How Do We Know What We Mean? Epistemological Dilemmas in Cultural Sociology
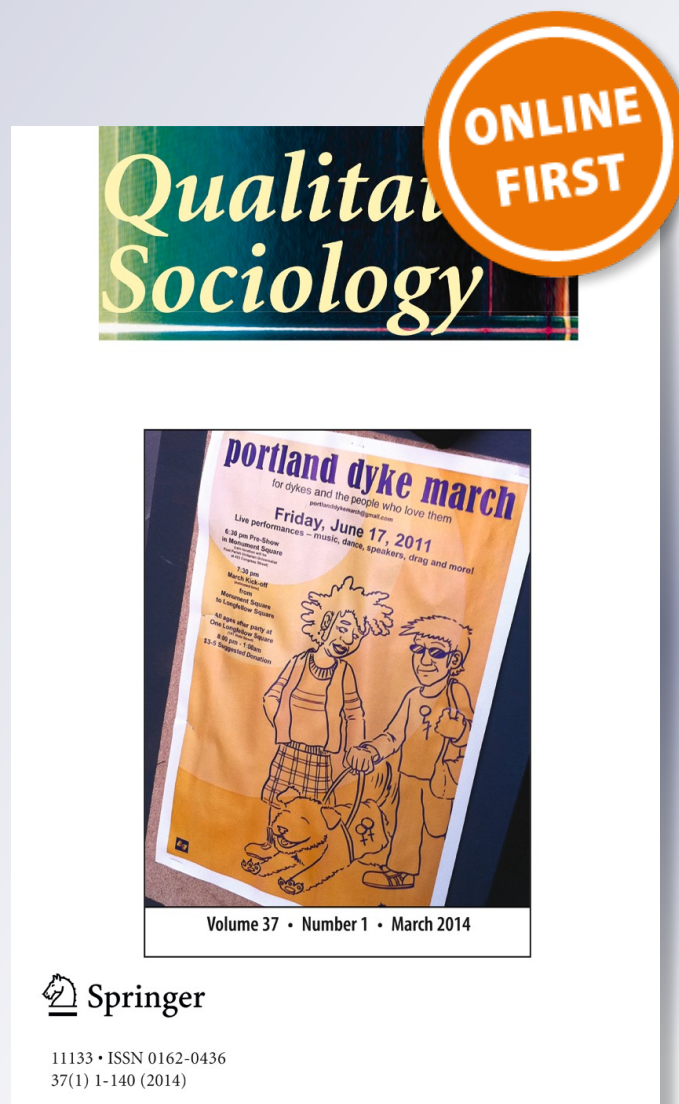
Mabel Berezin

Volume 37 · Number 1 · March 2014

Springer

Springer

Springer

How Do We Know What We Mean? Epistemological Dilemmas in Cultural Sociology

Mabel Berezin

Culture complicates. Sociologists, whether qualitative or quantitative in methodological orientation, tend to rely on a repertoire of analytic categories that structure research questions and answers. When sociologists ask, for example, if there is a relation between gender and individual income or education and occupation, we are reasonably sure of our findings because we are working with relatively fixed categories and measures. As sociologists, we know that there are a variety of social phenomena that prior research tells us interact with one another. When we submit this prior knowledge to process questions, that is, when we submit an established social relation to a *why* and a *how* question, we ratchet our analyses up another level. When we ask what our findings or categories *mean*, we subject relative certainties to a Pandora's box of contingencies. Interpreting the meanings of gender or the culture of aspiration (for example, Willis 1981/1977); Lareau 2011) that surround decisions regarding work and education is a nuanced endeavor that involves complexities that are avoided by structural and behavioral explanations of research results.

Sociologists of various stripes structure their research around the general question: How do we know that we know it? In contrast, cultural sociologists ask, "How do we know what it means?" Adding the question of culture—and culture and meaning are in many respects interchangeable—to sociological analysis brings richness, but it also brings methodological dilemmas that other sub-fields do not tend to encounter. In my own work, I often argue that cultural sociology is as methodologically rigorous as other forms of sociology. Cultural sociologists achieve rigor by building controls of various kinds into our research designs and paying particular attention to the strengths and limitations of our data. Methodological appendices have become as important a component of our books and articles as the content itself.

My first book on political communication and ritual in Fascist Italy (Berezin 1997) ended with a lengthy methodological appendix that described in minute detail how I

M. Berezin (✉)
Cornell University, Ithaca, NY 14853, USA
e-mail: mabel.berezin@cornell.edu

had collected my data, what I thought that it was able to show, and what its limitations were. When one works as I did—on historical materials from troubled periods—not only are potential informants dead, but even if they were alive it would be unlikely that they would be telling you anything other than a highly constructed version of reality. Historical documents from these periods are equally questionable. The extent to which these documents exist in archives is a proxy for their salience. What I have learned from working on the political culture of troubled times is that, whether your sources are newspapers, party propaganda, films, memoirs and even laws, the evidence available is always a story of what others would like you to believe rather than what they or their citizens believe or even what was in some sense actually happening in a society. Understanding this constitutive feature of non-democratic political cultures and regimes has served me well in my ongoing research into contemporary politics but also as a sociologist of culture more generally.

