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Workshop on Interdisciplinary Standards for Systematic Qualitative Research

Thinking about questions of rigor, imagination, and scientific value within the context of Law and Social Sciences is a bracing experience, in part because the intellectual ambit is defined by core topical concerns rather than by specific disciplinary programs or practices. The rubric extends across a wide range of theoretical and methodological perspectives, and the experiences of those of us who have participated in various of the law and social sciences panels speak directly – and likely will at greater length in our discussions – to issues of translation, clarity, and disciplinary divergence (and convergence) central to the workshop. I am also, nonetheless, necessarily writing as an ethnographer, and more specifically as an ethnographer with a particular commitment to the principled examination of language and interaction in multiple contexts, so several of my comments below are likely to be supplementary to those articulated by my anthropological colleagues. I also am writing as a former editor of a major journal in sociocultural anthropology and so recurrently was afforded the chance to think very concretely about questions of rigor in ethnographic writing.

As a framing comment, I would like to suggest that the assessment of rigor is not a global judgment, and that we necessarily have different expectations and turn to different criteria and kinds of criteria at various stages over the course of a research project. More concretely, there are at least three different moments in the life of a specific research project when we might be involved as evaluators: as proposal reviewers (whether ad hoc or as panel members), as manuscript reviewers (whether ad hoc or as editors), and as readers who draw upon the published work of others. Somewhat different understandings of rigor and of what can be taken as evidence for it come into play at each of these moments: when we are asked to consider a proposed trajectory (in the future tense), when we are both evaluating and helping guide the ways in which arguments and presentations based on work accomplished, and when we are evaluating the status and value of published (or otherwise disseminated) research with an eye to how it might shape our own scholarship. And these differences, especially between reviewing proposals and manuscripts, might well be even greater when we consider qualitative research, as a frequent hallmark of such work is the flexibility it affords in addressing unanticipated but empirically significant discoveries when in the field.

I will try here to be both candid and brief about what I looked for as evidence of rigor in proposals and in manuscripts. As a proposal reviewer, especially on as far-reaching a panel as those in law and social sciences, I often found myself reading proposals concerning topics and proposing methodologies with which I was relatively unfamiliar. I necessarily placed a great deal of weight on the logic of the research plan, which depended in turn on the clarity of the methodologies proposed and the explicit links made between specific research questions and strategies for pursuing them. As an ethnographer, I also looked for evidence that the proposed questions and methods made sense in and for the context in which the research was to be pursued, which in turn

depended upon the proposer locating the work effectively, if economically, vis-à-vis the specific site(s) of inquiry. Beyond rigor, I also looked for a strong and imaginative sense of research problem (and of ways of pursuing it), for indications that the proposer had the capacity to reshape the research in principled ways should their data surprise them, and for underlying ideas that both articulated with and moved forward theoretical questions. Proposals are necessarily in the future tense, and reviewers necessarily look for warrants of capacity, logic, imagination, and senses of principled possibility.

As a manuscript reviewer and editor, on the other hand, one is reviewing work that has already been completed, and a great deal of the evaluative weight rests on the relationships among method, data, and argument as presented in the text. When thinking about rigor, my own standards for manuscript evaluation include such dimensions as the effectiveness of contextualization, the relevance of the variables being pursued to the questions at hand, the internal analytical consistency of authors' consideration of their data, explicitness in laying out both the argument and the data, and provision of sufficient descriptive materials to allow readers to judge the effectiveness of the proposed analysis. Rigor by itself, obviously, is never enough to warrant publication; it must be put to the service of furthering, refining, and transforming our understandings of the phenomena we study.

