Methods for Mortals: Sociology for a New Sociological Methodology

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Abstract

Three challenges haunt all social research: how to minimize the temptations to professional narcissism when intermediating the relationship of readers and subjects? How to test findings on the understanding that the social world of prior studies can never be reentered? How to rationalize the inevitable leap beyond the geographic boundaries of what one has studied? Relative to the quantitative researcher, the qualitative researcher remains naked in the face of the seductions of self-reflective imagery, the hubris of denying the ceaseless passing of the known social world and the absurdities of generalization. But qualitative researchers can find a logic for their methods by shaping the sociology of the research process to create relationships with readers and subjects that challenge narcissism, draw readers into an ongoing research process and avoid the concerted ignorance of academic clubs in favour of empowering subjects to falsify claims.

Introduction

Methodological issues about social research are usually discussed in a rhetoric that ignores what goes on in the empirical implementation of formal research logics. How can we develop a perspective that answers methodological questions while taking account of the unique relationships with subjects and readers that can distinguish sociology from other ways of representing the social world? I first tried to spell out such a methodology in 1976, as an appendix to my dissertation. The premise was that methodological discussions are artificial when, as is typically the case, they are about worlds of immortal logic, offering standards that float independent of empirical understandings about how research is or can be done.

Qualitative social research fares poorly when examined by universalistic methodological thinking, for example when assessed on the criteria of replicability, reactivity, representativeness and reliability. My dissertation was an ethnography, and when I examined it with what I summarized as the ‘4 Rs’, I could not find a methodological
rationale in the literature. The response I developed was to reset methodological discussion within a sociology of the research process. While especially necessary for reconstructing a logic for qualitative research methods, an empirical approach to methodology should, like all sociology, be understood in a comparative perspective. Indeed, some of the most sophisticated thinking along these lines has been offered by preeminent quantitative researchers (see Duncan 1984).

In light of the probing commentaries on my initial methodological statement that editor Ouédraogo solicited, I revisit the discussion in order to bring clarity to a variety of issues that have long been unnecessarily obscured. I will again comment on the 4 Rs, but within a more general appreciation of the existential challenges that a sociological appreciation of social research raises for methodological thinking. I propose that we think about the methodology of social research by considering three challenges that haunt all social research: how to avoid succumbing to the temptations to professional narcissism that arise in the performance of the quintessential research role of intermediating the relationship of readers and subjects; how to test findings on the understanding that their social context can never be reentered; and how to understand and rationalize the inevitable leap beyond the geographic boundaries of what has been studied.

There is nothing superior or inferior about the answers that qualitative research can give to the fundamental existential challenges of social research. But conventions for conducting quantitative research routinely throw up a series of screens that provide an insulation denied to the qualitative researcher, whose stark confrontation with methodological dilemmas can be intimidating, leading many to avoid all methodological thinking. The qualitative researcher, having nothing more than a few flimsy conventions to hide behind, remains much more naked in the face of the absurdities of generalization, the seductions of self-reflective imagery, and the hubris of denying the passing of the social world that has been studied. For qualitative researchers, the only way to find a logic for their methods is on the grounds of a generally applicable sociology of the research process.

**The temptations of professional narcissism**

About fifty years ago, a sociological ethnographer, Julius Roth, warned of the dangers of ‘hired hand’ research (Roth 1966). Describing his experience and the behaviour of his colleagues in a research project, he detailed patterns of cheating that emerge when labour is divided between authors of social analyses and those gathering the data that the authors will use. Like any employees, agents hired to gather data have their own interests and, if sociology has any firm findings, one is that tensions arise between employers and employees, and the tensions are not always perceived correctly if at all by the employers (see also Becker 1967).

Using technical language, these methodological problems can be discussed as those of reliability, the data gatherers varying their conduct in ways that are not reported, but that elicit significant differences in the data produced. Sometimes the social processes
involved may be discussed as problems of reactivity. The data gatherers evoke from subjects responses to their appearance and research behaviour that are hidden behind the data produced.

For qualitative researchers, it may be more useful to collapse these methodological issues in a discussion of the temptations to professional narcissism that haunt all forms of social research. The research employees that Roth had in mind were shaping and avoiding interactions with research subjects in order to produce a strategic version of themselves. Roth and his colleagues turned from engaging with the portions of social life they were supposed to code. They cut corners and saved time by making up data and by otherwise compromising their research commitments. Straying from the objectives of the research project, they opted to produce records so that employers who would review their work would see them as having acted as competent agents in a research project.

Roth thought he had found a weakness specific to research projects implemented through a complex division of labour. He suggested that ethnographic research, at least when conducted as a one-person operation, would be exempt. But all forms of social research are bridging work. David Riesman, writing at mid-twentieth century, famously characterized sociology as a ‘conversation between the classes’. The researcher is a specific kind of communication medium, a translator. Translators, however, are not unbiased agents (Ghazala 2002). In courts, for example, they translate less as a witness’s muse than in anticipation of the needs of the official, institutionally dominant language (Morris 1993). In methodological writings on anthropological ethnography, a parallel observation, and parallel critiques, emerged in the wake of colonial retreats.

Roth’s critique created no shock for quantitative researchers’ methodological understanding. Multiple techniques had and would continue to be created to detect and correct for variations in subjects’ responses to demographic differences among data collectors, variations in what data collectors do with different subjects, and variations between what given data collectors do early and later on in a sequence of steps used to elicit responses or record observations. These include running statistical checks that compare the patterns in data produced by different data collectors, videotaping interviewers, creating ‘experiments’ by varying the ordering or wording of questions in a survey, as well as making protocols and data sets available to subsequent researchers so that they may try to replicate the findings.

Yet social reality continues to escape the surveillance, auditing and managerial design of research practice. A recent example occurred with a widely respected and disseminated finding about the historical rise of ‘social isolation’. As new technology produced alternatives to face-to-face communication, people were said to decrease their participation in social groups in favour of what Robert Putnam dubbed ‘bowling alone’. This view became so popular that it was said to have created a ‘moral panic’ about new communication technology. But it turns out that key findings cited in support of the ‘social isolation’ hypothesis were the product of interviewer ‘fatigue’, a polite
word for what others might label employee malfeasance. The critique may be taken as heartening, as it indicates the viability of conventional methodology for detecting and correcting distortions. But it took five years before the suspicion was authoritatively raised (Fischer 2009) and another five years before the research behaviour responsible for the ‘artefact’ was identified as the cause (Paik & Sanchagrin 2013). It appears that new communication technologies seduced readers and researchers to the charming belief that when they pick up their ‘i Phones’ and touch the send key on personal, substantively insignificant text messages and emails, they are participating in a massive, macro-social change.

Qualitative researchers are fond of pointing out the soft underbelly of the most sophisticated quantitative research operations. Ethnographers usually work as independent researchers when collecting data, so they do not necessarily see the implications when they point to employee alienation in hired-hand research. But qualitative researchers cannot obtain their data without being enmeshed in an intermediating social interaction, and in one way or another, they also are employees. Can it be that in the process of ethnographic research, the originating purpose of representing the social lives of subjects becomes distorted as the researcher/author becomes charmed with the image of self he/she is crafting for home audiences, which will often include university employers and sometimes other paying customers?

