LITERACY RESEARCH, THEORY, AND PRACTICE: VIEWS FROM MANY PERSPECTIVES

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ON MATTERS OF SUBJECTIVITY, KNOWLEDGE CLAIMS, ART, AND ETHICS IN LITERACY RESEARCH*

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I would like to begin with some of the conversations that I had with myself while reading a set of papers that recently appeared in the *Reading Research Quarterly* and a related article appearing in the *American Educational Research Association Journal*. The first paper in the set was a paper entitled "Research in the classroom—how will we ever know?" by Cathy Roller (1991). In the paper, she discussed her graduate students' reading of an article by Deborah Dillon (1989) in which Roller and her class encountered the following descriptions of discussions in a social studies class led by a teacher, identified as Mr. Appleby:

> To promote active learning, Appleby encouraged his students to freely call out answers and make comments during lesson interactions. . . . [W]hen Appleby asked a question or posed a problem he allowed and encouraged students to respond. He did not admonish Marty or LaVonne for jumping in without raising their hands or for inter- rupting each other. (pp. 245, 246)

Dillon went on to conclude:

> Appleby used whole class discussion as a means of encouraging students to actively participate in their learning as opposed to sitting back and passively listening to him. (p. 243)

A week later, Roller reported they encountered an article by Alvermann and Hayes (1989) in which the researchers were also studying Mr. Appleby's classroom. Roller quotes Alvermann and Hayes' description of Appleby:

> Students were free to speak without being called on, which often meant that several students spoke at once. . . . When the researcher asked whether the students who were the most vociferous in their attempts to get his attention made him feel uncom- fortable, Mr. Appleby said he did not want "to discipline them, for fear of stifling their willingness to talk." (p. 317)

Alvermann and Hayes (1989) went on to conclude:

> Yet in actual practice most discussions were dominated by Mr. Appleby and the same two or three students. Even these more talkative students, however, rarely produced more than two- or three-word responses. (p. 317)

. . . Despite his attempts to involve more students, the same two or three individuals continued to dominate class discussion. One of the individuals was particularly adept at drawing Appleby into a dialogue while the rest of the class looked on disinterestingly. For the most part, students' responses remained simple and unelaborated. (p. 317)

. . . The exchange pattern that was observed in Appleby's room at the time the baseline data was collected resembled a recitation rather than a discussion. (p. 318)

Roller (1991) questioned the variation in the findings of the two studies and argued that the apparent contradictions may have been tied to elements of subjectivity, as well as to the methods and lenses used in the two studies. Here is a sampling of her comments:

As we tried to resolve the apparent contradictions, we concluded that perhaps both studies had elements of subjectivity. . . .

Despite her [Dillon's] attempts to triangulate information from various sources, the picture of classroom discussions that she presented may not have been accurate. . . .

In the Alvermann and Hayes study the subjectivity appears to lie in the narrowness of their research question. My class felt that Alvermann and Hayes were so focused on their own agenda that they failed to notice important features of learning in Mr. Appleby's classroom. . . .

Could the apparent contradiction in these two reports be resolved? I think that they could have been. . . .

To understand what happens in classrooms will require greater energy and more effective techniques such as might emerge from new forms of long-term collaborations between teachers and researchers. The inherent issues of subjectivity and objectivity will require serious attention. . . . (pp. 327–328)

Although Roller (1991) described some of the issues that she and her students discussed, I wondered if they had also discussed their views of knowledge and ways of knowing. I sensed a tension between her call for accuracy and her final sentence. "The classroom is a complex environment, and understanding it will require the coordination of many lenses operating from many perspectives."

Obviously, I was intrigued with how Alvermann, Hayes and Dillon would respond. And, they did [at least, Alvermann and Dillon (1991) did] in an article entitled "Ways of knowing are ways of seeing."

