

Journal of Contemporary Ethnography

<http://jce.sagepub.com/>

What, We Worry? : The Pleasures and Costs of Defective Memory for Qualitative Sociologists

Joel Best

Journal of Contemporary Ethnography 2006 35: 466
DOI: 10.1177/0891241606286988

The online version of this article can be found at:
<http://jce.sagepub.com/content/35/4/466>

Published by:



<http://www.sagepublications.com>

**Additional services and information for *Journal of Contemporary Ethnography* can be
found at:**

Email Alerts: <http://jce.sagepub.com/cgi/alerts>

Subscriptions: <http://jce.sagepub.com/subscriptions>

Reprints: <http://www.sagepub.com/journalsReprints.nav>

Permissions: <http://www.sagepub.com/journalsPermissions.nav>

Citations: <http://jce.sagepub.com/content/35/4/466.refs.html>

>> [Version of Record](#) - Jun 30, 2006

[What is This?](#)

What, We Worry?

The Pleasures and Costs of Defective Memory for Qualitative Sociologists

Joel Best

University of Delaware, Newark

Inevitably, more is forgotten than is remembered; what is lost to memory may be viewed as collective amnesia. This article considers the development of qualitative sociology as a distinct social world within the discipline; its emergence reflects sociology's growth and shifts in publishing practices that encouraged sociology's division into separate specialties. However, these developments have costs: ethnography is increasingly divorced from its historical context, and despite receiving considerable lip service, synthetic grounded theory is uncommon. Ethnographers need to consider both the benefits and the costs of these developments.

Keywords: *qualitative sociology; collective memory; collective amnesia*

The notion of collective memory has attracted considerable sociological attention, even in the discipline's most prestigious venues (e.g., Schwartz and Schuman 2005). This focus on the process of remembering overlooks another important process—the way collectivities forget what was once known, what we might think of as *collective amnesia*. (This is just a bit of terminological shorthand and should not be taken too seriously; I am not trying to medicalize the phenomenon.)

Forgetting is probably far more common than remembering. Most of what happens—both in our personal lives and in the larger course of history—turns out to be forgettable: it may go unnoticed in the first place, or be noticed but judged unimportant, or be thought to have only temporary significance. Obviously, there is a constantly expanding set of events and experiences; they can't all remain cataloged in everyone's memory. As a practical matter, most will be forgotten.

Note, too, that the passage of time exacerbates collective amnesia. There must be a considerable recency effect: in general, what has only just happened

Author's Note: Kath Lowney made helpful comments on an earlier draft of this article.

is easier to remember, not just because the memory is “fresh,” but because we are more likely to be able to locate current events in a web of meanings, a context that we understand, and we are more likely to imagine that we ought to attend to recent events because they may shape what will happen in the near future. In contrast, the longer ago events occurred, the easier they are to forget: we have more trouble placing them in their full context, and we are more likely to doubt their relevance to our own future.

We can think, then, of collective memory as having a carrying capacity (Hilgartner and Bosk 1988). At least as individuals, we can recall only a small share of what might be remembered, not just about our own experiences, but about events remote in either time or space. Prospective memories must compete for inclusion within this carrying capacity, presumably according to standards that involve perceptions of relevance. Two qualifications come to mind: First, there are memory specialists, such as historians and archivists, charged with keeping track of recollections that most people let slip; and second, what is recalled or forgotten will vary from one social world to the next, because standards of relevance vary. What one group deems memorable, another may forget.

What does all this have to do with ethnography? I want to use this article to examine how collective memory—and collective amnesia—operate within the social world of qualitative sociology. In my view, the discipline of sociology generally—and our corner of it in particular—has developed in ways that encourage remembering some things and forgetting others. In particular, I want to point to some aspects of collective amnesia that seem to have consequences for our work’s place in the larger discipline.

