

Blaming the Victims: Hearsay, Labeling, and the Hazards of Quick-Hit Disaster Ethnography

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How can sociology contribute to knowledge about disasters, and what methods of research are appropriate for understanding risk and vulnerability during extreme events? The two articles (Browning et al. 2006; Duneier 2006) in this issue of *ASR*, which discuss my 2002 book *Heat Wave: A Social Autopsy of Disaster in Chicago* (hereafter HW), provide occasion to address these increasingly urgent questions.

The book HW focuses on two overarching puzzles: First, why did 739 Chicago residents, most of them old, alone, and impoverished, die during the July 1995 heat wave disaster? Second, why did their deaths prove so easy to deny, overlook, and forget? One chapter in HW, "Race, Place, and Vulnerability," explores the reasons why neighborhoods with roughly comparable demographic risk factors for heat deaths had widely disparate mortality rates. It also attempts to debunk prevailing myths that African Americans, who experienced higher proportional death rates than any other group and at every age level, were most vulnerable because of group-level physiological, cultural, or simple behavioral characteristics. These myths were harmful, because they helped perpetuate the official and journalistic opinion that people died because their families and neighbors did not care about or take care of them, or because they neglected themselves. Drawing on comparative ethnographic research in two neighborhoods, including interviews and observations of street-level conditions, HW issues the following claim:

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A key reason that African Americans had the highest death rates in the Chicago heat wave is that they are the only group in the city segregated and ghettoized in community areas with high levels of abandoned housing stock, empty lots, depleted commercial infrastructure, population decline, degraded sidewalks, parks, and streets, and impoverished institutions. Violent crime and active street-level drug markets, which are facilitated by these ecological conditions, exacerbate the difficulties of using public space and organizing effective support networks in such areas. (Klinenberg 2002:127)

The two articles in this issue of *ASR* assess this claim. In the most sophisticated quantitative analysis of the Chicago heat wave mortality to date, Browning et al. (2006:665) confirm that "[c]onsistent with Klinenberg's ethnographic study of the Chicago heat wave, commercial decline was positively associated with heat wave mortality and explains the affluence effect." Duneier is more skeptical. In the latest of his apparently endless efforts to cast doubt on HW, Duneier revisits Chicago for two afternoons of fieldwork, from which he derives three core claims: First, that all but four decedents in North Lawndale (NL) were living with others when they died, and that therefore they did not die alone; second, that HW commits the "ecological fallacy"; third, that "at least" half of NL victims were "alcoholics" and "drug users," and that this "suggests" why they died. He lacks reliable or sufficient evidence for all three claims, and his argument that NL residents died from alcohol and drug use is both unfounded and dangerous. In this brief comment I show why.

DYING ALONE

It is impossible to know precisely how many people died alone during the heat wave. There are two principal reasons for this: First, no official records establish whether a person was alone at the time of death. Second, there were

hundreds of “excess death” cases for which there were no autopsies or police investigations.

A close look at Duneier’s (2006) article reveals that *he actually did not find that most NL decedents were with other people when they perished*. He reports that 75 percent of the decedents whose cases he investigated “were living with their families or domestic partner” (italics added), but he does not show that they necessarily were with someone when they died. Surely it is possible for a person to die alone even though he or she technically “lives with” a partner. The partner, for example, might be out of town to escape the hot weather or might not spend every night at home. Although HW establishes a clear conceptual distinction between living alone and dying alone, Duneier conflates the two. This is one reason that his evidence does not challenge HW’s conclusions.

What, exactly, is his evidence? Duneier (2006) asks current residents of the victims’ former homes or neighborhoods what happened to the decedents. I tried this method ten years ago. I visited places where people died and asked “informed informants” how others died. Yet I lost confidence in the validity of the responses after hearing several unreliable accounts about individuals and places. “Yeah, I remember the heat wave. But we didn’t have any problems here,” said a white clerk at one single-room occupancy hotel where official records listed multiple heat deaths. “It was terrible. And I thank God no one passed on this block,” said an African American woman who lived a few doors down from a victim. So I abandoned the project of determining the exact number of people who died alone, and *not once* do I claim that the majority of people in North Lawndale or any other neighborhood died alone. (I did, however, dedicate the entire first chapter of HW, “Dying Alone,” to an ethnographically informed account of the phenomenon in Chicago. For reasons he cares not to share, Duneier has avoided discussing the HW chapter “Dying Alone” in his *ASR* article.)

