Theory: The Necessary Evil

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Epistemological Worries

Qualitative researchers in education have begun to question the epistemological premises of their work. Or, at least, someone in the arena is questioning those premises, and the questioning worries the researchers who actually do the work of studying schools, students, and education close up. Attacks on qualitative research used to come exclusively from the methodological right, from the proponents of positivism and statistical and experimental rigor. But now the attack comes from the cultural studies left as well, from the proponents of the "new ethnography," who argue that there is no such thing as "objective knowledge" and that qualitative research is no more than an insidious disguise for the old enemy of positivism and pseudo-objectivity.

The attack can conveniently, if somewhat misleadingly, be as "theoretical." Convenient because it is not a question of empirical findings; misleadingly, because the theory involved is not substantive. These worries, which used to take the form of a concern with the theoretical bases of our substantive findings, now focus on the theory of knowledge that underlies the whole enterprise. What bothers qualitative researchers in education, I think, is that they are no longer sure, as they once were, that they are doing things the "right way." They worry that the work they do may be built on sand, despite all their care and precautions, all their attempts to answer the multitude of criticisms that greet their efforts, and all their attempts to still the qualms that arise from within--that the whole thing will count for nothing in the end. Not only that our work will not be accepted as scientific, but also that that model of scientific work we aspired to is now discovered to be philosophically unsound and in need of serious rethinking. Who are we kidding with all this science talk? Why don't we admit that what we do is just another kind of story, no better or worse than any other fiction?

How shall we understand all this? Are these realistic worries? Are we building on sand? Is what we do just another story?

Theory or Social Organization?

We can take these debates at face value, worry over their content, and try to answer all the questions asked of us. That's the conventional way to deal with such problems. The literature discussing qualitative research in education parallels similar discussions in sociology and, especially, anthropology. These discussions center on the relative merits of qualitative and quantitative research, on the problems or virtues of positivism, on the importance (or danger) of subjectivity, and so on.

These are, of course, serious problems in the philosophy of social science. It is not clear how any of us, qualitative or quantitative, can justify what we produce as certified or warranted or credible knowledge. Whatever safeguards we take, whatever new tricks we try, questions can be, and are, raised. Qualitative research--we might better say research that is designed in the doing, that therefore is not systematic in any impersonal way, that leaves room for, indeed insists on, individual judgment, that takes account of historical, situated detail, and context and all that--research of that kind is faulted for being exactly all of those things and therefore not able to produce "scientific," objective, reliable knowledge that will support prediction and control. Research which tries to be systematic and impersonal, arithmetic and precise, and thereby scientific, is faulted for leaving out too much that needs to be included, for failing to take account of crucial aspects of human behavior and social life, for being unable to advance our understanding, for promising much more in the way of prediction and control than it ever delivers.

Epistemological issues, for all the arguing, are never settled, and I think it fruitless to try to settle them, at least in the way the typical debate looks to. If we haven't settled them definitively in two thousand years, more or less, we probably aren't ever going to settle them. These are simply the commonplaces, in the rhetorical sense, of scientific talk in the social sciences, the framework in which debate goes on. So be it.

Also, so what? Because I don't mean those remarks fatalistically. I don't counsel resignation, acceptance of an inescapable tragic fate. No. There's nothing tragic about it. It's clearly possible, on the evidence we have all around us, to find out things about social life in ways that are more or less good enough, at least for the people we are working with now. It's happened often enough in the past and there's no reason to think it can't continue to happen.

In fact, this is exactly the import of Thomas Kuhn's analysis of science, as I understand it. Whenever scientists can agree on what the questions are, what a reasonable answer to them would look like, and what ways of getting such answers are acceptable--then you have a period of scientific advance. At the price, Kuhn is careful to point out, of leaving out most of what needs to be included in order to give an adequate picture of whatever we are studying, at the price of leaving a great deal that might properly be subjected to investigation, that in fact desperately needs investigation, uninspected and untested.

That's alright. Because, though everything can be questioned, we needn't question it all at once. We can stand on some shaky epistemological ground Over Here for as long as it takes to get an idea about what can be seen from this vantage point. Then we can move Over There, to the place we had been treating as problematic while we took Over Here for granted and, taking Over There for granted, make Over Here problematic for a while. It's John Dewey's point: Reality is what we choose not to question at the moment. (There's also Lily Tomlin's point, as it comes out of the mouth of Trudy the Bag Lady, no mean philosopher herself: "After all, what is reality anyway? Nothin` but a collective hunch." And, she adds, "Reality is the leading cause of stress amongst those in touch with it".)

