

Methodological Pluralism and the Possibilities and Limits of Interviewing

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Abstract Against the background of recent methodological debates pitting ethnography against interviewing, this paper offers a defense of the latter and argues for methodological pluralism and pragmatism and against methodological tribalism. Drawing on our own work and on other sources, we discuss some of the strengths and weaknesses of interviewing. We argue that concern over whether attitudes correspond to behavior is an overly narrow and misguided question. Instead we offer that we should instead consider what interviewing and other data gathering techniques are best suited for. In our own work, we suggest, we have used somewhat unusual interviewing techniques to reveal how institutional systems and the construction of social categories, boundaries, and status hierarchies organize social experience. We also point to new methodological challenges, particularly concerning the incorporation of historical and institutional dimensions into interview-based studies. We finally describe fruitful directions for future research, which may result in methodological advances while bringing together the strengths of various data collection techniques.

Keywords Interviewing · Ethnography · Methodology · Cultural sociology · Methodological pluralism · Theory

Between 1984 and 2010, the number of undergraduate students who received a degree in sociology rose from 12,000 to 29,000 in the United States.¹ This period of relative prosperity occurred at the same time as our discipline dug itself out of the sectarian methodological fights that had plagued the sixties and seventies, particularly those opposing micro and qualitative to macro and quantitative (Mullins 1973; Turner and Turner 1990). We moved toward a period of pluralistic coexistence, with an acknowledgement of the benefits of living together under a big tent, one that made room for the simultaneous flourishing of various types of excellence (see

¹This rate is comparable to the growth rate of the social sciences. The rates were 48.1 for sociology and 50.5 for the social sciences for the period 1984–2010 <https://webcaspar.nsf.gov/OlapBuilder>

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Lamont 2009 on this theme). These developments made possible the diffusion of mixed-method research (Small 2011) and were encouraged in part by the extraordinary success of cultural sociology and related fields (created in 1986, the Culture Section became the largest in the ASA in 2008).² Cultural sociology quickly came to exemplify a catholic but predominantly qualitative field, one where scholars pursued interpretation and explanation with whatever type of data was deemed useful.

This period was also one of relative prosperity for qualitatively-based research. While systematic indicators are not available, a quick survey of past and current editors of leading qualitative journals reveals that the number of submissions to a well-established journal such as *Qualitative Sociology* has recently doubled (from 88 to 173 between 2009 and 2012), at the same time that new primarily qualitative journals were being created (for instance, *Cultural Sociology* which has been steadily receiving around 100 submissions for the last 8 years, and *Ethnography*, whose editor Paul Willis says it has been on a “steeply rising curve”).³ The NSF also saw a strong increase in the number of qualitative dissertation improvement grant proposals received, which led to the organization of workshops on the evaluation of such research.⁴

This period of growth and prosperity also saw the appearance of a growing number of aggressive controversies among qualitative sociologists (Wacquant 2002; Anderson 2002; Duneier 2002; Newman 2002; Duneier 2006; Klinenberg 2006; Becker 2009; Jerolmack and Kahn 2014). This may suggest that while the seventies was a decade when qualitative and quantitative fought with vigor, the post-millennium decade has now become a period of intensified boundary work among qualitative types, where contests over who has the best method are at times displacing contests over theoretical and substantive claims.

Although we believe debating is part of scientific inquiry, we do not view a multiplication of such controversies as a positive development. This paper adds to an emerging body of work responding to these debates by providing a defense for methodological pluralism against methodological tribalism (in line with Yin 1994; Luker 2010; and others—viz. Wilson and Chaddha 2009; Lareau 2012; Pugh 2013). Our stance is that each technique has its own limitations and advantages and that a technique does not have agency: all depends on what one does with it, what it is used for. In other words, there are no good and bad techniques of data collection; there are only good and bad questions, and stronger and weaker ways of using each method. Self-consciousness about the weaknesses of a particular method can be valuable, but only if such self-consciousness leads to better research or methodological innovations that attempt to overcome or compensate for an important limitation.⁵ We believe that debating techniques per se leads us down an unproductive path.⁶

We are coming out against methodological tribalism because we think the future development of the field depends on continuous engagement, but in a way that moves conversations forward. We advocate a pluralistic and pragmatic position, which reaffirms that the selection of methodological approaches should depend on the

² http://www.asanet.org/documents/New_Section_Officers_Training_Manual_2nd_edition.pdf

³ Personal communication.

⁴ Patricia White, personal communication.

⁵ DiMaggio (forthcoming) response to Jerolmack and Khan (forthcoming) summarizes a number of these (concerning survey methods).

⁶ Snow et al. (2003) noted a propensity among some ethnographers to eschew making explicit theoretical contributions. In Bourdieusian terms, ethnographic up-manship would be interpreted as an alternative strategy for gaining symbolic capital in the disciplinary field. In our view, engaging in theoretical discovery, extension, and refinement would be a productive response for the field as a whole. Ethnographers who leave their mark tend to be those who do precisely that.

questions being pursued, to be assessed on a case by case basis. Different methods shine under different lights, and generally have different limitations (e.g. depth versus breath, singularity versus generalizability, site-based study versus drawing on a wider range of respondents, and so forth).