Understandably, Alvermann and Dillon (1991) countered with an argument for the inevitability of subjectivity and questioned whether differences in interpretation should be resolved. As they stated "the filter through which a person views the world determines what counts as valid and reliable information" and "research is not value free." Then, in response to Roller's question "Could the apparent conflicts in these two reports be resolved?":

The answer is no—based on the differences that we discussed previously in our assumptions, methodologies, purposes and methods. We, along with Mr. Appleby, believe the two studies offer different, yet complementary views of the life in Mr. Appleby's classroom. Moreover, we venture the opinion that such conflicts should not be resolved. (p. 332)

In discussing how the data fitted together, Alvermann and Dillon (1991) explained how the two studies had very different foci, questions, and methodology. Alvermann and Hayes (1989) focused on textbook discussions across five teachers; Dillon focused on issues related to how a single teacher (Mr. Appleby) and his students interacted generally.

As I contemplated these issues—not just in terms of the present articles but also my own work and those of others—I reacted with a rash of questions.

Some questions related to subjectivity and issues of trustworthiness or authenticity: How did Alvermann and Hayes (1989) and Dillon (1989) address subjectivity at various points in the pursuit of research? How should we? What was the relationship between subjectivity and the forthcoming and emerging findings? In the context of subjective analyses how might the data "speak for themselves"? Is there a problematic relationship between subjectivity and trustworthiness? How does one provide readers with the opportunity to achieve their own perspective?

Other questions related to knowledge claims and the researcher's goals, such as: How fleeting and relativistic should knowledge be viewed? Yet other questions related to issues of the researcher's relationship to the subjects involved in the study: What was the reaction of the teacher? How did the teacher view the study? How was the teacher a partner in the research? What role did the students play?

In a stream of consciousness mode, I reflected upon the positions that were adopted and related them to a host of larger issues, namely, matters of subjectivity, knowledge claims, the art of science, and ethics of research.

On Matters of Subjectivity

As I read the Alvermann and Dillon (1991) response to Roller (1991) I had no difficulty with their claims regarding the subjectivity of research. In this regard I surprised myself. In many of my own articles I took pride in the extent to which I thought I had achieved objectivity via such methods as high interrater agreement, statistical analyses, and including a section called "limitations." Yet, I find myself agreeing with the notions that bias is unavoidable. As Namenwirth (1986) suggested:

Scientists firmly believe that as long as they are not *conscious* of any bias or political agenda, they are neutral and objective, when in fact they are only unconscious. (p. 29)

And, as Morgan (1983) has suggested:

. . . science is basically a process of interaction, or better still, of *engagement*. Scientists engage a subject of study by interacting with it through means of a particular frame of reference, and what is observed and discovered in the object (i.e., its objectivity) is as much a product of this interaction and the protocol and technique through which it is operationalized as it is of the object itself. Moreover, since it is possible to engage an object of study in different ways—just as we might engage an apple by looking at it, feeling it, or eating it—we can see that the same object is capable of yielding many different kinds of knowledge. This leads us to see knowledge as a potentiality resting in an object of investigation and to see science as being concerned with the realization of potentialities—of possible knowledges. (p. 13)

Agreeing generally with these notions is easy; addressing them (regardless of one's paradigmatic commitment) is less straightforward.

Indeed, as I contemplated how subjectivity might be addressed in research, my initial reaction was somewhat cynical. I suspected that studies in which subjectivity was embraced would either do little more than reference their subjectivity (in other words, claim subjectivity as some kind of birthright then proceed) or engage in a detailed description of their life history as background to a data presentation. Indeed, I had encountered the occasional paper in which the type of description offered included only a slightly veiled rendition of their own life story followed by an account of teaching or learning bearing only the most remote connection to the aforementioned life history. I suspected that in the name of subjectivity that I would encounter what Thompson (1978) has described (in reference to critical theory) as "an immaculate conception which requires no gross empirical impregnation" (1978, p. 13).

My cynicism extended to notions of reflexivity in research. I envisioned a kind of reflexivity that collapsed upon itself. I questioned how we can ever deal with the infinite regression associated with our subjective view of our own subjectivity. I was reminded of a joke that was popular in the early eighties as people tried to become more "centered." The joke went: "Did you hear about the guy who was extremely intent on being centered? Well, we are not real sure what happened to him. We suspect he disappeared into a dot."

