we should believe? Should her "original" (the second text if you will) be privileged over her granddaughter's interpretation?

I think not. I tend to agree with Borland (1991) in her analysis of the situation, although I am also open to other points of view on the issue of interpretive authority.[2] As Borland sees it, she has not betrayed her grandmother's narrative even though her intrusion into the text has made it *her* story. Claiming that the patterns on which she based her interpretation existed in her grandmother's narrative, Borland argues for the right to point out certain features and make certain connections, even if doing so results in an interpretation that differs from the original narrator's intentions. In Borland's words:

> Presumably, the patterns upon which we base our interpretations can be shown to inhere in the "original" narrative, but our aims in pointing out certain features, or in making connections between the narrative and larger cultural formations, may at times differ from the original narrator's intentions. . . .
>
> To refrain from interpretation by letting [our collaborators] speak for themselves seems to me an unsatisfactory if not illusory solution. For the very fact that we constitute the initial audience for the narratives we collect influences the way in which our collaborators will construct their stories, and our later presentation of these stories—in particular publications under particular titles—will influence the way in which prospective readers will interpret the texts. (p. 64)

If we accept Andrew Gitlin's (1990) notion that "the telling of a story is not the rendering of facts, but rather the putting together of a plot that imposes meanings on the events reported" (p. 460), then I think we might find Borland's interpretation a compelling one. That Borland sees herself as an interpreter, and not merely a recorder, of her grandmother's story is clear. For example, Borland (1991) describes Beatrice as framing the narrative, and herself as *reframing* that narrative during the interpretive process. To Borland's credit, she includes both versions of Beatrice's story, thus making it possible for prospective readers to bring their own histories and experiences to bear on understanding the issue. I see this process of reframing second texts, though different from the one used in writing slave narratives, as yet another step forward in sharing interpretive authority.

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[2]For a discussion of these views see James Clifford and George E. Marcus (Eds.). (1986). *Writing culture: The poetics and politics of ethnography.* Berkeley: University of California Press. Also see Joan Acker, Kate Barry, and Johanna Esseveld (1991). Objectivity and truth. In Mary Margaret Fonow and Judith A. Cook (Eds.), *Beyond methodology* (pp. 133-153). Bloomington: Indiana University Press. Finally, for a discussion of the nonauthoritative research voice, see Shulamit Reinharz (1992). *Feminist methods in social research.* Oxford, England: Oxford University Press.

But what if we were to embrace Beatrice's fundamentalist view that a text means what an author intends it to mean? Viewed from this perspective, Beatrice's intentions would be privileged over her granddaughter's interpretations. However, as Lorri Neilsen has observed (personal communication, November 19, 1992), perhaps the question is not *whose* version is privileged but rather, "For what purposes are the stories used and by whom?" The question becomes an ethical one. Is it possible, as Neilsen suggests, that Borland exploited her grandmother's story to make a point? Did *you,* in fact, interpret Borland's account of Beatrice's story as being exploitive in nature? According to Cleo Cherryholmes (1993), when we interpret research, our own "reading cannot be ethically neutral [for] interpretation requires choices and choices cannot be made without reference to values and preferences" (p. 36). Ironically, attempts to achieve neutrality may simply work to keep us uninformed (Tierney, 1992).

Ultimately, what seems clear to me is the importance of making public the "silent" second texts that inform our scholarly writing. Whether or not these second texts reflect a shared intent among authors is immaterial. Individuals who read our research deserve to know about the choices we made among interpretations as well as the values and preferences that drove us to those choices. Making such information available to readers reminds me of Shulamit Reinharz's (1992) argument that it is important to bear in mind that "what is conventionally called an objective social science stance is actually a particular view from a particular standpoint" (Reinharz, 1992, p. 228; see also Krieger, 1983).

Having said as much, what might count as "silent" second texts from my own research? I believe the best example comes from a new year-long collaborative classroom observation study that Rick Umpleby, Jim Olson, and I are still in the process of writing up. The "silent" second texts are Rick's basic-track ninth-grade students' written responses to the "Flour Baby Project," an idea sponsored by CBS television and promoted in *SCOPE* magazine. As its name implies, the "Flour Baby Project" made use of 5-pound sacks of flour to which students attached stuffed stockings for the make-believe babies' heads, arms, and legs. Then, for 1 week students were expected to care for the flour babies on an around-the-clock basis, both in and out of school. The purpose of the project was to discourage students from becoming teenage parents by having them reflect on the tremendous responsibilities of parenthood.

Because Rick, Jim, and I were interested in studying how basic-track students respond to highly imaginative writing assignments, the "Flour Baby Project" seemed a made-to-order activity. However, when we collectively wrote the first several drafts of a manuscript describing what we had learned about students' hesitancy to elaborate on ideas in their written assignments, we made no mention of comments like "I don't hate being a parent [to the flour baby]. I like being a parent. I'm already going to *be* a parent." And from another student—a young 15-year-old male—"My biggest weakness as a parent would be someone telling me I'm not treating [my baby] right. I would have trouble with people wanting to take her away."

Why include these "silent" second texts in our next draft? To do otherwise is to deny future readers of our research potentially valuable information. Readers will need to decide for themselves the degree to which our own lack of firsthand experiences as teenage parents influences our interpretation of Rick's students' hesitancy to

respond to what we deemed a highly imaginative writing assignment. More pointedly, who are we to say that teenage parenting, as a writing topic, seemed all that "imaginative" to Rick's basic-track students?