After locating the debate in a broader theoretical context, we provide a defense of interviewing, the data gathering technique both of us have practiced most. Even against the background of our plea for methodological pluralism, we think this is necessary given that this research technique has lately come under repeated criticism from researchers generally influenced by cognitive psychology or those coming out of the symbolic interactionist tradition (e.g. Vaisey 2009 (though see Vaisey 2014a); Martin 2010; Jerolmack and Khan 2014; for the UK, see Savage and Burrows 2007).⁷ This is followed by a discussion of our views of the limitations of interviews, or rather the set of questions that have pushed us to stretch or adapt our methods. Ideally, we would engage in a similar assessment exercise for each of the main techniques of data collection frequently used in qualitative sociology, and focus precisely on what each method is good and less good for, what it allows us to see, how it enables us to construct facts, etc. We should also systematically explore the “theory of reality” underlying each method (its social ontology (Guba and Lincoln 2004))—e.g., whether for “literal” or “realist” (as Emerson 2001 calls it) ethnography there is an assumption that one can witness in an unmediated fashion what “is,” whereas for interpretivists *à la* Clifford Geertz, there is an assumption that one’s lenses condition what can be seen. We would also consider which method is best suited to tap various types of relationships, analytical levels, causal links, etc. Instead, given limits of time and space, we highlight some of the main methodological challenges that the discipline currently faces, as well as innovative methodological cross-fertilization in the field. We make a plea for shifting our collective focus to such methodological innovation. But we also hope that our contribution will encourage similar assessments of other data collection techniques and help us collectively nurture a stronger culture of methodological pluralism.

While this paper can be read as “preaching to the choir” (and we concur that such a critique would not be unfounded), we want it to be guided by a spirit of constructive criticism, promoting a methodological pragmatism whereby reflective choices of methods are purposefully made based on the needs of the question at hand. This methodological pragmatism presumes an openness to a variety of methods, or a methodological pluralism, which should not be confounded with methodological relativism (everything goes, yours is as good as mine).

What this paper is not is an attempt to establish orthodox guidelines in the practice of interviewing. We are both experienced interviewers and reflect on what we believe interviewing is good for. But we also believe that the value of interviewing is to be assessed on a case-by-case basis, for the yield of the research at hand, in line with a practice-focused approach to knowledge production and evaluation (Camic et al. 2011).

⁷ The relationship between symbolic interactionism (where ethnography has historically been the preferred method of data collection) and cultural sociology is a complex one, in part because the influence of the former declined as the latter increased. At the same time, the two subfields are closely intertwined as a number of leading cultural sociologists are ethnographers, have been influenced by symbolic interactionism, or would self-define as interactionists. For its part, the renewed enthusiasm for ethnography is tied to a revival of the use of qualitative methods in urban sociology, the sociology of education, the sociology of the family, and other fields—a revival empowered in part by the remarkable success of cultural sociology. The growth in popularity of specific techniques of data collection should be understood in the context of the ebbs and flows of disciplinary currents where the relative importance of subfields and topics is tied to intergenerational dynamics (or field dynamics in Bourdieusian terms (Bourdieu 1990)).

From a Methods to a Theory Debate

Questions about methods touch on broader issues pertaining to the theoretical focus and direction of contemporary social inquiry. These questions include:

- a) The relative place and importance of meaning-making, emotion, cognition, and behavior in human interactions. The terms of the current debates are often reminiscent of earlier discussions about agency and “what is determinative in last instance” (for instance, “saying versus doing”). Some of the debaters want to know who is tapping the most “really real,” significant reality. All the methods we discuss capture realities, but the question, for example, of whether individuals have a deep, relatively unitary, motivating set of values that might be tapped by a single survey question (Vaisey 2009), or whether people have complex, contradictory motivations that might lead them to different choices at different moments, is a theoretical and empirical question that cannot be answered *a priori*. Indeed, methodological debates are often carried out as if there were some rock bottom “truth” that one method could capture that other methods cannot. But the naïve belief that facts speak for themselves—that, for example, if one observes a situation one knows “why” the actors acted as they did—ignores the combination of theoretical questions, careful research design, and persuasive evidence necessary for social explanation. All methods require consideration of what the observer brings to the situation and how his or her interpretations could be validated by others.⁸
- b) Implicit or explicit conversations with developments in other fields, especially cognitive psychology (which has a particularly thin and intentional view of cognition) and behavioral economics (e.g. Martin 2010; Zelizer 2007). The place of collective representations, inter-subjectivity and semiotics in how we understand meaning-making (Sewell 2005; Vaisey 2009; Tavory 2011) is among the topics debated. Authors disagree on what can be consciously activated and whether we make decisions about perceptions of reality. For our part, we are less interested in the intra-individual than in the dynamic production and consolidation of meaning, and thus stand firm in promoting the distinctive contribution of sociology as a non-individualist approach to the study of how humans collectively engage in meaning-making processes defined as “a set of publicly shared codes or repertoires, building blocks that structure people’s ability to think and to share ideals” (Eliasoph and Lichterman 2003, 735). In this context, focusing on the simple question of whether attitudes predict behavior is quite different than focusing on whether, how, and how much action is empowered by vocabularies, symbolic boundaries, cultural scripts and repertoires.
- c) How social causality and the nature of “social contexts” are conceptualized, e.g. whether producing an explanation requires taping face-to-face interactions only and/or other types of contexts and constraints. It is self-evident to us that understanding social interaction generally requires going beyond situational co-presence. We have to move beyond notions of contexts that glorify the face-to-face, especially given how mediated our lives have become. The “really real” has grown increasingly multi-scalar, and to tap causality, one has to follow the chains of interactions, virtual and otherwise, as they evolve in time and space, to connect individuals, resources, and institutions. As pointed out by Sewell (2005, chap. 10), the elements that shape practice or a specific situation can be traced by being

⁸ Extensive discussions within anthropology and sociology about the importance of “reflexivity” and the constructed nature of the social scientist’s narrative (e.g. Bourdieu et al. 1991) have left this positivist faith intact for some, hiding under the surface of debates about which method is “best.”

attentive to a wide range of enabling and constraining dimensions emanating from the spatial, material, and semiotic realms. Causality chains can be reconstructed through multiple paths and tracing their constitutive parts is likely to require drawing on a range of data collection techniques.

