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**SYMPOSIUM: VARIETIES OF EVIDENCE AND METHOD IN CULTURAL SOCIOLOGY**

# Methodologies, the Lifeworld, and Institutions in Cultural Sociology *

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Methodologies, the Lifeworld, and Institutions in Cultural Sociology

The development of an ever more mature and diverse cultural sociology over the past decade appropriately inspires critical reflection about its prospects as sociological inquiry. This is especially so for two reasons.

First, methodologically, research in cultural sociology now employs *all* the major methodologies in use by sociologists, from field research to historical and comparative methods to quantitative analysis, plus a variety of emergent methodologies such as discourse and narrative analysis. Indeed, interests in cultural analysis have built on and inspired uses and development of innovative methodologies – for example, Michel Foucault's genealogical strategy, Pierre Bourdieu's (controversial) use of multiple correspondence analysis, John Mohr's analyses using Galois lattice structures, and Qualitative Comparative Analysis – as well as controversies and developments of practices using ethnographic, survey, and cultural historical methodologies. Cultural phenomena are notoriously challenging to conceptualize, measure, or generalize about. The problem of meaning represents an especially difficult problem for sociological methodology in general, and it is not one that scholars outside cultural sociology have been especially good at confronting. The maturation of cultural sociology thus offers the opportunity to consider whether and how a specific focus on culture in relation to the social alters the whole gamut of methodological practices and their statuses, and such consideration can improve the sophistication of sociological inquiry more widely.

Second, theoretically, cultural sociology has come to occupy the space of a *general* sociology – something that has largely been missing since the collapse of the Parsonian project. Scholars in virtually every subdiscipline in sociology have taken on culture as a central concern in recent years, if they had not already done so previously. Culturally inflected explanations and interpretations now enter into most every topic of research, from globalization, demography, and stratification through institutional areas like economic sociology and health care, and in lifeworldly zones of research into crime, deviance, family, and lifestyles. The entire range of sociological research agendas from longstanding to emergent ones is inflected with interests in culture (Hall, Grindstaff, and Lo 2010). Because culture has become not only methodologically but also analytically central, reflecting on research methodologies in cultural analysis promises to have broad import for both specific sociological research agendas and for the discipline as a whole.

Five of sociology's most accomplished contemporary cultural analysts have contributed to this symposium on evidence and method in cultural sociology. They do not always agree with one another (for example, Richard Biernacki discounts research using mixed methods whereas Lyn Spillman articulates certain advantages). However, I find much to agree with in each essay. Nevertheless, the overall differences in approach as well as what they share despite their differences are worth noting at the outset. Most important, there is a radical difference between the projects of Biernacki and Glaeser on the one hand, and Lamont and Swidler, and Lyn Spillman, on the other. The former two offer something like first philosophies on how cultural analysis should proceed methodologically and on what ontological basis, both of them supporting a broadly Weberian approach to cultural analysis, albeit in different ways (Biernacki is centrally interested in the interpretation of meaning, whereas the focus of Glaeser is on the connections between meanings and institutions). On the other hand, Lamont and Swidler are more interested in multiple methods and their interrelationships, and Spillman makes a concerted argument for methodological pluralism that incorporates quantitative analysis. What all four of the essays share is an epistemological commitment to hermeneutic consideration of meanings in relation to action, interaction, and (institutionalized) social organization as the bedrock of cultural inquiry, and thus, as a central issue for sociology more generally. To explore these themes, I will consider the essays in turn, moving from the nuts and bolts of qualitative research methodologies to the status of quantification in the hermeneutic enterprise, then to the epistemology of studying meaning, and finally to the ontology of the social that we study.

**Michèle Lamont and Ann Swidler on methodologies in the lifeworld**

In today's diverse world, who among us sociohistorical researchers advocates tribalism? Who opposes diversity? Who thinks "methodological pluralism" is an unreasonable approach to inquiry? Such people, if they exist (and they do!), Lamont and Swidler want to reassess their positions. Surveying the landscape of recent methodological debates, "Methodological Pluralism and the Possibilities and Limits of Interviewing" makes a strong case for ecumenicism: specific methods *per se* – in their exemplary extended discussion, interviewing and ethnography – are not to be privileged. Rather, researchers should decide how to acquire data on a practical basis, according to the character of an investigation, the empirical issues raised, how data can be obtained, and what bearing that data will have on the questions that the research is intended to address.

