

WHEREIN LIES THE SCIENTIFIC RHETORIC?

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Comments, criticisms, all out rejections (with explanation) welcome; but, please DO NOT QUOTE or cite without permission of author.

A Preface for Conferring

Before heading into the analysis I have prepared for today, I think I should make more explicit the grounds on which I have constructed this work. There are at least two aspects of the horizon of intellectual assumptions which lay behind my work that I would like to put up front: First, I should say a few advance words about the nature of epistemological assumptions in my argument -- and hence the overall status of epistemology in my work; and second I should be quite clear about the political strategy intended and the context in which this strategy developed.

A refusal of philosophy. Roughly speaking, it seems to me that within the Anglo-educational research community, over the past two decades or so, a number of ostensibly philosophical arguments have been brought forth to defend differing methodological stances arising in the production, distribution, and consumption of educational research. (Recently I have come to view these processes not in these rather conventional terms, but, following Deleuze and Guattari, more as the processes of the desiring-machine known as educational research.) Typically a central axis of these debates is that central worry of Modernity: the epistemological base of science -- or social science more specifically. Here I am thinking of the plethora of arguments circulating around the dichotomous terms "quantitative vs. qualitative," "objectivity vs subjectivity," and/or the more recent reincarnation "realism vs. constructivism."

My position on most of the questions raised by such debate I sometimes have described as one of double opposition -- mutually, if only partially, rejecting both sides of these dualistic oppositions. Fearing that my position might be read a some form a pragmatic triangulation, I have more recently come to blatantly position my work in the historical break between sociology and philosophy. Unlike Dewey, admittedly a familial American compatriot, I do not believe much return can be gained now from the investments made in addressing philosophical questions raised by methodological research concerns.

Hence I have come to inscribe science as a form of rhetoric which is associated with social fields in which the major principle of differentiation lies amongst the struggles over reason, rationality and truth. As Bourdieu points out,

"Truth is the stake in a series of struggles of every field. The scientific

field...has this peculiarity: you have a chance of success in it only if you

conform to the immanent laws of the field, that is, if you recognise truth

practically as a value and respect the methodological principles and canons

defining rationality at the moment under consideration, at the same time as bringing into battle in the competitive struggles all the specific instruments that have been accumulated in the course of prior struggles.

The scientific field is a game in which you have to arm yourself with reason in order to win."

Following the dispositions I find within the translated writings of Gaston Bachelard and Georges Canguilhem and his students (Foucault, Bourdieu, Deleuze, Guattari, et al.), about my philosophical agnosticism I would simply reiterate the words of Canguilhem and say "to each his own discontinuity, his own revolutions in the world of scholarship."

A differing political context. In addition to my lack of philosophical Ñngst, I would also like to make explicit the fact that I constructed this analysis as part of a relatively calculated political strategy in the social chasm that lies between two factions of US educational research. Using what are undoubtedly terms of convenient fictions, I would dub these factions the "radical" and the "mainstream." Without belabouring some unnecessary exposÇ on what the consciously recognised functions of this political strategy were, I would simply like to acknowledge that this conference represents a much different context than the one in which I generated this analysis. Hence, I do not presume that what I have to say would mean the same here as it may have there. If anything, in addition to adding what I think are some interesting observations to Australian conversations about educational research, it is my hope that reactions to this analysis will help me to better clarify the nature and politics of Australian educational research. Hence, any responses, questions, or rejections are welcomed (at least in some sense).

Set up remarks. Some notes to set the stage for this analysis:

OBJECT OF ANALYSIS: US Sociologies of School Knowledge (aka "my field")

-- known as discourses which make claims about the relationship between

societal

dimensions of power and school knowledge. Four discourses inscribed herein:

- Structural Neo-Marxist (e.g. Apple, Carnoy and Levin, early Giroux),
- Structural Feminist, (e.g., Grumet, Nihlen and Kelly)
- Post-Structural, (e.g. Wexler)
- Post-Structural Feminist. (e.g. Ellsworth, Lather)

OBJECTIVE OF ANALYSIS: Describe communication (lack) between this field and "the mainstream."

