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Journalist Ethics ≠ Social Scientist Ethics

Why does society have limited ethical expectations for journalists when ethical constraints on social scientists are so strict? Why is it acceptable for a newspaper to name persons charged with a criminal offense without their permission when it is not acceptable for a social scientist to identify by name people observed in public places? For a social scientist to do so would be a serious breach of ethical responsibility (Kirsch, 2009).

What distinguishes autobiography from autoethnography or ethnography and journalism from oral history is not inherent to the methodology (i.e., the method of data collection) but in the historical basis of their ethical mandates—the First Amendment versus the U.S. federal regulations of human research. Autoethnography, ethnography, and oral history are academic disciplines and, as such, are situated within an academic institution and bound by the institution’s responsibility as discussed in Chapter 1, while journalism is not. Understanding this boundary is essential to planning ethically responsible research. Here is an example.

The photograph of beachgoers (see below) appeared on the front page of the *New Zealand Herald* on February 6, 2001. It depicts a popular Auckland inner-city beach on February 5, a day prior to a national public holiday. February 5 was a Monday and a normal working day. A *New Zealand Herald* photographer shot the picture, and two journalists questioned beachgoers about why they were at the beach and not at their place of employment. The photo caption reads: “Mondayitis: Mission Bay became a refuge for leave-takers and sickie-throwers who swung a four-day weekend yesterday.”

The journalists assume that workers should be at their places of employment, and if they are not, the public has a right to know about this delinquent behavior. The

journalists interviewed and identified six people on the beach and found only one person, Person #1, allegedly not at work. The other five beachgoers accounted for their presence on the beach. Orsman and Clarke (2001) listed the people's name, occupations, ages, and alibis. They were.

1. Anonymous. "You guys can't take my picture. I know my rights."
2. Amanda Hanrahan, 29, just arrived home after traveling overseas for two and a half years. "I am looking for a job, but it's sunny today."
3. Sharon Ramsay, 31, legal secretary for law firm Rudd Watts and Stone, took a day's leave. "Quite a lot of people in the office took the day off."
4. Richard Rigg, 31, builder, self-employed, decided to take the day off.
5. Nigel Watson, 34, "I'm between jobs right now. I finished my old job last week and I'm starting a new one next week."
6. Roger Schueck was visiting from Switzerland, where it was 35°F.

The person identified as #1 is declared to be anonymous. His declaration or plea becomes the story. As if to out the anonymous person, the newspaper reports his declaration, "You guys can't use my picture. I know my rights." Yet, the *New Zealand Herald* voided those "rights"; his protests were not recognized above the promise of a good story exposing, supposedly, an important public issue.

The rights of this anonymous man provide an excellent starting point to begin a discussion of social scientist research ethics and journalist ethics. The use of Person #1's photograph and his verbal protests—"You guys can't take my picture. I know my rights"—highlights a disjuncture between ethical practice in human

research and the everyday practice of journalists that all researchers must understand.

What rights do these six people have? Obviously, they are beachgoers and deemed to be in a public place, a beach. For these journalists and their editors, the beachgoers have no rights to privacy. Ethical human researchers share this perspective—up to a point. Most social scientists would have no qualms about observing these beachgoers' social interactions. The assumption ethical social scientists make is they may observe any person in public without informed consent

New Zealand Herald—Tuesday, February 6, 2001,
by Bernard Orsman and Josie Clarke



if they *aggregate* their field notes, meaning that the field notes contain no personal or identifiable information. Aggregation minimizes harm. However, any use of photographs to collect data makes the persons identifiable, thus the aggregate caveat does not automatically apply when photographs are taken for the purpose of publication. In most human research, it is useful to characterize the use of a camera as adding ten pounds of ethics.

Photographs complicate human research as they automatically breach confidentiality: The image makes persons recognizable. Person #1 is not anonymous; his coworkers or friends will clearly identify him by his age, gender, ethnicity, and the date on which the photo was taken. In addition, the potential harm to this person if he is taking a day off work is real, not imagined. For a social scientist, this photograph represents potential harm to the subject.