The case is even stronger for historical research that is solely dependent on archives and other material traces. Many people laughed at former U.S. Secretary of Defense Donald Rumsfeld when he noted that nation-states go to war with the army they have, not the one they would like. But historical researchers are in much the same position with data. Even when it is possible to interview people who participated in historical events of interest, those people carry their particular viewpoints and not some abstracted "view from nowhere," and some people with distinctive viewpoints either have not survived, cannot be found, or refuse to participate in research.[1] Real-world conditions make Lamont and Swidler's counsel laudable.

Yet the irony of ecumenicism is that it can exhibit preferences. And this is certainly the case with Lamont and Swidler's call back from the barricades. A general feature of ecumenicism in the social world beyond sociology is that it often is less than fully pluralistic. Ecumenicism in religion is almost inherently antisectarian! Lamont and Swidler are not so different. They endorse pluralism and assert that "debating techniques *per se* leads us down an unproductive path." However, it is not clear to me why this should be so. If a methodology can be improved, and needs to be specified as to its range of uses, what could be the harm of saying so? And what would be the benefit of ignoring methodological problems *per se* if they are significant?

Indeed, Lamont and Swidler acknowledge that "different methods shine under different lights, and generally have different limitations." Their real concern is that methodological controversies too often devolve into debates about whether one method is better than another *in general*. Yet such debates, they note, are important in a different way. The recent controversies are not simply about methods narrowly construed: they sometimes have broader epistemological and theoretical implications, for example, concerning meaning, its interactional and collective aspects, the relation of the here-and-now to institutions, and the goals of inquiry – "giving voice," understanding, or explanation. Such issues, pivotal as they are, might be construed as concerned with methodology in the broad sense, rather than with method as narrow technique. Yet raising these issues means that ecumenical pluralism requires qualification based on the theoretical debates from which they cannot be disentangled. In the garden of pluralism, a thousand flowers may be planted (amidst some weeds, let it be said), but not all of them will grow, and fewer still will be worth picking.

The pluralism of Lamont and Swidler, then, does not suggest that "anything goes" or that one method is as good as another. They are hardly postmodern relativists. Instead, they are willing to favor one side versus another in a methodological controversy, notably in countering Biernacki's ontological critique of methodologies that code meanings (discussed below) with an appreciation of the diverse approaches presently in play. Moreover, a central agenda of "Methodological Pluralism and the Possibilities and Limits of Interviewing" is not pluralistic: it is the defense of interviewing over and against claims for the superiority of ethnography, and more broadly, a call for tolerance of a range of methods that they rightly deem important for inquiry. In their detailed discussions of interviewing and ethnography, they offer something like a structural phenomenology of these methodologies. What, in principle, can and cannot be known by conducting in-depth interviews? What are the advantages and limitations of ethnographies? And how might these methodologies be linked to, combined with, or supplanted by other methodologies such as archival research, survey research, and quantitative analysis of large-scale data sets? Their answers to these kinds of questions show that there is no royal road to sociological knowledge, however construed.

Instead, inquiry has a number of methodological tools. In particular, although Lamont and Swidler willingly acknowledge that interviewing has its limitations, their extended comparison with ethnographic participation in the here-and-now claims it to have advantages in revealing personal and institutional contexts connected to what happens that might not be readily observed in the unfoldingness of the vivid present. No doubt ethnomethodologists and people who follow Bruno Latour and his colleagues' style of research in *Laboratory Life* (Latour, Woolgar, and Salk 1986) would retort that deep ethnographic immersion can reveal exactly how at least some contextual circumstances come into play in the vivid present, or at least, how individuals act *as though* they are in play. Moreover, a variety of field researchers these days do not religiously bracket the world beyond the immediacy of the here-and-now, instead pursuing people, issues, and connections beyond any narrowly delimited "site." Finally, as Lamont and Swidler observe, it would be nonsensical to draw too sharp a line between interviewing and ethnography. At least when an interview is conducted in person and in the interviewee's lifeworld, it provides some opportunity for ethnographic observation. And in my research experience, asking interview-ish questions in the course of unfolding social life can be tremendously productive as a research strategy. Moreover, ethnography is not to be essentialized, given that researchers play a variety of roles in such research – from full-bore participant to putatively objective observer. Indeed, the whole range of qualitative methods face shared problems, a situation that has received less than full acknowledgment because of their uneven and sometimes separate development (Hall 2014, forthcoming).