As this list suggests, while these debates may be couched in methodological terms, in fact what is at stake are often theoretical issues. Our perspective is very much in line with the development in the social sciences since the heyday of behavioralism in the sixties, a behavioralism that was largely rejected by several of the social sciences in the following decades. While we fully recognize the value of thoughtful behavior-focused research (e.g. Pager 2009), we believe that an exclusive focus on behavior at the expense of meaning ineluctably leads to an impoverished redefinition of the social sciences, one where a diminishing range of phenomena can be studied (preferably those amenable to randomized trials and experiments for instance).⁹ This is bound to lead to a narrowing of the scope of our discipline, and to an overpowering preoccupation with the individual and intra-individual at the expense of the intersubjective, structural, and supra-individual—some of the very domains of social life where sociology excels. In opposing a reduction of the social sciences to the study of behavior, we are rooting for a far more capacious conception of causal processes that makes room for considering how social and cultural structures and resources enable and constrain human actions. This again suggests that methods debates are in fact theory debates; it further suggests that we have to think deeply about the ultimate fit between theoretical questions and data collection techniques before the use of fMRI and cortisol samples (valued because they are “meaning-free” and unambiguously measurable) come to be seen as the *ne plus ultra* of data collection for most “valuable” social science questions.¹⁰

What are Interviews Good for?

Scholars who have criticized interviewing have tended to focus on a rather traditional and fixed understanding of interviewing. In contrast, we promote a more open-ended and pragmatic approach to interviewing, one where we aim to collect data not only, or primarily, about behavior, but also about representations, classification systems, boundary work, identity, imagined realities and cultural ideals, as well as emotional states. We compare this technique with the use of surveys and ethnography, as these have often been points of comparison for the critics of interviewing. When techniques of data collection are artificially pitted against one another (e.g. “being there” or not), authors overlook the fact that interviews often entail observation, and ethnography usually entails interviewing.¹¹ In such debates, positions are

⁹ In line with our plea for methodological pluralism, we appreciate and recognize the utility of such methods for a specific range of questions, but are concerned about their being prescribed as a universal methodological gold standard in some of our neighboring disciplines—in some subfields of political science and economics in particular.

¹⁰ The point made in footnote 9 applies again.

¹¹ There may also be a simplification of what ethnography means, used as synonymous with all observational methods. In fact, classic ethnography involved 1) long term (often very long term) embedding in a field site; 2) systematic mapping of the cultural and institutional system of the locals, which required enough interviewing to elicit “this is our kinship system; these are our rituals; these are our myths; this is why we do things this way”; 3) enough observation to trace the way those institutions and cultural meanings were actually employed, or what other patterns and practices organized life for the locals; and 4) repeated questioning to elicit explanations of practices and cultural forms the ethnographer did not understand.

sometimes artificially construed as antagonistic¹² and interviewers are criticized for being more naïve practitioners of the genre than their writings suggest.¹³

For good reason, interviewing is sociology's standard workhorse method. The term applies to an enormously wide range of research practice, from forced-choice telephone surveys to in-depth interviewing over many years and in multiple situations (Lareau 2011). Thus, while some sociologists are identified by their method, as "ethnographers," "network analysts," or "comparative-historical sociologists," there is no sub-field of "interviewists." Sociologists who rely primarily on interviewing tend to be identified by their substantive fields, such as the study of the family, gender, or religion, or by their theoretical allegiances, rather than by their method.¹⁴ We can nonetheless begin by noting some obvious virtues of interviews. They are, of course, a relatively inexpensive, quick way to gather basic factual data, even if that data is also less accurate than direct observations might be. While, for example, it might be more accurate to find out age, marital status, or education from administrative records, for most purposes reasonably accurate portraits of population characteristics can be derived from interviews, and generations of researchers have devised ways of correcting for inaccuracies both on such factual matters and on much more subtle questions like levels of racial prejudice (see DiMaggio, forthcoming). For other matters where observation may be difficult or impossible (sexual fidelity or condom use, for example), interviewing may still be the best method we have, even if results have to be taken with great caution. One of the challenges for survey researchers who collect data on sensitive topics is to try to estimate just how inaccurate their data is (Plummer et al. 2004) and to judge whether those inaccuracies make comparisons across groups problematic (as when women in Africa exaggerate how poor they are, while men exaggerate their wealth; see Miller et al. 2001).

From our perspective as practitioners of in-depth qualitative interviewing,¹⁵ we want to emphasize other relatively straightforward advantages of interviewing as a method. First, interviewing both allows for and encourages (although it certainly does not require) systematic attention to research design, especially comparison across contexts, situations, and kinds of people. Thus, Lamont (1992) systematically sampled French and American upper-middle class men, matched for educational and occupational backgrounds, in order to explore differences in the ways French and American men draw social boundaries based on cultural capital, money, and moral criteria. Swidler (2001) compared middle-class Americans who had remained married to those who had experienced divorce, in order to understand how cultural beliefs are (or are not) altered by disappointment and disillusionment. While there are excellent comparative ethnographies (Dohan (2003) compared two poor Latino neighborhoods with very different levels of crime and gang violence; MacLeod (1995) compared two groups of young men—one black and one white—in a single housing project), the difficulties of gaining entrée to a research site, the idiosyncratic features of the situations the ethnographer encounters, and the accidents of making connections in a community make such comparisons harder

¹² For instance, in his critique of the NSF report on the evaluation of qualitative research in the social sciences (Lamont and Small 2008), Becker (2009) bolsters his position by citing a passage by John Comaroff, which is part of the report and expresses positions that are central to its overall argument.