As cultural sociologists, we are always looking for deeper layers of social and political reality as we try to understand individual and collective behavior. All social science research confronts problems of epistemology, which often find solutions in discussion of method. Charles Ragin's (2000) work on "fuzzy-set" social science fits this category, as well as Gary Goertz and James Mahoney's (2012) recent extended comparison of quantitative and qualitative methods. Anthropologists and ethnographers have always been willing to reflect on method. But, a self-conscious social science of culture remains somewhat elusive. As I began to develop panels for the 2013 annual American Sociological Association (ASA) meeting in my role as chair of the culture section, I began to think about the ways in which our members positioned themselves with respect to methodology, evidence and design in their work. Since its beginning in 1987, the ASA culture section has grown by leaps and bounds. Culture is now one of the largest ASA sections, with over 1,200 members at last count. A large reason for this growth is that sociologists working in various subfields of our discipline have begun to recognize the importance of meaning and culture to their research findings. It is now unremarkable to encounter sociologists who work on "culture and"—and the "ands" range widely over economic sociology, political sociology, inequality studies, gender, family and the list goes on.

Cultural sociologists care about the relation between methods and meanings, and are often acutely aware of positioning themselves via their quantitative "others." Leading cultural sociologists have written about method, and we might argue that as a group we are hyper-conscious of it. Articles written in the early years of the section addressed methods broadly. Wendy Griswold's (1987a) article "A Methodological Framework for the Study of Culture" remains a relevant and compelling exegesis upon how cultural sociologists should address method and design. Griswold's discussion of "parsimony, amplitude and plenitude" as criteria for validity suggests that her goal was to push cultural sociology in the direction of scientific method. Her insistence on the necessity of comparison embedded in research design remains an important feature of rigorous cultural analysis. Her careful parsing of the elements of cultural production reflected the concerns of the emerging section, which tended to define culture and cultural objects more narrowly than it does today. In a similar vein, Paul Dimaggio (1987) wrote on classification categories that unambiguously focused upon art. Ann Swidler's (1986) article, "Culture in Action," was a first step in the direction of broadening subject matter and has characterized the development of the section. Of particular note, all three of these articles appeared in top journals in the discipline and served the role of agenda setting.

In the early 1990s, the section began to expand its focus as comparative historical sociologists and political sociologists (for example, the essays in *State/Culture* [Steinmetz 1999]) began to incorporate culture in their analyses. During the same period, Michele Lamont (2000b) called for cultural sociology to "broaden" its agenda and John Mohr (1998) raised the question of measurement. A younger generation of scholars such as Paul McLean (1998) wed culture to networks and historical analysis. We have now moved to a stage where "big data" is making inroads in cultural analysis. In a recent special issue of *Poetics* (2013), Mohr and his contributors illustrate the utility of "big data" to a range of cultural questions. Christopher Bail (forthcoming) has carefully analyzed how "big data" methods may contribute to cultural analysis. Bail (2012) used "big data" to great effects in a recent *ASR* article on how extraordinary terrorist threats, many of which never occur, end up, due to media selection, as defining the global Islamic community.

Culture as an analytic category is a staple of many academic disciplines such as anthropology, literature and history. Max Weber (1968) wrote about the logic of the cultural sciences and defined sociology as one of these. In the *longue durée* of intellectual life, a concern with culture is relatively new to sociology—particularly post-war American sociology with its positivistic and scientific emphasis. This is true, even if some members of the Chicago School produced their own version of cultural sociology by focusing upon what they described as the definition of the situation. In any case, cultural sociology is now a mature subfield—meaning that it is an established part of our discipline. The production of culture approach, a subset of organizational sociology, was heavily represented when the culture section began but now represents a smaller, albeit sophisticated, segment of the section (for example, Grazian 2003; Roy and Dowd 2010; Lena 2012).[1] As what constituted culture widened as the 1980s turned into the 1990s, this widening of cultural sociology, which came to include different subfields and distinctive conceptual approaches to the sociological study of culture, began to consolidate. For example, we can identify Lamont's emphasis upon boundaries (Lamont and Molnar 2002) and national cultural repertoires (Lamont and Thévenot 2000); Jeffrey Alexander's (2003) "strong program" with its emphasis upon cultural performance; the interactional approach of Nina Eliasoph and Paul Lichterman (2003); and a new entry with Stephen Vaisey's (2009) "dual-process model."