As a researcher who has used quantitative analysis, interviewing, participant-observation, narratological, coding, historical, and comparative methods, I certainly endorse Lamont and Swidler's affirmation that alternative methods can yield different kinds of sociologically relevant data. Practitioners of various methods thus would do well to stop their special pleading on any *general* basis; rather, we need to continue to crosscheck data – whatever their source*s* – against findings and interpretations. In this enterprise, the calls by Lamont and Swidler for increased attention to historicity, temporality, and (extended) institutional contexts are all important, as is their invocation of the methodological salience of comparison.

The affirmation of methodological pluralism in turn raises the question of how radically alternative study designs are to be integrated with one another, either within a given research project or across research projects. As I have noted elsewhere (1999: chapter 9), different methodologies and research questions are not equal in their potential capacities for intellectual interchange; instead, sociohistorical inquiry is characterized by a loosely "integrated disparity" of methodologies. In the final analysis, as Lamont and Swidler emphasize, many debates that *seem* methodological are in fact theoretical. Yet that conclusion suggests limits to an ecumenical pluralism, for methodological debates can have significant theoretical stakes. Like the methodologies they consider, these debates, I submit, are neither productive nor unproductive in general: rather, we have to get down to the nitty-gritty of theoretical (or methodological) engagement on particular issues. The status of quantification is a case in point.

**Lyn Spillman on mixed methods and quantitative research**

Like Michèle Lamont and Ann Swidler, Lyn Spillman acknowledges the salience of multiple approaches to cultural sociological analysis. Her central – and provocative – argument is that we should reverse the widely conventional characterization of qualitative investigation in mixed-methods research. There, the function of qualitative analysis is often portrayed as providing preliminary and anecdotal findings that can be used to generate hypotheses, offer nuanced consideration of context, and check and improve causal inference; such efforts are positioned as ultimately subject to scientific confirmation through quantitative inquiry. The relationship, Spillman holds, is the opposite. Quantitative work is best suited to *description* of an overall research landscape. But careful qualitative research is needed in order to achieve that *sine qua non* of science – *explanation* by identification of causal mechanisms and their effects.

Spillman offers a strong defense of the thesis that qualitative analysis of a single case itself can be explanatory – a point she raises in relation to the arguments of George Steinmetz, and that few historians or historical sociologists would doubt (indeed, in different ways, Arthur Stinchcombe, Charles Tilly, and a number of historians have given detailed consideration to the epistemological issues involved in a variety of approaches to single-case analysis; see Hall 1999). And like Isaac Reed, whom she also discusses, Spillman gives pride of a place to a broadly hermeneutic approach that embraces not just interpretation but a stronger claim – explanation.

However, Spillman parts company with Steinmetz, who argues that only in events themselves can mechanisms operate and, thus, be open to study. Against this implicit devaluation of quantitative inquiry, Spillman argues that quantification can play an important role – at any “level” of analysis – in “big picture” description of the ranges of empirical variation. Quantitative work, then, is "thin description" to be followed by more intensive *qualitative* inquiry.

I agree with Spillman that quantitative research often raises more questions than it answers, and that puzzling over quantitative findings can be an important springboard to hermeneutic inquiry. And in an era when even post- (and unreconstructed) positivists focus on the issue of causal mechanisms, qualitative research can be developed in ways that pursue opportunities for explanation previously foresworn. But what about quantitative research? It may well be that certain acts of quantitative measurement are at odds with particular ontologies that qualitative researchers affirm. However, quantification can be essentialized no more easily than the tremendous variety of qualitative methods. Qualitative researchers make (often implicit) binary distinctions, and these amount to dichotomous measurements. For their part, quantitative researchers are engaged in narration and interpretation at various junctures (Hall 1999). Moreover, as Spillman affirms in a footnote, there is a wide variety of quantitative practice, plus hybrid practices such as those advanced by Charles Ragin and Andrew Abbott.