¹³ As we both experienced. See the appendix of *Talk of Love* and Lamont 2004 for our published reflections on some limitations of interviewing.

¹⁴ An exception here is survey researchers, who typically use interview data from large, systematic samples. They, like others with strong methodological commitments, tend to have theoretical or analytic taken-for-granted that arise out of their methods, such as modeling explanation in terms of "variables" and "variance explained" (Abbott 1988a) or seeking explanations in terms of aggregated characteristics of individuals rather than interactional processes (Jerolmack and Kahn 2014).

¹⁵ Swidler has also done participant observation (Swidler 1979) and has worked with conversational diaries (Watkins and Swidler 2009; Tavory and Swidler 2009).

to interpret than for an interview project. What if MacLeod had happened to encounter a group of poor but upwardly striving white youth, rather than the rebellious kids he did meet? What if he had met a group of black teens who were gang members rather than the striving, but ultimately trapped, kids he did encounter? One of the great strengths of qualitative interviewing is that it can combine depth of understanding with purposeful, systematic, analytic research design to answer theoretically motivated questions.

Interviewing has other virtues. We agree with Pugh (2013) that interviews can reveal emotional dimensions of social experience that are not often evident in behavior. Those advocating ethnography over other research methods imply that only visible, public “behavior” is somehow real, or causally significant.¹⁶ But for many people the *imagined* meanings of their activities, their self-concepts, their fantasies about themselves (and about others) are also significant, and we generally cannot get at those without asking, or at least without talking to people—although one could imagine the topic emerging spontaneously and entirely unpredictably in the course of an interaction with an ethnographer. A superb practitioner of in-depth interviewing, such as Hochschild (1989, 2012), probes precisely the imagined meanings of ordinary events, highlighting, for example, the “magnified moments” (Hochschild 2003) that resonate in memory and give meaning to the flow of a life. Both Lamont’s *The Dignity of Working Men* (2000) and Swidler’s *Talk of Love* (2001) attempt to probe not “behavior,” but categorization systems, where people live imaginatively—morally but also in terms of their sense of identity—and what allows them to experience themselves as good, valuable, worthwhile people.

Finally, the claim that ethnography can capture the effect of “situations,” while interviews conducted in the artificial “situation” of the interview cannot, both exaggerates the advantage of ethnography and underestimates the value of interviews. Participant observation is a situation artificially created by the research just as is the interview situation. Moreover, as we discuss below, it may be difficult to explore the influence of “situations” if the researcher cannot systematically vary the situation in theoretically interesting ways.¹⁷ With the exception of studies like Eliasoph’s (1998) comparative ethnography of several political (and one apolitical) groups, most ethnographies cannot clarify which aspects of situations are consequential. Ethnographers of course have the enormous advantage of directly observing a variety of interactions, and for the study of interactional dynamics in natural settings ethnography is superior to interviews. But a skilled interviewer can encourage an interviewee to evoke a

¹⁶ Of course, building on Emerson (2001), we acknowledge the diversity that exists among ethnographers, including in their degree of interest and focus on meaning-making (after all, again, some of the best ethnographers are long-time card carrying cultural sociologists and they played a crucial role in the development of the field).

¹⁷ One could deepen one’s understanding of a situation by simply spending more time in it. Indeed, this generally improves the depth of the *verstehen* of the actors involved. But whether this results in theoretical advancement will vary with whether the researcher has at his/her disposal a broad theoretical tool-kit and set of questions against which to examine the empirical reality under consideration—as suggested by Weiss (1994) and more recently by Snow et al. (2003) and in Timmermans and Tavory (2012)’s plea for an abductive approach to qualitative research. Length and depth of fieldwork experience can no more serve as a warrant for the quality of a contribution than being an insider to a group determines the quality or authenticity of accounts of the group (Young 2004). Moreover, while ethnographers have won credibility by studying difficult and challenging populations in contexts far removed from the comfort of American academe, more reflection is needed among sociologist ethnographers concerning the types of capitals and representation claims associated with such feats, especially in light of the extensive anthropological debates about representation generated by the very influential *Writing Culture* by Marcus and Fischer (1986). Of course, many ethnographers are already well aware of such issues, as underscored by Emerson (2001).

variety of interactional settings, social contexts, and institutional situations¹⁸ and can probe their meanings in ways ethnography can rarely do. So, for example, Swidler (2001) noted the different ways middle-class Americans think about “love” when they consider whether to commit themselves to a relationship, versus the very different way they use the concept when they are considering how to sustain an ongoing relationship through the ups and downs of daily life.