Journalists operate under different rules of engagement. They are free to approach the six persons, interviewing them individually, requiring them to account for their presence at the beach. For journalists, taking a day's paid leave is a valid news story. Thus, in this example, the six beachgoers identified by name and age apparently have few rights to privacy or the right to refuse to participate in the journalists' data collection.

This data collection may be a legitimate exercise for journalists; however, researchers would be required to conduct themselves differently in order to protect the person supplying the information. At the very least, after introducing oneself to the potential informant and explaining the nature of the research—in this case, a study of leisure during the working week—the researcher would ask whether the person wanted to take part in the research. This represents the first part of informed consent.

Informed consent is a two-part process. After the researcher provides information about the project, the second part has the person say yes or no or seek further information. It is clear from Person #1's response that the journalist did not have permission to use his information or image in any newspaper reporting.

An ethically responsible researcher would have taken the person's protest seriously, as normally any research subject may withdraw from the study at any time. The fact that the journalist and the editor of the newspaper did not take the man's protest seriously, choosing to place the photograph on the front page, highlights a demarcation between research and journalistic ethics.

Why does society accept different ethical considerations? What are the origins that frame the boundary between journalist and academic research ethics?

FUNDAMENTAL DIFFERENCES: ORIGINS AND OBLIGATIONS

Journalist and human research ethics have different starting points with separate obligations. A journalist's ethical considerations originate as free speech found in the First Amendment of the United States Constitution:

Congress shall make no law respecting an establishment of religion, or prohibiting the free exercise thereof; or abridging the freedom of speech, or of the press; or the right of the people peaceably to assemble, and to petition the Government for a redress of grievances.

Researchers do not have these same rights—end of story.

Rather than having rights, constraints frame researchers' ethical considerations. Researchers do not derive their ethics from a legal mandate. Their initial source for ethics is the medical scandal at Nuremberg. Following World War II, individual doctors were put on trial, but so too was science itself. The outcome for science was a reprieve, a plea bargain of sorts recognizing science's potential as a public good but only on the proviso that all research subjects must consent to take part in any research.

The voluntary consent of the human subject is absolutely essential. The Nuremberg Code of Ethics, later revised in the 1964 Helsinki Declaration, stipulated a second constraint: that an ethics committee must review all research involving human subjects, meaning that ethical considerations in research became the shared responsibility of both the institution and the researcher. The obligations of journalists and researchers in terms of who they primarily serve further widen the boundary lines between journalists and researchers. A researcher who teaches journalism in a university setting justifies the boundary based on primary obligations: "The journalist's first obligation is to the public and the public's right to know and is not to the person or persons interviewed" (Dash, 2007, p. 873).

A researcher's first obligation is not to the public's right to know or to serve the community. Their obligation is to the individual who volunteers to be part of the research. This obligation is to do no harm to research subjects.

Social scientists have often studied poor and marginalized individuals [and have] fostered a tendency to view all research participants as vulnerable, lacking power, and needing protection. In contrast, journalists often interview and question individuals drawn from more powerful segments of society. . . . This distinction is obviously more true in the institutional imaginary than in practice, but it helps explain why the social sciences have been more willing to embrace an image of their research participants as being more vulnerable and fragile than a journalist's sources—even when these sources may be the exact same individuals. (Haggerty, 2004)

In journalism education, even within the university, students are taught to recognize the existence of distinct ethical boundaries and that journalists within academia serve different masters.

Journalists teaching journalism to university students emphasize the public service aspect of the craft and impress upon students that their first

obligation is to the public, just as practicing physicians' first obligation is to their patient. This concept is essential to journalistic ethics. (Gunsalus, Bruner, Burbules, Dash, Finkin, et al., 2007)

Funding also sustains the ethical boundary. Journalism's commercial interest makes it different from research, where institutions, even private universities, are supported at least in part by the public purse.