Because there is no definitive or official evidence that establishes how many people died alone, some investigators have inferred deductively what happened: It is easy to prevent heat deaths, because until the later stages of heat-related illnesses, afflicted people typically exhibit painful symptoms of sickness (such as cramps and nausea), and need only to be treated with

water or artificial cooling. Anyone who recognizes symptoms of excessive heat exposure can save the sick person’s life.

We also have evidence about the prevalence of dying alone. In HW, I show that Cook County investigated approximately 160 cases of heat wave victims who not only died alone but also had no one come to claim their bodies or estates. I also cite the U.S. Centers for Disease Control and Prevention’s (CDC’s) case-control study of 339 heat victims, 156 of whom lived alone, which establishes that “living alone, as compared with living with others, was associated with a doubling of the risk of death during the hot weather.” Moreover, the CDC gives the following caution:

As is true in other studies that rely on information provided by surrogate respondents . . . our results may underestimate the risk associated with social isolation, since people with few social contacts (and hence no identifiable surrogate) were excluded from the study. (Semenza et al. 1996:87–88)

Duneier not only ignores the problem of under-sampling isolated decedents in his research, but he also fails to disclose that his own sample excludes hundreds of heat wave victims in Chicago, including probably 6 or 7 in NL (its proportional share of the overall mortality). “The Medical Examiner” (ME), he reports, “classified 739 deaths as heat-related,” and he presents summary statistics as if they represent 739 cases—but they do not. The official “heat-related” death list contained only 485 cases for the week between July 14 and July 20. The “excess death” toll was 739, but this figure comes from statistical inference, not the ME list (Whitman et al. 1997). Duneier’s sample of 16 does not only (and inexplicably) exclude 3 known cases (approximately 19 percent of his total) listed by the state; it also leaves out all of NL’s “excess deaths.” Duneier may choose to avoid acknowledging the missing cases, but by doing so, he undermines his effort to learn precisely how many people died alone—particularly since high-death areas, such as NL, likely were overrepresented in the excess deaths.¹

¹ The Illinois Health Department reports that the NL mortality may be even higher: “although it is much broader than the classic definition, the ME definition of heat-related deaths still underclassified

By far the greatest problem with Duneier's evidence is that it is unreliable. His strong claims about the lives and deaths of specific individuals are based on decade-old, retrospective accounts from the *very people* whom city officials, the media, and countless citizens had publicly *blamed* for having allowed their family members and friends to die. Worse, many of these reports *are not even firsthand accounts* of what happened during the heat wave, but are secondhand information, literally "hearsay." Sociologists may use hearsay as evidence about what people believe to be true, or to understand rumors (Fine and Turner 2001), but we certainly should not present hearsay as reliable truth claims, particularly when it comes to explaining death.

Consider three of Duneier's key cases, "Ricky Coleman," age 48, "Leon Smith," age 80, and Robbie Lowery, age 79. Duneier (2006:683) interviews Coleman's sister, "Mrs. Dampeer," who recalls that their parents "were present just one room away" when Ricky died, and that "Ricky had been a heroin addict throughout his adult life." Duneier (2006:685) writes that "Mrs. Dampeer recalled her parents' shock when [Ricky] failed to emerge from his room to remove something he was cooking on the stove." But Mrs. Dampeer herself was not there for any of this. Her report raises important questions: Did Ricky complain of symptoms from heat-related illness, such as renal failure, dehydration, or fever? Was he using drugs before he died? Was he a "recovered" or "active" "heroin addict"? How can we confirm the "fact" that, although heat-related illnesses typically break down the body over 48 hours, Ricky was at one moment "cooking on the stove," apparently not complaining about either the temperature or the physically painful and mentally disorienting symptoms of heat stress, and at the next moment dead in his broiling room? This may well have happened, but we would prefer a direct report

the overall mortality attributed to the July 1995 heat wave . . . Across Chicago communities, this underclassification was more pronounced in high-risk community areas where the excess mortality was high . . . [T]he heat-related mortality rates in many community areas were only 40% of the excess mortality rates, and the absolute difference between the two rates became greater as the level of excess mortality rates was higher" (Shen et al. 1998).

from someone who was actually with Coleman when he died instead of a secondhand story from a family member who claims only to have heard it from another relative.

