Stephen K. Rice
Michael D. Maltz   *Editors*

Doing Ethnography in Criminology

Discovery through Fieldwork

Foreword by
Shadd Maruna

Springer

Doing Ethnography in Criminology

Stephen K. Rice • Michael D. Maltz
Editors

Doing Ethnography in Criminology

Discovery through Fieldwork

Foreword by Shadd Maruna

Springer

Editors
Stephen K. Rice
Criminal Justice Department
Seattle University
Seattle, WA, USA

Michael D. Maltz
Department of Criminology, Law
and Justice
University of Illinois at Chicago
Chicago, IL, USA

Foreword

An Ethnography of Ethnographers: Humans Being Human

It is an enormous and highly undeserved honor to be asked to write a foreword for such a fascinating collection of deeply personal reflections of some of the most accomplished ethnographers working in criminology today. I have to confess that the only reason I was asked to write this foreword is because I declined the invitation to write a proper chapter for the volume myself. I did so, not just out of laziness or lack of time (both of which are undeniable) but out of a sense of (at best) humility and (at worst) a sort of personal shame.

That is, this is a book not just about *ethnography* but about *ethnographers*. Indeed, if ethnography is the art of walking a mile in another's shoes, then this remarkable book is a sort of ethnography of ethnographers. Yet, I do not consider myself a proper ethnographer. Of course, I do my share of qualitative and mixed method research, and much of this is field-based and action-oriented. Indeed, like most of the authors in this volume (this is 2018, after all, and the world is falling apart all around us), I have lost interest in desk-based, purely academic chitchat about who has the best theory or scale or dataset. Instead, I try to utilize whatever time and small talents I have in work alongside real-world organizations dealing with real-world problems in these troubled times. Yet, as Mark Fleisher writes in his chapter, this does not make me an ethnographer. You do not get to be an ethnographer just because you attend a lot of board meetings of ex-prisoner charities or consult with justice reform groups or even go into prisons now and again to do interviews with residents and staff, like I do. Ethnography is about deep, sustained immersion into a "field"—walking that mile and then some. At its best (and some of the best examples of the method in criminology have been carried out by the authors of this volume), ethnography can be the richest form of social scientific research.

The last time I have done genuinely ethnographic research of that caliber was (shockingly, to me) 20 years ago, and as proud as I am of that work, I have not approached that level of immersion since. If I was ever an ethnographer, then, I am at best a "former ethnographer" (perhaps recovering ethnographer?) today, and my

dusty "war stories" from two decades ago are about as relevant today as Blur, Oasis, Tony Blair, and Bill Clinton are to the contemporary public conversation. That is, not irrelevant by any means but hardly the future of the field.

In this regard, at least, I can console myself that I am not alone. In the lovely phrase of the new-generation (and very much proper!) ethnographer Ali Fraser (this volume), the "ghosts of ethnography past, present and future" constantly haunt those of us considering an ethnographic career. Indeed, Fraser quotes the legendary ethnographer Dick Hobbs as saying that "the best ethnographies are often done by PhD students":

> The ghost of ethnographic future that stalks the field is therefore that it is destined to be constantly revivified by younger scholars, only to be extinguished as life takes over. Some people manage to a find a way to continue to think and work ethnographically at different points in their life – but for many others who are fortunate enough to have an academic job, the particular demands of academic – and family – life make the slow craft of ethnography challenging to say the least (Fraser, this volume).

A number of chapters in this exciting volume, by both newer and older ethnographers, call attention to these situational dynamics of the modern university context, in particular what Jamie Fader calls the "academic speedup" and the rational adaptations researchers make to cope with the absurd demands for research outputs and grant income. These pointless hoops and hamster wheels of modern academia are surely designed to ensure that we academics are unable to engage in the world in a meaningful way—what better way to silence a massive group of intellectuals and critical thinkers in a time when such voices could form an essential bulwark against government propaganda, corruption, and deceit? At any rate, these structural impediments to deep ethnographic engagement are not the only obstacle to criminological ethnography (and I would also say criminological insight) in academia today.

The other main obstacle is more intrinsic to the method itself, I would argue. Something about ethnography as a method elicits (over)reactions of the most extreme and unfortunate kind. Unlike researchers using interviews or surveys or secondary data in their analyses, the ethnographer is seen as something of a mythical folk hero or else racist usurper of stories, depending on one's perspective. For some, the title of "ethnographer" triggers images of a kind of Indiana Jones figure risking everything to penetrate secret worlds and bringing back fascinating stories from the edges of human experience. To others, the ethnographer (and strangely not the survey researcher or interviewer) is a colonial figure, profiting on others' misery, exploiting the stories of the disadvantaged to sell books and gain fame and tenure.

Perhaps the best example of these two extreme reactions can be found in the reaction to Alice Goffman's debut ethnography *On the Run*. The ridiculously overblown venom and vitriol directed toward this Ph.D. study was matched only by the unbelievable hype, hyperbole, and veneration the work received on initial publication (and the two responses were surely related). Similar over-reactions have greeted works like *Gang Leader for a Day* or *Slim's Table* with critics and accolades falling over themselves to exaggerate the daring originality (no one had ever thought of hanging around gang members?) or grotesque political exploitation (no one ever sold a book about poverty before?) of the works.

Indeed, perhaps the best way to set off a bomb in a room full of methodologists is to raise the subject of "auto-ethnography" (see Wakeman, this volume). Something about the method, in which individuals with life experiences (being victims or perpetrators of crime, for instance) write about their own experiences from a first person perspective, seems to make certain colleagues simply apoplectic: "That's not science!" "That's not research!" "That's navel gazing! That's anecdotal!" Of course, it is not hard to see that the positivist doth protest too much in such over-reactions. After all, it seems rather obvious to any rational creature that someone who survives a decade in prison might have some interesting insights into the prison experience or someone who has been a police officer for a decade might have some knowledge to share about police culture. At the same time, the polar opposite reactions—that *only* those who have been cops have anything useful to say about police culture or that only ex-cons can write about the imprisonment experience—are equally wrongheaded. Auto-ethnography has many undeniable strengths, but so does survey research or ordinary forms of qualitative research, and the method has all the same limitations that these other forms of research have as well. Just because someone has been a prison officer or a victim of assault, she does not have a right to speak on behalf of all prison officers or victims of assault. One still has to "do the work"— situating the findings in the existing literature, interpreting the findings using theoretical tools (grounded or pre-existing), and most of all subjecting the conclusions to the rigorous scrutiny of future research.

In other words, it is long past time that ethnography and auto-ethnography are treated just like any other piece of research, without all of the hype (whoop-de-doo an academic actually left the office and talked to poor people) or the aura of god-like "truth" that books by former police officers or former gang members sometimes receive. At the same time, the wider academic community needs to stop over-reacting to the limitations of such work as if ethnographers were uniquely guilty of exploitation of the disadvantaged and deviant, just because ethnographic research provides such in-depth coverage of these issues. We non-ethnographers need to take a long, hard look in the mirror if we think that our survey- or interview-based research is somehow exempt from such charges ourselves. Indeed, surface-level surveys of a community may not be as intrusive, but they can be far more harmful in their consequences!

What makes this volume so incredibly useful then is that it pulls back the curtain on the ethnographic process and cuts through some of the myths and mystifications surrounding the technique. As Andres Rengifo and colleagues write in their chapter, ethnographers sometimes fantasize about draping themselves in a sort of cloak of invisibility so as to observe the field without impacting on it, which is of course not possible and probably not even desirable. This book essentially lifts that cloak or camouflage to expose the real people attempting to blend into the background in the ethnographic process.

What we see, upon getting to meet ethnographers first hand, is that they are pretty darn ordinary social scientists doing a day's work. For instance, Mark Hamm points out that, contrary to popular mythology, ethnography is neither cool, nor exciting, nor fun. From reading the chapters in this remarkable volume, it becomes

clear that, like most other types of work, ethnography is a hard slog—often exceedingly tedious, almost always potentially awkward. There are certainly risks involved, but they are rarely the "jumping out of helicopters" variety of fantasy and fiction. Much more common risks are the occasional practical joke at the ethnographers' expense as described by Jennifer Sumner and colleagues, or else struggles with intransigent institutional review boards and ethics committees (see esp. Urbanik, this volume), or, most poignantly, the emotional churns and toils elegantly described by Heith Copes in his study of addiction in rural America. The absolute most harrowing experiences ethnographers seem to face involve not so much physical threats but legal threats, as described by Hamm when he found himself dragged into federal court by a white supremacist group who sued him for "everything, including my bank holdings and university retirement fund."

Above all else, the chapters reveal the remarkable leaps of vulnerability essential to the ethnographic process. To do ethnography right, it becomes clear, you have got to be willing to be "caught being stupid," in Copes' memorable phrase, or as Jeff Ferrell puts it, more crudely, you have to accept fundamentally that you "don't know shit." Perhaps this requirement for a suspension of professional dignity accounts for why ethnographic research is so much better suited to new Ph.D. researchers than to more established academics. The ethnographic process is one of deep humility—if not humiliation! Whether observing at a maximum security prison or an open air drug market, by definition, the ethnographer "does not belong" and enters the field with the curiosity of a newborn, hungrily seeking to figure out what is going on and why, when such questions seem ridiculous or naïve to those who need to function in the environment every day.

The image of the ethnographer as the fearless explorer of the rough-and-tumble mean streets then is almost entirely wrong. Yet, ironically, the sheer vulnerability that the method requires really does take tremendous guts—if not the swaggering machismo of lore, certainly a willingness to risk exposure and humiliation for the sake of knowledge. Not hiding behind a desk in the safety of our offices, but out there among our "subject matter." I continue to think, after reading the chapters in this volume, that this is the purest form of social science. It is also, as these contributions demonstrate clearly, a very human endeavor with all of the weaknesses, imperfections, and insights that are intrinsic to human ways of knowing the social world through interaction. Long may it thrive in criminology, or we will no longer be a science.

Queen's University Belfast   Shadd Maruna
Belfast, UK

Contents

List of Contributors

Amada Armenta Department of Urban Planning, Luskin School of Public Affairs, University of California, Los Angeles, CA, United States

Carla J. Barrett John Jay College of Criminal Justice, New York, NY, USA

John J. Brent School of Justice Studies, College of Justice and Safety, Eastern Kentucky University, Richmond, KY, USA

Fiona Brookman University of South Wales, Pontypridd, UK

Sandra M. Bucerius University of Alberta, Edmonton, AB, Canada

Gabriel T Cesar Florida Atlantic University, Boca Raton, FL, USA

Heith Copes Department of Criminal Justice, University of Alabama at Birmingham, Birmingham, AL, USA

Ben Crewe Institute of Criminology, University of Cambridge, Cambridge, UK

Dean Dabney Georgia State University, Atlanta, GA, USA

Scott H. Decker Arizona State University, Tempe, AZ, USA

Stephanie M. DiPietro Department of Criminology and Criminal Justice, University of Missouri St. Louis, St. Louis, MO, USA

Jamie J. Fader Department of Criminal Justice, Temple University, Philadelphia, PA, USA

Jeff Ferrell Department of Sociology and Anthropology, Texas Christian University, Fort Worth, TX, USA

Mark S. Fleisher Jack, Joseph and Morton Mandel School of Applied Social Sciences, Case Western Reserve University, Cleveland, OH, USA

Alistair Fraser Scottish Centre for Crime and Justice Research, University of Glasgow, Glasgow, UK

William Garriott Drake University, Program in Law, Politics, and Society, Des Moines, IA, USA

Cherrell Green Department of Criminology and Criminal Justice, University of Missouri-St Louis, St. Louis, MO, USA

Mark S. Hamm Indiana State University, Terre Haute, IN, USA

Jacqueline B. Helfgott Seattle University, Seattle, WA, USA

Steve Herbert Department of Law, Societies, and Justice, University of Washington, Seattle, WA, USA

Aaron Ho Department of Criminal Justice, Jersey City University, Jersey City, NJ, USA

Aaron Kupchik Department of Sociology and Criminal Justice, University of Delaware, Newark, DE, USA

Michael D. Maltz Department of Criminology, Law, and Justice, University of Illinois at Chicago, Chicago, IL, USA

Peter K. Manning School of Criminal Justice and Criminology, Northeastern University, Boston, MA, USA

Shadd Maruna School of Social Sciences, Education and Social Work, Queen's University Belfast, Belfast, UK

J. Mitchell Miller University of North Florida, Jacksonville, FL, USA

Vanessa R. Panfil Old Dominion University, Norfolk, VA, USA

Keramet Reiter University of California, Irvine, Irvine, CA, USA

Andres F. Rengifo School of Criminal Justice, Rutgers University, Newark, NJ, USA

Stephen K. Rice Criminal Justice Department, Seattle University, Seattle, WA, USA

Lori Sexton University of Missouri, Kansas City, Kansas City, MO, USA

Lee Ann Slocum Department of Criminology and Criminal Justice, University of Missouri-St Louis, St. Louis, MO, USA

Jennifer Sumner California State University, Dominguez Hills, Carson, CA, USA

James Treadwell Staffordshire University, Stoke-on-Trent, UK

Marta-Marika Urbanik Department of Sociology, University of Alberta, Edmonton, AB, Canada

Stephen Wakeman Centre for Crime, Criminalisation and Social Exclusion, Liverpool John Moores University, Liverpool, UK

Danielle Wallace School of Criminology and Criminal Justice, Phoenix, AZ, USA

About the Editors

Stephen K. Rice is associate professor of Criminal Justice at Seattle University. He studies cooperation and defiance in individuals' interactions with the justice system. Toward this end, his work has focused on emotions and crime, radicalization, procedural and restorative justice, racial/ethnic profiling (African Americans, Latinos, Muslim Americans), police/community relations (e.g., the police officer as "guardian"), final statements of the condemned, and social media and criminal justice. His publications have appeared in outlets to include *Criminology*, *Justice Quarterly*, *Deviant Behavior*, *Policing*, the *Journal of Quantitative Criminology*, and the *Harvard Executive Session on Policing and Public Safety*. He is also coeditor of *Envisioning Criminology: Researchers on Research as a Process of Discovery* (Springer) (with Michael D. Maltz) and *Race, Ethnicity, and Policing* (NYU Press) (with Michael D. White). His scholarship has been featured in public outlets to include *The Final Report of the President's Task Force on 21st Century Policing*, *The Washington Post*, *The Atlantic*, and *The New York Times*. He received his Ph.D. from the University of Florida.

Michael D. Maltz is professor emeritus of criminal justice and of information and decision sciences at the University of Illinois at Chicago and senior research scientist and adjunct professor of sociology at the Criminal Justice Research Center at Ohio State University. A past editor of the *Journal of Quantitative Criminology*, he was on the staff of the National Institute of Justice (1969–1972) and was a visiting fellow at the Bureau of Justice Statistics (1995–2000). In 1996, he held a Fulbright Scholarship at El Colegio de Michoacán in Mexico. His primary research interests have been on the assessing the validity of, and developing measures to extract useful inferences from, crime data. His 1984 book, *Recidivism* (Academic Press), was awarded the Wilkins Award for the outstanding book in criminology and the Lanchester Prize of the Operations Research Society of America. He is coauthor of *Mapping Crime in Its Community Setting* (Springer) (with Andrew Gordon and Warren Friedman), author of *Bridging Gaps in Police Crime Data* (US Government Printing Office), and coeditor of *Envisioning Criminology: Researchers on Research as a Process of Discovery* (Springer) (with Stephen K. Rice). He holds a Ph.D. in electrical engineering from Stanford University.

Introduction: Walking a Mile in Another Person's Shoes

Stephen K. Rice and Michael D. Maltz

When we conceived this book, we thought we knew what was meant by ethnography and how its practitioners went about approaching their trade. And we thought that we could categorize ethnographies by what their topics were: offenders, police, homeless persons, or other actors or organizations involved in criminology research. What we found, however, is that we were more able to explain what it is *not*: it is not based on administrative data, on primarily closed-form questionnaires, on statistical analyses, on percentages, and on p-values.

In general, it is an attempt to understand the perspective of the group or persons that one approaches. In some cases it is exemplified by the above title, walking a mile in another person's shoes, but in other cases it is just walking around the block with them, watching them walk around the block, or just standing on the sidelines while others walk or run or stand around (yes, it's a visually active space). In every case, however, the important point is to put aside one's own preconceived notions about the world and try to understand where *they* (or he or she) are coming from.

Ethnography research runs the gamut of living with people in a different culture for decades (A bit longer than a mile!) to using other means of getting "inside the heads" of people who have very different ways of thinking about life, as one of this volume's authors reminded us during the book's production (Thank you, Mark Hamm!). So what you have here is a bunch of stories that we hope convey the flavors of the ethnographic enterprise. There's a tremendous amount of lived "weight" explored in this volume, something we've aimed to honor fully.

Many criticize ethnography for not being objective because it often explicitly draws on the experience and background of the ethnographer, who views the things

S. K. Rice (✉)
Criminal Justice Department, Seattle University, Seattle, WA, USA
e-mail: ricest@seattleu.edu

M. D. Maltz
Department of Criminology, Law, and Justice, University of Illinois at Chicago, Chicago, IL, USA

she/he is seeing through a lens that is different from both "informants" and, very often, from others with different backgrounds. Of course, it is almost impossible to leave one's perspective behind, since we can't change who we are and what we've experienced. And it is altogether likely that different ethnographers "see" different facets of a situation and interpret their observations differently, so they may be criticized for their lack of objectivity. Moreover, the reader also has her/his own biases that color how the story is interpreted. While one's lens can mean different things to different people, in nearly all cases, it suggests sizable baggage in the areas of the personal, theoretical, and methodological.

Seeing things and actions through the imperfect lens of our all-too-human selves often gives rise to the complaint that ethnography is not "scientific" enough because it isn't "objective" enough. But this holy grail of scientific objectivity is itself subject to question, as Heims (1980, p. 360) noted:

> The ethos of science rests on two pillars, the politically useful myth of "value neutrality" and the article of faith most conducive to the growth of scientific bureaucracy, namely, that scientific innovations ("progress") and science-based technological innovations are *a priori* beneficial. While these two pillars clearly knock against each other, they continue to hold up the practice of science.

This book is an exploration of the ways that the personal is involved in understanding the actions of the actors in the systems we study. It is for that reason that we haven't found an easy way to group the chapters into specific categories, since those who deal with drug offenders, for example, are no more likely to have qualms about how their lives were impacted by their work than those who deal with the police. So we have taken the easy way out and have put the papers in alphabetical order, so readers can explore their way through the book without our having prepared a predetermined route. The criminological imagination can bear fruit at all sorts of times (e.g., the library stack at 1 a.m. or, more relevant to this effort, the hard slog at first and Main at 1 a.m.). We're hopeful that any dissonance the reader may experience due to this lack of structure becomes a positive thing, reflective of the variety (and richness) of the ethnographic enterprise itself.

The two of us also bring our own biases and mixed perspectives into play. One of us (SKR) looks at the ethnographic enterprise more from the inside (give or take, arguably), while the other half of this "odd couple" (MDM) sees things from the outside. Our stories are presented below.

Steve

My scholarship and teaching interests best track with the viscerality and summed insights afforded by ethnography (or, at the very least, going both ways–thanks Shadd Maruna (2010)).

On the teaching front, there is no doubt that 10+ years of placing my undergraduate and graduate students into (sometimes uncomfortable) service learning scenarios has made me a better professor and a better quasi-ethnographer. Beginning some

time ago while on the faculty of the John Jay College of Criminal Justice/CUNY Graduate Center, I made the executive decision to channel the respected JJ faculty (e.g., Ric Curtis, David Kennedy) by small stepping my non-ethnographically trained quantitative methods and theory students to Hell's Kitchen to better explore what qualitative "covariation" looks like within the urban muck.

What a rush! Where else could one take a short walk to a Hell's Kitchen playground on 45th Street made infamous by the Bronx-based Crowns, Heart Kings, and Vampires Puerto Rican gangs as they aimed to fight neighborhood Irish gang members in 1959[1] and then have open-air conversations with my students (and sometimes members of the neighborhood) about tenements, distressed immigrant realities, collective efficacy, social capital, broken windows, social disorganization, strain, perceived disorder, and anomie)?

These experiences put the flesh on staid criminological theories that sometimes fill our textbooks, a process particularly meaningful given that over the years I've been fortunate to write about some of these theories (general strain, restorative justice, social bonds, self-control, procedural justice). Being able to evoke characters such as the Capeman street-side blew me away (and was quite a contrast to clean ICPSR downloads).

More recently, thrusting students into service to the Seattle criminal justice community has been a core part of my self-imposed (perhaps Jesuit-inspired) teaching philosophy over the course of 10+ years, all of which finds its basis in street-level fieldwork where my students work as consultants to a given semester's client, making recommendations through interactions with relevant organizational or community data points that are of strong concern to the client. Our clients have been many (Seattle PD, principally, thank you) but also the Washington State Criminal Justice Training Commission, Belltown Community Council, US Marshals Service, Washington Association of Prosecuting Attorneys, and Seattle Parks and Recreation. Areas of focus for fieldwork have included communities and crime, mass transit and crime, low-level nuisance offenses, multigenerational drug sales, urban environmental design, bias crimes, body-worn cameras, at-risk youth, nightlife, human trafficking, and Internet Crimes Against Children (ICAC).

On the scholarship front, my dips-of-the-toe into ethnography parallel Jacqueline Helfgott's somewhat (this volume), as there have been a number of times when I've searched for a "glimmer" of classic ethnography amid methods which may or may not constitute true ethnography to classically trained qualitative researchers.

As was outlined in my chapter "Getting Emotional" in the Maltz/Rice edited volume *Envisioning Criminology* (Rice, 2015), my research has tended to coalesce around the "hot" and the "wet" (my term, imperfect but useful) or how matters such

---

[1] See Salvador Agron (the "Capeman") and his peers. Retrieved from http://newyorkcitygangs.com/?page_id=99). I write "aimed to fight" as there were multiple mistaken identifications that night, leading to the deaths of boys who were not members of the Norsemen (a neighborhood Irish gang). One wonders whether Salvador Agron's case would have received the level of attention that it did (Broadway musical by Paul Simon, with Marc Anthony and Ruben Blades) had it been a more straightforward conflict between the Vampires and Norsemen, as normalized violence.

as shame, humiliation, and rage condition defiance and criminal behavior and how, conversely, desires for reconciliation and repentance afford softer areas of understanding. These themes have been explored in varied research projects and have touched on ethnographic sensibilities.

In one, Danielle Dirks, Julie Exline, and I (Rice, Dirks, & Exline, 2009) conducted what is thought to be the first comprehensive content (?)/discourse (?) analysis[2] of final statements of condemned inmates. This was done by coding the emotional makeup of such utterances (e.g., statements of innocence, statements of admitted guilt, statements regarding the unjust nature of capital punishment, statements regarding unfit legal counsel, desires for forgiveness, desires for redemption) (the latter being a combinatorial measure) and assessing how these final statements ($n = 269$, welcome to TX!) relate to inmate characteristics and to the presence of homicide survivors (victims' families and friends) at executions.

Put simply, the "homicide survivor attendance effect" (attendance x desires for forgiveness, desires for redemption) was found to be profound per mixed method analysis (the largest effect you'll probably ever see, save previous behavior being predictive of future behavior).

It's presumptuous to assume that assessments of death chamber narratives approach the elusive "thick description," although here we are also focusing on not only the "facts" of executions but also providing commentary and interpretation. Further, as Danielle, Julie, and I remarked in the closing of the paper—despite any ethnographic aspirations we may find that we are ill prepared to find meaning in final statements—that the penal system's desire for minimization of physical pain (Sarat, 2001) may have led to theatrical representation of pain: punishment which plumbs the depths of the offender's heart. I will leave it to the extraordinary cast of characters in this volume to provide guidance on how best to frame and analyze statements by the condemned which can span from mere utterances ("I love you," or "I'm ready") to sizable transcripts worthy of socio-legal analysis.[3] Large swaths of the coding process were heart-aching 3 a.m. moments.

Another interesting research question provided Randy Horton, Nicky Piquero, Alex Piquero, and me (Horton, Rice, Piquero, & Piquero, 2012) the opportunity to work together reimagining ethnographic measures from Randy's Ph.D. dissertation into an Agnew-ian lens. More specifically, the study presented data from structured interviews and fieldwork in India and the United States that demonstrated variations in the understanding and experiences of anger among Americans, Tibetan Buddhist clergy, and lay Tibetans by way of the normative social approval of anger, whether techniques are thought to exist to prevent anger, the perceived effects on self in becoming angry, reaction tendencies, emotional memory, and in the moderating

---

[2] Will leave this for a late night Habermasian ASC conversation regarding whether it's a content analysis or discourse analysis. For the time being, I'll err on the side of discourse analysis, given that the meaning one attaches to final statements can be fluid and is most definitely socially constructed.

[3] I think of you often Napoleon Beazley: https://www.tdcj.state.tx.us/death_row/dr_info/beazley-napoleonlast.html.

impact of culture on recalled anger. Anger is of course of principal interest to Robert Agnew (2001) as general strain theory's key affective mediator, and we realized that a useful place in the paper vis-a-vis Tibetans' rich body of ethnopsychological writings and practices focused on emotional states of anger.

My (albeit limited) journey into the ethnographic imagination has provided very useful first steps. Even if I don't consider myself a "true" ethnographer, so much of my work has focused on the emotional impact of crime and its consequences that I'm closely attuned to ethnography's importance in criminology, criminal justice, and broader explorations of affect.

Mike

I'm best known for quantitative, not qualitative, research. An engineer by training, I had never taken any courses in social science when I began teaching in a criminal justice program in 1972. My entire criminal justice experience up to that point was based on my having been a staff member of the National Institute of Justice from 1969 to 1972, and I was hired by NIJ because of my engineering background and my experience in police communications—see below.

My introduction to social science and to the research techniques that were then used by its practitioners began when I joined the criminal justice faculty of the University of Illinois at Chicago. I was put to work teaching social science statistics and, not knowing much about it, used the books that others used before me. But even then I was mystified by the common practice of looking to achieve a low p-value as the be-all and end-all of such research. Untenured and with no experience in the field, I taught what others thought was important. But I soon wised up, and described my concern about the methods used, some 30 years ago (Maltz, 1984, p. 3):

> When I was an undergraduate in engineering school there was a saying: An engineer measures it with a micrometer, marks it with a piece of chalk, and cuts it with an axe. This expression described the imbalance in precision one sometimes sees in engineering projects. A similar phenomenon holds true for social scientists, although the imbalance is in the opposite direction. It sometimes seems that a social scientist measures it with a series of ambiguous questions, marks it with a bunch of inconsistent coders, and cuts it to within three decimal places. Some balance in precision is needed, from the initial measurement process to the final preparation of results.

And I further expressed my concern about the focus on "statistical significance" in a subsequent article (Maltz, 1994). Ethnography is a welcome and much-needed departure from that type of research. It deals with individual and group behavior that doesn't conform to statistical or spreadsheet analysis. Yes, an ethnography may just be a single data point, but it often serves as a marker of importance, an exploration of what additional factors should be considered beyond the usual statistics, or as a counterexample to some of the more positivist studies.

In this regard, three examples provide additional context to my strong belief in the need for a qualitative orientation. The first was my initial experience while consulting on police communication systems (true electrical engineering!) for the Boston Police Department from 1966 to 1969. To satisfy my curiosity about the ways of the police, I requested, and was granted, permission to conduct an "experiment"[4] on police patrol. The number of patrol cars in one police district was doubled for a few weeks to see if it had any effect on crime. And it did: compared to the "control" district, which had no arrests, the "experimental" district had six arrests. Moreover, there were no arrests at all for the same time period in either district in the previous year, so I could calculate that $p = 0.016$, much less than 0.05. What a finding! Police patrol really works!

On debriefing one of the arresting officers, one of the first lessons I learned was that police officers are not fungible. There are no extra police officers hanging around the station that can be assigned to the experimental district: they have to be drawn from somewhere else. The additional officers, who made all of the arrests, were from the BPD's Tactical Patrol Force—the Marines of the department—who were normally assigned to deal with known trouble spots, and the two districts selected for the study were generally low-crime areas.

In fact, the TPF officers already knew that a gang of car thieves/strippers was active in the experimental district and decided to take them out, which resulted in all of the arrests they made. They couldn't wait to get back to working citywide, going after *real* crime, but took the opportunity to clean up what they considered to be a minor problem. So after that experience, I realized that you have to get under the numbers to see how they are generated or, as I used to explain to students, to "smell" the data.

Another example: Some years ago I was asked to be an expert (plaintiff's) witness in a case in the Chicago suburbs, in which the defendant suburb's police department was accused of targeting Latino drivers for DUI arrests to fill their arrest quotas. My job was to look at the statistical evidence prepared by another statistician (the suburb's expert witness) and evaluate its merits. I was able to show that there were no merits to the analysis (the data set was hopelessly corrupted), and the case was settled before I had a chance to testify.

What struck me after the settlement, however, was the geography and timing of the arrests. Most of them occurred on weekend nights on the road between the bars where most of the Latinos went to drink and the areas where they lived. None were located on the roads near the Elks or Lions clubs, where the "good people" bent their elbows.

I blame myself on not seeing this immediately, but it helped me to see the necessity in going beyond the given data and looking for other clues and cues that motivate those actions that are officially recorded. While it may not be as necessary in some fields of study, in criminology it certainly is.

---

[4] Not quite under true experimental conditions, to say the least! The two police districts and the time period were specified by the police commissioner, who wasn't about to give an engineer with no street smarts free rein.

A third example was actually experienced by my wife, who carried out a long-term ethnographic study of Mexican families in Chicago (Farr, 2006) all of whom came from a small village in Michoacán, Mexico. Numerous studies, primarily based on surveys, had concluded that these people were by and large not literate. One Saturday morning in the early 1990s, she was in one of their homes when various children began to arrive, along with two high school students. One of the students then announced (in Spanish, of course), "Ok, let's get to work on the *doctrina* (catechism)," and slid open the doors on the side of the coffee table, revealing workbooks and pencils, which she distributed to the kids.

On another occasion, my wife was drinking coffee in the kitchen when all of the women (mothers and daughters) suddenly gathered at the entrance to the kitchen as someone arrived with a plastic supermarket bag full of something—which turned out to be religious books (in Spanish) on topics such as Getting Engaged and After the Children Come Along. Each woman eagerly picked out a book, and one of them said, "I am going to read this with my daughter."

Clearly these instances indicate that children in the catechism class and the women in the kitchen were literate. The then-current questionnaires that evaluated literacy practices, however, asked questions such as "Do you subscribe to a newspaper? Do you have a library card? Do you have to read material at work?" In other words, the questionnaires (rightly so) didn't just ask people outright "Can you read?" but rather focused on the domains they thought required reading. Yet no questions dealt with religious literacy, since literacy researchers at the time did not include a focus on religion. The result? The literacy practices of these families were "invisible" to research.

These anecdotes are but three among many that turned me off the then-current methods of learning about social activity, in these cases via (unexamined) data and (impersonal) questionnaires. Perhaps this has to do with my engineering (rather than scientific) background, since engineers deal with reality and scientists propound theories. To translate to the current topic, it conditioned me to take into consideration the social context, a recognition that context matters and that not all attributes of a situation or person can be seen as quantifiable "variables." This means, for example, that a crime should be characterized by more than just victim characteristics, offender characteristics, time of day, etc. and that an individual should be characterized by more than just age, race, ethnicity, education, etc. or "so-so" (same-old, same-old) statistics. These require a deeper understanding of the situation, which ethnography is best suited, albeit imperfectly, to do—to put oneself in the position, the mindset, of the persons whose actions are under study.

Other Stories

As with ethnography in general, no two stories are alike, and ours are as different as those of the authors whom you will meet in the succeeding chapters. Have fun!

References

Agnew, R. (2001). Building on the foundation of general strain theory: Specifying the types of strain most likely to lead to crime and delinquency. *Journal of Research in Crime and Delinquency, 38*, 319–361.

Farr, M. (2006). *Rancheros in Chicagoacán: Language and identity in a transnational community*. Austin, TX: University of Texas Press.

Heims, S. J. (1980). *John von Neumann and Norbert Wiener: From mathematics to the technologies of life and death*. Cambridge, MA: MIT Press.

Horton, R., Rice, S. K., Piquero, N. L., & Piquero, A. R. (2012). On the variability of anger cross-culturally: An assessment of general strain theory's primary mediator. *Deviant Behavior, 33*, 1–22.

Maltz, M. D. (1984). *Recidivism*. Orlando, FL: Academic Press.

Maltz, M. D. (1994). Deviating from the mean: The declining significance of significance. *Journal of Research in Crime and Delinquency, 31*(4), 434–463.

Maruna, S. (2010). Mixed method research in criminology: Why not go both ways? In A. Piquero & D. Weisburd (Eds.), *Handbook of quantitative criminology* (pp. 123–140). New York: Springer.

Rice, S. K., Dirks, D., & Exline, J. J. (2009). Of guilt, defiance, and repentance: Evidence from the Texas death chamber. *Justice Quarterly, 26*, 295–326.

Rice, S. K. (2015). Getting emotional. In M. Maltz & S. Rice (Eds.), *Envisioning criminology: Researchers on research as a process of discovery* (pp. 89–98). New York: Springer.

Sarat, A. (2001). Killing me softly: Capital punishment and the technologies for taking life. In A. Sarat (Ed.), *Pain, death, and the law* (pp. 43–70). Ann Arbor, MI: University of Michigan Press.

Seeing Like a Cop, Writing Like a Critical Scholar

Amada Armenta

In 1994, California voters passed Proposition 187, a ballot initiative to prohibit unauthorized immigrants from using state services. As an eighth grader growing up in El Centro, California, a town just 12 miles from the Mexican border, I was devastated when the election results came in. While the law was not technically directed at my Mexican-American family or me, it was directed at people who *looked* like us. Members of my family came to the United States from Mexico without permission in the 1920s, 1930s, and 1940s. The fact that we were citizens seemed like an accident of fortune, rather than an entitlement. Generations of legal presence did not protect us from the presumption that we were not "really" American.

While Proposition 187 was later ruled unconstitutional and was never implemented, it put immigration on the national agenda. In 1996, Congress passed new laws to punish unauthorized immigrants and directed extraordinary resources to border enforcement. As part of the country's sprawling immigration enforcement regime, the number of US Border Patrol agents in my hometown increased dramatically. Friends from school joined their ranks. Border Patrol vehicles cruised through city streets. Immigration checkpoints went up on every major highway in and out of town. The presence of immigration enforcement intruded on our lives. While I was merely subjected to the indignities of repeatedly verifying my status as a citizen, others were trapped in place, unable to travel because immigration checkpoints threatened their continued residence.

Today, my research examines the expansive power of US immigration laws to shape the experiences of Latino immigrants and their descendants. My passion for pursuing this research agenda stems from growing up in the shadow of the wall and repeatedly affirming my legal presence to travel freely in and out of town.

A. Armenta (✉)

Department of Urban Planning, Luskin School of Public Affairs, University of California, Los Angeles, CA, United States
e-mail: armenta@luskin.ucla.edu

My book, *Protect, Serve, and Deport: The Rise of Policing as Immigration Enforcement*, examines how local law enforcement agencies participate in immigration enforcement. I conducted the bulk of my research for this book in Nashville, Tennessee, between 2009 and 2010. At the time, Nashville had just implemented a program called 287(g), which empowered the Davidson County Sheriff's Office to enforce immigration law. The 287(g) program emerged from a buried provision in the text of the 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA), Section 287(g), which allowed local law enforcement agencies to be deputized as immigration officers.

By the mid-2000s, state and local immigration enforcement policies were emerging throughout the United States, as some jurisdictions sought to compel local law enforcement agencies to enforce immigration laws (Varsanyi, 2010). Moreover, Immigration and Customs Enforcement (ICE), the federal agency in charge of immigration enforcement, was steadily expanding its reach into jails and prisons through various programs and initiatives. While excellent research has examined the determinants of restrictive state and local immigration policies (Ramakrishnan & Wong, 2010; Walker & Leitner, 2011), they tell only *part* of the story. Missing from extant accounts are the on-the-ground processes that funnel unauthorized immigrants to jails and prisons where they are subsequently identified for removal. In other words, how do local law enforcement agencies understand their roles in immigration enforcement? How do local police make discretionary arrest decisions and "choose" whom to expose to immigration enforcement screenings? These are questions I sought to answer when I moved to Nashville, Tennessee, to understand the relationships between local policing and immigration enforcement.

Securing Access to Institutions

I arrived in Nashville with a plan to conduct interviews and do police ride-alongs, despite having few contacts. I plotted my strategy carefully. I decided against immediately showing up at law enforcement agencies and announcing myself as a student who wanted to study them. Instead, I focused on expanding my networks and getting familiar with the city. I drove around different neighborhoods, went to meetings and community events, and frequented an array of public places (parks, churches, restaurants). I studied Nashville government websites to learn about the local government and its various offices, as well as the structure of the local sheriff's office and police department. I pored through years of newspaper articles, city council meeting minutes, and state legislative records to understand when immigration emerged as a local political issue.

I had one contact in Nashville, Katharine Donato, a sociology professor at Vanderbilt who has been my mentor for years. As an immigration expert, the sheriff's office invited Katharine to attend meetings with the 287(g) Advisory Council, and she invited me to come along. In addition to Katharine and me, regular attendees included a representative from the police department (usually the deputy police

chief), sheriff's office employees (the sheriff, the communications director, the 287(g) supervisor, and a caseworker/liaison), attorneys (immigration attorneys and public defenders), representatives from immigrant serving organizations, and one or two Latino immigrant entrepreneurs. Occasionally, a representative from ICE also attended. The group convened four times a year at a conference room in the sheriff's office.

At the first meeting I attended, I introduced myself as a graduate student with interests in the 287(g) program. They received me courteously, and I continued attending meetings throughout my residence. At every meeting, the sheriff presented information about the number of people arrested and identified for removal through the 287(g) program. Occasionally, he explained matters related to the program's implementation, such as immigration bond procedures or how the jail handled immigrants' personal property. After I became a "regular" and knew everyone on the council by name, I asked employees in the sheriff's office for interviews. Over two days, a 287(g) supervisor allowed me to come into the jail and interview every deputy who participated in the program as an immigration officer.

I used a similar strategy to gain access to the police department. I started by enrolling in the department's Citizen's Police Academy (CPA). The CPA is a public outreach program designed to educate interested civilians about policing. The group met weekly for 12 weeks. Every week, officers from different precincts and units came to talk about their work. Presentations included information about property crime, neighborhood watch groups, emergency preparedness, terrorism, drug investigations, 911 dispatch, stop and search procedures, community policing programs, and community partnerships. Through our participation in the program, we took field trips to the emergency communications center, the police academy, and the academy shooting range. In addition, we participated in police ride-alongs.

While none of this data ever made it into my written work, these initial experiences with the department were helpful. I learned about the department's structure, I heard the language that officers used when talking about their work, and I learned the routine and procedures that officers went through before and after a shift. For example, I learned what it felt like to walk in a roll call room when everyone was ready to go out on their shift and realized it was better to arrive a few minutes early, before everyone had already assembled. I learned a few "ten codes," the numeric codes that officers use on their radios to describe the incident to which they were responding (a suspicious person, yelling, theft, a car accident, etc.). Most importantly, these initial experiences helped me navigate being in a patrol car with an officer for eight to ten hours at a time.

