

## **On The Nature Of Qualitative Evidence**

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Persuasion by our art is always linked with our science—in the exhaustiveness of our research, a display of our control over not only our discipline but of the disciplines on the edge of ours, an openness about the degrees of our certainties and uncertainties, a show of our adversarial skills.

Greg Denning, “Performing on the Beaches of the Mind,” 2002, p. 6

...no one learns the truth by being told it. Everyone needs to experience truth in some way.

Greg Denning, *op. cit.*, p. 8

A paradigm is coexistent with the rules of seeing and rules of measuring and thinking that give it its physical objects and scientific laws. Different natural objects are precipitated by different paradigms. What may be true in one, may be false in another.

H. M. Collins, 1983, p. 90

## **Introduction**

When these papers were commissioned, the task was to consider “the nature of evidence” and a “discussion of where we are at now from various perspectives.” This was not framed to be a debate between those who do qualitative research and those who do quantitative research, but rather as a way of considering the rules of evidence associated with very different forms of knowledge. As Berger observed not too long ago:

When it comes to questions that have some measure of controversy surrounding them, our field’s active researchers generally seem to be occupied chronically with arguing about the relative merits of various methodological approaches...These continuing methodological debates involve such polarities as qualitative versus quantitative, self-report versus observations, structured versus unstructured, laboratory versus field, cross-sectional versus longitudinal, and so on. In general, these debates have done little to advance the study of ...[education]... because they have taken place within a substantive theoretical vacuum. (1994, p. 11)

Having become associated with qualitative methods and having taught in this arena for many years, I have seen how indistinct, shapeless, and soft the ways are in which untrained individuals view qualitative research. Sharpening up the understandings, providing them with some contours, trying to show how one justifies evidence in phenomenological research, and firming the distinctions between the kinds of qualitative evidence is therefore a useful project.

The topic is complex not only because it involves taxonomizing, but because it is deeply implicated in considerations of validity, verification, the politics of knowledge, and what we consider not only “evidence” itself, but also what we consider “knowledge,” and what knowledge we consider worth having. One insight from trying to organize this paper has been, for instance, that many of the arguments over what we consider validity (Lincoln, 2001) are themselves questions regarding the nature of evidence and what constitutes knowledge. I have begun to see that, in fact, much of what we call “data” is itself phenomenological—that is, socially constructed, and “there” only because we are attuned to looking for it. Not only socially constructed but in fact, little more than social construction, an artifact of social and communitarian processes among disciplinary members who agree among themselves about how they will talk, and what the rules of talking will be (Brady, 2002). This holds true whether the data and evidence are qualitative or quantitative (Knorr-Cetina, 1981).

When I’ve written about this previously, I’ve approached it from the opposite site—usually, evaluation, and what we should consider evaluative evidence. But that has been so site-specific that it missed the *theory* altogether, that is, the socially-constructed parameters of what we consider evidence to be, and what suits our postmodern minds about definitions of evidence. There are four questions to be considered when justifying and evaluating evidence in qualitative

research. What do we mean by qualitative evidence? What weight do we give to the validity and rigor of that evidence? What are the assumptions behind different forms of evidence? And, what are the “politics” of evidence? Those four questions will structure the remainder of the discussion here.

### **What Do We Mean By Qualitative Evidence?**

First and foremost, no “evidence” is evidence until we see it from some theoretical, paradigmatic or metaphysical framework (Fisher, 1977). As Collins (1983, p. 90) observed in the quotation which appears in the beginning of this paper, “different natural objects are precipitated by different paradigms. What may be true in one, may be false in another.” Berger (1994) echoes this assertion by observing that “the general proposition suggested...is: *No evidence without explicit theory*” ( p. 13, emphases in the original). Thus, what constitutes evidence, and therefore, what justifies it, is the result not only of what questions are posed, but of the framework within which they are posed. The metaphysical paradigm with which one begins implies a preordinate sense of what “evidence” is, of where it might be obtained, of how it might be collected, and a set of implicit and explicit rules for judging how good (rigorous, thorough, “grounded”) those evidences might be (Fisher, 1977). The paradigm, or metaphysical position, of the researcher also has embedded in it the regulations for condensing, re-arraying and making plausible the arguments which are made, the implications teased out, and the conclusions drawn. As a result, before qualitative evidence is judged, we have to know the metaphysical framework—the paradigm—within which it was originally collected, analyzed and presented. It is not enough to say the data are qualitative; we have to know what the inquirer means by “qualitative.” Paradigms count, in more ways than one.