Duneier also reports hearsay as fact in his account of Leon Smith, a widower who died alone:

[A] neighbor who sits on his stoop throughout the day told me Smith's friend, a police officer named DeWallis Gordon, currently living in Florida, checked on him every day. In addition, he lived "13 minutes from the home of his daughter, Frances Woods (a curiously precise travel time for a neighbor to report). According to his neighbor, Jones was a regular at Lena's Lounge, an air-conditioned tavern just a few steps from his house and was not a social isolate or "shut in." (Duneier 2006:686)

How are we to assess this information? Duneier does not report that he contacted the officer to confirm that he "checked on [Smith] every day," let alone during the heat wave, when it mattered most. What about his daughter? Did she visit her father during the heat wave? Did Smith patronize Lena's Lounge during the disaster? If not, did his friends there, "just a few steps from his house" and, no doubt, aware that people were dying of heat, walk over to see if Smith was alright? Duneier gives no indication that he asked, and thus, we do not know.

Duneier does have a firsthand account about the death of Robbie Lowery, age 79. Lucille, her sister-in-law, reports the following: "I cooked her dinner, combed her hair, and gave her a bath. She was talking to me before she went to sleep on the couch. . . . And then she never woke up" (Duneier (2006:686). What is remarkable about this touching story is that Lowery apparently avoided the suffering so typical of heat victims, and experienced a perfect version of the "good death": painless, surrounded by family and friends, with the final moment arriving silently, during a slumber at home (Nuland 1993). Perhaps it really did happen this way—but given what we know about heat-related illnesses and deaths, should we not treat this "social fact" with some interpretive caution?

Duneier (2006:683) also claims that "Klinenberg's book provides no data on the individuals who died," questions whether the victims were primarily elderly, and accuses HW of rendering "invisible" the younger deaths. HW clearly *does* have individual-level data about people who died: for examples of descriptive

evidence, see pages 14–15 and 38–41; for statistical summaries, see the tables on page 19. Moreover, HW establishes that, after an age adjustment, African Americans between ages 55 and 64 were 1.8 times more likely than whites of the same age range to die during the heat wave, and African Americans under age 55 were 1.3 times more likely to die (although only 5 African Americans and 4 whites under age 55 were counted as heat-related deaths). “These stratified mortality figures,” I argued, “reflect typical patterns in Chicago,” where African Americans are always more likely than whites to die young (Klinenberg 2002:19). A reader need only crack the spine of HW to see that Duneier’s claims—that the book provides no data on individuals and makes the deaths of young people “invisible”—have no basis in fact.

THE ECOLOGICAL FALLACY

Duneier’s misrepresentations of the evidence in HW allow him to make another unfounded argument: that HW advances the “ecological fallacy” that “people died ‘because’ of the social environment in which they lived” (Duneier 2006:690). This is disingenuous, because HW does not make inferences about groups based on aggregate statistics. Instead, its claims about vulnerability are based on both primary observations of NL and the CDC’s study (Semenza et al. 1996), which shows—more powerfully and reliably than any research Duneier or I have done—that not having an air conditioner, lacking social contacts, and not leaving home were the key risks for heat death. (This is why all but one of Duneier’s “findings” about the “risk factors” for heat deaths—the purported substance abuse of decedents—are actually only confirmations of what we already knew.²) Yet the CDC study could not explain why neighborhood mortality varied so dramatically during the heat wave, nor why African American communities were disproportionately affected. My research

strategy was to cite the CDC’s individual-level findings and advance knowledge by focusing on the neighborhood level.

I cite a number of individual-level cases to support my ecological claims. For example, I report the following about “Mrs. Freeman” who is in her 70s:

[She] believes that the local youths [though not the ones from her block, whom she knows] watch her and are waiting for an opportunity to break in, and her conviction was strengthened when a neighbor found a local man trying to break into her back door. “It’s hard leaving your house,” she told me, “especially in this neighborhood. People are looking to see who’s out. They’ll come and rob you.” (Klinenberg 2002:102)

Father Michael, an African priest in his 30s who had just arrived in NL, confirmed that people in the neighborhood often vocalized their concerns about being outdoors:

I had no fear until they told me, oh no, it’s not safe. These people steal on the corners and so on. They might cause trouble. . . . They would tell me that there would be drive-by shootings. They would fight among themselves, but I would be caught in the crossfire and I would be shot. And some feared even that if you walk they can come and snatch you. (Klinenberg 2002:103)