Journalism is not supported by public funds. Because we are not journalists, however many similarities one can identify between ethnographic research and investigative journalism we have no reason to expect the same rules to apply. (Bosk & DeVries, 2004)

The lessons learned from any comparison between journalism and research ethics is that the same rules do not apply. Journalists' ethics originate from law, not moral indignation. They hold different allegiances (the community vs. the individual research subject), and they have different commercial perspectives. A journalist's goal is to write in the public interest, whereas researchers seek to protect those vulnerable persons drawn into any research project from harm. A researcher's primary responsibility is not to free speech but to enhance a subject's autonomy. The notion of informed consent inscribed by the Nuremberg Code (1949) states that any person taking part in a research project must do so voluntarily, informed about the nature of the projects, and they must explicitly consent. Additionally, the research subject is free to leave the research project at any time.

WHEN SOCIAL SCIENTISTS CROSS THE LINE

The next section discusses three researchers, Jared Diamond, Sudhir Venkatesh, and Mitchell Duneier, who have recently crossed the ethical boundary, writing as social scientists in a journalist genre. Diamond and Venkatesh fall afoul of human research ethical considerations in the shift from social science research with humans to journalism as the rules of engagement change. Duneier, on the other hand, manages to cross the boundary responsibly. An examination of these cases illustrates the pitfalls and ethics that social scientists need to heed when writing in a journalist's genre.

JARED DIAMOND

Jared Diamond (2008), Professor of Physiology and Geography at the University of California, Los Angeles, crossed the research–journalism ethics boundary when he presented a travelogue under the banner of the *Annals of Anthropology*¹ for the

¹Find a full version of Diamond's article at worldaffairsboard.com.

(nonacademic) *New Yorker* magazine. The *New Yorker* had no problem with what he wrote or how he wrote it, yet ironically, the journalist practice of naming sources (as opposed to using pseudonyms to protect the vulnerable) has exposed Diamond's story to intense criticism from within the academic anthropology community. Additionally, Diamond's article has led the two persons named in the *New Yorker* article to file a \$10 million-dollar suit for defamation (Glenn, 2009). In response, the *New Yorker* stated that it stands by the story but has withdrawn the offending article from their online archive. What follows is Stuart Kirsch's (2009) succinct summary of the *New Yorker* article.

In April 2008, an essay by Pulitzer Prize-winning author Jared Diamond appeared in the *New Yorker's* annual travel issue. His article "Vengeance Is Ours" contrasts the decisions of two individuals concerning acts of violence: Diamond's father-in-law, who was a soldier in the Soviet army in Poland at the end of World War II, and a man from the Highlands of Papua New Guinea whose tribe was engaged in an on-going battle with the neighbouring group. Having come face to face with the man accused of murdering his mother, sister and niece, Diamond's father-in-law struggled with a moral dilemma: should he kill the man or turn him over to the local authorities for what would undoubtedly be a lenient punishment? After agonizing over his decision, the soldier turned the alleged perpetrator over to police custody, where he spent one year in prison before his release. For the rest of his life, Diamond's father-in-law remained wracked with guilt and remorse for having forgone the opportunity to avenge the deaths of his family members.

Diamond's Papua New Guinean informant told him a very different story. His uncle had been killed in a tribal fight and the responsibility for avenging the death fell to him. He organized a raiding party that attacked their enemy. Someone shot an arrow which struck and paralysed a member of the opposing tribe. Diamond's informant, who organized the attack, was treated like a hero. He felt as though an enormous weight had been lifted from his shoulders. Moreover, he never hesitated. He never questioned his responsibility to exact vengeance, even though it entailed some personal risk for himself. Diamond treats the two cases as indicative of the transformations wrought by the emergence of the state, which claims a monopoly over the legitimate use of violence.

Jared Diamond's *New Yorker* article straddles the research and journalism genres, blurring the disciplinary boundaries. Rhonda Shearer, a media critic whose website www.stinkyjournalism.org, initiated the first investigation into the ethics of

Diamond's *New Yorker* article, leading the discipline's blogger sphere (i.e., Savage Minds) to reach the consensus that Diamond's primary obligation should not have been to his intriguing and compelling story² but to the individuals named therein. Diamond, although writing under the banner of the *Annals of Anthropology* was practicing established journalist ethics. Diamond identified the avenger, the organizer of the vengeance, as Daniel Wemp, a Papua New Guinean highlander, and the victim as Henep Isum. In naming these two men, Diamond provides his story with both the credibility and authority (Atkinson, 1990) that are the hallmarks of journalism.

