

Memo for NSF Workshop on Qualitative Methods
In search of social science

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"You might be sitting in a history class/ listening to the analysis of 'what was going on' in the thirties in new york, say/ and you hear nothing of shtetls where grandma's generation came from/ and the descriptions of sweatshops sounds oddly abstract and disembodied, that is, emphatically unsweaty-scientific-full-of clear-light - spared of the dust of ripped cloth - and quiet so you can hear yourself (or someone else) think and the machines screaming and bobbing has stopped, all put in terms of an analysis of the labor structure of the immigrant population, complete with statistics/ and politics sounds like this or that labor policy adopted by this or that administration/ not at all what grandma described doing to work as/ but you came to school to learn/ and it feels like an interesting addition to what you already know from family history and hot tea mornings in kitchens in brooklyn apartments/ but it still seems like the viewpoint of the other, of the officials giving the official line on what was happening - the politics at the pinnocle games just can't be reduced to "labor unrest"/ but we're going too fast"

"then its years later and you wonder again about the shtetls and what you might have lost in the history class/ and you focus on some imaginary moment when it happened - when the statistics and the analysis of the labor structure were no longer just interesting additions to the lived experience in new york of grandma and her friends but instead became the reality itself; and grandma's description about why her boss acted like he did was just shtetl superstition, or worst, silly, because at some point the feeling of learning new things was replaced by the idea of learning things they way they really are, free from superstition and prejudice, and stuff might be left out for the sake of time but what was there, presented as knowledge, was knowledge, in a particular form and in a particular language that you recognize as not the way you started out looking at things, but we're for education after all."

"and then you start wondering what if the language of true knowledge that you learned, the way of talking about things intelligently and dispassionately, was itself a mythology that contained prejudice and superstition; and then that its not just new york in the thirties, its the way the whole picture is organized, a whole hierarchy of what counts and what doesn't count that might present itself as neutral knowledge but is really just an ideology of power/ and the imaginary moment that you crystallized, the moment when the statistics and the analysis began to represent the true and the real against the superstitious, was the moment of self-denial and treachery as you implicitly agreed to a test of truth that would count out most of what you knew most deeply, even if you can't prove it."

Gary Peller, "Violence and the Mob" Tikkuyn, 1987

This self-conscious and stylized reconstruction of a supposedly spontaneously produced, authentic memory is the introduction to Gary Peller's 1987 essay, "Reason and the Mob: The Politics of Representation." In this essay, appearing in *Tikkun* magazine in its first year, Peller laid out the claims for, and methods of, deconstruction as a mode of interpretation and critique. Nineteen eighty seven was a heady moment in legal scholarship, at least for the community of progressive legal scholars. Critical Legal Studies - of which Peller was a major player - was at its zenith and moving out into the popular culture, with articles in such venues as the *Nation*, the *New Yorker*, *New York Times*, and the *New Republic*. The media attention - whether positive or critical - was a sign that this radical critique of law and legal institutions had reached beyond the ivory tower, out of the vaulted law school corridors.

At the same time, the cultural turn was just gaining adherents, developing scholarly potential, as well as securing occupational niches. Interdisciplinarity was emerging as the new buzzword, not only among interdisciplinary scholars but even among administrators and thus among disciplinary strongholds.

Similarly, ethnographic and qualitative analyses were expanding out of anthropology and the backwaters of sociology into quite diverse fields; and, importantly, from my perspective, attention to the meaning of action, not only its distribution, was becoming more and more important as an antidote to mindless quantification and ideological modeling in the heart of what claimed to be value free social science.

Many legal scholars, and social scientists, were particularly impressed by the erudite performances of literary scholars, for their ability to unearth complex and subtle meanings in representations of human action. And thus, arguments about the connections between literature and social science, literature and law, literature as a map of social life were becoming increasingly persuasive. Literary criticism seemed, to some, to offer a way of both accessing and representing the new interest in culture. Some describe this intellectual moment as a significant rupture in modernity and modern scholarship, others refer to it in terms of warfare: the culture and science wars.