Any working scientist must have a position on such questions, implicit or explicit (and the better shape the science is in the more the positions are implicit), just in order to get on with the work. Any working researcher's positions on these questions are likely--the chief fear of the philosophically minded--to be inconsistent, just because they have to be taken ad hoc, to deal with immediate problems of getting the work done. Not only is inconsistency unavoidable, it is the basis of everyday scientific practice.

For instance: I am devoted to qualitative work and think that the criticisms made of "simple minded counting" are often quite correct. But I also rely, whenever I can, on data from the U.S. and other Censuses. I'd be crazy not to. Any sensible analysis of social life will want to take into account the age distribution of the population studied, even though we know that people routinely misstate their ages. Why else are there so many more people who are twenty-five than there are twenty-four and twenty-six year olds? When we write and talk about schools and education, we routinely take into account the relative size of various ethnic and racial groups as reported in the Census, even though we know that those numbers simply report what people choose to put down as their race, which may have no relation at all to either biological or social fact. We use those numbers even though we know they are riddled with errors that demographers themselves have exposed.

Similarly, the hardest-nosed positivists, if anyone will admit to being such any more, routinely take into account all sorts of knowledge acquired with the help of"soft" methods, without which they couldn't make sense of their data. They may not admit it, but the interpretations they make of "hard findings" rely on their own understanding of the less easily measured, though still easily observed, aspects of social life.

In short, we all, qualitative and quantitative workers alike, have to use methods we disapprove of, philosophically and methodologically, just to get on with it and take account of what must be taken account of to make sense of the world.

The Necessary Evil

So we all have to be epistemological theorists, know it or not, because we couldn't work at all if we didn't have at least an implicit theory of knowledge, wouldn't know what to do first. In that sense, theory is necessary.

But the questions raised about the justification for what we do, which is what these theories are, cannot be definitively answered. That's an empirical generalization, based on the simple observation that we are still discussing the matter. To spend a lot of time on unanswerable questions is a waste of time (see Stanley Lieberson's discussion in *Making It Count*) and quite paralyzing. If you have convinced yourself that what you are doing can't be justified reasonably, it's hard to get up the energy necessary to do it. It seems better to continue discussing the problem in hope of finding an answer that satisfies you and the people who are aggravating you about the warrant for your conclusions.

In that sense, the pursuit of epistemological and similar questions in the philosophy of social science is evil. If you're accustomed to this dilemma it isn't a great trouble--you make a choice and go about your business. But some researchers--most especially graduate students--are especially vulnerable to the questioning doubts that paralyze thought and will and work. For them the evil is serious. To repeat, we still have to do the theoretical work, but we needn't think we are being especially virtuous when we do. Theory is a dangerous, greedy animal, and we need to be alert to keep it in its cage.

Social Organization

From a different vantage point, we can see debates over method and its justification as the kind of thing that happens in the world of social science, as a recurring social phenomenon to be investigated rather than a serious epistemological problem--in other words, to paraphrase ethnomethodologist Harold Garfinkel, as a topic rather than an aggravation. And we can ask sociological questions about debates like this: When, in the life of a discipline, or of a researcher, as my remarks about graduate students suggested, or of a piece of research, do these questions become troubling? Who is likely to be exercised about them? How do such unresolved and unresolvable debates fit into the social organization of the discipline?

The Relativistic Specter

To ask such questions immediately raises the specter of a paradoxical situation in which *I* presume, on the basis of a social science analysis which is itself philosophically unjustified, to give *you* the social science lowdown on a critique of what I am at the moment doing. It's a kind of debunking, not unlike psychoanalytically inclined writers who respond to criticism with an analysis of the unconscious motives of their critics. That is just the problem that is giving some contemporary sociologists of science fits, because they understand perfectly well that their analysis of the workings of science is in some sense a critique of science. If the critique is correct, then it applies to the analysis that produced the critique. You can see were that leads.

An alternative position is to accept the reflexivity this involves, indeed to embrace it, and then use our knowledge of the social organization of science to solve the problems so raised. In other words, if it's an organizational problem, the solution has to be organizational. You don't solve organizational problems by clarifying terms or arguments. Organizations are not philosophies and people don't base their actions on philosophical analyses. Not even scientists do that.

Science Worlds, Chains of Association

What does it mean to speak of the social organization of an intellectual or scientific discipline? We can speak here of scientific *worlds* in analogy to the analyses that have been made of art worlds. These analyses focus on a work of art--a film, a painting, a concert, a book of poetry--and ask: who are all the people who had to cooperate so that that work could come out the way it did? This is not to say that there is any particular way the work has to come out, only that if you want your movie to have orchestral music in the background, you will have to have someone compose the music and musicians play it; you can easily, of course, have no music, but then it will be a different film than the one whose action is accompanied by a score.