To the anthropological discipline, the act of naming his informants appears to be the most serious ethical breach (Kirsch, 2009). A lesser, but still significant, breach was his naming of the tribes involved (Balter, 2009). Diamond also failed to follow anthropological protocol and gain his informant's informed consent. Fluehr-Lobban (2009) claims that Diamond engaged in professional code-switching; metaphorically, she writes, Diamond was neither the fish of the anthropologist nor the fowl of the journalist in his dealings with Daniel Wemp.

Diamond's article described Daniel Wemp's vengeance as motivated by retribution for the theft of some pigs from his tribe, which had led to the death of Wemp's uncle. The attack on the other tribe resulted in Henep Isum, the victim, being paralyzed by an arrow. By naming the two tribesmen, Diamond's journalist persona created an ironic ethical quagmire for his academic persona, allowing others to fact-check the article after publication, discovering that, not only basic ethical considerations had not been followed (i.e., informed consent and leaving his vulnerable persons susceptible to retribution), but some of the facts of the story were questioned.

It is true that Wemp did know Diamond. In 2001–2002, Wemp worked for the World Wildlife Fund and was employed on occasion to act as Diamond's driver. On trips to and from the airport, Wemp took part in conversations with Diamond. Yet, as Fluehr-Lobban (2009) notes, Wemp was not a stranger, research participant, or informant engaged by Diamond. He was in his service. It was during these car rides that Diamond heard Wemp's stories of vengeance. At no time did Diamond inform Wemp he was writing an article, nor was Wemp given the opportunity to consent for his story to be used in the *New Yorker* article. Thus, there seem to be two ethical concerns here: (1) identifying people by name and (2) failure to gain informed consent.

[T]his sensitive story may have been justified for its insights into individual and collective vengeance in human conflict. However, [Diamond's] research and subsequent publication in a popular magazine would have

²Kirsch (2009) does not think the comparative cases are compelling, providing a number of instances where the families of Papua New Guinea men were murdered and their kin have not sought retribution but peaceful reconciliation.

been more ethically sound, as well as more scientifically interesting, had he practised the basis of informed consent in ethnographic research. (Fluehr-Lobban, 2009)

The facts of Diamond's *New Yorker* article are disputed. For example, the victim, Isum, is said to be not paralyzed or wheelchair bound and has been photographed by Shearer standing upright. Moreover, the anthropologist Pauline Wiessner (Balter, 2009, p. 872), a leading expert on tribal warfare in Papua New Guinea, thinks Diamond was naïve if he accepted Wemp's stories at face value because young men in Papua New Guinea often exaggerate their tribal warfare exploits or make them up entirely. "I could have told him immediately that it was a tall tale, an embellished story. I hear lots of them but don't publish them because they are not true."

The most questionable aspect of the story was the First World wheelchair in the Third World rural highlands. Shearer corroborates this intuition:

Shearer says that after reading Diamond's article . . . she immediately was "very skeptical" at the suggestion that Mandingo [Isum] could have continued to live in the remote, rugged Papua New Guinea Highlands while confined to a wheelchair and perhaps needing special medical care. (Balter, 2009, p. 873)

Balter (2009, p. 873), writing in *Science*, identifies not two but three world's colliding in Diamond's article.

First is the world of science, specifically anthropology, which uses fieldwork and scientific methodology to study human cultures. Next is the craft of journalism, with its own set of ethics and practices aimed at reaching the general public. Finally, there is Papua New Guinea, a young nation still struggling to integrate many hundreds of tribes and clans into a modern state.

In Diamond's defense, both he and his *New Yorker* editor David Remnick insist that these anthropological criticisms are irrelevant because Diamond was working as a journalist for a popular magazine, not as an anthropologist writing a scholarly article. Diamond says he did not find out about the *Annals of Anthropology* byline until shortly before publication, and he, now regrets it. Moreover, Balter (2009, p. 873) reports Diamond's account, "Everyone knows that the *New Yorker* is not a scientific publication; it's journalism. . . . In journalism, you do name names so that people can check out what you write."