The ASA culture section is now entering its 27th year. As a mature subfield, it is time to self-consciously address how we make claims about meaning in cultural analysis. With this in mind, for the 2013 ASA Meetings I designed a panel of scholars whose work was deeply empirical but who had also given considerable thought to the question of method in their own work as well as more generally. I named the panel *Methods, Materials and Meanings: Designing Cultural Analysis*—the same title as this special issue. I asked panelists to address the following in their papers: "Questions of epistemology, meaning and agency are constitutive of cultural analysis and pose ongoing challenges for research. Please discuss how you deal with questions of evidence and method in your empirical work, and second, generalize more broadly to our sub-field and the discipline as a whole on the issue of cultural analysis and research design."

---

[1] This was not surprising as one of the culture section's founding members, Pete Peterson, specialized in art and music production. Peterson's 1976 anthology, *The Production of Culture*, was considered a seminal work by members of the new section.

The articles

The contributors to this volume have distinguished themselves through their empirical work in various subfields of our discipline as well as the strong lens they have cast on their research methods and design. Their body of scholarship testifies to the fact that they have thought long and hard about incorporating culture in their analyses. All of the volume's contributors are comparativists. Working across geographical boundaries, cultural difference is implicit in their research projects. Michele Lamont has brought cultural analysis to the study of inequality (2000a) and disciplinary standards (2009). Ann Swidler has worked broadly in organizations and emotions (2001). She is now bringing her sociological acumen to health practices (Tavory and Swidler 2009). Richard Biernacki and Andreas Glaeser are comparative historical sociologists who have incorporated culture and history in their work. Biernacki (1995) has studied comparative labor practices across three centuries in Britain and Germany; Glaeser has honed his political cultural skills on studies of the Cold War German Stasi (2011) and police (2000). Lyn Spillman is an economic and cultural sociologist who recently has studied the cultural significance of business organizations (2012). She has also studied national commemorations in earlier work (1997). John Hall, who provides a concluding commentary in this issue, has published widely in cultural theory and method (for example, Hall 1999). He is also the author of culturally sensitive case studies such as the study of the Jonestown collective suicides (Hall 1987). Hall engages each of these articles analytically, while I locate them descriptively and attempt to tease out their highlights.

"In Praise of Methodological Pluralism" by Michele Lamont and Ann Swidler addresses the "methods" part of the panel's original charge and adds a theory component to the discussion. Needless to say, the authors are in favor of "methodological pluralism." They argue broadly that methodological approaches are neither intrinsically good nor bad. Methods are only as good as the extent to which they shed light upon the question a researcher is trying to answer. Culture poses more problems for research design than for example, social location, because students of culture are trying to get to deeper meanings that structure social action, and those meanings are not as obvious. A research subject's occupation and residence still yields sociological analytic mileage in contrast to trying to nail down, for example, "what's the matter with Kansas" (Frank 2004) as a mass marketed book tried to do some years ago.

Lamont and Swidler are experienced interviewers and much of their article is structured around the strengths and weaknesses of interviewing in contrast to other methods of cultural analysis. While ethnography is the principle focus of their discussion, they also introduce the role of political culture and institutional analysis at the end of the article as well as briefly commenting on the new opportunities that "big data" present to cultural analysts. The article finds some motivation in recent debates (Jerolmack and Khan Forthcoming; Pugh 2013; Vaisey 2009) that, in my view unfortunately, juxtapose ethnography against interviewing, as if there was some sort of competition involved.

Evoking Clifford Geertz (1973) and his concept of "thick description," I would argue that Lamont and Swidler are "thick interviewers;" that is, as cultural analysts, they are after more than the simple answers to the questions they pose to their research subjects. Lamont and Swidler argue that the type of interview style that they have pursued does not ask persons what they do, i.e., how many times have you gone to the refrigerator today to snack or how you "feel" about various social phenomena (i.e., do you hate your neighbor and why?). Rather they

use guided open-ended interviews and conversational prompts to uncover deeper meanings. For example, Lamont (1992) excavated a wealth of cultural knowledge in her early work when she asked a sample of French and American male members of the upper-middle class to whom they felt superior. The style of "thick" questioning that Lamont and Swidler have mastered in their empirical work has enabled them to connect personal biographies and dispositions to embedded national cultures and legacies—the location where individual experience meets collective practices and meanings.

But interviewing, even if it is pursued in the style that Lamont and Swidler advocate, is not the be all and end all of qualitative cultural research. They correctly point out that every good interviewer is an ethnographer: How can one ignore the milieu that one's interview subject inhabits? Even if one conducts an interview in a public space, how could a cultural analyst or any analyst not notice their subjects' clothes, speech patterns, etc.?