At that moment, Peller's didactic essay offered - at least to me - an entree, a window on what seemed like important transformations, and powerful insights. I chose Peller's little stream of conscious representation to introduce my comments for this workshop because when I first read it years ago, it seemed emblematic to me of my own journey as a scholar, why I teach research methods, and why I happily study in the field of law and society. I was, in some ways like Peller, someone who began from very similar circumstances - also listening to grandma and grandpa in the kitchen, captivated by their energy and passion, warmed by the mist of tea served in tall glasses, and intrigued by the stories of political battles I could not really understand. Peller's account seemed familiar to me in terms of the family dynamics he suggested, and indeed in terms of the seduction and transformation in consciousness and capacity that his college education seemed to produce - at least until his subsequent rejection of it. And like Peller, I too was attracted to literary theory and to what seemed like powerful methods for tracking the complex meanings in human relations. But, despite significant parallels in Peller's account and one I might construct of my own childhood and education, there are also big differences, differences that derive, I believe, from the attachment, confidence, and pleasure I find in social science, especially a social science of law, and in the kind of public scholarship and engagement it signifies for me. I believe that

social science offers the possibilities of just that engagement, connection, reason, and kind of truth telling that Peller sought, but somehow failed to find.

Unlike Peller, I didn't experience my college education - - way back then when I was in college or now as I reflect upon it, nor my graduate education for that matter -- as alienating abstraction, disembodied, unsweaty, or as self-denial or treachery of things that I knew most deeply. Nor have I come to understand what I learned in college as just other mythologies, just more prejudice and superstition. Neither, by the way, did my education encourage me to reject connections to my family's world.

Quite the opposite. I experienced my college education, and much of my work ever since, as a comforting liberation. I say liberation not only because I was freed from unwanted constraints, although I certainly was. Nor was it liberatory because I perceived something "full of clear light," as Peller says. Nothing was clear - not my grandfather's politics, nor my professor's lectures. I believe that my education was both liberating and comforting because I was offered an alternative world, a new kind of security, something that happily substituted for the warm tea in tall glasses and family chatter in the kitchen, but which also happily lacked the unreasoned authority, prejudice, sentimentality and superstitions that confounded and oppressed me as a young girl of moderate intelligence, insufficient beauty, and too much energy growing up in Brooklyn NY in the 1950s and 60s.

By my young middle age, I was grateful for not having been an adolescent star within my family or school; I came to believe and still do, that my outsider status as a child and young woman made me a much more astute observer of social situations and structure. Unlike the confining world of my family, what I learned in college was more general than idiosyncratic, more collective than secret, frankly less lonely, more predictable, and stable than what passed for received wisdom in my household. This new world validated me, my experiences of fairness and unfairness, my intuitions about the injustices of powerful authorities, the banality and oppressiveness of gendered expectations.

I grew up in a world where salt thrown over your shoulder protected you from the evil eye (I was never sure whose evil eye), where those evil eyes could cause financial as well as bodily damage, where wet bathing suits caused kidney problems, where young girls who were not married at twenty were problems for their family, where women did not have interests in politics, or justice, or what we would call the public sphere, and where any woman who had ideas (and voiced them) about politics and the public sphere was simply not a good woman.

I am not making a specifically feminist point here. I am merely using my experience to illustrate what I know many men also experienced - living in worlds they could not understand nor influence very well, but which they both loved and felt outside.

Along with the warm fuzzy memories, I had also experienced my childhood, as I have said, as tyrannical, cruel and unfair. A large part of it was unfair because, I didn't seem to have any influence, or what we might call voice, and because it seemed unpredictable. One could never really know why one thing was good or bad, preferable or to be avoided. Why could I drive my car - and I was old enough to drive and privileged enough to have my own car - could drive it anywhere within the boundaries of NYC but never cross the Hudson

River, that meant I could go to Harlem, Brownsville, the highest crime neighborhoods within the city but not to the Princeton University bookstore. These rules seemed like and were expressions of the blatant, absolute power of merely older persons whose authority was constituted solely because they were older - parents, teachers - none of whom seemed to see who I was. I was one of those obnoxious, but angry little children who would regularly decry to these powerful figures, including my older sister, "just because you are older, doesn't mean you are smarter or know more." I was their little problem, and they and I knew it.