New Yorker editor Remnick agrees with Diamond's journalism–science distinction: "Journalistic practice differs from scientific practice in a number of ways, and this seems to be one of them. Using real names is the default practice in journalism" (Balter, 2009 p. 873). The anthropological discipline does not see this issue in such

clear-cut boundary terms, believing Diamond blurred boundaries, producing widespread harm to people and to the discipline.

Harm can take various forms. Obviously, there is the alleged harm to the persons named in the lawsuit. There is also harm to the researcher, that is, Diamond's reputation. But, Shearer and Balter go one step further, pinpointing the potential harm to the discipline of anthropology at a time when others, namely Human Terrain and Chagnon's study of the Yanomami (Wynn, 2011), have subjected anthropological ethics to criticism for the first time since the aborted Project Camelot in the 1960s. Cultural anthropologist Alex Golub concurs: A story like this "affects our discipline's brand management. . . . It's important for people to know that if they meet an anthropologist, they are not going to be written up in *The New Yorker* without being told about it" (Balter, 2009, p.873).

To underline the lesson that disciplines vary in their fundamental purposes and correspondingly in their ethics, we present a poignant vignette about human rights research. In the following, an ethical conundrum where decisions to publish acutely affect people's lives, Dinah PoKempner, General Counsel of Human Rights Watch, provides her account of human rights research (see Box 5.1) conducted in a war zone.

Box 5.1 Human Rights Dinah PoKempner

Human rights researchers, particularly in times of war, often tread the same path as humanitarian workers and journalists. Their ethical orientation derives from both, yet is distinctive. Journalists hold central the public's right to know, and the value of exposing truth as a necessary element of democratic self-governance. Humanitarian workers tend to place front and center the life and security of the person, or of a community. Human rights workers have to juggle these two fundamental objectives continuously, exposing the truth about human rights violations to create pressure for their abatement, while at the same time protecting the victims of those violations, who have already suffered so much.

The documentation of torture is a scenario where these two values often contend. United Nations bodies, the International Committee of the Red Cross and human rights groups have developed protocols for protecting the integrity of such investigations in custodial situations. Two standard requirements are the right of the investigator to visit any area of any prison at any time, and the right of the investigator to speak to any inmate in privacy in a place in the facility of his or her choosing. It is usually also advised that the investigator try to interview a large enough pool of detainees (and sometimes a variety of

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places of detention) to further assure the confidentiality of any one interview, and protect inmates from reprisal.

But, conditions of access are often far from this ideal.

One of my most wrenching investigations took place early in the Balkans conflict (1991–1995), near the front lines of fighting between Serbian and Croatian forces. A Croatian militia had captured some Serbian fighters and held them at an encampment in what seemed to be a school near the Slovenian border. My translator and I presented ourselves as researching human rights in conflict (absolutely true) and asked to meet the Serbian prisoners, whom the local commander described dramatically as dangerous war criminals.

I was expecting to see hardened fighters, but instead we met two emaciated boys hardly old enough to have facial hair, bundled and huddling in dirty coats. The guard, echoing the narrative of “extremely dangerous war criminals,” insisted on staying at our sides, but we finally persuaded him that we would interview each one separately in a classroom and yell loudly if we felt we needed him to come in from the hall to protect us. Once the guard was out of sight, we quickly went to work, positioning each youth far away from the door, moving our chairs close, and speaking in whispers. Each boy could barely speak for trembling in fear; each had been viciously abused, purplish bruises evident on their faces. But before we could interview other detainees to provide a modicum of “cover,” our time ran out; the dull thuds of shelling began, and we had to leave for our own safety. There was no way to publicize what we found without endangering the youth.

This, more than the shelling or the race toward Zagreb at top speeds through deserted highways, put us in fear and trembling. The next morning, I went to the local office of the International Committee of the Red Cross, begging the representative to visit the encampment, if it was still there. I don’t know to this day the result or the fate of those two.