I realize now that my view of subjectivity was biased and narrow-minded, I was proceeding in the manner that I had criticized others of: I was defining subjectivity without careful study—perhaps indulging in my own immaculate conception. Very few, if any, advocates of subjectivity assume that we should accept the notion that "anything goes." Even advocates who are more ardent about embracing one's own subjectivity have emphasized the "taming" of subjectivity. Peshkin (1988), for example, in exploring ways of embracing one's subjectivity suggests:

> Untamed subjectivity mutes the emic voice. Further, knowing that I am disposed to see—and, no less consequential, not see—in the particular ways directed by each of the six I's. I can consciously attend to the orientations that will shape what I see and what I make of what I see. By this consciousness I can possibly escape the thwarting biases that subjectivity engenders, while attaining the singular perspective its special persuasions promise. (p. 21)

Guba and Lincoln (1985, 1989) and several constructivists in arguing for trustworthiness have moved toward what Reason and Rowan (1981) called "a rigor of softness," an "objective subjectivity," or what Cook (1983) refers to as "critical intersubjective verifiability across heterogeneous perspectives."

As I delved further into issues of subjectivity, I came to realize that researchers intent on addressing their subjectivity had established some specific guidelines. Essentially, they called for an approach which involved: (a) delving into data, not by imposing categories, but by allowing them to emerge; (b) detailing a rich description of the phenomena so that the reader can examine the data on their own terms; (c) using multiple data sources as a way of achieving corroboration; (d) embracing the complexities and nuances of context as well as the perspectives of the various participants; (e) exploring ways of knowing which afford different takes on the same data—a kind of dialectic or multilectic approach (Stanfield, 1985); and (f) examining one's own viewpoints and how they shift during the study.

More detailed discussions of guidelines can be found in Guba and Lincoln's "The

fourth generation of evaluation" and several articles by Lather (1986a & b, 1990, 1991a, 1991b), as well as Stake (1983), Mathison (1988), Firestone (1990), Feinberg (1983), Glesne (1990), Miles and Huberman (1984), Patton (1990), Smith (1988, 1990), and others.

The term "Multilectic" was drawn from Stanfield (1985). As he stated:

> Even internally, populations are simultaneously similar and different. Linear approaches, and even the more complex but still simplistic dialectic approach, to the world in the social sciences hinders conceptual multivariate analysis. We need to think in "multilectic" terms about how a diverse world with heterogeneous subparts originates and develops. (p. 411)

Stanfield (1985) states, in a footnote, that the "multilectic" refers to a multitude of opposites and their synthesis rather than the Hegelian/Marxist synthesis of two opposites.

What may or may not be so readily apparent is that these guidelines are intended to work together to foreground multiple and different takes or perspectives on the same data. Notions of intersubjectivity and competing/juxtaposed interpretations are embraced rather than screened out or viewed as limitations. Interpretivists, especially post-structuralists, might pursue various perspectives as a check on their data and to foreground alternative interpretations.

Now, if we were to think about our own work or return to the two studies (Alvermann & Hayes, and Dillon) or some other examples, how might we fare? In terms of my own work, I was somewhat heartened by the fact that I felt as if my procedures met some of these guidelines. In essence, I strive for rich descriptions, allow categories to emerge and pursue various cross-validation of my data by enlisting multiple measures, including measures representing the perspectives of those involved. However, the manner and degree to which I incorporated the perspectives of others has been limited. I tended to use some input from participants as a kind of member checking en route to seeking convergencies in my data. What I found revealing was the extent to which I tended to pursue convergence rather than divergence.

As I contemplated the Alvermann and Hayes (1989) and Dillon (1989) studies, I suspected that the former (i.e., Alvermann & Hayes, 1989) would display similar attributes and shortcomings to my own work. Since Dillon (1989) had used micro-ethnographic methods, I anticipated that her study would address aspects of subjectivity more fully. My speculations underestimated what both Dillon (1989) and Alvermann and Hayes (1989) had done (and, in turn, made me realize the extent to which I had overestimated my own view of subjectivity and how it might apply to my work).

Both studies offered different takes on the same data as well as shifts that had occurred in their own thinking. They incorporated ongoing interviews with the teacher(s) and examinations and critiques of their own thinking. Moreover, these interviews were integral to achieving checks on what was being espoused as well as somewhat bringing to the forefront alternative interpretations—much more so than I had done in my own studies. Perhaps a limitation of their studies was the involvement of the students. Although the involvement of the teachers was not muted, the students were not involved in offering their views of the research. In terms of the extent to which the studies pursued a dialectic or a multilectic, there was more of a dialectic across the two studies achieved than within the studies. Despite the extent to which variation

across and within teachers was made apparent, each paper exhibited attempts to pin down findings to single possible interpretations.