The key to interviewing and ethnography is how researchers connect their observations to their subject's embedded individual and collective histories and legacies—the point where collective and individual experience meets practice and meanings. Ethnography, according to Lamont and Swidler, has its merits because it lends texture to analysis in a way that interviewing alone often cannot. But, most ethnography today is based on fairly brief periods of intensive research. The exigencies of contemporary graduate school and faculty careers dictate that many ethnographers do not have the luxury of spending more than one year in their field site. And a year is a short time for the total immersion that used to characterize past anthropological practice. The other danger of ethnography is that if a researcher does manage to acquire the luxury of time, he or she might become so embedded in their research site that they lose sight of where the research ends and their own "real" lives begin—what anthropologists used to label the danger of "going native."

Comparison is, of course, the answer, and even ethnographers can compare with what Burawoy (2003) praised as the "re-visit," which allows analytic cultural leverage. Lamont and Swidler cite Lareau's (2011) second addition of her *Unequal Childhoods* that shows how the lives of children that she had interviewed over a decade earlier turned out. In my own work (Berezin 2007), I visited a French right wing's party's annual festival in 1998 and again in 2004. During my first visit, I felt deeply uncomfortable and "othered;" during my second visit, the party had professionalized and was attracting a different sort of operative and membership. I now fit in visually. The only thing about me that attracted attention was the fact that I was alone (none of my French friends would dream of going with me!), and who attends a political festival alone?

Richard Biernacki's contribution "Humanist Interpretation versus Coding Text Samples," shifts our attention from methods to measures, or, as my title suggests, to "materials" or evidence. Biernacki continues an argument that he began in a monograph, *Reinventing Evidence in Social Inquiry* (2012). His contribution is in the tradition of a polemic and its object is not data or evidence per se, but the handling of data and evidence. To put it bluntly, if simply, Biernacki criticizes the way that cultural sociologists, in the name of science, have often handled the coding of textual data of various forms—from novels to diaries to policy documents—to make cultural claims. In the period after World War II, American social scientists developed and refined text coding as a major technique to manage and scientize content analysis. Biernacki's paper covers a wide range of contemporary sociological practicioners of coding. Yet, he does not go back to the origins of coding in studies of political communication and propaganda that Lasswell and Leites (1949) and their collaborators developed. With the arrival of "big data" techniques (again, see Bail forthcoming), some of the issues of categorization that Biernacki takes up may either become obsolete or, worse, multiplied.

Nonetheless, it is not the reading of the texts but their interpretation that is the core of Biernacki's critique, and his criticism would hold whether a machine or a human were doing the readings. Coding often rips the objects or phenomena of cultural analysis from their contexts. In short, the categories that an analyst must construct in order to code removes history (temporality) and geography (spatiality) from the analysis, thus eliminating vital components of meaning, hence culture.

Coders, however, are not always so blind to time and space. Many try to incorporate change over time in their coding; and Biernacki's example, Griswold's article on the "The Fabrication of Meaning" (1987b), precisely tries to get at trans-cultural forms by holding constant a cultural artifact, the novels of George Lamming, and looking at their critical reception in three different literary cultural spaces.

Biernacki does not take design fully into account. Answers to certain questions (how you choose your sample to code, what you choose to code, what you will code over time, what you will try to incorporate different cultural spaces) often provide the context that, according to Biernacki, coding per se ignores or attenuates. In this sense, many of the works that he singles out for critique are not culpable of social science malpractice. In general I am sympathetic to coding, as it does have the capacity to illuminate certain issues that other methods of cultural analysis might lack (for example, it has the advantage of illuminating central tendencies). However, it may undercut what in other instances I have described as the "spectacular event" worth pondering (Berezin 2012). To stay in the literary field for a moment, Griswold has argued from her earliest work that it is only from the entire range of materials published in a given time period that we can understand the cultural meaning of a work. But this approach can also leave us unable to analyze, say, the remarkable literary events that become transnational best sellers. Can any form of coding or sampling technique account for the remarkable international success of the Stieg Larsson trilogy about a Swedish computer hacker, a muckraking left wing journalist and decade old sex crimes? Or the sometimes cultish following of a television series such as *Mad Men*? Sometimes exceptions are as culturally meaningful as central tendencies.

Biernacki's paper is not all critique. He offers what he describes as the "humanist alternative" to the current misuse of coding. He offers eight substantive critiques of coding with an interpretive alternative. As a counterpoint to coding, he poses Max Weber's conception of an ideal type. But, in the end, Biernacki is making a plea that cultural analysis is by definition historical analysis, and that any cultural analysis that purports to get at collective meaning must also find a way to get into the core of individual or collective experience—the temporal moment where the present interrogates the past and moves forward.