Margaret Mead achieved fame when her book on the sex life of (American) Samoans appeared in the late 1920s (Mead 1928). She was seen as making the case for cultural relativity, a basic warrant for anthropology, by arguing that for Samoan adolescents, sex was a matter of joyful promiscuity, and that the contrasting moralistic constraints on the sex lives of Western adolescents were repressive for reasons not rooted in what society must demand of human nature. The study vaulted Mead to the status of an admired public intellectual and a respected academic. About forty-five years later, Derek Freeman (1983, 1999) argued that she had been duped by the joking practices of her subjects, who freely enjoyed outrageous sexual humour but were subject to sexual repression in ways that paralleled and in some ways went beyond what Western adolescents had to endure. Freeman had conducted his own field research in (Western) Samoa, about twenty years after Mead’s fieldwork. His argument essentially was that Mead was taken in by the attractions of sculpting a self-image as the bearer of liberating news, an image celebrated widely in her home society but for decades unknown and, when known, rejected in Samoa. Freeman in turn was criticized by reviewers who found that he was taken in by the siren call of an image as a heretic, too eager to make his historic place in the annals of anthropology, which he built not from the significance of his own findings but from attacking Mead without sufficient empirical foundation. In particular he was faulted for not taking into account historical change shaped by Christian missionary efforts in the time between Mead’s and his own research (Shankman 2009).

The self-critique of ethnography continued in anthropology through the wide reception given to analyses of the ‘genre’ requirements for effective ethnographic writing. Clifford
and Marcus (1986) led a new industry of textual criticism, which highlighted the canons of verisimilitude that produced academic success. Whether or not readers were misled in the substantive understandings of subjects’ lives that they took away from ethnographies, it was clear that researchers/writers were transforming their encounters with subjects into portraits convincing to readers in ways that unwary readers had not been suspecting.

The upshots of this critical moment in the history of ethnography have been multiple. A field of study opened up: the work of representing social life is now understood as a social institution in its own right. By bracketing the truth of the matters asserted in texts, the techniques of construction can be systematically studied. If we take a Marxian perspective, we can appreciate that (self-) critical anthropology has the attractions of creating a work setting in the comfortable territory of academic life. For ethnographers both in anthropology and in sociology, data gathering has usually entailed living in relatively rustic or impoverished conditions of work, and/or assuming fly-on-the-wall diminishments of selfhood. Reading and commenting upon texts makes for an everyday work life identical to that lived by others who do high status work.

Another upshot was to create a powerful social institution for sustaining doubts about the veracity of accounts of subjects’ lives. Many understand that anthropologists turned self-critical as a result of historically changing political pressures: opposition to colonialism, the rise of independent voices for the subjects they had studied, a bristling over the common racial differences between anthropologists and their subjects, and so on. But a political explanation is too easy. In sociology in the 1960s a parallel self-critique emerged without these macro social trappings, in the work of the ‘ethnomethodologists’. Extending European phenomenological thinking to the American social science that constituted the world they worked in, Harold Garfinkel (1960), Aaron Cicourel (1964) and many who trained under their influence took as their object of investigation the methods by which social researchers – usually psychologists and survey analysts – glossed over the interactions that produced their ‘data’. The ethnomethodologists’ examinations of the taken-for-granted, unreported actions that produced data and substantive findings were of a piece with their examinations of how people in everyday social life created the ‘grounds’ for their actions, as well as how officials, like the police, and professionals, like doctors, constructed the factual basis of their empowered or esteemed work. Unlike the self-critical stance that the anthropologists’ took, the ethnomethodologists neither saw themselves as critics nor as specifically concerned with illuminating the practices of social researchers. They sought to build on the understanding of social ontology as previously developed by Alfred Schutz and Maurice Merleau-Ponty: their field of study was everything social.

Academic sociologists, however, saw them as critics of sociology, and the reaction was alternatively to treat their writings as incomprehensible, damn them as solipsistic and maintain a lingering fascination that they might be on to something. Neither the ethnomethodologists nor their critics produced a ‘solution’ to the methodological problems that the ethnomethodologists had revealed. The upshot of this now-dying movement in sociology was an ever-receding quixotic battle with academic sociology, and a sterility
stemming from the dilemma that the ethnomethodologist’s acuity in raising questions about the empirical foundations of any social actor’s understanding of another could be turned on the ethnomethodologist to ask how he/she had firm grounds for asserting an understanding of the behaviour he/she would treat as problematic.

The ‘hoist by your own poitard’ problem was manifested in a series of critical studies that became self-inflicted wounds. One example is Weider’s (1974) frustrated attempt to study convicts in a halfway house. It became an essay on ‘the convict code’, or ways that convicts prevent outsiders from penetrating their world. But kept on the outside, Weider could never adjudicate whether his uncertainties about the convict world were produced by the convicts, by his idiosyncratic ways of inquiry, or by his unverifiable faith that there was something they were hiding.

Bourdieu’s student, Wacquant (2000), offered an advance by advocating mastering the ‘habitus’ through which the subjects routinely produce their behaviour. This parallels the thrust in ethnomethodology to a ‘unique adequacy’ requirement, or becoming an adept in specific instances of competent performance. At its best, this means giving readers a path they might follow to obtain practical competency (e.g., Sudnow 1978). But the reader who is not an adept obtains little, if all that is offered is certification of the researcher’s accomplishment of having become a competent member. Establishing that a habitus must be acquired does not convey of what the particular habitus consists. Habitus is a dynamically embodied manner of shaping situations to elicit responses that facilitate continuing a pre-disposition. Evidence that only attests to results, such as still photographs that depict accomplishment, suggests a retreat to narcissism instead of an embrace of the challenge to describe the progressive acquisition of embodied competence.

Within academic sociology, critiques of narcissistic tendencies in writing ethnography emerged in parallel to the self-critique in anthropology. These include Gusfield’s (1976) analysis of the ‘literary rhetoric of science’, Van Mannen’s (2011 [1988]) deconstruction of ‘the realist genre’ and Becker’s (1986: 26–42) debunking of the ‘classy’ authorial persona. But in ways the anthropologists have not appreciated, these writers themselves represented a tradition that had its roots in the pragmatist philosophical foundations of American sociology, which from the start rejected the dilemmas that late twentieth-century critics attributed to positivism.

Psychologist/philosopher John Dewey published his article on the ‘reflex arc’ in 1896, arguing that the stimulus of visual perception is constituted by the perceiver. The point was transferred to sociology by Mead, who taught the first generation of sociologists to appreciate that a construction of self is always achieved through how the other is perceived. The assumption that the ethnographer could approach any social setting and register its nature as tabula rasa, with an empty subjectivity that could register the objective world, was alien to this perspective.

Often understood as a rejection of metaphysics and philosophy for its own sake, the pragmatists promoted a democratic dynamic in science, a vigorous array of interactions
among researcher/writer, subjects and readers. Through William James, the ‘reflection’
theory of truth was rejected in favour of an understanding of truth-in-the-becoming.
James took a practical stance that embraced the scientific attitude, not only of always
assessing evidence of different versions of the truth but of entertaining seriously even those
hypotheses that might be dismissed as unscientifically mystical, spiritual or transcendent.
James, like Dewey, wrote in a way that reached beyond academic audiences so as to
engage the thoughtful lay reader in the advancement of knowledge. The key to engaging
lay readers was, and is, providing the reader with access to the problems addressed,
and providing this access independent of the author’s answers. Dewey raised issues by
contrasting the idiom of ‘science’ and ‘common sense’; James wrote phenomenologically,
highlighting experiences that the reader would have had before entering his writing.

For good historical reasons, ethnography in anthropology was initially a way to produce
social knowledge in which a triadic structuring of interaction among reader, subject and
researcher was deeply compromised. In the early twentieth-century texts, readers could
only see the anthropologist’s subjects through the author’s lens. That made pragmatic sense
when texts were providing a first mapping of social worlds previously not encountered by
readers. The anthropologists’ objective was to understand social patterns independent of
proselytizing or colonial economic and administrative objectives. Readers did not blanch
when reading an ethnographic monograph in which not a single human being was
visible. As a first sketch, the ethnographic style of holistic modelling was useful; to pick
one or a handful of activities, like entering a house, and then to build up analysis through
the examination of dozens of instances, would come later (e.g., Frake 1980).