WHEREIN LIES THE SCIENTIFIC RHETORIC?

Certainly, Bourdieu has repeatedly employed war metaphors when speaking about scientific struggles over truth. And almost as certainly these metaphors would not be readily accepted by many radical educational thinkers who take offence at anything resembling war or violence. But aside from the connotations of Bourdieu's descriptions of the struggles which continually take place between intellectuals, (both within and outside those institutions commonly recognised as the Academy), there lies a recognition that not only are claims to knowledge and truth rarely uncontested within scientific fields, but that specifically scientific struggles are sociologically conducted through that historical construction called "reason."

For me, the intellectual consequences of this position for radical sociologies of school knowledge are as radically profound as they are obvious. To recognise scientific reason as a historical construction, and to recognise knowledge claims as a central capital over which intellectuals compete is to impose an epistemological break from philosophy. Reenacting the centuries old confrontation between science and philosophy, in this analysis, as one way to further assess discourses in my field, I attempt to describe on what ground radical sociologies of school knowledge engage in specifically scientific struggles.

Historically, it is probably no surprise that I raise this question here. At a time when intellectual debates question the end of philosophy, virtue and

Foucault, at a time when sociological theory contemplates giving up its scientific foundation, in short, at a time when the very notion of a "foundation" raises hair on the back of many academic necks, my question addresses on a micro-level issues that have been reverberating through the Academy for quite some time.

In this context I can not claim to have constructed an original approach to this task. There are many points of departure from which I have begun to address this question, some of which have not been brought to bear on my field. It is in this sense, as I specifically question the scientific authority of US radical sociologies of school knowledge through a sociological lens in this analysis, that I take my contribution to my field as unique.

Because this analysis specifically examines the discourses in my field as science, and because of the historical conflation of positivism and science in my field, I initiate this sociological reading from a point of departure chosen to emphasise its own understanding of science. That is, I begin this analysis by viewing science as a form of rhetoric. To question the modes of rhetoric in the discourses of my field, I then present a distinction between two forms of rhetoric: what I heuristically call a philosophic rhetoric of demonstration and a scientific rhetoric of evidence. Chosen to emphasise the ways in which these discourses have tended to rely mostly on philosophic discourse, and to question the structure of authority in their rhetorical mode, this distinction is admittedly and intentionally polemic.

Placing these discourses, once again, within a sociological reading, I then analyse how radical sociologies of school knowledge have disregarded or failed to meet three basic methodological canons of educational science: falsifiability, generalizability, and validity. (This does not imply that mainstream educational research has met these canons either.) Taking these canons not as some set of methodological norms by which truth is revealed, but as socially legitimating principles, I shall be implicitly arguing that

accepting these canons does not necessarily imply committing the sins of Positivism. To make this argument more explicit I conclude by distinguishing my sociological understanding of science from more philosophical arguments and close with one last observation about my field.

Science as one rhetoric (among many)

To begin I would like to recognise the limits of my scientific agenda. Metaphorically, I would suggest that we can understand the claims of the New Sociology as rhetoric, or modes of persuasion. Given a concern for understanding how the different discourses in educational research communicate and persuade one another, this metaphor is perhaps of apparent utility. But this perspective on science is not only mine. The notion that scientific discourse is a form of rhetoric has origins in many disciplines, one of which is the study of rhetoric itself, and has been suggested partially as one way of relating competing knowledge claims.

