Culture as Object and Approach in Sociology

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Culture is increasingly important to American sociology, but in what way? At least twenty years on from the (latest) "cultural turn," can we specify what it means that so many sociologists "study culture." A lot of work has been done to define cultural history and a lot of postmodern ink spilled over the epistemic status of cultural anthropology; this book is intended to reflexively comprehend what is going on in cultural sociology. It is about, in particular, how the consideration of culture—and its accompanying term, meaning—reforms and reframes the nature of sociological inquiry as a whole. It is thus concerned with method in both the broad, conceptual sense of the concepts that define and frame inquiry, and in the technical sense of what sociologists who study culture do in their everyday research practices. In this introductory chapter, I want to set out a schema for thinking about culture in sociology that puts into perspective the purpose and scope of the chapters that follow.

To begin with, consider a very intuitive distinction concerning how culture can be important to sociology: as object and as approach. Culture as an object would approximate what people mean when they refer to high and low "culture," and thus would include the sociology of art, literature, and television—and perhaps also religion, science, and other forms of knowledge. The study of what has been thought and said, and of the produced
cultural artifacts of this or that society, would take culture as its object and apply well-known sociological methods and theories to this object.

Culture as approach would refer to how standard sociological objects of inquiry (e.g., race, class, gender) are researched "from a cultural perspective"—that is, examined for their discursive or symbolic elements or aspects. Here all of sociology is fair game for the sociologist who "does culture," as the symbolic dimension of social life becomes an essential part of the description of social facts and the explanation of social action.

To specify this distinction in a more rigorous manner, I would like to make a distinction between the context of investigation and the context of explanation. The context of investigation refers to the social and cultural environments of operation of the sociological investigator herself. The context of explanation refers to the social and cultural environments of the actors who are studied by the investigator—the subjects and social phenomena that demand explanation. It is with full intention that I use the vague term "context" here, because the very specification of the nature of each of these contexts—through theory, research, or the combination of the two—is exactly what I am interested in exploring as a way to think about the role of culture in sociology.

According to this schema, what we mean when we talk about culture as an object for sociology is that we are expanding the context of explanation to include many more social facts to be described and more social actions to be explained (Which movies get made, how and why? Which movies get watched, how and why?). This is a strictly concrete and empirical expansion. The tools that we use to investigate these new objects may of necessity go through some changes, particularly at the most technical level, but in a larger sense the conceptual resources of the sociologist herself are merely reapplied and refocused. In other words, the context of investigation remains largely unchanged and unchallenged.

When we discuss culture as an approach, something slightly more complex is implied. Here, too, we can start with the context of explanation. The sociologist who practices a cultural approach to her object of study would argue that, in merely adding the production and consumption of cultural artifacts to the context of explanation, the sociologist commits the fallacy of misplaced concreteness. Certainly, it is useful and important to study culture in this sense, but to work theoretically as if one can point out this phenomenon as "cultural" and this other one as "political" or "economic"—or even "social"—is to labor under an illusion. It is rather the case that most social actions that demand explanation have a "cultural aspect." What the cultural turn implies, in this case, is that the context of explanation is seen to have a dimension that was neglected before.

Now, of course, the whole project of Parsonian grand theory was to recognize and synthesize the many "dimensions" of the context of explanation, understood as a social system—including the cultural dimension. But the consequences of taking the cultural dimension seriously have not turned
out to be what Parsons thought they would be. Parsons thought that the recognition of the role of culture in explaining action meant that a single, scientific model of culture—norms and values—could be integrated into the voluntaristic theory of action, and thus into a single analytic model for explaining social actions, social reproduction, and social change. However, in the course of the latest cultural turn in sociology, we have come to see that (1) "culture" consists of much more than norms and values, and also requires several different kinds of theoretical tools to comprehend it, and (2) the consequences of this "approach" version of culture run much deeper than adding "one more factor" to each explanation of social action. Among these consequences is the thorough reformation of the context of investigation.