But for sociologists, research sites and alternative ways of representing their social
realities – through journalism, fiction, diaries, the records of social agencies, etc. – were
always readily at hand. Park, who had been a journalist, embodied the competitive
knowledge context in which sociology emerged. As a response, sociology’s ethnographers
from early on understood that they must not only write in a distinctive ‘scientific’ manner,
but more importantly, that they had to present ‘data’ that would empower readers to
analyze their claims critically. Thus Thomas and Znaniecki’s (1918) multi-volume study
of ‘Polish Peasants’ in Chicago and in Poland contained a wide variety of descriptive
materials created independently of the researchers and made available to the reader
independently of the authors’ theory. These ranged from descriptions of murders taken
from police files to personal correspondence reflecting life in the US and in Europe.

In the late 1930s, the Social Science Research Council in New York commissioned
Chicago sociologist Herbert Blumer (1939) to assess the Polish peasant study, which had
been taken as a model for scientific sociology. The result was explosive at the time and,
while that critique is now rarely read, it has had enduring influence. Blumer essentially
compared the data, which were reproduced verbatim in the voluminous original texts,
and the claims made by the authors. Later W.I. Thomas would acquire a reputation as a
sociologist with an interactionist bent, but with Florian Znaniecki he offered to understand
the sociology of his subjects with a theory of ‘Wishes’ that was psychologistic in nature.
Blumer, who later would write influential critiques of ‘attitudes’ as explanations, effectively depicted the authors as themselves wishful, writing elaborations of their theory that patently were not supported by the material presented as evidence, as any reader—whether professional academic or layperson—could see.

The critique was seized upon by survey researchers, who were rising on the East Coast to compete with Chicago’s ethnographic tradition. But if some took Blumer as damaging the scientific status of ethnography, the students he and others subsequently trained at Chicago took away another lesson. For them, writing ethnography in a way that empowered the reader to assess the author’s claim was a sign of the genre’s methodological strength, just because it allowed lay readers independently to undermine an author’s claims of authority.

To generalize the methodological lesson, we need to focus not on any one technique or device but on how different ways of representing social life set up different social relations, in particular whether they set up a more or a less effective triadic interaction among reader, subject and author. The test of professional narcissism is whether the reader can confidently see or access the subject independent of the author’s version of their realities. The narcissist controls access where he/she could give it up. One way to liberate the reader from seeing the subjects only through the author’s theoretical or analytical lens is by presenting blocks from field notes or quotes from interviews, so that readers can perform an examination of the author’s claims, much as had Blumer when examining The Polish Peasant.

Often, readers of qualitative texts in social science cannot be sure what the author is writing about without entering the author’s interpretive world. Geertz’s ‘thick description’ is a misnomer that continues the anthropological tradition of relative indifference to empowering the reader. ‘Thick interpretation’ would be more accurate. Thick description would entail describing multiple episodes of cockfights and arguing for an analysis that the reader could assess against the various cases. Of course, the author remains in charge of which cases to select and how to edit them, but as a matter of fact that any ethnographer can verify by trying, the exercise of separating analysis from evidence often embarrasses an initial formulation, requiring revision or qualification, and sometimes a new start. At the centre of the methodological strength of qualitative research is a phenomenological feature of research, the author’s experience of constraint through a process of anticipating readers who will be able independently to compare analysis and evidence. Readers cannot supply that constraint if data and analysis are so merged that the author can respond to criticism by claiming that the reader has misunderstood the author and so has been looking for relevant evidence in the wrong place.

The requirement to display data separate from analysis was elaborated by sociology’s next generation of ethnographers. Boys in White appeared in 1961 (Becker, Geer et al. 1961). It was the product of an ethnography of medical students by a team that was headed by Blumer’s departmental colleague, Everett Hughes, and included Anselm
Strauss, who became a well-known editor of Mead’s lectures at Chicago, and two students of Hughes and Blumer, Howard S. Becker and Blanche Geer. From that study Becker (1958) articulated a practically useful methodological distinction. The researchers had become aware that what the medical students said and did when a student was only with one of the researchers often differed from what a student said and did when in a multi-party interaction that the researcher was observing. In assessing the descriptive evidence for asserting a proposition, Becker weighed the latter more heavily than the former. The social situation in which the observation was made or the interview done would be taken into account when assessing the evidence for propositions.

The researcher would now be compelled to consider whether subjects were playing to an image of themselves that they would like the researcher to have, or whether as they acted they had other business at hand, such that they were compelled to put aside a focus on the researcher’s project. In effect, by treating data as specific to the situation of its acquisition, Becker put narcissism to test in qualitative research. In the first instance the narcissism was that of the subject. In the second instance, however, the requirement to analyze and present the reader with the situation in which the researcher obtained his/her description of subjects’ perspectives put the researcher’s narcissism to the test.

The ways of undermining professional narcissism continue to proliferate. Recently, high profile voices have pressed ethnographers to identify their sites so that readers can get independent access. Ethnographers too often have protected themselves from review by invoking promises of confidentiality and hiding behind the requirements of anonymity imposed by research ethics review committees. When readers do the detective work to go back to imprecisely identified sites, what they find can be as unsettling as Blumer’s critique was seventy-five years ago (Duneier 2004).

When technology produced low-cost, high-quality audio-visual recording devices, it brought an extraordinarily powerful challenge to the attractions of shaping analysis to fit in with ongoing, prestigious academic debates. The sociolinguist was transformed from a semi-philosophical commentator who relied on a more or less aristocratic sensibility in analysis, to a worker whose competency was crafted through long stretches of meticulous data analysis. A further constraint on the researcher developed as subjects were empowered to certify the understanding that the researcher had achieved of their world. I refer here not to what some praise as ‘giving voice’ to the people studied, which often neglects that the in situ expressions of research subjects are manipulative and self-serving, whether done for each other or for the purpose of making records of their lives. I mean the pressure on the researcher to learn how to do what subjects do, not necessarily as well as the most proficient but well enough that the published understanding can emerge from the inside.

Deference to a subject-as-mentor has long been a hallmark of sociological ethnography. From early on, sociological ethnographers, in distinction to anthropology’s, not only acknowledged named mentors but described them as chief characters in their texts, if under pseudonyms like ‘Doc’. What has changed is that ethnographers now can anticipate
that mentors may later emerge from behind the mask to offer critical reviews, not only in their own neighbourhoods, but also in scholarly publications (Whyte 1992). Increasingly, the subjects have voice politically, talking back in letters to the editor and complaining to ethics review boards that can sanction a university researcher. Subjects also talk more powerfully as folk sociologists. They make their own training videos, produce or collaborate in the production of documentaries that play to entertainment audiences, and map their worlds with software that may be more powerful than what the researcher can afford. Sociological ethnography now has to make its way among a variety of burgeoning institutions for representing society. The phrase one hears these days in the academic halls of ethnography, that by ‘theorizing the case’ one distinguishes one’s work from journalism, business tracking, surveying performed for political control, and other prose forms of representing the same social world, including seemingly ubiquitous video cameras that routinely operate without any commitment to write up what is recorded, gets thinner every day. Because the competition has increased, I think it is accurate to say that making a contribution through academic ethnography is getting harder all the time.