That having been said, a second point is important. Qualitative evidence and qualitative data are not necessarily the same thing. Although data and evidence are often used interchangeably to mean the same thing, they do not necessarily mean the same thing. Data may be merely information. Evidence, however, is data brought to bear on specific questions, theories or experiences. Evidence is data with a purpose. The purpose may be historical, theoretical, evaluative or descriptive, but it is arrayed to some larger end, and it is arrayed in a specific kind of order. Evidence represents data to which have been added a layer—or multiple layers—of interpretation and rhetorical strategy.

Thus, data and evidence may be dealt with in two separate ways. Qualitative *data* is any form of empirical material, observable “traces,” or observation which is largely non-quantitative. Into the category of empirical materials would fall, for instance, letters, memoranda, requests for proposals, interview notes, tape transcripts, formal records (such as official school board minutes), films, photographs, teachers’ journals, teaching or research portfolios, newspaper articles, material traces. In short, many forms of documents, official records, and other material objects—children’s artwork, photographs, architectural drawings—belong in the empirical materials category.

“Trace” observations are those forms of evidence which Webb, Sechrest and their colleagues (1966) called “unobtrusive measures”—that is, they were neither left for the purposes of research, nor were they deliberately generated by inquirers for research purposes in ways which create reactivity. Rather, they simply exist. A classic example of the use of unobtrusive measures is the way in which sidewalks around elementary schools are generally constructed. Sidewalks are rarely poured until architects and planners can assess the “natural” paths that schoolchildren take up to and around the building as they enter and leave school. We might also

consider, for example, the kinds of trash thrown out as graduating seniors leave college immediately after the last ceremonies. Many times, the dumpsters begin to fill with spiral-bound notebooks, and other forms of classnotes; they become a symbol for some students of leaving behind, either temporarily or permanently, a formal and routinized “learning life.” Another such measure would be the number, selection and quantity of “used books” returned for cash to local bookstores upon graduation. Such measures as what is tossed into the trash, or what is sold for hard cash, are unobtrusive, in the sense that they are frequently anonymous, do not require “consent” to assess, and can provide some discipline-by-discipline “measure” of what is kept and what is returned or discarded as no longer useful.

Observations are equally useful data, and can be collected in virtually any public or quasi-public setting without fear of violation of individuals’ rights to either privacy or informed consent. All of these things—empirical materials, material traces, and observations—fall into the category of “data.” That is, they can be collected, subjected to analyses, aggregated and disaggregated, rearranged, compressed, and rearrayed in order to advance arguments and insights regarding the nature of social life.

Data, however, is not evidence until two things happen: first, someone recognizes it as data, and second, an inquirer subjects it to some form of systematic analysis, which turns it into evidence directed toward some question or argument. As Collins (1983) observes

Different *natural objects are precipitated by different paradigms*. What may be true in one, may be false in another....

...our giving of meaning to objects—*our interpretive practices*—are so automatic that we do not notice that any interpretation is involved. We tend to think that what we see must always have been what we see it *as*. That which we bring to the act of seeing is invisible and this gives the meaning of things an ‘external’ quality. In the sciences the apparent externality of things seen is celebrated and reinforced by the notion that anyone

would see the same things if they looked in the same place. If anyone would see the same thing, then its sameness must be a product of *it*, rather than a product of the see-er. (p. 90, first and second emphases added; third and fourth emphases in the original).

What Collins argues here, that a set of invisible rules in part determine what constitutes evidence, demonstrates quite well the argument that what evidence is, and what constitutes evidence is determined in large part by the community to which one belongs, and the paradigm—or what counts, in a metaphysical sense, as knowledge—within that community.