To gain permission to do research with the Metropolitan Nashville Police Department, I decided to work through a precinct commander rather than the police chief. At the time, there were six police precincts in Davidson County, and each corresponded to a different geographic area. Each precinct had its own building, and the department intentionally decentralizes its unit of command to give each precinct more autonomy. I focused on gaining access to the south precinct, the precinct with the largest concentration of Latino immigrant residents. I knew, from experiences riding in other precincts through the CPA, that I could ride multiple shifts without

ever encountering a Spanish speaker. I reasoned that officers in the south precinct were more likely to come into contact with Latino residents or have opinions about their presence; consequently, I thought riding "in south" would give me access to the kinds of interactions I was interested in seeing.

By this time, my networks in the city were more extensive. I learned that a local businessman who I met through a group called the Coalition for Education about Immigration attended the same church as the south precinct commander. When I realized they knew each other, I asked the businessman to introduce me to the commander as a Ph.D. student who was hoping to learn more about the department. I met the commander one afternoon and told him I was interested in the department's relationship with its diverse constituencies, particularly immigrants. He was delighted by my interest, explaining both the department's and the precinct's various outreach efforts. He believed in community policing and invited residents to come learn more about the department. He indicated he would be happy to help me complete my project and said I could do ride-alongs in the precinct.

While some police researchers utilize a sampling frame to implement their data collection strategy, I was not in a position to negotiate the terms of my access to the department. I was told I could ride "until it became a distraction" and that the department would let me know if that day came. As a result, my access to the police department always felt tenuous. Each ride required that I email the police chief or a supervisor to let them know I was coming. Each time, I worried they might respond saying they were no longer willing to accommodate me. While I rode until I reached data saturation, losing access was a lingering preoccupation.

Accounting for My Presence in the Patrol Car

Police researchers have been using ethnographic methods to understand policing and police organizations for decades (Bittner, 1967; Brown, 1988; Herbert, 1997, 2006; Manning, 1977). One researcher even *became* a police officer (Moskos, 2009). Historically, these police scholars share one common attribute. Like the majority of police officers in the country, they are white men. As a Mexican-American woman, I do not have the luxury of "blending in" with my research subjects. Rather than assume that this excludes me from being a police ethnographer, I followed the lead of other ethnographers who argue that officers' responses to field-workers should be considered data (Herbert, 2010).

Not surprisingly, officers wondered why I was riding with them. Some patrol officers assumed I was doing ride-alongs because I planned to work as a dispatcher in the emergency communication center. Twice, I was confused for a Latina police department employee who worked at another precinct on domestic violence investigations. Occasionally, officers assumed I was married to one of the three Latino officers who worked in the south precinct. These assumptions make clear that officers knew I was not one of them, but they were not particularly suspicious about my presence. While their assumptions placed me *outside* of the boundaries of their

community of patrol officers, they placed me *inside* the boundaries of a larger community of people who supported them: dispatchers, detectives, and partners.

I explained to officers that I was a student who was writing about policing for a school research project and that I was interested in how they did their jobs in a diverse precinct. This answer seemed to satisfy their curiosity, and officers volunteered to answer my questions and "get into things" so that I would have more to write about. When I kept showing up to ride, I got nods of recognition and occasional jokes. "You again?" someone might say, as I walked into the roll call room. "Haven't you had enough of us?" "Maybe you should just sign up for the academy."

There is a derogatory term that officers use to describe women who serially pursue relationships with police officers—badge bunnies. According to police folk knowledge, the "badge bunny's" romantic interest is insincere. She is attracted to the uniform, the perceived power and masculinity of those who wear it, and the officer's paycheck and benefits. She is to be avoided at all costs. She ruins careers. As a young woman doing fieldwork, I was sensitive to the fact that my sociological curiosity could be misinterpreted as romantic interest. As a precaution, I wore a (fake) engagement ring and was prepared to talk about my (nonexistent) fiancé. The ruse was ultimately unnecessary, as no one seemed to see me as a potential romantic partner.

I subtly modified my clothing and my behavior to signal that I would not be a liability, or an "annoying" civilian rider. Officers noticed when riders were ill-equipped to go on patrol. Civilians who showed up in inappropriate footwear (sandals or flip flops) and formal clothing (dresses and suits) were out of place. I regularly wore the same outfit: navy cargo pants, a white polo shirt, and black sneakers to mirror officers' navy uniform and black utility boots. I also avoided carrying a purse, and my shoes and pants were "rugged" enough so that I could walk through mud or run up the stairs with no concern about my attire. I paid attention to small details and mimicked them. For example, when approaching apartment complexes or parking lots, officers unbuckle their seatbelts, even if they have no intention of getting out of the car. They do this so that if they must jump out of the car quickly to pursue someone, they are ready to do so. When I wordlessly unbuckled my seatbelt as we entered an apartment complex, a veteran police officer looked at me with his eyebrow raised and nodded his head in approval.

While I was enthusiastic and curious to learn about police work, I strove to be otherwise dispassionate. I offered no reaction when an officer drove dangerously fast down a city street, fishtailing in the rain. When we got a call about a possible dead body and an officer suggested I stay in the car because it could upset me, I declined. After a tense stop during which officers realized a motorist was carrying a weapon, I reminded them that I had already signed a release form so they need not worry about my safety. However, just as they worried about their own safety, sometimes they worried about mine. One officer made me ride with a bulletproof vest on, which seemed to amuse other officers who saw me that day. Another officer showed me how to use the patrol car radio in an emergency, showing me where to push the button and where to speak into the microphone. Much to my surprise, one officer

even pointed out an extra rifle in the patrol car and indicated that I should use it if I needed to protect myself.

While I conducted semi-structured interviews of employees in the sheriff's office and other key respondents (lawyers, police department employees with desk jobs, members of the sheriff's advisory council, immigrant advocates, etc.), I never "formally" interviewed patrol officers, that is, I did not sit down, turn on an audio-recorder, and ask the officer to respond to specific questions that I had devised in advance. Instead, I relied on "field interviews," unstructured interviews that unfolded naturally during ride-alongs. I asked officers questions about their backgrounds, their decisions to become police officers, their opinions about the department and the job, and their experiences. This strategy allowed me to observe officers' behavior *in situ* and ask officers how they arrived at particular decisions. While a few officers were initially guarded in my presence, many felt comfortable enough to voice their political beliefs, insult their superiors, use derogatory and scatological humor, and complain about civilians who called them for help.

Ultimately, the fact that I speak Spanish made me a commodity. I realized this one afternoon when the officer I was with drove up to another patrol car with the window rolled down and told his coworker in a tone that was both taunting and triumphant, "She speaks Spanish!" My linguistic knowledge meant that I could help officers communicate with the Spanish speakers they would inevitably encounter. Technically, officers could always rely on volunteer translators who were on call to help them. In practice, officers preferred to fumble through interactions by themselves, utilizing rudimentary sign language and the few words they knew in Spanish. Thus, by translating, I facilitated the flow of information between officers and residents.

With me by their side, officers were never frustrated by an inability to communicate with Spanish speakers. Instead, officers told me what to say and what to ask, and I relayed the information that officers requested. Sometimes, I asked additional clarifying questions so when I reported back to the officer, I could give them information that I anticipated they would want. My presence made officers' interactions with Spanish-speaking residents smoother than it might have been otherwise. Consequently, I believe that officers responded to Latino residents more politely and less punitively than they might have in my absence.

Dilemmas on the Front Lines

Ethnographies of police behavior often highlight the dilemmas that officers encounter on the front lines. Officers arrive at their jobs with a set of beliefs, values, and orientations, which they must harmonize with the values and orientations of their profession and their department. Since complete enforcement of the law is neither possible nor desirable, police must choose when and how to deploy the law. Their decisions help determine whether the public thinks the police are fair and whether police authority is legitimate.

To the police, citations, stops, and arrests are mundane bureaucratic procedures. Rarely do they consider the consequences of their decisions or the idea that police practices can feel punitive. I studied the police because I cared about their role in immigration enforcement and their relations with immigrant communities. This perspective meant that I was keenly aware that arrests put unauthorized immigrants at risk of deportation and vehicle stops put immigrants at risk of arrest. Officers, however, had a different set of concerns, and we did not always "see" interactions similarly.

For example, one afternoon an officer and I arrived at a local park after being called about an unattended girl at a birthday party. We asked the girl, who was Latina, to take us to her mother. She reluctantly walked us toward her apartment, holding the officer's hand. Tears streamed down her face, and when her mother opened the door, the young girl hurled herself into her mother's legs and sobbed. After delivering the young girl home and speaking briefly with her mother, we left. As we got back in the car, the officer remarked upon the little girl's demeanor. He thought parents should raise their children to trust the police, and he did not know why the little girl was so afraid of him. In contrast, I saw a little girl who had learned about the risks of deportation to her family at far too young an age.

On another occasion, I disagreed with an officer's assessment of a "suspicious" motorist.

The driver was a young Latino man who appeared to be in his early 20s. He looked nervous and he could not furnish any form of identification. The officer interpreted the young man's apprehension as evidence that he might be involved in criminal activity, and the officer asked him to get out of his car. The young man's clothing suggested to the officer that he was a possible gang member. He wore a ribbed white tank top, baggy jeans, and a belt with a large M emblazoned on the buckle. The officer instructed me to ask the young man if he was part of a Central American gang known as MS-13. When I inquired, the young man insisted he was not a gang member and pleaded with me to tell the officer not to arrest him. Unsure of how to handle this entreaty, I told the officer haltingly, "He asks… that you don't arrest him." Looking at his watch and sighing in frustration, the officer decided to let the man go because it was the end of the shift and conducting an arrest could have delayed our arrival to the precinct by up to two hours.

Though the officer decided not to arrest the young man, the interaction highlighted the gap between my perspective and that of the officer's. The young man *was* nervous during the stop, but I interpreted these nerves as a logical response to immigrants' fear of the police. Moreover, the young man looked completely unremarkable to me, though the officer believed his demeanor and attire were evidence of possible gang affiliation (which is not a crime). While the officer's behavior conformed to professional standards, it left me uneasy. I asked the young man invasive questions that I thought were wrong, *and* I deflected his request for my assistance. Rather than intervene on his behalf, I translated the young man's statement directly to the officer. I wondered if my behavior made me an extension of the police and therefore complicit with their practices.

On one occasion, I *did* intervene on behalf of a civilian who called the police. The caller was a school-aged girl who called because of a domestic dispute between her parents. When we arrived, it was not clear if a domestic dispute had taken place—her mom had no physical marks, and she would not make a statement against her husband, leave her home to go to a shelter, or go downtown to ask for an order of protection. As the officers spoke to the girl's parents, I asked the girl if she was okay. She looked surprised by my question and confessed that she considered killing herself. At a loss, I asked for permission to give her a hug before we left. The next day, I called a friend who was a social worker in the public school system and asked her to check on the student that I had met during the domestic violence call. While doing so may have breached the young girl's privacy, I felt it was more important to preserve her safety.

Seeing Like a Cop, Writing Like a Critical Scholar

The goal of ethnographic fieldwork is to gain a deep understanding of respondents' social realities, their perspectives, and their ways of seeing and understanding the world. Early on in my fieldwork, I realized that if I wanted to "see" like the law enforcement agents whom I was spending time with, I should avoid spending too much time with Latino immigrants. I feared that access to one group might jeopardize my standing with the other, especially if I inadvertently came across an immigrant respondent when I was on the patrol with the police. This decision meant my research addresses law enforcement perceptions, practices, and policies vis-à-vis Latino immigrants, rather than how Latino immigrants see and understand the police.

Through my fieldwork, I came to understand how the police department's organizational priorities shaped officers' behavior. The police department made a staggering number of traffic stops, encouraging officers to "be proactive" by conducting as many vehicle stops as possible. The department believed stops were important because officers could use them as an opportunity to investigate motorists and identify additional violations (Armenta, 2017). In the roll call room, supervisors reminded officers to make stops while they were out on patrol. During meal breaks or lulls in activity, officers regularly talked to each other about their "stats" to see how they measured up against one another. Not only did officers not want to be seen as "lazy" by their peers or superiors, but the most "proactive" officers were more likely to have their preferences accommodated when it came to scheduling their shifts, getting promoted, or having a desirable patrol car.

To the department, aggressive vehicle enforcement was an important strategy. As a result, officers were vigilant about identifying minor infractions that allowed them to pull over vehicles, including window tint violations, tag violations, license plate covers, and inoperable headlights, taillights, brake lights, or blinkers. In fact, officers pulled people for these minor infractions more often than they did for driving violations. In short, officers conducted large numbers of vehicle stops because doing

so was an expectation. The department's focus on investigative vehicle stops is not unique. In their book *Pulled Over*, Charles Epp, Steven Maynard-Moody, and Donald Haider-Markel argue that investigative police rose to popularity in the 1990s, as part of the "war on drugs." Today, stopping motorists as a pretext to investigate criminal offenses is an institutionalized practice that has become synonymous with effective policing. Moreover, while the logic that departments use to justify investigative police stops is color-blind, research shows that investigative stops are disproportionately used to target minority drivers (Epp, Maynard-Moody, & Haider-Markel, 2014).

In Nashville, these discretionary stops posed particular dilemmas for undocumented Latino residents who were ineligible for state driver's licenses and identification cards. Like many states across the country, Tennessee requires residents to show proof of citizenship or legal presence to be eligible for state identification. Since undocumented immigrants could not get state driver's license and identification cards, they drove in contravention of the law. Driving without a license is a misdemeanor offense, and officers can choose to respond in three ways: they can issue a warning, they can make a physical arrest (booking the person into county jail), or they can issue a misdemeanor state citation (technically a noncustodial arrest in which the person deals with the charge on a later date). State law dictates that for many misdemeanor violations, officers should issue a misdemeanor state citation in lieu of making a custodial arrest. Technically, officers may not issue state citations when they cannot identify the person to whom it is being issued. As a result, officers have a great deal of discretion. Some officers routinely arrest motorists whose proof of identity does not meet the officer's standards, whereas others routinely issue state citations.

Officers operate in a social system that consistently disadvantages unauthorized immigrants. Federal law makes some immigrants "illegal," state law dictates that this "illegality" makes immigrants ineligible for driver's licenses, and department priorities ensure that officers make a lot of stops. Not surprisingly, when these stops occur in places where immigrants live, work, and drive, they ensure that officers will have contact with some unlicensed motorists. Driving without a license is a violation of state law, so it follows that some officers will choose to respond to these violations with criminal sanctions. An officer who issues a state citation in lieu of making a physical arrest may feel charitable because misdemeanants avoid being booked into the Davidson County Jail where they will be screened for immigration status violations through the sheriff's participation in the 287(g) program. While a state citation may be preferable to an arrest, these citations come with a court date and substantial monetary penalties. For those who fail to appear in court, the citation will turn into an arrest warrant that the officer will act upon during the person's next contact with law enforcement.

Since ethnographic methods require learning to "see" like one's respondents, it is tempting to interpret an officer's decision to pull over, cite, or arrest undocumented motorists exactly as the police do: as a reasoned and unbiased choice that draws from one's professional expertise. Early in my fieldwork, I struggled to move beyond the police department's characterization of its practices. While ethnography

requires immersion, critically interrogating what respondents take for granted requires distance. As ethnographers, we cannot simply report on what respondents say and do, but we must interrogate their claims (Van Maanen, 1995, p. 20). For criminologists and criminal justice scholars, this means acknowledging power differentials and linking organizational practices with structures of social inequality.

Across the United States, some cities and institutions have adopted policies and practices that incorporate undocumented immigrants as legitimate members and constituents. For example, some police departments routinely accept identification cards issued by foreign consulates (Varsanyi, 2006). Some cities have even gone so far as to create municipal identification cards for which all residents are eligible, regardless of their legal status in the country (de Graauw, 2014). In contrast, the Metropolitan Nashville Police Department has a laissez-faire policy on identification cards, essentially allowing individual officers to decide what constitutes acceptable proof of identity. While the police department sees this decision as rational and obvious, as critical scholars, it is imperative that we interrogate the claims that respondents take for granted. It is not an accident the police department only (routinely) accepts documents for which unauthorized immigrants are ineligible nor is it an accident that unauthorized immigrants do not have access to the only identification card that police are guaranteed to accept. Since most unauthorized immigrants are Latino (also, not an accident), this confluence of circumstances creates racially disparate outcomes.

Local law enforcement agencies, and particularly police departments, are loath to acknowledge that police practices contribute to deportations. However, local law enforcement agencies are critical institutions that connect immigrants to the immigration enforcement system. Indeed, institutionalized police practices and state and federal laws produce racial disparities in who is stopped, cited, and arrested. It is the convergence of law, institutional policies, and police practices that sends a powerful message about Latino immigrants' place in Nashville and in society more generally.

References

Armenta, A. (2017). *Protect, serve, and deport: The rise of policing as immigration enforcement*. Berkeley, CA: University of California Press.

Bittner, E. (1967). The police on skid-row: A study of peace keeping. *American Sociological Review, 32*(5), 699–715.

Brown, M. K. (1988). *Working the street: Police discretion and the dilemmas of reform*. New York: Russell Sage Foundation.

de Graauw, E. (2014). Municipal ID cards for undocumented immigrants: Local bureaucratic membership in a federal system. *Politics and Society, 42*(3), 309–330.

Epp, C. R., Maynard-Moody, S., & Haider-Markel, D. P. (2014). *Pulled over: How police stops define race and citizenship*. Chicago: University of Chicago Press.

Herbert, S. (1997). *Policing space: Territoriality and the Los Angeles police department*. Minneapolis, MN: University of Minnesota Press.

Herbert, S. (2006). Tangled up in blue: Conflicting paths to police legitimacy. *Theoretical Criminology, 10*(4), 481–504. https://doi.org/10.1177/1362480606068875

Herbert, S. (2010). From spy to okay guy: Trust and validity in fieldwork with the police. *Geographical Review, 91*(1–2), 304–310. https://doi.org/10.1111/j.1931-0846.2001.tb00484.x

Manning, P. K. (1977). *Police work: The social organization of policing*. Long Grove, IL: Waveland Press.

Moskos, P. (2009). *Cop in the hood: My year policing Baltimore's Eastern district*. Princeton, NJ: Princeton University Press.

Ramakrishnan, S. K., & Wong, T. (2010). Partisanship, not Spanish: Explaining municipal ordinances affecting undocumented immigrants. In M. W. Varsanyi (Ed.), *Taking local control: Immigration policy activism in U.S. cities and states* (pp. 73–95). Stanford, CA: Stanford University Press.

Van Maanen, J. (1995). *Representation in ethnography*. Thousand Oaks, CA: Sage.

Varsanyi, M. W. (2006). Interrogating "urban citizenship" Vis-à-Vis undocumented migration. *Citizenship Studies, 10*(2), 224–244.

Varsanyi, M. W. (Ed.). (2010). *Taking local control: Immigration policy activism in U.S. cities and states*. Stanford, CA: Stanford University Press.

Walker, K., & Leitner, H. (2011). The variegated landscape of local immigration policies in the United States. *Urban Geography, 32*(2), 156–178.

Doing Court Ethnography: How I Learned to Study the Law in Action

Carla J. Barrett

I never set out to study courts; it was never part of my plan. If I am being honest, I don't really know how much of a plan there was, but I thought I had one. In my applications for graduate school, I stated that I wanted to study community development. At the end of my last year as an undergraduate, shortly after mailing off those applications, I undertook an independent study of my own making (sponsored by a faculty member) that took me from Olympia, WA to Detroit, MI.[1] My intended goal, upon arriving in Detroit, was to learn about what grassroots organizations (as opposed to government initiatives) were doing to revitalize the inner city. I had trained as an undergrad in qualitative research (both interview and ethnography), and I had some connections to grassroots activists in Detroit, so it seemed a good place to conduct research on these issues. Personally, I was also dealing with a painful breakup and really needed to "get away" from the northwest.

When I arrived in Detroit, one of my activist friends set up a meeting for me with Grace Lee Boggs. At the time I had no idea who Grace was. I was just following leads. I would later learn that Grace was a renowned author and a civil rights and Black power activist (Look her up!). Grace kindly invited me into her home and spent an afternoon talking with me in her study. She patiently listened to my goals for my time in Detroit and then suggested that I reach out to the head of a local organization called SOSAD (Save Our Sons and Daughters), an all-Black organization dedicated to responding to, and preventing, gun violence in central city communities in Detroit. They deployed crisis intervention and community support teams whenever someone was shot, and they ran peace programs in the Detroit public schools. All the staff had themselves lost a son to gun violence. The folks at SOSAD welcomed me, a naïve white college student from the West Coast, into their

C. J. Barrett (✉)
John Jay College of Criminal Justice, New York, NY, USA
e-mail: cbarrett@jjay.cuny.edu

offices, and I interned there for the duration of my time in Detroit. Although I did other research around various grassroots movements for revitalization in Detroit, my time, my head, and my heart were consumed by my time at SOSAD and by my time with the city's youth. I left Detroit not really knowing anymore what I would focus on in graduate school, but I knew it would somehow be about urban kids.

The next year I moved to New York City to start a master's degree in sociology at The New School and began to volunteer with an organization called the Harlem Writers Crew, set up by one of the professors, Terry Williams. The Crew brought together various teenagers from around the city once a week for writing workshops and occasional field trips. A few of Terry's graduate students volunteered their time to work with the kids. During my time with the Crew, I kept hearing about the ways in which law enforcement and other aspects of the criminal justice system touched their lives ("my father is in prison," "my brother got arrested last night," "We had to run fast from the cops!"). I started wanting to understand more how all that was impacting the kids; all of whom were Black and lived in poor communities in Manhattan, Brooklyn, and the Bronx.

One afternoon near dusk, one of the boys in the Crew and I were walking through Tompkins Square in Manhattan's East Village. A police cruiser appeared in the park, moving slowly in our general direction, no lights flashing. I, a white woman in my 30s who lived in the area, didn't really think much of it. The 14-year-old Black kid I was with, who lived in public housing in Harlem, however, tensed up as soon as he saw the police car. He started walking backward very slowly, holding his breath, barely moving, and almost paralyzed. Ultimately nothing happened, the officers in the car said nothing, and we walked around the cruiser and went on our way. But it was a moment I have never forgotten; it was a moment that shouted at me loudly and impatiently: "You have so damn much to learn!" I started asking the kids to tell me more about their experiences related to the criminal justice system. I started to read and learn more about racial profiling and racial disparities in conviction and sentencing, over-policing, criminalization, and mass incarceration. My master's program didn't really offer classes around these issues, so much of this research was done on my own time, although I tried to work it into my papers whenever possible. I started turning more and more of my attention to understanding systems of punishment and to the role of law. This was the late 1990s, the decade of the "superpredator" myth (Look it up!) and a decade in which nearly every state in the union had passed laws making it easier to prosecute more and more kids as adults for an increasing list of offenses. I grew ever more curious about these laws, known as transfer laws.

How the Court Found Me

Just as I was finishing my masters, I told Terry Williams that I was interested in these issues, and he suggested I speak with a colleague of his at another school. She was kind enough to meet with me, suggesting two of the most important books ever recommended to me, neither of which was an academic text: the *Juvenile Offender Handbook* by Eric Warner and *All God's Children: The Bosket Family and the*

American Tradition of Violence by Fox Butterfield. She also asked if I would be interested in joining her class on a field trip to a criminal court where youth were tried as adults. I said I would love to.

I didn't set out to study courts, but after my first day sitting in the courtroom we visited, I knew I had to come back. I knew that whatever the story of this court was, I wanted to know it, and I wanted to tell it. I am still rather perplexed by the intensity of my reaction that day. I understood almost nothing of what transpired in the court, and I didn't yet understand how transfer laws worked in New York State. I hadn't yet read the *Juvenile Offender Handbook* or *All God's Children*. I had never read any court ethnographies or really knew that that was something one could do. But I was enthralled by what I witnessed, the interactions, the mix of human emotions and legal formality, and the human drama on what was just a routine calendar day. I felt compelled to understand the place, to know the stories contained within those courtroom walls, and to really understand them. I wanted to understand the law which had created such a court. I wanted to know how the law *worked*, not just what the law *was*.

When I started a PhD program in sociology at the CUNY Graduate Center the following year, I started making plans to do my dissertation research in that very court, which was known as the Manhattan Youth Part (courtrooms are called "parts" in NY). I don't really feel as though I chose the court as my ethnographic field site as much as it chose me. I don't think I am unique in this—I think many ethnographers tell similar stories.

Gaining access to do research in the Youth Part was ridiculously easy. One of my professors at CUNY happened to know the judge and made an introductory phone call on my behalf. The judge would have likely granted me permission anyway as he always welcomed observers to his court. Technically, I didn't really need "permission" to observe the court. Most criminal courts in this country are open to the public; one does not need permission to observe. I have learned, however, that Human Subjects Review Boards often require a letter of cooperation from the court and that being formally granted permission to do research can help facilitate entrée and rapport.[2]

Why Study the Law in Action?

So began my love affair with the study of "the law in action," a love affair that continues today. Early on, while doing research on the history of juvenile justice in New York, I ran across a passage in an obscure interlibrary loaned book from 1955:

> Serious consideration of what the law ought to be cannot begin without comprehension of what the law is. Existing law cannot be understood merely from collections of statutes in books. It must be explored in action, especially in the light of the exercise of the vast discretion vested in administrators.[3]

---

[2] In all my years of observing court proceedings, I have only once been denied "permission" to observe an open public criminal court. That one instance still astounds me because of its absurdity.

[3] Ludwig (1955).

This passage animates my research to this day. In fact, I have the words displayed on a wall in my office. It is a simple statement, and yet it conveys why the study of the "law in action" is vital to our understanding of law and of the criminal justice system more broadly. Laws written by state legislatures (within a particular political context), far removed from the actual practice of law, are often static and vague. To understand how such laws get enacted, interpreted, and brought to life in unique or routine ways, requires that we study the law in action on the ground in courts. For example, to more fully understand the complex uses of discretion, we must examine the many purposes it can serve and the myriad ways it might be deployed on a given Tuesday in a given courtroom in some city or town. Studying the law in action allows us to understand not just what the law is, but *how* it works, and how it doesn't.

Ethnographic study of the law in action is necessary. Simplistic conceptualizations of the "law" tend to conceive it either as a benevolent institution justly designed to maintain order in a democratic society or as a monolithic manifestation of state oppression, rendering ruin at every turn. The law is a multitude of things, some benevolent, some oppressive, and everything in between. This is exactly why in-depth and thoughtful study of the ways in which the law is enacted on an everyday basis is necessary. We need it to uncover how everyday people in courthouses all over the country employ the law in mundane, often boring, ways. Ethnographic study of the law helps us answer important questions about how the law actually functions regardless of its intent. It helps us answer all kinds of different questions such as: How does a law get enacted in generic courtrooms by human beings, against other human beings within their own local contexts? How does it change across different judges, cities, or decades? What does racial/gender/class bias look like in a given courtroom, and *how* is it produced or reproduced knowingly or unknowingly by court actors? *How* is discretion operationalized by court actors to oppress or to push back against oppression? *How* is retribution, rehabilitation, or even restoration facilitated by judges, prosecutors, or defense attorneys? What frustrates, motivates, or demoralizes those whose work is to apply the law? What is the language they use to describe what it is that they do, or what they think they do, or what they hope to do?

Over years of doing such research, I have learned that courtrooms, like any ethnographic field site, have a rhythm, a cadence, a language, and a culture unique to them. You must stick around long enough to learn and become comfortable within that rhythm and to be fluent in the language. Court actors can sometimes be slow to trust. You will need to demonstrate that you are willing to take the time to learn about their worlds. You will need to develop a working understanding of the court's culture. You must also understand the law. This does not come quickly unless you already have a law degree.

Taking the necessary time in the field to do all this well is rarely conducive to publishing trajectories, graduation plans, reappointment and tenure files, or research or funding proposals that state that you will do X in Y length of time. The demands of graduate programs and the academic workplace are real, and I do not mean to cast them aside lightly; they are the reality we all must operate in. However, for ethnographers, the "field" and the process of discovery, not the demands of the modern neoliberal university, should dictate the timeline, wherever and whenever

possible. I was trained by many excellent ethnographers. They taught me that to do ethnography well, you needed to prepare and plan and then be willing to rethink, readjust, and even abandon that plan once in the field. The initial planning helps keep you grounded as you adapt to changes once in the field, changes that often require more time than you originally anticipated. For many this uncertainty is what they find so difficult about ethnographic research. But the uncertainty is, in fact, the strength of ethnography. It is only within the uncertainty that you learn things you didn't know you needed to learn and start to ask the questions you never even knew needed to be asked. This organic process of discovery is the very nature of ethnographic inquiry. And discovery takes time. I would love to be able to provide some sage advice about how to manage all that and how to do quality ethnographic work in a timely fashion and make advisors and tenure committees happy. I can't, but I know that, somehow, I have managed to earn a PhD, publish a book, and get tenure. So it is possible, certainly not easy, but possible.

Altogether I spent 3 years in the Youth Part, observing calendar days, which were almost always on Friday. I would arrive to court before the judge and take a seat in the back row of the unused jury box (actual trials were extremely rare) with a notebook and pen. Friday sessions were generally three to four hours long. I filled up numerous notebooks. I also conducted a number of open-ended interviews with the judge, members of his staff, defense attorneys, and alternative to incarceration program representatives. I only managed to get one interview with one representative from the district attorney's office after making repeated requests for over a year (you win some and you lose some).[4]

An Interdisciplinary Approach

Since I had never set out to study courts, I was, in many ways, woefully unprepared to do so. Other than my qualitative methods classes, one class on criminological theory and another on the sociology of punishment, not much of my 60 credits of graduate sociology coursework had prepared me to study the law in action. I read several court ethnographies—those written by academics (Malcom Feeley's *The Process is the Punishment*, M.A. Bortner's *Inside a Juvenile Court*, Robert Emerson's *Judging Delinquents*, Aaron Kupchik's *Judging Juveniles*) and those by journalists (Edward Humes' *No Matter How Loud I Shout* and *Courtroom 309* by Steve Bogira). There were times that some of my fellow ethnographers, when hearing about my research, questioned whether court ethnographies are really true ethnographies since they really involve more observation than any *real* participation. Reading these court ethnographies provided me and my detractors proof that court ethnography was real ethnography. They also demonstrated what court ethnography was, how it could be written well, and why it is necessary work for understanding the law and criminal

---

[4] In later research projects, I have had much more success getting interviews with prosecutors, although I have often had to wait patiently for okays from higher ups.

justice policy. I undertook a self-directed immersion study of the sociology of law literature over 2 months in my living room, after asking a professor in the department where I was adjuncting who was an expert on the subject for her suggested list of required readings. It was here that I began to learn all the works that had been done on discretion, normal cases, procedural justice, and legal cultures.

I also spent a lot of time in law libraries and developed skills in reading and comprehending penal codes, criminal procedure law, and legislation. I learned to decipher the (maddening) footnote style of law journals and to understand Supreme Court decisions and dissents (not just their lay summaries). By the end of my time in the field, one of the public defenders from the court joked that I deserved an honorary law degree given how far I had gone to understand the relevant statutes and their histories and all the legal rules I witnessed in action in the court. In my experience, judges, prosecutors, defense attorneys, clerks, and other court personnel generally don't have much use for academics who come to study their work. Their respect and trust must be earned, and it is earned by taking the time and doing the detailed work to understand well, and genuinely care about, the world they inhabit.

Court ethnographies tell important stories about the particular courts they study, but when done well, they also tell important stories about the law more broadly, about crime and justice policy more broadly, and about the nature of society. My ethnographic study of the Youth Part became a lens through which to interrogate the uniquely harsh American practice of prosecuting adolescents as if they were adults. The stories of the kids in court were also stories about policies imbedded in the retributive turn and the ongoing criminalization of youth and race and poverty. To tell that story, I had to read up on the legal and social history of juvenile justice in the USA and New York. I had to get familiar with the criminology literature on juvenile crime and violence trends since the 1960s as well as literature on moral panics, transfer law legislation, and studies on transfer laws' deterrence effects (or lack thereof). I had to have a working understanding of the literature out of psychology and neurology on adolescent brain development, decision-making, and culpability. If one is studying law or the criminal justice system in the USA, one must also be studying race (and vice versa). Therefore, a deep dive into critical race theory (of which I had only tangentially been exposed to in any of my graduate classes) was crucial.

Court Narratives and Counternarratives

Ethnography is as much about the field research as it is about the writing. I am proud of some of what I tried to do on both fronts. Courts are often dehumanizing spaces. If not done with care, research conducted inside of them and the writing up of that research can end up replicating this dehumanization. If done well, court ethnographic research and writing can expose that dehumanization and reveal the intricacies of how processes of dehumanization take place and where and how they are resisted. As part of my telling of the court's story, I aspired to find ways to not

replicate dehumanization in my presentation of the research. I strove to depict defendants and court actors alike as human as possible, both in my field notes and in my final writing. I had often seen in previous qualitative studies of courts that people brought before the criminal court were routinely referenced as "offenders." Other times they were referenced simply as "defendant." It was not uncommon in academic texts to see something like the following as a way of depicting court interactions:

Judge: What do you have to say about this?
Offender: I have nothing to say.

I actively sought to portray the defendants I saw in court, and other court actors, differently. I wanted the reader to see them as human beings, which they were. I purposefully included physical descriptions, including descriptions of gesture, posture, and other nonverbal information. I took endless notes on all the details I could while sitting in court. I developed a shorthand, a sort of code, that allowed me to take faster notes. Commonly used terms in the court were assigned one or two letter codes, as were certain repeating court actors. This freed up time and space for noting gesture and emotion, allowing for richer descriptions when written up.

I gave defendants pseudonyms. I looked for ways in my writing to convey that Jeffrey was shy and acquiescent and that Javier was polite but could sometimes be defiant. I referred to the kids before the court as often as possible as "kids" because that was how most people in the court referred to them. I insisted on using the term "defendant" rather than "offender," which is an inaccurate and loaded term which labels all court defendants as having offended, reproducing a presumption of guilt. I noted, in my field notes and in my writing, the racial differences in the court—how most all the kids were black and brown and so many of the lawyers and program representatives were not.

Given the ways in which the young defendants in the Youth Part made repeated, often monthly, appearances in the court over a 3-, 6-, or 9-month period, I began to separate out my field notes into a separate file for each kid. In so doing, I was able, over time, to construct "court narratives" for many of the young defendants and was able to follow their individual stories across their time in court. Through an analysis of each of the narratives, I was able to show how each kid's journey through the court exemplified the court's attempt to revive a type of individualized justice that harkened back to the rehabilitative intent of the early juvenile courts (for the purpose of reducing the impact of adult prosecution where possible). In my writing, I made a conscious decision about how to present my arguments about this. The usual way of presenting qualitative data is to state the claim and then back it up with pieces of evidence, state the next claim, provide other pieces of evidence, etc. A snippet of Javier's narrative or a snippet of Jeffrey's might be used to validate one claim, while other snippets of their narratives might be used as evidence for a claim elsewhere. I knew to follow this pattern would mean chopping up the court narratives I had constructed which had been so useful for understanding how the court operated. So, I went another way. I chose to present a court narrative intact (e.g., telling Jeffrey's court narrative) and then discussed how it verified various claims I wanted to make about the court's ideological orientation to case processing. I found

this to be difficult writing, and I am still not convinced that I did it well, but I remain convinced of the intent of this approach and of my attempt to find new ways of writing about people within criminal justice system settings that do not reproduce dehumanization or reduce a person's real-life experience of the system simply to pieces of evidence to support my academic argument.

Upon reading a final draft of a chapter in which I deployed this method, a PhD advisor who had given me an amazing amount of useful advice over the years told me that the way I had chosen to present my data was a bad idea, suggesting that was just not the way it was done. I pushed back, explaining my methodological and representational reasons for trying something new. He countered, with some frustration, that while that might be okay for my dissertation, it would not get published that way. A few years later, the book was published that way after going through two separate rounds of peer review. Not a single reviewer, nor my editor, ever took issue with it. In fact, one compliment the book often receives is its court narrative approach. The moral of that story? Take the risks necessary to chart new territory, and always write the story that is true to the field.

I never set out to study courts, but that is where the questions I sought answers for led me. I continue to study courts because my love affair with the law in action has never ended. I can still be riveted by an hour in an arraignment court or moved by the ways the failings of criminal justice policy reveal themselves in a Bronx summons part on a Thursday morning in May. I continue to be enthralled by how judges, prosecutors, and defense attorneys talk about their work when anyone takes the time to listen. It is from the study of the mundane and ordinary, often boring (to some), spaces where the law lives that we gain important and much needed insights into what the law actually is, what it is not, and what it should be.

Reference

Ludwig, F. (1955). *Youth and the law: Handbook on laws affecting youth*. Brooklyn, NY: Foundation Press.

Problematizing School Discipline and Struggling for *Verstehen*

John J. Brent

Introduction

Admittedly, I was initially excited to write a chapter focusing on the contextual matters of ethnographic research and to note the "why" of topic selection, the relationship between the research and researcher, and the complexities associated with fieldwork. After all, many would agree that these are all important aspects of qualitative work. Interestingly, these issues often receive little attention in methodological texts or are hidden from the routinized organization of peer-reviewed articles. My initial enthusiasm, however, diminished a bit after asking myself "what in the world could a new faculty member add to the already rich discussion within qualitative criminology"—especially given the list of well-known and established ethnographic scholars within this volume. The task, once exciting, quickly became intimidating.

To get some bearing, I turned to my past work and looked through written field notes, photographs taken from the field, recorded interviews, analytic notes, and thematic coding strategies. I cringed at the number of misplaced interview questions, premature topic changes during conversations, missed opportunities for follow-up/probing questions, and attempts to smoothly guide interviews. Nevertheless, these data afforded tremendous insight into lived realities, emotive underpinnings, and individual motivations. Perhaps more importantly, I saw that the ethnographic process of discovery and understanding is much more dynamic and complicated than the methodological "nuts and bolts" outlined in many textbooks and publications.

For instance, criminological scholars often note the importance of *verstehen* in qualitative research and ethnographic work (see Ferrell, Hayward, & Young, 2008).

J. J. Brent (✉)
School of Justice Studies, College of Justice and Safety, Eastern Kentucky University, Richmond, KY, USA
e-mail: John.Brent@eku.edu

Borrowing from Weber's (1978) conceptualization, *verstehen* refers to the interpretive process of gaining an empathetic understanding of the motivations that give meaning to individual actions. Despite its significance, little work highlights the complexities associated with achieving a culturally empathetic appreciation. That is, outside of discussing ethnographic methods that facilitate *verstehen*, little work addresses what this process may look and feel like. Further, few efforts outside of positivistic criminology highlight how social, cultural, and political ideologies that create and perpetuate realities play a significant role in gaining or hindering an appreciative understanding.

Therefore, this chapter offers a self-reflection on the interpretive process of gaining an empathetic understanding that is fundamental to ethnographic work. More specifically, I draw on my experience conducting ethnographic work exploring the institutional culture underpinning the punitive practices and policies associated with contemporary school discipline. Therefore, the following pages highlight the difficulty and complexity I experienced while attempting to gain an empathetic appreciation of the ideologies behind exclusionary school discipline—especially when the institutional systems of meaning were in opposition to empirical evidence, standing theories, and my own personal frameworks.