An art world is made up of all the people who routinely cooperate in that way to produce the kind of works they usually produce: the composers, conductors and performers who produce concert music; the playwrights, actors, directors, designers and business people who produce theatre works; the writers, designers, editors and business people who produce novels; the long list of everyone from director and actors to grips and accountants and caterers and transportation captains who work together to make Hollywood films; and so on.

The cooperation that makes up an art world and produces its characteristic works depends on the use of conventions, standardized ways of doing things everyone knows and depends on. Examples are musical scales, forms like the sonnet or the three movement sonata, the Hollywood feature film, the pas de deux--a list that suggests the variety of elements that can be so standardized. When everyone in an art world recognizes and uses the same conventions, collaboration proceeds easily and economically, somewhat at the expense of originality and variety. If we all agree to use the twelve-tone scale of Western music, we know that players and listeners alike will know and be able to deal with our music, but we give up the opportunity to use scales constructed differently.

That's an art world. A science world, by analogy, would consist of all those people who cooperate to produce the characteristic activities and products of that science. This means more than the people who make up the scientific community to which Kuhn called our attention. It includes, for instance, the people who provide the materials with which the science works: the experimental animals, the purified chemicals and water to experiment on them with, the carefully controlled spaces to do it all in. For social science it typically means, importantly, the people who provide us with data by gathering statistics, doing interviews, being interviewed, letting us observe them, collecting and giving us access to documents. Just as with art works, the kinds of cooperation that are available, and the terms on which it is available, necessarily affect the kind of science that can be done. A contemporary example is the conflict over the use of laboratory animals in biological research.

One of the distinctive characteristics of science worlds (as opposed, e.g., to art worlds) is the emphasis on proof and persuasion, on being able to convince someone else by commonly accepted "rational" methods to accept what you say even though they'd rather not. Bruno Latour has made this the cornerstone of his analysis of "science in action." He speaks of scientists trying to get more and more people to accept their statements, by enrolling "allies" with whom opponents of their statements will also have to contend. Footnotes and appeals to the literature serve to line up allies with whom people who disagree with you will also have to disagree. In Latour's analysis, people agree with each other not because there is a basic scientific logic which decides disputes, and certainly not because Nature or Reality adjudicate the dispute, but because one side or the other has won a "trial of strength," on whatever basis such trials are decided in that community. In a series of provocative dicta, Latour says things like (I'm paraphrasing), "It is not that scientists agree when the facts require them to, but rather that when they agree, what they agree on become the facts."

A beginning on this kind of (what we might call) organizational epistemology is to note that every way of doing research and arriving at results is good enough, good enough for someone situated at some point in the research process. If it weren't good enough for someone, no one would be doing it. Who it has to be good enough for and when it has to be that good are empirical questions that depend on the social organization in which that bit of knowledge arises.

The most general finding here is that, though every scientific method has easily observed technical flaws and is based on not very well hidden philosophical fallacies, they are all used routinely without much fear or worry within some research community. The results they produce are good enough for the community of scientific peers that uses them. The flaws will be recognized and discounted for; the fallacies will be acknowledged and ignored. Everyone knows all about it, knows that everyone else knows all about it, and they have all agreed not to bother each other about it. So the Census, with all the flaws I alluded to, is plenty good enough for the rough differentiations social scientists usually want to make. But that's because the social scientists who use census data have made the collective hunch that these data are good enough for the purposes they will put them to, not because the flaws don't exist. Few enough people we would ordinarily think of as white say they are black and few enough people we would ordinarily think of as black say they are white to change any conclusions we base on these numbers, and we don't think the difference between twenty-four and twenty-five large enough to invalidate the conclusions we base on age statistics.

An interesting corollary of this is that what methods and data are acceptable depends on the stage of the scientific process at which they are used and presented, and the purpose they are used for. At an early stage of the scientific process, for instance, we are mainly playing, exploring ideas for the further ideas or explorations they might lead us to. We don't much care whether the results are valid or not, or whether the conclusions are true. What we really care about is that the discussion proceed, that we find something interesting to talk about. This stage may take place over a cup of coffee, in a seminar, in casual conversation with a colleague. I remember a seminar with Everett Hughes, in which a student interrupted one of his discursive explorations of a "fact" he had heard somewhere to say that later research had shown the fact wasn't true. Without breaking stride, Hughes asked what the new fact was, and continued to explore *its* possibilities.