Qualitative data generally fall into two categories. I would call those categories *physical data* and *social constructivist, or constructionist data* (social constructionist would be the term which Gergen (1994) utilizes; either term is correct). Each of these kinds of data has its own uses to which it might be put, and its own system of verification and validation. It is in the verification and validation process where many individuals part company with the use of qualitative data in social science research, and so I shall discuss later verification and validation processes in the two categories. The most difficult problem non-phenomenological researchers seem to have is with what are generally called the “socially constructed” data. These are data extracted from the meaning-imputing, sensemaking processes in which humans as social beings engage in continuous fashion. They are often data from interviews, from documents, diaries, observations, and other frequently non-quantifiable sources.

These are data which are considered by hardline conventional researchers as anecdotal, subjective, and given to multiple interpretations. These words are highly coded discourses for non-rigorous, unsystematic, and therefore, invalid. Phenomenologically-oriented researchers, however, assume that since humans are meaning-making, meaning-ascribing creatures, knowing about how meaning is created, interwoven, threaded through with other, long-held meanings, and

connected to values, beliefs, attitudes, facts, and “purposeful, constructive, and self-sustaining actions” as well as “situated and practical knowledge” (Ray and Mayan, 2001, p. 66), and “personal experience” (Fitch, 1994), is an important activity for understanding human behavior. Consequently, qualitative researchers take as a primary source of both data and evidence the accounts of meaning-making activities of individuals and groups.

### **What Weight—What Validity and What Rigor—Do We Assign to Different Forms of Evidence?**

One of the sharpest and most cruel realizations of the postmodern critique has been that all knowledge is partial, incomplete, standpoint-determined, and therefore, suspect in its claims to universality. The *partialness* of social accounts, as a result, render its truth claims partial likewise. A second painful realization stemming from the postmodern critique has been the growing understanding that some knowledges, like the famous pigs in Orwell’s *Animal Farm*, are more equal than others. Hawes points out that “Foucault’s...genealogical work aims at elaborating and crediting minor, marginal knowledges and practices in order to multiple knowledge and experience rather than unify it” (Hawes, 1994, p. 7), and that therefore, the “task [of both the researcher and qualitative research] is to multiply particular accounts and knowledges, not to unify an abstract and supposedly universal Knowledge” (*Ibid.*, p. 10).

In the process of multiplying “particular accounts and knowledges,” we have come to understand that accounts, voices, stories, and some significant aspects of social life are missing. Those accounts are now being rendered by voices which are gendered, colored, bi- and multi-lingual, bordered, previously colonized, and otherwise marginalized. What we have given up in



an assumed universality—and therefore, generalizability—we have gained in richness, texture, flavor, vicarious experience and deeper understanding of lives lived differently from our own.

In pockets of radical positivism, the criticism is still voiced that qualitative data is serviceable if one is operating strictly in a discovery mode, but of course, such data have little “validity.” As a consequence, the issue of validity in qualitative evidence has taken on the quality of heavy freight. Within the constructivist community, many proposals have been forwarded for forms and types of validity which are appropriate to qualitative data (Lincoln, 2000) and some engaged in the philosophical hermeneutics of Gadamer (1970) and others have called for an end to the cult of “criteriology” altogether (Schwandt, 1996) as a concern rooted in positivist philosophical positions, and unrelated to the more central question of rhetorical persuasion inherent in scientific reasoning and discourse. That is, as Wittgenstein might argue, language is a game. Change the language, and one changes the rules of the game. Thus, multiple languages, or discourses, circulate about what science is, what the nature of evidence is, and therefore, what we can consider, within any given scientific community, what is valid, reliable, and worthwhile of acceptance.

The entire question of validity, however, is far more complex than merely assigning new criteria to new methods (i.e., qualitative methods). While such proposals are useful *within their own communities of discourse*, they beg the prior questions of what we think “evidence” is, and to what purposes that evidence will be directed.