Important aspects of “situations” are often not visible to the direct observer of interactions. Ethnographers have privileged access to the immediate interactional situation and to many local codes or aspects of interactional style that may not be available to an interviewer. At the same time, the ethnographer who observes an immediate interactional situation may miss important elements of the “situation” in a larger sense: The background factors that might shape different people’s actions differently even in the “same” interactional situation—awareness of one’s access to family help in an emergency, obligations to kin, having (or not having) a high school diploma or a prison record—may not be observable in the interactional situation but are definitely part of the actor’s larger “situation.” And, for those aspects, a researcher might have to ask the person, even at the risk that some or all of what he or she tells the researcher might be inaccurate. Interviews, then, can sometimes reveal more relevant features of reality than immediate observation can, simply because they empower the researcher to probe about facts or about ideal responses or situations, as well as imaginary scenarios and fantasies that simply are not visible in everyday life. For instance, one can explore the ideal self in an interview in far greater detail than ethnography would allow.

Asking questions, as in-depth interviewers do, is not as distant from ordinary interaction as some critics imply, especially if the interviewer is experienced (in the less felicitous cases, interviews may seem overly formal, especially for individuals who do not share the middle-class culture of the typical interviewer). Indeed, conversation is such a fundamental part of most human interactions that ethnographic observers also necessarily talk with those whom they observe, or risk being an obtrusive irritant. There are of course wide cultural and class differences in how normal it is for people to ask and answer questions (Briggs 1986), and particularly personal questions (on the historical emergence of the type of American self that allows for this, see Igo 2007); but far from diminishing the importance of “talk,” a focus on how ordinary human interaction proceeds makes it clear why asking questions and listening to the answers is such an indispensable tool of social research.

The Limitations of Interviews

Our methods all have blind spots, and we discuss some of what interviews miss below. But as with any method, insight usually comes from systematically comparing across analytically

¹⁸ The difficulty of finding the right terminology for analyzing “situations” suggests the need for theoretical work that could clarify the choice of research methods for analyzing socially situated action. Erving Goffman developed a complex typology of situations, defined by the interactional rules that obtained in each, from behavior in public places (1963), to the rules that demarcated “front stage” and “backstage” in occupational settings (Goffman 1959), to the “frames” that people used to define what kind of situation they were in and what rules therefore applied (Goffman 1974). But most subsequent ethnography has shown less interest in a systematic analysis of kinds of situations. There are important analyses of specific idiocultures (Fine 2012) and examples like Eliasoph and Lichterman’s (2003) suggestion that “group styles” powerfully shape interactions. But we lack even a reasonable vocabulary for thinking about how specific “settings” (such as being at home at the dinner table versus in a store or at work), particular “contexts” (as when a polarized political system gives even small gestures larger meanings), or enduring community contexts (such as a neighborhood community, with its own culturally marked geography, recognizable groups, and more or less stable personal ties) influence action.

relevant domains using the same method. Differences across cases—whether it is Republicans and Democrats evaluating the same facts differently in surveys (Bartels 2002) or Dutch versus American parents and teenagers talking about sex very differently in interviews (Schalet 2011)—turn out to be particularly revealing.

The Challenge of Studying Institutional Patterns

We turn to our own scholarship to discuss both the ways we tried to use interviews and the limitations we encountered. We each developed distinctive ways of conducting in-depth interviews, suited to the questions we were asking: Swidler used vignettes as well as biographical interviewing to tap not only what people thought, but the range of things people could think. Rather than seeking interviewees' "true" opinions or attitudes, she sought to understand the cultural frameworks they had available to think about a problem. Thus she probed what interviewees told her, asking for examples when they offered generalizations and sometimes challenging their reasoning more aggressively than standard techniques recommend. These approaches showed the multiple contradictory arguments people have access to when they want to explain or justify their own life trajectories, and the ways people often ground their answers to abstract questions of value or morality in concretely imagined cases, using those as touchstones against which to measure the generalities they profess. Lamont used carefully matched samples of individuals with whom she conducted her in-depth interviews, finding these respondents using census tracts with specific characteristics. She innovated in the questions she used to elicit features of the world in which people operated: She focused not on people's own views as much as on their sense of where they stood, morally and socially, with respect to other relevant groups. She did this by probing systematically about "boundary work" (a type of behavior) which people typically do not know they produce, although they do so constantly (when describing who they are similar to and different from, respect or look down upon, etc.).

Each of us has also in different ways come up against the limits of what interviews can teach us about certain questions. We have each written about this (Swidler 2001; Lamont 1992, 2004). The challenge, which becomes most apparent in carefully-designed comparative cross-national or historical research and is not unique to our studies, is how to describe, contextualize, or measure the relevant features of institutional contexts, especially when these themselves are not fixed, but can be redesigned by political actors, often in light of broad cultural debates, whose dimensions and effects we also have poor ways of conceptualizing. Thus Swidler (2001) grounded her analysis of the multiple cultures through which her interviewees understood "love" in an analysis of the institutional features of marriage. But the sources of those institutional features—the resilience of the ideal of marriage despite the manifest failure of many actual marriages, the erosion of the legal claims attendant on marriage, and the later transformation of marriage to include same-sex couples—could not have been derived from the interviews she used, except perhaps the continuing power of the ideal of marriage for Americans, even where its practical realization is impossible (Edin and Kefalas 2005). In order to understand these features, Swidler had to look beyond her interviews. Here, the challenge was that of accurately explaining meaning-making within the interview situation with reference to larger, historically-shaped institutional patterns. Ethnographic observation of couples in natural situations—as husbands and wives argued or shared everyday tasks, or as couples decided whether to break up or get married—could have added another dimension to her analysis. But characterizing the institution of marriage, whose structuring power, she argued, shaped love for both the married and the unmarried, was a different task. It required mobilizing

a range of evidence that had to be coiled from historical studies as well as from a range of secondary sources.¹⁹