So, when I got to college and discovered political science and then sociology and then anthropology, I thought I had found nirvana. Here, it not only was possible but was expected to have arguments about what was just or fair, who should be heard and how they should be heard, what was true, what was not plausible, and what constituted the good. I loved studying philosophy and when I discovered the logical positivists and analytical philosophers, I thought I had been handed Excalibur. I thought that through empirical research, we could come to shared conversations, if not conclusions on these questions.

Unlike Peller, my college education transformed me but did not alienate me. It offered a new home, one that was less partial, less particular, less idiosyncratic than the world of my family. And because this new world of social science was more open and becoming increasingly transparent to me, and because uncertainty was recognized and valued, it was liberatory. It was liberatory because it offered the security of community, of boundaries, and yet of action and imagination - what I like to think of as method. Not anything I said or wrote, or anyone else, would constitute a reasonable account. There were shared expectations of how to go about constructing one's accounts of the world and those methods - quite diverse and varied, including surveys and models, and narratives, and deep ethnographies - were open to inspection and interrogation. They were shared, collective and public, debated, subject to imaginative reconstructions and deployments.

My point is simply this: I believe that knowledge construction occurs collectively, and publicly. Indeed, I don't simply believe this but I think we can show this. Moreover, the collective, public constitution of science, including social science, has a built in politics, a built-in justice claim. What we know, through normal science, we know because we have created it through a relatively open, and I stress relatively not absolutely, participatory community. However, to the extent that we - as scholars - do not explain to our audience how we produced our accounts, we might as well return to the world of authority, or unreason.

To the extent that we hide the ball so to speak, we are engaged in a more rather than less, private and personal activity. We are left to perform or display ourselves, a solipsistic, perhaps even narcissistic self-referential mirror, but we do not offer knowledge to be shared, exchanged, acted upon. To the extent that we study law, with its aspirations for justice, I think we have an obligation to enact those very same collective aspirations through our methods of study - open, transparent, participatory, thus public and accessible.

Liberation consists of knowing the rules, whether of a powerful apparatus such as the law, that is rules of public coercion, or the rules of reason and knowledge production. I imagine that Peller might have been disappointed in the enlightenment of his education because it did not reveal the rules of its construction - It might as well have been revealed truth. Thus, I was drawn to Peller's

account because we shared an attraction for literary techniques and Peller, unlike many, does not simply perform his deconstructive reading, but he instructs his readers on how to do it, he does not hide the ball. He does not offer revelation, but demonstration. Thus he taught me and I was grateful. But, unlike Peller, the more I encountered and was surrounded by interpretive scholarship that was, for want of a better word, performative, the more I longed for normal social science. By normal social science, I do not mean mindless counting, or atheoretical description. By performative scholarship, I refer to what are often engaging, artful, seductive displays of erudition, insight, and playfulness that are offered without a display of or map of its construction.

Thus, my thought about how to improve qualitative methods is simple and banal: make them transparent, as much as we can. Of course, there will always be leaps of intuition, serendipity, inferences that are not entirely supported, but we should, nonetheless, explain to our readers, and especially to our students, how we came to the conclusions we offer. In this regard, I have several worries and suggestions.