Remembering: The Emergence of Qualitative Sociology as a Social World

Let me begin with a historical argument: I want to trace and explain the development of qualitative sociology in the United States since the Second World War. (Obviously, any such account is necessarily selective—it remembers some things but ignores or forgets many others.) For my purposes, however, it makes sense to begin with what has been called “the Second Chicago School” (Fine 1995). During the postwar years, a remarkable number of people who would become leading qualitative sociologists did their graduate work at the University of Chicago. In particular, they were influenced by Herbert Blumer’s exposition of the theory and methods of symbolic interactionism, and by Everett Hughes’s interpretations of social processes and

social worlds. During the 1950s, members of this new generation—people like Erving Goffman, Howard S. Becker, Anselm Strauss, and Fred Davis—began publishing their work and taking academic positions.

During the 1960s, several developments within the discipline fostered the spread of qualitative sociology. People like Goffman and Becker had become prominent and influential. They were mentoring their own graduate students (while Goffman was at Berkeley, he worked with John Lofland, John Irwin, Jacqueline Wiseman, and other students who would in turn have productive careers). In addition to teaching his own students, Becker edited both the journal *Social Problems* and Aldine's "Observations" book series; these became publication venues for both established and novice qualitative researchers. A self-conscious how-to literature also emerged to guide would-be qualitative researchers (the single most important work of this sort was *The Discovery of Grounded Theory* [Glaser and Strauss 1967], which appeared in Becker's series). At the same time, the decade's social turmoil coupled with the Baby Boomers' arrival in colleges inspired a huge increase in sociology enrollments. Textbook publishers, eager to tap this market, proved willing to publish all manner of qualitative sociology—including monographs aimed at upper-division classes (e.g., Prentice Hall published revisions of ethnographic dissertations by such Berkeley students as Lofland, Irwin, Wiseman, and David Sudnow). Although there was a lot of talk about the hegemony of functionalism and quantitative sociology, opportunities for qualitative sociologists expanded markedly during the 1960s.

Some of these upbeat trends continued into the 1970s and beyond. While sociology enrollments dipped and textbook publishers became less interested in publishing titles aimed at upper-division classes, other venues emerged for publication. In particular, there was a proliferation of new journals, including titles devoted to qualitative sociology: *Urban Life and Culture* (i.e., the title that would evolve into this journal) began publication in 1972, and *Qualitative Sociology* and *Symbolic Interaction* both began in 1978. In addition, the numbers of articles and books discussing qualitative methods continued to grow. And, of course, as each cohort of new PhDs trained in this approach joined graduate departments, there was an ever-increasing number of faculty who thought of themselves as qualitative sociologists and who could train their own students.

As this social world expanded, it also became more insular. In part, this was simply a matter of the overall profession's size. Sociology—like the rest of academia—grew tremendously between 1950 and 1975. At the same time, departments on all campuses were raising their standards for hiring, tenure, and promotion. There were more sociologists, and they were under

more pressure to publish. No wonder journals were proliferating (and the number and size of professional meetings was growing); more sociologists needed additional outlets for their scholarship. But, of course, as more and more sociological work found its way into print, the proportion of what was published that anyone could hope to read shrank.

In the face of the profession's expansion, and with the need to stay on top of whatever literature one hoped to contribute to, pressures for specialization grew (Best 2003). No one could possibly keep up with the entire discipline of sociology, but perhaps one could follow some particular literature. There were multiple bases for specialization: substantive concerns (e.g., deviance), theories (e.g., symbolic interaction), or methods (e.g., qualitative sociology). Often, these social worlds developed a full range of institutional trappings: scholarly societies (or sections or divisions within existing organizations) with their own meetings, honorific offices, and prizes; journals with editors and editorial boards; book series and—most recently—reference books devoted to the specialty.

Such elaborated social worlds offered avenues to professional success. People could publish, hold organizational and editorial positions, develop reputations, and generally acquire all of the sorts of prestige conventionally available to academics. In some ways, success was easier because obstacles could be circumvented: a paper submitted to a specialized journal would be screened by reviewers and editors more likely to be sympathetic with the author's project; similarly, audiences at conference sessions were more homogeneous and less likely to be critical. But these triumphs occurred on relatively small stages. Reputations did necessarily carry over from one specialization to the next. Thus, the number of sociology monographs published might swell each year, but average sales began falling. In part, this reflects higher prices and reduced library purchases (reflecting tighter budgets but also more efficient interlibrary loan systems). But it is also likely due to increased specialization: it became ever harder to justify reading outside the confines of one's specialty. Note that, so long as the profession continues to grow, all of these trends can continue. Specialties themselves subdivide. We now have a *Journal of School Violence*.