In fact, it is often seen as an intellectual mistake to dismiss ideas at this stage of work just because they might not be true. The worst thing that can happen to a research community, in some sense, is to run out of researchable problems. Yuval Yonay has pointed out that researchers will often accept all sorts of anomalies if the general position containing them opens up a lot of new researchable questions, whose exploration can produce publishable papers and the feeling of progress.

At a somewhat later stage in the research process, we are mainly interested in getting an idea worth the time and effort we are going to put into it. At this point, not just any idea will do. We want some assurance that the idea we choose will bear the weight we are going to put on it, that it is not so unsupported in fact that taking it as a starting point will not leave us stranded, that taking it seriously will in fact produce a result. So we look in the literature to see what others have done and how it worked out. Before we go to the trouble of writing a research proposal or setting up a project--a more sizeable investment than one makes in a casual conversation--we want to know that we are building on a solid foundation. We subject what we find in earlier reports to careful scrutiny, and bring more rigorous methodological standards to bear, because we don't want to waste our time. If there's something wrong with this way of working, we want to know it now. Putting down a larger bet, we want better odds.

We could pursue this analysis through a variety of steps. What kinds of rigor do we demand before we accept a journal article for publication or a paper for the annual meeting of the tribe? (Here we might note the role of practical considerations. While everyone insists that only the highest standards are employed in choosing papers for these purposes, it is also well-known that scientific associations commit themselves to fill a certain number of rooms in the hotels in which they meet with paying customers; otherwise they will be charged for the meeting rooms, the Presidential Suite, and so on. The best way to ensure that a sufficient number attend the meeting is to accept their papers for the program and require that everyone on the program register for the meeting. The people who organize these programs usually receive a nicely worded double message: maintain standards and maximize participation. It's not clear that these are compatible.)

A final stage has to do with what work receives the highest honor, which does not take the form of a prize but rather of imitation. What research becomes paradigmatic in the Kuhnian sense, providing exemplars of the work that particular scientific community has standardized on, has taken as exemplifying the problems, methods, and styles of reasoning that everyone will work on? Oddly enough, at this stage we aren't really very critical, precisely because a whole community has accepted this work as paradigmatic. All the mechanisms of scientific training and community formation Kuhn describes combine to convince people that what everyone already believes is what they better believe too. Obviously it doesn't always work that way but, of necessity, it does work that way every time a scientific community adopts a paradigmatic way of working.

Specialization (philosophical and methodological worry as a profession)

When intellectual specialties reach a size sufficient to support specialization (this is one of those demographic facts I spoke of earlier) they often (and in the social sciences almost invariably) develop specialties in theory and methodology and philosophy of science (as it applies to their particular discipline). The specialists in these topics do some work which members of the discipline think is necessary to the entire enterprise but which has become too complex and specialized for everyone to do for themselves.

The social sciences have probably (this is speculative intellectual history, and could be checked out in the appropriate monographs, although I haven't done that) developed specialized methodologists and philosophers of science because they have come under attack, in ways that hurt, from people who think that the enterprise is not philosophically (especially "scientifically") defensible. The attacks have frequently come from the natural sciences, and have had serious practical consequences in the struggle for academic recognition and advantages (faculty positions, research funds, etc.), so they have been seen as requiring answers. The job therefore must be done and, to be done right, must be done by people who can hold their own in that kind of argument, people who know the latest stuff and the most professional styles of argument.

One consequence of turning this part of our business over to specialists is that the specialists have interests which don't fully coincide with ours. They play to different audiences. Philosophers of science, even if they come from our own ranks, have as at least part of their audience the world of professional philosophy, at least that part of it which concerns itself with their topic. What makes them useful to us is also what makes them difficult. They know all the tricks of philosophers of science in large part because they have *become* philosophers and are part of that world. In consequence, they are sensitive to the opinions of other philosophers of science, philosophers who do not have one foot in one of the social sciences, even when those peoples' opinions push them in directions that are not relevant to the concerns of working scientists.

Philosophers and theorists of knowledge, concerned to meet the standards of the philosophical discourse they are involved in, frequently follow their logic to conclusions which make the day-to-day work of science impractical or impossible. They seem to conclude that social science, as we now do it, can't be done. I'm reminded of Donald Campbell, who used to say that these people are very convincing but, if they're right, then what have we been doing all these years? That is, to say that it can't be done is only to say that it can't be done in a way that meets some set of standards that is not extant in the research community in which the work is actually being done.

The same thing is true when we consider the specialists who deal with technical questions, claiming to derive the warrant for their strictures from philosophical premises. Science is, remember, a cooperative enterprise in which all the cooperators have something to say about what is done. That includes, to bring this down to some earthy and necessary considerations, the people who pay for what is done and the people who are the objects (or subjects, since what term we use to describe these people is contested) of our study.