On Matters of Knowledge Accumulation, Generalizability and Application

How do these notions of subjectivity, description and relativity relate to the accumulation of knowledge and the generalizability and application of findings? Are we ever expected to progress or accrue understandings if research findings are so tied to a context and the researcher himself or herself? Could we adopt a view of knowing that capitalizes upon the notion of complementary or dialectal/multilectic approaches to knowing? Although Roller's (1991) question "How will we ever know?" appears to suggest a concern over rigor, it also gets to a basic ontological concern: To what extent are findings generalizable? To what extent are findings situation specific as well as researcher specific? To what extent should findings be viewed as fleeting?

There does not seem to be a single answer nor simple answer to this issue. Issues of generalization are complicated by the acceptance or non-acceptance of relativism, by the recognition of complexity, the sensitivity to uniqueness and myriad of concerns associated with the various moralities/ideologies and preferred rhetoric of those pursuing constructivist or interpretative research. For example, just as there are different ways individuals pursue and report constructivist research, their are different views of the purpose of the research, of the relationship of the research to potential readers, and of the projections of how the research might be generalized or applied. Indeed, one could perceive the logical extension of proceeding relativistically/constructively would be to approach each reading of each study as a somewhat separate instance of knowledge construction; the act of reading a research article becomes an act of research.

As I struggled with positing guidelines for making generalizations, I reflected back over my reading of selected articles in recent weeks. I had a kind of epiphany that there is no single ideal reading of research. My reading of research was not simply a matter of checking the research against a set of standards and against a template that would make clear its contribution. In other words, my reading failed to afford either a clear-cut analysis of what might be deemed usable or an algorithm for applying these descriptions or interpretations to other research.

For example, I could describe my reading of Alvermann and Hayes (1989) and my subsequent reflections as what, for want of a better term, I would refer to as "real reading." My "real reading" was akin to the descriptions of reading offered in sociocognitive accounts of reading, as well as by schema theorists and literary theorists.

As I read the Alvermann and Hayes (1989) paper, I was struck by the extent to which I was not simply taking findings and conclusions as the authors may have intended them and transporting them to other contexts; instead, I found myself focusing upon different facets of the research at different points in accordance with a multilayered and rather complex view of their research—especially its relation to other similar research. For example, the study was memorable in terms of my current presentation. I suspected that I would revisit the research for purposes of my planning ways to involve teachers in future studies. I found myself fitting their work into other

work on classroom discussions with which I was familiar. At the same time, I paid attention to my understanding of Alvermann and Hayes' (1989) claims—especially across the different case studies. Furthermore, since my second reading of the article followed my encounter with Roller's (1991) critique, I spent a considerable amount of time delving into the rhetorical nuances of the article, such as how they addressed aspects of subjectivity, knowledge claims, and so on.

Throughout my reading of their work I felt that I was involved in a complex transaction with my own predispositions, background, and developing understanding of their work. In terms of predispositions, I did have certain expectations that emerged regarding their treatment of the previous literature and the way that they couched their questions, defined their data collection procedures, findings, and interpretations. The expectations that I applied were similar to those Grice (1975) specified in the Cooperative principle. In particular, I expected that their research should be: (a) multilectic or allowing for multiple perspectives; (b) open-ended rather than definitive; (c) reflexive or self-examining and self-effacing rather than self-indulgent or self-denying; (d) collaborative or joint venturing; (e) tempered and thoughtful; (f) problematizing rather than artificially neatening; (g) nurturing, generative, and robust rather than restrictive, preconceived, and selective. It is important to note that the contract was also reciprocal. I felt as if there were certain expectations in place for myself as the reader of their study: a willingness to entertain multiple perspectives, to be open-ended, relativistic and address complexities. The terms of this contract, therefore, connected the author's assumptions, claims, and ideas *to* my expectations of these texts and their potential.