Another way of putting this is that the recognition that "culture" exists as a dimension of the context of explanation changes the basic relationship between the context of explanation and the context of investigation, and therefore how we consider the context of investigation itself. In Weberian terms, the cultural approach to sociology recognizes that to explain social action, one has to interpret culture. And to interpret culture, one has to retool the context of investigation around the interpretation of meaning, so that the "culture" that exists in the context of explanation can be adequately grasped and harnessed for the task of sociological explanation. In particular, one has to involve oneself in the messy work of studying subjectivity and its manifestation in, and molding by, discourse. The theories of semiotics, linguistic structuralism, and hermeneutics must be marshaled into service to answer sociological questions, and to comprehend collective representations and their enactment. Sociologists must start looking for narratives, genres, conceptual categorizations, and metaphors in the context of explanation; they must also start examining their social consequences.

This book takes up the cultural approach to sociology in exactly this sense. It is about meaning—specifically, the role of meaning in social life (i.e., in the context of explanation). And it is about method—the reformation of the context of investigation so as to better comprehend social meaning. The overall purpose, then, is to contribute to a conceptual restructur ing of sociology toward a cultural approach, evident in the chapters in the first part of this book.

**PART I: CULTURAL APPROACHES TO SOCIETY**

Part I of the text begins with two examples of the necessity of interpreting culture to explain social action, even if the actions under study are not culture in the sense of culture-as-object.

In "A Special Camaraderie with Colleagues: Business Associations and Cultural Production for Economic Action," Lyn Spillman brings a cultural perspective to bear on a question that has long been of interest to
neoinstitutionalists in sociology, political economists, and the new institutional economists: Why do business associations exist, and what is their role? For political economists, such associations (like Spillman’s favorite example, the International Concrete Repair Institute) are political interest groups, lobbying in Washington and other national capitals for policies that are to their advantage. For neoinstitutionalist sociologists, these associations serve as the site for mimetic institutional isomorphism—they are the means by which businesses learn to copy and calibrate their ideologies and practices. And for the new institutional economics, these associations are just another example of a social formation that serves to reduce transaction costs. Spillman finds that all of these perspectives are partially correct and that some of these associations do serve such purposes, some of the time. Yet she also finds much more.

Using the lens of cultural sociology, Spillman fleshes out several hunches of neoinstitutionalism, goes beyond them, and ends up with a remarkable new account of the basis for economic action. She finds that such associations construct and construe social ties, enforce standards, define and shift the boundaries of industries, and set the cognitive maps for how economic actors think of themselves and the environments they operate in. One can see in Spillman’s work the leaps and bounds that the study of culture has made in recent years. Hers is not a heavy-handed discourse arguing that values, norms, and solidarities, instead of power and interests, determine actions and social outcomes. Rather, she examines how culture works through cognitive mapping, identity formation, boundary maintenance, and so on. She marshals this conception of culture, furthermore, not to show that economic interests do not matter but, rather, to show how culture shapes these interests as well as the view that economic actors have of the environment they are acting and strategizing in.

If Spillman’s chapter builds out from neoinstitutionalism toward even more cultural insights into the nature of economic action, the chapter by Jerry Goodstein, Mary Blair-Loy, and Amy S. Wharton takes on a programmatic tone that is definitively “postneoinstitutional.” They set out a research program that is syncretic in its ambition to combine the insights of the “old” institutionalists (e.g., Selznick) with the new (e.g., DiMaggio and Powell) while reframing the emphasis on norms and values in terms of the newer, more subtle terms of cultural sociology. Specifically, they suggest that, if we view organizations (and in particular, firms) as actors, it is important to recognize that (1) they not only respond to their external environment but also iterate, maintain, and adapt internal ways of doing business, treating employees, and pursuing profit—what Goodstein, Blair-Loy, and Wharton refer to as “core ideologies,” and (2) external “isomorphic” pressures and the internal forces of core ideologies structure action (organizational and individual) not just cognitively but also morally.