Similarly, Lamont (1992, 2000) traced important differences in the repertoires of justification and the form of moral, social, and economic boundaries between Frenchmen and American men in varied class and racial/ethnic locations (also see Lamont and Thévenot 2000). At the explanatory level, to fully understand what made those very different repertoires of justification sound plausible in France versus the US, Lamont had to go beyond the interviews and draw on the broader cultural repertoires available to her interviewees (e.g. French republicanism, socialism, and the valorization in the Catholic tradition of “solidarity” in France versus its marginalization in the US). She also had to take into consideration institutional differences between the unitary standards of excellence institutionalized in the centralized French school system, especially the dominant position of the elite universities where the French elite is formed and legitimated, versus the more open American educational system. Lamont used these cultural and institutional differences (and the ideologies that constitute them) to explain the differences she found in her interviews, even while these repertoires and institutions themselves were undergoing transformations.

Some of the best investigations of the deep linkages of culture and institutions—and indeed the best sociology period—in recent years have come from innovative comparative and historical work: Fourcade (2009) on economics and economists in France, Britain, and the US; Biernacki’s (1995) masterpiece on the constitution of labor in Britain and Germany; Collier’s (1997) study of the institutional and cultural transformation of Spanish life from the 1960s to the 1980s; Glaeser’s (2000) study of the integration of the East and West German police forces after German reunification; Ferree’s (2012) brilliant comparative analysis of gender and women’s movements in Germany versus the U.S. These works suggest, perhaps better than any abstract argument could, how the combination of ethnography, interviewing, archival analysis, and many other methods is necessary to pursue deeper theoretical questions. But it is only with powerful questions that these methods can bear fruit.

Other Weaknesses of Interviews

Without aiming to be exhaustive, we want to point to two additional pitfalls associated with interviews. One is that, because it typically focuses on the views of individuals, this data collection technique easily leads us down the slippery slope of methodological individualism when it comes to explanation. Indeed, too often in interview-based studies, the explanans is an individual attribute—race, class, gender, sexuality, nativity, etc.—as opposed to field-level or relational explanations (Desmond, Matt. *Relational Ethnography*, Unpublished Paper), social or cultural processes, or more meso or macro levels of reality. This bias is not inherent to the method, but frequently manifests itself, as interviews lead us to think of individuals as carrying properties, and of properties as driving outcomes. The growing popularity of cultural analysis that mobilizes the analytical tools of frames, narratives, symbolic boundaries and repertoires (as reviewed for instance in Harding et al. 2010 and Lamont and Small 2008) is due in part to their usefulness in demonstrating the institutionalized character of such worldviews.

Another pitfall of interviews is that they encourage us to find coherence in narratives and worldviews. However coherent vs. disjointed the culture that people have available, the interactional setting of the interview—like ordinary situations in which people offer accounts of themselves or their life experiences—encourages narratives that give coherence and meaning to a life. Thus reliance on interviews can lead to an image of individual selves as more

¹⁹ See Cherlin (2010) for a complementary approach using historical and survey data.

coherent, with less contradiction and unpredictability than real lives normally encompass. Indeed, a life told from the present backward almost certainly will appear to have inevitability and a direction that it did not have as it was lived (see Riessman 1993; Stein 1997; Vinitzky-Seroussi 1998).

This drive for coherence can be even stronger in hermeneutic methods, which privilege coherent interpretation and explanation of meanings. Indeed, hermeneutic methods such as structuralism posit that one has found the interpretative key (aka “the code”) when coherence emerges from semiotic elements. But this “finding” may be more in the eye of the beholder than in the reality studied. Interviewers, like other interpretive analysts, including ethnographers, need to be mindful of not imposing order where there is none, and this requires considerable epistemological alertness and clarity about what kinds of evidence a specific type of data collection technique makes most likely.

Contemporary Methodological Challenges

Here we consider not the advantages or disadvantages of any particular methods, but the challenges, especially in our corner of the field, that either have, or should, stimulate methodological innovation.

A major challenge is how to capture and represent more fully the historical or temporal dimension of human action. Both interviewing and most ethnography have limited temporal depth. Yet as sociologists, many of us are interested not in specific “behaviors” as they occur in a particular situation, but in something more like life trajectories, or group trajectories, or even global trajectories. Studying a person, a profession (see Abbott 1988b), or a nation at a given moment may give the false sense that the current situation represents the full causal dynamics in play. Most of our current methods (including both interviews and ethnography) have trouble discovering or analyzing the effects of historical change—shifts in larger cultural and institutional factors that set the context for whatever we currently observe. Renato Rosaldo’s (1980) watershed book about historical change among Ilongot headhunters challenged the notion, then built into the ways most anthropologists conducted ethnography, that what the ethnographer was capturing in one or two years of fieldwork were enduring “native” cultures and practices which had always been there. What Rosaldo found instead were historically dynamic societies with shifting group alliances, clan conflicts, and innovative marriage strategies that constantly disrupted and reformed basic social arrangements.