Returning to the larger epistemological issue at hand: How will we know? I find myself wanting to ask those who ask such questions to reflect upon: What are you looking for? What are your expectations? Different readers will encounter different ideas in the same text and these ideas are apt to contribute in different ways to knowledge that is complexly configured. Ideas spurred by a single piece of research can be subsumed in existing schema in different ways (see Donmoyer, 1990). These ideas are not isolated from one another nor are the contexts within which they are presented and read. Sometimes their relationship to existing schema and their contribution to other contexts can be subtle; at other times their influence is readily apparent. As I contemplated the types of knowledge that I perceive counts, I realized that I may have been previously held hostage by views of knowledge accumulation (including generalization) that have their roots in a view that phenomena are simple and knowable and by the view that knowledge can be transformed in mechanistic ways.

This shift in how I have come to view the contribution of research to the accumulation of knowledge is both liberating and demanding. On the one hand, it sanctions different ways of knowing using terms of negotiation that might be more befitting the complexities of literacy, teaching, learning, and more current understandings of knowledge acquisition. I suspect that these ways of knowing are more in line with a cognitive and situated views of information processing within the realm of ill-structured domains. On the other hand, we have not addressed in any substantial fashion how the shift to relativism, may challenge our attempts to pursue research syntheses and to apply research findings. In terms of the former, I suspect that approaches to synthesis which ignore issues of complexities, ill-structuredness, and

multiple subjectivities may be viewed as problematic. In terms of dealing with applying research findings, I suspect that we may need to consider the various ways research findings might inform us, and study the possible pitfalls that occur in misappropriating research findings. This is consistent with arguments being made on several fronts by people beginning to grapple with issues normally labeled external validity [see Campbell (1986, 1988), Cronbach (1980), House (1991), and others) and also to research dealing with cognitive flexibility and the development of understandings from cases [see Brown (1989), Spiro et al. (1987)].

Perhaps our stance toward knowledge accumulation, claims, and use should assume more of a combination of the forms suggested by Eisner (1988, 1992) and Guba and Lincoln (1989). As Eisner (1992) has argued:

> Knowledge accumulation is not building with Legos . . . what may be said to occur in the doing of research is the creation of resources that those who know of their existence can use to think about the situations in which they are interested. . . . This view regards as mistaken the expectation that there can be a single theory that unifies the social sciences or a single language that will do what a multiplicity of tongues cannot achieve. . . . connections have to be built by the reader of the research and generalizations made by analogy and extrapolation, not by a water tight logic applied to a common language. Problems in the social sciences are more complex than putting the pieces of a puzzle together to create a single, unified picture. (pp. 27–28).

Moreover, as Guba and Lincoln (1989) have suggested, the shift toward accepting the inevitability of relativism and the complexities across different settings may require the ongoing, ecumenical, and recursive pursuit of shared possibilities rather than a single set of absolute truths. As they stated:

> *One consequence of this model is that parochial absolutism yields to ecumenical relativism. . . .* The world is no longer viewed as a closed system operating by immutable laws, which, once discovered, lay an inescapable mandate for behavior on us all. No longer can the discoverers and the manipulators of the law be viewed as a special priesthood. Instead, *the world becomes relative, with a variety of views not only tolerated but sanctioned.* There is no special priesthood, only groups who, by virtue of the knowledge that they have acquired and their sophistication in using it, are entitled to have constructions respected. Everyone is empowered in the process. The implication is that scientists (i.e. inquirers, evaluators) cannot stand aside from the constructions as though Nature herself had handed down "findings." Scientists, especially social scientists, must take as much responsibility as stakeholders and participants in the manner in which their constructions are used—or misused—unable to hide behind the cloak of "science."

> *Let us note again that this conclusion does not mean the "anything goes." The opposite of absolutism is not anarchy.* And it is not the case being relativist means that one has no power to criticize ideas or constructions. *All ideas must be subjected to critique within some framework. All that taking a position of relativism implies is that the framework within which any critique of ideas occurs is relative and may change from context to context, given the particulars of that context.*

> The second consequence . . . is *accountability yields to shared responsibility and . . . a "mutual simultaneous shaping" view of the world.* (pp. 256–257; emphasis added)

On Matters of the Art of Science or Science of Art

In coming to grips with alternative ways of knowing, I suspect that I am as cautious as the next person. Nonetheless, I hope that we can embrace, rather than recoil from, these new ways of knowing. I suspect that we need to be willing to take more risks in terms of how we pursue and report research.