For example, in an analysis of what it means to treat knowledge claims as forms of rhetoric, Michael Calvin McGee and John R. Lyne, begin by echoing similar remarks on positivism's critics to those found in educational theory literature for some time now. In a section of their essay subtitled, "The difficulty of being positive," McGee and Lyne recall the importation and influence of a general "academic positivism" which saw the transformation of politics into political science and moral philosophy in social science. They also note that positivism was initially intended to unite all forms of knowledge claims and provide certainty -- but that these hopes were never accomplished. The continued existence of positivism, according to McGee and Lyne, has been largely (though not entirely) as an epithet created by its opponents. Commenting on the shift toward a scientific academy, they suggest:

"This state of affairs alarmed those academics whose work could not be scientific -- artists, literati, rhetoricians metaphysicians, and most historians.

Nervous pliants from such quarters helped mask the fact that the rhetoric of the positivist movement never quite produced the projected unity.

Positivism was kept alive more in the minds of opponents than in the daily practice of scientists, becoming in the end more epithet than signifier, more the ghost of horrified imaginations than a coherent body of thought... [And]... This is not to say that positivism was entirely the child of threatened imaginations. Rather, it simply did not exist as a unique and coherent philosophical position after the breakup of the Vienna Circle."

According to McGee and Lyne, the major influence of positivism has not been as a philosophical position, but as "an attitude toward the ideal relationship of the fields of knowledge," an attitude they term "scientism." What these authors describe as scientism seems virtually the same as Giroux's culture of positivism. To convey this similarity, their description is worth quoting at length:

"The story of scientism is at once cold, calculating, and romantic. The greatest miseries of humanity have been caused by irrational belief in magic, religion, and other intellectually indefensible rubbish. Reason, understood as virtually equivalent to scientific method, can save humanity. Science is universal in the sense that the logic of its inquiry is the same in any domain where knowledge is possible. The universal objective of inquiry is explanation and prediction. An event is explained by showing that it occurred as the result of laws, rules, conditions, and so makes prediction possible. Inquiry is 'value-free' or 'value-neutral'; it strives to be as objective as possible, showing how to change circumstances to produce results, but never recommending that one particularly policy be selected.

Scientists may offer value judgements, but science is mute on the problem of decision, for no 'ought' claim can be derived from knowledge of facts. No claim will be acknowledged as fact until it has been verified by observation, and no proposition will be treated seriously even in theory unless it is possible to envision the conditions of its verification." McGee and Lyne argue further that recognition of the failure of scientism, the failure to provide a unified basis for deciding between competing knowledge claims,

has lead many academics toward rhetoric and the rhetoric of inquiry.

To find such a description of the current state of academic affairs that is so similar to those found in my own field is somewhat comforting. Even if the New Sociology's Critique of Positivism didn't radically undermine mainstream educational science, it seems the New Sociology may have been on to something. But McGee and Lyne offer a much different cure for the ills of the academy than do any of the current discourses in my field.

Outlining four possible "routes" towards rhetoric, McGee and Lyne propose, of course, their final option: a path in which rhetoric is taken as a means to adjudicate or negotiate between competing knowledge claims -- when competition results from exigencies in academic communities "as part of the natural evolutionary process of science," in response to interfield or interdisciplinary competition for material resources of knowledge production, or simply as the consequence of interfield debates among those who study the same thing, or as a result of having experts comment or give advice in the public interest. These authors continue on at length to defend their choice of route and to argue for understanding rhetoric as a means of negotiating between knowledge claims -- not as a means to unify knowledge claims.

Whatever managerial role McGee and Lyne propose for rhetoric, as a discipline, is pragmatically not of import to my agenda; but their position on science is of most concern to me. According to them, this mode of rhetoric is not opposed to science, but rather sees scientific inquiry as one type of rhetoric. In their own words:

"We prefer this last route to rhetoric of inquiry because it exposes and features a dialectical tension that is only implicit elsewhere. Other arguments lose the issue because they make it appear that rhetoric is setting itself against the rigour and reliability of a practice that results in the largesse of technology. Rhetoric in fact values scientific discourse, if only because the scientific method is a powerful and persuasive form of

argument. But it is neither the only nor the most persuasive in all situations.