Since the issue with qualitative and other forms of postmodern research lies in its theoretical and metaphysical commitments, different criteria of rigor and relevance apply when judging the nature of evidence (Lincoln and Guba, 1985; Guba and Lincoln, 1989; Lincoln, 1995a,b; 2000). One way of viewing this issue is to argue, not incorrectly, that justifying

evidence in qualitative, and particularly postmodern qualitative, research, is less a matter of conventional criteria than of other considerations. For instance, Hawes (1994, p. 6) argues that “Today, I am more concerned about accessibility and accountability than I am about admissibility [of evidence]. Valid knowledges, it seems to me, have less to do with exclusivist, technical admissibility criteria than it does with inclusionist, accessible knowledges and experience.” In other words, Hawes is arguing for a recapture of knowledges which have been traditionally excluded from conventional inquiry, worrying about the technical considerations surrounding rigor far less than the importance of expanding the range of knowledge about social life available.

Others, however, do propose rules of evidence<sup>1</sup> that speak to the validity or rigor of such knowledge. Wilson, for instance, poses five criteria, including: that evidence “should be consistent with a researcher’s chosen epistemology or perspective”; that “evidence should be observable”; that “evidence should be gathered through systematic procedures”; that “evidence should be shared and made public”; and that “evidence should be compelling” (Wilson, 1994, pp. 26-30). Fitch, however, poses criteria in a somewhat different way. She poses two sets of circumstances for criteria: those which obtain for “qualitative data to be admissible as evidence for claims about social life,” and those “for a qualitative study to count *as* evidence” (Fitch, 1994, pp. 36-38).

Because the two sets of criteria fall between two camps, the radical positivist and the radical phenomenological, they are well worth spelling out here. Certainly they make sense for

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<sup>1</sup> For a fairly thorough review of the varieties of validities proposed for qualitative research, see Lincoln, 2000. While this review is far from complete, especially in light of work which has gone on since 1999, nevertheless, it provides a good spectrum of the ways in which validity (and rigor) has been treated, and, by implication, what various inquirers believe to be important aspects of qualitative research.

certain kinds of work. Criteria which are necessary to be admissible as evidence for claims about social life include the following:

1. researchers “should have been deeply involved and closely connected to the scene.” This corresponds highly with anthropological dicta regarding what constitutes sound fieldwork, i.e., persistent observation and prolonged engagement.
2. researchers “should achieve enough distance from the phenomenon” to permit recording action and interpretations relatively free of the researcher’s own stake. If researchers need to withdraw from the field in order to get some distance on what they have seen and heard, this is entirely consistent with practices reported throughout both anthropology and sociology. Peer debriefing is one way to achieve this “reality check.”
3. “Claims should be based on an adequate selection of the total corpus of data.” Indeed, the problems of selection of data, and bounding the case study, have been labeled far earlier as ethical problems as well as evidentiary problems (Lincoln and Guba, 1985; Guba and Lincoln, 1989).
4. “Data should come, at least partly, from publicly accessible observation records.” This criterion responds to the need to have available, through an inquiry audit or other form, some external review of the interpretations which are made, and the logic models and processes which account for those interpretations. While the logical processes should be clear and coherent within the research report, they are not always. This means that some data should be inspectable by others.

5. “Data and analysis should include consideration of inferences and interpretations, as well as concrete phenomena.” (Materials in quotation marks from Fitch, 1994, pp. 36.).

But two characteristics of qualitative work mark it also as evidence in and of itself: findings must be grounded in the situation examined, and if possible, comparisons will be created, and findings from one study should be “translatable to other studies, theories or problems” (*Ibid.*, p. 37). It is important to note that while Fitch strongly supports objectivity in the criteria which she elaborates, she does not call for generalizability, but rather for “translatability,” a criterion far easier to live with when we realize that while contexts might be similar, they rarely exhibit a one-to-one correspondence with each other.