In "Mixed Methods and the Logic of Qualitative Inference," Lyn Spillman, takes on the issue of qualitative versus quantitative data in ways that complement and contrast with the other articles in this volume. She addresses issues of epistemology head on. Spillman's article begins by laying out the core assumption that is virtually axiomatic in quantitative approaches to social science. Iterations of that core assumption usually take the following form: Quantitative data is constitutive of scientific analysis of social processes because it permits us to generate random samples based on large-N data. The process of randomization enables us to develop testable hypotheses that permit us to weigh and assess the comparative merits of various causal relations that we garner from the data, while being aware of the limits within which the data are representative. Quantitative data analysis is scientific because it allows for falsifiability in a reliable manner. What often gets lost among those who enter discussions of the merits of quantitative versus qualitative data analysis is that hypothesis testing only results in an explanation of a process that is more or less likely than another one. No social science based on sampling produces or reproduces absolute reality, or truth (take your pick of terms here).

Given that as social scientists we are dealing with approximations, no matter what the size of our data set or the methodology that we deploy, what is, if any, the best way to proceed? Spillman steps into the fray with a review of the work of four sociologists: Michael Burawoy, Mario Small, Isaac Reed and George Steinmetz. Focusing upon their research methods of choice, ethnography versus historical analysis, each of these four have argued in their own distinct way that qualitative case analysis can be as "scientific" as more standard quantitative methods. Spillman's choice of foils is inspired because it allows her to open up a new analytic space. She persuasively argues that, after juxtaposing the arguments of all four against each other, "there is no single alternative model to conventional statistical inference from quantitative data, and it is increasingly meaningless to make simple binary oppositions (i.e., *quantitative* vs. *qualitative*)" (13).

She then asks the key question, which not only informs her article but also is the core of cultural analysis: What is the appropriate role of quantitative data in qualitative analysis, or qualitative case materials in quantitative analysis? Posing the question in this way is important because it allows Spillman to take on the recent trend in quantitative research that touts the virtues of "mixed methods" studies. Although she does not formulate it in this way, Spillman's argument brings up a number of interesting questions regarding the popularity of mixed methods: What exactly do numbers add to a case study, or vice versa? Is there a strong analytic reason for pursuing mixed methods? Or is this just a way to enable quantitative social scientists with access to research funding to add interviewing, ethnography or historical work to the stew pot of a research project?

Spillman skillfully navigates through number of works and positions on the subject of large and small-N research. She assembles nuanced comparisons and contrasts, carefully weighing positions against each other. Her innovation is the introduction of her concept of *thin description*. Drawing upon Clifford Geertz's (1973) well-known concept of *thick description*, Spillman asks why quantitative data could not add to the explanatory power of a small-n study by allowing analysts to locate their study on the landscape of all such instances of the object under study. In other words, Spillman argues that quantitative data, rather than existing in opposition to qualitative case analysis, could serve as one of its tools. Spillman cites several authors who have pursued this tactic. From my own experience, quantitative data lends cogency to qualitative arguments. In effect, Spillman has given us a name for what many of us do. One of the important and novel insights of this article is that it valorizes the explanatory power of description on multiple levels.

Andreas Glaeser's contribution moves us from epistemology to ontology. In contrast to the other contributors, Glaeser proposes a new analytic category and ontological status: "hermeneutic institutionalism." The empirical focus of Glaeser's (2000, 2011) prior work has been the institutions of surveillance and control that dominated in the former German Democratic Republic (GDR), as well as in post-unification Germany. In East Germany, fear of and worry about the secret police, the Stasi, dominated the public imagination. In post-1989 Germany, the police were actively engaged in the formation of the new German state's security and identity.

Glaeser's intense empirical engagement with these state agencies of coercion has given him an angle from which to see aspects of institutions that elude other theorists. Glaeser acknowledges that institutions are a bread and butter concept of sociological thinking, but argues that most institutional analysis takes inadequate account of temporality. He develops the concept of "instituitiosis," according to which language plus emotional and temporal flows are constitutive of institutions. Temporality is at the very core of "institiosis." In much the same spirit as the Biernacki's contribution, Glaeser carefully documents the *lacunae* in institutional theory.

Glaeser argues first of all that standard theories of institutions view them as mutually interdependent and self-sustaining. In this section, Glaeser takes on many, if not all, of the

leading scholars of institutions, although references to March and Olsen (1989), the arguable leaders of the institutional turn in social science, are curiously absent. The second feature of institutional analysis that Glaeser takes exception to is the standard argument that certain aspects of institutional design affect outcomes. The human capacity to negotiate institutions—their fluidity and maneuverability—would be one lesson learned from studying surveillance institutions such as police. "Instituitiosis" as a concept allows for what Glaeser calls "lateral association." As an example, he uses a wedding ceremony. All weddings legally place two people in the institution of marriage and create a family. But there are many different ways in which weddings may take place. The institution of marriage may have a legal fixity to it—even in a cross-cultural context. The rituals around marriage vary in multiple ways and signal cultural variation and agency, and may also make the institution of marriage look very different in different cultural spaces.