Darcy Baker, who had lived in NL for more than 40 years, told me that these problems were particularly bad in the mid-1990s:

If you were standing here [in 1995] you’d see someone selling drugs on every corner. . . . groups of people. . . . There were dealers standing in front of your home, hiding drugs in your yard. We spent all our money planting flowers and putting grass down, and they were hiding their drugs in front of our house. . . . There were bullets coming down our block. You couldn’t sit out any longer. We used to sit outside all night and just talk and do whatever. But that’s changed. (Klinenberg 2002:101)

HW also uses interview data to show that NL residents who were not particularly worried about crime had a different reason to stay indoors: there was little for them to do outside, particularly (but not exclusively) if they were elderly. One informant explained that the reason why so many people stayed at home was that “[t]here’s not very much in the streets for people to do here anymore” (Klinenberg 2002:94). Another complained, “There’s no grocery store, no Walgreens, no pharmacy, nothing for us here. . . . All of the places here are deserted.”

² Duneier’s finding that “unemployment” was a risk factor for heat deaths is also a confirmation of my own claim, that massive job loss was one key source of vulnerability in NL. As I explain in HW, the CDC did not examine whether poverty was a risk factor, but we can use the presence of home air conditioning as a rough proxy for poverty

Sarah Jones also mentioned how public life in the neighborhood suffered: “You ain’t got no houses. You got nothing but lots. . . All this land you lookin’ at and you don’t see people. You ain’t even got no store open. And Roosevelt [Street] used to be full of stores.” Tellingly, the most common phrase people in NL used to describe the major streets that anchored NL was “bombed out.” (Klinenberg 2002:97–8)

In HW, I argued the following:

[T]here is little evidence that during the heat wave the most isolated and vulnerable residents of places like NL suffered because members of their community did not care about them. Yet there is good reason to believe that residents of the most impoverished, abandoned, and dangerous places in Chicago died alone because they lived in social environments that discouraged departure from the safe houses where they had burrowed and created obstacles to social protection that are absent from more tranquil and prosperous areas. (Klinenberg 2002:127)

This claim comes from observations and interviews, not from ecological inferences based on aggregate data about groups. The articles presented here by Browning et al. and Duneier give little reason to doubt it.

BLAMING THE VICTIM

Duneier makes only one original contribution to the debate about the causes of heat wave mortality in NL: that “at least” half of the heat wave decedents in NL “were said to be alcoholics and/or users of illicit drugs” (Duneier 2006:688), and that this is a possible cause of their deaths.

How reliable or useful is this information? Duneier does not say whether they were using alcohol or illicit drugs during or immediately before the heat wave. He does not tell us if any of the subjects were “said to be” *recovered* or *active* alcoholics and users of illicit drugs. He does not cite medical literature that establishes an association between certain kinds of illicit drug use and heat wave mortality, or explain whether a history of alcoholism or illicit drug use is associated with elevated risk of heat mortality, *even if the subject is no longer drinking or using drugs*. This is a crucial point, because, in a telephone conversation on March 27, 2006, the medical examiner Edmund Donoghue told me, “We did toxicology on these people, and

unless their records say so, they did not die from alcohol intoxication, cocaine, or opiates.”

Duneier says he has confidence in the alcoholism and drug use attributions because “the information provided by my informants *on the ages and names of spouses* conformed with that provided on the death certificates and Medical Examiner records” (Duneier 2006:689, emphasis added). But consider this carefully: Does the fact that informants can state the name of a spouse and the age of a victim mean that they are a reliable source of information about whether the person was an alcoholic or drug user, or whether the victim was abusing substances before his or her death? And what if they actually get the victim’s age and name wrong? According to Duneier, that makes them less credible. Duneier’s method for confirming the retrospective reports from his informants about the substance abuse of people who died nearly a decade earlier inspires little confidence in his findings. It also begs a crucial question: Since Duneier has the ME reports, why does he not provide concrete evidence establishing whether alcoholism or intoxication were listed as causes of death?

It would be easy to do so. Duneier says that he has copies of the ME reports and death certificates for all 16 cases in his subsample of NL decedents, and that he confirms the information he gathers through fieldwork by matching it to these official documents. Given the nature of his “suggestion” that substance abuse was a key cause of death, Duneier has a clear responsibility to provide an answer for one basic question: For how many of the individual victims whose cases he analyzes did the autopsy report list “chronic alcoholism” or illicit drug use as a contributing cause of death? This information is on the ME reports that Duneier uses as evidence—I know this not only because Donoghue told me so, but also because I have a sample of the certificates, and “alcoholism,” “cirrhosis of liver,” or “cocaine and opiate intox” are listed as additional causes on some of them.