Here, too, we see the bringing together of new theoretical tools from a reformed, postcultural-turn context of investigation with new discoveries
and insights into a specific context of explanation—in this case, the social world of organizations. Hence Goodstein, Blair-Loy, and Wharton's call for a redirection of cultural research on organizations toward "moral action" is an imperative both to reform the context of investigation, adding new interpretive tools and theoretical orientations to sociology, and to reconsider the context of explanation—organizations—in its cultural dimension.

Of course, there is no reason why the "cultural approach" cannot be brought to bear upon "culture-as-object" as well, and Kenneth Thompson's chapter points us in this direction. He takes cultural artifacts as his objects of sociological study: Janet Jackson's Super Bowl performance, Mel Gibson's film *The Passion of the Christ*, and the reactions to both in the mainstream media. Yet he also uses a cultural perspective to analyze the events, and to explain the public outrage and change in FCC regulations that followed upon Jackson's exposure of her breast, as well as the massive monetary success and tremendous popular appeal of Gibson's film. He does so, furthermore, via a risky but ultimately productive theoretical move; he brings into dialogue with each other the (primarily Durkheimian) literature on moral panics and the (primarily Foucaultian) literature on governmentality or "government at a distance."

What holds this all together is a specific tool of the context of investigation—discursive theory—that allows him to grasp, in the context of explanation, the binary discourse of American civil society. Thompson shows how these notions of civility must be performed via binaries that define and denigrate "uncivil" others. He also points to the complex way in which the American version of these civil binaries is inflected with an appreciation of visceral violence and a discomfort with sex. Ultimately, then, Thompson is able not only to explain the varying reactions to these two "scandalous" media events but also to draw a rich portrait of the cultural power of the religious right in the United States.

All of which brings us to the apotheosis of Part I, Georgina Born's chapter on the sociology of the arts. By taking on the fundamental opposition between the sociology of art and art history/art criticism, she sets the stakes high. How can a nonreductive, cultural-sociological approach to art remain sociologically relevant and avoid the fetishizations and idealisms of the kind of art criticism that remains in thrall to its object? More generally put, how can one take a cultural approach to culture without ending up in a vicious circle or a dead end? It is with the accumulated experience of two ethnographic studies of cultural production, a broad set of theoretical resources, and a keen eye for the heart of the matter that Born moves the sociology of art forward.

She does so through a critique of Bourdieu, an appropriation of certain insights from the anthropology of art, and by setting out the key theoretical changes that would be necessitated if sociologists are willing to admit to the relative autonomy of the aesthetic logic of artistic production. In particular, she emphasizes how such a move could make the sociology
of art more sensitive to historical particularity, and better able to comprehend and explain the strategies and subjectivities of both artists and critics. Another result of this shift would be an ability to consider "together the object and performance arts, mass media and popular culture," as opposed to delimiting the high arts as their own "field" of battle. Born is thus able to overcome Bourdieu's notorious ambivalence about popular aesthetics.

Finally, then, Born suggests an epistemological and methodological shift to a "postpositivist empiricism," which combines ethnography and genealogy. This will enable a sociology of art that "[cultivates] an intimate knowledge of, and a close dialogue with, the most contemporary developments and thinking in the arts, music, and media, but without capture or capitulation to the siren calls for affirmation."

Thus Born raises a set of fundamental epistemological issues that, more generally, the question of culture raises for sociology as a whole. It is to an agonistic working through of these issues that the second part of this book is dedicated.

PART II: ON ABSTRACTION AND INTERPRETATION—
THE BIERNACKI-EVANS DEBATE

In recent years, the epistemology of sociological research has become the subject of debate in American sociology, in disputes over rational choice theory's applicability in historical sociology (Kiser and Hechter 1991; Quadagno and Knapp 1992; Skocpol 1994; Somers 1998; Kiser and Hecter 1998; Boudon 1998; Goldstone 1998; Calhoun 1998; Mahoney 1999; Mahoney 2004; Sica 2004) and over the political valences of ethnographic research on the American underclass (Wacquant 2002; Anderson 2002; Duneier 2002; Newman 2002). In Part 2 of this book, we bring to a sociological audience another dispute of this nature, one that concerns the methods available for studying meaning in general, and, in particular, the applicability and utility of uniform coding schemes to large amounts of textual evidence.