Given these circumstances, what is needed is careful evaluation of *particular* quantitative and hybrid practices, much along the lines that Lamont and Swidler offer for interviewing and ethnography. Yet quantitative methodologists seem more concerned with refinement of particular methods than with considering fundamental issues of analytic logic. Thus, a great irony: whereas a half-century ago, quantitative methodologists were at the forefront of considerations about the epistemology of social-scientific inquiry, in relation to contemporary methodological issues, quantitative discussions have not well engaged with general methodological debates, and there is considerable and growing need for a thorough-going epistemological recasting of alternative approaches and methods of quantitative analysis – an enterprise that most quantitative social scientists are either ill-equipped or loath to carry out.

Imagining the results of such recasting, I am of the view that quantitative inquiry has a good deal more potential than is sometimes acknowledged among qualitative researchers, partly because quantitative researchers have largely fallen into a sad methodological formalism, rather than seeking to link quantitative analysis to logics of testing, inference, and analytic reasoning, which was the promise held out by people like Hans Zetterberg, Herbert Costner, and Arthur Stinchcombe.

Lyn Spillman has opened one door to that recasting by formulating an important but previously largely implicit role of quantitative analysis in qualitatively oriented hermeneutic analysis – that of "thin description" – surveying the empirical, and potentially theoretical, variation in populations of cases – presumably defined in hermeneutically relevant ways. As Spillman notes, qualitatively-oriented cultural analysts such as Robin Wagner-Pacifici are now seeking common cause in the analysis of meaning with other cultural analysts like John Mohr, who have used quantitative analysis of cultural structures to good effect. The question is whether a group of cases actually can be defined in hermeneutically relevant ways, and thus, whether there is a tenable role for quantification in hermeneutic analysis. And that is one of the central questions that Richard Biernacki addresses.

**Richard Biernacki on Coding**

The essay by Richard Biernacki amounts to a detailed and devastating critique of particular coding practices in cultural sociology, and by extension, in the social sciences and humanities more generally. Taken seriously, it surely will provoke a good deal of discomfort, uneasiness, and defensiveness. However, I would urge scholars who use coding practices (and, one way or the other, that is most of us) to take Biernacki's critique both seriously and constructively. His analysis re-marks a longstanding divide in cultural sociology between interpretive approaches and those building on the quantitative positivistic tradition. At stake is the legitimacy of cultural sociology – in its own terms and in relation to the discipline and to coding practices in other disciplines. Some of the most successful cultural sociologists have used formal coding techniques, on the prospectus that cultural sociology could be just like the "harder" subdisciplines of sociology. But what if coding of cultural meanings cannot be sustained either epistemologically or ontologically? The devil is in the details of meaning, Biernacki argues, and those details are elided by coding practices.

The issues that Biernacki raises warrant serious and wide discussion. We do not know where this discussion will lead, but we should all take the journey, for we need to get this right, going forward. We don't want to be like the drunk searching for keys to the car in the light, rather than where they were lost. Biernacki's discussion of important exemplars of cultural analysis proceeds, reflexively in relation to his own critical position, by the use of exemplars, in order to suggest methodological issues at stake in the broader practices of coding in cultural sociology, and, I would submit, in sociology and other social sciences more generally. Central to his argument is the characterization of meaning – whether in discourse or a text – as both situational and contextual. We cannot get around the specificity of meaning through coding, he asserts, because coding is a second-order practice that effaces nuances intrinsic to the original material.[2]

At this juncture, however, Biernacki's analysis remains cautionary rather than decisive. By his own analysis, cases of coding practices are manifold. There is no easily specifiable universe of them. His argument proceeds by reference to exemplars. By his discussion of them, Biernacki wants to identify difficulties of coding *in general*. However, on Biernacki's own argument, it is impossible to argue from exemplars to the general. Therefore, it now becomes important to pursue something like a hermeneutics of coding situations. As Biernacki describes it, "The purpose of coding is merely to present general trends in summation." However, some scholars, for example, John Mohr, have used cluster analysis that describes not trends, but alternative and interrelated constellations of meaning. Perhaps such practices suffer from the same problems that Biernacki already has described. But because Biernacki does not directly discuss Mohr's and various other practices, their epistemological status in relation to Biernacki's critique remains an open question. Because Biernacki's argument cannot (yet?) be taken as a general one, scholars who wish to use coding of cultural meanings as a methodology have free reign to consider whether and how specific coding procedures might resolve the serious methodological issues that he has raised.