VENKATESH VERSUS DUNEIER: A TALE OF TWO INNER-CITY RESEARCHERS

In what follows, we contrast two remarkably similar junior ethnographers who explore the informal economies of inner-city black men in Chicago and New York cities, respectively. Both studies had limited input from formal IRB review, yet the level of ethical responsibility practiced by both is so dissimilar as to be instructive.

Venkatesh's (2008) *Gang Leader for a Day* and Duneier's *Sidewalk* should be read by all researchers as how research is conducted with and without implementing ethical responsibility, that is, thinking in advance on how to protect those who are brought into the study. Venkatesh's memoir is a candid description of a study without much ethical consideration. Duneier's *Sidewalk* comprises painstakingly planned ethical responsibility.

Gang Leader for a Day

In 1988, Sudhir Venkatesh was student in his first semester of graduate school at the University of Chicago when he began his project studying inner-city poverty. He turned up one night in a South Chicago high-rise and was confronted at knifepoint before being incarcerated in a urine-soaked stairwell by members of the Black Knights gang. Venkatesh was only released when the local drug lord named J. T. listened to his research plans. From this inauspicious start and with Venkatesh's life at risk, he began his eight-year study, placing those he met at risk. Venkatesh tells his own story.

His name was J. T. and while I couldn't have known it at the moment, he was about to become the most formidable person in my life, for a long time to come. (Venkatesh, 2008, p. 15)

The gang leader, J. T., took from this first meeting with Venkatesh an understanding that Venkatesh would write a book giving voice to J. T.'s life and times. Yet, Venkatesh had misrepresented himself: He was neither writing a book nor a dissertation. At the time, Venkatesh was a first-year graduate student completing course work, four years away from assembling a dissertation committee and seven years away from becoming a sociologist with a PhD.

In this first prolonged and nervous encounter in the stairwell of the slum high-rise, J. T. saved Venkatesh from a savage beating, and Venkatesh never challenged J. T.'s assumption that Venkatesh was writing a biography about the life and times of J. T. "I realized that I never formally asked JT about gaining access to his life and work" (Venkatesh, 2008, p. 35).

J. T.'s patronage opened the door to a world that Venkatesh had never experienced before, and via this patronage he was welcomed like family into J. T.'s many girlfriends' houses with their children. J. T.'s mother, Mrs. Mae, treated Venkatesh like a son.

Mrs. Mae, like all the other persons Venkatesh met under J. T.'s patronage, was fodder for a dissertation that was four years from beginning and seven years from completion. Venkatesh quoted all he met verbatim with no attempt to inform them about his research or to seek consent. Yet, informed consent is a two-way street. It is first incumbent

on the researcher to know what to inform the subject about. For Venkatesh (2008, p. 67), this was impossible. “He [J. T.] had no real sense of what I would actually be writing—because in truth, I didn’t know myself” (Venkatesh, 2008, p. 67).

Over the next eight years, Venkatesh became a familiar face in the high-rise, facilitating interviews with non-Black Knight gang high-rise residents, including his second powerful gatekeeper, Mrs. Bailey, the high-rise manager. She too also vouched for his safety and legitimated his access to other research subjects.

As I spent more time with Ms. Bailey over the coming months, I found that most tenants were less suspicious of me than they’d been in the past. Sometimes, when a tenant came into Ms. Bailey’s office to talk about a problem, the tenant would say, “It’s okay, I don’t mind if Sudhir listens.” Like J. T., Ms. Bailey seemed to enjoy the fact that I was interested in her. Perhaps she, too, thought I was going to be her personal biographer. I could see why she might make that assumption. I took every opportunity to express my fascination for her life, which seemed more fascinating the more I hung around. (Venkatesh, 2008, p. 165)