Thus Part II of the book opens with Richard Biernacki's "After Quantitative Cultural Sociology: Interpretive Science as a Calling," which takes as its case study John Evans's book Playing God: Human Genetic Engineering and the Rationalization of Public Bioethical Debate (2002). Biernacki mounts a fierce critique of the abstraction necessitated by Evans's coding and cluster analysis methods, arguing that the decontextualization that is necessary for these methods to "work" is in fact exactly what makes them unable to grasp the meanings that actually explain action. Instead, coders claim to "find" a set of meanings that were in their coding schemes to begin with; abstraction guarantees a vicious, as opposed to productive, hermeneutic circle.
Biernacki's larger project is to criticize the modes of abstraction that disable interpretation in all sorts of sociological studies of culture. Yet his critique is so reliant on his in-depth study of Playing God that we as editors felt it necessary to elicit a response from John Evans, which he has provided. Biernacki's extended critique is thus followed by Evans's response, "Two Worlds in Cultural Sociology," in which Evans defends his work and proposes to clarify why he and Biernacki differ so ardently, not only about the value and verity of Playing God but also in their prescriptions for the field of cultural sociology. And then we are off and running: Biernacki responds with "The Banality of Misrepresentation," and Evans responds in turn with "Imperviousness to Disconfirming Data." As is perhaps evident from the titles of these replies, the debate becomes quite heated. The frustration of both Biernacki and Evans is palpable, as each scholar clearly feels increasingly misunderstood as the debate proceeds.

Such is the glory and the bane of the methodenstreit. Clearly we have here not only two passionate and dedicated authors but an issue that arouses great passion. And it should: At stake is the very ability of cultural sociology to discover and write truth, and to produce sociological explanations. What is also at issue is whether or not the dominant mode in which mainstream sociologists comprehend truth (through quantitative research and large-N studies) can be brought into harmony with the demands that culture makes on sociological investigation. Thus it is a question not only of how truth works "inside" the subfield of cultural sociology but also of whether or not the truths found and discoursed upon by cultural sociology are even communicable to quantitative sociologists. I think it would not be an exaggeration to say that, within American sociology departments at least, this is an issue that every sociologist who studies culture must deal with, in one way or another.

A key aspect of the Biernacki-Evans exchange is the way the dispute travels from the heights of metatheory to the details of empirical data, thanks to the fact that both Evans and Biernacki are familiar with the same set of sources. The reader should certainly note and appreciate the irony that Biernacki has performed an exercise of the scientific norm of repeatability that is exceptional in its depth and attention to evidential detail, so as to argue that the basis for scientific sociology cannot be found in the repetition of data collection and coding. Biernacki's argument is that the nature of the scientific authority that he and Evans derive from their work with empirical materials must be radically reconceived; Evans's response is that this radical reconception is unworkable and antisociological, and, indeed, that Biernacki has produced not a reinterpretation of the evidence but a willful misreading of his original work (Playing God) and of the evidence itself.

For this reason, I urge the reader to consider carefully Biernacki's most harsh claims—contained in his many boxes that insist upon the contradictions and discrepant data reports of Playing God—and compare
them to Evans's original text. On this substantive issue, no introduction can do justice, and the reader must decide for herself "who is right." What I hope to do here is to draw out from the debate the core claims of each author and delineate the essential theoretical differences between them. This should serve a clarifying function, since it will allow us to assess when Biernacki and Evans are speaking to each other, when they are speaking past each other, and what vision of cultural sociology is implied in either case. In doing so, the previously outlined distinction between the context of investigation and the context of explanation will be particularly useful.