Of course, for those more familiar with science, the notions that there is a "natural evolution" of science or that there is such a thing as "the scientific method," may seem troubling. But the dialectic tension McGee and Lyne see in their proposal nicely captures, for me, the social chasm that seems to lie between radical sociologies of school knowledge and mainstream educational research. As they explain:

"The dialectic that undergirds the turn to rhetoric in contemporary letters

lies in an opposition between passionate and prejudiced social reason (traditionally associated with the rhetoric of the marketplace and forum)

and the antirhetoric of cool, comfortably neutral technical reason (associated in the public mind with computing machines and sterile laboratories)."

Where McGee and Lyne are concerned with largely analytical categories in this depiction

(even as they speak of socially associated meanings), my concern is more context specific.

Given that it is possible to recognise science as one form of rhetoric, and not a means

for unifying knowledge, what kind of rhetoric has been employed in my field?

Rhetorical tendencies in the field

Elsewhere I have introduced a distinction between the two conceptions of

philosophic discourse, that associated with (mainstream) analytical educational philosophy

and that associated with radical social critique. On the one hand, I suggested analytical

philosophy was largely restricted to deductive introspection. In this form of philosophic

discourse, beginning from stated premises, arguments are largely constructed within the

limits of logical coherence and deductive reasoning, building toward a conclusion. On

the other hand, I suggested radical social critique built its premises and arguments from

a continual dialogue with empirical (social) observation. In this later form of philosophic

discourse, observations of the social world are explicitly conducted through theoretical

interpretation.

Here I would like to reconsider these forms of philosophic discourse through a

more stark distinction drawn between what I call the philosophical rhetoric of demonstration and the scientific rhetoric of evidence. The difference between these two modes of persuasion can be understood readily through a visualist analogy.

Imagine the intellectual in place of the clergy, presenting a view or image of the world from the elevated pulpit. Speaking out over the masses, this philosophical rhetorician proceeds to critique the world outside the halls of the church. His sermon is lengthy and full of references to the evils of external secular life -- the life where contact must be made with the non-initiates.

The mode of presentation in these sermons is of course connected with the spatial geography of the church's habitat, but it is the mode of discussion which is of concern here. Presenting "the vision" held of the outside world, (through allegory, reason and appeal to supposedly shared subjective experiences), this intellectual for the masses attempts to persuade his audience that His is the real Truth. As my label suggests, this rhetoric relies heavily on philosophical, a priori, analytical demonstration. External references are made, but the basis of persuasion lies in the eloquence of the presentation -- in the artistic, poetic construction of reality. (Justice and equality are often banners of the ultimate appeal.) In a sense, in the extreme form, this rhetoric is hermetically sealed, enclosed. Connection with its message can only be made through a leap of faith, a will to truth.

On the other hand, the promise of science potentially lies in its differing rhetorical mode. In contrast to my Church analogy, I think an alternative image can be found in the court room -- the secular home of truths. Here persuasion does not wholly rely on demonstration. Here an appeal must be made to evidence. Here the model is not of a sermon, nor even a dialogue between two parties. Here a mutually accepted (socially constructed) view of evidence, acting as a third party arbiter, is the result of a process of triangulated communication. Here questions left unresolved in the abyss of incommensurate knowledge claims can be mutually opened to possibilities of

empirical matters. To me, the most significant difference between the philosophical rhetoric of demonstration and the scientific rhetoric of evidence lies in the way in which each rhetorical mode constructs authority. In the philosophical rhetoric of demonstration authority lies in the positions of the author, the speaker; whereas, in the scientific rhetoric of evidence authority is dispersed -- if "evidence" is explicitly (and partially) framed and deconstructed. (In a court room, the explicit framing of evidence is manifested by the role of purposely prejudiced contending legal representatives -- the lawyers.)