Specialization has implications if you try to follow the money. As the 1960s boom wound down, Prentice Hall published Wiseman's *Stations of the Lost* (1970). Under the liberal marketing policies of the time, free examination copies were widely available; the publisher anticipated making it up in course adoptions. The book won SSSP's prestigious C. Wright Mills Award. Those days are so far gone, they now seem the stuff of fantasy. Today's publishers want to concentrate on the most profitable lines, such as

lower-division textbooks (which must be constantly revised to thwart the ever more efficient used-book market), journals (for which libraries can be charged hefty subscription prices), or reference volumes (also pricey, but usually unavailable through interlibrary loan, so that one can hope for substantial library sales). As prices rise, fewer individuals are willing to buy books or subscribe to journals—decisions made easier by growing access to electronic versions of books and journals. In turn, the number of readers falls. Would-be book authors in sociology are beginning to hear publishers talk about subventions (i.e., fees the author pays the publisher to support publication costs)—a practice long familiar to scholars in the humanities, who pioneered the process of specialization aimed at ever smaller audiences.

All of this helps explain the worried critiques that sociology lacks a “core” (e.g., Cole 2001). Most critics attribute this to the discipline’s failure to agree on a basic theoretical framework, a common vocabulary, and a methodological approach. But their arguments miss the centrifugal pressures that reward specialization. Good things—publication, promotion, professional honors—are available through participation in specialized scholarly social worlds. Addressing like-minded specialists has another advantage: these are the audiences most likely to find one’s work interesting and respond favorably to it. The only disadvantage—so small it hardly merits mention—is that these audiences tend to be much smaller.

In my view, qualitative sociology has been part of this process, but there is nothing unique about qualitative sociology. Many of the pressures and processes that led to its emergence as a specialty were essentially those that shaped recent developments in the sociology of gender, or conflict theory, or other sociological specialties we might name. And it is not just that qualitative sociology has distanced itself from the rest of sociology. Within qualitative sociology, as elsewhere in the discipline, subspecialties have emerged, so that we find increasingly separate camps of conversation analysts, ethnomethodologists, postmodernists, and on and on (for one effort at classification, see Adler and Adler 1999). Note, for instance, the recent appearance of thick reference books devoted to guiding qualitative researchers, each filled with dozens of chapters addressing quite specialized topics (Atkinson et al. 2001; Denzin and Lincoln 2005; Gubrium and Holstein 2002). As the sociological universe expands, it separates into distinctive clusters (and those clusters break into subclusters), each specialty based on some particular subject matter, theory, or methodological approach, with each cluster and subcluster becoming progressively distinct from the others.

Whereas histories of qualitative sociology tend to focus on intellectual developments, the rise and spread of these ideas were affected—at least in

part—by these trends in the organization of the discipline. Opportunities to receive training, get jobs, and share one's ideas provide contexts within which intellectual social worlds flourish or wither. Scholars encounter fewer obstacles (such as unenthusiastic reviewers) by locating and reaching audiences most likely to appreciate one's contributions; unpleasant dissension, disagreements, and debate among dramatically different points of view can be minimized. Trade-offs in return for these benefits are inevitable: a smaller, supportive audience of fellow specialists offers rewards, but we should not forget the costs.