As it happens, I was able to locate the ME records for a “Ricky Colemon,” age 47 at death. (I confirmed the age and spelling of his name through the *Chicago Sun-Times* [1995]. Duneier, who calls him “Coleman” and “48,” does not explain the discrepancy between his data and the official records that he uses.) Did this man’s death record “conform” to the description of him

given to Duneier? No. His name and age appear to be wrong, and the errors mean that Duneier has not met even his own, weak standards for confirming the case. By the far the most troubling information on Colemon's ME record, however, is that it *does not* list "alcoholism" "cirrhosis," or "intox" as contributing causes to Colemon's death. His record gives only "A.S.C.V.D. [Atherosclerotic Cardiovascular Disease], PT. II [Type 2 Diabetes]" and "HEAT STRESS" as the "causes of death."

This discovery concerned me, and so I decided to take a close look at the names and ages listed in Duneier's (2006) article and match his cases to those in the ME records. (Duneier claims that he disguises the names of all the purported substance abusers except for Colemon, but he failed to do so in the article accepted for publication here. I was therefore able to identify 11 victims directly and to deduce the other 5 names.³ For how many of these 16 cases was alcoholism or narcotic use listed by the ME as a cause of death? *One*: "James Franklin," age "37," who died July 24 of "Heat Stroke Pt II, Chronic narcotism, and Septicemia." (It is curious that "Franklin's" death—the only case of a confirmed death from substance abuse on Duneier's list—is also the only case Duneier includes that is beyond the week of the heat wave: July 14 to 20.)

Then I noticed another problem: there are discrepancies between Duneier's findings and the names or ages at death listed for 4 of his 8 "confirmed" addicts on the ME reports: Colemon, "James Franklin" (whose age the ME lists as 44, not 37), "Grover Wilson," and "Willie Rose." In other words, *the information he received about half of the "confirmed addicts" did not conform to the ME's reports*, after all. Duneier fails to meet his own questionable criteria for confirming the validity of crucial information for half of his cases.

This is beyond troubling. The clear inconsistencies raise serious questions—not only

about the reliability of Duneier's evidence, but also about the reliability of his own report on what he has confirmed. What, exactly, is the information "provided on the death certificates and Medical Examiner records" (Duneier 2006:689) that corroborates his informants' claims that decedents were drug addicts and alcoholics? If Duneier has any such corroborating evidence about substance abuse, why not show it? Why does Duneier fail to disclose the fact that the ME reports do not, in fact, corroborate his own suspicion about the role of drugs and alcohol in the NL heat deaths, but actually call it into question?

If the death records and police reports do not list these conditions as contributing causes for half of the NL decedents, what explains the discrepancy between the autopsies and Duneier's (2006) findings? Does Duneier believe that the ME, whose reporting he otherwise treats as completely reliable, failed to identify traces of the substances in their toxicology exams? (The ME acknowledged that he missed hundreds of cases because bodies were not autopsied, but not that his staff made diagnostic errors with the decedents they examined.) If so, why, given that the ME lists "alcoholism" or "intox" for other heat wave victims? Is it possible that the "informed informants," who had already been publicly blamed for the heat death of their relative or neighbor, overstated the victim's alcoholism or illicit drug use, or that they subscribed to the folk notion that "once-an-addict-always-an-addict"? Could they, despite or even because of their close ties to decedents, have engaged in what sociologists such as Howard Becker call "labeling" (1963), offering a "quick description" or lay explanation of a long-deceased person to an unknown social scientist who appears, unannounced, at their doorstep?

Duneier's failure to report the conflicting evidence in the death records while providing suggested causes of death for *actual people* is inexcusable—particularly in an article that claims to deepen knowledge based on "shoe leather." Drawing upon research based on a mere two visits to NL, "additional interviewing" by two research assistants, and "additional telephone calls," Duneier issues politically consequential and morally loaded allegations about the lives and deaths of individuals he has hardly taken time to understand. Although in previous research, Duneier (2004) has implored other

³ The two "brothers" had different last names but shared the same address; there was only one 35-year-old female victim and one 39-year-old male victim; the 63 year-old man had no exact matches because the ME did not record a 63-year-old dying in NL, but he did match a 64-year-old. This is the only case that is only a "likely" match to the pseudonym.