Some of you may say I am already behind the times. Others might consider some of the shifts that I am advocating problematic—questionable in terms of rigor, and perhaps, more art than science. Yes. In some ways what I have shared is behind the times. A great deal has been written on pursuing alternative ways of knowing and there are a number of examples of research in our field done along these lines.

As I reconcile my timidity and concern for rigor with risk taking and the possibility of new ways of seeing, I have found the world of art illuminating as a way of beginning to understand new ways of knowing and their various and sundry ramifications. In particular, as I have contemplated the shift toward postpositivism in educational research, I have found myself reflecting upon developments in art—especially the shifts that occurred a century ago away from naturalism and representational art to modernism, post-modernism, and expressionism.

I was reminded of the criticism Edvard Munch received for his break away from clearly representational art to expressionism and subjectivity, how he was criticized for the essence of what he was trying to embrace—his own vision. Just as those in art have struggled with defining what art is, particularly in terms of the relationship between the artist's vision, the media, and the public, so I sensed research needed to come to grips with itself as art had already. It is as if research must come to face with its artistic underpinnings, not unlike the arguments that Eisner (1988) [rooted as he is in Cassirer (1945, 1957), Langer (1942, 1967), and Dewey (1934)] have been making:

> Art gives a richer more vivid and colorful image of reality, and a more profound insight into its formal structure. It is characteristic of the nature of man that he is not limited to one specific and single approach to reality but can choose his point of view and so pass from one aspect of things to another. (pp. 16–17)

> The artistic treatment of form provides what Langer calls a nondiscurvise form of knowledge. It is knowledge of the forms of feeling, secured by virtue of the way the artist had employed a technique to treat a medium so that it will have an effect upon those competent to read its message. *The need for such messages, I am arguing, is as important in the conduct of educational research as in those forms that have for so long dominated our conception of how we are to go about our work.* (p. 17; emphasis added)

On first glance, relating research to art may seem problematic. Indeed, I have found myself twisting and turning as I have considered the parallels between artistic endeavors and research. Just as art is always iconic and framed, so research is always framed. Just as art has accepted a kind of "here and now" vision of itself and is viewed largely unto itself as well as a record of something else, so research might begin to be viewed on similar terms. Some of the differences that exist between how art is viewed and research has me wondering at times whether art has advanced further

than research as a program of science or at least as ways of exploring alternative ways of seeing. Certainly, developments in art over the past century were achieved as individuals explored alternative ways of knowing at great risk, sacrifice, passion, and discipline.

Finally, does this perspective threaten to compromise the rigor of the research? I do not believe so. Instead, the possibility of more fully embracing postpositivism will likely cause us to be more demanding. In conjunction with engaging in the pursuit of specific research studies, post-positivists have developed a number of guidelines for dealing with subjectivity, achieving different perspectives of the same events, and embracing rather than recoiling from new ways of knowing. Notably one is *not* let off the hook with respect to rigor when pursuing a variety of analyses to peel back what might occur in the readers' subtexts and what may occur over time and in different settings. Nor are comparisons and interventions excluded from this approach to research. What it does not assume is that comparisons across contexts are clear-cut nor does it ignore the artistry of the research or the perspectives of those involved and the methods used to allow such perspectives to emerge.

On Matters of Ethics in Research

The shift to post-positivism has brought to the fore some issues that I have been guilty of neglecting, whether by benign neglect, lack of awareness, or having ulterior motives. It has brought to the fore the necessity to come to grips with some of the goals and values I hold for research. They include: reconciling with relativism and constructivism versus a search for absolute truths; moving away from simple-minded views of transfer with somewhat behavioristic underpinnings; confronting if not embracing my own subjectivity as well as the perspectives of others rather than sidestepping, dispelling or confining them; reconsidering the notion of generalization and knowledge accumulation; and finally, rethinking the purposes of research and the relationships which are established between the various parties involved in research.