Increasingly analysts studying contemporary societies have tried to bring a historical dimension to their work. For surveys, longitudinal data collection is expensive, but very valuable for understanding how individuals’ attitudes, careers, and family situations change over time. In-depth interviewers, with few exceptions, rarely revisit the same subjects to get a sense of how the lives they described earlier worked out. Major longitudinal studies, like the Berkeley and Oakland Growth Studies,²⁰ are remarkable exceptions. So is Annette Lareau’s (2011) “update” on the families she had studied a decade earlier, when their children were young, allowing us to see how those children’s lives worked out in young adulthood (also see MacLeod 1995). Others, such as Furstenberg (2007) and earlier Elder (1974) and Hareven (1982), often using a creative combination of in-depth interviewing and documentary evidence, developed a similar long-term view and integrated individual histories with social

²⁰ A broad overview of the Berkeley and Oakland Growth Studies can be found at <http://ihd.berkeley.edu/igs.htm> and at http://lifecourse.web.unc.edu/research_projects/oakland_berkeley/

history. In very different ways, both Tugal (2009) and Mische (2009) have integrated ethnographic methods into a dynamic, temporally-deep picture of the transformation of political identities in periods of social turmoil. More recently, Armstrong and Hamilton (2013) have used a combination of interview and ethnographic methods to add depth to our understanding of how class, culture, and personality interact to shape young women's college trajectories and their access to occupational opportunities.

If the ethnographer stays around long enough, he or she can also learn how things “came out.” Did the couple who pledged eternal love stay together? Did the working-class kid ultimately make it into the middle class? Do the ways people talk about inheritance to an anthropologist shape how children, or cattle, or land are actually redistributed when someone dies? But these critical contexts of people's experience—what they realistically expect or anticipate and what it means—are not likely to be available to the one-year ethnographer, any more than to the one-time interviewer. And indeed, since the ethnographic observer, unless she also interviews, is unlikely to spontaneously hear much about past history, or to have the full implications of current interactions made explicit, for all the interview's limitations, the interviewer might learn as much or more about how (at least some things) “actually came out” than would the ethnographer.²¹ Accordingly, new longitudinal qualitative interview techniques are being developed (Luke et al. 2011; Hermanowicz 2013) and they should prove particularly useful. Moreover, long periods of time in the field or “punctuated ethnography” (see Collier 1997) would be a valuable corrective to hearing what people say at any given moment, or observing what they do at a given moment.

To highlight the difficulty of understanding social processes without being able to follow them historically, we tried to imagine what an ethnographer, an interviewer, or a survey researcher, might have made of the Comstock movement for moral reform at the end of the nineteenth century in New York City, Boston and Philadelphia, which Nicola Beisel (1997) studied. Which method might have grasped the struggle for status group closure among elites across the three East Coast cities? An ethnography of the Comstock movement would have described the advocates' obsessions with moral purity, their passion about outlawing pornography and masturbation, and perhaps their fear that their society was going to ruin. But only a carefully designed interview study, similar to Kristin Luker's (1984) *Abortion and the Politics of Motherhood*, using in-depth interviews to compare advocates and opponents of the Comstock campaigns, might have shown why individuals from particular backgrounds were especially distressed about sexuality, prostitution, pornography, or abortion. Comparative interviews might also have revealed links between those anxieties and individuals' anxieties (or lack of anxiety) about the fates of their own children. A good survey researcher might even have identified correlations between people's class backgrounds (perhaps quite specific elements of those backgrounds) and their positions on the issues. Only a historical comparison across three cities with different contexts of immigrant threat to dominant elites, and with differing degrees of unity among elites, however, allowed Beisel to identify the powerful status group interests, the political capacities, and the context of group struggle that turned anxieties about sex and class preservation into a powerful social movement in Boston, a failed social movement in New York, and no social movement among Philadelphia's securely entrenched upper class. If the larger theoretical goal is to understand class and status-group reproduction, interview-based work on symbolic boundaries, such as that by Lamont (1992), innovative work such as Rivera's (2012) combined interview- and observation-based study of recruitment

²¹ Of course, ethnographers often hear their subjects talk about the past. But it is difficult for them to obtain an extensive narrative without resorting to interviews. This shows the extent to which the distinction between interviewing and ethnography is largely an artificial one.

at elite personal service firms, as well as historical work like Beisel's (see also DiMaggio 1982; Karabel 2005), makes clear what is at stake in political battles among status groups.

In these examples, then, we see the challenge for those using interview and ethnographic methods to sufficiently take into account processes of change in the larger institutional context. We also see, across these diverse cases, the fruitfulness of methodological pluralism, and the special value of studies that bring multiple methods to bear on the same sorts of large questions. More broadly, we would advocate that the method of the comparative case study has remained powerful partly because it requires such a complementary mobilizing of various types of evidence. Glaeser (2011)'s historical ethnography of the complementary worlds of the secret police (Stasi) and dissidents in pre-1989 Berlin also stands as a powerful exemplar of the analytical advantages of taking such a qualitative multi-method approach. Just as a stool with three legs has the greatest stability and strength, a study that mobilizes in a complementary fashion various types of qualitative data can improve robustness and rhetorical power.

New Methods for New Questions

In our field, the sociology of culture, there are several exciting frontiers of social investigation, some of which require—or invite—innovations in methods. In each case, we suggest interesting recent work that has been done, but also where, if we focus attention on creative challenges, real progress is possible.