A methodology for mortals puts aside the epistemological and metaphysical issues of knowing ‘the other’, treating such philosophically framed debates as cop-outs. For sociology’s ethnographers, the ‘other’ was never far away. Even when the ‘other’ was from a lower social class or a ‘deviant’ social world, studies garnered authority when they were conducted by a researcher who had crossed into academia from the other side (Anderson 1923). By the mid-twentieth century, the ‘other’ in US sociology has been within immediate reach, as a region of the self. When showing how their subjects present their selves to influence how others in their worlds see and respond to them, sociology’s ethnographers were cueing readers to see that they, the ethnographers, were doing something similar. In the 1950s and 1960s, while anthropologists were still composing ethnographies that left individual subjects invisible, flattened out to fit the tropes of analytical models, and detached from their historical context (Goodman & Silverstein 2009), sociology’s ethnographers were writing with an interaction sensibility that turned the reader’s gaze back onto the self-portrait that the author was indirectly composing through the images rendered of subjects. It is only by forgetting this history that ethnographic methodology can revert to immortal debates over abstracted tensions between subjectivity and objectivity, idealism and realism, solipsism and positivism. Qualitative social research has for about a hundred years increasingly made these methodological doubts matters of empirical investigation. Still the siren calls of philosophical self-reflections play loudly in the auditoriums of graduate schools, tempting novices to shape their version of a research self from an island of tranquillity existing somewhere outside of the dynamic and humbling interactions among researcher, subject and reader. This is the same preoccupation with metaphysics, and the same temptation to the narcissistic satisfactions of aristocratic ways of life, that the pragmatists were fighting a century ago under the auspices of what they invoked as the American democratic tradition.
Mortal scale and sociological hubris: on attempting to replicate a prior step into the river

If we examine the methodological standards for ethnography within an empirical perspective on social research as a social process, the usual evaluations change. Consider now the criterion of replicability. While generally considered a hallmark of science, replicability has different meanings, depending on whether it is conceptualized as an abstract standard or as a statement about research practice. It is a meaningful standard to apply to ethnography, one that gives useful guidance, but only after considering the distinctive social ontology of the phenomenon under investigation in each particular study.

‘Replicability’ is paradoxical. In abstract formulations, replicability calls for specifying protocol, such that a subsequent investigator can ‘redo’ the research. But the more specific the protocol, the more difficult to insure that a subsequent study is a replication. If replicability refers to checking a study’s results by trying to reproduce them, then the less demanding the protocol, the more likely verification will be attempted in fact. In weighing the two meanings of replication, researchers will often face a trade-off.

Ethnographic field research, especially a project conducted through participant observation, illustrates this paradox in the extreme. Participant observation depends on negotiating access with subjects. Access for data gathering is inevitably shaped by idiosyncratic features, including features of the researcher’s personality that he or she is unaware of and cannot practically change, and the sensitivity of those who control access to a research site, which may vary radically depending on the historical moment in which the study begins. If the participant observer will enter multi-party scenes, replication is unlikely, at least in the sense of repeating protocol. Research protocol will not be the only influence on what the researcher does.

On the other hand, when ethnographic fieldwork produces a micro analysis of ubiquitous phenomena, replication is feasible in the practical sense of testing whether an imitative study produces the same findings. Erving Goffman’s work exhibits an inimitable combination of personal talents: a humanistic perspective on personal autonomy in the face of the constraints of social life, a profoundly sociological sensibility for comparative analysis, and a concern to facilitate testing of his work by application to instances of social life accessible to readers. This combination of sensibilities guided not only his studies of the structures of social processes in everyday life but also his Simmel-inspired essays in comparative theorizing, which included his studies of the ‘cooling out’ processes that he found in the social processes of con games, the firing of employees and how intimate relations end; and his sociology of ‘total institutions’ like prisons, mental hospitals, merchant ships and convents, which are exceptionally difficult for researchers to enter, but which are also so varied and universally interesting that autobiographical, journalistic and fictional descriptions are widely available. No one could describe what Goffman actually did in his research; he left some files with newspaper clippings and a few scant
methodological reflections. But his readers readily could try to replicate his findings.

If we consider the granularity of social phenomena, we come across a second paradox of replicability. The larger the scale, the more ‘important’ the study in a conventional sense, and thus the easier to get resources (eager researchers, funding) to do a re-study; but the harder for a sociologist to replicate, in the sense of entering the same field twice, as the phenomenon is more tied to history. Consider the prospects of replicating studies of either crime or emotions, two phenomena that may be studied on both small and large scales.

On a ‘micro’ scale we can study the course and contingencies of various forms of common criminal offenses like sneak thefts, robberies and violent assaults. Each instance emerges and ends over a temporarily short span, in interaction situations that often involve only a few people. Working on a similar scale, we can study the rise and fall of episodes of laughter or crying. The ontology of the explanandum is well-suited to replication in the practical sense of the ability to test explanations of recurrent phenomena, in part because the short temporal and small social scale of the phenomena gives reason to assume that many ‘background factors’, such as gender, birth order, physical dimensions, the state of the economy, etc., remain constant while a criminal offense or a burst of laughter begins and ends. Relative to the methods used for studying larger scale phenomena, the materials are likely to be qualitative, and in that respect they may appear less scientific as they are less controllable by protocol. But testing is likely to be more scientific in the senses of infinitely repeatable, widely accessible to subsequent researchers, and less dependent on the necessity to rely on conventional political beliefs to support ceteris paribus assumptions.

Now consider the application of the standard of replicability to explanations of larger scale phenomena, such as historical changes in crime rates or variations in depression over the life course. In one distinctive sense such studies are easier to replicate just because of their macro scale. The rhetoric of political discussion and policy formation typically speaks in macro terms. Macro discussions shape the understanding of research priorities for the people who control funding decisions and the young people whose participation is necessary to keep research programs continuously alive across generations.

But in other respects, replicating findings from studies of large-scale phenomena is more difficult. There is greater need to appeal to the gods of ceteris paribus, or to rely on conventional beliefs in order to make sense of ignoring the many rival hypotheses that cannot be tested. If we want to explain a dramatic ten-year rise followed by a dramatic ten-year decline in the urban crime rates of the late twentieth century in the US, what did not change over that twenty-year period? If we wish to explain city crime rates, variations in the plausible explanans far exceed variations in the explanandum. The very idea of treating rates as city specific requires an initial, fateful bow to a particular set of political conventions. People move among cities, such that many people ‘at risk’ to offend in one city will have been affected by changes in many potentially important explanans, such as neighbourhood environment during early childhood, while they lived in other cities.
And many powerful influences on local life emanate from national centres to spread non-randomly over clumps of cities that share policy-relevant characteristics.

Sociologists have provoked spirited controversies by pointing to the methodological challenges presented by ‘small n’ phenomena (Lieberson 1991), and creative methodological thinking has grown in response (e.g., Ragin 2008). But even if there are existential limits on a phenomenon’s variation, such that the potential to replicate is severely limited or inconceivable, there is also an existential demand to know. The mysteries that animate cosmology, astronomy, and the discovery of the origins of biology’s species do not become less compelling as intellectual challenges because the phenomenon in question occur rarely or only one time, and so achieving replication, at least in the sense of testing explanations on new instances of the same phenomenon, is problematic. Likewise for changes in criminal offending that leave hundreds of people alive rather than dead in each big city.

The understanding that larger scale phenomena resist replication is not new but it remains news. When devising a methodology for ethnographic or qualitative research, it is critical to keep the pragmatics of replication in mind. The convention in academic sociology is to ignore these problems, with the result that the logic of qualitative research remains uniquely impaired.