During the conference session at which I initially discussed Biernacki's analysis, I commented that the problem of coding is not finessed in the alternative approach that Biernacki favors – ideal-type analysis. In his essay for this journal, Biernacki disputes that. His description of ideal-types – as what I would call benchmarks rather than categories – well describes both what I regard as the correct understanding of their character as concepts and accords with my own practice, involving holding up ideal types and empirical phenomena in relation to one another to tease out substantive meanings in relation to ideal types, themselves clarified by such activities.[3] And Biernacki is certainly right that the transparent analysis of specific meaning complexes is a more appropriate scientific practice then obscuring procedures of coding. But I continue to think that ideal-type analysis requires coding decisions, at the most general level, in that any assertion that one meaning is similar to or even equivalent to another represents a coding decision. Whether such characterizations are transparent or not is a separate issue. Biernacki, I think, "miscodes" (!) my comment about the use of ideal types. I agree with his characterization of Weber's method. Indeed, I find his discussion to be one of the most sensitive to Weber's methodology and practice that I have read. The real issue here, I suppose, is whether we can describe the cultural meaning of "coding" as an ideal type and whether ideal-type analysis itself approximates such a type, and if so in what ways.

A different, more substantive, matter emerges toward the conclusion of Biernacki's essay, where he suggests that coding of cultural meanings "standardizes the apperception of cultural expression into supposedly commonsensical topic categories" in a way that "aligns with American corporatized language, in which the basic units are words and phrases." This characterization raises the intriguing possibility that some meanings come, as it were, "pre-coded." That is, processes of rationalization of the social may produce conditions in some quarters where Biernacki's concerns about meaning holism, context, and specificity wither away. These days, the proliferation of such conditions, already energized by corporate and mass production of culture, has only intensified with the rise of the internet and forced-choice radio buttons. In short, there may be more room for second-order coding of meanings when first-order meanings themselves are both public and pre-coded.

Behind the pre-coded character of some cultural meanings can be found another issue, the classic one, from Alfred Schutz's (1967) critique of Weber, concerning intended meaning versus observed meaning. Biernacki's analysis, like Schutz's critique, seems to assume that the ultimate goal of inquiry concerning cultural meanings is to get at "real" or "intended" meanings in texts and discourses. However, we are all participants in the hermeneutic circle. Sometimes, new meanings about texts and discourses get made by those who apprehend them. Here, scholars are in the same boat as everyone else. Although one project may be to get at the intended meanings in texts and discourses, it seems to me that observers also have license to make new meanings about cultural products that we observe. Necessarily, doing so is a very different project than getting at intended meanings, and analysts need to be clearer and explicit about what they are doing. However, making new meanings about previous meanings is both a legitimate and an inevitable social process, for scholars as for everyone else.

Dialectically, as my discussion so far suggests, the practice of coding is not just to be found among sociologists, but in the wider world as well. Thus, Biernacki's close critical analysis of sociological coding should inspire a broader sociological research into coding as social practice, and its relation to a coded construction of knowledge and understanding.

We may all differ about this point or that in Biernacki's analysis. But let me be blunt. The tendency of researchers is to ignore methodological critiques such as

Biernacki's, because seriously confronting critique would require the kind of fundamental reorientation of already conventionalized practices that would disrupt sociological work. But ignoring the issues would amount to sociological bad faith. If we cannot have a serious and consequential discussion that moves toward collective resolution of debates, then let us drop the pretense embodied in the word "science."

**Andreas Glaeser on institutions**

I can only read Glaeser's essay "Hermeneutic institutionalism" in light of Richard Biernacki's call for the rigorous employment of an ideal-type methodology of interpretation. Glaeser's essay builds on his 2010 book *Political Epistemics: The Secret Police, the Opposition, and the End of East German Socialism*, by consolidating and extending the ontological implications of that study. The essay engages on so many fronts and with such a coherent direction that any attempt to summarize its most significant points bears the risk of simply redescribing the essay in less compelling language. Nevertheless, an observer's appreciation of Glaeser's project may serve to underscore its importance.

Glaeser agrees with Biernacki's precept of what Glaeser calls "meaning holism," and he both (1) employs in an exemplary way the methodology specified by Biernacki, by seeking to disentangle the originary general precepts of a social hermeneutics (largely extracted from the work of Vico and Herder) from historical localizations and wrong turns, and (2) builds out what he calls a "process ontology." This ontology, in my view, offers a general (though not ahistorical) description of the conditions of social life and social epistemology that underscores the importance of Biernacki's methodology, and it makes great headway in consolidating a hermeneutic approach as a "big tent" that offers a novel and welcome alternative to extant social theory. Glaeser, in short, offers a major theorization of how social inquiry as an enterprise can become reconstructed. Whatever our own epistemological, ontological, and methodological commitments, we can advance our collective enterprise by charting our positions in relation to Glaeser's account of hermeneutic institutionalism.