Forgetting: The Costs of Growing Separation

So, what are the costs of these changes? Many of them can be ignored, because the organization of specialization makes it easy to forget what has been lost. This is where collective amnesia kicks in: much is forgotten—particularly links to other forms of sociology. Let me begin with an example from the sociology of deviance. During the 1960s boom in qualitative sociology, deviance—along with Goffmanic studies of face-to-face interaction—was one of the topics most likely to attract qualitative researchers. At the time, two of the leading books on deviance were *Symbolic Crusade*—Joseph Gusfield's (1963) study of the nineteenth-century anti-alcohol movement's shift from temperance to prohibition—and *Wayward Puritans*—Kai Eriksen's (1966) analysis of the place of deviance in colonial New England. These books—although very different in their theoretical assumptions and methods from, say, either Goffman's *Asylums* (1961) or Becker's *Outsiders* (1963)—became part of the deviance canon. At that time, qualitative sociologists interested in deviance were interested in reading and even doing historical analyses. Today, historical and ethnographic analysts—not just in deviance, but generally—seem to occupy distinct, virtually unconnected social worlds, and once-important links are forgotten.

We could point to lots of parallel examples: take how conversation analysis has split off from other studies of face-to-face interaction. Perhaps there was a golden age when qualitative sociologists were familiar with not all but at least a fair share of one another's work. But as the sheer volume of work makes that impractical, and with all of the pressures promoting specialization, the sense of a shared enterprise becomes diminished.

Notice that reintegration is a generally unattractive prospect. The prospect of intellectual interstimulation aside, there are few benefits to, say, combining two specialties into one—this can only lead to a reduction in the

rewards specialization brought in the first place. Instead, why shouldn't those interested in, say, both historical studies and ethnographies establish a new specialty, based precisely on that link? It can have its own association, journal, . . .

But let's return to qualitative sociologists' declining interest in history; it has another consequence—our attention becomes focused on the present as we forget about the past. This means that analysts tend to view the current manifestations of social arrangements as, if not unique, at least less closely linked to any larger context. That we live in a “rapidly changing world” is a central cliché of our culture; it implies that somehow things are different, maybe completely different. After all, don't we have cell phones and MTV and the Internet and globalization? This sort of thinking ignores the historical record in a couple of respects. First, it is not at all clear that basic social arrangements have changed all that much. I encourage my students to read Sinclair Lewis's novel *Babbitt* (1922). I know—George Babbitt, to the degree that his character is remembered, is usually thought of as a small-minded, small-town booster. But anyone who takes the time to read the novel may be surprised by how contemporary it seems—Babbitt's relations with family, job, community, and, yes, the mass media seem quite contemporary; the reader can easily conclude that American culture and social organization have not changed all that dramatically in the last 80-plus years.

Second, a truncated view of history allows us to forget how social issues come, go, and then reappear. This is one of the clearest lessons of the historian Philip Jenkins's constructionist analyses; in several books, Jenkins (e.g., 1998, 2000, 2001) explicates our society's recurring worries about child molestation, cults, or even secret gospels long suppressed by the religious authorities. This demonstration that Americans discover, forget, and then rediscover social problems is interesting. Not only does it remind us that collective amnesia is experienced, not just by sociologists but by those we study, but it also suggests that our society's structural and cultural contours make it more likely for some concerns to arise instead of others. Ethnographers who ignore this will miss a large part of what's going on. The ability to locate social events in a longer historical context was a central feature of what Mills meant by the sociological imagination; our readiness to lop off history as irrelevant to what we study is tragic.

Moreover, it encourages us to reinvent the wheel. One of the justifications for breaking off into specialized scholarly worlds is that each new world is somehow unique—unique in its theoretical apparatus, its methodological techniques, or its subject matter, or some combination thereof. We

are, the new world's proponents claim, exploring a new frontier, seeing new things in new ways. This means, of course, that they think of themselves as explorers, pioneers in uncharted lands. In turn, this makes it unnecessary to worry about those who may have preceded them, because even if those predecessors were interested in similar things, they were armed with different concepts, different techniques—their approach was erroneous and may be ignored, forgotten. And that's fortunate, because it makes the analyst's job so much easier.