The postmodern critique, however, intervenes in such a way as to create another problem with criteria. As Fitch puts it, “ In questioning the possibility of ‘objective’ examination of social life, it [the postmodern critique] challenges the notion of ‘evidence’ to its core. Almost anything is admissible as evidence. Candor about the researcher’s identity and role in the scene are highly valued kinds of evidence, things that are out of bounds (or beside the point) in traditional social science” (1994, p. 34). But there are, she points out, major issues with postmodernism and the criteria generated for post-rational work, concluding that the contribution of one form of rather positivist analysis (conversation analysis, or CA) is rigor, while the “contribution of postmodernism [is]...vigor” (*Ibid.*, p. 35). I would argue that it is possible to have both rigor and vigor in our qualitative studies, but that frequently, trade-offs will have to be made. In part, this is the far older conundrum of rigor vs. relevance which conventional researchers have faced for a century.

Postmodernism has, however, also made enormous contributions to our vision of what inquiry is all about. In addition to extending what we consider valid or worthwhile knowledges to possess, and where they might be derived, there is, equally, a kind of rebalancing of the power structure between researcher and researched in newer forms of research. Rather than treating research participants as mere repositories of data from which researchers can extract relevant bits and pieces (Oakley, 1981), researchers now view the relationship between inquirers and participants as far more democratic, mutual, and reciprocal. Hawes says, for instance, “For me, evidence, to be admissible, must be dialogical, or at least conversational” (1994, p. 7). The implications of a “dialogical,” “conversational,” or, for critical theorists, “dialectical” social science are not only anathema to some radical empiricists, but the implications are relatively new, fresh, and historically recent in arguments about what social science or educational research should look like.

In my tendency to agree with Hawes, I am assuming that the more dialogical the evidence, the likelier it is that multiple social constructions have been systematically collected, analyzed and included in any research report. Among other things, the presentation of multiple social constructions can point to the pressure points of pluralism in social life, and contradict the seamlessness of assumed cooperation and lack of conflict so often present in older works of social science.

### **What are the Assumptions Behind Different Forms of Evidence?**

Swanson observes that “the nature of these questions differs in quantitative and qualitative investigations” (2001, p. 75). Indeed, one of the foundational assumptions behind commitments to qualitative research overall is that some questions are answered elegantly within

some paradigms, and other questions—equally reasonable questions—appear foolish or unanswerable. The “other questions” which inquirers have, however, may, in the process of switching models of research, gain elegance, theoretical power, and scientific meaning. The question is always one of “fit”: does the model of research chosen “fit” the research questions of interest?

An important point to be made here is that we cannot assume that all realities are equally critical to all inquirers. Rather, all knowledges are forms of social constructions, albeit “constructed” under different sets of principles, different sets of assumptions, different rhetorical structures, different forms of reasoning. Scientific reasoning is not based in a set of realities or scientific objects which are mutually and agreeably sensed by all see-ers (Knorr-Cetina, 1981). Instead, a major conflict has occurred in the conflation of, and in the “distinction between the two (natural or technological sciences...and the social or cultural] sciences” (p. 136). Knorr-Cetina argues that

...this distinction between the two sciences has not been proposed by empirical studies of science, but has proliferated mainly within methodological discussions centred around the inadequacy of certain measurement orientations for the study of the social world. Such orientations were called positivistic, and identified with a model of scientific method set by the natural sciences. In fact, new rules of social science method have been developed, displayed and defended in a constant dispute over the standard provided by this model, and a departure from this standard has been the declared goal of an indigenous social methodology. (1981, pp. 136-7)

She proposes, however, “the question of whether the practice of natural and technological science can be distinguished from the symbolic, interpretative, “hermeneutic” practice of the social sciences, and of social science itself. I hold that it cannot.” Rather, she “...*underline[s]* the *essential similarity* between the two modes of the production of knowledge” (1981, p. 137,

first emphasis added; second emphasis the author's own). That is, all scientists are, in the profoundest practices of science—whether natural science or social science—both literary (i.e., rhetorical reasoners and “symbolic reasoners” (i.e., interpretive thinkers).

If this is the case, then it becomes critical to interrogate two questions. First, what constitutes evidence, or what will we consider to be an “object” of scientific curiosity? Second, by what methods shall we collect this evidence, and how shall it be subjected to analysis?