In order to accomplish this, this chapter first contextualizes qualitative research historically and methodologically while also highlighting the significance of *verstehen* for ethnographic methods. Afterward, I discuss how my interest in the punitive currents of school discipline developed and became an object of study. The following sections outline my experiences trying to gain an understanding of school discipline from the perspective of both students and staff. These pages also acknowledge the aforementioned complexities in trying to achieve an appreciative understanding and avenues taken to better facilitate the process. Finally, the chapter concludes by outlining how striving for *verstehen*, no matter how difficult, can lead to unexpected findings, novel insights, and a true appreciation – even though it may conflict with one's own position.

Qualitative Criminology and Centering *Verstehen*

Though present during the nineteenth century, qualitative methods began to prosper within the social sciences during the early twentieth century. During this time, criminologists and sociologists within the Chicago School began moving away from strict positivistic notions of science that emphasized pure objectivity, universal realities, social determinism, value-free research, and studying objects from a distance. Rather, those within the Chicago School espoused a more interpretive philosophy of social science that strived to understand peoples' lived experience in their natural settings. This alternative approach sought to uncover social constructions of reality and appreciate culturally significant systems of meaning. As a result, the interpretive social science championed immersive methods that advanced the importance of

uncovering lived experiences, individual subjectivities, theoretical developments firmly grounded in data, and a sensitivity to perceptions and values. Since our relatively young field is replete with scholarship enlisting ethnographic work, direct observations, and depth interviews to study and understand the sociocultural, political, and economic contours of crime (see Charmaz, 2006; Copes & Miller, 2015; Ferrell et al., 2008; Glaser & Strauss, 1967).

At this point, one cannot help but to recall Jock Young's (2011, p. 180) work as he eloquently identifies two different epistemologies within criminology. Consider the following excerpt from his book, *The Criminological Imagination:*

> There are two criminologies: one that grants meaning to crime and deviance, one that takes it away; one which uses an optic which envisages the wide spectrum of human experience: the crime and law-abiding, the deviant and supposedly normal – the whole round of human life, the other a lens that can only focus on the negative, the predatory, the supposedly pathological...

It is important to note that Young moves beyond simply identifying two distinct methodological paradigms—as many have. More importantly, he argues that each paradigm reflects—and is shaped by—their discrete criminological purpose. That is, he problematizes the quantitative canons of knowledge production in criminology that abstract lived experiences into numerical form, forces them into existing theoretical constructs, and analyzes them for statistical significance. In their place, he discusses the importance of qualitative methods that seek an appreciation of crime and its meaning, a qualitative criminology that attempts to understand the variety of motivations and rationalities associated with crime and crime control.

This brief history and peek into the interpretive philosophy provides the context to understand the concept of *verstehen*—one of the core components of qualitative methods and ethnographic research. For Weber (1978), *verstehen* refers to the process of gaining a deep empathetic understanding of social behavior. When studied further, it represents an interpretive and systematic understanding of the motivations and meaning that underpin individual's purposive action. More specifically, Weber states that "empathetic or appreciative accuracy is attained when, through sympathetic participation, we can adequately grasp the emotional context in which the action took place" (Weber, 1978, pp. 4–5). When situated in criminology, Ferrell (1997, p. 10) writes that:

> criminological *verstehen* denotes a researcher's subjective understanding of crime's situational meanings and emotions…implies that a researcher through attentiveness and participation, at least can begin to apprehend and appreciate the specific roles and experiences of criminals, crime victims, crime control agents, and others caught up in the day-to-day reality of crime.

It should come to no surprise then that—given their ability to uncover the emotive frameworks, motivations, and situated meanings associated with crime and crime control—ethnographic methods represent a cornerstone among qualitative methods and interpretive-based research (Ferrell & Hamm, 1998).

Cultivating Interest in School Discipline

Whether a result of background factors, academic training, or personal interest, my studies have generally tracked along two dominant threads. The first thread takes a critical approach to studying and theorizing criminal justice and crime-control initiatives. While an impressive literature tries to understand the why of crime, this focus tries to understand the why of criminal justice. The second thread attempts to uncover and note the cultural and structural conditions that give meaning, purpose, and significance to "deviant" conduct. As someone who studies the mechanism of social and formal control, I was fortunate enough—even lucky—to link up with a number of mentors and colleagues examining the punitive shift in school discipline. They were asking questions such as: What are the school- and student-level consequences associated with punitive school discipline (see Kupchik, 2010)? How do high-security environments in school impact family engagement and involvement (see Mowen 2015)? What are the collateral consequences associated with criminal justice-based policies in school? Are contemporary disciplinary regimes impacting all students equally (see Hirschfield, 2008)? I, however, became interested in exploring the institutional culture underpinning the punitive policies associated with contemporary school discipline.

Knowing little about the subject and trying to get "on par," I turned to the literature. Already averse to punitive crime-control initiatives, it was disturbing to recognize that the practices and policies once reserved for criminal justice had become commonplace in schools around the country (Robers, Zhang, Truman, & Snyder, 2013). At one level, schools were increasingly adopting invasive security measures such as surveillance systems, metal detectors, armed police officers (or school resource officers), and drug-sniffing dogs (Casella, 2006; Robers et al., 2013). At another level, disciplinary measures appeared to be more punitive given the increased use of zero-tolerance policies, expulsions, and in- and out-of-school suspensions (Fabelo et al., 2011; Kupchik, 2010; Robers et al., 2013). Still further, these trends were instrumental in creating a school-to-prison pipeline that disproportionately impacted racial/ethnic minority youth, students identifying as a gender and sexual minority, and those from economically disadvantaged backgrounds (see Hirschfield, 2008; Losen & Martinez, 2013; Mitchum & Moodie-Mills, 2014). Having attended high school in a time when hardly any of these features were used, it was bizarre to read reports of students being removed from school under zero-tolerance policies for relatively minor infractions. As Hirschfield (2008) and Kupchik (2010) identified, the "criminalization of school discipline" reshaped disciplinary climates that, in turn, helped reframe student misconduct as being a potentially criminal matter.

From here, it was easy to question why schools were becoming sites of punitive discipline and enhanced security. Many are quick to point out the occurrence of highly publicized school shootings over the last two decades (see Addington, 2009),

while others are crystalized in the hearts and minds of many, such shootings include Columbine High School, Colorado (1999); Red Lake High School, Michigan (2005); Virginia Tech, Virginia (2007); Sandy Hook Elementary School, Connecticut (2012); and the University of California, Santa Barbara (2014). Even as this chapter is being drafted, there have been shootings at Marjory Stoneman Douglas High School, Florida; Marshall County High School, Kentucky; and Aztec High School, New Mexico. However, with school crime reaching near historic lows, scholars have interpreted the criminalization of school discipline alongside structural-functional theories, neoliberal conditions, shifting models of crime control, and general fears and anxieties (for a thorough overview of theoretical explanations, see Hirschfield & Celinska, 2011; Kupchik, 2010).

Overall, this body of scholarship had well-documented and theorized the escalation of punitive disciplinary practices. Further, it exposed a host of negative outcomes associated with contemporary school discipline. This literature had also shed some qualitative empirical light on the perceptions and experiences with discipline and security. However, little work had explored the institutional culture associated with this new disciplinary regime. Relatedly, research had yet to gain an empathetic understanding of the motives and rationalities driving established disciplinary structures.

Pursuing an Empathetic Understanding

Reflecting on My Role and Disposition

For the discussed project, I conducted an ethnographic case study of a mid-Atlantic high school which has been given the pseudonym Easton High. It is important to note that my involvement within Easton began well before the start of the study. That is, I volunteered at the school given its probationary status and imposed "at-risk" label attached to many of its students. During this time, I had routine contact with school administrators, teachers, staff, and students. Interestingly, the exclusionary disciplinary practices associated with Easton's school district had been under scrutiny given its frequency and disproportionate application. As a result, the school was forced to alter its disciplinary code and build in remedial measures. And, as prefaced above, I went into studying school discipline with a certain knowledge gained from the literature. The message was consistent and conveyed that current disciplinary practices had increasingly relied on punitive sanctions that resulted in a host of negative outcomes at the student and school level. This certainly bolstered my own critical orientation toward examining the social and formal mechanisms of punitive governance.

Discourses Against and For Punitive Discipline

Although I thought participants would be hesitant to discuss the disciplinary culture of the school, they were often receptive to such conversations. Many participant narratives, especially those from students, echoed the literature documenting a qualitatively and quantitatively new disciplinary culture. Consider the following from Ronnie—a student at Easton High:

Yeah man, it's like they just want to punish you and go all hard all the time. They are doing too much, being all extra with students.

Bree—a senior at Easton—also shared her perspective but spoke more broadly about the school's overall disciplinary climate:

If you see a good kid walking out in the middle of the street you're gonna save them or say don't go out there. But, if it's a kid you don't like, you let them walk out there and get hit. Instead of steering them in right direction, you are only punishing them so you are not helping them or telling them to get out of the street. You're pushing them out there.

And Paul, another one of Easton's students, offers a narrative suggesting that discipline has become a chief organizational principle for the school:

This school is all about punishing kids. I mean the bad kids, they are going to get more attention. But, the good kids aren't getting enough good attention because everyone is so focused on the bad kids or that Easton High is a bad school.

Even school officials, like Mrs. Brooke, seemed to critique the school's standing disciplinary practice in favor of more evidence-based and restorative approaches:

Yeah, sure, sometimes the discipline can be harsh. But you know what, sometimes it's needed. The issue is what is being taken away when we punish these kids. Our purpose here is to educate students, create a safe environment, and help develop good people. Hard discipline can get in the way of that. We have to learn to make discipline an applied practice where students learn a lesson and know that we care about what they are doing. I think that second part gets lost a lot. If they know we care, then the discipline means something and it sinks in with them. If they don't know, then we punish them for no reason and we lose them.

I do not provide this small sample of narratives and data points to discuss what the ethnography "found"—that is the task for articles. Instead, I highlight these few excerpts to illustrate some of the individual positions, values, and experiences that give meaning to the disciplinary climate of the school. More importantly, I highlight these examples because they aligned with existing research, theoretical frameworks, and my own acknowledged disposition toward punitive discipline. As a result, pursuing *verstehen*—the interpretive process of gaining an appreciation of the subject position—followed with little difficulty.

While narratives calling out and challenging Easton's disciplinary regime were easy to understand, there were other positions that were more difficult to empathize with. For instance, many of the school staff expressed discontent with the changing nature of school discipline. Disciplinarians often noted that Easton's discipline "is watered down," "coddles students," and "is a joke." Mr. King, a teacher, argued that Easton needed to move away from "soft" discipline if it wanted to correct its disciplinary issues. More specifically, he stated:

I think that if our school, man…the District needs to let us drop the hammer on these kids. I bet it would only take 3 months of getting real.

Mr. Jackson, a disciplinary official, similarly voiced that:

Everybody's being babied nowadays. I mean, discipline is so watered down in this building. I think we need to get back to real discipline.

These and similar conversations were interesting. One the one hand, it was understandable that the shifting disciplinary mandates—due to the aforementioned scrutiny at the district level—created some discontent. On the other hand, it was difficult to understand the resistance toward practices and policies that were more restorative and remedial.

Again, I do not highlight these data to demonstrate what was found. Rather, these are presented to illustrate subjective positions that counter those above critiquing school sanctions. And just as the interpretive social science asks that I understand subjectivities challenging discipline, I too had to try and understand these. However, I found it very difficult to empathize with those advancing exclusionary discipline. It was tough to appreciate why school personnel would resist corrective and restorative approaches that were empirically supported.

Agreeing with Versus Understanding

Of course, sociology and criminology are full of ethnographic studies in which scholars study social phenomena they don't approve of in order to better understand them. And though it was one thing to be aware of this, it was another to actually feel and navigate the disjuncture between agreeing with something and understanding it. In the case of school personnel favoring punitive models of school discipline, it was difficult to have empathy for something my training, empirical evidence, and own disposition opposed. To overcome this, I began shadowing Easton's disciplinarians, following the daily routines of teachers, and talking further with school administrators—in essence, engaging ethnographic and interpretive methods further.

After doing so, I started to recognize realities previously hidden and better understand staff's motivations. For instance, it became apparent that the disciplinary culture among much of the staff was structured by institutional inertia, resources, and demands. Consider the following field note:

Today I spent a lot of time shadowing Mr. Jackson…He talked about how he and the other SCAs serve as disciplinarians for the school while also being a social resource for students. Interestingly, he discussed how he wants more time and resources to build relationships with students, start community engagement initiatives, and engage students in positive situations. While stressing this importance, I asked what he was filling out. He replied "Oh about 75 referral slips for detention, ISS, Saturday school, and time-out"….

This point was also brought up by Mr. Camp, another disciplinarian at Easton:

Sure, building relationships, outreach initiatives, and EBPs (evidence-based programs) sound great. I would love to do those things. But I am forced to be a disciplinarian first.

When asked whether new mandates were having an impact on his job, he continued:

Yeah, I have to wear the hat as a SCA and counselor but how, with what, and when? There is always a fight, skipped class, misconduct investigation, and mediation to oversee. It's always something that forces us into the discipline role.

Mr. Vincent similarly explains:

Not all schools have the resources so they are forced to use what they have. There might be best practices or EBPs but those are not feasible with what's going on here. These reforms mandate supportive climate initiatives but to make that transition requires a lot of resources we don't have.

Coming from more attentive methods, these data suggest that staff's subjective positions and rationalities were intimately bounded by their experience with institutional constraints. Stepping back further, this also suggests that students' often negative take on school discipline was also likely bounded by their experience with institutional currents and dictates—an insight that may have gone unnoticed.

Discussion

The pages above reflect on an ethnographic case study examining the institutional culture underpinning school discipline and punishment. More specifically, it sought to offer personal experiences related to the interpretive process of gaining *verstehen*—an empathetic understanding that is fundamental to ethnographic and qualitative work. In doing this, one can realize that the process of appreciating lived experiences, emotive frameworks, and individual motivations is much more dynamic and complex than often written about in texts, publications, and peer-reviewed articles. One can also see how the relationship between the researcher and research topic can impact this process. Likewise, it is evident that various ideological positions and institutional constraints have a significant ability to structure individual realities, narratives, and systems of meaning that must be considered.

Given this, the task of acquiring a deep empathetic understanding may be more difficult, value laden, and complex than often written about or discussed within publications. For instance, I found it challenging to empathize with agents advancing exclusionary discipline and punitive sanctions. I also found it tough to appreciate why school personnel would resist corrective and restorative approaches that were remedial and empirically supported. Consequently, it was difficult to have empathy for something so opposed. However, by recalling prior ethnographies facing similar issues and engaging ethnographic methods further, I was able to better appreciate where participants were coming from—though still being at odds with many of their practices and policies. And though I was able to move closer to understanding and gain an appreciation, I still question the degree to which I would be able to fully grasp their subjective positions.

Though difficult, pursuing this interpretive-based understanding was significant along a number of lines. First, and already alluded to, it afforded greater clarity as to how individual and institutional level factors produce, and are impacted by, normative structures of discipline and punishment within schools. Second, and related, these grounded data—without doubt—helped develop organizing concepts and theoretical frameworks that can be beneficial for research and practice moving forward. Third, and perhaps most importantly, these grounded insights speak to opportunities for policy implication, openings for meaningful change, and chances to alter negative conditions so as to create better outcomes for those mostly affected by current disciplinary regimes.

References

Addington, L. A. (2009). Cops and cameras in public school security as a policy response to columbine. *American Behavioral Scientist, 52*(10), 1426–1446.

Casella, R. (2006). *Selling us the fortress: The promotion of techno-security equipment for schools*. New York: Routledge.

Charmaz, K. (2006). *Constructing grounded theory: A practical guide through qualitative analysis*. London: Sage.

Copes, H., & Miller, J. M. (Eds.). (2015). *The Routledge handbook of qualitative criminology*. New York: Routledge.

Fabelo, T., Thompson, M., Plotkin, M., Carmichal, D., Marbanks, M., & Booth, E. (2011). *Breaking schools' rules: A statewide study of how school discipline relates to students' success and juvenile justice involvement*. New York: Council of State Governments Justice Center and Public Policy Research Institute.

Ferrell, J. (1997). Criminological verstehen: Inside the immediacy of crime. *Justice Quarterly, 14*(1), 3–23.

Ferrell, J., & Hamm, M. S. (Eds.). (1998). *Ethnography at the edge: Crime, deviance, and field research*. Boston: Northeastern University Press.

Ferrell, J., Hayward, K., & Young, J. (2008). *Cultural criminology*. London: Sage.

Glaser, B. G., & Strauss, A. L. (1967). *The discovery of grounded theory*. Chicago: Aldine.

Hirschfield, P. (2008). Preparing for prison: The criminalization of school discipline in the USA. *Theoretical Criminology, 12*, 79–101.

Hirschfield, P., & Celinska, K. (2011). Beyond fear: Sociological perspectives on the criminalization of school discipline. *Sociology Compass, 5*(1), 1–12.

Kupchik, A. (2010). *Homeroom security: School discipline in an age of fear*. New York: New York University Press.

Losen, D. J., & Martinez, T. E. (2013). *Out of school and off track: The overuse of suspensions in American middle and high schools*. Los Angeles: The UCLA Center for Civil Rights Remedies at the Civil Rights Project.

Mitchum, P., & Moodie-Mills, A. C. (2014). *Beyond bullying: How hostile school climate perpetuates the school-to-prison pipeline for LGBT youth*. Washington, DC: Center for American Progress.

Mowen, T. J. (2015). Parental involvement in school and the role of school security measures. *Education and Urban Society, 47*(7), 830–848

Robers, S., Zhang, J., Truman, J., & Snyder, T. (2013). *Indicators of school crime and safety: 2013*. Washington, DC: US Department of Justice, Bureau of Justice Statistics and US Department of Education, National Center for Education Statistics.

Weber, M. (1978). *Economy and society: An outline of interpretive sociology* (Vol. 1). Berkeley, CA: University of California Press.

Young, J. (2011). *The criminological imagination*. Cambridge, England: Polity Press.

The Sense and Nonsense in Planning Ahead: The Unanticipated Turns in Ethnographies on Crime and Drug Dealing

Sandra M. Bucerius

I stumbled upon my research topic by accident. As an undergraduate student, I had spent countless hours working as a research assistant (RA) on a project my future supervisor was directing: interviewing drug users about the crack epidemic. While I enjoyed the RA work, I was set on becoming a psychoanalyst for children and teenagers and had been working part time (and sometimes full time, when my studies allowed) at different psychiatric institutions in Frankfurt, Germany. When my future supervisor encouraged me to pursue a PhD, I was clueless about what that would entail. I had never thought about furthering my studies beyond a masters and had my future planned out. Regardless, the idea of a PhD intrigued me, especially, because my eventual supervisor suggested I could mimic some of the American ethnographers like Philippe Bourgois (2003) or Lisa Maher (1997) who have looked at drug dealers. Scholarly knowledge on drug dealers in Germany was nonexistent, and becoming the first German researcher to study drug dealers in Germany sounded exciting to my 22-year-old self. Plus, having a PhD as a psychoanalyst would not hurt, I thought to myself.

Crime ethnographies are not common in Germany, and I could not find any Chicago-style crime ethnographies in the German context to prepare me for my research. However, having read a few American ethnographies, I thought they played on my strengths. Building strong rapport with people, yet remaining emotionally distant enough to analyze the data, or in Mary Douglas's (1966) words to be "stranger and friend," is something I did at the psychiatric institution all the time. Moreover, I figured I could build rapport with drug dealers relatively quickly, given my considerable experience working with teenagers who showed signs of "defiant behavior," as psychiatrists would call it.

S. M. Bucerius (✉)
University of Alberta, Edmonton, AB, Canada
e-mail: bucerius@ualberta.ca

I could not have been more wrong. Thinking back on it, I believe my naiveté was partly due to my young age when entering the field and also a function of my lack of supervision when it came to ethnography. With no urban ethnographers in my faculty, there was no one "experienced" I could ask any of the questions my own graduate students run by me all the time. For example, "how do you best build rapport in the first few days?", "what do you do when your participants ask you to hold drugs for them when the police are approaching?", "how much do you share about yourself when trying to build rapport?", "did you ever feel compelled to report any crimes to the police?", etc. Not having a role model for ethnographic research and no peers engaging in similar projects, I had to rely on my gut instincts and "book knowledge" to navigate the field.

I quickly learned that having experience working with defiant youth in a psychiatric hospital does not translate into having an easier time establishing rapport with the 55 young men who eventually made up my core participants for my book *Unwanted—Muslim Immigrants, Dignity, and Drug Dealing* (Bucerius, 2014). Working at the psychiatric hospital, I could always rely on my colleagues having my back and protecting me if necessary. The camera system would pick up any violence directed against me, and there was a clear power hierarchy elevating the employees above the patients. At the end of the day, I was always in a position of power. Doing ethnographic research, in contrast, requires building rapport with people who might be opposed to your presence and where you are not supported by a clear power hierarchy. It is an almost completely different endeavor. That said, I benefitted in many ways from taking a trial-and-error approach.

We oftentimes assume that detailed planning of research will lead to success with our project and potentially minimize risks. Planning ahead and preparing for every, often extremely remote, possible scenario are required by research ethics boards and, in the case of graduate students, by dissertation committees. Researchers usually have to submit a detailed plan detailing their research objectives and chosen methodologies. Funding agencies are reluctant to fund a project where the researcher proposes to "find something interesting" once in the field and therefore intends to let the data guide the study. However, good ethnographic studies often depend on exactly such happenstance. Research takes unexpected turns, and much of what we do, who we gain access to and build rapport with, as well as what we ultimately study is based on chance encounters and circumstances we cannot predict beforehand.

In this chapter, I describe some of the ways my ethnographic work took unexpected turns and did not play out the way I, or my professors, had anticipated. Ultimately, I argue that while preparation is obviously useful, in reality much of what will happen in ethnographic research depends on trial and error and unfolds in unexpected ways. My hope is that by highlighting this fact, budding crime ethnographers feel more at ease when their research does not develop in the way they had initially envisioned. The most interesting findings are usually unanticipated.

Planning Ahead: The Overemphasis on Risk

When I started my fieldwork on drug dealers, I had never before hung out with dealers. I figured gaining access to the population I wanted to study would be easiest if I went through a community youth center. It was common knowledge among university students in Frankfurt that these centers were hangouts for young people engaged in some sort of criminal activities on the side—and drug dealing was frequently associated with these locations. By coincidence, I read an advertisement on my department's bulletin board looking for an intern at the community youth center located in the same neighborhood as my university. I knew nothing about the place, but I figured it might be worth inquiring whether this center could be an appropriate fieldsite. A friend's father who worked for the police confirmed this youth center was known as a hangout for drug dealers. By complete happenstance (seeing a flyer on a bulletin board), and without any active planning on my side, I had identified the place where I would spend a considerable proportion of the next 5 years.

Eager to make a connection, I called the center and had a long talk with the director about my research plan. I offered to work as an intern if allowed to hang out there. As I have described elsewhere (Bucerius, 2013), the director generally supported using his community youth center as an access point but tried vigorously to convince me to study something related to young girls. His youth center also had an after-school program for girls aged 10–14 in a separate building—and it was there he wanted me to conduct my research. He stressed that the part of the community youth center where the young men hung out was a "dangerous space" for a woman and that no woman had entered that part of the building in more than 10 years. As a consequence, the social workers were skeptical about my research plan—they were worried about my safety in a space occupied by hypermasculine and often violent men. They were also worried they would not be able to protect me if something happened and said as much to me before I started my research. I also believe they were uncomfortable with the prospect of losing their status as gatekeepers to these young men; me being around and getting to know "their" young men meant they might lose any prestige derived from being the only people who had access to, and detailed knowledge about, the population. While I was eventually able to convince the gatekeepers to allow me to use the community youth center as my research site, building rapport with the young men who hung out there was far more difficult than I had anticipated. In the following section, I discuss five unexpected turns in my research.

Protecting the Data from Interested Police Officers

Before entering the field, I met with my supervisor and another professor, an ex-police officer, to discuss ethical issues that could arise and how I might deal with such eventualities. German universities at the time did not have dissertation committees as is common at North American universities. I consequently did not have the opportunity to talk with a committee about research-related matters or raise

questions about my project, as commonly occurs with my own students. In Germany, the committee examining a student's dissertation is formed *after* the PhD student submits the completed thesis and consists of professors often entirely unknown to the student. Professors only read and evaluate the "final product" and examine the student based on that in the oral defense.

During the initial conversation with my supervisor and the other professor, we determined how I would best store my field notes and interviews to protect my participants in case my research became of interest to the police. I worked out a meticulous system of anonymizing my field notes even at the writing stage and for storing field notes, interviews, and transcripts at three different locations. In case the police would raid my apartment (something I thought was unlikely, but my professors weren't so sure), the police would only ever have access to one of the data sources. The others were stored at the university and at a friend's house. In contrast to many North American universities, where PhD students have offices and access to locked file cabinets, this option was not available for me—I had to rely on a professor storing my notes in his office.

During our initial conversation, I was also advised on how to handle potential police intervention. We carefully planned out the "what if's" should the police become involved, but none of these scenarios ever came to pass. Instead, my presence among the young men seemed to deter police from asking for their identification (also known as "carding"), and there were few instances during my 5 years in the field where officers approached my participants while I was with them. This was in stark contrast to what Bourgois (2003) describes about his experiences in East Harlem in his book *In Search of Respect*. To this day, I wonder whether the gender component played a role in this or if the police made a conscious decision to leave me and my participants alone when I was around. In fact, the few times the local police did approach my participants when I was present, the officers were usually extremely friendly, and the encounters did not reflect the negative experiences my participants often described when telling stories about their relationships with the police. I did observe one incident where a tactical team stormed the youth center, but the officers focused exclusively on the one person they came to arrest, ignoring everyone else, including me. I was never asked for my notes, nor did an officer ever pull me aside and ask for information. In other words, while I was prepared in detail to handle any police encounters, I luckily never had to put those plans into action.

I was much less prepared for how to actually handle my day-to-day interactions with my participants. In focusing on the extraordinary "worst possible" scenarios, like having my notes subpoenaed, we forgot to contemplate the mundane, but arguably more important, aspects of my ethnography, such as how I would engage the young men in my study.

The First Few Days

While over-prepared for handling law enforcement, I was utterly underprepared when it came to interacting with the young men in the field. Naturally, when I first set foot in the community youth center, I didn't have a real vision of how fieldwork there would unfold and what kind of drug-dealing activities I might observe. Having dealt with many difficult children and teenagers in my various jobs, however, I thought I was reasonably prepared to build rapport with drug dealers—who I originally viewed as not too different, albeit older, than some of the teenagers I encountered in other areas of my life. However, the similarity in age—the average age of my sample was 23 when I entered the field and I was 22—and our gender differences made a huge difference in building rapport.

The social workers had announced my first day at the youth center well in advance. On that day, I met the three male social workers in their small office, where they quickly briefed me on some of their routines and practices. Essentially, the youth center had no programs. It was an open "hang out space" where anyone living in the neighborhood (or coming from elsewhere) could socialize. To that end, the youth center had a large rectangular room containing a pool table, a ping pong table, foosball table, dart board, several couches, and a TV. There was also a smaller, adjacent room where—as I would later learn—the young men would often weigh and package drugs.

After meeting the social workers, I entered the large room for the first time. Thinking back about the day and revisiting my field notes, the youth center was probably never again as well attended as it was on my first day. I assume many of the young men came to the youth center to see what I was all about. The room was filled with about 60 young men, standing in groups of 4 or 5, engaged in conversations, playing pool, watching the pool game, playing a ping pong match, watching TV, or just lounging around. Regardless of what they were doing, as I entered the room, they stopped momentarily to look me over and quickly resumed their activities.

While I wasn't sure how this first day would play out, I had thought the young men would come over and talk to me. Maybe they would be confrontational or perhaps just openly curious about what I was doing "in their space." In marked contrast, they completely ignored me, meaning I had to try to initiate conversations. I walked up to the different groups to introduce myself, and I vividly remember feeling *profoundly* awkward and self-conscious about approaching the young men who were clearly not initiating conversations and whose curt answers to my questions made it incredibly hard to engage with them. I was also overwhelmed by the sheer mass of guys and by not being able to remember their names. Several picked up on that fact and chose to make things even more uncomfortable for me by asking me a few minutes into our conversations: "So, what was my name again?"

Here I quickly realized I had spent far too much time thinking about potential ethical dilemmas—many of which were quite esoteric and unlikely—and not enough time thinking about how to actually get through the crucial first day. I had created scenarios in my head: what would I do if the young men asked me to store drugs at my house, as occurred in Adler's (1993) study? Was I willing to smoke marijuana with them and, if so, in which circumstances? Were there crimes I would report to the police, or would I always protect my participants, regardless of what they are involved in?[1] Yet standing there on day 1, making painfully awkward attempts to get conversations going, I had no idea how to even reach a point where these ethical dilemmas could possibly arise—because as far as I could tell on day 1, the young men would never even offer me a marijuana joint. I had spent way too much time anticipating future hypotheticals and little time thinking through strategies for my first few days. I had naively thought my experiences working with youth would have prepared me for this day and had not paid enough attention to the fact that the young men had no reason to be nice to me. I wasn't their tutor, their swimming coach, or someone working on a psychiatric unit to which they had been admitted, as had been the case in all of my other jobs.

While I hadn't planned my first day in detail, I had naively expected more interest in my person or presence. I did not expect to be warmly received, but I essentially expected *some* sort of reaction I could engage with. Having to navigate reactions to the ethnographer's presence, positive, negative, or outwardly hostile, gives the researcher something concretely to deal with. What I encountered, however, was profound outward indifference, which to me was much harder to navigate. I know the young men were not *actually* indifferent to me and had lots of questions about me and my research which they posed over the next few weeks and months. However, their strategy for day 1 was to pretend I wasn't of much interest, and I was completely unprepared for that.

Over the next couple of days, I tried different strategies to engage the young men in conversation. While they showed more curiosity about who I was and what I wanted to do, I did not feel I could break the ice with many of them, especially not with the young men who I sensed constituted the "core group" and who seemed to have the most influence among the others. Every night, I would reflect on my day and try to figure out a way to talk with these guys. My initial field notes are full of remarks about how awkward I felt at the youth center and how my attempts to build rapport fell flat. The young men and I had little in common, and I realized quickly that meaningful conversations between 23-year-olds of the opposite gender are difficult when there are no shared interests or experiences. I bonded with some of the younger ones by talking about sports or school, but the conversations with the young men my age were either characterized by their attempts to flirt with me or telling me

---

[1] To that end, it is important to point out that German universities had no research ethics board at the time, so there were no official guidelines that could have posed restrictions on me or provided some guidance.

to leave, neither of which were particularly conductive to my research ambitions. Looking back, I was obviously impatient. I had expected some initial resistance I would eventually overcome. I certainly had not expected the lack of interest in the first few days or the stark resistance some of them showed in the first few months (see, e.g., Bucerius, 2014).

My first real breakthrough happened completely by happenstance. The youth center was on a trip to a swimming lake, and two of the young men challenged me to a swimming race (see Bucerius, 2013). Neither knew I used to be a competitive swimmer, so I knew beforehand I would easily win. My two opponents triumphantly announced beforehand "I would rather die than to lose against a woman" and made everyone else watch the spectacle from the shore. I quickly rose to fame among the younger guys who were in awe that I had beaten some of the most respected athletes in their group in a sports event—as a woman nonetheless (!). This certainly gave me some sort of "in" with some of the young men (although not with others), and, if nothing else, it was a conversation topic for a few days.

The fact I participated in a swimming race of all things was—again—complete happenstance. Had the young men challenged me in any other sport, I would have clearly lost and not increased my status in the group. No ethics board, no dissertation committee, could have foreseen I would find myself competing in a swimming event with drug dealers. Here again, the unpredictable nature of ethnographic work is evident. Spending extended periods of time to plan for all sorts of hypotheticals that will likely never materialize is often wasted energy. Instead, budding crime ethnographers are probably better served to prepare themselves for the more realistic, if less sensational, interpersonal scenarios. My main advice would be to be patient and be prepared to feel *unbelievably* awkward. Put yourself out there. The breakthrough will likely come when you least expect it.

Conceptualizing My Data

As with building rapport, conceptualizing the focus of my research study happened more by chance than as a result of careful planning. For weeks, I was frustrated about not being able to meet the young men outside the community youth center. Two months into my study, they would not even tolerate me hanging out with them in the evening. I started to seriously doubt my ability to write a dissertation about the drug trade. While I visited the youth center almost every day and took detailed field notes, it took me a long time to realize I *was* collecting valuable data, just not on the topics I had originally anticipated. The following excerpt from my field notes in the early days shows just how much I was already learning about the young men and their world—although being fixated on "learning about the drug trade" blinded me from seeing the value in my data at the time:

"Hey Bullock,[2] come over." Hearing Akin[3] calling me over made me nervous. He and I were still having trouble, he was clearly opposed to my research and it was just last week that he locked me into the washroom for about an hour, turning the power off and letting me sit in the dark. When I showed none of the reactions he potentially expected, like pleading or screaming for help, calling the police or friends to help, yelling or crying, he sent one of the younger guys to open the door and let me out again. Him calling me over made me wonder whether he had yet another scheme planned for me. The youth center was buzzing today. Little groups of guys were scattered across the big room, four were playing darts in the back corner, two groups of five and six, respectively, were standing around talking, and another five guys were watching TV. I was playing pool with three of the young men. Ozgur has made it his mission to teach me some skills, and to his satisfaction, I seem to be getting better.

Inanc and Ali were at the foosball table, debating whether Ali was a good Muslim given he did not quit selling drugs even though it was Ramadan. The two were engaged in a passionate debate about the topic, which seemed all the more ironic to me given that Inanc, who seemed to have very strong opinions on why Ali was not a good Muslim, was smoking a joint while arguing with Ali. I was amused about the scene and the fact Ali did not seem to notice that being schooled by a marijuana smoking guy about Ramadan seems somewhat contradictory. It was Talat, Inanc's cousin, who seems to be living a somewhat legitimate life and holds a steady job, who pointed this out to them. "What the fuck are you talking about, Inanc? You can't lecture him on Ramadan and smoke a joint." Ali, who sensed an ally, chimed in and said: "Yeah man, what the fuck? You're not being a real Muslim either." Talat was quick to reply: "Shut up, man. You can't even shut your business down for three week, stupid Albanian." I chuckled to myself. As always, the young men seemed to be performing their boundary work; the question of who is a real Muslim and who is *not* constantly pervades the conversations. As I am listening, I become frustrated about still not being able to ask about their drug trade. How can they justify dealing drugs at all, given that they claim to be devout Muslims? How does that fit together? And how long until I can finally dig into these topics?

It's my turn at pool again, and I hear Akin's voice yet again: "Bullock, finish up that stupid game and come over here, I need to talk to you." I somehow manage to win the game for Ozgur and me. Ozgur gives me a high five: "See, I'm teaching you well!" I tell him I have to see what Akin wants, and he mutters back: "Have fun." I see a smirk in his face, so as ever so often I wonder whether everyone is always informed about Akin's plans for me, whether it is locking me into a washroom, threatening me with rape, cornering me to ask about my sexual preferences, or stealing my bike? Do they know and support his decisions and actions or do they know, secretly disagree but cannot say anything because of Akin's

---

[2] They called me "Bullock" given my first name is the same as the Hollywood's actress's name "Sandra Bullock."

[3] All names are pseudonyms.

reputation among the young men? Or do they have no idea what's coming? With these thoughts in mind, I walk over to Akin. He sits at a table, rolling a joint and watches me approach him. His eyes give off the tell-tale sign that this was clearly not his first joint of the day. It's four pm by now, and he looks pretty stoned. Clearly, he is not taking Ramadan seriously either, but I have a hard time understanding what Akin stands for anyways. Except for him having assumed the role to test out my limits and making it his declared goal to show me that I am "not made for this" and should rather do a research project on "little children at kindergarten" as he would constantly tell me, I haven't yet gotten a good sense of his personality. It is confusing to me that he seems to be neither athletic—an attribute that seems to play a huge role among the young men—nor in other ways ticking off any of the boxes that seem to be ticked off by other young men who enjoy a higher reputation. He does not seem to be known for being extraordinarily skilled at violent altercations. He is not particularly attractive, in fact, he's actually over-weight. He seems to be stoned most of the time—yet another attribute that isn't necessarily looked up to. He does not seem to have a girlfriend. Yet Akin calls most of the shots at the youth center. What he says or does gets the attention of everyone and even though I have only spent six weeks here, I have become frustrated more than once that he seems to be able to influence the group dynamics so much that young men who were just telling me something very personal would suddenly turn against me, provoke me, or tell me to "f… off," just because Akin enters the room. I hear that he is the most successful dealer. But how, I wonder. He does not strike me as super smart either. Again…I am not sure whether I will ever be able to ask him or get inside knowledge on his business. For now, he seems fundamentally opposed to my presence, which seems to be standing in the way of me getting any insights into the drug dealing world at all. Just last week Georgio told me that he's happy to have me tag along once Akin thinks it's cool. What if that is never the case?

I took the chair. He starts "Why the fuck do you not smoke, Bullock? This would be easier if you smoked. You'd have an easier time. We could smoke together." I wonder whether that was his interpretation or whether he picked up that line from the director of the youth center, who had just recently told me that smoking would make my life easier with building rapport. Akin looks annoyed. He fixates me, turns on his joint and blows the smoke right into my face. As I inhale the marijuana, I try not to blink but to maintain eye contact. Akin takes a second puff. He blows it into my face again—this time, much more slowly as he is trying to create round circles while blowing. I don't move. Akin smirks: "Bullock, take the fucking joint. I know you smoke. Can't tell me you can sit there without coughing. Just take it." He offers me the joint and I refuse. He scoffs. I wonder what this is about. Akin has never offered me anything in seriousness. Yes, he's been trying to tease out whether I would smoke marijuana for weeks now, but usually in front of others. This time, it is just me and him at the table. I become hopeful—maybe, he's finally more open to my presence at the youth center. I say: "Ok, so, what's up?"

He blows into my face again: "So, you want to write about us, right? You want to write a book about us?", "Yes," I reply, "Eventually." He calls Talat over. "Oh, no" I think. "Here we go again....who knows what he has now planned with Talat." Talat comes over. He's dressed way more professionally than all the other guys- he works for the city and clearly came directly from work. He pulls a chair over, sits down and says: "What's going on, Akin?". Akin turns to him and says: "She wants to write a book about us." He nods in my direction. "You think we should tell her something about us?". "Sure." says Talat. "What do you want to know, Bullock?". I am unsure how to deal with the situation. I can't read whether Akin is serious and is about to actually talk to me, or whether he will turn whatever I now say against me. So, I give a half-baked answer, saying that I am mostly interested in their lives and "what you guys do for a living." I did not want to use the term "drug dealing," as I fear Akin will just rip that apart. Talat looks at me as if I am crazy. "I work. For the city. You know that." Akin laughs. "Bullock, you want to know something about the business, right? You want to know why we are criminals?" He blows his smoke into my face again. "I don't like the term 'criminal.' I kinda think of it as your way of making money" I say. "Of course. You're a student. Gotta use proper terms, right Bullock? But whether you like it or not, we are criminals. Want us to tell you about it?". "Sure," I say given that I am happy about just about any conversation with Akin in which he does not tell me to fuck off. "You want to know why we deal drugs?" "Sure," I say. "Well, I will tell you this" Akin says. As I wait in anticipation, he locks eyes with me again. We stare at each other for what feels like a minute. It felt super long and I know he won't talk about drug dealing at all. The next thing he says is "What do your parents do for a living?". I tell him. And this is where he launches in a thirty-minute long monologue about the guest worker history of Germany. He talks to me about his dad arriving at the Frankfurt train station in the 60s and being greeted by people with flowers. How his mom came to Germany and got welcome gifts. Talat joins in and fills in his bits. They tell me how their parents worked hard jobs, how very quickly no one seemed to care about them anymore, how the beautiful welcome with flowers and gifts turned into being treated as "dirt." Talat tells me about the back injury his dad experienced from years of hard and menial labour, how their family is still living in a two bedroom apartment with no chance of ever moving up. Akin tells me how his mom is cleaning houses for Germans 24/7, and his dad is never home because he works too much. "And you, Bullock, do you live in a house? I share a room with my brother and my parents sleep in our living room. That's how Germans treats their foreigners."