Consider the vast amount of sociological energy that has been devoted to studying, say, police over the past 40 years. During the 1960s, sociologists began to figure out that police could be studied ethnographically, through ride-alongs or—less commonly—by becoming an auxiliary or even a fully sworn officer. A large literature began to emerge—there must now be hundreds of ethnographic studies of police (to say nothing of research on police based on other methods). At this point, it is difficult, if not impossible, to read it all, and probably few read more than a tiny fraction of these works. (Note that I am not pointing to some problem that is unique to scholars of policing. The same dynamics are likely in any specialization.) But, if scholars don't know what others have already learned, how can they know whether they're learning something new? This is where the justification of specialization becomes so useful: it is possible to discount earlier research as being theoretically or methodologically primitive, even wrong-headed; and therefore it can be forgotten.

Which leads us to another forgotten virtue—synthesis. Philosophers like to suggest that science is cumulative, that over time, findings build to support conclusions. This means that analysts ought to know what researchers before them have found, that those findings ought to guide their choice of subjects to study, and that a primary goal ought to be to add to a growing body of knowledge. Although this does not require remembering every preceding bit of research, it does suggest that a familiarity with the research most relevant to one's own studies is desirable, even necessary. Dismissing previous research as old-fashioned, misguided, or irrelevant damages the prospects of cumulative knowledge.

In particular, for qualitative sociologists, this damage strikes at the heart of the theory-building enterprise first sketched in Glaser and Strauss's classic *The Discovery of Grounded Theory* (1967) and extended in Strauss's later writings on theory and method (e.g., Strauss 1987). Part of Glaser and Strauss's approach was, if you will, *intra-analytic*—that is, it directed researchers to compare and contrast within their own data, so that they could develop inductive generalizations that summarized and reflected their findings. By following these guidelines, ethnographers could devise

theoretical propositions that discovered patterns in their studies' data. But that was only the first part of the process. Glaser and Strauss also advocated *interanalytic* theory building, in which theories devised in different case studies could be synthesized into more abstract, more general grounded theories of social processes.

Here, I think, qualitative sociologists have done less well at meeting Glaser and Strauss's challenge. It is easy to rattle off dozens, hundreds of interesting qualitative case studies—many with obeisant citations to Glaser and Strauss or similar texts. They are relatively easy to carry out: the researcher enters some setting in the field, gathers data, and develops and writes up an analysis. (I am not arguing that this work is without its challenges, but notice that it is quite common for graduate students to manage to complete these projects, write acceptable theses or dissertations, and publish the results. This suggests that those challenges are not insurmountable.) These studies, particularly to the degree that their subjects seem exotic or otherwise entertaining, attract readers interested in that corner of the larger world.

There is, I think, a strong tendency to define those corners in terms of their substantive setting. To continue picking on students of police, we are quick to recognize that a study of, say, how police officers' work incorporates understandings of the geography of the area in which they're working adds to the literature on policing. But we are, I think, less likely to locate it within the more abstract body of knowledge about how actors use their sense of geographic place. (One solution to this, of course, is for those interested in sense of place to organize their own specialty, in hopes that those interested in cops and place can locate and communicate with those interested in, say, UPS drivers' geographic understandings. Of course, this solution just means we're off to the same old greater specialization races.)

All of this means that interanalytic syntheses aren't much valued. It is hard to point to many successful—that is to say, widely read and widely cited—syntheses, grounded theories that integrate the findings of many case studies. At first, this might seem surprising. Why wouldn't there be greater interest in synthetic analyses that pull together the literature and develop a coherent sociological approach?

In practice, however, such works seem to fall between the stools of publishing opportunities. First, it is possible to doubt whether an inductive theory that builds on existing studies offers anything new, and to the degree that editors are moved to ask that question, publication prospects are reduced. Second, the more abstract level of the analysis is likely to be appreciated by the relatively narrower audience of serious professionals—faculty and graduate students. This means that it is tough to convince a textbook house, let alone a trade publisher, that they want to publish a work of grounded theory.