Consider the progress toward existential limits that has been made as sociology has pursued its holy grail, the explanation of social inequality. Even today in the most educated societies the popular discussion of inequality is dominated by comparisons of populations over time, along the lines of, there is more inequality now than before, or that inequality is greater in this versus that society. In popular culture, such comparisons are typically delivered with a strong but unexamined air of pathos. But if each twenty-four hours, individuals in a highly unequal society are randomly assigned to a status level, while individuals in a more egalitarian society hold their relative positions from birth to death and over generations, the typical evaluations would have to be reconsidered. Anticipating this objection, sociologists with a Marxian flair invoke claims of continuity with a biological metaphor, speaking of the ‘reproduction’ of class or status position (Willis 1981, [1977]; Bourdieu 1984). More technically inclined academic sociologists understand that the assumption of pathos presumes continuity of personal position in a stratification order, and so they specify whether they are claiming ‘legacy’-like inter-generational stability in position or long-term biographical, intra-generational continuity.

It was understood early on in stratification research that snapshot comparisons of inequalities across populations, as well as snapshot comparisons of the same population at different times, were inadequate. They gave way to retrospective studies of relationships between the statuses of adults of different cohorts and their parents (Blau & Duncan 1967). For another host or reasons (in particular, the problem that ‘sampling on the dependent variable’ by asking adults about their parents misses the fates of many in the prior generation), retrospective studies were bettered by longitudinal or ‘panel’ studies in
which measures of the same people were made at successive points in their biographies. These emerged in academic sociology at about the same historical moment that a similar logic was inspiring the ‘7 Up’ documentary (David Apted, director), in which, since 1964, the same British individuals have been revisited, allowing the viewer to judge whether earlier differences remain decades later.

The best longitudinal studies show that differences in youth, say whether or not one obtains a college education, matter in the long run, and more for some than other subsections of the population (for a recent, well-regarded study, see Brand & Xie 2010). Or more precisely, the best studies show that certain influences or resources have mattered. Even today, such studies in the US are based on repeated measures of people born in the 1950s and 1960s, before the relaxation of immigration restrictions began to transform the US population. Nothing in the logic of such studies, and usually nothing in their published reports, speaks to the logic that is required to extend or replicate the findings. Note that stratification studies have greater value for demonstrating the reproduction of inequality when they show that early life influences have long lasting effects. But that means that replicating the findings now will require a similarly long-range study. On what grounds should we assume that the economic sorting mechanisms of 2015 remain the same as those of 1965, or that the value of a college education obtained now will be the same as it was when a much smaller percentage of the population had a college education?

In a famous essay, C. Wright Mills (1963, [1943]) attributed the formulations of sociology’s first generation of ‘social pathologists’ to their common religious background. We have yet to appreciate the fundamentally religious character of what most treat as the hardheaded, empirically-driven understanding of social inequality. The conception of the ‘reproduction of inequality’ shares many of the features of tragic Greek narratives. The individual is fatedly cursed at birth, when the neighbourhood, class or ethnic/racial context sets limits on life chances in the long run. Like an Oedipal drama, passionate individual struggles in adolescence and early adulthood, in which young men try to run away from their fate, just serve to elicit official stigma, which deepen attachment to early class position (cf. Willis 1981, [1977]). The culture of ‘equal opportunity’ just masks the power of a transcending structural logic.

Advances in quantitative methodology have reframed rather than eliminated the act of faith necessary to believe the inequality myth. (NB: Myths are not necessarily false. They may capture truths that are empirically unverifiable.) Consider the leap of faith required in order to take findings on the fates of people born in US households in the 1950s and 1960s and apply them to the current population. Over the last fifty years, the opening of immigration has reshaped the US population, to the point that, in populous jurisdictions like Southern California and New York, a majority of residents’ are first or second generation immigrants whose lives reflect influences from early childhood environments shaped by a variety of non-US societies. Longitudinal studies have vastly improved our confidence in biography-specific findings, only to highlight the inadequacy of methodological thinking that has yet to think seriously about the issues that massive immigration poses for mobility.
analysis, such as how to conceptualize cross-national relationships of parental and/or childhood status in societies-of-origin and adult status in societies-of-destination.

It may be protested that to take ‘replicability’ as meaning the practical ability to test a study’s findings is to muddy methodological thinking. But treating replicability in the narrow sense of specified protocol is to ignore the methodological re-thinking that historical change requires. Ethnography, a methodology that encourages keeping research attentions closely and continuously ‘on the ground,’ repeatedly shows why we need to keep sensitive to replicability as a claim that one can enter the same river twice.

Consider an explanation of crime decline in the US that achieved significant popular and academic attention a few years ago, the argument that the legalization of abortion in the early 1970s led to fewer undesired children, resulting about twenty years later in a reduction in the crime rate (Donohue & Levitt 2001). The findings were subject to various objections, some on the theoretical level (why would a one-time change produce the progressive rate declines that have been recorded since the early 1990s, as opposed to a one-time step down?) and statistical replications that challenged the procedures used in the original study. But a fundamental problem with the argument is that it is based on long breaks between analytic glances onto origin and outcome moments. Elegant thinking that operates on a highly abstract level and deigns only occasionally to note what is happening in the social realities below risks missing massive informal changes on the ground. Between the early 1970s and the early 1990s in the US, the social composition of poverty, which defines the population ‘at risk’ for committing the types of crimes in question, was transformed by the movement of African Americans out and the movement of Latin American and Asian-origin populations in, meaning that during family formation phases of life, substantial segments of the ‘at risk’ population were subject to legal jurisdictions that were not affected by US law changes.

It is common to rely on a methodology taken from the immortals and to consider the value of different social research methods from an Olympian perch, arguing the issues in ahistoric (even if temporal or sequential) and universalistic terms. Certain research designs are deemed ‘stronger’ than others, whenever and wherever they are applied. For analyzing conversational interaction, audiovisual recordings provide infinitely superior data to what field notes offer. Some quasi-experimental designs make it possible to rule out more alternate explanations than other designs. Probability statistics may be used to specify confidence in the relationships observed among social phenomena in a given population, providing that sampling protocol was rigorously followed. Fatal flaws enter a data set when uncontrolled departures from research design occur in any of the critical practical links in the process of identifying subjects, posing questions to them, and recording responses.

Methodological thinking takes an immortal perspective when research design is applied independent of the social ontology of the phenomena under study. Quantitative research commonly considers some aspects of such ontology, such as: Is the explanandum
dichotomous or continuous? Rare or frequently occurring? Suddenly or gradually emergent? Idealized methods are then designed as blueprints for a social organization that will gather data on social phenomena conceived as ideal types.

But if a phenomenon is appreciated as historically unique, a qualitative logic of method will be appropriate. If replicability is to be a useful goal, it must be understood as cumulative testing rather than repeated protocol. We may appreciate that certain social phenomena are historically specific in the sense that they take shape based on a past sequence of events that no other instances of the class have experienced, the specific past sequence of events having established a unique ecology of contemporaneous forces that continuously shape a wide range of significant phenomena. An example is the social geography of the city, including the location of the business centre, the formation of distinct neighbourhoods, and their interrelations. Chicago and Los Angeles expanded from villages to cities through an inversely related sequencing in commercial transportation. Chicago started to grow as a river/lake harbour city, and then about twenty years later became a regional rail transportation centre. Los Angeles began to grow rapidly when the intercontinental rail network reached the area that had been the administrative centre of the pueblo under Spanish and Mexican rule. About twenty years later the city added a deep water port on the Pacific coast, which, when the Panama Canal opened a decade later, dramatically expanded the social reach of Los Angeles. In both cases, the initial site of regionally significant commercial transportation became the ‘downtown’ financial and administrative control centre, with the site of second commercial transportation becoming a working class industrial area.