Glaeser does not strongly emphasize the point, but hermeneutic institutionalism offers a radical alternative to the semiotic structuralist approach that has predominated in cultural sociology in recent years. Taking a different trajectory than conventional approaches to hermeneutics and epistemology, Glaeser largely forgoes consideration of the classic interpretive problem – knowing the mind or intentions of another person. Instead, located within what I have broadly call the "social interaction approach" (for which he rightly points to an expansive conception of "interlinked ... action/effect flows"), he offers something like a sociological philosophy of interpretation that places it at the center of institutions, hence, "hermeneutic institutionalism." In doing so, he insists that institutional analysis be capable of understanding grounded social action. For his part, Glaeser artfully connects institutions not only with cognition but also with emotions (where, for example, Freud's repression figures) and embodiment.

For me, the central enterprise with which Glaeser connects is that of Max Weber. Over the years, sociologists of a neo-weberian bent occasionally have lamented that Weber "didn't have a general theory." This concern was more one of academic positioning than an assessment that something major was lacking in Weber: he somehow needed a "handle" that would allow a school of thought to consolidate more effectively around his approach. Glaeser's essay both makes a considerable advance in resolving that absence and shows that it really was an absence, and not just a matter of positioning.

Weber, of course, was prolific in producing substantive analyses – on topics as diverse as religion, agriculture, and capitalist infrastructure. And he offered detailed epistemological statements that delineate a clear approach of *Verstehen*, based on methodological individualism yet building from there to analyze complex social formations. Yet most of Weber's ideal types are static descriptions of meaning complexes (inner-worldly asceticism, for example) and social fields organized around meaningful principles (legal-rational bureaucracy, patrimonialism, and so on). Weber often specified *theoretical* transitions, for example, between feudalism and patrimonialism, and he often described empirical processual developments. But only occasionally, for example, in discussing routinizing directions of charisma, did he theorize process. One of Glaeser's accomplishments is to provide such an account, based in hermeneutics, capable of engaging diverse considerations, for example, temporality, spatiality, materiality, embodiment, the senses, and the unconscious.

Glaeser's account, I submit, should become foundational not only for sociologists of a weberian bent, but also for symbolic interactionists and others more broadly (these days often lacking a banner under which to gather) who are concerned with connecting meaning, culture, action, and institutions in historical and comparative analysis. Glaeser has conducted a sort of "salvage ontology" that retrieves hermeneutics from its originary overly holistic tilt, reconstructs it in relation to a variety of approaches that tended to sidetrack its possibilities (including any solipsistic or purely episodic interactionism), and positions it as a viable (and in my view, superior) alternative to both critical realism and

Durkheimian cultural and social structuralist approaches insofar as they lack bases in social interaction (cf. Reed 2011). He also discounts practice theory as a starting point because, he argues, it comes already entailed with assumptions about the structure and character of institutions.[4] Especially important, none of the other approaches that Glaeser considers seems to have provided an alternative to neo-liberalism as ideology and to its social theoretical counterpart, rational-choice theory. By contrast, he argues, hermeneutic institutionalism does just that, by linking action and institutions in social processes in ways that offer a basis for political engagement in social reconstruction.

Hermeneutic structuralism construes the social (not reified "society"), Glaeser submits, "as a dense thicket of multiply intersecting action-reaction effect sequences," themselves transpiring in the context of previous social events that stabilize and lock in social action. I am not confident that Glaeser's neologism for this process, "institutiosis," will take hold, but his theorization certainly ought to. He neatly shifts beyond the tired dichotomies (culture versus structure, agency versus structure, micro versus macro) that others have worked to transcend in recent years. The solution is both simple and elegant: rather than deal with static abstractions, Glaeser construes the social as unfolding, processual – that is, social life that is lived. Institutions then, are not isolated from the everyday lifeworld, they are enacted (or not) within it.