I would like to end my paper with a brief discussion of this last issue. Too often, research treats the individuals as objects—subjects bearing little relationships to one work other than being sources of data or informants. I suspect that human subject review procedures intended to protect subjects should be relabelled Guidelines for Protecting Researchers from Demands of Human Subjects. In my opinion, the language of the guidelines is tilted more toward the interests of the researcher than toward the researched. Consider the language of sections of The Ohio State University guidelines (1992):

> The risks to subjects must be acceptable when measured against possible benefit to him/her or by the importance of the knowledge to be gained as a result of participation. (p. 1)

Moreover, among the activities exempted from review are:

1. Research conducted in established or commonly accepted educational settings, involving normal educational practices, such as:
   a. research on regular and special education instructional strategies

   b. research on the effectiveness of or comparison among instructional techniques, curricular or classroom management methods
2. Research involving the use of educational tests. (p. 1)

As I contemplated the ramifications of these notions for research, I found myself considering some of the arguments made by various groups: individuals such as Apple (1982), Foucault (1980), Giroux (1983), McLaren (1989), Soltis (1989), Morgan (1983), and Freire (1985) arguing for an emancipatory orientation to research; others such as Guba and Lincoln (1989) and Lather (1986a) suggesting as axioms for research the notion of catalytic validity directed at engaging the research participants as actively involved co-investigators; House's (1991) arguments for scientific realism in which he argues for directing research at real problems with real people; and finally, Gage's (1989) call for a pragmatic approach to research—a more direct approach for dealing with various ways to improve teaching and learning. As for myself, I realize the need for a more intimate and reciprocal relationship among researchers, teachers and students. As Oakley (1981) has suggested, ''intimacy without reciprocity'' (p. 49) is impossible.

In a number of ways, this is an issue of ethics. As I contemplate reorienting my research from my somewhat selfish and self-serving interests to those of the students and teachers that I do hope my research will help, I find myself asking a simple question of value: Couldn't I pursue the questions that I am interested in exploring in a manner which helps teachers and students? Indeed, what right do I have to pursue changing someone without their full understanding of my goals and some stake in what might be the returns? Couldn't I approach almost any research study collaboratively or in their interest without compromising the value of the research? In those situations in which I am examining their behavior or responses, shouldn't they be party to the interpretations that are assigned to their performance? Even when I have been involved in studies which explore issues apart from the teacher's everyday teaching, I have found that they are usually interested and willing to indulge my pursuit. Yet, I usually avoid letting them have a stake in what I am doing and, instead, offer teachers and students promises of results, anonymity or delayed gratification in the form of a thank you. If they are given a stake, I usually want to define rather than negotiate the terms. I suspect that the cost far outweighed the benefits. Once a collaborative relationship is established which affords teachers and students a stake as well as voice in the research, then I suspect that there is a host of advantages which might be accrued from such research: (a) multiple perspectives are apt to be more forthcoming; (b) relevance and complexities will be more apparent and problems more visible; (c) teachers and students will likely feel empowered; (d) researchers, teachers, and students will feel more interconnected and united; and (e) mutual respect and joint negotiation is apt to contribute to commitment and investment rather than detachment and divestment. I suspect that my intent to exclude teachers and students who served as my subjects was tied to the view that their involvement would likely intrude upon the data and affect my ability to achieve a kind of neutrality. In retrospect, my subjects were never neutral; I simply oftentimes chose to keep them uninformed and prone to generate their own view of what I was doing. Essentially, I suspect that my neutrality kept me uninformed.

Some Closing Remarks

Some may perceive this paper as ruminations of someone shifting toward constructivism and interpretist research with some leanings toward a mix of deconstruction or post-structuralism and critical theory. I would say yes, no, and maybe. I might argue that the interpretivists did not invent notions of reflexivity, rich description, complexity, subjectivity, multilectic inquiry, and so on. Indeed, I suspect that I and most educational researchers have been wrestling with many of these issues as we have dealt with the complex pragmatics of doing research. Arguably, I might be able to show you evidence of my attempts (albeit often frustrated and underdeveloped) to offer triangulated and thick description, multiple perspectives, a semblance of reflexivity, and to problematize generalizations and the application of findings. In addition, I have pursued various ways of involving teachers and students in my research as co-investigators.