In drawing this more stark distinction between philosophic and scientific rhetorical modes, and reconsidering the rhetoric of radical sociologies of school knowledge, something of an irony appears. Where before I suggested that the philosophical claims of the New Sociology relied heavily on empirical observation, here I would argue that even though this reliance is obvious, within the discourses of my field appeals to socially recognisable "evidence" are rather limited. Given this, it seems to me that where the New Sociology imported an alternative view of philosophy in the endeavour to critique empirical social phenomena, its rhetoric has remained a philosophic rhetoric of demonstration. I am not suggesting that empirical observations are not made in these discourses, but I am questioning the degree to which radical sociologies have presented observations in a form their Others might recognise as "evidence" supporting the claims they make.

This charge may seem to recapitulate Liston's criticism that Marxist educational theorists have not empirically verified their claims. But I raise this criticism in the context of viewing science as a form of rhetoric to emphasise the difference I see between Liston's concern and my own. Unlike Liston, I am not calling on a "realist epistemology." Rather, by focusing on the rhetoric of radical sociologies of school knowledge I would simply reframe Liston's critique as evidence of my claim

that the discourses in my field have failed to present empirical observations which are socially recognised as "evidence" by their Others. Even with his explicit political sympathy for Marxist theory, Liston doesn't see evidential support in the Marxist educational discourse.

There is of course a very important issue raised here. Given that radical sociologies of school knowledge do make social observations, the question at hand is what counts as evidence for the theoretical claims being made by these discourses. With this issue in mind, the historical struggle of ethnography and qualitative methodologies to gain acceptance in US educational research stands as one reminder that within the "larger" field there are continually struggles over what counts as evidence. Of course, these methodological battles continue with, for example, Lather's arguments for recognising "catalytic validity." Such struggles can be seen as successful redefinitions of what counts as evidence. But the question then becomes, for me, if these new methodologies are presenting evidence that counts, for whom does it count?

Since my concern here is with the persuasiveness of radical sociologies of school knowledge, and since I wish to further support my claim that the discourses in my field have not employed a socially recognisable scientific rhetoric of evidence, I turn now to an analysis that begins from something of a different point of departure and attempt to show the ways in which mainstream educational research canons can deny the radical discourses of my field's scientific authority.

Being read through canons not of your choice

Before beginning this analysis, I should justify its strategy. Here I shall be following Bourdieu's notion that within scientific fields relatively stable canons of methodology form the doxa and orthodoxy which regulates what gets consecrated as scientific capital. In this view, the scientific field is not at all a level playing field, but it is a field of unequal power relations in which legitimating principles function to the advantage of those in "higher" relative positions of power. If this view is correct, and if

the traditional methodological canons of educational research serve the interest of mainstream researchers (in the US), then viewing radical research through mainstream canons ought to tell us something about the persuasive capacity of that research within the mainstream. In effect, when I suggest that the rhetoric of radical sociologies of school knowledge has not been a scientific rhetoric, I am presuming that it is mainstream orthodoxy/ doxa which has defined what counts as scientific. Thus, I take what follows not only as evidence for the tendency toward philosophical rhetoric, but also as further evidence for my claim that radical sociologies of school knowledge have not been that persuasive to the mainstream.

Unfortunately for these radical discourses, being identified as holding only limited scientific capital can be done in rather short order with only three conventional criteria for educational science: falsifiability, generalizability, validity. Below, I'll consider each of these criterion in turn, giving a brief outline of what each roughly means (here) and then screen the discursive strands in my field to see if each criterion is met.

Roughly put, the Popperian notion of falsifiability is intended to delimit the kinds of claims which can be scientifically tested. The basic idea here is pretty simply. Because science is supposed to build on past empirical inaccuracies, there has to be some way in which theoretical claims can be said to be inaccurate. In other words, there at least has to be the possibility of showing a theory to be wrong.