Glaeser is of course not the first scholar to make this move. Weber's sociology itself had a strong lifeworldly cast (for example, in his discussion of legal-rational bureaucracy). Berger and Luckmann (1966) provided a phenomenologically based account of the social construction of reality. And Bourdieu (1977) critiqued symbolic structuralism and its institutional implications by showing how the strategic agency of social actors undermines and transforms structures otherwise thought to be enduring. There is also an affinity with the Geertzian metaphor – that we are spiders living on the web of significance that our predecessors and we ourselves have spun.

Nor is Glaeser the first to (as he puts it) "scale up" the hermeneutic approach. Indeed, implicit (and sometimes more explicit) in much recent work in historical sociology is the sort of process ontology that Glaeser maps (see the essays in Adams, Clemens, and Orloff 2007), and Richard Biernacki's (1995) study of the measurement of labor in nineteenth-century factories is an obvious and compelling exemplar of the possibilities. In short, scholars are already delivering on the promise of a hermeneutics that inquires into the constituted institutionality of everyday life.

Glaeser concludes his essay by noting that his approach offers the opportunity to better understand how some actors versus others are "implicated in the formation of institutions," and thus offer a basis for better seeing how we can (possibly) change the world. Yet he recognizes that his very arguments suggests the need for "further theoretical localizations which are much more specific to particular institutional domains." As Glaeser recognizes, ontology offers a beginning, not a conclusion. The immediate task before us is to develop such theoretical localizations. This agenda, I submit, has affinities with what I have elsewhere (2009) termed a structural "phenomenology of history." It can fruitfully be pursued in relation to Glaeser's agenda through a structural phenomenology that specifies in greater detail the social temporalities of action and their institutional congealment. Detailing such an endeavor goes beyond the scope of the present discussion. However, in its general outlines, the challenge for such an endeavor is to employ theoretical localizations developed out of the hermeneutic institutionalism that Glaeser has presented to realize its promise of more effectively characterizing historical and contemporary social formations, thereby providing opportunities to clarify political alternatives in complex societies.

**Conclusion**

The essays in this symposium should advance concerted discussions within cultural sociology that, because of the broad importance of the topics that contributors consider, have a variety of important implications for the discipline as a whole. Whereas I strongly endorse the basically Weberian claims about meaning made by Biernacki and the allied call for hermeneutic institutionalism advanced by Glaeser, the promise and potential of their approaches will best be fulfilled by engaging – carefully! – with the methodological pluralism advanced by Lamont and Swidler and the uses of quantification proposed by Spillman. As I proposed in *Cultures of Inquiry*, we can respect the diverse research methodologies of sociology and enhance their analytic rigor by engaging in a close critique of their various possibilities, and by being willing to acknowledge limitations, either by resolving them, clarifying the scope conditions of a given method, or potentially, acknowledging an approach as untenable, and abandoning it. Methodology, in any specific inquiry, is, or should be a craft, highly attuned to the kinds of questions being asked, the sources of data available, and the character of the phenomena being studied. But craft practice depends on an honest and forthright consideration of methodology. Unfortunately, sociology has become all too complacent in its various wings and their methodological practices. Hopefully, we can all follow the lead of the authors in this symposium, down a new path seeking intellectual rigor.

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**Notes**

[1] All problems that colleagues and I have encountered when conducting research about stigmatized events (Hall, Schuyler, and Trinh 2000).

[2] Here, Biernacki's position converges with the critique of Max Weber by Alfred Schutz (1967), a point to which I return below.

[3] The complexities of the methodology of ideal-type analysis prevent an extended discussion here. As Guenther Roth (1976) has argued, Weber used typifications at various levels of substantive detail, from the highly abstract to characterizations of historical complexes of meaning. At a formal level, ideal types as case-pattern concepts have associated with them a series of analytically definable attributes, just as empirical cases have associated with them attributes measure in quantitative analysis by variables (Hall 1999: 107-11). It is this quality that allowed Weber to identify points of transition between ideal types closely associated with one another, for example, feudalism and patrimonialism. In my view, we "code" both ideal types and cases in terms of their attributes. Acknowledging this process does not deny Biernacki's point, that the ideal type and meanings in the empirical world cohere as totalities that are more than the sum of their attributes. Such coherences, I would argue, can only be understood by identifying various attributes that compose them.

[4] In this regard, Bourdieu's theory of fields, I would argue, should be regarded as a substantive theory about a set of processes that occur in particular kinds of domains, rather than a general social theory or ontological framework.