Their stories of their parents applying for jobs and not getting them, of working "all the time" essentially making little money, and how their dreams of coming to Germany and getting rich quickly vanished are heartbreaking. I feel uncomfortable listening, mostly because I feel embarrassed that I know so little about the guest worker system. At German schools, history classes end with the second world war—none of my teachers ever touched on Germany's history beyond that. Worse, even in my sociology classes, the plight of the guest workers or the guest worker system played no role. As I am following their stories, I am won-

dering how many of my friends really know anything about our immigration history since the second world war. Probably not many… Akin points out to me that "…and we can't go back to Turkey, because it's embarrassing, you know? They think we are all rich. They think we made it. They think we have all the money. But we have nothing. And everything we have goes to the village back home anyways. Because they think we have to share our richness, you know?". He concludes: "Look, if you want to understand anything about me, just know that I will not be a donkey like my dad. I won't work my arse off for nothing. I won't play that game."

This was the first of many conversations I had with the young men about their and their families' position in German society. While I became increasingly interested in their social, economic, and political situations—the social exclusion they faced and the future they envisioned for themselves—it took me quite some time to realize this would become a central focus of my work, as opposed to my initial interest in the drug market. By being so determined to get access to their drug-dealing activities, I did not realize I was already collecting interesting data on their views on gender; their conceptualization of being Muslim; their attitudes about religion, criminality, and drug use; or their situation growing up in a country that would not grant them citizenship (despite being born and raised in Germany). German researchers had given these topics some attention, but firsthand accounts like my ethnographic work were nonexistent. However, because I was fixated on collecting data on drug transactions, I was unable to realize the potential value of my data in these initial weeks and months.

I did eventually gain access to their drug-dealing activities and observed hundreds of transactions and negotiations between customers and other dealers and learned how the drugs came to Frankfurt, how they cut and stored drugs, and who they did and did not do business with. I gathered many firsthand accounts on how they dealt with people owing them money, how they felt about certain substances over others, whether their families knew about their drug dealing, and how they kept the women they were dating out of the trade. And while I have written about their drug dealing in great detail (Bucerius, 2007, 2008, 2014), my larger project became about how the young men made sense of their situation in Germany as second-generation immigrants. Part of my ability to do so derived from being able to capitalize on my gender and knowledge about sexuality in ways I had not anticipated, giving me the opportunity to collect data a male ethnographer likely would not have, and to build rapport precisely because I was a woman.

Ethnographer as Sexual Educator

Before entering the field, I had thought about many ways my gender might negatively influence my ability to build rapport with a group of hypermasculine guys. Being a female crime ethnographer among male-only research participants is rare,

and there were few research accounts from fellow female crime ethnographers for me to study (see Bucerius and Urbanik 2018). In essence, I knew my gender would be salient throughout the entire project, but before entering the field, I thought it would mostly be a hindrance, not an advantage.

The gender dynamics were complicated and compounded by my age. Being close to their age meant the men perceived me as a potential sexual partner, something that undoubtedly influenced the process of establishing research relationships with them. Some attempted to flirt during the first few months of the project. However, because I wasn't respected by everyone yet, the guys only flirted with me in one-on-one situations rather than in front of the other young men. When others joined us, their flirting quickly turned to provoking. At the same time, the homosocial setting of the youth center allowed for many discussions of the "perfect woman." Since I clearly did not fit that picture due to my different social and religious background and my views on gender relations and because I did not resemble women pictured in the many porn magazines laying around the youth center, there was never a moment when the young men overtly flirted (in front of others). Later in my research, when I had become an integral part of the group, viewing me as sexual partner was no longer an option. As Ozgur would say many times, I functioned as a sister to many of them—so sexuality was out of the question. However, despite the sexual connotation always present in gender-mixed interactions, being a female also enabled me to act as a sort of relationship counselor and, more importantly, as sexual educator.

Although the young men talked about women constantly, they did not talk with women about their sexual hopes and dreams or other intimate matters. Their relationships rarely lasted more than a few weeks. Over the course of my 5-year study, only 5 of my sample of 55 men were in relationships lasting longer than 3 months. Nevertheless, all were sexually active—if not with their current girlfriends, then with one-night stands or prostitutes. In many ways, I was saddled with the role of sexual educator expected to present the "female perspective." My experience contrasts sharply with that of Horowitz (1986) who was initially identified as "the lady" by the men she was studying, which meant sexual matters were not discussed in front of her. In my case, sex and women (as long as they were "only" sexual partners and not steady girlfriends) were constantly discussed in front of me, something that continued throughout the many years of my research. Discussions about sexuality helped me earn trust, as all the young men were interested in learning more about female sexuality. This gave me a chance to impress and be useful to the young men: I had information to offer they could not easily or comfortably access from anyone else.

Rahim   Sandra, come here, we're having an argument.
Sandra What's it about?
Ibor   We're talking about how often a woman has her period and this moron thinks it's four times a year! I've been telling him it's just twice.
Rahim   Whatever you idiot! Sandra, tell him I'm right!

Before entering the field, it did not cross my mind I would become a sexual educator. I certainly would never have predicted that sex education would be a good way to build rapport. Had I written a research proposal before entering the field (an exercise German graduate students do not have to do!), the methods section would have had little connection to what became the reality. Likewise, I could not have predicted that talking with the young men about sex, their views on women and marriage, and their sexual encounters with sex workers and women they had relationships with would make up a big part of my data. These were topics I wasn't originally interested in or even imagined would be possible to pursue. Yet because the young men were keen on sharing their views and hearing mine, sex and marriage opened up themes I likely could only gain access to because I was a female researcher. My gender was consequently a clear advantage in some ways and not just an additional hurdle I had to overcome.

Capitalizing on Chance Encounters

When doing ethnography, we often set out to observe specific moments and participate in encounters we deem in advance to be particularly valuable for our research. I was initially most keen to observe drug transactions and negotiations, both with customers and wholesalers. As with many ethnographers, I would happily tag along anywhere my participants allowed and would make time whenever they said "come over, something interesting is going down." While I learned a lot about the drug trade, my most interesting encounters, however, were not at all related to the things I had set out to discover. Rather, I had many chance encounters that deeply enriched my data in unanticipated ways. One such unanticipated encounter happened when Aissa, one of my key participants, and I were meeting for a coffee. Aissa was working at the airport and had left his drug dealer career behind. However, he was struggling to make ends meet and wanted to talk about whether he should start making occasional cocaine deals.

As we entered the café, I am walking ahead of him and saw our usual table was available. I went straight for "our" spot. As I pull out my chair to take a seat, I heard commotion behind me and turned around. To my surprise, Aissa hadn't followed me, but was still at the entrance of the restaurant, engaged in a heated conversation with two men sitting at the table by the entrance. I couldn't hear what they were saying, but within ten seconds, Aissa slapped one of the guys across his cheek. I heard Aissa say: "This is your lesson," his face flushed with anger.

Aissa certainly had the reputation of being one of the most skilled fighters among the young men. More than once, others had told me they would call on Aissa if they ever were in real trouble. I also knew he was facing a court hearing for having brutally beaten a man who allegedly assaulted one of his older sisters. He had seen the man as he was driving, pulled over his car, took out the baseball bat he had been

driving around with—just in case he would encounter the man—and attacked him on the open street. While I knew these and other stories, I had never personally seen Aissa be violent, so the scene at the café was new to me. With me, Aissa was usually extremely level-headed. He was the one I would ask for advice on my project. He was the one I would take to my university to co-teach a seminar, and he was the one who would spend hours explaining to me intricate details about the best drug mixing practices or other things I could not understand by pure observations. In other words, Aissa was one of my reliable "go-to" people. When I was with him, I wasn't "on edge" or expecting anything out of the ordinary to happen. When hanging out with some of the other guys, in contrast, I never knew whether I would have to disappear from a scene because they got into a particularly violent altercation, or whether the evening would take an unexpected turn because one of them would hit on a woman, go to a brothel, smash a car window, or do a break and enter. My meetings with Aissa were predictable. We would meet at a café, bar, or restaurant and chat about life. This is why I was startled to see him slap the young man.

I immediately thought the interaction must be related to drug debts and figured we would likely leave the café. I pushed my chair back to the table and took two careful steps toward the entrance. Since I wasn't sure what the interaction was about and how it would evolve, I wasn't sure whether I should walk past them and out of the café or hang back. I certainly did not want to get caught in the middle of a fight. I was not sure whether the other young man would pull a knife or hit back. Likewise, I had no idea what Aissa would do next. To my surprise, Aissa turned around and came toward me. His posture reflected confidence. He did not seem to fear retaliation and did not even turn around to make sure he wasn't being followed. I asked: "What the fuck was that….are we leaving?" "No" he said, "Sit down."

Aissa proceeded to tell me he was "defending me." "Defending me? I don't even know that dude." "Sandra, you should have seen the way he looked at your ass! He was eating dinner on your ass. It was disgusting!". I had no idea how to react. I was amused by the fact Aissa thought he was "defending me," annoyed by his hypermasculine slapping of someone in a public space for "staring at my ass," and, ultimately, really quite unconcerned about someone looking at me. When I shared my latter feelings with Aissa, saying: "Why does it matter – let him look?", he was appalled. He felt like he had just done a good deed for me, whereas I now questioned whether that deed was even necessary. I told him I could not care less if someone looks at me, as long as they keep their hands to themselves. Aissa launched into a longish speech about appropriate behavior and how these "looks" are essentially similar to sexual assault. This seemed deeply ironic given Aissa (and the other young men) were checking out women constantly.

Ultimately, Aissa and I had an excellent discussion about the fact that him slapping the man had nothing to do with defending *me*. Instead it had everything to do with Aissa's feeling of being disrespected. When he said over and over again that the young man "could not have known I wasn't your boyfriend," he was able to recognize, and discuss, why *he* felt disrespected when he saw the man "checking me out." This conversation led to one of my best interviews during the entire 5-year

project, as we unpacked many of the complex ways hypermasculinity was embedded in a host of decisions made by the young men.

Concluding Thoughts

While I did not have the benefit of having a supervisory committee I could run questions by or an ethnographic role model I could look up to, in many ways I consider myself lucky I did not have to report to a dissertation committee or ethics board. I did not have to plan everything and anticipate every possible event ahead of time, precisely because I was not bound to a proposal my committee had approved or guidelines established by my university. This freed me to take a trial-and-error approach: I did not have to get my research site approved by the university and could have chosen a different one any day, had it not worked out. While I was concerned about not making fast enough progress in building rapport with some of the young men—at least in my mind—I did not have to worry about being held to a strict timeline set by my committee. And while I had a topic in mind I wanted to study, I was able to switch my focus to immigration issues when I thought that framework to be more fitting than that of drug markets, much to the dismay of my supervisor who was not as interested in the immigration context.

In supervising my own students, I see the advantages of committees and ethics boards and do not call them into question in general. However, I believe to produce interesting ethnographic work, budding crime ethnographers are best served by a system allowing them more flexibility to go into the field and be guided by the data. As my experience shows, much in crime ethnography cannot be planned ahead of time: whether it is the conceptualization of your research topic, the challenges you may face, the role your positionality plays, or the breakthrough moments, either in terms of building rapport or opening up new areas for discovery, such as in my case occurred around the topics of marriage and sex.

And while we probably over-prepare students regarding ethical concerns and how to handle potentially "risky situations," we underprepare them for the often prosaic but much more important aspects of fieldwork: building rapport and making connections. During my first week in the field, it became instantly clear to me that thinking through the hypothetical scenarios of having my notes being subpoenaed or me being present during an arrest were farfetched. The young men's initial disregard for my person signaled they were not even interested in having a coffee with me, so being present during a drug deal was close to a fantasy at that point.

Regardless of the initial phases of rapport building that every crime ethnographer has to go through—unless they are insiders like Contreras (2013)—there are also risks in sensationalizing the risk for students wanting to go into the field. Like many crime ethnographers, I did eventually observe violence and many criminal acts, but these events made up a minimal amount of my overall fieldwork. Focusing on the spectacular events blinds us to the mundane day-to-day activities that make up the

lives of our participants. And it is only by looking at their day-to-day lives and the struggles they face that we can begin to understand why our participants potentially lash out in violence or criminal behavior. I worry that by focusing too much on discussing "risky" situations in dissertation committees and ethics boards, budding crime ethnographers will miss out on observing the social interactions that *actually* matter, all the while they are too focused on identifying the spectacular and unrepresentative. Similarly, it took me a long time to understand that observing drug deals and gaining a deep understanding of the drug market were not the most interesting aspects of my data.

When preparing students for the few risky incidents or focusing on them in our publications, we also risk losing perspective. While the young men I studied originally threatened me with rape (yet never followed through), as time went on they actually became quite protective of me. For example, on one afternoon in the initial phases of my research, one of the young men who was twice my size pushed me into a side room of the youth center, trying to close the door behind us. This was a seriously threatening situation, and I was scared. Another young man with whom I had not been able to establish any kind of rapport to that point saw what was happening. He immediately came to my defense by pushing the young man off me, beating him, and telling him he could not come to the youth center for the following week. He told me: "Such behavior is just not on, no one is going to touch you." In another instance, four of us were trying to enter a club, but the bouncers would not let us in. As the young men started to argue with and physically confront the bouncers, Akin handed me his car keys and said "Take my car and go home. You don't need to stick around for this, who knows what will happen." There are far more times when the young men took a protective stance toward me, as opposed to the few times they actually threatened me—yet it is those few times and the "could haves," "would haves," and *nonevents* that ethics boards are most concerned about and that crime ethnographers like to talk about most.[4] While we should not make the mistake of negating the risk in crime ethnographies, we are probably best served to put such risk into perspective.

---

[4] Putting risk into perspective, I believe that almost at any point during my own research, I was at a higher risk of being sexually assaulted when going into a bar with complete strangers than being around the young men who grew protective of me. The greatest risk I probably faced during my fieldwork was when driving to my fieldsite on the German Autobahn and risking a car accident. Likewise, unanticipated risks probably dominated in the research process. When I talk about my research, people automatically assume that the greatest risks for me were either to get into a violent altercation during a drug transaction or the sexual risks of getting assaulted given that my participants were hypermasculine males. However, more mundane forms of risks were much more prominent. For example, there was often the chance that I might be in a car accident because one of the guys I was driving around who was high on cocaine would try to take the steering wheel, something which was a *much* greater risk than being hurt in a violent drug transaction.

References

Adler, P. (1993). *Wheeling and dealing*. New York: Columbia University Press.

Bourgois, P. (2003). *In search of respect: Selling crack in El Barrio*. Cambridge: Cambridge University Press.

Bucerius, S. (2007). What else should I do—Cultural influences on the drug trade of young migrants in Germany. *Journal of Drug Issues, 37*, 673–698.

Bucerius, S. (2008). Drug dealers between Islamic values, everyday life in Germany and criminal activity. *Zeitschrift für Soziologie, 3*, 246–265.

Bucerius, S. (2013). Becoming a trusted outsider - Gender, ethnicity, and inequality in ethnographic research. *Journal of Contemporary Ethnography, 42*, 690–721.

Bucerius, S. (2014). *Unwanted – Muslim immigrants, dignity, and drug dealing*. New York: Oxford University Press.

Bucerius, S. and Urbanik M. (2018). When crime is a "Young Man's Game" and the ethnographer is a woman: Gendered researcher experiences in two different contexts. *Journal of Contemporary Ethnography*. https://doi.org/10.1177/0891241618785225

Contreras, R. (2013). *Stick-up kids*. Berkeley, CA: University of California Press.

Douglas, M. (1966). *Purity and danger*. London: Routledge Classics.

Horowitz, R. (1986). Remaining an outsider: Membership as a threat to research rapport. *Journal of Contemporary Ethnography, 14*, 409–430.

Maher, L. (1997). *Sexed work*. Oxford: Clarendon Press.

The Promise and Process of Ethnography: What We Have Learned Studying Gang Members and CPS Kids

Gabriel T Cesar and Scott H. Decker

The challenge of social science is to describe the broad parameters of social life by paying close attention to the details. The tension between broad description and specific detail is found in the analysis of most social phenomena, from conformity to deviance. Social scientists have addressed this problem with a variety of methodological approaches. That said, qualitative approaches formed the foundation of the social sciences (Bernard, 2012). Certainly, many of the earliest criminologists, especially at the University of Chicago, used ethnographic methods and fieldwork to understand crime causation in various contexts. Wright, Jacques, and Stein (2015, p. 339) observe that the roots of criminology in the USA are "anchored firmly in qualitative research." They specifically identify *The Jack-Roller* (1930), *The Professional Thief* (1937), and *Street Corner Society* (1943) as examples. We might add *The Gang* (1929) to this list. For criminology as a field, firsthand experience, fieldwork, and qualitative analysis generated many frameworks and typologies which we continue to study today.

For individual researchers, qualitative research can also lead to interesting career paths. Qualitative researchers can attain status by generating innovations in theory (e.g., Anderson, 1923; Decker & Van Winkle, 1996; Goffman, 1968; Katz, 1988; Manning, 2015; Miller, 2008; Wright & Decker, 1994, 1997). Qualitative approaches can interrogate ideas in novel ways and drill into the "black boxes" that often emerge from quantitative data analyses. They can also provide interesting alternatives to the mind-numbing tables that characterize so much quantitative research. Ethnographers are frequently desired partners at social gatherings, owing to their "tales of adventure." But these benefits come with costs. Compared to quantitative analyses of secondary data sets, ethnography is less predictable, more risky, and incompatible with the role of the "detached, impartial researcher." Put more directly, ethnography

G. T Cesar (✉)
Florida Atlantic University, Boca Raton, FL, USA

S. H. Decker
Arizona State University, Tempe, AZ, USA

can help your career be awesome but also comes with some important considerations. These considerations include long-term, personal commitments, ethical and safety concerns, and research marketability. In our experience, these concerns can be successfully managed through collaboration with mentors, colleagues, and IRBs, self-reflexivity and reflection, and rigorous methodology (and documentation thereof).

In this chapter, we begin by describing our individual pathways into ethnographic work. We follow this by outlining the innovations and challenges we have encountered conducting ethnographic research with young people in street gangs and child protective services. We then synthesize what we have learned from our relative perspectives as an early-career ethnographer and his more practiced (older) mentor. Decker's career in fieldwork has its roots in the 1970s, while Cesar's journey started in the late aughts. Separated by several important criminal justice reforms and a generation of scientific development, our perspectives and experiences have important implications for graduate students and emerging scholars planning research careers. But we think (hope) any researcher interested in "digging deeper" into a research question, initiating a new direction of inquiry, or developing a contextual understanding of the people and problems they study may benefit from an informal discussion of our respective pathways to ethnography. We conclude with some suggestions to help researchers develop strategies for planning, conducting, and marketing ethnography.

Decker's Pathway to Field Work and Ethnography

I was fortunate to have a unique experience as an undergraduate. My university (DePauw) had initiated a "4-1-4" program, in which students took four classes in the fall and spring semesters and an intensive field experience in January. Dr. Paul Thomas taught the criminology course I took as a junior and had helped me develop relationships at both the Indiana State Farm (a medium security facility) and the Indiana Boys School (IBS) (the secure residential facility for juvenile males). The Warden at IBS in Plainfield (just outside of Indianapolis and about an hour from campus) found lodging for me on the campus of IBS, and I spent most of January 1972 there. My lodging was a room in the infirmary in the campus, outside the locked area. I had an ID tag that gave me extensive access to the facility[1] and was assigned to "Cottage 8." Cottage 8 was unique because it was the only residential facility at IBS that was not based on age. The boys in Cottage 8 had been designated as having "mental problems," and as a consequence the Cottage had boys who were over 6 feet tall and weighed more than 200 pounds alongside boys who were not quite 5 feet tall. Not having had ethnographic training (Dr. Thomas gave me a book to read before I went to Plainfield) and this being my first field experience, I decided

---

[1] The late Charles Manson had spent time at IBS and I got to read his file. This is just one example of how getting out into the field and "getting your hands dirty" can be exciting for a young student and an experience that can maintain relevance over the course of a career.

to make notes about "a day in the life" of a boy at IBS. I tried to test the assumptions of differential association, labeling, conflict, and social disorganization against what I saw and heard from the boys.

There was no institutional research board (IRB) approval,[2] the final report was to be of my own choosing, and I began with no observation schedule or instrument. It was exciting to have been granted access to these boys and be on my own but also very discouraging. I got to know the boys as someone with "in-between" status, not a member of the staff, but certainly not one of them. I saw them "huff" copying fluid from rags to try and get high, boo other residents who played visiting basketball teams, and engage in other self-destructive and detrimental behaviors. IBS was walled on three sides and had a large "moat" on the fourth side. A few of the boys decided to effect an escape on a sub-zero Indiana day and were pulled nearly frozen from the water. While these were individuals with a preponderance of definitions favorable to law-breaking, that didn't make differential association a better explanation of their behavior. The warden took the time to read my report and discuss it with me. He acted interested in what I had "found" though it hardly had much of a scientific foundation and offered little more than what he already knew. The lasting impression on me was how complicated the lives of the boys were and how simple their decisions to offend were. This contrast is something that has been a constant in much of the field work I have done.

I managed to take a qualitative research course in the Sociology Department at Florida State University while completing my PhD. I also spent nearly 3 months observing a rural sheriff's department as the basis for my master's thesis. This was a more systematic and orderly process than the IBS experience, though hardly as systematic and orderly as my colleagues who were working with quantitative data sets. I wrote a very long first draft of my thesis and proudly delivered copies to the members of my committee. One of them called me back to his office later that week and sat me down. Waiting for the praise that was sure to follow, he took my (200 page?) thesis draft and placed it ceremoniously in the trash can. He told me, "don't ever hand in anything like this again.[3]" I was wondering if the job teaching tennis I had been offered might still be open. He then asked me an important question: In one sentence, what is this thesis about? I couldn't answer the question. He told me not to come back until I had an answer to that question and that he expected it to take several days if not a week to come up with the answer. This was one of the best four or five things I learned in graduate school and one I continue to pass down to my students and colleagues. What is the point? What is the research question? It was hard work, harder work than I imagined. I returned to his office, offered a piece of paper with a succinct, one sentence summary of what the thesis was about and waited for his response. He told me to make a copy of the sentence in very large print and affix it to the wall above the desk I wrote at and that every paragraph should reflect that question.

---

[2] The National Research Act of 1974, responsible for the Belmont Report and the Act that initiated IRBs at most universities, had not been passed yet. Important changes have been made in recent years to the so-called common rule (Hudson & Collins, 2015).

[3] Recall that this was 1974 and there were no electronic files; all papers were typed or handwritten and then typed and xeroxed.

One of the best things that ever happened to my career was Richard Wright. I had the good sense to hire him, my first faculty hire in what would be a string of over 30 tenure-track faculty hired while building two PhD programs. Richard was a real ethnographer and had studied residential burglars. After getting his feet on the ground in St. Louis, he led our collaborative study of residential burglars. Among his many outstanding qualities is his attention to methodological detail; as a consequence, we have chronicled the methodological foundation of that work in a few different places (Wright, Decker, Smith, & Redfern, 1992; Wright & Decker, 1994, 1997). This collaboration led to fieldwork with armed robbers as well. I learned many things from him that I still depend on today. But late in the burglary field work, around 1994 we noticed a number of things changing on the street. The graffiti that we had observed on the back of buildings and primarily on abandoned buildings began to be found on the front of buildings. Some of those buildings were commercial places and others were residences. The day we saw red graffiti on the front of a church, we knew that things on the streets of St. Louis had taken a large turn for the worse. The graffiti was predominantly red and blue, featured letters, numbers, and symbols that were not highly stylized and often featured the name of an individual and the letters "RIP." Gangs had come to St. Louis, despite the protestations to the contrary by the police chief and many elected officials. I applied for a grant from the Department of Health and Human Services, Administration of Children, Youth and Families. I was fortunate enough to be funded; as it turns out all six of the research proposals were funded. At the first-year meeting of grantees, we were told that we would be extended for 2 more years.[4] The university waived the indirect costs, giving us enough money to complete the fieldwork for *Life in the Gang* and the various projects that emerged from that study.

The transition from the burglary study to the gang study was smooth, and it was fortuitous that we had been in the field for 2 years at the time the gang study began. Despite many successes in contacting and interviewing gang members, we struggled to engage as many family members and ex-members as we would have liked to. Fortunately, the ex-member interviews were saved, and gang exit is a topic I returned to with my students David Pyrooz and Richard Moule nearly 20 years later. *Keep good records and save them carefully*. That is much easier to do today than in the paper and pencil days. Floppy disks and Microsoft Word were improvements. More recently, I have used Google Docs and Dropbox to organize and maintain my research and writing materials. Street Daddy was our central point of contact for years, and we managed to keep him engaged and in the university payroll system long enough to qualify for a small pension. One of the unanticipated consequences of the burglary, gang, and robbery projects was the transition of Street Daddy from life-long involvement in crime and negative relationships to a role as a highly respected diversity counselor at one of the most exclusive Catholic boy's schools in St. Louis. This is quite a remarkable journey, and he remains a friend.

---

[4] Street Daddy attended the conference with me in what was his first airplane flight and stay in a hotel. He was amused by the range of options on pay-per-view and the availability of a minibar in his hotel room.

I have completed multiple field research projects with large qualitative components since leaving St. Louis, and the outstanding colleagues I had there such as Richard Wright and Dietrich Smith. The Office of National Drug Control Policy, US Coast Guard, and Drug Enforcement Administration funded a project to understand drug smuggling into the USA. I was asked to lead a team comprised of Margaret Chapman and a translator to interview 35 of the highest-level drug smugglers in US federal prisons. On average these individuals were caught with more than 800 pounds of cocaine, hardly nickel and dime dope dealers on the corner. We were granted access to these individuals through the US Federal Bureau of Prisons which allowed us to use tape recorders inside the prison. Despite this access and approval, Margaret Chapman and I we were detained separately for taking pictures of the outside of FCI Coleman (over concerns that photos could be used to help plan escapes). Our translator had quite a flamboyant style of dress, and we had to arrange for her to replace her tight toreador pants, stack heels, and form fitting top with more appropriate attire for interviewing in prison. Was this a concern from overly meddlesome senior faculty members or simply a misguided instance of political correctness? Our visit to Yazoo City MS was notable for both the quality of interviews and the culture shock it presented for the members of the research team, one from Chicago and the other from Long Island. Perhaps the most notable finding was the sense of injustice and outrage expressed by many of the drug smugglers over their arrests and convictions in "dry conspiracies." A dry conspiracy is one that does not involve drugs, only talk about smuggling drugs. In each of these cases, the subjects had not seen or touched drugs, only had conversations (often with undercover agents) about them. The other notable finding from this project was the response to the question about why smugglers brought drugs to the USA. It was a simple and direct answer: because Americans like to use drugs.

I would like to think that my career was carefully planned, with an epistemological foundation that enabled me to move seamlessly from project to project. Things may look like that (a little) in retrospect, but at the time chaos explains more of what happened. The phone in my office rang in 2012. I answered, somewhat amused because it seldom rings. The voice on the other side identified themselves as Jared Cohen, CEO of Google Ideas. Yeah right, was my first thought, CEOs call me all the time. *Not*. In an irony not lost on me, while he spoke I "googled" him. He sounded like his picture. I asked how he got my name, and he told me Marc Sageman (one of the leading authorities on terrorists and terrorism) had given him my name. By chance Mark and I had met at a conference in England to discuss the similarities and differences between groups such as organized crime, terrorists, drug smugglers, religious cults, and gangs. Mr. Cohen wanted to know if I could complete a study of the role that technology played in gang leaving and present the results at a conference in Dublin (Ireland) later that year. Yes was the correct answer. On a relatively small amount of money, we used contacts that had been built over the years (gang outreach programs, jails, probation and parole, youth intervention projects) and completed 629 surveys in five different sites in an 11-month period.

These data became the foundation for several papers, with more to come. We also made a point to capture qualitative responses, and one of the very first such responses was telling for a line of work that my colleagues (David Pyrooz, Richard

Moule Jr.) and I have followed ever since. One of our first respondents in the project (a "former" LA gang member) answered "yes" to the ever a member of a gang question and "no" to the "currently a member of a gang" and when asked what he did to leave his gang, told us that he could never leave his gang; it was impossible. This led us to revise questions regarding gang membership and disengagement and ultimately the conceptual model of disengagement that we used. That model, treated disengagement as a process, characterized by fits and starts, rather than absolute cutting points. It is possible to have one foot in gang life (if a gang member is physically assaulted) and one foot out of the gang (when family responsibilities call). That is what a transition process is all about. Over time, becoming an ex-gang member means reducing involvement, weakening ties, and becoming less embedded in gang activities and culture. Had we not used qualitative questions we would never have learned this important finding.

Often qualitative components have been "add-ons" to larger quantitative projects. To paraphrase Maruna (2010) we "went both ways" (used mixed methods) before it was hip to do so. A notable example is the evaluation of Project Safe Neighborhoods (PSN) in St. Louis. PSN was a gun focused set of interventions, the success of which hinged on raising the perception of increased prosecutions, increased certainty of conviction, and more prison time for gun crimes. A slick advertising campaign built around two precepts was begun: (1) gun crime means real time, and (2) you do the gun crime your family does the time (in the sense that you were isolated from family and they suffered as well). One measure of success (and one we used) was an increase in gun crimes in the neighborhoods targeted by the intervention. Another measure was the extent to which the message about gun crime penetrated to the target population. We interviewed arrestees for misdemeanors and felonies and conducted qualitative interviews in the holdover, where individuals were held subsequent to arrest. We asked a simple set of "awareness" questions about whether they had heard of the program, what its goals were, whether it was real in its consequences, *and* when they got up and left their house, whom did they fear more, police and prosecutors who could put them in prison for carrying a gun, or rivals on the street. Fundamentally, this was a question about who you were more frightened of, the law or the unlawful. The early proportions showed much greater fear of the streets than the system. One of the gun prosecutors called this the "gun thermometer" and called for increasing perceptions of the threat.

Cesar's Journey to Ethnography

My pathway to ethnography was perhaps more circuitous than my coauthor, mentor, and (now) colleague's. By the time Scott was hanging out with gang members in St. Louis, I was in Detroit[5] completing my GED. Like many of the youth, Scott was

---

[5] For clarity, my family is from Corktown. I was born on Cabot Street in Southwest and grew up in Inkster. Many of my earliest childhood lessons about fieldwork and human interactions were

working; by the time I was 18, I had been in more fist fights than I can count and had friends in prison and several who had died through violence or overdose. Over the decade it took me to pay my way through an Associate's degree; I worked on a few construction crews, in an epoxy resin foundry, a tire shop, and a print shop. As I completed my Associate's in Liberal Arts and transferred to Wayne State University, I also worked as an overnight security guard at a defunct meat packing plant on the Near East Side of Detroit (in the Eastern Market district) and started a process server business where I served eviction and protection orders to defendants. By the time I came to Phoenix for graduate school at Arizona State University, I had worked with lots of people, in lots of contexts.[6] These are not examples of formal ethnographic training but life experiences that paved the way for me to perceive social problems (and legal reactions) from the bottom up. Throughout my studies, I worked to synthesize what I learned in college with what I experienced in my life. I started to notice early on that much of what was presented in textbooks, often as facts, made little sense in applied settings.

As an undergraduate at Wayne State University, most of my professors were career-long Detroit Police lieutenants (and one Michigan Department of Corrections officer) who had participated in the Law Enforcement Education Program (LEEP). Their stories from the field illuminated my early thinking about criminal justice and resonated with my experiences growing up. Together, those classes cemented criminology as my intended field of study. At the same time, they caused me to approach the study of human behavior with the critical eye shared by the cop and the criminal. In other words, my approach to academia has never been purely academic and has leaned toward fieldwork and firsthand experience.

As a master's student at ASU, I took a class called Violence in America, taught by Robert Fornango. As a student of the St. Louis School (SLS) of criminology developed by Scott and his colleagues at UMSL, Rob's reading list consisted largely of books and articles by that group. Wright and Decker's (1997) and Jacobs' (2000) work on armed robbery were particularly resonant. For instance, in contrast to much theorizing about criminal motivation and crime prevention, the narratives of offenders themselves succinctly summarize the sense of optimistic fatalism in which I grew up:

> The way I think about [the threat of being apprehended] is this: I would rather take a chance on getting caught and getting locked up than running around here broke and not taking a chance on even trying to get no money. (No. 71, as quoted in Wright & Decker, 1997, p. 121)

---

forged at my grandfather's side at the Express Bar on Michigan Avenue and 12th Street. But I went to high school at Wayne Memorial High School in the suburbs.

[6] As Scott noted earlier, this sounds more intentional than it was. I moved because work was hard to come by in Detroit, and I had heard there were more lucrative and accessible opportunities to be had in Phoenix. In fact, I secured a job that doubled my salary and got me my first office job during my first week in town. During my master's, I worked as a skip tracer and garnishment administrator at a collection law firm.

That is how people talked in my world. And that is how I was introduced to ethnographic research in criminology. Concepts uncovered by the SLS integrated the theoretical and empirical orientations I had learned about in school with the street-level wisdom I was reared on. Such concepts include the functional skills needed to conduct a successful robbery (or to prevent or survive one, e.g., establishing copresence, enactment, completion), retaliatory calculus (Jacobs & Wright, 2006), alert opportunism (Topalli & Wright, 2014), and affect perceptions (Topalli, 2005) and existential fatalism (Brezina, Tekin, & Topalli, 2009).

Growing up, I had learned that it was my responsibility to ensure my own safety, and that that was best accomplished by keeping my ears and eyes open. I was taught to always look out for and prevent anybody trying to get "the drop" on me tactically. I was taught to sit facing the door and to walk curbside to protect my companion. I was taught to minimize my exposure to police activity. I was taught that a gun cannot help you if you let someone get the edge on you. The series of urban ethnographies that came out of the SLS were consistent with the jailhouse and streetcorner wisdom I had grown up with but went beyond journalistic description to analyze systematically what those narratives mean for criminological theory, the prevention of crime, and the development of policy. That is how the SLS inspired me to start thinking about fieldwork, data collection with "active" respondents, and qualitative analysis.

Throughout my graduate studies, I volunteered with a local nonprofit art mentorship organization working with youth who live in shelters and group homes in the custody of child protective services (CPS). When I first volunteered, my goal was not to conduct an ethnography. I just answered an email from an alumni group to chaperone three 5–7-year-old orphans through the Phoenix Children's Museum. It was a free trip to what I had heard was a cool museum and a weekend escape from the anxiety of grad school. I'd always said I would volunteer somewhere after I finished my undergrad studies, and kids in child protective services ("CPS kids") sounded like a worthy cause. But after a day at the museum[7] with three 6-year-old unaccompanied minors, I found myself signing up for mentor training and volunteering to be a "weekly art mentor" at a group home in a nearby suburb. That group home housed a rolling roster of about ten boys, aged from approximately 10 to as old as 18. My co-mentor and I were told that the boys could be rambunctious and that the last male mentor there had his wallet flushed down the toilet. It did not take long for me to see the overlap between the experiences of youth in CPS and the study of crime and justice.

It was frankly sometimes a strain to spend hours each week hanging out with teenaged boys every Sunday wrestling and cooking nachos, writing poetry, or multimediaing. The commitment was especially burdensome with coursework and a thesis (and later comprehensive exams and a dissertation) looming. But it was always a priceless respite from the pressure of grad school. Along the way, I developed relationships with dozens of CPS kids and young adults who had "aged out" of custody. I also met and worked with a large network of volunteers, group home

---

[7] The museum was, in fact, cool.

staff, and agency administrators who run local shelters, group homes, and service providers. When the time came to plan my master's thesis, I knew I wanted to collect data in that area. I thought I might be able to interview 40 or 50 active CPS kids as SLS researchers had with active criminal offenders before me. But my institutional review board had reservations about a graduate student interviewing minors in state custody. Ultimately, my thesis sample was restricted to three young adults who had aged out of the system at 18 but elected to stay in custody voluntarily. I conducted the interviews in each of the respondents' group homes, in a private room. The unique context of the respondents' lives, and their positions on the precipice between childhood as a ward of the state and adulthood in the community, would eventually form the foundation of my dissertation.

One of the very best things to happen to my career was Scott Decker. During the early days of my graduate studies, Scott called me to a meeting. He assured me that he knew I was not the type to quit on the kids I had been working with. He was right. He then suggested that to be successful, I should think about linking my dissertation with my volunteer work. My thesis had allowed me to learn the IRB process, engage my network of service providers and administrators, and to design and deploy an interview protocol. Scott encouraged me to engage more systematically with my fieldwork and to reflect continually on how the experiences of youth in CPS inform the study of criminology and criminal justice. You can never really predict how an ethnographic endeavor will go. But a collaborator with experience in problem-solving on the go, navigating the vagaries of fieldwork and disseminating qualitative outputs, can help a new ethnographer anticipate and overcome emergent barriers and complications. Scott saw some of the hazards I would face a mile away and had ready suggestions. Other hazards were novel to my own project and required me to make independent "executive" decisions. Having a well-known and successful ethnographer as my chair also helped legitimize my ethnographic work and afforded me substantial latitude in my department as I moved through graduate coursework, annual evaluations, and comprehensive examinations. For a budding ethnographer planning an ethnographic dissertation, having a mentor like Scott can go a long way, both substantively and procedurally.

My relationship with Scott and my dissertation demonstrates some of the tension that comes with graduate studies and academia more generally. Scott's work and that of his SLS colleagues gave me a well-tested road map for addressing research questions with ethnographic methodologies.[8] As my mentor, Scott was charged with helping me produce a high-quality project that was feasible on a timeline, valuable to the field, and marketable for prospective tenure-track assistant professor positions. As my collaborator, Scott gave me substantial latitude in defining my questions, exploring in the field, and critically analyzing sociological theory in the context of what I was seeing. In both areas Scott was a phenomenal chair of my dissertation. And in both roles Scott was able to draw on his own vast experience to

---

[8] Sarah Tracy's (2010, 2012) work on "phronetic methodology" and Johnny Saldaña's (2015) coding manual were also extremely helpful in formalizing and communicating my qualitative research ideas and analyses.

keep me focused by interrogating my approach. Just as he and Richard Wright had constantly had to consider whether they were studying burglars or burglary, Scott regularly asked me "is this a study of CPS or CPS kids?" It was an evolving process, but that friction honed my arguments and my dissertation to a point. The sentence that I wrote in big letters and posted in my work space says: "How does CPS help and hurt CPS kids?" My dissertation is a study of CPS kids.