With a cultural turn to recognising resistance and human agencies which oppose the hegemonic control of Capital, the Structural Neo-Marxist discourses in my field, particularly the ever expansive Parallelist Position, have virtually guaranteed whatever is observed relative to the functioning of hegemony, it will confirm their theories. Similarly, by basing their theoretical construction on the notion of "contradictions," Carnoy and Levin, Ginsburg, and Aronowitz and Giroux also have proposed theories which defy this scientific tenet. Unfortunately, when Structural Feminists argue that Patriarchy

functions in contradictory ways, they too fall on the non-scientific side of Popper's dichotomy. Likewise, when each of the post-structural variants begin to speak of multiple and contradictory subjectivities, they have moved beyond the realm of falsifiability.

Hence, on the level of broad societal claims (in the case of the structural arguments), or in terms of their "basic" claims about subjectivities (for the post-structural variants), none of the theoretical discourses of my field meet the falsifiability criterion. Of course, these kinds of arguments are only some of the claims advanced in the radical discourses. However, on another level, many more detailed or context specific claims have been proposed within each of the discourses of my field.

For example, among the Structural Neo-Marxist arsenal is the claim that teachers have been de-skilled. Structural Feminists suggest that the distribution of school knowledge within classrooms is such that young women and girls are not taken seriously, academically speaking. Post-structurally, Wexler argues that the micro-economy of self-production will define everyday educational processes. And Ellsworth has suggested that the rationalism of critical pedagogy marginalised and silenced students in her class. Each of these claims, I suggest, could be drafted in a way which would make them susceptible to the falsifiability criterion. That is, I think it is possible to imagine studies by which these claims potentially could be shown to be wrong. But have reasons or evidence been given to suggest that such phenomenon occur across many social contexts? I think not.

Here the conventional notion of generalizability presents some serious problems for the research in my field. As has conventional research wisdom would have it, the qualitative or interpretive evidence upon which these claims have been largely substantiated does not suffice for believing that these phenomena are generally true. This isn't to say they are not generally true, but that evidence hasn't been forthcoming to show that they are. In fact, unfortunately, the basic tenet of conventional ethnography

suggesting that all things are context specific works directly against generalising.

Where Apple largely bases his claim about de-skilling on an interpretive analysis of one science curricular package and one qualitative study, to my knowledge there have been no studies done in multiple contexts to show that the technologies Apple sees as de-skilling are in fact de-skilling large numbers of teachers. (Of course Apple also has the further historical problem of showing that teachers actually once had the skills he says they have lost.) Nihlen and Kelly have already suggested that there is a need for data to support the qualitative insight that, within classrooms, school knowledge is differentially distributed along gender lines. (Between classroom school knowledge differentiation according to gender, of course, has been substantially documented.) While Wexler has offered some qualitative evidence for his claim, there is no evidence beyond his own. And while Ellsworth's interpretation of her classroom's use of critical pedagogy may be true, the generalising force of her arguments depends on her theoretical assertions about the historical relationship between rationalism and its creation - Others.

Each of these arguments, I think, are based on strong analytical claims. And I think there is reason to believe each of them may be true across many contexts, in many classrooms; but from the evidence presented by the field to date, I do not think it would be safe to assert any of these things are generally the case (although, based on my own observations, I do think Nihlen and Kelly's claim is a relatively safe bet). Here the field's heavy reliance on qualitative evidence, that has been so fruitful in further developing more detailed and nuanced theoretical claims, becomes seriously restrictive. Without evidence gathered (constructed) from within multiple classrooms, none of these claims stands a chance of meeting the conventional criterion of generalizable substantiation.

This leaves, I think, the more general concern about these theories' validity. While there are many types of validity about which mainstream educational

researchers

worry (construct-validity, internal validity, face validity, etc.), the general issue of validity concerns the very simple question of whether or not radical sociologies of school

knowledge have presented interpretations of data that mainstream

researchers are likely

to see as valid representations of what actually goes on in schools.

Lather has discussed

the problems raised when one theoretically maintains that individuals can be seen as

holding a false consciousness. As Lather suggests, claims about false consciousness

need significant justifications for privileging the theorist's

interpretations of social actions

over those of the people being researched.