The Ethics of Triangulation

Venkatesh eased himself into these subjects’ lives, and his study developed from being about poverty to being a study of the informal economy of the high-rise—an account of how the tenants earned money—by babysitting, prostitution, fixing cars, and so on. Much of this paid work went on under the radar of both the Internal Revenue Service (IRS) and the two high-rise power brokers, J. T. and Mrs. Bailey, who brokered his access to this informal economy. When Venkatesh sought to triangulate his data with his two gatekeepers, he admits not fully believing what the tenants had told him. As a result, the tenants’ interviews are commodified: Tenants’ secrets are exchanged for gatekeepers’ verification. Venkatesh’s (2008, pp. 200–201) narrative tells his story:

“Hey, you know what, I could actually use the chance to tell you [J. T. and Mrs. Bailey] what I’ve been finding,” I said, taking out my notebooks. “I’ve been meeting so many people, and I can’t be sure whether they’re telling me the truth about how much they earn. I suppose I want to know whether I’m really understanding what it’s like to hustle around here.” . . . For the next three hours, I went through my notebooks and told them what I’d learned about dozens of hustlers, male and female. There was Bird, the guy who sold license plates, social security cards, and small appliances out of his van. Doritha the tax preparer. Candy, one of the only female carpenters in the

neighborhood. Prince, the man who could pirate gas and electricity for your apartment. J. T. and Mrs. Bailey rarely seemed surprised, although every now and then one of them perked up when I mentioned a particularly enterprising hustler or a woman who had recently started taking in boarders.

I finally left, riding the bus home to my apartment. I was grateful for having had the opportunity to discuss my findings with two of the neighborhood's most formidable power brokers.

The information he divulged about the tenants' employment had been given to him freely because he was a trusted person under the patronage of the two power brokers. There was no informed consent, no confidentiality, no respect for persons, only his ability to harm the most vulnerable of vulnerable people.

The response from his informants was immediate; he was shunned by those who had shared their stories of the informal economy. One tenant candidly warned Venkatesh (2008, p. 206) of possible retribution.

Clarisse sensed my anxiety. . . . "Don't worry, little baby! You probably never had an ass whuppin,' have you? Well sometimes that helps clear the air. Just don't take the stairs when you leave, 'cause if you get caught there, they may never find your body."

PERR2 is not the first to critique Venkatesh's book (see Clampet-Lundquist, 2009; Charles, 2009; Young, 2009). Tierney (2010) casts Venkatesh as a first-rate writer but found his ethics questionable. Jeremy Freese's (2008) blog ridicules Venkatesh's methodological naiveté. His three points ask:

1. If you are going to test a survey in an obviously dangerous public housing project, do not have your first question be arguably the worst survey question I have ever seen. [How does it feel to be black and poor?]
2. If you hear a person planning a drive-by shooting of another person, you are under a legal—as well as, some of us might add, ethical and moral—obligation to do something about it.
3. If you are told information by interviewees, you should not divulge that information to criminals you know already are extorting money from some of the interviewees.

Venkatesh experienced ethical problems at each stage of his study. Getting in the front door on day one, he misrepresented his credentials. Getting along, he failed to respect persons' confidentiality and practice the principle of doing no harm. Getting out provided mixed feelings of success and guilt.

I still feel guilty about all those years that I let J. T. think I would write his biography. I hope that he at least reads these pages someday. While a lot of it is my story, it plainly could never have happened without him. He let me in to a new world with a level of trust I had no reason to expect; I can only hope that this book faithfully presents his life and his work. (Venkatesh, 2008, p. 290)

Venkatesh's eight-year study of informal economies in a high-rise apartment building in a predominately African-American area of south Chicago, *Gang Leader for a Day*, is essential reading for any social scientist wanting to learn about planning ethically responsible research. The book must be read as a treatise on how not to conduct ethically responsible research. If ever there was an ironclad justification for IRBs reviewing social science research, this book provides it.

There is poetic irony in the subject matter of this book—poverty. The most impoverished aspect of this book, notwithstanding that 96% of the people in that high-rise apartment were on welfare, is Venkatesh's ethics. There is honor, maybe even virtue, among thieves—more honor than Venkatesh showed in the eight years he spent with these people. “He [J. T.] loved the idea that I might be writing his biography. But in general everyone respected my privacy and let me do my work” (Venkatesh, 2008, p. 52).