As a study of kids in child protective custody, my dissertation is not squarely within the scope of traditional, mainstream criminology. But the overlap between CPS, criminological theory, and the practice of criminal justice (particularly regarding juvenile justice and incarceration) is substantial. If I had started with a traditional criminological problem (e.g., cops, courts, corrections) and then endeavored to write a dissertation that tested a criminological theory or evaluated some criminal justice practice by analyzing an existing data set, I would never have found CPS. But that would have been a more safe, predictable, and (perhaps) more marketable option. After all, what hiring committee cares about an *n* of 38? How generalizable is *that*? Is this study of child welfare even criminology? (Scott never asked me either of the first two rhetorical questions, but asked the third one like a million times.) In graduate school, I published enough to not perish, but I saw that time primarily as an opportunity to explore new ideas, develop expertise, and collect some data.

I was raised to believe that if I have an advantage or skill, I only have it to share. I witnessed the power of labor unions, mutual aid, community justice, and solidarity. I entered the field to hang out with some orphans, not to do research. I did not initially see a CPS dissertation as a marketable criminological product. I was also not always sure I wanted to mix the rigors of academia and the "happy place" I had developed with Free Arts. But after years in the field, semesters of grad school, and through many conversations, it became clear that criminological theories had a very hard time explaining CPS-related phenomena. Should we expect CPS kids to do worse over the life course after placement due to separation from the control of their parents and increased associations with other troubled youth in custody? Or should we expect them to do better due to increased access to services and association with prosocial adults? In other words, how does CPS help improve the life course trajectories of the kids they serve and/or hinder their progress to "successful" adulthood? What even *is* successful adulthood? Do we expect 18-year-olds that do not age-out of CPS to be entirely and sustainably self-sufficient? Research regularly suggests that placement in CPS is a risk factor for subsequent offending, victimization, and a host of negative social, mental, and physical outcomes over the life course, even controlling for confounding factors (Russell, Kerwin, & Halverson, 2018). So why do we still do it? If court actions should be "in the best interest of the child," and CPS is a risk factor, is placing kids in CPS even legal or ethical? How do these questions inform the study of criminology and criminal justice?

These huge questions have implications for the study of human development, the administration of justice, and the role of the state in both. They therefore place my work in conversation with criminology giants like Travis Hirschi, Rob Sampson and John Laub, Peggy Giordano, Cathy Spatz-Widom, Erving Goffman, and Barry Feld to name a few. That is daunting company to a new criminologist working through grad school but not so much for a street kid from Detroit. Accordingly, Scott and my

dissertation committee (Drs. Danielle Wallace and Michael White)[9] pressed me to flesh out my ideas and recommended more readings to help make sense of what I was finding. Instead of encouraging me to defer to those giants, my committee pushed me to climb on their shoulders and see what I might see. In one long sentence, my ethnographic dissertation was the product of a supportive (and always productively incredulous) faculty; an intrepid grad student willing to take risks, make long-term commitments, and invest in their community; several agencies and service providers interested in learning more about their clients; and 38 abused, neglected, and otherwise traumatized and institutionalized young people in my local area.

Every summer, Free Arts of Arizona conducts "Theater Camp" at the Herberger Theater in downtown Phoenix. Over 2 weeks, over 100 CPS kids meet daily with teaching artists, mentors, and group facilitators to write, develop, and produce a theatrical performance based on their own narratives. In three previous camp seasons I had served as a volunteer group facilitator, and after years in various volunteer roles, I was (and am) an informal mentor to many youth I have worked with and befriended over the years. But in 2015, I took the opportunity to deploy a formal, semi-structured set of interviews with youth currently in CPS custody. I had interviewed young adults in CPS custody voluntarily, but for minors, my IRB wanted stronger safeguards than I had used before. Primarily, the IRB was concerned that since my instrument contained Likert-type scales, I should not need to audio record my interviews (they suggested I should be able to hand note the open-ended responses). They also suggested I include a social worker or other trauma-informed mental/behavioral health professional to sit in on the interviews and assist any respondent who might experience distress. Some of my qualitatively inclined colleagues furrow their brow at the first suggestion. But for better or worse, I deferred. This restricted my data to what I could write down, but I was able to elicit some meaningful qualitative narratives and captured some interesting demographic, family history, and criminological nuance.

The second suggestion (for an on-site social worker) turned out to be a valuable asset to the project. No respondents displayed or reported distress, but having Nelly[10] there helped me reflect on the interview process as we moved through data

---

[9] Throughout this process my committee was invaluably helpful in honing my dissertation. Dani helped me plan my analysis and was a constant source of support. Without Dani's guidance through the perils of grad school and professional academia, there is no telling where I, or my dissertation, would be today. As a first generation, working class academic like me, her perspective on my development as a professional scholar has always been uniquely insightful. Mike was instrumental in making sure my work produced meaningful implications and was accessible to criminologists and criminal justice professionals not steeped in the CPS literature. His keen eye and advice were priceless. In fact, during my first prospectus defense (I had two), Mike singled out the term "CPS Paradox" and suggested that as a concise, compelling title.

[10] Nelly [pseudonym] is a social work student who works professionally with special needs youth in CPS. She agreed to participate as the trauma-informed practitioner for a month, five hours a day, five days a week, as a volunteer on my unfunded dissertation. The IRB's insistence on an on-site behavioral health professional would have been a steep hurdle for my project without voluntary participation. In this case, having some friends in the social work department paid off big time. But even my meeting Nelly was serendipitous. Friendships and informal networking are a key tool in

collection, and I think having some gender diversity on the research team (in this case, a man and a woman) helped set a balanced and safe tone for the interviews. It also bears saying that Nelly, the social worker, knew many of the kids at Theater Camp through her work as a group home staff member. The "street cred" that comes from time spent in the trenches (and demonstrating respect, accountability, and trustworthiness) cannot be understated when dealing with abused and neglected youth who are growing up in CPS (Cheng, 2018). More than one respondent commented to Nelly at the end of their interviews that they were not going to participate until other kids had told them that I was "cool." I've never been in CPS, but coolness goes a long way where I come from as well. Once as a new weekly mentor at the group home, one of my mentees threatened another mentee with a pair of scissors we were using to do a multimedia art project. When I told him to chill out and put them down, he told me to "come and get 'em if I want 'em." As I walked over to him to snatch the weaponized scissors, I thought that this would be a quick, violent, and unceremonious end to my art mentorship career. But when I got to him, he said "damn dog, I was just playin'."

During Theater Camp 2015, I berated "Bradley," who had climbed a 20-foot, cactus-lined wall at a restaurant where we were a (uninsured) group of 100 CPS kids. The owner is a friend of Free Arts (if you're ever in Phoenix, hit up Free Trade Café on Central!) who had donated the space and catered lunch for us. When he told me that he would come down when he wanted, I assured him that I would whoop his ass in front of everybody when he did. He came down, and through an intermediary (another group homie that had asked me about cognitive behavioral therapy and neuroplasticity during our interview), we made peace. He said he did not know about the insurance, the donated space, or my friend that put herself in economic danger to help some kids. I told him I did not mean to belittle his manhood. We apologized, dapped-out (fist-bumped), and went back in, both with our pride in check.

I also once cried during rehearsal at the Herberger because playing a scumbag stepdad character was triggering[11] me. Events like these (especially the one about me crying) are still brought up by kids I work with now and have become part of the Free Arts folklore. They also demonstrate some of the potential perils of working in

---

the ethnographer's tool box, and the importance of nurturing relationships (by helping others) cannot be overstated.

[11] "Clinical descriptions of PTSD emphasize the importance of flashbacks, powerful multisensory image-based memories **triggered** by reminders in which traumatic events are reexperienced in the present rather than in the past…" (Brewin, Lanius, Novac, Schnyder, & Galea, 2009, p. 369; emphasis added, citation removed). In this case, the theater scene that preceded mine was a recreation of family members screaming at and over each other. The sound (all day long) of them screaming, the sight of the dinner table they were standing around, and the stress of being that raw on stage made me feel like I was small and weak and at the mercy of scary, violent adults. I grew up in a home where yelling, hitting, belittling, and insulting was fairly common. Revisiting those experiences makes me feel like a cornered rat. I told the 50 or so kids assembled around the stage that if I started hitting people there, I would get physically tired before anyone could stop me. They responded with a huge group hug. I cried. The show went on. In ethnography, the researcher is the data collection tool. This process often involves self-reflection in the trenches that was unanticipated at the outset.

the field with traumatized youth. For me, it has been a journey that has included continual challenges, victories, and losses, as well as excitement, sadness, and joy. My dissertation ended up including mixed-methods field interviews with 38 minors in CPS and 3 case study narratives of young adults who have recently aged out of custody. I have uncovered some interesting characteristics of the group. The kids I talked with reported a remarkably high level of adverse childhood experiences (ACEs) measured via the ACEs scale[12] (Felitti et al., 1998). They were not generally shocked by the abuse and neglect questions but recoiled pretty consistently at the "code of the street" questions. Their narratives evoked both positive effects (e.g., stable housing, reduced delinquency, and victimization) and negative consequences (e.g., missing their siblings and feeling powerless to help them).

Far from being the final word on the developmental impacts of CPS, my dissertation is the beginning of what could be a long and fruitful research agenda. Many important criminology and criminal justice-related questions have been raised. Most importantly, CPS has traditionally been ignored by criminologists, despite the substantial overlap between the individuals and institutions involved with CPS and criminal justice systems. My ethnographic dissertation makes clear that the study of CPS kids has important implications for social control, learning, and strain theories. A second result of that project is the identification of CPS as a fundamental component of a "bifurcated" system of juvenile justice. In other words, a juvenile court judge can place kids in CPS or jail depending on the perceived needs and risks of the kid and family. Along the way, I was able to assist Free Arts in establishing some quantifiable metrics for success and program evaluation protocols that helped the agency to secure a 2.5 million-dollar grant from Bob and Renee Parsons Foundation in 2017. That grant funded the purchase of a new building that allows for on-site programming. The grant also includes funding for the summer Theater Camp series through 2019.

My journey into ethnography has been fruitful in both tangible and intangible ways. Professionally speaking, as a graduate student interested in an interdisciplinary ethnographic dissertation, I was required to "make the case" that I would be able to attain and maintain access in the field and that I would be able to see the project through. I also wanted to have a final product that would demonstrate a level of expertise consistent with the tenure-track professorships for which I would soon be applying. I always felt confident in my theorizing, my research, and my committee. But you never really know until people outside of your department start considering, critiquing, and commenting on your work. In 2014, I was honored with ASU's "Martin Luther King, Jr. Student Service" award. That award brought with it university-level publicity that put my work on the radar of my dean as well as my university's president. I was also invited to do an interview on our local PBS affiliate. Taken together, the award process added a level of legitimacy to my graduate work, and boosted my confidence in my dissertation immeasurably.

---

[12] The average score in my dissertation was 6.26/10. This score places the group well above the 85th percentile nationally. In other words, the US Center for Disease Control (CDC) estimates 87.5% of the population has an ACEs score of 3 or less, while only 12.5% score 4 or higher. https://vetoviolence.cdc.gov/apps/phl/resource_center_infographic.html.

More recently, in 2017 I was awarded a training fellowship for emerging child abuse researchers funded by the National Institute of Child Health and Human Development (NICHD). My 14 colleagues at the summer institute held at George Washington University at St. Louis were mostly medical doctors, clinical psychologists, and social work researchers. I was one of only two criminologists and the only ethnographer of the group. Many of the trainers and trainees were involved in exciting clinical trials that involved experimental designs, psychological interventions, and biological measures. Going in I was, let us say, cognizant of these facts. But the grant was administered by Drs. Cathy Spatz-Widom and Melissa Jonson-Reid, two prominent researchers in criminology. And my long-term fieldwork experience and relationships with youth and young adults who had been placed in CPS provided a novel perspective in that group. Despite my initial apprehension about how noncriminologist and non-ethnographers would perceive my work, I was able to contribute meaningfully to the group in discussions, and my resulting grant application extends the findings of my dissertation into a future set of focus groups to analyze the social support networks of young adults as they age out of the system. Stated plainly, the clinical "quantoids" did not hate on my work, they welcomed my perspective. I learned from them, and they learned from me. That week-long training fellowship was one of the most enriching experiences of my graduate studies.

I have also gained friends and a professional network that I never would have if not for my volunteer service and the fieldwork it facilitated. CPS kids that I met when they were 13 and 14 are entering their 20s now. I now have friends that are social workers, professional and teaching artists, and program administrators. Many of these relationships have involved research and program evaluation collaborations. As Scott was able to help me in sagelike ways as I started planning my dissertation, I have been able to offer what I think is sound advice to colleagues interested in "thinking qualitatively." I always suggest reading *Armed Robbers in Action*, *Robbing Drug Dealers*, and *Life in the Gang*. Then reading Sarah Tracy's work about qualitative rigor (2010) and methodological processes (2012). Johnny Saldaña's (2015) work on coding and analyzing qualitative data is like a *Choose Your Own Adventure*[13] treasure map for developing a systematic analysis plan. But more generally, I always suggest (as Scott, Dani, and Rob Fornango suggested to me early on) that if you are willing to work hard for an indefinite amount of time, and take responsibility for whatever happens, then an ethnographic approach to research can be an accessible way for a student of the social sciences to start from the "ground level" of the social phenomena they are interested in studying.

I have until recently tempered that advice with an acknowledgment that I did not yet know how my dissertation would play once I hit the tenure-track academic job

---

[13] *Choose Your Own Adventure* is a series of children's gamebooks where each story is written from a second-person point of view, with the reader assuming the role of the protagonist and making choices that determine the main character's actions and the plot's outcome. *Choose Your Own Adventure*, as published by Bantam Books, was one of the most popular children's series during the 1980s and 1990s, selling more than 250 million copies between 1979 and 1998. https://en.wikipedia.org/wiki/Choose_Your_Own_Adventure.

market. I can now say that my ethnographic dissertation was well received on the market. And although my first job talk accidentally went well beyond my time limit, the work sparked discussions about theoretical, system-level, and individual-level implications with an enlightened faculty that ultimately saw fit to hire me. It seemed to me that two of the most desired professional traits that were noted on the calls for applications in the 2017/2018 market were "community engagement" and a "commitment to diversity." My work with CPS kids and collaborations with local charities, shelters, museums, libraries, theaters, and service providers provided a lot for me to talk about in my letters of interest and application materials. So I also always suggest volunteering as a functional way to get out into the field and do some exploring.

Reflections from the Beginning and End of Careers in the Field

In retrospect, were the risks taken in conducting fieldwork worth the outcomes? After all there is inherent danger in conducting fieldwork with active offenders, and we are aware of threats experienced by our own colleagues in the course of such research. The risks include those to personal safety as well as to the other members of the research team. There are also potential risks to the reputations of those who do qualitative work. They can be accused of "going native" and getting too close to their subjects, which in turn may shade their observations, interpretations, and conclusions. This is particularly important given the increasing role that fieldwork plays in policy. "Translational Criminology" now mandates that researchers use the products of their research to improve policy, create interventions, and address social problems. Such a focus, of course, is not new. Fifty years ago, Howard Becker (1967) posed the question "Whose side are we on?" Becker argued that sociologists (especially those who study deviance and crime) are caught in a "crossfire" between individuals who argue that the demands of science require objectivity and distance and those who argue that the needs and challenges of research subjects must be reported in a way that addresses policy and improves their lives.

The recent controversy surrounding the work of Alice Goffman (2014) underscores many of these challenges. Goffman documented the extreme challenges and constant police harassment faced by young black men in Philadelphia. She has been accused of overstating the findings of her data, being involved in the lives of her subjects outside of the purview of the research, and shading her story. The criticisms of her work have taken on a personal tone at times and led to a lack of stability in her academic career. Sudhir Venkatesh's (2008) *Gang Leader for a Day* sparked controversy about the distance field workers should maintain from their subjects and the extent to which they should participate in the activities of their subjects. He has subsequently faced scrutiny over IRB and budget issues. These are risks that quantitative researchers are not immune to, but they seem to garner less publicity

and negative scrutiny. Perhaps it is the provocative nature of much fieldwork that accounts for such attention. It strikes us as well that there are others who have met those risks and succeeded. Jamie Fader's (2013) phenomenal ethnography of young men succeeding upon reentry, Jody Miller's (2008) highly contextualized work with young women who walk the edge between exploitation and self-respect, and Desmond Upton Patton and colleagues' (Patton, Eschmann, & Butler, 2013; Patton et al., 2014) work with young people and their use of social media are three examples of sound qualitative research that also meets Becker's call to action.

There are other risks to fieldwork. One of those is the slow pace at which it can proceed and consequently the large investment of time necessary to produce scholarly products. While qualitative work appears well-suited to support monographs, the investment of time in such research is substantial. The situation would be easier if there were more data sharing among qualitative researchers, but this is an area that lags behind quantitative research. One of the institutional risks to fieldwork comes with the IRB. Few IRBs are equipped to deal with ethnographic proposals. Indeed, criminologists have documented (Jacques & Wright, 2010; Tapia & Martinez, 2017) the many challenges faced by ethnographers in working through human subjects issues. Facing the same IRB Jacques and Wright wrote about, an earlier proposal by Decker was turned down because one member of the board told us that the research "depressed" her and she couldn't vote for it under those circumstances. We have found being proactive with the ASU IRB has made a difference. When we have what we know will be a difficult proposal (like interviewing high-level drug dealers in federal prison, interviewing large-scale marijuana growers in states that have legal marijuana sales, or reviewing cases of potential rape that were misclassified by a police department), we write up the case and meet in person with the section head of the social science IRB. This process allows us to make our case clearly and directly and counter or adapt to concerns raised by the chair of the IRB. For our CPS work, considering trauma-informed practice has also helped address our IRB's concerns and minimize risk to our participants.

Despite these challenges, many of which are substantial and largely unique to qualitative work, the benefits in our mind outweigh the challenges. First, the scholarly products of fieldwork are essential for science to move ahead. The trend toward "mixed methods" (Maruna, 2010) is undeniable, both for basic science as well as applied policy analysis. This is largely based on the depth of understanding of individuals, processes, and organizations that is produced by qualitative work. Second, qualitative work confers an added degree of legitimacy for the researchers who conduct it. This is particularly true in working with agencies. There is some skepticism, indeed antipathy, toward researchers on the part of many agencies, particularly law enforcement and child welfare agencies. The idea that Scott has interviewed hundreds of gang members in a dozen cities and been part of a team that interviewed more than 100 *active* residential burglars and nearly 90 *active* armed robbers resonates with police at every level of management. Gabriel's record with Free Arts as a volunteer, youth mentor, and program evaluator has also opened doors to new research opportunities. When done well, the products of fieldwork are well-regarded professionally. Look at the book awards of SSSP, ASC, and ACJS, and you will find

books based on fieldwork overrepresented. The Editorial Board of the ASC reports that qualitative papers have a higher probability of being published than do quantitative papers. For the first time in its history, the journal *Criminology* has an ethnographer (Jody Miller) as a co-editor and several ethnographers on the editorial board.

We are often asked, "would you recommend qualitative work to a doctoral student?" These are challenges that mentors and dissertation chairs have to take very seriously, as students may not find that qualitative work is the most direct path to a dissertation. As ethnographers, we think the answer is best determined by a series of questions: Do they write well? Can they deal with the multiple challenges of the field? Do they have a passion for the work? Are they well-prepared to find contacts in the field and maintain access? If some of these answers are a "yes," we enthusiastically encourage them to give it a try.

References

Anderson, N. (1923). *The hobo: The sociology of the homeless*. Chicago: University of Chicago Press.

Becker, H. S. (1967). Whose side are we on? *Social Problems, 14*(3), 239–147.

Bernard, H. R. (2012). The science in social science. *PNAS, 109*(5), 20796–20799.

Brewin, C. R., Lanius, R. A., Novac, A., Schnyder, U., & Galea, S. (2009). Reformulating PTSD for DSM-V: Life after criterion A. *Journal of Traumatic Stress, 22*(5), 366–373.

Brezina, T., Tekin, E., & Topalli, V. (2009). "Might not be a tomorrow": A multimethods approach to anticipated early death and youth crime. *Criminology, 47*(4), 1091–1129.

Cheng, T. (2018). Recruitment through rule breaking: Establishing social ties with gang members. *City & Community, 17*(1), 150–169. https://doi.org/10.1111/cico.12272

Decker, S. H., & Van Winkle, B. (1996). *Life in the gang: Family, friends and violence*. New York: Cambridge University Press.

Fader, J. J. (2013). *Falling back: Incarceration and transitions to adulthood among urban youth*. New Brunswick, NJ: Rutgers University Press.

Felitti, V. J., Anda, R. F., Nordenberg, D., Williamson, D. F., Spitz, A. M., Edwards, V., et al. (1998). Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: The adverse childhood experiences (ACE) study. *American Journal of Preventive Medicine, 14*(4), 245–258.

Goffman, A. (2014). *On the run: Fugitive life in an American City*. Chicago: University of Chicago Press.

Goffman, E. (1968). *Asylums: Essays on the social situation of mental patients and other inmates*. Piscataway, NJ: Aldine Transaction.

Hudson, K. L., & Collins, F. S. (2015). Bringing the common rule into the 21st century. *New England Journal of Medicine, 373*(24), 2293–2296.

Jacobs, B. A. (2000). *Robbing drug dealers: Violence beyond the law*. Piscataway, NJ: Transaction Books.

Jacobs, B. A., & Wright, R. (2006). *Street justice: Retaliation in the criminal underworld*. New York: Cambridge University Press.

Jacques, S., & Wright, R. (2010). Right or wrong? Toward a theory of IRBs' (dis)approval of research. *Journal of Criminal Justice Education, 21*, 42–59.

Katz, J. (1988). *Seductions of crime*. New York: Basic Books.

Manning, P. K. (2015). Qualitative research as theorizing. In J. Miller & W. R. Palacios (Eds.), *Qualitative research in criminology* (pp. 51–66). Abingdon, UK: Routledge.

Maruna, S. (2010). Mixed method research in criminology: Why not go both ways? In A. R. Piquero & D. Weisburd (Eds.), *Handbook of quantitative criminology* (pp. 123–140). New York: Springer.

Miller, J. (2008). *Getting played: African American girls, urban inequality, and gendered violence*. New York: NYU Press.

Patton, D. U., Eschmann, R. D., & Butler, D. A. (2013). Internet banging: New trends in social media, gang violence, masculinity and hip hop. *Computers in Human Behavior, 29*(5), A54–A59.

Patton, D. U., Hong, J. S., Ranney, M., Patel, S., Kelley, C., Eschmann, R., et al. (2014). Social media as a vector for youth violence: A review of the literature. *Computers in Human Behavior, 35*, 548–553.

Russell, J. R., Kerwin, C., & Halverson, J. L. (2018). Is child protective services effective? *Children and Youth Services Review, 84*, 185–192.

Saldaña, J. (2015). *The coding manual for qualitative researchers*. Thousand Oaks, CA: Sage.

Shaw, C. R. (1930). *The Jack roller*. Chicago: University of Chicago.

Sutherland, E. (1937). *The professional thief*. Chicago: University of Chicago Press.

Tapia, M., & Martinez, R. O. (2017). Ethics review and the minority ethnographer: A case study in racialized invalidation. *Race and Justice, 17*(2), 127–143.

Thrasher, E. (1929). *The gang*. Chicago: University of Chicago Press.

Topalli, V. (2005). Criminal expertise and offender decision-making: An experimental analysis of how offenders and non-offenders differentially perceive social stimuli. *British Journal of Criminology, 45*(3), 269–295.

Topalli, V., & Wright, R. (2014). Affect and the dynamic foreground of predatory street crime. In J. L. Van Gelder, H. Elffers, D. Reynald, & D. S. Nagin (Eds.), *Affect and cognition in criminal decision making* (p. 42). New York: Routledge.

Tracy, S. J. (2010). Qualitative quality: Eight "big-tent" criteria for excellent qualitative research. *Qualitative Inquiry, 16*(10), 837–851.

Tracy, S. J. (2012). *Qualitative research methods: Collecting evidence, crafting analysis, communicating impact*. Hoboken, NJ: Wiley.

Venkatesh, S. (2008). *Gang leader for a day*. New York: Penguin.

Whyte, W. F. (1943). *Street corner society*. Chicago: University of Chicago Press.

Wright, R., & Decker, S. H. (1994). *Burglars on the job*. Boston: Northeastern University Press.

Wright, R., & Decker, S. H. (1997). *Armed robbers in action*. Boston: Northeastern University Press.

Wright, R., Decker, S. H., Redfern, A. K., & Smith, D. L. (1992). A snowball's chance in hell: Doing fieldwork with active residential burglars. *Journal of Research in crime and Delinquency, 29*(2), 148–161.

Wright, R., Jacques, S., & Stein, M. (2015). Where are we? Why are we here? Where are we going? How do we get there? The future of qualitative research in American criminology. In Miller, J., and Palacios, W. R. (Eds.), *Qualitative Research in Criminology*, 339–350. New Brunswick: Transaction Publishers.

"Did I Just Get Caught Being Stupid?" Experiencing and Managing the Emotional Labor of Fieldwork

Heith Copes

My family comes from small rural towns in Northeast Louisiana near the Mississippi Delta. Growing up I enjoyed visiting these places immensely. My father's parents lived out in the country. In the back were crops (usually cotton or soybean), the side had a personal garden, or "pea patch" as we called it, and in the front ran a small bayou. The place felt warm and comforting, but as I got older, I began to see another side of these rural towns. While there are great things about them, there are also high levels of poverty, vast economic inequality, a general lack of resources, and limited opportunities. There is also a growing problem with illegal drug use. As a criminologist, I have long wanted to go back to these towns and sit with the people who live there and see how they make sense of their lives in the midst of rural poverty and drug use. Unfortunately, my family's home is a long way from Birmingham, Alabama, where I currently live, so I have not been able to make this happen. Then an opportunity came to study methamphetamine use in rural Alabama, and I thought it isn't home, but it is close.

My plan was to do a photo-ethnography of people who use methamphetamine on Sand Mountain, Alabama. I teamed up with photographer Jared Ragland to document the lives of people who were actively using methamphetamine—or "dope" as they call it. I would document their lives through their stories and Jared would take photographs. Our larger goal was to show the complexity and humanity of people

H. Copes (✉)
Department of Criminal Justice, University of Alabama at Birmingham,
Birmingham, AL, USA
e-mail: jhcopes@uab.edu

who use meth and present a counter-narrative (or counter-visual) to the various anti-meth campaigns that stigmatize and demonize those who use the drug.

As is typical with ethnographic research, the first few months were slow going in terms of recruiting people and making contacts. Eventually we were able to gain the trust of people in the area. Several months into the project, Jared and I decided that we would stay the night up on the mountain. That way, we could be around if something exciting happened. We got a call from our friend JC who said he had someone for us to meet. This new person was a longtime user, dealer, and manufacturer of meth, and he sounded like a good contact and source for the project. We got his address and headed out to his place. After driving a way down winding roads and through swaths of farmland, we finally found the place. It was a bit of drive—far enough that we lost cell phone coverage. It was an old trailer set on hilly open land. As we pulled in, I noticed a swastika nailed to the telephone pole. As the lights of the truck shown on the trailer, a large swastika painted on one side and an anarchy symbol on the other became visible. A tattered American flag flown upside down was waving. The front yard was lined with large pine trees, and what looked to be a noose hung from one of the branches. There was a large hand-painted sign facing the road. One side read: "Not all are welcome." The other side read: "Don't get caught being stupid." As we got out of the car, a shirtless man holding a machete appeared in the doorway of the trailer. He jumped down over the makeshift stairs and began walking quickly toward us, yelling incoherently. At that moment I thought: "Did I just get caught being stupid?" (Fig. 1).

Fig. 1 Chico with his "Don't Get Caught Being Stupid" sign

Fortunately, Chico meant us no harm and went on to be very open and honest with us. He even helped recruit other participants. But as I learned, there are other ways of getting caught being stupid.

I have been interviewing people for more than 15 years. I have spoken with hundreds of people who have committed crimes as diverse as carjacking, identity theft, bar fighting, drug manufacturing, and theft. I've been to dozens of prisons and halfway houses. Because of these experiences, I assumed this project would be no different than the others. I underestimated the emotional costs of this project. Sure, I would hear sad stories, but I've heard these sorts of stories for a while now. Something was different this time.

For the first 7 or so months of the project, I would often come home exhausted. Following a day on the mountain, I would need another day to rest and recoup. I assumed this was just because of the driving (Sand Mountain is close to a two-hour drive from my house) or because I'm getting older and don't have the energy that I once had. Then one day I received a text from Alice.

Alice is a quiet and often awkward 21-year-old girl, but there is something very likeable about her. I first met Alice at Chico's trailer. She didn't really talk to us much, so it was surprising when she reached out to me. She had recently reunited with her boyfriend, Ryan, who had been in jail. Ryan is 22; he has a youthful, energetic charisma. The two of them were going to try and make a clean start, get off drugs, find jobs, and move forward. I knew they had a great deal going against them—they had no jobs, no family to help, and no place to live—but I was rooting for them and offered my support. After a few meetings with Alice, she sent me a text message. The message she sent was a picture of a shot glass of water and spoon (the tools needed to shoot meth intravenously), with the caption: "Struggling." My heart sank. I knew then that they wouldn't make it. It also was then that I realized I had become emotionally invested in the people on the mountain.

Weeks later I was with Misty and her son Michael, visiting her husband JC, who was on work release at the time. We were visiting him after his shift had ended. As Misty and JC talked, I decided to give them some space and walked to the other side of the parking lot. Michael, who was wearing JC's hard hat and security vest, walked up to me and said, "Why do you worry all the time?" The question caught me off guard. I really like Michael, but he isn't the most sensitive 8-year-old. Had I been so anxious that even he could sense it? I didn't know how to respond so I just gave him a hug and answered, "I don't know?" (Fig. 2).

This question caused me to really think. Do I worry that much? Why am I so anxious? I started recounting some of the events that had occurred since we began the project and remembered:

– The day I texted Misty to ask how she and JC were doing. She replied saying that he was likely going to prison because he beat and raped her.
– The day we drove Misty to visit her brother. She cried in the backseat of the car because she was going to get kicked out of her house, which would mean she would lose custody of her daughter. Three months passed before she saw her daughter again.

Fig. 2 Misty, JC, and Michael visiting after work release

- The night I spoke with Alice when she received a text from her daughter's father saying that he was not going to let her see her child again. I held her as she cried uncontrollably.
- The night we went to Chico's house to find him crying because Alice left him. He was carrying a letter she wrote for him, reading it over and over.
- The day I received a text from Alice to tell me Chico had put a bounty on her head. She said she had sat in the middle of her trailer holding a knife all night because she was so afraid (Fig. 3).

And these were just the emotional tragedies of Misty, Chico, and Alice! There are numerous other people I've come to know who have had equally, if not more, tragic events happen during the project. It finally dawned on me that it wasn't the driving or my age that was making me so tired. Instead it was the emotional labor of experiencing the tragedies and suffering of the people I had come to care about. I realized I hadn't been eating much (I lost 22 pounds), and I hadn't been sleeping well. I was physically responding to the stress.

But the weight of everything really hit at a trailer park. The place should have been condemned—the trailers were old and dilapidated, with holes in ceilings, walls, and floors. Many of the doors wouldn't lock or even shut. Most were infested with roaches and other pests. The trailers were occupied by young people squatting, mothers and sons who used meth together, women who sold their bodies for drugs, and children who were suspicious that Jared and I were police. It was here that the sadness and suffering of these people became the most visible to me. On one occasion we were in

Fig. 3 Misty taking her "nerve pills" after fighting with her sister

a trailer that had no furniture or running water. A young man we were interviewing best expressed the mood of the place. He said that tomorrow he would wake up in this trailer, with no family, no opportunity for getting a job, and no hope for a future whether he was high or not, so he might as well be high. He wasn't alone in feeling this way. Many of the people we came to know lived constantly with feelings of hopelessness, abandonment, and suffering. They all had tales of people leaving them, betraying them, and treating them poorly. Few had any expectations of escape.

Being around so much suffering began to be unbearable for me. I remember standing in the trailer park one night and realizing that I can't be a part of this pain anymore. It was too much. It made me question what I was doing there. Was I doing any good for these people? Was me telling their stories doing more harm than good? Was I adding to their pain? I had a moment where I thought I would just quit and detach from all those on the mountain completely. I could go back home and never visit again.

I sought advice about my feelings and what I should do from my wife and other academics, and the message was clear from most of them—I had to stop. They pointed out that I had to take care of myself. A part of me wanted to take this advice. Certainly the anxiety would go away if I didn't go back to the mountain. But there was another part of me that thought about how everyone leaves these people. And because they are so often abandoned, they push away the good in their lives because they feel like they don't deserve it. I knew I couldn't just quit. I didn't want to give up just because things got hard, but I couldn't be a part of the suffering either. It wasn't fair to me or to those around me.

I decided to make a change in *my* life, a change in how *I* viewed and interacted with the world. I decided that I would find ways to minimize the suffering that *I* cause in the world. Rather than withdrawing, I opened myself up more and began to show kindness and concern like I had never done before. I began by giving up eating meat. This was partly in response to the poor working conditions of the food processing plants that are prevalent in the area. I sought to make sure they all had resources if they needed help. I took them to get driver's licenses, to doctor's appointments, and to other places. I brought them food when I could. On a more intangible level, I made a point of being kind and respectful to everyone. I offered encouragement to those in need. A few responded by trying to take advantage of me (playing up sad events to get rides or other material goods). While this hurt my feelings a bit, I decided that it wouldn't matter how they responded; what mattered for me was that I was doing the best I could.

But it didn't stop on the mountain. I began to act this way toward everyone, including family, friends, students, and strangers. I started reaching out to students more than ever. While these changes have made me more vulnerable, I realized that the anxiety was slowly going away. Sleep became more regular. My appetite returned. I began to feel better about myself and the people around me. I began to see more beauty in people. I enjoy interacting with others (especially kids) more than ever before. In short, exposure to the true hardships of these people's lives and the necessity of a mental shift has all in all made me more compassionate. I also recognize that living among them without interludes would undoubtedly wear me down.

Then the people around me began to act differently as well. Chico, who at first came across as mean and aggressive, began to open up. He sent a text saying, "It's definitely been a pleasure getting to know you Heith, I'm a better man for knowing you." For my birthday Kristy sent a text:

> You're such a role model in my life and you have saved me more than you know. So thank you and have a wonderful birthday from my lil family. P.S., you have become part of our family. God sent you and Jared to help me realize life is more than the way I was living. So thank you. I hope your day is full of joy and happiness. We all love you like family.

Even Alice, in her own awkward way, has let me know that she appreciates our friendship. Alice has been off meth for a while now. And although she has occasional "mess ups," she is on a road to being free from meth. Her boyfriend told me that her recent success was largely due to my support and encouragement (Fig. 4).

Now when I think back to that day at Chico's, I realize that I did get caught being stupid. I didn't account for how the project would affect me at a human level. But in the end I think I've come out a better person. I recognize that saying kind words and hugging people won't stop the suffering in the world, but it is something that I can do everyday to make things better for myself and for others—even if momentarily. I believe that beauty can be found in a single moment, and if someone can feel better because of something I can give, then it is absolutely worth it. While I understand that such investment in those we study is not for all ethnographers, I do believe my openness with them made it easier for them to open up to me.

Fig. 4 Alice working after getting clean from methamphetamine

Process and Insight in Prison Ethnography

Ben Crewe

All projects start somewhere, and their beginnings are usually instructive. In around 1999, when I was a doctoral student at the University of Essex, I attended a seminar by Eamonn Carrabine on the Strangeways prison riot of 1990. In his talk, Eamonn mentioned that uniformed staff in the prison had referred to it as 'The Lady'; and something about the intimacy of that term hooked me in. Why was the prison female, and what did this imply about the affective bond between officers and the institution? I had never been to a prison before and had barely considered prisons as an object of inquiry. But, at the time, I was coming towards the end of a PhD on a topic that involved the close analysis of narratives of class and masculinity, and so this gendered inflection—this hint that prisons were shaped by subtle, gendered discourses—was tantalising.

To understand these discourses, affiliations, and subjective meanings required that I get 'close to the action'. In 2002, then, I began a post-doctoral research project on 'Masculinity and modern penal culture', which entailed a 10-month period of immersion within HMP Wellingborough, a medium-security establishment in the UK, holding male prisoners. By this time, I had made myself familiar with the canon of prison sociology and had noted, with genuine excitement, Rod Morgan's (1997) comment in the second edition of *The Oxford Handbook of Criminology* that: 'Few social scientists have been permitted to set up their anthropological huts on British prison landings' (pp. 1176–1177). With all of the hubristic ambition of an early career researcher, I therefore resolved to write an ethnographic account of prison life in the tradition of Gresham Sykes and James Jacobs, based—like their books—on sustained presence within a single research site.

Sykes's (1958) *The Society of Captives* is still my benchmark and beacon. But as a text shaped by the prevailing framework of structural functionalism, its account of the prison as a 'social system' was slightly desiccated. The voices of prisoners

B. Crewe (✉)
Institute of Criminology, University of Cambridge, Cambridge, UK
e-mail: bc247@cam.ac.uk

appeared infrequently and at low volume. In Jacobs's'(1977) *Stateville*, prisoners were less muffled, but it too felt somewhat shorn of humanistic depth. As a reader, I derived little sense of prisoners' interior lives, of the ways that the prison's social structure was shaped by hungers, resentments, and needs. Prisoners were socio-political agents, certainly, but as human beings they seemed opaque. Although I did not articulate it explicitly at the time, to myself or to others, my objective was to describe the prison as a *human* social system, one that was occupied and determined by sentient, reflective agents, with biographies, fears, and desires.

This objective dictated a number of aspects of the research design and process. First, it meant a commitment to the ethnographic method and perhaps its core component: *putting the time in*. For a period of around 10 months, I regularly spent 4 days each week in the prison, driving for an hour each way from my house in Cambridge and spending either a working day or an afternoon and evening in the establishment. In this respect, it is hard not to reflect on the degree to which ethnographic research is best done at particular times of life: as a relatively young post-doctoral researcher, I had few professional or personal commitments of the kind that would now make this kind of immersion virtually unthinkable. Even then, I did not relish the time commitment, or the long periods 'hanging out' in highly unstructured ways on the prison wings, in the workshops, or in other areas of the institution. In that respect, I am nowhere near the kind of ethnographer that I wish I could be. Certainly, I am less at ease with and skilled at 'chat', less charming, and more impatient to get to the nub of the issue, analytically, than many other researchers I know. Often, on days when I approached the prison without a clear idea of what lay ahead, I did so with a coil of dread in my stomach, not because of any sense of fear but because of the interpersonal demands of ethnographic engagement. I was always more comfortable interviewing one-to-one in the small office I was given by the prison than I was just 'being there' ethnographically, without obvious purpose.

I put the time in, then, because I knew that it was the only means by which to get under the skin of the establishment and into the shoes of its inhabitants. And, of course, when someone comes voluntarily to a place where most people are held involuntarily, people notice. Many prisoners gently derided me on seeing me in the prison on Friday evenings or public holidays—didn't I have a girlfriend to see, a drink to drink, or a family to look after?—but they seemed to appreciate the sincerity of interest that my presence conveyed.

The second way in which my ambition shaped the process was in my use of life history interviews. Over the course of 2 or so hours each, and prior to each substantive interview, I asked my participants simply to tell me the story of their life. These conversations generated a depth of understanding that was invaluable. They illuminated the connections between biographical experiences—of witnessing domestic abuse, of drug addiction, of victimisation—and some of the norms of prisoner community, relating to violence against women, the stigma associated with in-prison heroin use, and attitudes to bullying. They also helped me see the links between prisoners' biographical experiences, identifications, and adaptive decisions: that is—for example—the reasons why some regarded prison officers as mindless social failures, while others considered them upstanding upholders of moral norms.