It is striking that even while the path dependencies indicated by these histories were available to researchers when urban sociology began, they were ignored. Universalistic methods and ahistorical concepts have ruled urban sociology’s academic research from the start. The universalizing tradition stretches from the ‘concentric circles’ of Park and Burgess, which were offered not only as appropriate for Chicago in the early twentieth century but for industrial cities in general, all the way to current efforts to explain the impact of neighbourhood qualities (‘collective efficacy’, segregation, immigrant versus native composition) on personal life outcomes (educational achievement, criminality, earning capacity).

Without questioning the utility of conceiving of some urban phenomena in ahistorical and geographically universalized terms, we may also seek to answer questions that, while comparative in nature, acknowledge the unique historically structured ecology of each metropolis. The appropriate methodology will be ethnographic in nature. Replicability will remain an evaluative criterion, not in the sense of specifying protocol that can be repeated but by encouraging cumulative testing of how new influences work their way through a theorized ecology. In his recent book on Chicago, in which he integrates the findings of multiple studies conducted over fifteen years, Rob Sampson (2011) offers both a theory of ‘collective efficacy’ in universalized terms and traces how the recession that began in the late 2000s has ramified through the uniquely interrelated complex of
Chicago’s neighbourhoods.

As Sampson’s example illustrates, the critical debate is not between quantitative or qualitative methods. The shared challenge is to remain fundamentally a sociologist when considering methodological issues, such that methodological evaluations are guided by empirical understandings of how research practices work in relation to the social realization of the phenomena in question. That may seem a truism but in the souls of sociologists it is not widely embraced. A fundamental shift in theoretical sentiment is required, especially when the phenomena may reasonably be hypothesized to as historically unique.

Where the phenomena in question are understood as universally recurrent, the love goes to the explanans. Whether it is Marx, Weber, Durkheim, Freud, Bourdieu, network analysis or game theory, the explanandum is always subordinate in charm to the explanatory forces or concepts. When we talk of a type of theory, we point to a kind of explanans. Durkheim is not a suicide, religion or work scholar, he is Durkheimian, whatever he looks at. The researcher moves from one substantive problem area to another (variations in explanandum) in order to show the power and value of his/her theory (variations in explanans).

Where the phenomena in question are understood to be historically unique, either in the path dependent sense or as unprecedented, love should turn to the explanandum. Failure to appreciate this is why, even as the descriptive power of social research has moved incomparably beyond the methods prevalent in 1970, substantive sociology at the turn of the twenty-first century has proven disastrously inadequate to the challenges of its time. In field after field of study, the research gaze has failed to anticipate the changes under way. Substantively, sociology has been playing catch up for a generation. For studying a range of matters from the dynamics of urban social geography to the dynamics of social inequality, the challenge now is to appreciate how the key phenomena are being constituted in a historically novel way. The current challenge in sociology is to confront an ever-more institutionalized tension between the historically unprecedented dimensions of social phenomena, on the one hand, and on the other, the practical demands of acquiring craft expertise and developing social support for a research career. This tension plagues both qualitative and quantitative researchers. Excellence in analyzing data of any sort increasingly requires long periods of specialized focus. Appreciating historical novelty requires a generalized investigation in which discipline specialization must give way to an integrating research perspective that is likely to humble practitioners from any home discipline.

Consider what it means to depart from disciplinary expertise in the study of urban social life. Social ecology, the interaction fabric of everyday neighbourhood life, regional economics, transportation technology, legal scholarship, the ecology of the natural world, the cultural representation of the city and the unique history of each city must be treated as essential resources. But not by different disciplines or scholars. As demanding and as unwise as an academic career strategy as it may be, all these approaches must be combined
in a transcending effort to appreciate each city holistically.

If there is any research perspective that is most flexibly open to this challenge, it is ethnography, which (at least in some parts of the ethnographic tradition) puts ‘getting it right’ about a singular case in a position of top priority. What replication comes to mean in this version of the research enterprise is a testing of prior research, not by repeating field procedures nor by seeing whether a preferred theory of a given discipline needs amendment but by honouring the contributions of the various disciplines through interrelating them in a continually improving appreciation of what is specifically different about the instant case. If this reading departs from textbook definitions of replication, it honours the first requirement of replication-as-practice, which is to attend to past efforts to improve knowledge.

The inevitable qualitative leap: statistical representativeness and ‘casing’ or ‘theorizing’ an ethnography

A map as big as the thing mapped is not very useful. So found an obsessive society of cartographers as imagined by Borges (‘On Exactitude in Science’). Sociologists have appreciated the implications for our work (Becker 2007). All social researchers face the absurdity of generalizing: if we get enough data to be sure that we make claims only on a fully firm base, the results will be perfectly descriptive but practically useless. The only justification for research is to make inferences from descriptions of social life in some times and places to others we have not studied. Our claims always require a leap of faith beyond what we know. But how can science support faith?

Social research, qualitative and quantitative alike, typically proceeds by ignoring this existential challenge. The common quantitative strategy is to produce statistics on ‘representativeness’, such that a reader has confidence to a specifiable degree of risk that relationships found in the observed data can be projected to ‘the population’ sampled. But this is a rhetorical slight-of-hand: ‘representativeness’ is not ‘generalizability’ in the sense required to justify research in the first place. If the knowledge produced by social research is to be practically useful, the objective is always knowledge of a ‘universe’ of relationships that goes beyond the population studied in ways that are unknowable when the generalizations are made (Camilleri 1962).

At some level every quantitative sociologist knows this, but in professional publications the leap of faith will rarely be addressed explicitly. To do so would require qualitative reasoning that cannot be supported by probabilistic calculations. If sociology were a professionally honest discipline, it would routinely require explicit commentary on the reasoning for extrapolating not just from sample to population but from population to ‘universe,’ i.e. to the abstractly stated categories that reach beyond the time and place studied. Perhaps the most common convention is to claim that findings represent social reality in a specified political-geographic jurisdiction, like ‘the United States’, as if that were a Platonic entity and not a rhetorical convenience for gesturing toward a constantly
changing, worldwide collective process. Journal editors routinely honour the quantitative sociologist’s Latin prayer, ceteris paribus; they do not require a section reasoning the relationship between the bounded world studied and the world of any possible application.

One way that academic sociology keeps the faith is by not looking back at studies that were celebrated when published years ago but that have lost their lustre in ways the authors did not explicitly anticipate. Consider what sociologists since the 1970s have gotten wrong or failed to get right: the collapse of state socialism, even when claims that countries like Hungary had found a ‘middle way’; the rise of China and India, even while they persisted with what theory and comparative historical case study had brilliantly shown were the wrong religions and cultures; the ‘return’ of the US city as a vibrant social fabric; the rise of religion even in Western societies whose rationalization was supposed to have disenchanted social life; the decline of crime in the US since the early 1990s. My favourite example from the last generation is the argument made in the 1980s that in the urban areas of the US, crime could be explained by social inequality (Blau & Blau 1982). As social inequality has increased dramatically in the US, crime should have increased dramatically as well. My favourite minor example is a paper of the early 1980s in which a famous urban sociologist sung the Marxian song of the day, which dictated that, because multi-national corporations were eager to exploit the increasingly global economy, low-wage work would dry up in the US as it was displaced to distant places where the social costs would be borne by third-world societies. Even as the ink was drying on this exercise in macro-social theorizing, US cities like Los Angeles were in the throes of a rapid growth of undocumented immigration that was bringing the garment and other low-wage industries back into brisk business. But like newspaper editors whose current opinions run against what they advocated when the party in power was different a few years ago, there is no professional requirement to review the fate of past views. Reading the major sociology journals, one would think that there is no crisis at hand for the discipline, nothing to examine in this history for clues about what may be wrong with the way sociologists do their work.