Sidewalk

Duneier (1999) won the prestigious C. Wright Mills Award in Sociology for *Sidewalk*, an ethnographic narrative account about the lives of (mostly) black men who were involved in an informal street economy, selling books and magazines that they had recycled from Dumpster dives in central New York City. Although the subject matter, the researcher's social distance from the subjects, and the methodology are identical to that of Venkatesh, the ways in which Duneier created and sustained relationships with his subjects were polar opposites of Venkatesh. Our goal here is not to document Duneier's book but rather to introduce *Sidewalk* sufficiently to induce the reader to read and contrast both books.

Duneier wrote two books. The first was his dissertation titled *Slim's Table*. The weakness of this ethnography was that it focused on one bookseller's table. The subsequent book *Sidewalk* retains the same subject matter but broadens the focus beyond one bookseller to both examine the other book and magazine sellers who took part in the informal economy as well as the persons who bought these recycled articles.

IRB approval is a red herring in Duneier's study. For the research that produced *Slim's Table*, he sought no IRB approval as his advisor Edward Shils said it was not

necessary. For *Sidewalk*, Duneier gained IRB approval from both of his teaching institutions (University of California, Santa Barbara, and New York University). However, the approval, as Duneier admits, had little ethical consequence for a “diagnostic ethnography” that identifies ethical issues as they arise in the field. Duneier’s study differed from Venkatesh’s in four core areas: interaction with key informants, informed consent, the use of pseudonyms, and the fact that Duneier thought deeply about how he respected his subjects while advocating for social change.

Interaction with key informants. Hakim was the equivalent to Venkatesh’s J. T. Once the relationship was established, Hakim introduced Duneier to the community and legitimated the presence of a white, middle-class, Jewish, academic researcher on the street. Moreover, it was Hakim’s critique of Duneier’s first book *Slim’s Table*, written about Hakim, that led to the more expansive mix of a micro- and macro-analysis of the street vendors and their customers. Duneier utilized Hakim as the key informant by arranging for him to be paid to coteach a course at the University of California, Santa Barbara, for a 10-week quarter. The relationship with Hakim was honest and authentic.

Informed consent. Duneier’s informed consent was an example of process consent, which is described in Chapter 7. The consent for getting in and getting along was elaborate, time-consuming, and generous. Before the book was published, Duneier rented a hotel room and read relevant excerpts of the book to each of the subjects. In the case of some of the subjects, this took upwards of eight hours. All of the subjects consented, though it is instructive to note that most subjects were more interested in how they appeared in the photographs featured in the book than in what Duneier had written. One subject became drunk during the reading, leading Duneier to suspend the consent until a later time. Duneier’s relationship with his subjects was genuine. He shared the book’s royalties with the subjects, but he did not tell them that he would do this until after he gained their consent. Duneier regarded it as coercive to disclose this payment before the consent was received.

The use of pseudonyms. Duneier did not use pseudonyms, opting to gain the subjects’ consent to use their real names in the book. Similar to Diamond, Duneier (1999, p. 348) believed that disclosing the real names increased the accountability not only for the author but for the reader. In his expansive methodological appendix, Duneier discloses that everyone named in the book gave their permission for their accounts to be identifiable.

The *PERR2* authors find relief, even joy, in Duneier’s seamless practice of ethical research, which embodies the principles of the Nuremberg Code. We urge any person planning ethically responsible research to read these two books, comparing and contrasting the ethical issues used (or not used) by Duneier and Venkatesh.

REFLECTIVE QUESTIONS

1. When considering your social networking settings, do your privacy settings ensure confidentiality and anonymity, or do they reflect pseudonymity?
2. Consider how a researcher and a journalist wanting to interview people in a public place would interact differently with those present. How and why are their ethical considerations different?
3. If a journalist's primary responsibility is to the public and their right to know, what is a researcher's primary ethical obligation?
4. Compare and contrast the ethical considerations given to subjects in the two inner-city research projects. If replicating these two studies, how would you plan a study that is ethically responsible?