(1) *Teach more qualitative methods courses.* I recently surveyed all the major sociology and anthropology departments to determine whether they offered courses in qualitative methods and/or ethnography. It turned out that although several universities across the nation provide excellent training in fieldwork and ethnography, it is not as common or consistent as is the preparation in quantitative data collection and statistical analyses. The major sociology departments offered at least one but did not require such a course although they all required courses in general research methods, another in statistics and a range of offerings in advanced quantitative analyses. Anthropology departments did not regularly offer nor require fieldwork methods, and political science rarely offered courses in qualitative data collection or analysis. Although case studies and historical studies are not unusual, the methods courses were more often survey methods. So my first worry is that we cannot create more transparent methods if we do not teach them as regularly, or consistently.

(2) *Pay attention to reliability and validity.* My second worry is that in the well-founded critique of positivistic behaviorism, qualitative scholars have too easily ignored collective and reasonable criteria for reliability and validity. In rejecting a correspondence theory of truth, we have left ourselves open to criticism and rebuke. Thus, although the use of qualitative research methods has grown in recent decades and writing about qualitative methods has also proliferated, there has been "no parallel proliferation of studies of the actual process of doing qualitative research (Huberman and Miles 2002:x). The cause of this inattention to the process is over-determined, a product of ethnography's own history, the epistemological debates between different approaches within qualitative methods no less between qualitative and quantitative researchers, as well as the culture and science 'wars' of the post-structural turn in the social sciences I mentioned at the outset. As a consequence, the processes of data collection and analysis, as well as the distinctions and connections between these, are not well understood, especially among non-practitioners. The publication of many new texts and handbooks does not seem to have helped. Often, one observes a general lack of credibility for the results of qualitative research. When researchers "spend more pages explaining why they will not deploy particular methods than on describing their own conceptual and analytic moves, they leave behind too few footprints to allow others to judge the utility of the work, and to profit from it" (Huberman and Miles 2002:xi). Thus, it seems important for improving qualitative methods that we begin to unpack the process and make it more

transparent. This is as important for the collection of data as for the analysis, which in ethnographic fieldwork is always continuous and simultaneous with data collection (Becker 1998, 2004; Silbey 2004).

One of the most frequently voiced concerns about ethnography is whether a different observer would have come away from the field, independent of the variations in the voice with which the account may be written, with the same basic account. In other words, how reliable is this description of the social world depicted in ethnographies. In ethnographic research, however, reliability is closely connected and perhaps best understood as a form of validity (Hammersley 1992:79). Although these terms are normally reserved for quantitative and positivistic research, I use them to refer to the ability to produce similar data from multiple observers, and to produce consensually agreed upon, corroborated, accounts of a site, person, or process. They can be deployed for ethnographic research with some modifications. In a succinct account of these, Maxwell (1992) for example, proposes five types of validity for qualitative researchers that offers an advance on the usual discussions of reliability and validity. *Descriptive validity* refers to the factual accuracy of an account, that researchers “are not making up or distorting things they saw or heard” (1992:45). This is the basis for all other forms of validity. As Geertz (1973:17) put it, “behavior must be attended to, and with some exactness, because it is through the flow of behavior – or more precisely, social action – that cultural forms find articulation.” This “reportage” (Runciman 1983) refers to specific events and situations, as well as to objects and spaces. As Maxwell (1992:46) says, “no issue of generalizability or representativeness is involved. These are matters on which, in principle, intersubjective agreement could easily be achieved, given appropriate data.”

Interpretive validity refers to representations of what the described behaviors, events, and objects mean to the actors observed, one of the central goals of qualitative research, especially ethnographic fieldwork. Interpretive validity seeks, in one conventional framing of qualitative research, to capture the participants’ perspective, description in emic terms. Interpretive validity “has no real counterpart in quantitative-experimental validity typologies... [It] is inherently a matter of inference from the words and actions of participants in the situations studied... grounded in the language of the people studied, [and] relying as much as possible on their own words and concepts” (Maxwell 1992:48). The goal of interpretation is to describe the actors’ “lay sociology” (Garfinkel 1964) or “theories-in-use” (Argyris and Schoen 1978). This criterion of interpretive validity distinguishes a form of accuracy that lies between the first form, descriptive validity, resting entirely on observable data and the more contestable interpretations of the third, theoretical validity, to which I will turn next. While there is “no in-principle access to data that would unequivocally address threats to [interpretive] validity,” the descriptive accounts serve as warrants, and consensus should be achievable within the relevant community of actors about “how to apply the concepts and terms of the account” (Maxwell 1992:49). The concepts and terms of both descriptive and interpretive validity are, in Geertz’s term, “experience-near,” the local language in use among the actors, although interpretive validity might also involve assessments of the accuracy of informants’ reports. “Accounts of participants’ meanings are never a matter of direct access, but are always constructed by the researcher(s) on the basis of the participants’ accounts and other evidence” (Maxwell 1992:49).