Biernacki's core claim can be understood as an empirical one about the context of investigation as itself a field of social action. What he refers to generally as "quantitative cultural sociology" is, he argues, engaged in a set of ritual practices that claim to be scientific knowledge but in fact are a mystifying repetition of truths already agreed upon in advance. Evans's *Playing God* and the various positive reviews of it form his case in point. Biernacki means ritual practices in a quite precise sense; these actions, he maintains, constitute their own, self-contained world of "research" that, through the mechanism of coding, brings the outside world/data/evidence into a neat, orderly fit with a meaning system that is already in place. As a result, data are necessarily distorted, and any chance of actually producing empirical truth is abdicated. Like a tribe that interprets the arrival of strange people, or strange happenings, strictly as repetitive manifestations of the myths they already believe in, quantitative cultural sociologists go to the textual data and find what they are looking for.

In Evans's case, argues Biernacki, the ritual magic involves two interrelated moves: (1) Evans presents his definitions of bioethics, and his schema for interpreting discourse about human genetic engineering (HGE), to fit his story of the social transition from substantive to formal rationality. And (2) by breaking apart his textual sources, coding them, and thus interpreting them out of context, Evans makes it appear as if he has discovered the shift from substantive to formal rationality *in his data* as opposed to *foisting it upon the data* (and upon his readers). In Biernacki's view, this is not at all an infrequent or deviant activity on the part of Evans; rather, it is the sine qua non of quantitative cultural sociology. The shamans of abstraction that rule this field of research conjure up a faux context of explanation that in fact only mirrors the working myths of the context of investigation in an endless (and vicious) hermeneutic circle. Any actual engagement with the context of explanation is exorcised by well-trained coders whose incomprehension is not chaotic but, rather, extensively and precisely structured.

Now, as Evans adeptly points out in his reply, the essential issue here is not quantification in the sense of the literal use of mathematics but the act of abstraction that is necessitated by the consistent, repeated use of a coding scheme whose formal structure does not change, or does not change after the initial "inductive" moment of creating the scheme itself. The extent to which the formal methods developed by John Mohr and others, and cod-
ing programs such as Nvivo and Atlas TI—or, in this specific case, John Evans’s set of indicators for the presence of substantive or formal rationality in an argument—do or do not enable the comprehension of the context of explanation is where Biernacki and Evans really differ.

Biernacki does present an alternative practice to the rituals of abstraction he so harshly criticizing, a prescription for a better context of investigation. It is derived from his interpretation of two aspects of the Weberian tradition of sociological analysis: the idea of a calling and the ideal type. The first leads him to a staunchly individualist ethics of inquiry, in which the investigator is expected to forgo both the comforts of appraisal by her scientific colleagues and the comforts of their criticism. Criticism by others does not, from this point of view, excuse the investigator from always trying to prove herself wrong. She must, Biernacki insists, embrace the continual possibility that the context of explanation will defy her most well-wrought sociological expectations and scramble her most well-designed theories.

The conceptual basis for this ethical behavior is the development of ideal types. Here Biernacki argues that this kind of analysis can produce the historical sensitivity needed to actually grasp the dynamism of the context of explanation. As opposed to Evans’s “reifying” coding schemes, ideal types connect directly to Biernacki’s vision of the “scientific calling”:

[An “ideal type” in Weber’s research program is neither a hypothesis nor an approximate description of reality. It is not a concept intended to correspond to external institutions or texts, so it cannot be adjusted in a trade-off between empirical exactness and generalizability. This feature makes ideal types cognitively double-edged, yet ethically stringent. They ensure that an imaginative guess remains patently that. Weber withholds any general warrant for the correctness or respectability of an ideal type from beginning to end (Weber 1968a, 193). An ideal type initiates only a self-critical research process in which we may hope to judge the “adequacy of our fantasy” (ibid., 194) The inconvenience of evidentiary findings for our ideal types and the pressure these findings put on the value positions anchoring those types comprise the truth process as it is available to Weber. (p. 178 in this volume)]

I will return to the question of Biernacki’s alternative, but its initial function is clearly negative, throwing into strict relief his harsh criticism of the abstractions and illusions of coding systems whose hopes for hermeneutic adequacy are, according to Biernacki, destroyed by their inflexibility; their “logic of closed standards of relevance and of endless repetition of the same distinctions” (p. 179 in this volume).