Third, in the publications that resulted from the study, I made liberal use of quotations, mainly from interviews but also from my fieldwork notes. In the (2009) book that arose from the study, *The Prisoner Society*, I sought to weave the voices of particular prisoners throughout the text, almost as protagonists. Here, the goal was not to 'give voice', in the more political sense of the term, although that was certainly not irrelevant to my aims. Rather, I wanted to convey subjective meaning; to highlight variations in the ways that prisoners were oriented to matters of compliance, hierarchy, and 'inmate norms'; and to 'represent' prisoners as full human agents: to flesh them out in all of their moral intricacy. Good ethnographers are not sentimental; they recognise the full spectrum of human behaviour, and—this seems to me to be key—they grant people their complexity. To quote the journalist Janet Malcolm, 'Real people … are so much more complex, ambiguous, unpredictable, and particular than people in novels' (2011, p. 122). Good ethnographic writing grapples with this complexity rather than seeking to diminish it. It seeks to represent people fully and authentically, without straining to present them as angels, as well as seeking to ensure that they are not seen as devils either. As Loic Wacquant (2002) notes, the role of the ethnographer is not (or, I would prefer, is not, in any simple sense) to exonerate people or 'attract sympathy for their plight' (p. 1470) but to describe and dissect 'the social mechanisms and meanings' that govern their practices (p. 1470). There is no need to feel guilty, therefore, about any ambivalence we might feel about our research participants. Rather, we should embrace it, representing them in a way that is faithful both to human and theoretical complexity, rather than flattening them out in the interest of advocacy.

Portraying prison as a human social system meant giving emphasis to elements of prison life that have received relatively scant attention. For current purposes, the example I want to discuss in more detail is that of friendship, an area of interest that, with some exceptions (such as Clemmer, 1940), is rather lightly covered in the classic ethnographies, but features prominently in more humanistic studies of prison life (e.g. Cohen & Taylor, 1971). Companionship is a fundamental human and existential need, and so its relevance to imprisonment should be obvious. As Cohen and Taylor note, however, while in prison 'the circumstances which normally impel one to seek an intimate or friend are heightened … the taken-for-granted nature of friendship is surrounded with problems in this environment' (1971, p. 63).

In Wellingborough, relations and rituals of friendship, allegiance, and affinity could be observed almost everywhere: in the ways that prisoners walked around the prison's exercise yards, mainly in twos or threes, heads down, hands in pockets, talking *sotto voce*; in the sharing of a roll-up cigarette, passed silently from a man to his associate once two-thirds smoked and drawn on tightly; in the practical jokes between wing cleaners and their affectionate teasing of each other's chances of staying free of crime or renewing relationships with loved ones. These were practices that prisoners did not articulate in interviews, in part because they were so mundane and automatic. But the interviews that I conducted—starting only after a 4-month period of reserved participation—allowed me to probe my participants on the norms of allegiance that shaped them and the submerged meanings that were attached to them.

Much of what I learned was about the inadequacy of the term 'friendship' to describe social relations among prisoners. On the one hand, most prisoners disavowed the term almost as a reflex, because it implied a greater level of trust and intimacy than prison allowed: 'There's no such things as friends in prison', I was often told, 'only associates'. On the other, many descriptions of the loyalties and intimacies that their relationships entailed surpassed the kinds of commitments that most people I knew lived by. Even when they scoffed at the notion of friendship, participants would sometimes recount stories of ultra-loyalism, compassion beyond any logic of self-interest, and intimate disclosure. They talked of profound sadness, or of swelling pride, to see men whom they had grown close to be transferred out of the establishment, of providing emotional support to cell-mates on the precipice of emotional breakdown, and of the kinds of domestic routines (a morning cup of tea; a goodnight chat) that clearly contradicted public discourses of an emotionally denuded social space.

Insight came slowly, not through moments of dizzying epiphany but through a gradual layering of comprehension that was iterative and incremental. Engaging slowly and ethnographically helped me to identify (and, at a later point, theorise[1]) important distinctions between the ways that different kinds of prisoners thought about and practiced friendship: for example, chronic drug users *were* able to make new friendships in prison, precisely because relationships in the prison felt more authentic than those in the community, as they did themselves, and because the process of self-reinvention was reinforced by supportive partnerships. It became clear, too, that, within a culture of generalised mistrust, most prisoners did, in fact, have one or two peers whom they trusted and to whom they were willing to disclose at least some personal information. Identifying these patterns, and the different terms and limit points of friendship, took time. It required me to accept that there was no simple way to theorise imprisonment, unless I was to disavow fieldwork altogether and make generalisations from the seductive comforts of an office. It also led me to a form of typological analysis, which could do justice to the range of social relations that I encountered.

Insights also derived from direct—if, of course, highly diluted—exposure. I am uneasy about claims that prioritise 'feelings' over analysis, as if the former have an innately superior status. But I would not dispute that, handled carefully and reflexively, personal experience of an environment constitutes an important form of data. Certainly, I learned something—or perhaps, more accurately, I *felt* something significant—about the nature of prison friendship through an interaction I describe in the appendix of my (2009) book. Taking a break from an interview with a prisoner whom I had got to know well (whose cell I had sat in; who had exchanged heroin in front of me; and who had opened up to me about his life), I had been required to ask him to leave the interview room when I did so myself. As I locked the door, I apologised that doing so implied that I did not trust him. I was right not to trust him, he

---

[1] Moments of bright illumination came more often after the fieldwork had ended, at the phases of analysis and writing. It was at this point—having established some distance from the kaleidoscopic confusion of the field—that I was really able to *see*.

said. 'He was a thief, he explained', and had I left him in the room, he would certainly have stolen some of my coffee and might well have looked for other items to take. When I expressed surprise, he schooled me in the logic of our relationship: we were not really friends, he reminded me; and his position of scarcity ('it's the circumstances I'm in, y'know') meant that he would exploit whatever opportunities were at hand. 'It's nothing personal', he said, with both amusement and kindness, in the face of my obvious disappointment. And I understood better the ways that deprivation shaped the relational possibilities of prison life.

With other men, I got some sense of a deeper form of prison friendship, one that entailed a tentative intimacy, without full revelation, but involving sufficient disclosure to communicate trust and recognition. I was constrained both by professional ethics and by my pledges to the prison's security department. Notwithstanding these restrictions, I was able to talk relatively candidly about many aspects of my life to men who showed interest. Indeed, when these men opened up to me or asked me questions, it felt wrong and absurd not to reciprocate. With a few participants, in particular, the mutuality of our conversations was indicative of the warmth of an unspoken bond. Inasmuch as the environment allowed it, then, these men seemed like friends, of sorts, though working out exactly what sort was, at the time, almost impossible.

In this respect too, then, my experiences echoed those that prisoners described to me. Establishing close relationships in prison was difficult, prisoners said, because you knew so little about most other people's lives before incarceration and were unlikely to see them after release. Unless you were already acquainted with them, or would return to a community in which they were active, the only part of their existence that you could reliably judge was in an environment that did not count or that in itself distorted behaviour beyond a point where it could be meaningfully evaluated. In such circumstances, you could engage with people, but only on the basis that their presence in your life was fleeting, and their persona constrained by the rules of the environment.

In recent years, it has struck me that these terms capture rather well not only the touristic position of the fieldworker but also many of the conditions that generate anxiety among ethnographers about the potentially exploitative aspects of fieldwork. More recently, in unusual circumstances, I have been able to explore such issues. As part of a small conference in October 2017, I conducted a public interview with Nathan,[2] a man whom I had first interviewed 15 years earlier, in private, when he was serving a life sentence in HMP Wellingborough, and who was now a full-time academic criminologist. Our aim was to focus on the experience of *being researched* and to reflect upon the implications of this insight for active prison researchers. Among the insights that Nathan provided were, first, that prisoners 'know the score' and that prison fieldworkers are excessively anxious about the possibility that they are exploiting their participants: the exploitation, Nathan argued, goes both ways. Prisoners are not under any illusions about the nature of the

---

[2] Here, I use the pseudonym that I gave him in *The Prisoner Society*. He has read and approved this chapter.

relationship with the researcher and are not intoxicated by the constructed intimacy of the interview situation. They are very aware that the exchange has limits, that the researcher's interest in them is primarily instrumental, and that, once sated by data, he or she will disappear from the research site. Indeed, this—Nathan claimed—was part of the appeal of getting involved in research: the knowledge that the researcher had little power or claim over the prisoner's life and offered an opportunity to safely download a set of personal experiences that had no other outlet in the environment.

Nathan also commented on my research persona. It was always obvious to me that the 'chain-linked fence' that almost always stands between the researcher and participant was, in my case, particularly thick: the prison researcher is always an outsider, but the social distance between me and most prisoners was obvious in the kinds of 'social tics and the signs of the body' (Crewe, 2009, p. 421) that were, to some degree, the focus of my inquiry. Building relationships of meaning had required me to be faithful to my lack of fit—not to pretend to be any less educated or privileged than I was and not to be embarrassed when I did not understand what I had heard or observed. This, Nathan noted, had not worked against me. If anything, he said, my 'awkwardness' in the environment had made me seem more sincere, and my 'honest vulnerability' had made me seem 'unthreatening and open'. Compared to other researchers he had encountered, whose smooth personas had been alienating, I had, at least, been *authentically* awkward. This is one reason why 'teaching' ethnographic methods is so difficult. While some procedures and practices can be learned, 'techniques' are likely to generate estrangement. The key instrument of data collection is not a technical device, or some calculated strategy of self-presentation, but the researcher's authentic personhood.

Concluding Comments

None of my subsequent research projects have involved the same degree of ethnographic commitment. A study comparing public and private sector prisons required similar techniques, but for shorter periods; a more recent piece of research on prisoners serving very long sentences comprised long interviews, but without the same focus on the institutional environment. And in the intervening years, as my workload and family have grown, the ethnographic possibilities have shrunk. But the Wellingborough research has been the foundation for every other piece of prison research that I have conducted since. It secured me a degree of self-legitimacy, a confidence that I understood the deep structures of imprisonment. Above all, it gave me a sense of the *feel* of prison, in a way that other methods cannot: its vivid humour and language (what Sykes and Messinger (1960, p. 11) call the 'pungent argot of the dispossessed'); its distinctive institutional form, expressed in drab paintwork, gurgling heating pipes, and polished floors; the shouting from windows; the bitten tongues and submerged pains. It allowed me to get to know the prison, not just as a system of social action, but in more depth and with more nuance, as a site of human life, struggle, and survival.

References

Clemmer, D. (1940). *The prison community*. Boston: Christopher Publishing House.

Crewe, B. (2009). *The prisoner society: Power, adaptation, and social life in an English prison*. Oxford: Clarendon Press.

Cohen, S., & Taylor, L. (1972). Psychological Survival: The Experience of Long-Term Imprisonment. Harmondsworth: Penguin Books Survival. Harmondsworth.

Jacobs, J. (1977). *Stateville: The penitentiary in mass society*. Chicago: University of Chicago Press.

Morgan, R. (1997). 'Imprisonment', in Maguire, M., Morgan, R., & Reiner, R. (Eds.). *The Oxford handbook of criminology* (2nd ed.). Oxford: Clarendon Press.

Malcolm, J. (2011). *The journalist and the murderer*. New York: Vintage Books.

Sykes, G. (1958). *The society of captives: A study of a maximum security prison*. Princeton: Princeton University Press.

Sykes, G., & Messenger, S. (1960). The inmate social system. In R. Cloward, D. Cressey, G. Grosser, R. McCleery, L. Ohlin, G. Sykes, & S. Messinger (Eds.), *Theoretical studies in social organization of the prison* (Pamphlet 15, pp. 5–19). New York: Social Science Research Council.

Wacquant, L. (2002). Scrutinizing the street: Poverty, morality, and the pitfalls of urban ethnography. *American Journal of Sociology, 107*(6), 1468–1532.

Fieldwork with Homicide Detectives: 60 Minutes of Reflections from a British and American Criminologist

Dean Dabney and Fiona Brookman

The Interview: Setting the Scene

DD   My name is Dean Dabney. I am a professor of criminal justice and criminology at Georgia State University. I've been conducting fieldwork in organizational settings for 25 years; the last 10 years concentrated on large urban police departments. I'm here today in Philadelphia with Fiona Brookman, Professor of Criminology at the University of South Wales, an accomplished qualitative researcher with a track record of conducting fieldwork within police organizations. It's November 16, 2017. We were asked by Steve Rice and Mike Maltz to provide some first person insights into the distinct methodological journey we have undertaken to capture and elaborate on the "texture" of our past ethnographic fieldwork within police organizations. We have chosen an informal conversational format to capture and convey these insights. As such, we're going to have a conversation regarding our experiences of doing fieldwork in police departments, with a special emphasis on doing such work in homicide units within the USA and UK. The format will involve me throwing out a topic to Fiona for her to respond to reflecting upon her US- and UK-based fieldwork projects. From there, I will add my own observations on the topic at hand by reflecting on my US-based homicide unit fieldwork.

What follows is our conversation. The text has been edited slightly to remove any unnecessary repetition and to clarify any confusing passages. We have also added some headings that organize the conversation around key themes that reflect distinct

D. Dabney (✉)
Georgia State University, Atlanta, GA, USA
e-mail: ddabney@gsu.edu

F. Brookman
University of South Wales, Pontypridd, UK

considerations or issues "in the field." Otherwise, it is an accurate verbatim account. We have also included, at the end of this interview, a short list of some of our favorite pieces of qualitative research in this area and a few of our own pieces that emerged from our own ethnographic work.

Why Fieldwork and Why Homicide Detectives?

DD   To start off with, I thought we would talk a little bit about the draw of fieldwork in general. Specifically, when you got interested in policing research, why did that marriage to fieldwork make sense to you?

FB   My research at police units has mainly been with homicide detectives. Fieldwork just seemed like the only way to get to the heart of what I hoped to explore with them. I had previously conducted qualitative interviews with burglars (for my Master's Thesis), murderers, and other violent offenders (for my PhD and other projects that followed) and for all of those pieces of research I always set out to speak to offenders and gather their perspectives. I'd always thought, you know, to really, really understand their crimes and their world, you need to spend some time with them—but in the UK it is very rare for criminologists to conduct research with active offenders and so my interviews with offenders had usually taken place in prisons on a one-to-one basis. So, it was a logical extension for me to want to interview detectives but of course I now had the chance to try to spend time with them, to undertake some sustained fieldwork. So yes, when considering a method, I just thought I would need to get as close to them in their world as I could really. I was also generally a fan of ethnographic research and wanted to try it out for myself.

DD   Did you find it a difficult transition from offender-based research to ethnography with police?

FB   Do you know, I can't even remember how it happened. I cannot remember when I suddenly thought, "Oh I'm interested in the investigation of homicide now, not why people commit homicide." I genuinely can't remember. But somehow I became interested, and obviously it started in the UK with British police. I remember that I observed some detective training and started to become interested in how officers become detectives and in the detective culture. I was always interested in difficult-to-solve cases; that was always an underlying theme.

DD   As for me, I grew up with a father that was a chief of police and a brother who was a physical problem solver. And so when I got into criminology, I always said, "most definitely, I'll never do research on police or juveniles." Juveniles because, good luck. And as for the police, I was in a situation where if I wanted to see my father, I had to go to the police department, because he was always at work. So, it was like, "I've had enough of that." But then I was doing a project with Volkan Topalli and Sarah Britto and Sue Collins, three of my colleagues at Georgia State, and we were looking at an open air drug

market; trying to hash out how people do social order in a seemingly disorderly place. The project had an active offender component that Volkan Topalli was the obvious best fit for. It also had a stakeholder piece to it. That was kind of Sarah Britto's area of interest, talking with folks from the churches and the schools and whatnot. And then there was a community piece. Sue Collins was good at making the residents feel at ease so she was the logical choice for that piece. And, so by default, I was left stuck with the police. I was like, "well, okay, why not? I understand the police mindset so it's not going to be difficult for me to relate to them and kind of put them at ease." So that's how I happened into policing research.

Access to the Closed World of Homicide Detectives

DD   So upon getting interested in policing research, we both found ourselves drawn to the homicide unit as a research setting for our ethnographic work. As we both know, that represents the inner sanctum or one of the most mysterious specially units within a large police department. Simply put, a homicide unit is a pretty tough nut to crack. I was hoping you could talk a little bit about the challenges you faced in gaining access to your research settings.

FB   I am not sure that I would have got access to homicide units in America without "inside" help. Thanks to Professor Ed Maguire who, at the time, was based at American University in Washington, D.C., my access began. I had first met Ed when I invited him to a symposium that I hosted in Cardiff in 2011 on homicide and major crime investigation. We stayed in touch, and he kindly invited me to be a visiting professor at AU. Ed introduced me to the IACP (International Association of Chiefs of Police), and I then presented some of my British findings and research plans at their conference in San Diego. I also then met Dianne Beer-Maxwell, Program Director at the IACP, and she keenly supported the research and was key in securing my access. She and Ed between them helped me to gain access to four quite distinct homicide units.

DD   For me, the access to the homicide unit was spawned by several earlier projects within the same police department. You might say I paid my dues and earned their trust through a series of other research projects and consulting efforts. First off, the above-mentioned drug market project allowed the command staff to gain a certain level of comfort with my approach and professionalism to put them at ease with me; I guess they didn't see me as a threat that was seeking to air out their dirty laundry. On top of that, I had spent several years trying to translate my research findings into positive change or just helping them make sense out of various organizational issues they were facing. When I pitched one of my best contacts on the idea of a homicide project, he was willing to listen and run it up the chain of command. I am

very grateful for that trust and commitment by the department in question; they didn't need to open their doors as they did to me. That's something researchers need to heed…that trust and ethics make a big difference in whether you get access or not.

Preparing for Fieldwork

DD   Shifting gears, I was hoping you could speak to whether you did anything to prepare going into the field for the first time?

FB   Yeah, let me talk about the US project, because I think it's more interesting. Firstly, the work that I had done already in the UK in terms of interviewing many detectives and shadowing some "live" investigations was an amazing grounding, because it meant that when I actually started talking to detectives for real in the USA, they knew that I understood the area, albeit there was lots to learn, of course, about how it was done in a different country. But they told me that they could tell from speaking to me and by the questions that I asked that I'd done my research. So, my preparation was making sure I was really up-to-date with the literature that was relevant to the sorts of things I was hoping to find out about. It was important to me to have a polished interview schedule, which of course altered throughout the trips. In fact, after almost every interview, I found myself thinking, "Ah, ok, I need to add that question now." But it was polished enough going in. So in terms of preparation, it was lot of reading literature and having a fairly good focus about what it was I want to explore.

DD   Yeah, I found that when doing ethnography or interviews with *offenders*, ignorance is one of your biggest assets, right? Kind of playing ignorant and saying things like, "Well tell me about that," in a nonjudgmental, just open manner. You know, using the "tell me your story" kind of a thing. But you can't do that with *police*, right? If they deem you as ignorant, it's going be a tougher road for you. So you have to demonstrate a certain cultural competence and understanding of their language and of their procedures if you expect them to let you in further. And, you know, I've always used the fact that I grew up around police. I'll mention that and then say, "well yeah, I kind of understand that." I find that that usually gets their guard down toward me a little bit. But still, as an outsider, you miss things. Like if you don't, if you don't understand their language. You know, they often include official signal calls and abbreviations unique to the department. If you don't know those, you have to stop and ask them to clarify. Invariably, you have to do some of that because they talk in a language that, even as you prepare, you can't follow all the time.

FB   It was probably tougher for you in a way, though, because I think one of the advantages for me when I was in the States was me being from the UK – that distance I think actually enabled them to let their guard down a bit more. Also

they were very interested in how things worked "over the pond," as they'd call it. For example, they were fascinated by the fact that UK homicide detectives are not armed. But also, I really didn't know some of the language, protocols, and rules you know, so I *was* naïve sometimes. For example, during some of the interviews with US detectives, we discussed the Miranda warning. I was able to get very detailed information about how they used trickery and deceit to encourage a suspect to waive their rights, and in part I really was ignorant to some of that but also I would play on that ignorance some of the time to really try and get as rich a description as I could. So I definitely felt that sometimes they opened up more because they didn't worry about me. It was just like, "well, she's going back over there [UK], and she's going home with that data. I don't think we need to worry about her." Whereas, if I'd been maybe from around the corner in a US university, they may have been a little more cautious.

Focusing the Research: Themes and Questions

DD   So, when you first started in with the homicide project, what, what was the research question or substantive focus that was driving your field work?

FB   Broadly, I was interested in the art, craft, and science of homicide investigation. I had an observation schedule and an interview schedule. My main research questions were: (1) how did they actually investigate homicide and go about go about solving them? (2) what were the challenges and why did they fail to solve some cases? and (3) how did they use science and technology and what kinds of impacts did it have? I'd already explored these issues from the UK perspective, but what was really interesting to me was whether it was different in the USA and how they were doing things over here.

DD   And did your focus kind of broaden or morph as you got deeper into the field?

FB   Yes, absolutely because new things come onto the radar, don't they? Things that you weren't thinking of like the Miranda warning but also other aspects of how suspects were treated. For example, it was somewhat vague whether you record the interview with the suspect and the detectives would sometimes not make it at all clear to the suspect that he was a suspect in a homicide. These were starkly different to the UK where all interviews with homicide suspects are video recorded and the suspect is clearly informed that he is being interviewed in relation to his suspected role in that homicide. So all those things became hugely interesting as the research evolved. Gender is another one. I hadn't really thought that I was going to be particularly exploring gender, but when I interviewed my first female detective, she raised some issues with me that made me think I should be asking questions about gender. So, that was really interesting. But, perhaps most of all, I became very interested in the role of witnesses and the link between poor police-community

relationship and unsolved cases. I had a hunch that some of the difficult-to-solve homicides were going to be those where the relationships between police and communities were most strained and that was borne out many times.

DD   Well with my homicide field work, it was interesting too. As often in my career, a project came to be out of serendipity. I always tell my students that, "I play with the criminals or play with the cops." In the case of the homicide fieldwork project, I was watching an episode of the reality TV show *The First 48* and was like "I want to go play with the homicide detectives. That's what I want to do!" And, as I said, I had a good working relationship at the time with the police department that I ended up using. It's the one that I've done most of my work with. So before I could approach them, I needed an angle. I thought, "OK, I can't just go hang out with the homicide cops." I need a research focus. So, Heith Copes, Rick Tewksbury, and I wrote an NIJ proposal to study stress in policing. The idea was to look at how homicide officers cope with stress. And that was enough to get me into the field. But, just like you, I started wandering quickly into other topics. And the plan called for us to use the Atlanta site as a pilot site to demonstrate the viability of the project to grant reviewers. So I started with a small interview guide and just started expanding it as I went along with the ride-alongs. While the proposal never got funded or expanded to the research sites that Rick and Heith had worked up, it eventually turned into a spinoff project, in the sense that it turned into a homicide case file review on the back end. That was intended to develop into a ceasefire intervention but that never happened either. Still, the file review produced a whole host of other quantitative data on clearance rates that produced a dissertation for my project coordinator. So one of the things that I put in my "lessons learned" category is don't constrain yourself. When you get in there, if an opportunity arises or you see an interesting avenue of inquiry, go for it.

FB   Definitely! Because that's the nature of ethnographic research, isn't it? You do need a focus to be given access. If you just said, "I don't really know what I'm doing. I'm just coming to look at you all and follow you around," you probably wouldn't get access anyway. So, there does need to be some initial restraints, but, as you say, I think you've got to be flexible and go off on a tangent where necessary.

Fieldwork Planning and Introductions to the Field: Schedules, Protocols, and Layers of Access

DD   Talk to me a little bit about your fieldwork planning. Like, what went into the preparation of your protocols and getting ready to go into the field?

FB   The main things were the interview and observational schedule, which I spent quite a lot of time developing. And also reading the literature. What I

didn't have, and I look back and see it as a mistake, is I didn't have a data collection protocol where I neatly or carefully recorded everything that I had discovered from documents. And, looking back, I think that was probably a mistake. I was so used to just doing tape-recorded interviews that I think I probably missed an opportunity there to get more information beyond the interviews. I did gather documents if they had them available, but I can't say that, routinely, for all of the homicides that I shadowed and learned about, I gathered everything I could have and had a checklist.

DD   And you did a shadowing approach as well, right?

FB   Yes. I spent time at four different US police departments and interviewed homicide detectives at each of these, but it was mainly at one of these where I did the most sustained piece of research. I went back there four times in total over the course of 2 years. For one period, I spent about 6 weeks there. So, I would just turn up every morning, and soon I had my own desk in one of the squad rooms, because every squad was an officer down due to cutbacks and the space was open. There were six homicide squads in this department. One of the squads had their own office, and the rest were all in a large office that they shared, and it just so happened that I was introduced to the squad in their own office in a way that they really bought into the research. That was really interesting actually, because some of the detectives didn't quite understand why I was there and what I was doing. You know, it's the whole layers of access thing. The chief says, "she's coming." And suddenly you're there. You know, the deputy chief knows, the lieutenant knows, but they don't always really explain to everybody else who you are and what you're doing there. But on this particular day, I was introduced to the squad in the room on their own by the lieutenant, and the sergeant was this really well-respected investigator, and he'd been an officer for 20, 30 years—and you could tell that his squad really respected him. He had a great sense of humor and an infectious laugh (the squad laughed a lot actually), and he just took to me and to the research, and then the whole squad bought into it as well. That was a kind of pivotal moment in the research, really. Because from that moment on, he made sure that I got to see whatever I wanted when I was with his squad and I got introduced to other people, and he just kind of helped me to roll the whole thing out.

DD   I can't agree more about how important it is to nurture relationships during those first couple of interviews; those first couple of days are key, as you need that ice to break. For me, my first icebreaker was the unit commander. So we went through the normal human subjects protocols with the university, right? Participation was to be explained as being voluntary. And I get initial access to detectives through the commander to explain the project to the unit but then do a follow-up contact with individuals to gauge interest from them individually. Anyone who remained willing at that point would get scheduled for a ride-along interview. Well, it didn't play out like that so much. I literally walked into a unit staff meeting with the commander and he's like, "OK you're all riding with him." And here I am like, "oh that's a violation of the

protocol, but can't undo it, so OK." And they all nodded in agreement after I explained the protocol to the group. Hell, the commander even went so far to say, "you're not going to schedule with them, because you'll never get—they'll give to the runaround. I'll just schedule you with them," which was tremendously helpful and streamlined the whole sampling effort because I had three different watches to get with; so I was doing day, evening, and morning watch. It would have been a disaster to coordinate scheduling across all those shifts and detectives. It was a big unit of about 20, men and women, and I rode with all of them for at least a day, sometimes two. So once I got with them, my protocol included an interview guide that included broad topics. And I really went out of my way to blend in while in the field and to not be in their way. So, I dressed like them. I went to dinner with them when they did. And more importantly, I said, "we'll talk when you have time. I'm here with you for eight hours and we're probably going to record an hour or two hours of interview time. But during car rides or at your desk, you go ahead and take care of what you do. I'll just pick my spots to ask questions and get to what I need." That worked nicely, because you get to see them do their work, and also and interact with one another and get a lot of really rich field note observations came from just watching as opposed to talking. The talking became almost secondary to me.

FB   Absolutely.

Becoming Embedded

DD   I'm always curious about organizational culture – That's the thread that holds all my research together. So, one of the things that immediately struck me was the distinct culture that is present in a homicide unit. It extends all the way down to the environment, like the significance of the homicide board and how it works to motivate the detectives; you know, they kind of push each other by virtue of the case record reflected on that board. That is really neat to me. So you glean a lot about the culture by just watching them work and not trying to talk all the time.

FB   Yes, that's a really good point about just interviewing them when you can. And you've got to get used to interviews being put on hold because people are busy and they've said to come in at a certain time but then they've gone off to do a warrant, or they're doing something. I've got a PhD student now who's doing research with the police on a different topic, but he gets concerned when they don't reply to his emails or remember to tell him when he can shadow them. I have to remind him, "they're really busy people. And even when you're physically there with them, you know, they've got all these things going on in their minds that they're thinking about. It's about patience." I was amazed how they did remember to call me out to a crime scene at three in the morning. And they'd even come and get me, because I wasn't staying close to

that police department. So somebody would have to come and fetch me and bring me to the scene if it was late at night or in the early hours. And so that was really, really good of them. You know, they spent a lot of time, looking after me. And then I'd get there and the sergeant would say, "Hey! You've arrived just in time! Take some photos!" Take one of the body if you like, it's just about to be taken away. That was completely different than in the UK where I would have felt hesitant to even ask to take a photograph. This sergeant and others too invited me to things like postmortems, to go to court with them, to sit in on meetings with the DA, and so forth. Once I'd been accepted it felt like it was all-encompassing, and they didn't hold anything back.

DD   Yeah, and the camaraderie of the unit really works to your favor, right? Because again, I had the same thing. I didn't think they held anything back. In fact, they did and said things that I thought, "did you just do that in front of me?" You know, I can remember—this is a little bit off color—but, we went into a homicide scene, a domestic homicide in an apartment. And the first thing the investigator did was go over and pull open the drawer next to the bed to see what kind of sex toys or what were in there. And they didn't even hesitate. And I'm like, "oh, that's kind of a violation of their privacy." But it was like, "uh, any time you got this kind of scene, this is the first thing you do for kicks." And then they're reading stuff on the table. There was the guy who had a collection of pornographic VCR tapes, and they're sitting there having a field day reading off the different raunchy titles of the movies. So I think that those things tighten the workgroup aspect. I think it's important, as a researcher, to understand the group that you're with and play on those particulars to get them to open up and let you in. As I said, my focus was on stress and homicide so that was an important piece for me. And I agree with you completely that the commander knows exactly what's going on. Maybe the second in command knows, but the detectives, they have no idea what they've signed on for or what this research is. They don't listen to your protocol description. Like I remember the first interview I did. It was perfect. It was just by accident that I ended up with this guy, but all the other detectives were sitting around and he's like, "Yeah man, it was the Rorschach test that really shook me up doc," and you know, he made light of my work. And from that point on – he broke the ice for me – everyone was laughing, and they realized, "oh, this guy's not here to, you know, pick my brain. He's not a psychiatrist. He's not gonna judge me." But those early interviews are big. I think the second guy that I interviewed, he was like the larger than life detective in the unit. He was really boisterous and loud and crazy. And he'd constantly joke around and be like, "Yeah, doc. I'm dreaming in black and white again. Is that a problem?" Or, "I woke up with a gun in one hand and a bottle of whiskey in the other." Everyone would just laugh me off; those early forays really, really helped. And another thing I wanted to note was based upon something else you said. I had a bit of a different arrangement than you. They would come get you. I did not have that. They would call me from the homicide scene. And that was a little bit difficult at first, because trying to get

the lieutenant, who was a very serious guy, to remember to take a moment to call me; it was difficult. So how it started to work was I quit relying on him. I had one detective that I was kind of closer with on each watch. And I would say, "hey, can you give me a ring just in case the lieutenant doesn't?" So, I drove around for a year with a change of clothes in the back of my car. You know, a suit coat and tie. And anytime a homicide would happen, it was like Superman. You know, I go switch into my outfit and drive out to the scene wherever it was.

Attending Homicide Scenes: Being Prepared

FB   I suppose it was because I didn't have a car. I wasn't from the States, and because I wasn't staying close to where they worked, the police department just wasn't that accessible for me. That reminds me of some other things about being prepared. On a few of my visits, they had these dry spells, and there weren't any homicides. And they would say, "hey, Fiona's come back. She's our lucky charm." But, obviously from my point of view, I was only able to come for large blocks of time, because I had to get back to my work. And, so it sounds awful, but I was obviously waiting for a homicide to happen in much the same way that the squad that was "up next," as they called it, was waiting for their homicide to happen. They're kind of on pins and they're not supposed to drink too much, and you know, I felt much the same. And then one weekend I was invited out for some drinks, and I thought, "oh, well nothing's going to happen tonight" and went out, and then the call came, at about 10 in the evening. By the time I actually got to the office, I had a hangover that was beginning to kick in. I had just grabbed some water. I had no food with me and I remember thinking, "right, this can never happen again." You have to have snacks in your bag, you know, crisps, peanuts, bananas, just something. You have to have stuff with you, because you don't when you're going to get to eat.

DD   And you don't know when you're going to go home, either, right?

FB   Exactly.

DD   You learn very quickly that they're not taking you off the scene, right? If it's a complicated scene, and it's going to take eight hours to process it, then you better wrap your head around the idea that you're staying those full eight hours.

FB   Yes. Four o'clock in the morning, on that night, we were in the middle of a wood with a bloodhound, tracking the suspect. I had dressed in my normal attire, which was a smart pair of shoes with a heel, which was nowhere near suitable for that terrain. I can remember teetering over branches and bracken and leaves, in the dark. But it was an amazing, exciting adventure that I'll never forget.

DD   Well I was decked out like Inspector Gadget. I had this expanding folder that I always carried. It always had some gum and some sort of snack of some, like a Power Bar or something like that in there. But, also, with the weather,

I always had a trench coat in my car, and I had earmuffs. I had gloves. I had an umbrella. You know, I was like prepared for any type of weather. I was set for however long it's going to last, right? I had a scarf. Because, you know, you get out there at night and you're on the scene for four hours. It gets cold real quick. Because, you know, as they say, "we don't get to pick where people die." And you know, I found that the environment can be one of the challenges in the sense that you get called to say an apartment where the body's been there for 5 days. And there's that beautiful smell of body decomp. It's just all around, and you're going to be there for four hours. Just get used to it. You can't be a disruption, right? It was first and foremost for me to say, "I am not going to interfere with anything you do." As a researcher, I don't think you can anticipate all the twists and turns that are coming, but you have to be a trouper.

FB   And it also makes you much more respectful of how tough their job is, because you endure some of those pains, albeit you're not doing the job all of the time. But you discover how hard it is and see some of the conditions under which they're making decisions. You're a bit more appreciative of the complexities of it all. So, I think it's good. I think it's important to have a little bit of hardship and forget to bring your food. I wasn't as organized as you.

DD   And I always took care of the detectives. I always bought the lunch, or the meal, for whoever I was with. Not for the whole group, but whoever I was shadowing that day. I was like, "you're not paying," you know? It wasn't much, but to me, it's those little tokens of respect that pay off. And if they were having a bad day or had worked a long stretch the day before, I would just say, "let's reschedule. I mean, I'm not going to be in your way today." I don't want them deal with that. And I noticed that I did quite a bit of navigating between the detectives and the superiors, right? So those relationships, you know—and not everyone gets along in a family—were challenging. I had to kind of deliberately stay out of the way of that and not find myself in the middle. You know, you'd have a sergeant talking smack about the detectives and I'd think, "I didn't hear anything," or vice versa.

FB   And this is a related point, really. I remember interviewing a lieutenant, and as we came out of his office into the large office space of some of the senior officers, he started cracking jokes saying, "hey that was an interrogation." And then the other detectives start asking me "so what did he tell you?" And, luckily, I instinctively knew not to say anything. But I could easily have slipped up, you know, maybe said something harmless that to me was harmless about what he'd said, that could have been a really big mistake. And so that's a piece of advice that I was going to say at the end; never ever tell anybody what anybody else has said to you in an interview or informally over coffee or in the corridor. They have got to trust you. You can write about it later, that's different, because then it's anonymized, and you have to be true to the date but, you know, in the field, you have to be careful.

DD   And you can step in it real fast. Right? You don't even see it coming because you don't know the backstory of what's going on. You're only in the unit for a few hours at a time, while they spend days, weeks, even years working

together and developing complex relationships. You only scratch the surface on that history and larger context. And I think that's an important point. I think you have to be very careful to know and respect your limited role within the group. But you have to act natural at the same time, right? If they think you're on guard, then the dynamic changes.

Recording Data in the Field: Schedules, Diaries, and Digital Recorders

DD   Let's talk a little more about your protocol. Did you use an interview guide?

FB   Yes I had an interview schedule and, as mentioned earlier, and an observational schedule too. The interview schedule was organized by themes, and then a number of questions and prompts under each category. I would always tape record the interviews. In fact, that was a really interesting issue, because Ed Maguire, who as I said was really instrumental in getting me into one of the first police departments, he had said to me, "don't even think about asking them to go on the record; there's no way that they'll let you record them." And so I didn't for the first few interviews. But asked the detectives afterward, "what would you have thought about me recording that?" And they said, "oh, that'd be fine." And so, from then on, I always asked if I could record the interviews. There was only one officer who declined. He was the major at the one homicide unit. He seemed a little cautious of me and the research, but he still spoke to me candidly, and I just took detailed notes. Everybody else was audio recorded. I did most of the transcribing when I got back home to Wales, because it would have been too time-consuming while I was there. However, I regularly listened back to the recordings and took notes, and I was often thinking about whether there is something I needed to add to the schedule the next day. Was there some new theme I needed to explore? Did I need to do some tweaks on some of the questions I already had?" So, that was a kind of iterative process for me. I kept a research diary as well with all kinds of thoughts and reflections recorded each day.

DD   Did you keep the diary by pen and pad?

FB   I wrote it in my notebook but then I typed that up on my laptop every day.

DD   I used a similar protocol. And I think that most ethnographers would agree that less is more. But you've got to have some protocol. I like to think of them as topics; I try and keep it to less than a dozen topics that I'm going to talk about. I don't care what order they play out in. I mean, I think you let them dictate the conversation and don't force it because you'll get a lot more useful insights if you let them talk. And you'll figure out, you know, when and how to work in the topics that they don't bring up. But, I didn't use pen and paper to take notes; I always took notes on the audio recorder on the way home.

FB   Brilliant. As you were driving home?

DD   Yeah, driving home, I just turned on the recorder and in a stream of consciousness, I would make verbal observations. Some of it was me really not capturing idea for dexterity as much as me thinking out loud. Like, "OK, tomorrow I want to explore a new line of inquiry or I want to think about X." But a lot of what was in those audio field notes was really rich and important—me making connections and referring back to other interviews, you know, like, "in the interview with such and such, there was a similar thing said there." So, I when I was doing my analysis, I was able to use those audio notes. I didn't transcribe my own audio materials. I sent them off to have someone transcribe them, and I didn't do that until the end. So I had to kind of come up with a game plan as to how I was going to do the grounded theory work along the way. And I thought taking written notes would be intrusive. But I figured, "I've got a good enough memory that if I tried to do it later, on the ride home, would be perfect." So I usually had about a twenty- or thirty-minute ride to get home. So it was good. I would usually talk for at least fifteen minutes … just into the recorder; just let it run while driving.