But beyond this one theoretical notion, there is yet another more general problem

for the field; namely, as radical theories these discourses are presenting general

arguments most people are not exactly going to readily accept as valid.

For example, I

would point out how Hargreaves questioned Anyon's interpretations of behaviours she

took to be "resistance." Conventional ethnographic wisdom suggests that multiple

interpretations of data need to be generated and analysed, through a process of ruling

out those interpretations that only marginally "fit" the data. Since each of the discourses

of my field has advanced a "new" theoretical perspective, and since only Lather has

presented possible interpretations which were ruled out, the data presented in these

discourses is almost all only that which fits the interpretation each theory advances. For

mainstream researchers who aren't exactly amenable to these interpretations in the first

place, there has been no methodological defence given for the claims advanced in these

discourses.

Overall, this brief analysis suggests, I think, that when viewed through the

philosophical-scientific dualism I imposed above, the rhetorical mode of radical

sociologies of school knowledge is clearly not recognisable by mainstream educational

research as scientific. Such conventional methodological charges could be expected from

a "positivist," of course; but they also suggest, I think, a crucial

political point that many radical sociologists of education have missed.

The missing element in these discourses, I think, is an acceptance of the need to construct defensible evidence. Armed with the view that evidence is not innocent and always created out of particular world-views, these radical intellectual clergy have come to an immensely arrogant conclusion: that it is up to them to mould evidence. The view in these discourses, I think, is that since evidence is not innocent, and since we know research works in particular interests, it is in the (our) masses interest to have our evidence moulded to the reality we see.

But socially recognisable evidence is rarely presented. This style of rhetoric erases out the possibility of having the audience view the evidence themselves -- "directly?" (knowing that it is not innocent). If presented in/through the scientific rhetoric of evidence, however, it would be possible to appeal to evidence as a third party arbiter. It is this appeal that I take to be restricted within the sociologies of school knowledge.

On the limits of the field's socially recognisable evidence

I do not mean to dismiss the issue of what ought to count as evidence here. This is, of course, a central concern when making arguments about a lack of presenting evidence. However, I think one could argue that while the radical discourses have entered into the debate of what counts as evidence, making the issue problematic or unsettled, they also have filtered what little evidence they presented through a theoretical framework few would be willing to accept prior to empirical persuasion. By arguing that a scientific discourse appeals to "evidence," I am simultaneously arguing that its evidence is socially recognised as evidence.

Nor do I mean to suggest that the radical sociologies of school have presented no data which can be commonly recognised as evidence. While I think this is generally the case, there are exceptions. But even these exception have their rhetorical limits. For example, in his *Teachers and Texts*, Michael Apple presents a political economic analysis of textbook publishing which incorporates quite of few standard statistical

measures of the textbook industry. There we find Apple citing measures of the textbooks industry's profits and income, and proportional measures of how much of the market is made up by individual States. In all Apple employs these data to begin an analysis of the production-consumption circuit of school knowledge.

However, even with this analysis, Apple himself recognises the limits of his research. As he puts it:

"This points to a significant empirical agenda as well. What is required now is a long-term and theoretically and politically grounded ethnographic investigation that follows a curriculum artefact such as a textbook from its writing to its selling (and then to its use.) Only then will we have a more accurate portrayal of the complete circuit of cultural production, circulation, and consumption."

As much as I applaud Apple's recognition of the empirical limits of his analysis, his proposed research agenda still seems to me limited. Assuming that a "theoretically and politically grounded ethnographic" study would generate claims mainstream educational research would take to be valid (a big assumption I think), and assuming that this study does construct "a more accurate portrayal of the complete circuit of cultural production, circulation and consumption," what would a portrayal of one artefact have to say about an entire national industry? Once again, such an agenda would continue radical sociology of school knowledge's tenuous treatment of the generalizability issue.

