

## Thinking about Standards

Memo for the NSF Workshop on Standards in Qualitative Research

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### Theoretical Framing

One source of rigor in qualitative (and quantitative research) is the theoretical framing of the research. This involves using theory to derive questions, explain why something is a puzzle, or to suggest what one expects to find and why this is a contribution to knowledge, for example, either by filling in gaps or in suggesting how the world has changed in ways that our theories cannot account for. Among other things, theory is an orienting device which focuses our attention by highlighting or problematizing some parts of the world while ignoring others. This role needs to be made explicit and defended in research. Researchers need to offer a convincing account of why this location, these comparisons, this event, or these people are a good place to study the theoretical questions or issues that motivate the research. An important part of rigorous theorizing involves describing what abstract concepts or relationships might look like on the ground, in the particular context one is studying them with the kind of evidence one is mobilizing; including examples of disconfirming evidence, evidence that would call into question an argument or an interpretation, or require theoretical revision.

In my account, theory does a lot of heavy lifting. It offers a framework for identifying questions, helps suggest locations for investigating those questions, and it offers a means for both specifying the particulars of a social location and for generalizing from particular contexts or cases to others. Theory also provides a framework for helping to elaborate the terms under which explanations are suspect or discredited, by suggesting the conditions or findings that would cause us to revise our predictions or reject our explanations. Making the link between theory and evidence as explicit as possible is a standard that can be extended to many styles of research. And doing so does not mean that a theoretical framing (or the methods that devolve from it) cannot be revised as the researcher's engagement with the world deepens. One of the great virtues of qualitative methods, especially field work, is a flexibility which permits researchers to adapt to what they are learning, to follow their noses. Efforts to be precise in one's theoretically framing allows the researcher to see more clearly and quickly where and how expectations are wrong, or where concepts do not materialize in the forms one imagined, something which permits a deeply sustained iterative relationship between theory and evidence.

### Multiple Methods or Triangulation

Another strategy for promoting rigorous research involves incorporating different types of evidence collected in different ways, what some call "multiple methods" or "triangulation." My own research has mostly been conducted on or in organizations and I routinely use different strategies to gather evidence. The methods I have used most often include field work (where my role has ranged from that of participant to being mostly an

observer, although one is never purely anything in field work), interviews, the analysis of both contemporary and historical documents--both official documents as well as informal, private documents, and archived, historical sources of various kinds--letters, drafts of documents, memos. I have also on occasion conducted brief surveys of people who attend functions I am observing. And my collaborators have performed statistical analyses on quantitative data. Each of these methods (and variations on methods) calls for distinctive forms of expertise and skepticism.

The standard line on "triangulation" is that the strengths and limitations of different sorts of evidence can complement one another when sources are made to speak to each other. I believe this. So, for example, in my work on law school rankings I am reassured when a statistical analysis of the applications and yields of law school applicants confirm what administrators and law applicants tell me: that a change in ranking affects people's decisions about where to apply and attend law school.

The statistical analysis shows that the patterns my respondents describe extends to many different law schools and it helps me to be more precise in determining how big an effect a change in rankings has on people's decisions. But what the statistical analysis can't tell me, and what my respondents and observations can, is how this process works, the nature of the dynamic relationship between variables, or what Merton, Stinchcombe and others call the "mechanisms" of social relations. And statistical analysis cannot say much about the meaning and construction of numbers that are the object of analysis, and the authority that members grant them (or not).

As my example suggests, there are many versions and uses of "triangulation." Different kinds of data might be used to approximate the type of "fact checking" or

corroboration that journalists and their editors deploy to verify information or accounts. Or different methods may be used to get at different but related questions, in a sort of division of labor. We may, for example, compare historical records with contemporary accounts to try to assess changes over time, or we may conduct intensive field work in one part of an organization and conduct interviews or review documents to learn about other parts. This division of labor may be organized to exploit the relative advantages of different approaches, it may be an expeditious way to use scarce resources, or it may reflect the power relations of those we study, since the terms of access that researchers are granted to the people and places they study are saturated with power. (This is one reason why, as many ethnographers have noted, people's reactions to a researcher's efforts to obtain access become crucial data to analyze). Triangulation can take the form of collecting different kinds of evidence, all of which is directed toward answering the same questions or examining the same phenomena. In practice, this form of triangulation is often organized as a series of comparisons. We compare the evidence collected from different sources in order to better understand the biases or omissions of each and to produce a more comprehensive view of the social phenomena we investigate. So, for example, we might compare official accounts with informal ones, what people say (in interviews, surveys, informal conversations, emails or written reports, on their websites, to the media) with what they do, what they do in one context with what they do in another, what they do over time, and so on. These sorts of comparisons will almost inevitably lead to a more complex account of what is going.

But it is not always easy to reconcile or even interpret the differences that such comparisons might reveal. Are these differences inconsistencies? Do they represent

alternative views that correspond to different social locations? Do they reflect duplicity, politics, or people's sophisticated understandings of the constraints of genre or the demands of audience? There is no simple set of principles which allow a researcher to interpret the patterns or gaps that emerge in comparisons of these sorts but an effort to explain them will almost always prove fruitful. I should add the field of law and society was founded in response to exactly this type of comparison. The discrepancy between what has become known as law on the books, the formal, textual law of legislation and judicial opinions that is the province of exegesis, and the unruly law of practice, gave rise to a vibrant, interdisciplinary community that has spent almost 50 years trying to sort out the dazzling variety of forms of expression that this gap can take.

### Reflexivity and Reactivity

Because people are reflexive beings, because we continually monitor and interpret the world and adjust our actions and expectations in light of that, our efforts to study social worlds and the knowledge that we produce about them will alter them. This is usually described in methods literature as reactivity and it is often depicted as a methodological problem to be solved or at least managed. How we analyze our reactivity is one standard for evaluating the rigor of research but it is also a source of divergence within social science.<sup>1</sup> Some researchers try to eliminate or at least tame reactivity, while others incorporate into their accounts. Researchers' responses to reactivity does not always map neatly onto disciplines or even methods. For example, there are ethnographers who manage their reactivity by adopting the role of distanced observer and

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<sup>1</sup> For a nuanced discussion, see Burawoy, Michael. 1998. "The Extended Case Method" *Sociological Theory*, 16: 4-33.

by trying to remain as unobtrusive as possible, just as there are ethnographers who make people's responses to their interventions a central part of their analysis. As someone who has tried to watch closely the often arduous production of official organizational knowledge, both texts and numbers, and then investigated how these forms of knowledge mediate the behavior of actors, I believe we should extend the same critical eye towards our own interventions. Analyzing patterns in reactivity as reflexive responses is, I believe, an extremely promising arena of investigation, especially for qualitative researchers.