Third, as noted above, the prospects for scholarly monographs are much bleaker today: changes in library budgets and publishing costs lead to ever lower sales for monographs, which in turn discourages publishers from bringing out too many titles. And an editor can be forgiven for suspecting that a nice ethnography, ideally one with an exotic setting or topic that promises to be appealing, will be more attractive than a synthetic piece of grounded theory. Fourth, anyone who regularly reads the qualitative sociology journals may suspect that either people aren't writing much grounded theory or editors aren't choosing to publish those manuscripts. Grounded theory seems to be the equivalent of satire on Broadway—it's what closes Saturday night. Once more, the scholarly reward structure shapes what analysts do. We may give lip service to the desirability of building more general inductive theories, but we don't seem to do it all that much or to appreciate those who make the effort.

In other words, collective amnesia is common. Let's be clear: this is not because qualitative sociologists are people of weak character but because their work is structured in ways that make it very easy for them to forget, particularly to forget links to other parts of the sociological enterprise.

Recollecting: Is There a Better Way?

I've argued that the organization of contemporary scholarship encourages the emergence of ever more narrowly focused specialties that are increasingly divorced from one another. This poses, in my opinion, particular risks for qualitative sociologists. Sociology's center ring—*American Sociological Review (ASR)* and *American Journal of Sociology*—does not exclude qualitative research, but it does insist that researchers frame their work as making significant contributions—which almost always means the development of a new theoretical framework, new methods, or a sophisticated test of theoretical propositions. Again, this is not restricted to quantitative studies. (Skeptics should take a close look at the recently released "ASR's Greatest Hits"—the list of *ASR* articles that have received the most citations [Jacobs 2005]. Eighteen articles had at least five hundred citations listed in *Social Sciences Citation Index* by late 2004 [this of course ignores citations in books, journals not covered by *SSCI*, etc.]. Among these top articles are works quite familiar to qualitative sociologists, including Scott and Lyman on accounts, Sykes and Matza on techniques of neutralization, and Mills on vocabularies of motives. In fact, examination reveals that twelve of the eighteen do not have a single table displaying quantitative data—a fact that suggests that quantitative studies do not have a lock on the

top journal's pages [and also offers a lesson about the relative importance of theory].)

Still, even if a few qualitative studies make it to the discipline's center stage, most of them—and, of course, most quantitative studies, too—find themselves relegated to the periphery, to less prestigious venues. This is consequential. It is particularly desirable to place a paper in *ASR*, not just because that journal is seen as especially selective and therefore presumably publishes the discipline's very best papers, but also because a relatively large share of sociologists are ASA members who receive *ASR* (although the proportion of members who choose not to subscribe to the flagship journal has been rising). That is, the next issue of *ASR* will land in a lot of mailboxes, which means that there is at least a chance that lots of people might notice—even read—an article therein. The problem with publishing a paper in a specialized journal is not that those journals don't take excellent work; we can all point to first-rate articles that appeared all sorts of journals. Rather, the problem is that those journals tend to have fewer subscribers, particularly if they are published by a commercial publisher (as opposed to a university press) and if they are not an official journal of some scholarly society (and therefore automatically distributed to all its members). As individuals, I imagine all of us have considered subscribing to one more journal—perhaps one directly in our specialty—and decided that it was too pricey, that it would make more sense to follow it in the library (or, increasingly, in an electronic edition that our library may make available on whatever computer we use to log on). But it proves to be much easier to glance at the new hard copy that arrives in our mailbox than to remember to keep up with journals stored in the library (let alone in cyberspace). This is precisely why citation counts show that articles in *ASR* are cited, on average, more often than articles in any other sociology journal; articles in the prestigious journals are seen by more people, read by more people, and actually used and therefore cited by more people. That's why those journals are considered more prestigious venues for publication.

In contrast, less prestigious journals attract smaller—in the case of most specialized journals, much smaller albeit more homogeneous—audiences; how much smaller, no one can know for sure. A fair share of journal articles never get cited, an indication that they either weren't much read or didn't make much of an impression. One problem may be that, in trying to break into specialized journals, authors believe that they should address that narrow audience—a strategy that may, in fact, improve one's chances of acceptance but at the cost of losing any prospect of having broader influence.

My goal in writing this article is to jog qualitative sociology's memory. I sense a certain smugness in our circles. It is as though we've made it to the Big Time. After all, there are all of these proofs of our importance—we have our own journals, we have our own associations and our own honorific offices, editorial posts, reference books, banquets, prizes, etcetera, etcetera. What clearer proof of our significance?