Cheree Carlson (1994) lays out three criteria for determining the answer to the first question. She observes that

A critical issue is evidence thus becomes how we should make use of it. In rhetorical criticism, the data does not sit quietly in a table or hum pleasantly in a statistical package. It wriggles and shape shifts, and refuses to be pinned down. The critic takes this stuff and weaves it into the pattern of an argument. The resulting fabric is judged, on the most basic level, by three criteria. (p. 21)

The three criteria are whether the data are “pertinent to one's critical perspective”; whether “the evidence selected [is] relevant to the claim,” and “has the critic done a credible job of persuading us that the best available data has been utilized” (*Ibid.*, p. 22). The second of Knorr-Cetina's questions embodies the question of method and forms of analysis. It is here that most of the arguments begin between quantitative and qualitative researchers, but it is, as I and others have pointed out, a largely useless argument, since the real question is what the “critical perspective” of the researcher is, whether or not that critical perspective is relevant and explicit (Berger, 1994), and what one takes as reasonable information on social and lived experience. The point is that different critical perspectives elicit different forms of rhetorical (i.e., literary or discursive) reasoning and symbolic (i.e., interpretive) reasoning. But all critical perspectives

engage in both processes, which in turn produces (“manufactures”) different forms of knowledge, assumed to be worthwhile for different kinds of purposes.

This leads to a final point about what we can say about the assumptions which undergird qualitative evidence, or any evidence, for that matter. The question is not which is “better” information, but rather, which kinds of evidence best address certain questions, and, at a foundational level, *which kinds of literary-rhetorical devices are being employed, and which kinds of symbolic-interpretive processes are being brought to bear in the mounting of a persuasive argument?*

### **The “Politics” of Evidence**

There is a “politics” of evidence. Beyond the questions of legitimacy, hegemony and reward structures at universities, there are larger questions which subsume mere issues of legitimacy. Three of those questions are whether or not science has a moral aspect; who determines what counts as evidence and who is persuaded by it; and what is the nature of the “language game” which is being played out in the politics of evidence?

*Science as a moral project.* Ruth Bleier once remarked that her response to individuals who claim to be unconscious of any bias in their research are not unbiased, but rather simply unconscious. Science is a uniquely human project, and therefore, riddled with human values, predilections, assumptions, and social positionings. The claim to be engaged in the pursuit of objective truth is, in itself, a moral stance, from which one might absolve oneself, as did Pontius Pilate, of anything the crowd might wish to do with the data. As Krieger asserts, “All decisions about science, from the choice of topics to the methods and interpretations of research are...political and moral decisions” (Krieger, in Morse, et al., 2001, p. 99; but see also Guba and



Lincoln, 1981, 1989; Lincoln and Guba, 1985; Schwandt, 1989, 2002). Each set of decisions made about the research process—research question, theoretical framework within which the question might be pursued, site for the research (natural setting vs. laboratory), methods, the ways values which inhere in the site are taken into account, final report form—all indicate value choices and consequently, some moral stance on the meaning and form of the research.

The major differences on whether or not there is a moral stance to inquiry lie once again in the rhetorical structure of the arguments and reasoning. To claim objectivity for one's research is to pointedly refuse to deal with its moral dimensions, and perhaps even to hide the root assumptions of the questions asked in such a way as to obscure the values undergirding the structure of the research project. One of the major breaks with classical science is the effort to make the claims, standpoints, values and belief systems that undergird researchers and their research explicit, transparent.

*Who nominates evidence for consideration?* Ray and Mayan (2001, p. 59) ask: "Who decides what counts as evidence? The real questions 'become 'Whose account is being privileged?' and 'What knowledge remains unprivileged, unworthy of being characterized as a relevant outcome?'" A second contribution of the postmodern critique has been the nomination of other forms of knowledge as privileged accounts. This is a far larger question than merely whether the data are quantitative or qualitative; rather, it revolves about the concern for what accounts have been omitted in creating pictures of lived experience. The answer, of course, first from the feminist theorists, then from the theorists of race, color and ethnicity, then from gender theorists, and now from postcolonial peoples, has been that accounts of all those *not* considered dominant majorities—that is, white, male, heterosexual, non-colonized—have been omitted from social science, and from our accounts of social life and experience.