Table 1. Medical Examiner Records for the 16 Heat-related July 1995 Deaths in North Lawndale Reported by Dumeier

Last Name	First Name	Age (years)	Date of Death in 1995 (month-day)	Zip Code	Recorded Cause of Death
"Jackson"	"Walter"	39	07-15	60623	ASCVD, Pt. II. Heat Stress
"Williams"	"Eddie"	59	07-15	60623	ASCVD, Pt. II. Heat Stress
Lowery	Robbie	79	07-15	60623	ASCVD, Pt. II. Heat Stress
"Wilson"	"Grover"	83	07-15	60623	ASCVD, Pt. II. Heat Stress
Mauldin	Jimmy	77	07-15	60623	ASCVD, Pt. II. Heat Stress
Colemon	Ricky	47	07-15	60623	ASCVD, Pt. II. Heat Stress
Moore	Anna	78	07-15	60623	ASCVD, Pt. II. Heat Stress
Johnson	Ora	92	07-16	60624	ASCVD, Pt. II. Heat Stress
"Douglas"	"James"	67	07-16	60623	ASCVD, Pt. II. Heat Stress
"Thomas"	"Letitia"	35	07-17	60623	Heat Stroke
Washington	Booker	71	07-17	60623	HCUD Pt. II. Heat Stress
"Franklin"	"James"	43	07-24	60623	Heat Stroke Pt. Ii. Chronic Narcotism, Septicemia
Coburn	Elnora	86	07-15	60623	HCVD, Pt. II. Heat Stress
Jones	Eddie	80	07-15	60623	ASCVD, Pt. II. Heat Stress
"Rose" ^a	"Willie"	64	07-15	60623	HCVD, Pt. II. Heat Stress
Cowans	Beatrice	75	07-18	60623	ASCUD, Pt. II. Heat Stress

Source: Cook County Medical Examiners Office Records. Heat-Related Deaths. July 1995.

Note: Names in quotation marks are the pseudonyms employed by Dumeier (2006). ASCVD = Atherosclerotic Cardiovascular Disease; HCUD = home care until death; HCVD = history of cardiovascular disease; ASCUD = Atherosclerotic cardiovascular disease. Pt. II = Type 2 Diabetes.

^a "Willie Rose" is the only pseudonym whose identity could not be confirmed, because there is no 63-year-old man listed in medical examiner's records for North Lawndale. This case is used because it is the only 64-year-old man on the list.

ethnographers to be cautious about using stereotypes that spoil their subjects' reputations, here Duneier relies on both hearsay and decade-old recollections as evidence that people who (obviously) cannot represent themselves died because of drug use and alcoholism. Where is the cautionary note explaining that the "informed informants" had been publicly blamed for the deaths of their neighbors and relatives, and so we should regard their accounts with some skepticism? Do sociologists' ethical obligations to their subjects and their subjects' reputations end after those subjects die?

In the case of the heat wave, the public consequences of Duneier's purported substance abuse findings are particularly serious. As HW demonstrates, leading Chicago political officials and media outlets immediately offered explanations for the disaster that explicitly blamed the victims and their families for irresponsible behavior that caused the crisis. Mayor Daley publicly blamed families and neighbors of the victims for failing to care for their relatives and friends. "Go over there, see your mother or father or aunt," he instructed. According to one aide, Daley "kept saying things like 'You've got to be watching your neighbor. That's the problem here. You've got to take care of them.' So he was spreading the blame in a sense, or spreading the responsibility" (Klinenberg 2002:175–6). The *Chicago Tribune* offered another reason for the deaths. The largest and boldest frontpage headline in the July 18, 1995 edition reported "Casualties of Heat Just Like Most of Us: *Many Rejected Any Kind of Help.*" Daniel Alvarez, the Commissioner of Human Services, also blamed the victims for their own demise. "We're talking about people who die because they neglect themselves," he declared. (Klinenberg 2002:213, 172, emphasis added).