What these comfortable thoughts ignore is that very much the same claims can be made by our counterparts in every specialized corner of sociology. Our rise has been fostered by a structural and cultural context—by enrollment patterns, shifting profitability for different sorts of publishing, trends in library budgets, and so on. In general, the conditions that encouraged qualitative sociology to emerge as a distinct social world also fostered the appearance of many other specialties that are able to tell analogous, self-congratulatory tales about their own rise.

But—and this is my point—making those stories appealing requires a good deal of forgetfulness, of collective amnesia. Being an independent specialty has its attractions, but it also carries costs. In particular, we lose many of our ties to the larger discipline, not just to those incomprehensible multipage regression tables that *ASR* so loves—a lot of us might be willing to let those go—but also to intellectual traditions that used to enrich us, such as our connections to historical sociology. Is it really desirable to think of ourselves as, say, ethnographers if that means severing links to the rest of the discipline?

References

- Adler, Patricia A., and Peter Adler. 1999. The ethnographers' ball—revisited. *Journal of Contemporary Ethnography* 28:442-50.
- Atkinson, Paul, Amanda Coffey, Sara Delamont, John Lofland, and Lyn Lofland, eds. 2001. *Handbook of ethnography*. Thousand Oaks, CA: Sage.
- Becker, Howard S. 1963. *Outsiders: Studies in the sociology of deviance*. New York: Free Press.
- Best, Joel. 2003. Killing the messenger: The social problems of sociology. *Social Problems* 50:1-13.
- Cole, Stephen, ed. 2001. *What's wrong with sociology?* New Brunswick, NJ: Transaction.
- Denzin, Norman K., and Yvonna S. Lincoln, eds. 2005. *The Sage handbook of qualitative research*, 3rd ed. Thousand Oaks, CA: Sage.
- Eriksen, Kai T. 1966. *Wayward Puritans: A study in the sociology of deviance*. New York: Wiley.
- Fine, Gary Alan, ed. 1995. *A second Chicago School? The development of postwar American sociology*. Chicago: University of Chicago Press.
- Glaser, Barney G., and Anselm L. Strauss. 1967. *The discovery of grounded theory*. Chicago: Aldine.

- Goffman, Erving. 1961. *Asylums: Essays on the social situation of mental patients and other inmates*. Garden City, NY: Anchor.
- Gubrium, Jaber F., and James A. Holstein, eds. 2002. *Handbook of interview research: Context and method*. Thousand Oaks, CA: Sage.
- Gusfield, Joseph R. 1963. *Symbolic crusade: Status politics and the American temperance movement*. Urbana: University of Illinois Press.
- Hilgartner, Stephen, and Charles L. Bosk. 1988. The rise and fall of social problems. *American Journal of Sociology* 94:53-78.
- Jacobs, Jerry A. 2005. ASR's greatest hits. *American Sociological Review* 70:1-3.
- Jenkins, Philip. 1998. *Moral panic: Changing concepts of the child molester in modern America*. New Haven, CT: Yale University Press.
- . 2000. *Mystics and messiahs: Cults and new religions in American history*. New York: Oxford University Press.
- . 2001. *Hidden gospels: How the search for Jesus lost its way*. New York: Oxford University Press.
- Lewis, Sinclair. 1922. *Babbit*. New York: Grosset & Dunlap.
- Schwartz, Barry, and Howard Schuman. 2005. History, commemoration, and belief: Abraham Lincoln in American memory, 1945-2001. *American Sociological Review* 70:183-203.
- Strauss, Anselm L. 1987. *Qualitative analysis for social scientists*. New York: Cambridge University Press.
- Wiseman, Jacqueline P. 1970. *Stations of the lost*. Englewood Cliffs, NJ: Prentice Hall.

Joel Best is professor and chair of the University of Delaware's Department of Sociology and Criminal Justice. His most recent book is *Flavor of the Month: Why Smart People Fall for Fads* (University of California Press, 2006).