Sociological description has made great progress over the last generation. We can now produce more accurate, more detailed descriptions of various forms of social life, more economically than ever before. Good description makes a contribution to knowledge by revealing how prior knowledge was based on superficial, inaccurate views of social worlds. But improvements in the capacities for description only make the point of the Borges story more vividly relevant.

Qualitative research in sociology does not escape the need to generalize beyond patterns observed in data to the ‘universe’ of events not limited by time or place. Even qualitative research that is offered as ‘just descriptive’ implies a position on questions of explanation: why describe ‘this’ rather than ‘that’, unless the claim is that ‘this’ has more explanatory potential? And explanation implies generalization.

Qualitative research does not explicitly or directly argue for the ‘representativeness’
of the examined data for the ‘population’ studied. Instead, qualitative researchers write in a language that indirectly makes the claim by asserting that a given phenomenon has been observed in ‘massive’ numbers, is ‘typical’ of the case, has shown up in an examination of a quantified number of field notes (e.g., 2,000 pages), or reappears in interviews with a certain number of subjects. The inference is that the observed patterns are not randomly but systematically produced, and so, characteristic of the overall data set collected in the study.

But again, qualitative researchers face the further challenge of warranting generalization from the times and places studied, to the ‘universe’ of phenomena which reaches infinitely beyond and without an observed relationship to their site-specific data. No matter how ‘massive’ the supporting evidence in their case study, a qualitative study is practically useless unless it can claim what it is ‘a case of’. In its own ways, qualitative research has adopted conventions which, like sampling logic, can effectively obscure the qualitative reasoning necessary for generalizing beyond the temporal and spatial limits of the researcher’s mundane data set.

Conversation analysis has created a precise example of the challenge demonstrated by Borges’s cartographers. A descriptive revolution occurred about forty years ago, when Sacks and Schegloff began using recordings of conversations as the basis of analysis. In an instant, all prior social research and philosophizing about language, which had been based on memories of conversation and transcripts created to serve institutional interests, became suspect. Worlds of previously unknown forms of social life became available through the inspection of the interactive contingencies of talk as audible in recordings and as visible in transcripts created for the sole purpose of describing the social interaction of talk. Much of Goffman’s work, including his Forms of Talk, now had to find justification as a kind of social psychology; his casually collected and paraphrased data would no longer sustain serious investigation of the contingencies of talk.

It became possible to create recordings as extensive as the phenomenon recorded. Now it is not fantastical to imagine that everyone would have every utterance he or she emits preserved in recordings, from birth cry to death sigh. Such a map of a life’s talk-communicative experience would actually be bigger than the thing mapped, since recordings would create a physical permanence that outlives the constantly self-effacing ontology of lived talk. For practical use, something like sampling would be necessary. But sampling requires a pre-definition of the matter to be sampled, and once sociologists began inspecting audio-visual recordings and the transcripts made from them, they began finding unimagined forms of social life.

How to work with an infinitely expandable stock of recordings so as to make claims about universal contingencies of talk? The answer developed was in the nature of analytic induction, although neither that self-reflective methodology nor any other has ever been explicitly adopted by conversation analysts.\(^1\) In practice, conversation
analysis works from hunch to specification of an explanandum as it searches for negative cases. Upon finding negative cases, the researcher identifies and specifies new versions of the explanans or explanandum. Doubts that a relationship may be the product of randomness are overcome, not through sampling design but by assertions, backed by multiple examples and an offer to share access to recordings, that the relationship has been observed with massive recurrence. Doubts that the relationship may be ‘biased’ or specific to the selection of distinct social-cultural worlds to study are met by demonstrating the relationship in diverse social settings, languages, and across differences in speakers’ demographic characteristics. The thrust is a qualitative logic to answer concerns about representativeness. Emanuel Schegloff’s treatment of ‘repair’ after third position turn in conversational sequencing (1992) is an exceptionally well worked out example of generalization based on the hunt for negative cases.

Just as statistics claiming ‘representativeness’ can be misunderstood as claiming generalizability to phenomena beyond the practical, time/space reach of a sampled population, so analytic induction, which famously pursues ‘universal’ explanations, may be misunderstood as a claim of omniscience instead of as a solution to a sampling problem. Analytic induction essentially is a guide for the practical decision of how to select the next case, i.e., search for negative cases.

In practice, sampling statistics ironically have substantive not abstract meaning. They refer not to the universal validity of an observed relationship but to bias in the data selection process. Their function is to negate suspicions that the way the data were gathered was itself responsible for the observed relationships. Note that sampling statistics refer back to the data gathering process. Without specifying the social practices through which bias in case selection might have produced observed patterns, they provide evidence to reject that relatively unspecified alternative history of the research project. The language of ‘representativeness’ misleadingly suggests generalizability, grounds for projection to future social life. Actually, the effort to establish statistical representativeness turns sociological research into a historical exercise of clarifying what has already been seen. The vision of social life in times and places beyond those sampled is left to the omniscience of hermeneutics, or what in another writing, and in a nod to the ancient poet’s resonance when relating myths, I refer to as theory singing (Katz 2004).

In this respect, qualitative research differs, at least when it is governed by analytic induction. The researcher remains continuously focused on the prospects for generalizing to cases not yet examined. In effect, the research process is dominated by anxiety about disconfirming encounters when data gathering turns to other places and subsequent times. The mortality of the methodology consists of the increasingly modest qualification of the analysis, which, although set in a rhetoric of universality and omniscience, ironically indicates an essentially vulnerable posture. If statistically representative findings offer specific levels of confidence about what has been discovered, but no logical basis for generalization beyond the time and place described
by the data, qualitative research conducted in the manner of analytic induction displays its future-fearing Achilles’ heel behind every step taken to ground theoretical claims.

Social researchers develop distinct careers, depending on the craft they need develop to work with distinctive forms of data. All confront an existential leap between the spatially situated social life addressed by their data set and the application of knowledge to social life in other places. To that end, each kind of research uses an immortal logic, whether the mathematics behind statistical demonstrations of representativeness or the ‘quest for universals’ in analytic induction. Confusion sets in when the immortal character of the methodology is taken not as a means but as an end. This occurs in quantitative research when statistics on representativeness are treated as establishing the generalizability of relations found in the data set, rather than as providing grounds for ruling out rival interpretations that the relationships appear in the data because of bias in case selection. In qualitative research conducted with the logic of analytic induction, confusion occurs when the results are read without recalling that the universalized statement of claims was a means for finding negative cases, and the value of the research is no more than the distance travelled from an initial formulation through successive revisions. One might say that the methodological quality of qualitative research is a kind of quantitative judgment, more or less calculable by the number of qualifications introduced from an early, rough formulation.

Participant-observer ethnographers do their best work, methodologically speaking, to the extent that their perspective is ‘grounded’ not simply in subjects’ perspectives but in their limits. The researcher’s contribution lies in going beyond the limits of subjects’ knowledge of their world. The reason the participant observer has anything to contribute, beyond being a translator or bridge for subjects, is that he or she can move from subject to subject with a freedom they do not have, such that the researcher can see what they do not see of each other. The distinctive freedom behind the methodological value of ethnographic fieldwork is to transcend the mundane limits subjects face in their interaction with each other, which, depending on the site, may be what management does not see about workers, and vice versa; how he and she remain ignorant of each other even in intimacy; the self-knowledge that is blocked when another is ‘othered’; etc. In a sense, the fieldworker takes the perspectives of subjects on each other as folk sociological hypotheses and negates them.