Cultural Representations

One of the principal things researchers do is write. We write to represent what we have come to understand through our research. More than likely what we write, therefore, is an incomplete or partial translation of the reality of others. To paraphrase only slightly an anecdote told by James Clifford in his introduction to *Writing Culture* (Clifford & Marcus, 1986):

> [Researchers] are more and more like the Cree hunter who (the story goes) came to Montreal to testify in court concerning the fate of his hunting lands in the new James Bay hydroelectric scheme. He would describe his way of life. But when administered the oath he hesitated, [saying], "I'm not sure I can tell the truth. . . . I can only tell what I know." (p. 8)

But to tell only what *we* as researchers know is to labor under the assumption that we can represent accurately the culture of classrooms and the literacy instruction that goes on within those rooms. The fallacy of this assumption is ever present in the growing number of critiques by individuals like Leslie Roman and Linda Christian-Smith (1988), Patrick Shannon (1989), and Valerie Walkerdine (1990) who challenge researchers' authoritative voices and voyeuristic practices.

Having dealt with interpretive authority in the previous section of this paper, I focus here on coming to terms with the voyeuristic aspect of our research, and in particular, its consequences for observational research. To this end, Valerie Walkerdine's (1990) work is especially helpful. "Observation, like all scientific activity," she writes, "constitutes a voyeurism in its will to truth" (p. 173). It is this will or desire to know, not the intrusiveness of observation per se, that is problematic. For as observers of teachers and students engaged in the culture of literacy teaching and learning, we become what Walkerdine terms "Surveillant Others" who not only watch but also produce written records meant to inform the policies and practices regulating those whom we observe. Thus, the political implications of our research are indeed strong.

Thomas McCarthy, in the April 1992 issue of *Ethics,* expresses a similar concern about the political consequences of observational research. "Cultural representations," he says, "help form the images we have of others; if assimilated by those others, they help form the images they have of themselves as well. They get embodied in institutions and inform policies and practices" (p. 641).

How to account for one's presence as a "Surveillant Other" is an issue occupying much of the current postmodernist debate on cultural representations (Khare, 1992). Merely inserting ourselves into our texts—for example, disclosing who we are, the problems we encountered, and the contingencies we dealt with in doing our research and preparing it for publication—is not enough. We cannot count on such behind-the-scenes prose to dismiss the power differentials that concern Shannon (1989), McCarthy (1992), and Walkerdine (1990). According to Walkerdine: "Together, observer and observed constitute a couple in the play of power and desire. We therefore need to examine the response of the observed to their experience of surveillance" (p. 195).

Although I have been unable to locate a single literacy study in which researchers systematically inquired about students' responses to being observed, I do know from my own work that opportunities for such inquiry exist, and in fairly good number. For instance, in reviewing my field notes from a semester-long observational study conducted 3 years ago of 11th graders' understanding of what it means to be labeled "at risk" of dropping out, I discovered no fewer than 24 such opportunities. Through questions and comments like the following, students were opening the door for inquiries into how they experienced my presence as observer:

> February 7, 1989—Before class today, Lafayette stopped me as I made my way to my seat in the fourth row, three seats from the back. "What are you studying?" he asked. I answered him truthfully, but I did not ask how he felt about being studied.
>
> February 21, 1989—Students were role-playing as they read aloud from *Requiem for a Heavyweight.* Toledo, one of the better readers in the group, was putting lots of expression into her role as the doctor. Several members of the class turned around to look at me. I smiled approvingly, but I never thought to ask them later why they were interested in my reaction to Toledo's reading.
>
> March 9, 1989—Dexter, a new boy in class, stood squarely between my desk and Vicky's desk shortly before class began. He interrupted Vicky, who was paging through a jewelry catalogue: "You tell the lady what you called me—how you cussed me out—called me a _____!" Vicky looked at me, laughed softly, and shrugged like she didn't know what Dexter was talking about. Dexter said, "You know you did! If you say it again, she'll [referring to me] write you up." The second bell rang, and I dismissed the incident as unrelated to the study's purpose.

These missed opportunities, on my part, to find out what the students whom I was observing thought about being observed still concern me. What would I have learned? Would I have represented the culture of the classroom differently had I known what the students were experiencing and thinking in their role as the observed? Undoubtedly so, for as Thomas McCarthy (1992) points out in his essay on *Doing the right thing in cross-cultural representation,* when we provide space for others whom we represent to contest our representations, there will be marked changes in the way we—the Surveillant Others—go about our observations and write up our reports.

Conclusion

In researching the literal of muted voices, second texts, and cultural representations, I have argued in favor of scholarly writing that makes overt our histories, gender, beliefs, values, preferences, and cultural assumptions. In making this argument, I have tried to show how questioning privileged ways of writing and knowing, sharing interpretive authority, and coming to terms with certain voyeuristic aspects of observational research can provide a fuller and richer written account of all that we study. I hope for a few of you, at least, the challenge of writing in a manner that is more accessible and self-revealing will seem worth the effort. For granted it will take effort, and, I suspect, cause more than a little discomfort at times.

But surely the discomfort will be no greater than that which we have imposed on others, in the past, in our attempts to be objective. To quote from Sandra Harding's (1987) more graphic representation of the same idea, "We need to avoid the 'objectivist' stance that attempts to make the researcher's cultural beliefs and practices invisible while simultaneously skewering the research objects' beliefs and practices to the

display board" (p. 9).

In closing, perhaps like some of you, I am not at all certain I *can* be successful in writing research reports that lay bare the influences that have shaped my work. I am willing to try, however, believing like J. Elspeth Stuckey (1991) that "the most likely way change will ever happen is incremental, local, one person at a time" (p. 126).

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