Another problem arises when it is recognised that radical historical analyses of teaching also provide recognisable empirical support for their claims. To take Apple as an example again, consider his historical argument that teaching has been constructed within the patriarchal hegemonic definitions of "women's work." To support this claim Apple has, among many other sources of data, presented the text of a teacher's contract from 1923. This contract explicitly restricts women's private behaviours, and I think lends credence to Apple's overall arguments. But as an historical

analysis, such research will continually face the question, how do we know this is still relevant in schools today? In the US, after all, exploring frontiers has often meant leaving history behind us (both literally and metaphorically), and teacher contracts do not look the same as they used to. This means such arguments are in the position of having to persuade readers that even after relatively visible social movements have asserted themselves and been publicly associated with some social change, (such as the Women's Movement over the past two decades in the US), these kinds of historical critiques still carry contemporary validity. While historically minded researchers may find such analyses persuasive, I suspect many do not. As historical analyses, the data presented in such research does not necessarily provide evidence of what is happening now.

Given that there continue to be strong debates over what constitutes acceptable evidence in US educational research, and given many radical educational researchers are centrally located in these debates, it may seem odd that I have at all attempted to apply standards of evidence which these radical discourses reject. In the next section I would like to explain how it is that I justify my imposition of criteria these radical researchers would reject (and) without basing these criteria on the grounds a radical rejection might assume.

A break from philosophy

Throughout my discussion of the ways in which radical sociologies of school knowledge have not met conventional canons of educational science, my concern was with constructing "socially recognisable evidence." This phrase, of course, carries a host of implications. If read through analytical philosophical lenses this perspective might seem dangerously closed to the notion of "justified true belief." Or, pushing this slant further, many conventional so-called philosophers might see the simple imposition of a naive epistemological relativism. From the perspective of an educational philosophy which has yet to find a way out of endless relativism v. objectivism debates, this

perspective on my work may seem valid. Sociologically however, such universalistic and reductive claims are evidence that in the endeavour to construct social sciences, philosophy has its limits.

The distinction which separates my reading of radical sociology of school knowledge's lack of persuasiveness in the US field of educational research from Liston's claims for an epistemologically necessary empirical verification is homologous to the historical break between sociology and philosophy. The grounding of this analysis has been constructed in opposition to both the positivist image of science as a mirror of reality and the conventionalist image of science as purely a social construct. Unlike Liston, who seeks more or less accurate models of some underlying reality in an "epistemological discourse designed to ground and to justify science in and by a normative methodology tied to a logical reconstruction of the progress of science," I view struggles of scientific authority as taking place within a field of unequal power relations in which methodological canons are the effect. In this view, the epistemological positions advanced under the authority of philosophy are simultaneously accepted at their word, as manifestations of the "native view," and constructed as social facts. Hence when I impose conventional notions of scientific methodology on radical sociologies of school knowledge it is in the sociological attempt to describe some of the legitimating principles by which radical educational theories would be deemed non-scientific.

This analysis, of course, is not without its own subtext. If the New Sociology's Critique of Positivism was on to something, I would suggest it is in the recognition that US educational research is indeed a field in which there is a struggle over scientific authority. And if the Critique was mistaken, it was in part mistaken in the miscalculation of the marginal return on its investment in philosophic discourse.

In his history of science, Georges Canguilhem suggested that, "It should be laid down as a general principle in the history of science that discord and rivalry within the scientific community can never totally impeded communication, certainly not since the seventeenth century."

This historical observation is for me quite telling. Entering into the educational Academy in the wake of two decades of "Paradigm Wars," there are indeed many theoretical avenues open to current students of radical sociologies of school knowledge. Each of these rhetorical paths would open lines of communication within some social contexts to be sure. But it seems odd for a field of inquiry so sensitive and concerned with understanding and perhaps altering unequal power relations to have left one of the most persuasive rhetorics in the hands of those against whom they struggle.