Glaeser's argument is very nuanced and philosophically qualified at every turn. In this brief introduction, I can only provide the flavor of his style of argument. But his principle point is that there is an extreme fluidity to social life—what he labels a "processual ontology"—that also extends into the world of institutions, whether they be political, social or economic. Surprisingly, his analysis neglects to include religious institutions, which I would place at the core of the collectively fungible. Nonetheless, the central tenet of "instituitiosis," the claim that "everything is emerging," does present a challenge to standard social science method and analysis. Glaeser's argument may challenge traditional social analysis, but it also gets to the core of cultural analysis that has to capture the variety of the material and mental world. The important contribution of this provocative paper is its claim that culture generates institutions and not simply meso-level structural phenomena.

Going forward

In a forthcoming *Annual Review of Sociology* article, Orlando Patterson (Forthcoming) reminds us that culture is still an unresolved notion within sociology. The stimulating articles in this volume attest to the value of his insight. After describing the articles in this "Introduction," I have three qualifications that I would like to put forth as readers move forward with this volume. First, we must be careful about *theory shading into method, or method shading into theory*. Theory and method are related but they are also distinct and we should be aware of that distinction moving forward. Second, we should be alert to the *slippery slope of interiority*. While culture is indeed particular and complicating, we should attempt to bring discussions of method into closer alignment with other discussions of method (again for example, Goertz and Mahoney 2012). In this regard, it would be useful to think about issues of falsifiability and cumulation. Lastly, in the end, we should not ignore what I label the *radical contextuality of meaning*. Cultural sociologists should embrace the "thickness" of culture rather than running away from it. We should push ourselves to understand the deep meanings that, again invoking Geertz, enable us to distinguish between a "twitch" and a "wink," and pay more attention to the descriptive and narrative dimensions of culture.

To end where I began, culture complicates and it is often in the complications that meanings, as well as social analysis, emerge. Take, for example, the two photographs on the cover of this issue. I took these pictures in July 2010. I visited the immigration history museum, *Cité Nationale de l'Histoire de l'Immigration* (CNHI), on the outskirts of Paris to see an exhibit entitled *Football* (soccer to many of you) *and Immigration: Intertwined Histories*. Many of the great French soccer players came from immigrant backgrounds. The spring and summer of 2010 seemed as good a time as any for the French to mount an exhibit to

praise the virtues of integrated immigrants. The World Cup was occurring in South Africa between June 11 and July 10, and then French President Nicolas Sarkozy was starting a national identity campaign as well as beginning an initiative to ban the burqa in France. There was a special event in May at the museum to honor the great player Zinedine Zidane who led the French team to victory in 2002. Zidane was born in France to Algerian immigrant parents (see cover photo). He was and is a clear role model for French youth.

Given that I am not known for my strong interest in soccer or sport in general, why was I headed out to the soccer exhibit on a that sunny summer day? In summer 2010 French soccer and Zidane himself had fallen on hard times. As a scholar of cultural politics, and more recently European and French politics, the exhibit caught my eye when I read about it in a French newspaper. Soccer in the European context, with its strong nationalist undertones, is always more than simply a game. Since 2002, soccer in France had become a blank slate upon which French citizens and politicians projected both national triumph and anxieties about national decline.

In July 2002 France won the World Cup, which had been played in France.[2] Zidane was the player that carried the French team to victory. April 2002 witnessed Jean Marie Le Pen, leader of the right wing French National Front party, come in second place in the first round of the French presidential election. Although Jacques Chirac cruised to victory in the second round, the French viewed Le Pen's second place as a national disgrace and source of collective embarrassment. The soccer victory lifted the national mood. Chirac used the occasion to celebrate French republicanism and universalism by making the soccer victory the center of the annual Bastille Day celebrations. But 2002 did not repeat itself, at least in the realm of French soccer. In 2006, France made it to the final game of the World Cup, but Zidane disgraced himself—and by extension France—when he head-butted an Italian player. The Italians won. The 2010 World Cup proved even more difficult for the French. The French team was eliminated from competition after the third game. The French public, aided by the media and politicians, attributed the failure of the team to rebellion in their ranks. Before the third game, the coach expelled a player of immigrant origin. The multiracial team bonded in solidarity against the coach and refused to practice. The team returned home to France in disgrace and the failure of the team was discussed in terms of the failure of integration.