In his response, Evans makes an interesting and important shift of emphasis. He considers Biernacki’s claims, but mostly from the perspective of what such formal coding procedures imply about the context of explanation. Evans is both nonplussed and unimpressed by the idea that
his coding schemes would put him into a bubblelike context of investigation, unable to access the context of explanation because of the rituals in which he is engaged. He is more intrigued by the possibility that different methods of interpretation imply quite different social theories of what that reality "out there" in the context of explanation consists of, and how it tends to work. Thus he argues, indirectly but consistently, that he (along with other investigators of his stripe) has constructed a quite productive context of investigation—one that enables sociologists to reach effective conclusions about the motivations of social action and the causes of social change.

This difference in emphasis is a very important point to keep in mind as the reader takes on these texts, because it helps sort through the various interpretations of Weber that dot the debate. Biernacki is concerned primarily with Weber's writings as they apply to the investigator's own social and conceptual context. Evans is primarily concerned with how the same Weberian texts that Biernacki relies on for the idea of "science as a calling" also push him toward an unnecessarily individualized and idiosyncratic vision of any given context of explanation. Evans thus defends *Playing God* by giving a broad account of the difference between his approach, which leans toward the insights of the late Durkheim, and what he sees as Biernacki's insistence on the singularity of historical context and individualized meaning. For Evans, Biernacki's chapter is an "impossibility argument"—denying the very possibility of the generation of sociological knowledge—dressed up as a critique of quantitative cultural sociology and acting out a set of faux Weberian imperatives.

Interpretations of Weber aside, what interests Evans is this: Given that the abstract coding schemes he uses do indeed go "across texts" and thus do not represent this or that text in particular, or this or that author's subjective intention, what do they reference? He contends that the generality they reference is a collective level of discourse extant in the context of explanation. Biernacki's distrust of formalism, he argues, disallows in advance the possibility that these schemes are being used to grasp something real. For Evans, Biernacki's "absolutist" position is one that so insists on getting individual texts right that it is usable as a way to study many texts. And there are important sociological questions, Evans argues, that can be answered only by methods that study a large number of texts. Comprehending and explaining the shift in discourse about Human Genetic Engineering is one of these questions.

We then arrive at this basic difference between Biernacki and Evans: Biernacki argues that formal methods of coding and abstraction disable any relationship between the investigator and social reality; instead, the investigator imposes her own categories and meanings upon her evidence, and thus creates her own illusory "social reality" and, along with it, the false confidence that she has "discovered" something. Evans argues that formal methods of coding and abstraction are the only way for the investigator to
grasp the general social reality that sits above and behind social actors in the real context of explanation; the problem with the hermeneutic perspective is that it disables the possibility of studying culture structures and pushes the investigator toward a "purist" position where remaining true to a small number of cases is the endpoint of any analysis.5

But then, to quote a well-known edited collection, What is a case? Is it an individual text, a group of texts, or the social situation that produced them? One of the more painful ironies of the Biernacki-Evans exchange is that, in a debate about the analysis and understanding of texts, each author feels that his texts have been badly analyzed and misunderstood. I do not think Biernacki would accept the notion that his version of Weberian analysis disables the comprehension of cultural structures—or economic or political ones, for that matter. He would argue that, in fact, using historically sensitive ideal types is the only way to comprehend such structures, and that such analysis should indeed search for a "robust explanatory mechanism." On the other hand, Evans would not accept the notion that, by relying on coding to process a large amount of textual evidence, he has forgone interpretive judgment—quite the contrary. Thus he spends several pages disputing the argument that the patterns imposed upon data generated by formal schemes or quantitative techniques are "guesses." It is Evans's contention, rather, that his methods combine generalization with careful interpretation, as do John Mohr's. Indeed, he writes with irony, "the methods described by Mohr have always been considered to have similar interpretive challenges as qualitative methods—that is why strict quantitative methodologists do not like them" (p. 225 in this volume).