*What is the nature of the language game that is played out in the politics of evidence?*

The ongoing debate about discourses is not a trivial debate between people who have little else to do, epistemologically. Language shapes and structures what can be seen, what is believed about what can be known, and the appropriate methods for gathering empirical materials and labeling them “evidence.” Ray and Mayan (2001) put it quite baldly and succinctly when they observe that

Enthusiasm for concept analysis..., meta-analysis..., and minimal data sets...serve to further structure and institutionalize the way researchers view their study population and frame their questions. These strategies do achieve focus, consistency, and the statistical power of large databases. However, conceptual and methodological conformity also *reproduces the status quo and dictates the boundaries of inquiry...Innovative or divergent conceptualizations are marginalized or excluded when conformity is the priority.* (p. 57, emphases added)

This is a larger issue than the ‘rigor versus relevance’ question explored in the foregoing sections. This is an issue of regulation and dominance in the pursuit of social science. In the Panopticon of discourse, alternative models of inquiry, divergent methods and/or variant or substituted explanations are simply not permitted expression.

Several good examples of the policing functions of discourse recently surfaced. In the report from the Committee on Scientific Principles for Educational Research, called *Scientific Research in Education* (2002), the current national administration has promulgated what will be “acceptable” and classified as “true scientific research” (and consequently, funded or considered for funding). Virtually all six of the “principles” call for a model of research which is conventional, and which has been the subject of ongoing criticism for over 30 years. While there is nothing inherently wrong with large databases, or conventional research, there is something

very, very wrong about a presidential task force which mandates that this is the only variety of research which will be funded or supported during its administration. Such a policy effectively silences alternative voices, divergent readings of social and educational life, and new constructions and solutions for persistent and intransigent educational problems.

A second chilling example of the policing functions of discourse lies in the behind-the-scenes “Department of Education initiative to remove from the Department’s web pages any references to non-Bush era education research.” (Corby, 2002, via e-mail, P. Lather, September 15, 2002). Kate Corby, the Education and Psychology Reference Librarian at Michigan State University, goes on to report that she was contacted by a reporter from *Education Week*, who wanted Ms. Corby’s opinion on the initiative, especially since

The [Education] Department maintains an extensive Web presence to assist students, parents, and teachers in their educational information needs...the Department has decided that information (and this is largely research results, not opinion papers) that does not support the current administration’s views should not be made readily available to the public. In addition to this change on the Department of Education’s site, [Ms. Corby] has heard from other sources that a move is under way to limit funding for the ERIC system, which is the other way that this information gets disseminated. (*Ibid.*)

If correct, these two examples, the latter especially, serve to illustrate the power of discourse to shape debate, and are potent and commanding examples of *Fahrenheit 451*<sup>2</sup>—the removal of books, literature and other forms of text for the purpose of controlling ideas.

Consequently, moves to open up discourse, to invent new forms and new languages with which to speak of lived experience—including experimental texts, and narrative, oral and

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<sup>2</sup> Ray Bradbury’s novel about burning of books because they contained “ideas” which only led to “more ideas,” and gave otherwise compliant citizens notions about what a government ought to look like. Savonarola, a 16<sup>th</sup> century monk, had books burned. Hitler had books burned. A county in West Virginia 15 years ago had books removed from school library shelves and burned.

performance formats—contribute to keeping educational research, whether qualitative or quantitative, an open and discovery-oriented form of human enterprise.

### **Conclusion**

When we begin to speak of evidence, at a minimum, we need to think about these four questions: What do we mean by evidence? How do we determine the validity and rigor of various forms of evidence? What kinds of assumptions lie behind different forms of evidence? And what are the politics of evidence? When we can outline answers to those questions, we will have defined our theoretical and assumptional base for conducting the kinds of research we do, and we will have justified and legitimated the choices we make for what qualifies as research or science.

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