I want here to note some of the kinds of qualitative methods, perspectives, and opportunities that figure in ethnographic research, whether in sociolegal studies or more generally. The list is meant to be suggestive rather than exhaustive, and my comments are quite abbreviated:

- Structured interviewing can obviously be both rigorous and powerful; it has long been a central strategy in qualitative research, for good reason. At the same time, two caveats seem important. First, this strategy most likely works best when pursued in complement to other types of qualitative work. Second, questions of the context-appropriateness of interviewing are critical; while as social scientists we might view interviewing as neutral and unexceptionable, many of our consultants might understand it quite differently, as Briggs (1986) has argued.
- Other, perhaps locally-defined genres of conversation, instruction, and the like, ones which are less susceptible to prior topical and sequential structuring but may be quite revealing. In some of these genres the researcher may be an active participant, in others an open-eared observer.
- An ongoing concern, the forms of which take different shapes for different ends and in different sites, for observation, situating research and local practice, and the like.
- Narrative elicitation and analysis, especially life histories. A particularly provocative and convincing example of such work in sociolegal studies is Engel and Munger's (2003) study of the consequences, legal, social, and personal, of the Americans with Disabilities Act, one that affords understandings that a more orthodox consideration limited to subsequent legal cases and appeals could not offer.
- Discourse analysis in the "small d," linguist's sense of discourse, i.e., taping, transcribing, and analyzing naturally occurring, connected speech in situ. Some

fine examples of the detailed analysis of conflict discourse can be found in Watson-Gegeo and White (eds.: 1990) and Briggs (ed.: 1996). A further elegant and powerful example of such work in the sociolegal arena is Mertz's (n.d.) forthcoming study of pedagogical talk (and, most specifically, of instantiations of the Socratic method) in first year law school classrooms. Talk, whether in a classroom or in the context of village disputes, is both about social practice and social practice in itself. Thorough, systematic, and rigorous analysis of talk can effectively illuminate both its subjects and its social consequences and implications.

- As Mertz's work and a recent book by Amsterdam and Bruner brilliantly demonstrate, careful and systematic attention to the contexts and practices of socialization – of socialization into profession, craft, or other kinds of sociocultural knowledge, practice, and position - can provide powerful social and cultural data.
- Various kinds of case-based approaches to the course of related events over time have long figured centrally in both social anthropology and sociolegal studies (e.g., in the work of the Manchester School). Such cases not only provide an approach to understanding specific disputes and the like; they also often shed light on much broader materials for social analysis. Rigor of a more conventional kind is often easier to pursue in a specific synchronic moment; systematic and comparative case-based research can help provide a rigorous basis for the usually less tractable consideration of related social activity over time. And here the use of the case method is oddly parallel to the examination of discourse, as in both examples it is the connectedness of action over time, whether within the short-term frame of a specific verbal interaction or across months or years.

I want to address two other issues quickly here. The first concerns the divergence issue, where variation often has less to do with methodology per se than with the polysemy of “theory” as a term. As a quick example, I found during my time as editor of American Ethnologist that manuscripts drawing heavily upon either quantitative data or formal modeling often elicited responses from reviewers characterizing them as “undertheorized,” while colleagues working in such quantitative traditions found the journal's articles frequently interesting and engaging but argued that they generally did not contribute to “theory building.” How we define the goal of our inquiry and the nature of better and less effective epistemological means to those goals are critical questions, and we often, in not examining our own preconceptions carefully enough, fall into false dichotomies.

A final point has to do with what I see as a potentially invaluable resource for considering the rigor of published work. Making our data public, i.e., providing for subsequent readers at least some of the materials on which we base our analysis, has long been desirable and is now, with the possibility of electronic archiving and distribution, closer to feasible. Replication of social science research in the classic sense is rarely possible, but the reanalysis of relatively raw material, or at the least the reading of publications with an eye to the materials on which they are based, now might be more likely. Linguistic anthropologists have long been frustrated by the inability of most journals to

publish extensive transcript material, just as many doing sociolegal research have found it impossible to publish the extended cases upon which they base their interpretation. With the increasing availability of electronic data bases (an example being the development of the AAA's AnthroSource), a very inexpensive resource for such storage, access, and, most important, reanalysis could become broadly available. NSF might want to consider hosting such extended empirical materials across the social sciences.

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