So, ultimately what is at issue here is the relationship between coding and judgment, and how each can or cannot be a basis for understanding social reality. And here, at least, I sense a possibility for agreement and compromise between the two authors, no matter how much (and for how long) they disagree about the discourse surrounding HGE. The compromise I propose would return the debate to the context of investigation as itself a field of social action (Biernacki's main emphasis) but, for the purposes of this introduction anyway, would use the shorthand of "two worlds of cultural sociology"—namely, Durkheimian and Weberian (Evans's schema)—to describe that context.

For, whatever Biernacki's account of the context of explanation is (and I doubt it is as individualist as Evans claims), his prescription for how the investigator should proceed is strikingly individualist. Sociology becomes a lonely endeavor wherein the possibility for truth-seeking relies almost entirely on the self-imposed ethical structures of the investigator. And, whatever Evans's position on collective discourse is (and I expect he does in fact grasp a bit more of the discourse around HGE than Biernacki claims), his utter bewilderment at Biernacki's skepticism shows perhaps a little too much faith in the collectively ratified procedures of interpretation that are embodied by coding schemes, quantification, and other methods
that rely upon the repeated appearance of certain indicators to infer a
general reality, Evans is satisfied to point out how the methods Biernacki
criticized are not exactly the methods he used, and so he does not take
on the larger question of whether methods—in the sense of mechanically
followed rules and procedures—do or do not detract from true under-
standing. Thus, Biernacki’s account of the context of investigation feels a
little too “Weberian” in the sense that the individual responsibility of the
investigator toward the truth seems to give short shrift to the social and
cultural structures that make scientific investigation possible in the first
place; Evans’s defense of his book feels a little too “Durkheimian” in its
confidence that the categories generated by practicing sociologists, accord-
ting to collectively ratified methods, will indeed function as they are supposed
to. Surely the answer, at least concerning this aspect of the debate, lies
somewhere between Biernacki’s skepticism about the profession and his
reliance on individual judgment, and Evans’s confidence in the discipline’s
ability to evaluate itself and to formalize its methods for understanding
that wild, wonderful thing we call “culture.”

Thus whether the reader believes Biernacki or Evans about the data,
one question inevitably emerges from this debate: How should we sociolo-
gists conduct ourselves as a practicing collectivity aiming to produce truth
about the social world? This sounds, of course, like an ethical question,
and it is. But it is also a question about method: How are we to design
and reform our approach to the reality we claim to study and speak for?
What discursive forms do we want to place ourselves in and work with, in
an effort to produce a language of inquiry that does, indeed, understand
the world? This book is about how meaning—as a prominent part of both
our own social reality and that of those we study—changes the answer to
these questions.

NOTES

1. At last check, the Section on the Sociology of Culture of the American
Sociological Association had more members than any other section, with the
exception of the Section on Sex and Gender.
2. Jeffrey Alexander and Philip Smith (2003) detail the “cultural turn” in
American sociology in the 1980s.
3. For a good review of the recurrent nature of this debate, see David Frisby’s
introduction to Adorno et al. (1976) or the opening pages of Entrikin (1990).
4. Again, this is something that should interest almost any sociologist, and
it is also an area in which I would implore the reader to consider carefully the
arguments of both authors. As sociologists at work in a conflicted field, we find that
our preconceptions—perhaps derived from experiences of on-the-spot epistemic
and methodological challenges at conferences—may create in us the tendency
to take one side or the other almost immediately; but if there is one thing that I
believe both authors would agree to, it is that in producing this debate they have
read each other's work in painstaking detail. The passionate tone of the debate may suggest dogmatism, but the work belies this notion.

5. For an argument that hermeneutics can in fact provide a productive framework for the study of cultural and social structures, see Reed (2008).

REFERENCES