A simple example: some years ago a distinguished sociological methodologist reasoned that the newly invented technique of path analysis could be used to deal with measurement error in survey research. It was quite easy and straightforward: all you had to do was have the same interviewers interview the same respondents on three separate occasions using the same interview guide. Easy enough, except that neither interviewers or respondents would cooperate. The interviewers felt like fools asking the same people the same questions over and over again and, when they got their nerve up to do it, the respondents wouldn't answer: "You asked that twice already. Are you stupid? Or what?" The philosophical theory and its technical application were clear; the social logic was off.

Great advances in social science often depend on increases in funding. For years, most of what was known about fertility came from detailed analyses of the data of the Indianapolis Study, in its time the most detailed body of materials available on married couples' choices about how many children to have and when to have them. A major step forward occurred when increased funding made it possible to use national samples to study the decisions of couples to have children. It had never, of course, been methodologically defensible to use Indianapolis as a surrogate for the entire United States, but what choice was there? So it was used. Once a "more adequate" sample became available, the ante was upped, to the point that when I asked a leading demographer what he would do if he could no longer finance national surveys of fertility, he could not consider the question seriously; you just had to have them and that was that. In the same way, Bronislaw Malinowski's enforced four year stay (as an interned enemy alien) in the Trobriand Islands set a new methodological standard for how long and in what degree of intimacy anthropologists had to be in contact with "their people."

In other words, general statements of what must be done to be scientifically adequate rely, usually without acknowledgement, on practical matters and, in this, they follow rather than lead everyday practice.

Audiences

Audiences (and especially the people whose lives and activities we study) react to what we say in variable ways and researchers worry about that. Some of our philosophical and epistemological and theoretical concerns have to do with justifying what we do to such "external" audiences.

Educational research is particularly vulnerable to problems of justification. Everything educational researchers do has some consequence for people in the education business. Do we find that one method of teaching is superior to others? The people who are committed to the others--not just "philosophically" but also by virtue of not knowing how to do the new thing or having built their reputations on the way they now do it--will want to find reasons why these results are not valid.

I don't mean that it's just mercenary. It's more complicated than that. If you have a reason to look for trouble, you're more likely to look. Every method having flaws, if you look, you'll find. As I remarked earlier, every way of doing business is good enough--for someone at some time for some purpose. Conversely, no way is good for all purposes and all people at all times. So it is always possible to criticize how things are done if you are a different person at a different time with a different purpose.

Finally

To come full circle, the reasons and the people and the times for research are organizational facts, not philosophical constructs. Epistemology and philosophy of science are problems insofar as we cohabit with the people who make those topics their business and are thus sensitive to their opinions, questions, and complaints. Educational researchers, poised uneasily as they are between the institutions of (mostly) public education, the scientific and scholarly communities of the university world, and the people who give money in Washington, who aren't sure which of those constituencies they ought to take seriously, have the unenviable task of inventing a practice that will answer to all of them more or less adequately. The difficulties are compounded by the splintering of the academic component of the mix into a variety of disputatious factions, which is mostly what I have been discussing. No amount of careful reasoning or thoughtful analysis will make the difficulties go away. They are grounded in different standards and demands based in different worlds. In particular, as long as theory consists of a one-way communication from specialists who live in the world of philosophical discourse, empirical researchers will not be able to satisfy them. In my own view, we (the empirical researchers, among whom I still count myself) should listen carefully to those messages, see what we can use, and be polite about the rest of it. After all, as Joe E. Brown remarked in the last scene of "Some Like It Hot," when he discovered that the woman he wanted to marry was a man after all, "Nobody's perfect!"

Bibliographical Note

Thomas Kuhn's ideas can be found in his *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). The remarks of Trudy the Bag Lady appear in Jane Wagner, *The Search for Signs of Intelligent Life in the Universe* (New York: Harper and Row, 1986), p. 18. Stanley Leiberson's *Making It Count* was published by the University of California Press in 1986. I have analyzed the idea of an art world at length in *Art Worlds*(Berkeley: University of California Press, 1982). The fullest statement of Bruno Latour's views is *Science in Action* (Cambridge: Harvard University Press, 1987). I've discussed the idea that every way of presenting knowlege is good enough for someone in "Telling About Society," in my *Doing Things Together* (Evanston: Northwestern University Press, 1986). Yuval Yonay's thoughts about the utility of even bad ideas in opening up research questions are contained in his dissertation (in progress at Northwestern University) on the history of contemporary economics.