FB   I think that's a great way to do it. I mean, I just couldn't have done that, because I was going back and forth on the Metro. It was quite a journey. It was noisy and quite often crazy things happening on the Metro. One man wanted to borrow my phone, somebody tried to rob me. So, on parts of the journey I felt like I had to have my wits about me. But I do think you're right. I think there are some interesting issues to be considered about taking notes. One of the things I was fascinated about when I first went to one of the scenes was how stories of the homicide are relayed. So you've got the patrol officer there. He or she has already taken a whole load of notes, and then they verbalize all of that information to the homicide detective who writes it all down again and then tells the other detectives on the squad and later the lieutenant and so on. And I remember being fascinated by this kind of movement of stories and words. It's what they do back home in Britain as well. But something felt really odd about that. It wasn't often clear what were "facts," what was really known, and whether inaccuracies were creeping into these accounts along the way. So the journey of the story that was created about each homicide was really fascinating. But that's how they did it, and so in the end, I had the same notebook as them. They gave me the same kind of notebook that they would write in. That helped me to blend in with them but, most importantly, with the witnesses too. They just always assumed I was a cop. Of course, there are ethical issues about that, about whether witnesses, family members should have been told, "well this is a researcher, and she's from the UK." But, of course, I dressed the same as them in smart formal clothes and I was always just introduced to witnesses as, "this is my colleague." We never discussed how the detectives might introduce me to the people that they spoke to during their investigations. It just happened that they either said nothing (and I guess it was assumed I was a detective) or they introduced me as one of them. And that's how it happened. That's just how it always seemed to happen.

DD   I had exactly the same experience. I didn't really have a plan, but they just took over, and it worked. You know, I think that we often think about ride-alongs with police as typically being with a patrol officer. Shadowing detectives is a different exercise. So, I thought they did a good job of just kind of doing what worked. And sometimes I had some challenges, like the time I was doing a single ride along with partners. You know, sometimes they work as partners. This department didn't have a team format, but some of the officers just always worked together on their own. So I had to kind of revise the protocol and say, "OK well, this is going to be a joint interview, here. I'm just going to take what I've got, and feel free to chime in if I'm talking to him but you have something to add." That I think worked out well, because it produced a lot more details. You know, then I would follow up the next day with the other person. And I looked at it as being doubly good, I get more information.

Ethnography and Emotion

DD   Did you find, as a researcher, there was an emotional side of the fieldwork? Can you talk about that?
FB   Yes. The emotional side…there were definitely some moments. I mean, for a start, I was always a little bit homesick. You know, on the one hand I really felt privileged to be able to come and do the research, to have been given the access. It was great, and it was exciting. But it was also somewhat traumatic living on my own in a fairly violent city, having to navigate the Metro. And then of course, all day long you're just hearing and seeing awful stories of misery and destruction of human life. And detectives love to tell you stories. So, aside from the fact that you're there actively gathering information about past and active homicide investigations, they often tell you stories about homicides that stick in their minds from the past. I vividly remember one detective—they would quite often give me lifts home if I didn't catch the Metro. So, at times, somebody would say, "hey, if you're going back that way, give Fiona a lift,"—and one evening on the journey back, this detective told me a horrible story of a police officer who had murdered his girlfriend in her car. She had her baby in the back of the car, strapped in a car seat. He recounted that it was a really, really hot day, and the baby was left to die in the heat of the car. He described in graphic detail what had happened to the baby in those scorching conditions; it was really disturbing. And he talked about how they could still see the tears almost melted into the baby's face. I remember having nightmares for a couple of nights after that, because he described it so vividly, and it became embedded in my brain. It's so weird, because at other times you see awful things, and it doesn't affect you. So, yeah, it's not a cheery topic, obviously.
DD   For me, I remember thinking, "it's going to be interesting to see how I react to this first dead body." You don't know when it's happening just like them,

right? Well, it was an evening watch shift, but it was at the very end of the shift. So I ended up staying four or five hours longer than I had anticipated. And I went home and I was going right to bed; it was, 3 o'clock in the morning or something like that. And I thought, "am I gonna have nightmares?" And, for me, it was kind of a relief that it didn't affect me. I always tell folks that I don't mean that to say I'm a sociopath that has no feelings, but, for me, what makes it easy for me is that you're focused on the work, right? And, so you're focused on understanding and watching what's going on. And that body just kind of melts away as part of the scene. And you're left trying to understand why they're doing what they're doing, or what they're doing, and how it fits. So, I found it to be a lot less emotionally taxing than I anticipated it would be. But it was more physically challenging for me, the sleep schedule. Because I did the project while I was on sabbatical, which, if you can work that out, it's tremendous, right? Because you don't have to be anywhere. But even still, I had a life, and I had a wife and a kid. And trying to ride a morning watch shift and come home at 7 o'clock, 8 o'clock in the morning and have to still be functional for the day…I'd try and grab a couple hours of sleep and, and get back into the reality of life. But, I think police ethnographic work, because of the fact that you have to understand that what they're doing, it comes to take precedence, right? There's a dead body here, and nothing possibly could be more important to them. They're not going to stop what they're doing for you. You have to embrace that and be prepared for it. And, you know, there were a lot of late night texts to the wife…"not going to be home" I mean, the detectives were doing the same thing. But you, you get an appreciation for, as you said, the complexity and the rigor of what they have to go through physically and emotionally. And, and the idea of "being up"….waiting to catch a case. I don't think you can describe how hard that is. In my unit, there were 10-, 12-year veterans who had lived through that hundreds and hundreds of times over the course of their careers.

FB   Oh, they're tense waiting. There would be detectives wandering around saying, "this is, it, I've had enough now. This has been going on for too long." But there was a lot of humor too and lots of eating during the waiting. The popcorn machine would get filled up again and again as they talked about just how long this dry spell had gone on for. You could tell there was relief when one actually arrived.

Leaving the Field and Writing-Up

DD   Talk a little bit about getting out of the field once you finished the ethnography. And perhaps you can also speak to the proverbial next steps.

FB   It's weird, because I never ever said that I wasn't going back. It was always sort of, in fact, when they were saying, goodbye, they were saying, "oh, you'll be back." And maybe I'll will go back one day. I don't know. It was never sort of, "right, that's definitely the end. You're gone now." Because, of

course I went back so many times. So, I think they just probably think I'm going to go back one day. I stay in touch now and again with emails. But I'm not hugely in touch with people there, really. It's only now and again if somebody drops me an email. They're probably wondering where the hell all my publications are. Because, I mean you need a clear publication plan after these things, and I did not have that. And then other things just swamped me as soon as I got back to work, because nobody was waiting for these papers, or this chapter, or this book. It never had to be a priority, whereas other things had deadlines. So they became the priority, and I think that's something I would say to all ethnographers is, "however much time you think you've got, make sure you build in more time than you think you need to analyze and write it up."

DD   Well, I think it takes so much time to collect the data. As I tell my students, "you better have a plan, because, otherwise, that dream of tenure is going out the window. If you do a year-long ethnography and you get one publication out of it, you're doomed." A lot of the paper ideas kind of come up along the way. But I think, going in, you need a publication plan. I knew, "OK, these are the three articles for sure that I'm looking at." And the third one never really happened. It turned out that there wasn't that much there, which was OK, because something else kind of comes in. But I think you're right about planning going into the analysis, right? When you start your coding, you need to be as efficient as possible, right? But sometimes you just have an idea comes up later and you just have to be willing to go back in and start recoding. But I think that approaching the coding process with a plan is important. But I always over-code, right?

FB   Me too.

DD   I always think, "it's going to take a long time, but I'm going to have everything in here that I possibly need." You know, I think that coding varies from person to person. I used NVivo as my software. And I'm a very much a theoretically-driven coder. I'm not going to do the decision trees stuff or take advantage of the technology stuff; I'm an old school coder. You know, I code, then over-code, and then double-code. But the good news about that is, on the back end, I have plenty to work with. Like, one of the papers was done with a grad student as a lead, but because the coding was so thorough, she could just go in there and was able to get it done. I want to stress to readers that having a plan and taking seriously that coding will pay huge dividends down the road.

FB   Definitely. I mean I've now, for the first time, got a researcher who is working on a paper with me using the data from America. There's so much data. I've coded it all but not written from it in a systematic way. We are writing a paper around confessions and interrogations and Miranda—I have had the title and abstract written for ages—it's going to be called "Dancing around Miranda." I've always wanted to write that paper but never got around to it. I've recently submitted a paper that I worked on here and there since returning from America. It was called "The Difficult to Solve Homicide" paper, but it

became known as "The Difficult to Write" paper, because it just became so huge. It's based on interviews and field notes with detectives in the UK to Britain. I didn't really know what the main messages were. If I was doing it all over again, I would be much more organized and have papers in mind going in. It was all much more haphazard and exploratory, and I didn't even realize I was going there until quite soon before it happened, in a way. So, yes, I wouldn't recommend any of that.

DD And I think the writing process is something worth mentioning, too. You hit the nail on the head in that regard. I mean, writing papers always takes so much longer than you anticipated it would. You get so close to it and I can't let it go.

Some Final Reflections on Lessons Learnt

DD And I make it much more complicated than it needs to be. Any final thoughts for the readers? What sort of lessons did you learn that wouldn't be obvious to someone thinking about doing field work with police?

FB Well, there are two things here. Firstly, what would I do differently? And I would definitely say that being super organized is really important. In terms of things that worked well, I think it was an advantage that I went back and forth on many different occasions, because you see the continuity and the change by having these separate stints over the course of a couple of years. So, I think that was useful, because I couldn't obviously go for a whole year, because I didn't get a sabbatical. Another recommendation I'd make is to take photos. I think it's great, if you can, to have some visual triggers, even if you're not going to use them in the research – because there might be all sorts of complexities about that. If they will let you take photographs…nothing gory necessarily, but just something that will take you back to that day or night, at three o'clock in the morning, then do it. Because your field notes are brilliant, and they do take you back but, a photo is really good as well.

DD I hadn't thought about that. That's a great idea. I didn't do anything like that. But thinking about it, I can see how there would be some great advantage to that. And, to your point, they don't care.

FB No, they really don't. They were encouraging me to take photographs. And, of course, we've usually all got our mobile phones with us that have an inbuilt camera, and so there's no need to carry around a camera. It's so easy for us all to do that. So I definitely recommend that. The other things that I think are really important is staying neutral at all times. I did hear and see things that I found to be somewhat inappropriate or even distasteful, you know? Sometimes police say things that don't sound great about the people that they're about to interview, or even the family members. You just can't say anything about that. But, equally, I think it's really important that you're honest when you write these things up. I know there are complexities about not wanting to muddy the water with the police department because you can't go back there again. But you can write up carefully and protect the identities of those you've stud-

ied. But you do have to tell the story—good, bad, and ugly—about what you've learnt. It's not always going to be lovely nice messages in there, so I think we have to think ahead to that, you know, before we do this kind of research, about how we're going to tackle these ethical issues.

DD   And, to follow on that, I went into the field expecting that most of the emotional work was going to be dealing with the crime, right? But it was equally taxing seeing the police at their best and their worst, right? You know, it's difficult to see someone kind of half ass an investigation, and you can sense that they're just not going to take this case seriously no matter what happens. I mean, the sergeant's in there telling them that more work needs to be done… doing a case review and pushing them to do more. And it doesn't matter. This case has no appeal to the lead detective. And you think, "hell, I could solve this thing! Give me two days! I'll put this thing together," right? But there again, you can't do that. You can just report on it.

FB   Yes, you can write about it afterward.

DD   But you have to respect that you can't interfere with their work. I also I think you can expect to find yourself in some tight spots that you just never would have anticipated, and sometimes, you just have to roll with it. Like, I remember I was once doing an interview with a detective that was dispatched over to a cold case unit within the prosecutor's office. And I was in the prosecutor's office waiting for the detective to come around when she received a call from an informant in a prison in Florida. And I knew exactly what she was doing when she was doing it, but I thought, "oh you didn't just do that." She put the call on speakerphone because she needed a witness to its contents. So she made me the witness without asking me. And I was like, "you didn't just do that." But she did. Afterward, she apologized. She's like, "I'm sorry to put you in that spot, but this is an important case, and I needed you. I didn't have any other option." And I thought, "well, OK, let's hope I don't get subpoenaed on that one, because there's going to be a whole host of ethical decisions that are going to play out there." But you just can't anticipate those kinds of things. I guess I could have stood up and walked out of the room, but the consequences of that would have been dire.

FB   Yes, and that's that balance, isn't it? I can remember with one case, the victim had been shot about a year earlier. And he was in hospital, and he died. So we go to the hospital, and we go and see the body. And the relatives are already there. The mum, aunt, and girlfriend. They're really upset, and they want to be allowed to go into the room to say goodbye to their loved one. And the two detectives that I'm with—who were actually really fun guys…they were known as the pranksters in the department—they wouldn't let them go in. They kept saying, "no, no, you can't go in, we have to preserve evidence, we can't have any destruction of evidence." So I said to them afterward, "but what do you mean?" I said, "there is no evidence. He's been in homes and hospitals for a year." And they just said, "Ah yeah. We don't want them in there messing around." To me there was no reason for them not to let them in. Then, we drive back to the police station, and they shut them in these awful

interview rooms that resemble cells. They asked me to sit in the room with the victim's mum, while they interviewed the victim's girlfriend in another room. She assumed I was a detective and started asking me things that I couldn't really answer. I felt as if I'd been complicit in all of this, a situation where they could have just been kind, and just said, "go on, go and say your goodbyes." It really distressed the family not to be able to say goodbye, and there seemed to be no rationale for it. I decided not to challenge the officers, but I did ask some questions, in an inquisitive way. It's about how you ask those questions without sounding judgmental. That brings me to my other recommendation for ethnographers, and that is don't ever be afraid to ask. I've sometimes held back from asking, "can I go with you and do that?" And I've always regretted it. And when you do ask, sometimes you get the most amazing opportunities. I was interviewing one young detective who was fairly new to homicide one day. He had one homicide that was open, and he was desperate to solve it. It was his first homicide, and it was irritating for him having this unsolved homicide on his record. He had to cut the interview with me a little bit short, because, as he explained to me "oh, I'm going over to meet with an informant. He's an informant that works with the tobacco and drugs people?"

DD   Alcohol Tobacco and Firearms, ATF?

FB   That's it. And he said, "but they think he might have some information about my unsolved homicide." So I was thinking, "I'd love to go. I'd love to go! Shall I ask?" So I just said, "it would be really great for me to understand all about that. Do you think there's any chance I might be able to come with you?" And he said, "I'll ask the boss." I sat back in the squad room, and I was typing away, and I could see them talking, and I was thinking, "it's going to be a no, and I'm going to feel a little bit embarrassed, but never mind." And then he just walked toward me and said, "come on then girl!" And that was the most amazing experience. We drove to this undisclosed building, headed up to an anonymous floor where every door, including toilet doors, were locked, and we met the informant. It was a really brilliant aspect of the research. So, never be afraid to ask, because what's the worst that can happen? They say no, and you might feel a little bit foolish.

DD   Exactly. And I think the other big piece of advice I would give is, "you're going to want to be judgmental at times, but you just can't be."

FB   No. Absolutely.

DD   After you get out of the moment, it's a lot easier, right? Because you reflect, and you can put yourself in their shoes and kind of appreciate them. You're able to appreciate where they're coming from. But, you know, I've probably done well over a hundred ride-alongs now and I've only once encountered an intransigent officer who wouldn't talk, wasn't going to play along. He was just going to do the minimum. He answered the questions, one word answer, and looked at his watch as frequently as he could, like, "are we done with this yet?" And at some point, I just quit asking questions, because I'm like, "we don't need to go through this anymore. We'll drive around for a few hours,

and I'll watch you police." But, you know, I think that was just his paranoia about me as an outsider, it wasn't me judging him, but him judging me. And I think you need to be prepared to see some things that you're not ready to see, and be consciously aware of the context. When things happen, you can't react.

FB Yes, and you hear some things you don't want to hear. I had detectives say, with no harm in it really, but they'd say weird things like, "so did you say you had a husband?" And I'd say, "I've got a boyfriend but he's like my husband – we've been together forever. Oh, and he doesn't mind you being over here on your own? Doesn't mind that we might take you out for drinks?" And there'd be some of that little undercurrent. But I always just dealt with that with humor. There was no real harm in it. I think some researchers might have been offended. I figured they were just showing off in front of each other some of the time. It was an interesting dynamic. I never let any of that bother me either.

DD And if you do let it bother you, well then, you've got to get out, right? I mean, I always say to students, "look, you're going to be put in some uncomfortable situations that are, that are going to be offensive, but, but play to your strengths." Like, there's some benefit as a female ethnographer; not getting exploited, but rather, recognizing that it's easier for them to talk to a woman than it is to a guy, right? And, with me, I use the fact that my father was a police officer to just kind of look for the opportunities to get the conversation on a more even keel. Make it where they're, they're comfortable, and then they're more willing to talk. And, you know, is that manipulation? I guess so, sure. But I think, in the end, what conversation doesn't have manipulation in it?

FB Exactly.

DD And, you know, I never misrepresented myself. I never did any of that. But I think that it's wise to be taking stock of what your assets are and crafting them as much as you can. You know, drawing on your past experiences and your personality.

FB Yes.

DD I would also use humor a lot to try and break the ice.

FB A big benefit with me was being from the UK, because they had so many questions. They're inquisitive people like us. So they would often say to me, "no really? Really, they don't carry guns?" And, you know, we would have lots of conversations about the fact that the UK officers are generally unarmed. They'd ask, "so what happens in this scenario, in that scenario?" And that was nice, because you felt that you were giving something back to them. . So, I think that helped me; that I had that geographical national distance from them. I think it helped me to gain access into a police department that had never been researched before. In fact, I was told, "you won't get in there. It's corrupt, they've had too many things happen." But I just think they didn't fear the research.

DD Like you couldn't do harm from across the pond?

FB Yes, definitely! I think that was, that was part of it. I've thought of one other tip that I think might be helpful for people for the future – mixing the way we interview up a little bit. So, you know, my standard was a one-on-one inter-

view. I did that when convenient to the detectives. And there's all the other things that we've talked about: the casual conversations, observations, and all of that. But, sometimes, I had the opportunity to interview two people at once; it was just more convenient for them. And I found that sometimes that really worked. Or, at other times, I'd be in the squad room, and they'd start talking about something that was really relevant to the research. And I'd say, "can I turn the recorder on, because this is a really fascinating conversation?" And, as they became more comfortable with me, they'd say, "yeah! Yeah, turn it on." Most of that was really rich data, because they were having casual conversations, and I was just asking a question now and again. Also recording conversations on the way to and from crime scenes worked really well for me. You get a sense of what they are thinking and feeling in the moment but also you can gather information about the area in which the homicide happened as you drive along through it.

DD   Yeah, to follow on that, I tape recorded all of my interviews, but I didn't run the recorder the whole eight hours. I always found it useful to let them know when I was recording, like even just kind of holding it out so they could see the recorder in some way. You know, I didn't want them to feel as though I was trying to be sneaky in any way shape or form? And I would always tell them, "if you want to say something off the record, just say so and I'll turn it off." Or I'd say, "if you said something and you want me to erase it, just let me know and I'll back it up and erase it right now." That stuff is important because police, by nature, are a paranoid group. So whatever you can do to kind of put them at ease about that is good; you'll usually find that will go a long way. And to the point that you made earlier: after a while it just becomes so seamless to give them the license to talk and act in a way that feels most comfortable for them. Once you get there, you know, the sky's the limit. Because then, like you said, you're at dinner with the crew and all of a sudden they're talking about how they're going to an informant interview and you get invited along. Getting to that comfort zone is how you get to see things that you wouldn't see if you're too rigid. And I think it's okay to kind of play into their stereotypes. You have to kind of go with the flow, the more they feel like you're one of them the better it is. And you're right, I mean, I found myself in all sorts of places where I knew they were using me. Like the time the investigator said to the perp, "the lieutenant here," as he pointed at me. Basically, he convinced the perp that I was his supervisor. And I'm thinking, "you didn't just do that," right? But I looked the part, and the guy was like, "OK, yeah" and did what was asked of him. I mean, you've got to know that they'll take advantage of any angle they can; they're trying to figure out a case, and they're going to get there the best they can. They use whatever resources they have at their disposal without blatantly breaking the rules.

FB   One of the funny stories I have, was this one. I was interviewing one of the detectives, and it just so happened that the only room that was spare in the department that day was a room where they would normally interview suspects or witnesses. So he and I go in and the interview lasted probably about

an hour or an hour and a half, maybe—and the clown of the unit, the crazy, funny detective who was always up to something (pinning pictures of other detectives on the wall with word balloons on them saying funny things) who I knew well by now, he decided to turn on the recording equipment he video recorded the interview. We come out of the room, and he says to his colleague who I've just interviewed, "we've just recorded all of that…and I'm going to be making a special video of what you said in there"—everyone is laughing. He gave me a copy—which was useful for me to learn about my interview technique—and said that there were no others but I am sure he used some snippets for fun down the line. I did say to him, "you breached confidentiality. That interview was supposed to be confidential." I said it in a jokey way. But I thought to myself, "I will never use that room again, because I can't trust him not to record it again." So there was always that funny stuff going on as well, but it was all in good humor.

DD   And that's a good point. I don't know about you, but I've really enjoyed my time with the unit, and that's OK, right? You might as well. You're going to be there. Soak it up. But I do think it's important to keep those boundaries there; you know, you're friendly with them, but you're not friends.

FB   And you'd go for a drink, but you never get drunk.

DD   Exactly.

FB   That would be on my list of recommendations.

DD   Yeah, it's OK to have a drink unless you can only have one drink and get drunk. If you're a lightweight, don't even have one.

FB   Yes, definitely. Because they'll try and get all sorts of information out of you at that stage.

DD   Exactly. Well that's all I have. Thank you for your time, and let's hope that the readers can glean some valuable insights from this conversation.

Further Reading[1]

Brodeur, J. P. (2010). *The policing web*. Oxford: Oxford University Press.

Brookman, F. (2015). Researching homicide offenders, offenses and detectives using qualitative methods. In H. Copes & M. Miller (Eds.), *The handbook of qualitative criminology*. New York: Routledge.

Brookman, F., & Innes, M. (2013). The problem of success: What is a 'good' homicide investigation? *Journal of Policing and Society, 23*, 292–310. Part of a special issue entitled, Homicide Investigation: International Research and Insights.

---

[1] Below we include some of our favorite texts that have adopted ethnographic methods or some kind of sustained fieldwork, as well as a few of our own pieces based on the research described above. Most of the publications are based on ethnographies of police departments. The few that aren't – such as the texts by Tony Parker – nonetheless have been inspirational to one or other of us and contain useful information about the art and craft of speaking to, and researching, difficult to reach groups.

Brown, M. K. (1988). *Working the street: Police discretion and the dilemmas of reform*. New York: Russell Sage Foundation.

Dabney, D. A., & Tewksbury, R. (2016). *Speaking truth to power: Confidential informants and police investigations*. Berkeley, CA: University of California Press.

Hobbs, D. (1988). *Doing the business*. Oxford: Clarendon.

Innes, M. (2003). *Investigating murder: Detective work and the police response to criminal homicide*. New York: Oxford University Press.

Jackall, R. (2005). *Street stories: The world of police detectives*. Cambridge: Harvard University Press.

Manning, P. (1980). *The narcs' game: Organizational and informational limits on drug law enforcement*. Cambridge, MA: MIT Press.

Muir, W. (1977). *Police: Streetcorner politicians*. Chicago: University of Chicago Press.

Parker, T. (1970). *The frying-pan: A prison and its prisoners*. London: Hutchinson.

Punch, M. (1979). *Policing the inner city. A study of Amsterdam's Warmoesstraat*. London: Macmillan Press.

Rubenstein, J. (1973). *City police*. New York: Farrar, Straus & Giroux.

Simon, D. (1991). *Homicide*. Boston: Houghton Mifflin.

Soothill, K., & Morris, T. (1999). *Criminal conversations: An anthology of the work of Tony Parker*. London: Routledge.

Wilson, J. Q. (1968). *Varieties of police behavior*. Cambridge, MA: Harvard University Press.

Wilson, J. Q. (1978). *The investigators*. New York: Basic Books.

To the Bridge and Back: Risks, Rewards, and Rookie Mistakes in a Study of Postwar Bosnia

Stephanie M. DiPietro

To the Bridge

It was June 2014, and I had reached the end of my 6-month stay in Bosnia, where I had been gathering life histories and conducting ethnographic fieldwork for a cross-national study of postwar adaptation among Bosnian refugees and nationals. I spent the day before in a maximum security prison about an hour outside of Sarajevo conducting back-to-back life history interviews with five men incarcerated for violent offenses ranging from assault to homicide. The day at the prison was long, and the interviews were meant to be my last before leaving Bosnia, so when Darko[1]— my translator and a key "informant"[2] for the study—called to say he had arranged another interview for the following day, I was tempted to decline the offer. I was fortunate to meet Darko soon after arriving in Sarajevo. He responded to an email soliciting study participants that I circulated among students at the University of Sarajevo, where I was housed as a visiting professor. I liked Darko right away. He was savvy and quick-witted, with a perpetual smirk, and he showed a real interest in my research, offering his own insights and ideas about the directions it was taking. He spoke perfect English, a by-product of having spent 4 years studying in the USA, which made building rapport feel natural and effortless. Further, as a matter of coincidence, Darko had something that would prove invaluable to my research: connections to a network of criminal acquaintances to which I was desperate to gain access. It was not long after our initial interview that I suggested he work for me as my

---

[1] All names are pseudonyms.

[2] I use the term "informant" here to refer to study participants, as well as individuals who helped make introductions and bridge relational distance between myself and potential study participants (see, e.g., Jacques & Wright, 2008).

S. M. DiPietro (✉)
Department of Criminology and Criminal Justice, University of Missouri St. Louis,
St. Louis, MO, USA
e-mail: dipietros@umsl.edu

translator and my informant—someone who could connect me with willing study participants, who, for the equivalent of about $20, would share their personal histories with me. I explained to him the major focus of my work: examining the shared experiences of individuals exposed to wartime violence in childhood, as well as the divergent pathways that led some individuals to crime and violence in adulthood. What I needed, however, as a point of comparison with the interviews I had already conducted, was to reach a clandestine population: criminals, preferably violent ones. Could he be of help? He asked me to list my criteria: men and women between the ages of 22 and 38,[3] who had been exposed to the war in their youth and had a history of crime and/or violence. "No problem," he said. I was elated.

With Darko as my informant, the opportunities to explore some of the more pressing questions that led me to Sarajevo in the first place began to unfold: How do men and women exposed to wartime violence in their youth make sense of their experiences? How are these cognitive schemas woven into their narratives of identity? What are the perceived sources of risk and resilience in individuals' life histories that help shape pathways to crime and violence in adulthood? Darko proved a useful informant right away. Within a matter of days, he had arranged interviews with several participants. There was Asad, a charming and affable college student who supplemented his income smuggling immigrants across the border of Bosnia, and Igor, sullen and wiry, with (by his own admission) an explosive temper, who shared with me the story of soldiers bargaining flippantly over his life when he was just 8 years old. Darko introduced me to Irena, who proudly recalled her own military service during the war when she was just a teenager and her days working at a local betting shop that catered to Bosnian mafia. The interviews were fascinating and at times devastating, and each one seemed to open a Pandora's box of more research questions than I could ever hope to answer. I was quickly overwhelmed with data, but I felt a sort of exultation doing this work I had never before experienced. I recalled a colleague and mentor telling me "qualitative work is life changing." I was beginning to think he was right.

On that day in June, Darko was almost breathless with excitement on the phone. He told me he had arranged for me to meet someone, a man who had served as a child soldier during the war and was now somewhat of a fabled criminal in Bosnia, having been formerly incarcerated and deported back to Bosnia for a litany of criminal offenses, including, Darko informed me, rape and murder. This guy is "crazy," Darko said, a "real bad mother fucker." Did I want to meet him? It was without the faintest hesitation that I answered. Absolutely I did.

The following morning, I waited with Darko at the courtyard café of a local hotel, where I was conducting many of my interviews. It was a neutral meeting place, I felt, located in the heart of the city but somewhat removed from the noise and chaos of old town Sarajevo. My presence had become familiar to the waiters at the café, who regarded me with suspicion and even disdain as I sat, time after time, always with different people, the light of my small recorder glowing red on the

---

[3] The age range was selected to include only people who were between infancy and adolescence during the war.

table. Abdullah arrived just as the rain started. He was an imposing figure, maybe 6 ft tall, nearly 300 pounds, with a constellation of scars and tattoos covering his arms. He barely glanced at me when we said hello, offering only the slightest nod in response to my outstretched hand, speaking to Darko in a sideline murmur. Right away, he seemed agitated and out of place at the hotel, which catered more to wealthy tourists than locals. When the waiter approached to take our order, he seemed almost startled at the sight of Abdullah sitting there, brooding and silent. Did they know one another? It was not clear to me but I watched our waiter tap the arm of another and gesture toward us as he made his way to kitchen. We sat for a while, sipping coffee and smoking cigarettes, while Darko translated awkward, mostly one-sided, small talk about the impending storm. Abruptly, Abdullah said something to Darko that I could scarcely understand. After 6 months in Sarajevo and more than a year of studying the language, my Bosnian was still rudimentary at best, probably akin to the level of a 6 year old. Darko looked wary. "He won't do the interview here," he said, while Abdullah stared at me, stony and cool. "He wants to talk in his parked car, where no one can hear us." If there was a moment at which some internal alarm should have sounded for me, this should have been it, but there's a seduction to this kind of research that rarely makes its way into published articles. I was well aware from my conversation with Darko that Abdullah was capable of extraordinary violence, particularly toward women, and it was *because* of that fact, not *in spite of it*, that I agreed, with minimal hesitation, to leave the hotel. I wanted to hear his story.

We walked together along the cobblestone streets of Baščaršija,[4] Abdullah and Darko ahead, with me trailing just behind. Whatever looks of recognition I thought I observed on the waiters' faces were unmistakable now. Eyes turned to us as we passed by. Abdullah was a commanding figure, but there was something distinctive in the faces of people sitting at the cafes and restaurants along the way. It was a look of recognition, maybe even awe. Abdullah seemed aware of it too, walking steadily ahead with his chest puffed out and his hands, balled into loose fists, at his side. Abdullah's car, a two door, was parked in a small lot away from the main strip and the crowd. Just as we reached it, the rain came in torrents, and I fretted silently over the way in which the storm would affect the quality of my audio file. Darko climbed into the back seat, and I sat in the front, my notepad and recorder in my lap, ready to begin. When Abdullah slumped into the driver's seat, I felt the car heave under his weight. He addressed Darko with a nod in the rearview mirror and a few quick words in Bosnian, which Darko relayed to me. "Uh… he wants to take us somewhere, to show us a bridge." There was uneasiness in Darko's voice; he was not asking for my consent. Before I could respond, Abdullah was backing out of the spot, and we were off, the bustle of the city fading quickly into the background as we drove into the surrounding hills.

The road to the bridge was winding, full of gravel and potholes. Abdullah drove fast, glancing at me as he took particularly perilous turns, as if to gauge my reaction. We rode silently for the first few minutes, and I felt a sense of panic creeping into

---

[4] Baščaršija is Sarajevo's old bazaar and considered the historical and cultural center of Sarajevo.

my chest. Sarajevo, like many of the cities and villages in Bosnia, is still marked by signs of the war; abandoned houses, burned to the ground or riddled with bullet holes and mortar shells, are abundant, particularly in the city's outskirts where we were headed. Entire villages destroyed during the war have yet to be rebuilt or repopulated, and the sense of isolation driving through these areas is palpable. Abruptly, Abdullah began to talk, and Darko began translating simultaneously. He began with a dizzying inventory of his many crimes, telling me first about a man he kidnapped not long before because he dared to curse at his young daughter, only 12 years old. He searched for this person for a month, he said, and when he found him, he brought him to an old bridge outside the city, remote and surrounded by woods, where he broke his kneecaps and his eye sockets. This is where he was taking us for the interview, he said, so we could "see for ourselves." He rambled about the multiple incarcerations and deportations he had experienced, volunteering that "they" [unclear who "they" are] tried to confine him to an orphanage when he was only 13 because there is no juvenile system in Bosnia for young offenders. He told us of his business endeavors, the restaurants and bars he had owned at one time or another and the brothel he managed before he was deported back to Bosnia. He took out his cell phone and began scrolling through photos, one hand precariously minding the steering wheel. There were photos of cars, "ones he had stolen," buildings he had burglarized, and then a face, bloodied and swollen. It was the man from the bridge, he said with a laugh. I asked him if he worried about the risk of keeping these photos on his phone, and he said, affronted if not a bit amused, that he "worries about nothing." He had no education, he said. His only schooling was the war and prison and the life he led on the streets. A diagnosed sociopath by his own account and deemed by "the government" too unstable to be confined in any institution. He is "the real deal," he said to me. And to Darko, "Make sure she writes that down."

It was maybe twenty minutes before we arrived at the bridge, which had been demolished in the war and was now little more than a heap of bullet-ridden stones, overgrown with shrubs and weeds. The remoteness of this spot weighed heavily on me. I glanced at the surrounding woods, wondering, if I had to run from the car, where I could possibly go. Although Darko's presence in the seat behind me was reassuring, I sized up the difference in their stature and realized with grim acceptance that if something were to go wrong, there was little he could do to protect me. I was in way over my head. Abdullah turned off the ignition, lit a cigarette, and turned to me, his eyebrows raised in a question, "What else do you want to know?"

The Road to Sarajevo

The path that led me to Sarajevo was circuitous and serendipitous. I came to the University of Missouri—St. Louis (UMSL) in 2010 as a visiting assistant professor, with no intention (and no real option) of staying beyond my 2-year contract. Trained as a quantitative researcher, my research interests centered primarily on immigration, with a focus on patterns of assimilation and delinquency among immigrant

youth, which I studied with large-scale secondary data sets including the Project on Human Development in Chicago Neighborhoods (PHDCN), the National Longitudinal Survey of Youth (NLSY97), and the Children of Immigrants Longitudinal Study (CILS). As a newly minted PhD, I felt the pressure of the "publish or perish" mantra hanging over me like an ominous cloud, so I set to work building a record of publications that would help me to land a tenure track position and eventually solidify my case for tenure, wherever I ended up. It was early on in this phase of my career, however, that a couple of serendipitous (albeit mundane) occurrences ultimately changed the course of my career and, in some ways, my life.

First, even though I was barely out of graduate school, I found myself grappling with a mounting sense of frustration with the research agenda I had laid out for myself, fueled largely by the disconnect between my research questions and the data sets I had been using to answer them. My interests were in the nuances of immigrant assimilation, the pre- and post-migratory experiences of immigrant and refugee groups, and the changing dynamics of family relationships in the wake of migration. Whereas a voluminous body of work dating back to the turn of century explores these issues, much of the quantitative research—including my own—relies on blunt instruments to capture constructs as complicated and nuanced as "assimilation" and "acculturation." Absent from much of this work are the voices of immigrants themselves and the *meaning* they ascribe to their own experiences. Further, with some important exceptions (see, e.g., Coutin, 2016; Menjívar, 2000), much of the contemporary immigration literature conflates refugee and immigrant status, neglecting the unique push and pull factors that motivate individuals to leave their home countries to rebuild their lives abroad. Housed under the same umbrella term of "immigration" are two vastly different social processes, each with their own unique implications: voluntary migration versus exile. There was something about the latter that was of great interest to me.

Second was the opening of a tenure track position at UMSL. In the few short months I had spent there, I had grown to love St. Louis, my department, and my colleagues so I was elated at the opportunity to stay. Securing the position at UMSL—and knowing I would be able to settle in St. Louis for at least a few years—also presented an opportunity to do something I had not attempted as a graduate student: to collect my own data, more finely tailored to my research interests than the secondary sources I had been using.

Lastly was a rather banal conversation I had one day with my department chair shortly after accepting the tenure track position, which ultimately was the catalyst for the Bosnian project. It went something like this: "You study immigration right? There's a whole lot of Bosnians in St. Louis. Maybe you should look into that."

Indeed, in the mid to late 1990s, St. Louis, Missouri, became the unlikely home of tens of thousands of Bosnians, refugees of the war, and genocide that ravaged the country between 1992 and 1995. The State Department designated St. Louis as a preferred community for resettlement because of the availability of housing and the relatively low level of ethnic competition over low-wage jobs that typify gateway cities such as New York and Los Angeles (Matsuo, 2005). Subsequent to the initial resettlement in 1993, St. Louis experienced a large-scale secondary migration of

thousands more Bosnians seeking affordable housing, job opportunities, and the chance to reunite with friends and family divided by the war (Matsuo & Poljarevic, 2011). Today, an estimated 50,000–70,000 Bosnians live in St. Louis, making it home to the largest concentration of Bosnian refugees worldwide.

My initial foray into the Bosnian project started in 2012. I began by reading countless books and articles about the Bosnian war and genocide. At the same time, I searched for research that had been done on the Bosnian settlement in St. Louis and on patterns of crime and deviance within the community. Although the settlement was nearly 20 years in the making, surprisingly little had been written about it, and almost no studies had endeavored to study patterns of crime or delinquency specifically. Part of this omission was due, no doubt, to the paucity of data that tracks the nationality of offenders, as the collection of such of information is rare. Rather, much of what was known about crime within the Bosnian community in St. Louis was limited to journalistic accounts and anecdotal evidence, which painted a paradoxical portrait of a community that was faring well in some respects and struggling in others. For example, while the community has long been credited with revitalizing the Bevo Mill neighborhood of South St. Louis,[5] anecdotal evidence suggested that drug use, crime, and violence are emerging social problems. Some members of the community expressed fear that young Bosnians "have lost their way" or have become "too Americanized," the latter of which has, for some, become synonymous with deviant behavior. I came across a handful of local news stories detailing crimes committed by young Bosnians in the community. There was one about three 17-year-old Bosnians charged with second-degree burglary after stealing from a local Bosnian cafe, another about a young Bosnian who had stolen more than $5000 from a local mosque, and another about a 19 year old, once a rising basketball star, arrested for particularly nefarious home invasion. And then I came across the story of two men, both refugees of the Bosnian war, dated March 29, 2008:

> "Fight that killed 1 immigrant leads to 10 years for other."

> While one died a violent death the night of July 2, 2006, the other's plans crumbled Friday as a St. Louis Circuit Court judge sentenced him to 10 years in prison. Bekir Ademovic, 31, was convicted of voluntary manslaughter in the death of a Sejfudin Suljic, 42, who was found dead in a alley next to the Kemix Bar, at 4701 Morganford Road.
>
> Defense attorney Ted Luby argued Friday that Ademovic's violent response during a bar fight was instinctive for a man who spent years steeped in a war in Bosnia and held in a prison camp.

It was the last line that struck me. If lethal violence was indeed "instinctive" for a man "steeped in war," then logic would dictate that rates of violence among war refugees should be extraordinarily high. Many of the refugees in St. Louis came from the most war-torn villages in Bosnia, including Srebrenica, the sight of the worst genocidal massacre of the war, during which 8000 Muslim men and boys were executed in the span of just a few days. Of course, the defense attorney's proclamation was not

---

[5] http://www.stltoday.com/business/local/immigrants-are-very-welcome-in-st-louis-and-much-of/article_b20abb98-9594-5213-8a39-62e201f1b3fe.html.

rooted in empirical research, and there was no indication that rates of violence were particularly high in the community, but it raised important questions about the enduring consequences of exposure to extreme violence. I broadened my literature search to include research on postwar adaptation, post-traumatic stress, and the behavioral implications of exposure to war zones, particularly among children. Much of this work was quantitative and came from other disciplines (e.g., psychology, medicine, anthropology), as, until very recently, criminology as a discipline has been curiously silent in the discussion of war and its implications (see, e.g., DiPietro, 2016; Hagan, 2015; Jamieson, 2014; Walklate & McGarry, 2015). On balance, the research suggested that exposure to war—particularly for children—is a risk factor for a variety of maladaptive outcomes, ranging from depression to violence. What was less clear, however, is how and why some individuals exposed to wartime atrocities maintain their resilience.