With this background in mind, I approached the exhibit, which was physically located in a huge building that had originally served as the entrance hall to the 1931 Paris Colonial Exhibition. I wondered around the museum, which on a Friday afternoon was relatively empty except for a few school children. I was probably the only person who might qualify as a tourist. The soccer exhibit ended with a five minute video of Zidane and his 2002 winning play, shot in slow motion with the theme from *Star Wars* as accompaniment! The story of French football ended in 2002—no losses or head-butts exhibited. As I left the exhibition, I walked into a huge center hall and there I encountered a few teenage boys happily playing a table soccer game (see cover photo). This multiracial group caught my attention and I asked if I could take their pictures. They laughed and laughed, as well as answering me in perfect French, and then went back to their game as my photograph captures. So ends my visit to the museum, but the narrative goes on.

When the editors of *Qualitative Sociology* asked for a cover image, I went into my photography archive, as I regularly take pictures when I am on a research trip. My short concluding narrative raises several questions relative to cultural analysis—only some of which I will try to answer. I told the editors that I wanted to use these images only if I could juxtapose both. It is in the juxtaposition that much of the interpretation or cultural analysis occurs. On one level, if one simply entered this scene with no prior knowledge, the youth playing table

---

[2] Berezin (2009, 111–119) describes the 2002 World Cup events in detail.

soccer would seem to confirm the message of the museum. Immigration and sport integrate. But there is prior history. France's recent relation to soccer is not straightforward.

Soccer is fraught with history, politics and national identity. In some sense, French soccer is carrying more political and cultural weight than it can manage. In summer 2010, Sarkozy was beginning to come down hard on immigrants and immigration in many ways. And life in the *banlieue,* housing projects on the outskirts of Paris, is hardly a multicultural picnic. Culture complicates but it also elucidates and, in some instance, simplifies. Culture is thin as well as thick. It is our job as analysts to work among its multiple levels. But there is also more—there always is in social life. If you are a poor urban teenager having a free, clean, cool and safe space to play (even under the guise of an educational experience) is not such a bad thing. Sometimes a soccer game is only a soccer game.

References

Alexander, Jeffrey C. 2003. *The meanings of social life: A cultural sociology.* New York: Oxford University Press.

Bail, Christopher A. 2012. The fringe effect: Civil Society organizations and the evolution of media discourse about Islam since the September 11th attacks. *American Sociological Review* 77: 855–79.

Bail, Christopher A. Forthcoming 2014. The cultural environment: Measuring culture with big data. *Theory and Society* 43.

Berezin, Mabel. 1997. *Making the fascist self: The political culture of interwar Italy.* Ithaca: Cornell University Press.

Berezin, Mabel. 2007. Revisiting the French Front National: The ontology of a political mood. *Journal of Contemporary Ethnography (Special Issue: Racist and Far Right Groups).* 36(2): 129–146.

Berezin, Mabel. 2009. *Illiberal politics in neoliberal times: Culture, security and populism in the new Europe.* New York: Cambridge University Press.

Berezin, Mabel. 2012. Events as templates of possibility: An analytic typology of political facts. In *The Oxford handbook of cultural sociology*, ed. Jeffrey C. Alexander, Ronald Jacobs, and Philip Smith, 613–635. Oxford: Oxford University Press.

Biernacki, Richard. 1995. *The fabrication of labor: Germany and Britain, 1640–1914*. Berkeley: University of California Press.

Biernacki, Richard. 2012. *Reinventing evidence in social inquiry: Decoding facts and variables*. New York: Palgrave Macmillan.

Burawoy, Michael. 2003. Revisits: Toward a theory of reflexive ethnography. *American Sociological Review* 68: 645–679.

DiMaggio, Paul. 1987. Classification in art. *American Sociological Review* 52: 440–55.

Eliasoph, Nina, and Paul Lichterman. 2003. Culture in interaction. *The American Journal of Sociology* 108: 735–94.

Frank, Thomas. 2004. *What's the matter with Kansas? How conservatives won the heart of America*. New York: Henry Holt & Company.

Geertz, Clifford. 1973. *The interpretation of cultures; selected essays*. New York: Basic Books.

Glaeser, Andreas. 2000. *Divided in unity: Identity, Germany, and the Berlin Police*. Chicago: University of Chicago Press.

Glaeser, Andreas. 2011. *Political epistemics : The secret police, the opposition, and the end of East German socialism*. Chicago: University of Chicago Press.

Goertz, Gary, and James Mahoney. 2012. *A tale of two cultures: Qualitative and quantitative research in the social sciences*. Princeton: Princeton University Press.

Grazian, David. 2003. *Blue Chicago : The search for authenticity in urban blues clubs*. Chicago: University of Chicago Press.

Griswold, Wendy. 1987a. A methodological framework for the sociology of culture. *Sociological Methodology* 17: 1–35.

Griswold, Wendy. 1987b. The fabrication of meaning: Literary interpretation in the United States, Great Britain, and the West Indies. *The American Journal of Sociology* 92: 1077–117.