But when ethnographers expostulate on ‘the strategic case’, when they ‘theorize the case’, when they pin the significance of their work on ‘what this is a case of’, they abandon their commitment to transcend subjects’ understandings of each other and reorient to their sociological peer community. They do so by engaging a rhetoric of immortal significance, commonly by invoking a historically transcendent intellectual hero, someone like Marx or Bourdieu. That move risks losing touch with the research subjects and especially with the alternative explanations they might supply, which might better account for the variations in the data that the researchers have collected.
These two contradictory implications of the researcher’s relations with subjects form the crucible of the ethnographic self. On the one hand, the ethnographer’s warrant for making a contribution to knowledge depends on locating and going beyond the patterned sources and reinforced limits of members (or ‘subjects’) understandings. On the other hand, the researcher’s independence from members’ understandings creates a temptation to ignore their explanations of the phenomena at issue. When the ethnographer treats the site of research as a synecdoche for some universal phenomenon, the risk of hubris emerges in the form of ignoring reasonable rival explanations which members of the scene themselves would offer, were they not silenced by the ethnographer’s theoretical orientations and academic career ambitions.

The quality of an ethnographic study depends critically on the sociology of the social research process, namely how the researcher navigates interactions with subjects such that their perspectives are honoured, but the limits of their perspectives are recognized and transcended. One path to error is identifying with subjects to the extent of not questioning the boundaries they put on their understandings of their social world in order to make their lives liveable. The other is limiting the immersion in subjects’ lives so as to avoid recognition of explanations that rival the one the ethnographer uses to narrate the data. In current academic life, the latter is the greater danger. As a practical matter it usually means not following subjects in their lives beyond the site in which the ethnographer has made a place for observation, and limiting investigation into the lives of subjects before the ethnographer came into their lives. By these stratagems for limiting the ethnographer’s own work demands, readers – who commonly who look on at a distance that prevents appreciating what the ethnographer does not describe – have their critical capacities pacified.

That Borges’ cartographers pose an empirically real challenge to sociology, and to all other forms of social research, is indicated by its stylized disregard in academic social research. In quantitative research, the style is an ‘of course’ acknowledgement that ‘representativeness’ is about negating non-randomness in the data selection process, not about transcending the time/space limits of the population sampled. ‘Of course’ external validity is another matter. Because the difference between representativeness and generalizability is not news, it need not be explicitly addressed. But it is equally obvious that studies that are similar in achieving statistically representative findings do not succeed similarly in journal acceptance, grant applications and academic promotion processes. For readers, researchers and research communities, the inexorable challenge is how to respond to the silence, how to understand the potential influence of political affiliation and academic club-like sentimentalities in making the leap from a demonstration of relationships in a geo-temporally limited population to the more indeterminate populations of policy application and to the infinite populations addressed by sociological theorizing. Since that leap is inevitably done back stage, in all the practical decision-making that determine academic research careers, is it a mark of professional modesty to leave ‘external validity’ out of explicit considerations in a
journal article, or is that a stylistic way to obscure the operation of bias in service of conventional beliefs, stratification in the academy, or the power of the already elect? Or perhaps changes in management strategies, like changes in other conditions of work, reflect a rise over the first two-thirds of the twentieth century in the bargaining power of US workers.

In a neat inversion, a seductively stylized silence about bias in data analysis has entered contemporary sociological ethnography. Under the trope of ‘theorizing the case’, field researchers present descriptions of social life that illustrate a preferred explanation and rely on contemporary academic sentiment to neglect rival explanations that would appear equally convincing, were they explicitly discussed. In his best-known work, Michael Burawoy, an elected president of the American Sociological Association, uses a field study of a factory to argue that capitalism has evolved to manage workers’ consent to their own exploitation. He neglects nearby, contemporaneous evidence of worker rebellion (Stepan-Norris & Zeitlin 2003: 183) and the investigation of whether different management strategies were in play. That might be dismissed as the sort of concern about external validity on which ethnographers should get a pass. But, under the banner of what he labelled the ‘extended case method’, Burawoy also neglected to demonstrate that there were higher levels of rebellion in earlier eras – the prior study at his research site by Donald Roy showed different forms of worker game playing, but not necessarily lower overall compliance with management expectations for production. Further, his text shows no concern to negate the possibility that – independent of management strategies at the work site – the quiescence of his subjects was shaped by their understanding of labour market conditions and perhaps the less attractive opportunities of their kin and peers. Maybe ‘new’ management styles are primarily ways for managers to create a culture of sophistication that raises their salaries but lacks discrete impact on workers, who struggle with the limits of the market outside of any employment site.

In perhaps the most successful academic ethnography of the last generation, Arlie Hochschild described a contrasting mobilization of male and female identities in bill collecting and flight attendant work. She claimed to find deleterious ‘results’ from gendered power differences in ‘deep acting’ at the job site, bolstering her claims by quoting from a sex therapist ‘who had treated some fifty flight attendants’ (Hochschild 1983: 183). But the biographical origin of damage could not be documented within the researcher’s original observations of training programs and interviews about on-the-job work experiences. Left unaddressed was the possibility that the damage was due to gendered differences in popular culture and socialization which pre-dated labour market entry. Perhaps gender-stratified work stimulated collective awareness of shared problems, which facilitated referrals to therapy, which in turn built the personal confidence to address pre-existing, societal-rooted problems (possibilities noted by Smith-Lovin 1998).

The neglected alternative explanations are not necessarily to the political Right or
Left of the favoured theory. Ethnography does not as clearly show political bias as academic community strategies for easing researcher work conditions by simplifying the range of objections that must be routinely considered. Ethnographers’ success in limiting demands on their work appears to have become possible because of a more widely shared, unannounced and unspeakable agreement. When they publish articles in high status journals, quantitative researchers routinely remain silent on the logical leaps necessary to argue external validity. Qualitative researchers, exercising theoretical preferences in the cases they choose to describe in field notes and present in texts, exercise bias by maintaining a convenient silence on reasonable alternative explanations. For all the historic tension between qualitative and quantitative methodologies, the two have become inverse strategies for developing university careers. After fierce mid-century battles between quantitative and qualitative researchers, academic sociology in the late twentieth century has worked out a rapprochement in methodological thinking based on perfectly inverted, common commitments to concerted ignorance.

Explicit recognition of statistical issues of representativeness is often a device for silencing the discussion of generalizability. Ethnographers’ discussion of or what, beyond the time and place covered by data collection, their study is ‘a case of’, is often a rhetoric for silencing native voices which, were they heard, would urge a reasonably thoughtful rival explanation for phenomena in the site studied. The point of this writing is not to judge nor to give systematic criteria for judgment, but to break the silence in handling methodological challenges.

Why this essay now? The immediate provocation is the opportunity given me by Method(e)s to reconstruct my methodological thinking, and the substantial help given by commentators on my essay from the 1970s. But the thoughts here have been developing for some years on the understanding that the time may be ripe to re-launch qualitative research on a footing that comprehends and moves beyond the debates of the past. Qualitative social research is specifically suited for times of rapid social change. In American sociology, ethnography was born with the geometric population expansion in Chicago over the decades spanning the nineteenth and twentieth centuries. After immigration to the US was closed down, ethnography retreated in competition with survey methods. At the turn of the twenty-first century, as immigration and information technologies have returned to once again transform US society, ethnography has been revived through faculty appointments, prized monographs and new journal outlets. For young researchers making career choices, the challenge is to make a deeply personal bet as to whether the phenomena of interest are so historically novel and ecologically specific as to require ethnographic methods. If the answer is positive, it should be helpful to reconstitute the logic of qualitative research methods so as to appreciate the insidiously ubiquitous seductions to immortal thinking about methodology, even while refusing to tremble before the wrath of the gods.
Notes

1. Analytic induction is commonly used in qualitative research without being recognized in methodological commentaries. See Vaughan (1986) for an example in social psychological research; Newman (2004) for an example in the effort to understand school shootings.

References


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