Under pressure from citizen groups, Alvarez ultimately apologized for his remarks. Yet Duneier's (2006) findings, which are sure to capture the attention of journalists and Chicago officials, suggest that Alvarez should take it back. For if at least half of the people in NL died because of their alcoholism and drug use, does such individual-level behavior also explain why, across the city, African American neighborhoods were disproportionately affected? Surely Duneier's "social facts" suggest that this is a plausible hypothesis, that the victims neglected themselves. Duneier speculates that heat

deaths from substance abuse spanned the city, and encourages a search for hidden addicts. "I wonder," he writes, "whether some of the white decedents might have had alcohol problems or complications from drugs similar to those I discovered in many NL deaths. The white community may have simply been more tight lipped about this" (Duneier 2006:690). One can only imagine the follow-up research plan.

In the Epilogue for HW, where I describe the potters grave and beggars funeral used in the burial of 41 unclaimed disaster victims, I argue that the aftermath of the great Chicago catastrophe helped to illustrate the enduring legacy of inequality, which all Americans carry to the grave. "Far from being the great equalizer, the deaths of the Chicagoans for whom no one came only reinforced and perpetuated the degradation of their lives" (Klinenberg 2002:237). I cannot find better words to describe Duneier's account of how the residents of NL contributed to their own demise. He has produced the kind of epitaph that feeds perfectly into what Herbert Gans calls the "war against the poor"—a fitting new chapter of heat wave history (Gans 1995). Perhaps, now that Duneier has shown the lengths he will go to challenge sociological research that challenges his own beliefs, he will let the issue rest in peace.

DISASTER SOCIOLOGY IN AN AGE OF EXTREMES

Yet this is hardly the time to give up on the sociology of disaster. With hurricane Katrina, the 2003 tsunami, and September 11 terrorist attacks behind us, and the threats of global warming, flu pandemics, and bioterrorism ahead, refining social scientific methods and theories for studying extreme events has become an urgent task. Duneier's (2004; 2006) two published articles about the Chicago heat wave establish a program for the sociology of disaster that government agencies looking for fast, easy, and politically convenient findings are likely to find attractive.

I share Duneier's belief that it is useful to identify individual-level risk factors in disasters, and that is why every chapter of HW addresses what actual people were doing during the crisis as well. I also recognize, however, that producing comprehensive and reliable knowledge about what happened to large groups of

individuals during catastrophes is enormously challenging, particularly for a lone ethnographer doing research many years after the event. Producing responsible ethnographic research requires knowing what questions fieldwork is and is not well-suited to answer. That is why I relied on the large-scale case control study that the CDC conducted immediately after the heat wave, and then focused my ethnographic energy on understanding the broader, neighborhood-level conditions that establish the social context, and in turn contribute distinctively sociological knowledge to puzzles that interest scientists from a number of fields.

I believe that the research method Duneier employs here is both scientifically flawed and dangerous for public policy, because it could discourage attention from structural conditions that make some people and places more vulnerable than others. I worry, for example, about what would result if an individual-level study that treated hearsay about deviant behavior as fact were conducted with Katrina victims in New Orleans, where numerous political officials and journalists speculated that many people stayed behind because they were alcoholics and drug addicts.

My concern is rooted in the history of disaster research. Scholars who study catastrophes have demonstrated that quick-hit studies designed to assess the impact of individual-level behaviors rather than economic or political conditions have long played a role in naturalizing disasters and obscuring the structural sources that make some people and places more vulnerable than others (Davis 2002; Steinberg 2000). That is why so many contemporary social scientists take inspiration from the economist Amartya Sen, who famously established that people die in famines because of political economic institutions that limit the distribution of food, not because of individual-level conditions; and from the medical anthropologist Paul Farmer, who showed that poor people are more likely to die from infectious diseases, such as HIV/AIDS and tuberculosis, because of political decisions not to invest in public health projects and economic development programs, not because they, as individuals, act irresponsibly (Farmer 1999; Sen 1981). Recognizing the contributions of their scholarship can help prevent sociologists from inadvertently engaging in the perilous practice of

overstating the agency of individual victims during social disasters.

I designed the social autopsy for *Heat Wave* by integrating the sociological project of explaining health outcomes that Durkheim established in *Suicide* (1951), the approach to contemporary disaster scholarship that runs from Sen and Kai Erikson (1976) to Farmer (1999), the classic Chicago School techniques for the firsthand observation of cities, and the critical attention to symbolic power and social violence encouraged by Pierre Bourdieu (1992). The assemblage of these disparate scholarly traditions yields powerful analytic and methodological tools for the study of disasters and other extreme events. The research process in which I put them to use requires carefully developing sociological concepts and spending more than a few afternoons in the field, but the results are worth the effort.

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