Hall, John R. 1987. *Gone from the promised land: Jonestown in American cultural history*. New Brunswick: Transaction Books.

Hall, John R. 1999. *Cultures of inquiry: From epistemology to discourse in sociohistorical research*. New York: Cambridge University Press.

Jerolmack, Colin, and Shamus Khan. Forthcoming. Talk is cheap: Ethnography and the attitudinal fallacy. *Sociological Methods and Research*.

Lamont, Michèle. 1992. *Money, morals, and manners: The culture of the French and American upper-middle class*. Chicago: University of Chicago Press.

Lamont, Michèle. 2000a. *The dignity of working men : Morality and the boundaries of race, class, and immigration*. New York: Russell Sage.

Lamont, Michèle. 2000b. Meaning-making in cultural sociology: Broadening our agenda. *Contemporary Sociology* 29: 602–7.

Lamont, Michèle. 2009. *How professors think: Inside the curious world of academic judgment*. Cambridge: Harvard University Press.

Lamont, Michele, and Virag Molnar. 2002. The study of boundaries in the social sciences. *Annual Review of Sociology* 28: 167–95.

Lamont, Michèle, and Laurent Thévenot (eds.). 2000. *Rethinking comparative cultural sociology: Repertoires of evaluation in France and the United States*. Cambridge: Cambridge University Press.

Lareau, Annette. 2011. *Unequal childhoods: Class, race, and family life*. Berkeley: University of California Press.

Lasswell, Harold D., and Nathan Leites. 1949. *Language of politics; Studies in quantitative semantics*. New York: G.W. Stewart.

Lena, Jennifer C. 2012. *Banding together: How communities create genres in popular music*. Princeton: Princeton University Press.

March, James G., and Thomas Olsen. 1989. *Rediscovering institutions: The organizational basis of politics*. New York: Free Press.

McLean, Paul D. 1998. A frame analysis of favor seeking in the renaissance: Agency, networks, and political culture. *The American Journal of Sociology* 104: 51–91.

Mohr, John W. 1998. Measuring meaning structures. *Annual Review of Sociology* 24: 345–70.

Mohr, John W., and Petko Bogdanov. 2013. Introduction—topic models: What they are and why they matter. *Poetics* 41: 545–69.

Patterson, Orlando. Forthcoming 2014. Making sense of culture. *Annual Review of Sociology* 40 (August).

Peterson, Pete A. 1976. *The production of culture*. Beverly Hills: Sage.

Pugh, Allison J. 2013. What good are interviews for thinking about culture? Demystifying interpretive analysis. *American Journal of Cultural Sociology* 1: 42–68.

Ragin, Charles C. 2000. *Fuzzy-set social science*. Chicago: University of Chicago Press.

Roy, William G., and Timothy J. Dowd. 2010. What is sociological about music? *Annual Review of Sociology* 36: 183–203.

Spillman, Lyn. 1997. *Nation and commemoration: Creating national identities in the United States and Australia*. Cambridge: Cambridge University Press.

Spillman, Lyn. 2012. *Solidarity in strategy: Making business meaningful in american trade associations*. Chicago: The University of Chicago Press.

Steinmetz, George. 1999. *State/culture: State-formation after the cultural turn*. Ithaca: Cornell University Press.

Swidler, Ann. 1986. Culture in action: Symbols and strategies. *American Sociological Review* 51: 273–86.

Swidler, Ann. 2001. *Talk of love: How culture matters*. Chicago: University of Chicago Press.

Tavory, Iddo, and Ann Swidler. 2009. Condom semiotics: Meaning and condom use in rural Malawi. *American Sociological Review* 74: 171–89.

Vaisey, Stephen. 2009. Motivation and justification: A dual-process model of culture in action. *American Journal of Sociology* 114(6): 1675–1715.

Weber, Max. 1968. *The methodology of the social sciences; Max Weber on the methodology of the social sciences* (trans: Shils, E.). New York: Free Press.

Willis, Paul E. 1977/1981. *Learning to labor: How working class kids get working class jobs*. New York: Columbia University Press, 1981.

Mabel Berezin is Associate Professor and Chair of Sociology at Cornell University. She is a comparative historical sociologist whose work explores the intersection of political and cultural institutions with an emphasis on modern and contemporary Europe. She is the author of *Illiberal Politics in Neoliberal Times: Culture, Security, and Populism in the New Europe* (Cambridge University Press, 2009) and *Making the Fascist Self: The Political Culture of Inter-war Italy* (Cornell 1997). She is the author of numerous articles, review essays and contributions to edited volumes. She has edited three collaborative volumes, including a special issue of *Theory and Society* (2009), entitled "Emotion and Rationality in Economic Life."