So why the hoop-la? Because, in the past, my attempts to deal with these issues tended to be limited or underdeveloped. Work in the post-positivistic tradition of the last 15 years has brought these issues to the forefront—giving me a new understanding of their nature and role; challenging me to address and embrace these complexities further; and offering me the theoretical support and methodological illustrations to pursue possibilities that previously seemed outside of my reach. Put simply, I believe the ongoing work in this area makes an important contribution to how we pursue research, how we view the goals, approach, and understandings or findings that are suggested or emerge from research. I suspect a willingness to examine and more fully address these issues is long overdue.

In a number of ways the offerings of post-positivism remind me of key events in the early European settlement of Australia involving three explorers (Blaxland, Wentworth, and Lawson). In the early 1800s, just after the first European settlement in Australia, there were several attempts to cross what was called the Great Dividing Range (known to some of you travelers as the Blue Mountains) which separated the eastern settlements along the coast of Australia from the plains to the west. For years a route over the mountains was not forthcoming as people approached this task looking to the valleys as a way across the range. They concluded that the mountains were impenetrable. What they did not count upon was the fact that these were plateaus disguising as mountains. It was not until Blaxland, Wentworth, and Lawson adopted a different orientation, equipped themselves somewhat differently, and pursued a course along the ridges that they were able to make the crossing. Like these early explorers we do not have detailed maps for or an understanding of the topography of our new forms of research.

Whether or not you might agree with my view of the contribution of these paradigm shifts or the specific points that I have raised, I hope that you agree with my intent to put a mirror up whereby we can begin to study our research enterprise. As an educator/researcher these are issues I find that I am struggling with and find unavoidable. I do not view backing away from them, or having them roll over me, as a politically or ethically tenable stance.

I suspect that how we deal with these issues is likely to define our next several years of literacy research and practice. Moreover, I am also mindful that one might become so engrossed in the interrogation of their research that they might be hesitant to venture beyond theorizing. I hope that my paper will contribute to conversations about research not in terms of addressing what their research should be, but what it could be and will become.

An Afterword

Following my address several individuals confronted me. A number of researchers, with reputations for publishing studies that might be identified as "experimental" or "quantitative," were concerned that my comments would license less rigorous research. Some argued that literacy research can and should be done using techniques that they associated with the "hard sciences." I disagreed and argued that we needed to address the complexities of literacy research as we know it. Certainly, I was raising questions about tenets that had governed literacy research especially on the teaching and learning of literacy. But, I was not arguing for less, but more rigor. They did not seem to disagree.

Several colleagues questioned whether my portrayal of most literacy research as positivistic was warranted. I argued that I was not wanting to perpetuate stereotyping, but was concerned with the rhetoric of research regardless of the paradigm to which allegiance was being declared. In defense of their rhetoric, they emphasized that their claims did not exceed their data. In response, I reiterated my concern that most researchers pursued convergence rather than divergence. Moreover, the rhetoric of our research articles tended to suggest absolutes. They did not seem to disagree that the consumers of their research may overstate or overgeneralize the findings of research. In turn, I raised the questions: Should we persist with a rhetoric of reporting research studies which perpetuates such reactions? Should we explore alternative methods of interpreting research—approaches that bring to the forefront the possibility of alternative interpretations?

I do not feel as if I am alone in expressing a concern over our rhetoric. As Lincoln (1990) espoused: "The language of science . . . is itself a model of detachment and presumed objectivity" (p. 85). Or as Popkewitz (1984) noted:

> We can think of social science as dialects of language which provide heuristic fictions
> for supposing the world is this way or that way. These fictions or theories are made
> to seem neutral by the conventions of science which decontextualizes language and
> makes knowledge seem transcendent. (pp. vii–viii)

What seemed obvious was that some researchers were feeling as if they were being alienated by the rhetoric of those engaged in discussions of qualitative research. Over the course of the conference, I became astutely aware of the various and sundry ways that this may be occurring. I felt as if the tendency to use terms such as traditional and mainstream were actually marginalizing those researchers enlisting conventions usually associated with quasi-experimental studies. I concur with Gage (1989) and Guba (1990) that if we are to move ahead in our field we need cooperation rather than competition, diversity rather than uniformity.

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