The Challenge of Integrating Interpretive and Systematic Methods of Cultural Analysis

One of the exciting recent developments in the sociology of culture, beginning with Mohr and Duquenne (1997), Mohr (1998), and carried forward in a variety of ways by Martin (2000), Pachucki and Breiger (2010), Goldberg (2011), and many others, has been the effort to understand, and perhaps “map,” the underlying semiotic codes that structure whole arenas of meaning. This question has been, classically, the preserve of the interpretive analyst (Geertz 1973; Alexander 2003), with all the problems of inter-subjective reliability, and the sheer impossibility of interpreting with real insight a very large number of texts compared over time or across domains. The availability of increasingly powerful computing, with increasing capacity to “read” texts, has created a challenge and an opportunity (here we depart from critics like Biernacki 2012). Innovative scholars like Bail (2012) have found creative ways to use existing technologies, for example, to see what messages resonate with wide publics at particular historical moments. Others have sought in both productive and less productive ways to “reduce” large bodies of texts to manageable, yet meaningful, patterns. In the best of such work, such as McLean's (2007) innovative analysis of networks and patronage letters in Renaissance Florence, deep understanding of the context is combined with both interpretive and more systematic analysis of texts to link semiotic codes, specific meanings, and social-organizational patterns. But there is much more that could be done if those with the deepest commitments to interpretive analysis could combine their insights with those seeking novel ways to tap broad cultural patterns. The current collaboration between Robin Wagner-Pacifici, who does a richly-interpretive form of textual analysis, and Ronald Breiger and John Mohr, who model meaning structures using computer-assisted analysis of large bodies of texts, suggests that some, at least, are trying to bridge these methodological frontiers (e.g. Mohr et al. 2013). By using “big data” these authors are able to identify patterns of distribution of meaning that are simply not available via interviews, ethnography, or traditional interpretive

analysis. When used in a theoretically-informed fashion (as opposed to an all too frequent mode of “data mining”), such analysis promises to make possible the identification of deep structures of meaning that underlie everyday interaction.

The Challenge of Studying “Contexts” or “Situations”

Ethnography alone normally has great trouble explaining why situations are the way they are, even if it can describe situations with greater richness and depth than other methods usually allow. Despite their immersion in and their deep knowledge of particular situations, it is problematic for ethnographers to assess the “effects” of situations if there is no comparison across situations. So, for example, studying a housing project, or a street corner, may allow analysis of the varied contexts individuals in that community confront in the ordinary course of their lives, but it does not systematically tap the influence on action of the street corner or the housing project itself. Work such as that of Eliasoph (1998) and Eliasoph and Lichterman (2003), which directly analyzes the effects of situations, is rare outside the world of social psychology, where such issues have been investigated systematically (see Vaisey 2014b and Mischel and Shoda 1995 for a classic example). Carefully-designed multi-site ethnography can assess effects of situations, as can multi-site interview studies, and even single-sited ethnographies sometimes allow internal comparisons of variations across situations, although it can nonetheless be difficult to know precisely which features of situations are causally important in shaping action. Nonetheless, the assumption that if one has closely observed a situation, one knows “why” the actors acted as they did, is not persuasive in our view. Indeed, the power of sociological explanation is often that it extends analysis beyond the immediate situation.

Indeed, ethnography as currently practiced in sociology usually involves the ethnographer’s immersion in a single community, often one that is dramatically different from the analyst’s home milieu. The ethnography succeeds if lives that might have seemed alien and motives that might have seemed incomprehensible start to seem more understandable. Such broadened understanding certainly contributes to the explanation of social processes. But ethnographers, like interviewers, need to seek ways of analyzing the broader cultural codes as well as the origins and logics of the structural circumstances that contribute to making the communities the ethnographer studies what they are. We should remember that even an understanding of people’s motives, and of immediate constraints and opportunities as they present themselves to individuals, does not necessarily provide an explanation of the situation in which these others find themselves.

Conclusion

This brief *tour d’horizon* barely scratches the surface of a much broader set of issues. The lessons to be learned from the wave of methodological exchanges that the post-millennium decade has brought us are many. One is that different methods shine under different lights and that one should choose the most appropriate data collection technique based on the question being asked and the types of facts and theories one wants to operate with. Another is that substantive innovation often emerges from a fearless orientation toward mixing methods and research genres so as to develop a multidimensional understanding of social phenomena. Going down a well established path has rarely been conducive to major intellectual innovation. Moreover, thinking about causality adequately often requires going beyond “being there” and beyond the street corners so celebrated by classical ethnographers, to consider causality and/or historicity in a wide ranging set of enabling and constraining factors. In this digital age, the

contexts of our lives are reaching scales that cross time and space in a way that was barely imaginable two decades ago. Our theories and methods have to follow if we are to continue to be a dynamic and engaging field. This is the challenge that we should try to meet together.

Cultural sociology has been a major intellectual social movement in the discipline over the past 30 years. Its successes are directly linked to the ability of its knowledge producers to create a big umbrella instead of feeding factionalism. We believe the same productive intellectual cross-fertilization can continue to nourish our field as the interest of younger scholars returns to important topics, such as that of urban poverty. Building bridges, not digging deep moats, is the key to fostering a strong field.²² More specifically, a communal effort toward a methodological pragmatism is more likely to lead us in a fruitful direction than the tit-for-tat that can be fed by methodological tribalism. While Abbott (2001) and Bourdieu (1990) have argued that fractionalization is an essential characteristic of academic fields, ebbs and flows do lead to more or less productive debates. In this paper, we have aimed to steer the conversation in a different direction, which we hope will have a positive effect on future theoretical developments.

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²² The current debate is a replay of old tendencies within symbolic interactionism, as Blumer was also criticized for advocating the exclusive use of ethnography, which is not without perils. In Best’s (2006) words, “Following in Blumer’s footsteps, interactionists run the risk of retreating into armchairs cushioned by theoretical presumptions we find comfortable, and then characterizing this retreat as principled” (12). We are hopeful as a number of the younger practitioners of the tradition are already distinguishing themselves by their theoretical acumen and their ability to connect with a range of traditions, which include French and American sociological pragmatism as well as cultural sociology.

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