Theoretical validity moves the ethnographic account further from the actors' behavior, language, meanings, and interpretations to a more abstract account that proposes to explain what has been observed. "Theoretical validity thus refers to an account's validity as a theory of some phenomenon" (Maxwell 1992:51). Both the concepts used and the relationships proposed are independently assessed for what is conventionally called construct validity (Bernard 2000:50-51) and inferential or causal validity (Cook and Campbell 1979), although not all theories proposal to explain causes. The key distinction between these types of validity, in this schema, lies in the "presence or absence of agreement within the community of inquirers about the descriptive or interpretive terms used. Any challenge to the meaning of the terms, or appropriateness of their application to a given phenomenon, shifts the validity issues from descriptive or interpretive to theoretical" (Maxwell 1992:52). *Generalizability* is refers to "the extent to which the particular situation is representative of a wider population" (Hammersley 1992:79). There is, however, a level of analysis issue here concerning generalizability that distinguishes internal from external validity. To what extent were the observed persons and activities representative of that particular group or organization? This "internal generalizability is far more important for most qualitative researchers than is external generalizability because qualitative researchers rarely make explicit claims about the external generalizability of their accounts." As Friedson (1975) writes in warranting qualitative research, "there is more to truth or validity than statistical representativeness." Nonetheless, we need to be careful about the claims made, implicitly and explicitly, for generalizability, internal and external. Qualitative researchers normally elide external generalizability by offering the particular case as an example from which to generate typologies and hypotheses rather than test theories. Maxwell offers a final fifth form of *evaluative validity*, referring here to the normative assessment of that which has been described or explained. This category is perhaps most appropriate for policy studies and are not intrinsically different in qualitative than quantitative studies.

(3) *Make social science a collective, public enterprise. Teach and do fieldwork in groups.* One of the special features of graduate education in science is its collective form. Scientists work in groups. Laboratory groups vary in size: number of actual rooms and researchers. The research done under each professor's supervision is conventionally referred to, depending on the particular speaker, as "a lab," "our lab" or "Johnson's group," invoking the name of the faculty supervisor. The faculty member or principal investigator bears responsibility for designing the research agenda, for selecting the researchers (technicians, post-doctoral fellows, graduate and undergraduate students). Laboratory groups normally meet once or twice a week to discuss work collectively, to present results to each other, to comment on papers. Group meetings are key for developing the interpretations of data, as well as the camaraderie in the group and the authority of the supervising faculty (Owen-Smith 2001). One's identity as a scientist is tied intimately to this group and its advisor, with successions of faculty and students describing themselves as kin, often publishing their genealogies on their web pages. Although the genealogies highlight, in bold color, a Nobel prize in the line, it is not unusual to find such lineage charts going back much further, securing a direct link between a contemporary scholar and one of the 17th century founders of modern science. The laboratory group is a family, not only as source of identity and authority but also as a supporting network. Social scientists enjoy this support less often, and humanists almost not at all. We would do well to consider the benefits that accrue to teaching students in groups, meeting and discussing research in groups. In this way, we would demonstrate in the very training of social scientists that this is a public, collective enterprise, not solely a matter of individual talent,

insight, or creativity. We would, by working and teaching in groups, create the conditions for articulating the grounds of how we know what we know.

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