It was at this point that I began conceptualizing how I could go about studying the enduring consequences of exposure to wartime violence among Bosnian refugees in St. Louis. My initial idea was to develop a survey instrument that I could disseminate in the Bosnian community through the help of some initial contacts I had made. But, as I labored over the design of the instrument, codebooks from other data sets spread out around me; I felt that same frustration creeping up again. The research questions I was formulating did not lend themselves to surveys or any type of purely quantitative data collection. I wanted to know how Bosnian refugees made sense of their own experiences and how they narrated their own journeys. It was becoming increasingly clear to me that I needed to talk to people.

The idea of conducting in-depth life history interviews was a daunting endeavor for a couple of reasons. First, my experience with qualitative research methods was limited to my coursework as a sociology master's student, during which I hung around a fringe religious organization for months conducting ethnographic fieldwork on processes of collective behavior and influence within the group. It was then that I fell in love with qualitative methodology, but my doctoral program provided few opportunities to pursue it. Second was the pressure to publish. Gathering qualitative data, transcribing interviews, coding, and analyzing data are a time-consuming process, and I feared it would set me back in my progress toward tenure. I knew the project was potentially risky for that reason, but in the end, my curiosity won. With the aid of a small seed grant and help from one of my colleagues and best qualitative scholars in the field,[6] I began gathering life histories from Bosnians in St. Louis in the fall of 2012. My initial sampling strategy was largely one of convenience as I had a number of Bosnian students who were willing to share with me the names of friends and relatives with whom I could speak. A handful agreed to participate themselves and sat with me for long hours in my office or in coffee shops telling me the stories of their family's struggles during the war and their migration to the USA. From these initial contacts, I adopted a "snowball" or chain-referral strategy

---

[6] I am indebted to my former UMSL colleague Richard Wright who was a strong advocate of this project and encouraged me to take the plunge into qualitative research. Without his encouragement, mentoring, and advice, I would not be writing about this journey.

(Biernacki & Waldorf, 1981), whereby I built my sample incrementally from the recommendations of my participants. As I honed in on emergent themes in the data, I began sampling theoretically (i.e., searching for individuals that met increasingly specific criteria, so I could develop my emerging theoretical categories). I augmented my in-depth interview data with ethnographic field notes taken while attending Bosnian cultural events and speaking informally with individuals with strong ties to the Bosnian community (e.g., imams, scholars, refugee outreach coordinators).

With each interview, the larger contours of the study began to take shape. Some of the initial themes I identified in the data were congruent with the existing literature on acculturation, and acculturative dissonance, within immigrant families. Other emergent themes were wholly unexpected. I started paying close attention to the ways in which refugees narrated their own experiences with war and learned, much to my surprise, that many regarded the war in a positive light, a time during which neighbors and family members bonded together in solidarity and support. These unexpected insights shifted the focus of my original research questions and led to progressively more purposive sampling. I began to wonder about the power of narrative for shaping identity and behavior. How do Bosnian refugees narrate their experiences with war? How do these cognitive schemas figure into their constructions of identity and behavior over the life course?

Some suggest that qualitative inquiries are often rooted in the biographical experiences of the researcher (Lofland & Lofland, 1984). Certainly, some of the best ethnographic research in criminology is written by scholars with a personal connection of the groups or communities they research (see, e.g., Contreras, 2013; Rios, 2011). Sharing social characteristics (e.g., gender, race/ethnicity, nationality, etc.) with one's informants is undoubtedly an advantage to building rapport and gaining trust; however, from my experience, my outsider status was a double-edged sword. Language barriers, unfamiliarity with local politics, and culture inhibited my understanding in some instances, but it also gave me an air of neutrality when asking about emotionally or politically charged issues. Absent the assumption of shared understanding, I was free to press for details on certain issues (e.g., the politics of the war) and to ask for clarification without sounding foolish or judgmental. That is, the social distance between my study informants and myself allowed us to take on the roles of student and expert, the latter of which seemed to be empowering for the men and women in my study.

Given my outsider status, it never surprised me in the course of my interviews when I was asked from where my interest in this population stemmed. Was I an immigrant? Was I Bosnian? Had I been to Bosnia? There was—at least to me—an air of doubt in these questions, a criticism of sorts that I could not quite put my finger on until one day, in the course of a conversation at a local coffee shop, one of my informants asked me, rather bluntly, "How can you study Bosnians without spending time in Bosnia?" It was a fair question and one I had been pondering myself since the project began. It also hinted at one of the more ambitious angles of the project with which I had been grappling: a truly cross-national comparison in the same vein as my intellectual heroes, Thomas and Znaniecki (1918–1920), who

captured in exquisitely rich detail the experiences of Polish peasants who migrated from Poland at the turn of the century as well as those of the families and communities they left behind. The idea of going to Bosnia to gather comparative life histories from nationals who stayed behind after the war was an ambitious goal, but I decided it was worth pursuing. I set about finding a way to get there.

For students and academics wanting to conduct international research—particularly qualitative research on a subject that has still yet to make its way into mainstream criminology—funding opportunities are considerably scarcer. Large funding agencies such as the National Institute of Justice (NIJ) and the Bureau of Justice Statistics (BJS) are oriented toward research based in the USA, and foundations like Carnegie and Guggenheim tend to select more seasoned, established scholars than myself, who was not yet two years out of grad school. Talking over my options with a colleague who had been supportive of the project from its inception, I came upon what felt like a long shot possibility: I would apply for a Fulbright fellowship.

After a 9-month application period, during which I passed (with both joy and surprise) each successive round of the Fulbright review process, I was granted a Fulbright fellowship to spend a semester at the University of Sarajevo as a visiting scholar in the Department of Criminology, Criminal Justice and Security Studies. I arrived in January 2014, just a few weeks before the city erupted in what the BBC termed "the worst unrest since the end of the 1992–1995 war."[7] Frustration over high rates of unemployment and rampant government corruption had bubbled over, and Sarajevo citizens took to the streets for three days of violent protest. I admit to feeling uneasy at this segue into my Bosnian adventure. The constant assurances I had made to my family that the situation in Bosnia was "perfectly safe now" started to feel a bit premature. The protests ended quickly, however, and I set about building a rich data set on the experiences and perceptions of Bosnians who stayed after the war (or returned after a period as refugees) to compare with my St. Louis sample.

Although I had fewer initial contacts in Sarajevo than when I began my data collection in St. Louis, my residency at the University was a boon to connecting with people, as was the small notebook I carried with the names and phone numbers of more than a dozen people in Sarajevo—friends and relatives of people I knew in St. Louis. My language skills were limited—and far below a level of competency needed to conduct interviews—but I was surprised to find that many people in Sarajevo spoke fluent English, which also was an enormous benefit to my small translation budget. In addition to my initial set of contacts, I connected with other potential informants by striking up conversations wherever I went. One of the most informative and interesting interviews came about because I asked my taxi driver about the bullet dangling from his rearview mirror. And then, I circulated the email advertising the study that would lead me first to Darko and eventually to Abdullah.

Back from the Bridge

Were it not for the time stamp on my audio recorder, I would have struggled to esti-mate the length of my interview with Abdullah at the bridge. Although it felt far longer, it was just under two hours. My suspicion that he would be unwilling to let me record our conversation was totally unfounded, as he seemed to relish the oppor-tunity to give an interview, signing my consent form with a giant "X" and asking more than once if "the book I was writing was about him." Abdullah rambled for much of the time, cataloguing his life and his many violent offenses with pride, tell-ing me more than once that he was "a real soldier" and a "real warrior," nothing like those men who were "too chickenshit" to serve their country. At times, I would interject, asking him questions about his family: "Do you have any siblings?" my voice on the audio file is clear but full of trepidation, as if I was balancing on a powder keg. "Only my brother," he said, pulling out a handgun and explaining to us that he always keeps a bullet in the chamber. The gun was his only "brother," he said, "the only one I can trust." He placed it on the dashboard in front of me, a ges-ture that felt strangely like some sort of dare. "No sisters then?" I asked, trying to sound unmoved. I asked if he still thought about the war and his time as a soldier. Did he think it shaped the man he is today? To this, he pulled a bag of white powder from his pocket explaining that he uses it to calm himself down. "I rarely sleep at all," he said, offering the bag to me. I declined politely, thanking him in Bosnian. After a few snorts from his pinky nail, long and yellow, he explained: "I was not afraid of anyone. That must have been in their heads. Because I was a minor when I went to war. They thought I would shoot for them, and that they would push me to go to jail, and they would keep living. But here I am."

   It was more than an hour into our interview when Abdullah's mood shifted. He appeared to lose interest in regaling us with stories of his bravery and his willing-ness to "defend what is his at any cost." His focus shifted to more salacious topics: women and sex. For the second time, he pulled out his cell phone to show us photos, this time of various women, his "girlfriends" he told us. The photos were of differ-ent, scantily clad if not completely naked women, posing awkwardly, their eyes staring blankly at the camera. I glanced at Darko, whose expression reminded me of the rabbits I find in my garden when one of my dogs has them cornered. We both nodded and offered affirmative "Mmm hmms" to Abdullah's slide show, but (as we would discuss later) it was at precisely at this point when we both knew we needed an exit strategy. Abdullah put away his phone with a sigh and said something, leer-ing at me. Darko did not translate, which agitated Abdullah, now rather high on speed with his gun still on the dashboard. "Reci joj. Reci joj," he said. This I under-stood. It means "tell her." Darko sounded nervous. "He says he falls in love real easy. You shouldn't be teasing him like this." I let out a startled little laugh and checked my watch, every muscle in my body tensed. Abdullah looked me up and down. "He wants to know if you know any nice girls for him." "Sorry, no," I offered. Abdullah explained to us that he had been dating a "really hot girl from the betting shop," but she was "running her mouth," so he "kicked her so hard in the stomach

she pissed in her pants." "Right there where you're sitting," Abdullah added with a laugh. "You're in the hot seat," he said, winking at me.

The few friends and colleagues with whom I have shared this experience have asked me how I managed to bring this interview to an end and get Abdullah to take us back to the city. To this, I always credit Darko, who had the good sense to tell Abdullah that I was expected back at the American Embassy, a clever white lie that seemed to break the tension in the car and distract Abdullah from whatever thoughts he had about the turn our interview was taking. Darko must have said that phrase three or four times, "Američka ambasada," until Abdullah finally relented and started the car. A wave of relief swept over me as we made our way to the outskirts of the city. As we neared the city center, Abdullah spotted a young guy in a soldier's uniform standing guard in front of a bank, a large automatic rifle in his hands. Abdullah slowed the car and uttered something to Darko, a malevolent grin spreading across his face. "He wants to know if you want him to take his gun. The guard's gun," Darko said. "No, no, no, that's okay," I said, trying to sound impassive and amused. Abdullah beckoned to the guard anyway, leaning over me and yelling something to the guard, who walked stiffly toward the car, hands on his rifle, a look of unmistakable rage in his face. I was frozen and utterly terrified by what was about to happen. Darko whispered to me in English, "Look, look, here comes this mother fucker." A few feet from the car, the guard caught sight of Abdullah's face. He stopped in his tracks and retreated without a word back to the bank. Abdullah laughed raucously and yelled something else at the guard, who stood motionless at his post, as if he heard nothing. Darko laughed too, although it sounded forced and tense, so unlike the affable cackle I was used to hearing from him. He whispered in my ear, "See? No one can touch this motherfucker." We sped away and Darko asked me where I wanted to be dropped off. I pointed to the first corner, about an eight-block walk in the rain from my apartment. "Here. Tell him here is fine."

Lessons Learned: Risks, Rewards, and Rookie Mistakes

My interview with Abdullah was the 96th I conducted for the Bosnian project. Although, the experience was atypical in many ways, it is illustrative of some of the most important lessons I have learned over the course of this project, the subject of which I turn to now.

First, one of the biggest challenges in qualitative research is gaining access to the culture or group you wish to study. This difficulty is exacerbated when the relational distance between yourself and the group is great or if the group includes difficult to reach populations, such as active offenders or otherwise "fringe" populations. Because this topic has been written about extensively (see, e.g., Jacques & Wright, 2008; Sluka, 1990; Williams, Dunlap, Johnson, & Hamid, 1992), I defer to those experts for some practical advice. From my own experience, there was a bit of luck involved in finding my informants. For example, my efforts to gain access to the men incarcerated in Bosnia were largely reaching a dead end until I happened to

meet an individual with connections to the Bosnian Ministry of Justice, who was able to streamline my request, bypassing what might have been months of red tape. Likewise, meeting Darko early on in my time in Sarajevo was invaluable for helping me reach an understandably hidden population. Finding key informants or brokers who can bridge relational distance between you and the population you wish to reach is critical. Further, the very best informants fulfill multiple roles. More than study participants, they offer their own insights into the nature of the phenomena of interest and, in some instances, serve as "protectors," helping to navigate dangerous terrain. Of course, this method of "snowball sampling" (i.e., relying on informants and study participants to provide names of other participants) presents a certain type of hurdle for qualitative researchers, a subject on which I elaborate below.

Second, although qualitative methods have gained much in legitimacy and popularity in recent years, criminology as a discipline has traditionally been preoccupied with and dominated by an approach to science that favors quantitative methods and leaves little room for personal experiences (i.e., our "human selves") in our research endeavors. As quantitative researchers, we are taught to distance ourselves from the research process, for fear that our interests and positions—our very presence—somehow contaminate research and diminish its value as science. Qualitative methods have been derided as journalistic, impressionistic, and too informal to constitute "scientific objectivity." By contrast, quantitative methods by their nature allow for a level of detachment and anonymity from the research process, a quality that has been lauded by many as more "scientific."

This dominant paradigm has a twofold effect on the discipline. First, it means that qualitative researchers are asked to defend their methodological approaches to a greater degree than quantitative scholars. Although qualitative and quantitative scholars have their own criteria for establishing quality and rigor in their work, at times we are held to the same measuring rods. For example, although the use of snowball sampling is quite typical in qualitative studies, particularly those that include active offenders or otherwise tough to reach populations, some reviewers might question the "generalizability" of our work, a criticism that is more appropriate for quantitative studies, which aim for statistical generalization. This scrutiny is potentially off-putting, particularly to graduate students who fear (in addition to the time commitment involved in qualitative research) they might be taken less seriously than scholars working in the quantitative tradition. As qualitative researchers, our goal is to recognize patterns in our data, to make sense of the phenomena we are studying. To this end, we do not seek generalizability as much as we seek to ensure that our sampling strategy is purposeful and aligned closely with our emerging process of data collection and theory building. Second, by distancing ourselves from the research process, we forfeit insights that might help us to understand the phenomena we are studying. A reflexive methodology allows for the recognition that the researcher is not disembodied from the research process; on the contrary, our human selves play a vital role at every stage, from conceptualizing the study and research questions and conducting observations and interviews to writing up the final product. For me, being cognizant of how my own presence shaped each phase in my study allowed for a richer understanding of the emergent themes in my data.

From his renegotiation of the terms of our interview (e.g., moving from the café to his car) to his displays of control and power in the car (e.g., speeding through the hills of Sarajevo, brandishing his gun, and threatening the security guard at the bank), Abdullah enacted his masculinity for me, teaching me on a visceral level what it means to him to be a "real man," a "real soldier," the "real deal," as he put it. Thus, rather than disregard my experience interviewing Abdullah as tangential to the process of "truth finding," I have come to appreciate the value of his "performance" and view it as integral to theorizing how identity and masculinity are jointly constructed in the context of interviews.[8]

Lastly, one of most rewarding insights of my study—what I felt was the missing piece in my quantitative research—is recognizing the power of one's own narrative as a determining force of their behavior. I began this work intrigued by a news story that put forth a very specific, testable hypothesis: Men exposed to wartime violence are more likely to be violent. It is a hypothesis that lends itself to quantification: one can easily define the concepts, gather survey data, and regress violence onto a range of independent variables. I admit, as a rookie qualitative researcher, my initial interviews are filled with questions geared toward "testing" this hypothesis. As I grew more comfortable with the process and more in tune to exploring the emergent themes in my data, my orientation shifted toward understanding the *subjective* meaning of one's lived experience. Some of the richest insights to be gleaned from ethnographic research, whether it involves observation or in-depth interviews, stem from examining *why* individuals think and act as they do. In the process of conducting my in-depth interviews and fieldwork in Bosnia and St. Louis, I learned that the *meaning* and *symbolism* individuals ascribe to their lived experiences are of critical importance to their identity, their behavior, and to processes of change. To illustrate, one informant told me in response to a question about his continued violent behavior, "For me, the war is still going on." However empirically incorrect this statement may be, it is fraught with meaning and symbolism, both of which have implications for behavior. As the classic Thomas theorem states, "if men define situations as real, they are real in their consequences." Thus, the ethnographic journey—messy, complicated, and at times even risky as it may be—ultimately provides the chance to explore the individuals' perception of the world around them and their definition of their "situation," the value of which is immeasurable for the field of criminology.

References

Biernacki, P., & Waldorf, D. (1981). Snowball sampling: Problems and techniques of chain referral sampling. *Sociological Methods & Research, 10*, 141–163.

Contreras, R. (2013). *The stickup kids: Race, drugs, violence, and the American dream*. Berkeley, CA: University of California Press.

---

[8] See Presser (2004) for an excellent paper on the construction of identity in the context of qualitative interviews.

Coutin, S. B. (2016). *Exiled home: Salvadoran transnational youth in the aftermath of violence*. Durham, NC: Duke University Press.

DiPietro, S. M. (2016). Criminology and war: Where are we going and where have we been? *Sociology Compass, 10*, 839–848.

Hagan, J. (2015). While criminology slept: A criminal war of aggression in Iraq. *The Criminologist, 40*, 1–6.

Jacques, S., & Wright, R. (2008). Intimacy with outlaws: The role of relational distance in recruiting, paying, and interviewing underworld research participants. *Journal of Research in Crime and Delinquency, 45*, 22–38.

Jamieson, R. (2014). *The criminology of war*. Aldershot, UK: Ashgate.

Lofland, J., & Lofland, L. H. (1984). *Analyzing social settings*. Belmont, CA: Wadsworth Publishing Company.

Matsuo, H. (2005). Bosnian refugee resettlement in St. Louis, Missouri. In P. Waxman & V. Colic-Peisker (Eds.), *Homeland wanted: Interdisciplinary perspective on refugee resettlement in the west*. New York: Nova Science Publishers.

Matsuo, H., & Poljarevic, A. (2011). Life satisfaction of the Bosnian refugees in St. Louis, Missouri. *International Journal of Humanities and Social Science, 1*, 30–38.

Menjívar, C. (2000). *Fragmented ties: Salvadoran immigrant networks in America*. Berkeley, CA: University of California Press.

Presser, L. (2004). Violent offenders, moral selves: Constructing identities and accounts in the research interview. *Social Problems, 51*, 82–101.

Rios, V. M. (2011). *Punished: Policing the lives of Black and Latino boys*. New York: New York University Press.

Sluka, J. A. (1990). Participant observation in violent social contexts. *Human Organization, 49*, 114–126.

Thomas, W. I., & Znaniecki, F. (1918–1920). *The polish peasant in Europe and America volumes I and II*. New York: Dover.

Walklate, S., & McGarry, R. (2015). *Criminology and war: Transgressing the borders*. New York: Routledge.

Williams, T., Dunlap, E., Johnson, B. D., & Hamid, A. (1992). Personal safety in dangerous places. *Journal of Contemporary Ethnography, 21*, 343–374.

Keeping Classic Ethnographic Traditions Alive in the Modern-Day Academy

Jamie J. Fader

Introduction

Classic ethnographies such as Liebow's *Tally's Corner* (1967), Stack's *All Our Kin* (1975), and Anderson's *Code of the Street* (1999) were years in the making. In the past, ethnographers were encouraged to dedicate substantial time in the field gathering rich data through observation and interviewing, establishing long-term relationships, and developing *verstehen*, or a deep understanding of the meaning of social situations from the perspective of participants. Ethnographers typically worked as "lone wolves," producing work in the form of scholarly monographs at a pace of approximately one a decade. These classic works—as well as their modern counterparts—retain their unique value because they represent lived experience, generate new theoretical advances through inductive analyses, and provide rich contextual details that generate deep understanding of social phenomena.

The modern academy, however, has ramped up expectations for publishing and sped up production of scholarly work, leaving ethnographers to wonder if these classic traditions may be abandoned. The number of published products required to achieve tenure and promotion has increased, as has pressure to secure external funding. Journals' impact factors have become an important metric of quality, as rewarded by merit raises and promotions. A new cottage industry of meta-publications has arisen, which documents the predictors of scholarly productivity, the publication acceptance rates of various journals, and the most productive "stars" in the field. Collaboration and co-authorship of publications has increased, allowing for more efficient division of labor. Scholarly monographs have lost their former prestige and, in the new atmosphere of quantifying contributions, often "count" for only a handful of articles in merit and tenure/promotion guidelines.

J. J. Fader (✉)
Department of Criminal Justice, Temple University, Philadelphia, PA, USA
e-mail: jfader@temple.edu

129

This emerging scholarly context presents opportunities to consider ethnography's contemporary role and to light a path for early-stage scholars who are committed to continuing the ethnographic "bloodline" in social scientific fields. Herein, I explore (1) what ethnographic traditions must be kept alive and (2) how to honor ethnography's unique qualities and strengths while meeting the demands of today's academic reward structure. This chapter will discuss strategies for maximizing the data that can be drawn from field research, constructing a research pipeline that includes ethnographic work, and crafting ethnographic products that can be published in a variety of scholarly outlets.

The "Academic Speedup" and Rational Adaptations by Scholars

Although there is little systematic documentation of the increased demands for productivity within the academy, it is hardly a stretch to say that most of us—regardless of our status as graduate students, tenure track or tenured faculty, or researchers working on "soft money"—feel them. It is not uncommon for first-year graduate students to learn in only their first few weeks of coursework that they must publish a number of peer-reviewed articles to be competitive in today's academic job market. (A graduate director at another institution used to pound his hand on the desk during the first proseminar, exhorting, "You. Must. Have. Pubs!") Increasingly, academic job advertisements note the need for evidence of scholarly productivity and potential for external funding (one of my current colleagues, hired as a postdoc, applied with 19 peer-reviewed publications). Tenure and merit pay requirements have increased, leading some senior scholars such as Nobel award-winning physicist Peter Higgs, who discovered the Higgs boson identification process, to conclude that he would be unemployable in today's academic culture because he was not productive enough. Moreover, he says, today's demands for productivity would never have provided the space and time for his discovery.

In *The Slow Professor: Challenging the Culture of Speed in the Academy* (2016), Berg and Seeber point to the corporatization of colleges and universities as the primary driver of the academic speedup. As graduates struggle on the job market and the value of a college education increasingly comes into question, the university must justify its existence in the public's eye. Increased demands for productivity and for self-funding are part of an increasingly competitive, consumer-driven market that places a premium on department and university rankings, on standardization of university functions, and constantly surpassing existing benchmarks. Fine and Hancock (2017) note that "the extended temporal engagement upon which ethnography depends … no longer fits a university in which academic demands increasingly resemble other forms of bureaucratic labor" (p. 261).

In addition to generating significant stress in the daily lives of academics, the academic speedup has had a number of structural effects as scholars have made rational responses to the newly emerging reward structure. Collaboration with co-authors has increased as a form of division of labor; the project or manuscript is thereby carved up into discrete roles for maximum efficiency of construction. Less flatteringly, some have been accused of "salami slicing," or submitting multiple manuscripts that could reasonably have comprised a single article. Journal impact factors (or "bibliometric accountability") have taken on increased significance in the process of ranking and quantifying scholarly products for merit raises, tenure, and promotion. As a result, some editors have been accused of artificially inflating their journal's impact factors through the excessive use of self-citation. The most egregious response to the academic speedup is outright falsification or gross mis-characterization of data, as has been levied in several recent high-profile cases in psychology, political science, and sociology.

The most widespread of these adaptations is the heavy reliance on secondary data analysis, which is made possible on a wide scale through the ICPSR's ware-house of federally funded data sets. The overwhelming majority of these are quantitative in nature. Empiricism, or the phenomenon of having available data drive researchers' questions, has been one unfortunate consequence of this trend. Another has been the ascendance of quantitative research to dominant status, with the concomitant marginalization of qualitative research and application of quantitative standards (i.e., generalizability) during peer review of qualitative research.

Although criminology and criminal justice are dominated by a positivist episte-mological frame, there is growing recognition of the value of ethnographic or quali-tative research. A search of top-ranked departments finds that most have at least one qualitative researcher, even if they occupy a token status. Thanks to the advocacy of Jody Miller, the American Society of Criminology has sponsored a qualitative methodology workshop prior to annual meetings almost every year since 2008. Some funding agencies and journals are taking steps to be more inclusive. For example, in 2005, NSF convened a panel of leading qualitative scholars to develop a series of guidelines that would be adopted for evaluating the rigor of qualitative research. A recent invitation to review for *American Sociological Review* included instructions that read: "Please note that this particular manuscript has been identi-fied by the editorial office as one that employs qualitative and/or ethnographic methods. Accordingly this article should be evaluated using standards appropriate to this particular approach to social inquiry."

Although these developments are promising, the academic speedup continues. Today's ethnographers, then, are working in an emerging context of increased ostensible support for qualitative research, but requirements for productivity and external funding are still inconsistent with the core traditions of ethnography. In the next section, I will attempt to locate ethnography within other forms of qualitative research, provide several working definitions, and identify a number of these core traditions that have been the hallmark of this form of research.

Ethnographic Traditions

In 2015, I developed and led a workshop called "Keeping Ethnographic Traditions Alive in Modern Criminology" as part of the abovementioned preconference methodology series. There, I presented a lengthy list of ethnographic traditions that came from my own methodological training in a department filled with ethnographers (Penn Sociology) and from my extensive reading of classic and modern ethnographic works. I also unsystematically polled a number of colleagues for their reflections on the question of "what must not be lost" in future generations of ethnography. These form the basis for my assertions below, although readers may identify other traditions that are consistent with their own training and experience.

Before delving into these traditions, I review some working definitions of ethnography, which include:

- "Systematic study of culture"—(Elijah Anderson)
- [Studying] "People in places"—(Robert Zussman)
- "...paint[ing] a picture of what people say and how they act in their everyday lives" (Taylor & Bogdan)
- "Research conducted in natural social settings, in the actual contexts in which people pursue their daily lives" (Robert Emerson)
- "…Examin[ing] social structure directly through observing people in action"—(William Foote Whyte)
- "Making the strange familiar and the familiar strange"—[attributed to the eighteenth-century German poet, Novalis].

As suggested by the range of definitions listed here, ethnography encompasses a variety of stances, including positivist and post-positivist epistemologies, and is practiced on a continuum between art and science. There is also a wide variety of types of ethnography, including but not limited to urban ethnography; institutional ethnography; critical, feminist, and/or liberatory ethnography; autoethnography; historical ethnography; "outlaw" ethnography; ethnomethodology; and visual ethnography.

Despite these differences, the following traditions are specific to ethnographic research and are the characteristics that make ethnography so valuable for informing both theory and practice:

1. *Immersion*—The tradition of immersing oneself in a social setting involves spending a long time—often years—in a single field site and is possibly the most threatened by the academic speedup. Related to immersion, field researchers often develop deep, personal, and lasting relationships with those they study. Relationships between Clifford Shaw and Stanley and Darrell Steffensmeier and Sam spanned decades. Moreover, and consistent with the critical stance toward objectivity noted below, these relationships typically go beyond professional bounds. In my own research, for example, I became godmother to my primary informant, Sincere's two sons. Tim Black's impressive work in *When a Heart Turns Rock Solid* (2010) documents almost two decades of his relationship with

three Puerto Rican brothers, one of whom he advocates for to get into a residential substance abuse treatment facility.

2. *"Thick description"*—A term coined by anthropologist Clifford Geertz, thick description is a quality of description in which the author employs an interpretive frame to embed written records of observations within the social context in which they were collected, in short, to explain the causes and intentions behind human action. A classic example of thick description is found in Whyte's account of bowling, in which he observes that the score reflects the social hierarchy of Doc's male peer group. Thick description is what distinguishes ethnography from most journalistic accounts, as it demands an interpretive lens and search for situated meanings.

3. *"Verstehen"*—First used by Max Weber, which, literally translated from German, means to "understand." Related to but distinct from thick description, the idea of *verstehen* involves a deep understanding of *people*, an almost psychoanalytic interpretation of motives, understandings, and decisions—only possible when an ethnography comes to know someone so well that we channel their voices in our heads as we write. This deep level of insight is apparent in Elliot Liebow's (1967) analysis of why marginalized men appeared not to take work seriously, explaining that "the job fails the man, so the man fails the job."

4. *Participants' own words*—The data of ethnographic field research involves written descriptions of settings and events drawn from the researcher's field notes but is generally also liberally peppered with excerpts from interviews with participants that captures their speech patterns and language, their imagery, and their manner of organizing the world. This tradition serves a number of purposes: to establish the authenticity of the author and analysis, as "data" or evidence employed to support an argument made by the author, to craft a richly textured scene for the reader, and—among those engaged in post-positivist epistemologies—to share some of the power of knowledge production with the subjects of study. Randol Contreras does this especially effectively in *Stickup Kids* (2013), as he makes use of extended Spanish passages to capture the interactions between Dominican American men in the Bronx.

5. *Systematic data collection and analysis*—As suggested by Anderson's definition of ethnography, an underappreciated quality of ethnography is the degree to which researchers *systematically* conduct field research and analysis. For example, grounded theory instructs us to employ an iterative process of data collection and analysis, developing hypotheses *in situ* that can be systematically investigated in the field. Moreover, it guides ethnographers to seek out "negative cases," or cases that are purposely designed to disprove working hypotheses. Ethnographers systematically document their research through the process of writing extensive field notes, employing "jottings" while in the field, and recording a high level of detail as soon as possible after leaving the field. Moreover, thematic coding is the traditional method of making sense of the data; this involves applying shorthand names to characterize events or tropes uncovered during field research and examining how these themes relate to one another or to existing theory. "Member checking" is a common means of ensuring validity of

data and accuracy of analysis, by asking research subjects to review it before going to press. Similarly, triangulation or "checking it out" is another way of ensuring validity, by fact-checking claims made by research subjects or generally relying on multiple sources of data. As I will discuss below, capitalizing on the systematic nature of ethnography is an effective means of convincing quantitatively oriented reviewers of the scientific validity of our work.

6. *Context*—The ethnographer's analytical frame is often formed by the context she provides as a way of interpreting the data. Context can include history, demographic characteristics, spatial characteristics and social ecology, physical descriptions of people or places, and the political and economic conditions of the research setting. Context is a critical and unique aspect of ethnography but one that also quickly consumes word counts; this is why most ethnographies are published as books. Fitting ethnographic studies into 8000–10,000 word limits maintained by most scholarly journals almost always necessitates the editing down of this context.

7. *Multiple levels of analysis*—The most sophisticated ethnographies attend simultaneously to three sources of human behavior: structure, culture, and human agency. Although most social scientists acknowledge that all choices are constrained by cultural norms and practices and structural features such as the labor market, status hierarchies (e.g., race, class, and gender), or spatial organization of resources, these multiple levels are difficult to model in statistical analyses the same way they are lived in experience. However, ethnographers can represent these levels as they operate concurrently and interactively in the lives and stories of our subjects.

8. *Reflexivity and a critique of "objectivity"*—Not all ethnographers take a postpositivist stance, but a critical stance toward objectivity is commonly adopted by ethnographers, who position themselves in relation to their research subjects and acknowledge the co-constructed nature of their work. It has become a standard practice to engage in "reflexivity," or the treatment of how one's own personal background and biases have affected the interpretive lens applied to the data generated by field research. My favorite example of this comes from Kenneth Clark's *Dark Ghetto* (1965/1989): "The reader should know that the author is a Negro, a social psychologist, a college professor, and that he has long been revolted by those forces in society which make for Harlems… Some form of subjective distortion seems inevitable whenever human beings dare to make judgments about any aspect of the human predicament" (p. xxxv).

9. *Transgression*—Although "outlaw ethnography" involving a degree of participation in illicit activities is fairly uncommon, one may argue that ethnographers—because of the dynamic nature of our research, our marginalized status within academic communities, our tendency to work alone, and our involvement with criminal subcultures—are more likely than other kinds of researchers to break the rules in various ways. For example, Jack Katz—who has a law degree as well as his PhD—has been outspoken about the implications of the expanded powers of the institutional review board (IRB) for field research demonstrating how human subjects boards have been used to censor controversial research.

Erich Goode became well known for admitting to having sexual relations with his research subjects. Despite the fact that few ethnographers would approve of such behavior, transgression maintains an important place in ethnography, if only by the critical stance that field researchers often apply to standard "scientific" conventions.

What Must Not Be Lost?

I polled several ethnographers about their perspectives on what must not be lost in the course of the academic speedup:

> People, people must not be lost. People in all their messy, complex, contradictory and maddeningly human ways must not be lost from our inquiries… Ethnographers…must maintain their own deep appreciation for, and devotion to, the meaningful contributions of our methods. We must staunchly defend the legitimacy of our training and of our scholarly products. We must demand that our work be judged by standards appropriate to the methods used. We must demand that qualitative research be judged by qualitative standards. For the sake of our human subjects and for all they have to teach the world, we must not give up on this.—Carla Barrett, author of *Courting Kids* (2012)

> We must remember that most/all of the classic works in ethnography, etc., were not 'rigorous' or 'scientific,' but rather impressionistic, emergent, humanistic, etc. So, my real fear is that if we try to make qual work more 'social scientific,' or to prove its worth on the terms of quant researchers and pseudo-scientists, we're going to take the heart out of it.—Jeff Ferrell, author of *Empire of Scrounge* (2006)

> Classic urban sociology ethnographies were so important because they shed light on how 'other' communities lived...By demonstrating similarities across communities, while highlighting how social structure and context influenced lives so deeply, these books allowed scholars to better understand the lives of groups who were distant from them, socially and economically… Ethnography represents our best chance at helping to bridge this gap by forcing privileged groups to better understand life from other, less advantaged perspectives.—Aaron Kupchik, author of *Homeroom Security* (2012)

> Ethnography cannot survive and thrive without books. Books are the lifeblood of our field--and they require A LOT of time to write. … But, as I look at today's job market, I often wonder: How many people could spend the time needed to research and write one of these long-term community studies? … I fear that we are coming to a point where talented ethnographers might not have the space to go out, explore, and 'get lost' in the world they're writing about… My biggest fear is that we are quickly moving into a context in which these kinds of books will no longer get written by emerging ethnographers.—Jooyoung Lee, author of *Blowin' Up: Rap Dreams in South Central* (2016)

> …I think about ethnographies written in the anthropological tradition – ethnographies that are multi-faceted, that render the unknown – or even the known – in complicated ways, ways that cover life from top to bottom, from left to right, from the visible to the invisible, ways that make us appreciate the hard work of the ethnographer in piecing what appear to be incongruent facets of life into coherent, comprehensible portraits. Yet when I think about ethnography today, … I think about ethnographies that provide limited snapshots of people

navigating their worlds – worlds that are too brief, concise, and isolated from wider society, worlds that are unimpressive, trivial renditions of social life. ... I think about how the promise of ethnography will soon go unfulfilled as it gets cut to pieces to conform to mainstream institutional norms.—Randol Contreras, author of *Stickup Kids* (2013)

Maximizing Ethnographic Data

The remainder of this chapter is designed to help ethnographers, particularly early career scholars whom are most affected by the academic speedup, think about how to maximize the richness of your data within the constraints of the modern-day academic reward structure. I have learned many of these lessons on my own path to tenure and promotion, which I am grateful to say I earned, despite ignoring advice from higher-ups about writing articles instead of a book.

Let Technology Do the Work for You

A major weapon in the war on the academic speedup involves harnessing the power of technology to increase efficiency and richness of data collection and analysis. Micro recorders have been used very effectively in a number of street ethnographies to capture the conversations between respondents that would be difficult to render accurately if we relied on memory alone and to retain the integrity of the language they use. In Bourgois's *In Search of Respect* (1995), his micro recorder captures the sound of gunshots, sirens, snorting cocaine, laughter, and masculine banter, all of which he was able to integrate into his work to bring the reader into the scene. Since his own voice is captured, it also positions him in relation to his respondents. Similarly, Contreras uses voice recorder in *Stickup Kids* (2013), noting that those he studied enjoyed speaking directly to the reader through the device.

Recorders can also be used in the researcher's absence to capture events and conversations that occur after she leaves the field, effectively extending the data collection period without the cost of actually being there. For example, Mitch Duneier (1999) left his cassette recorder behind to capture interactions between sidewalk book vendors and their customers. In my own research, I used a more structured approach, providing my respondents with microcassette recorders and collecting weekly "ethnographer's journal" assignments. Respondents used these journals as personal diaries, engaging in "desistance talk" that would have been unlikely to have been shared directly with me during the early days of the study.

Cameras are another aspect of technology that can enrich field research. Visual ethnography allows scholars to document changing social spaces and establish validity by allowing the reader to interpret scenes for themselves. The lines between ethnography and photojournalism have recently been blurred, with some journals

(e.g., *Contexts*) publishing photo essays accompanying scholarly treatments. Moreover, with the rapid improvements in cell phone technology, most of us have high-quality cameras that can be called upon without any advance planning.

In addition to containing cameras, smartphones also contain voice recording technology which can be quite useful. Researchers such as Naomi Sugie (2016), who intensively studied the employment searches of returning prisoners, have made innovative use of smartphone technology. With software she developed for the project and funding from the National Institute of Justice, she provided smart phones to respondents and paid for their data plans in exchange for their agreeing to complete regular surveys via the phone and have their texts and contacts analyzed to examine the extent of their social networks. Although the data she generated were numeric, this technique could easily be extended to qualitative research. Moreover, providing smart phones and a data plan as a participant incentive was a very effective technique for retention of a longitudinal sample.

Google Street View is a useful tool for allowing researchers to virtually be in the field to record details of the physical setting without traveling to the site. Virtual tours of a setting are beneficial when selecting a new field site and getting a "lay of the land" and for recording details that may have been missed during data collection, which allow for a more accurate description of street corners, houses, and store fronts. Google Street View has also been used for systematic social observation, conducted in the tradition of social disorganization theory. Whereas earlier scholars traveled up and down streets in a van with their own cameras and coders, documenting visible signs of disorder, others have found that virtual systematic social observation is both reliable and cost-effective.

As the use and influence of social media has spread, Facebook, Twitter, and Instagram have become fertile ground for content analyses. For example, Lauren Mayes (2017) examined the social media presence of police departments in nine cities, with the aim of documenting whether the content of their Facebook and Twitter posts was consistent with their objective of community policing. Mayes's work is also notable in that she moved beyond the standard method of using web crawlers to capture (or "scrape") online content. Because this content is so dynamic (i.e., posts and responses are deleted), she paired two applications—Inoreader, a Rich Site Summary (RSS) aggregator, and If This Then That, which link multiple apps together—to develop an automatic daily content "dump" that is saved into a Google Docs spreadsheet. As new apps are developed daily, these may present significant advances in the way multiple forms of online content can be captured automatically.

Social media can also be a good way to track respondents over time, as their posts can signal important life events such as new jobs, deaths in the family, or plans to move to a new neighborhood or city. In my own work, I have followed a small sample of formerly incarcerated men over the last 12 years, and Facebook reminds me of their birthdays and keeps me apprised of their emotional states, who they are spending time with, or how they curate their online personae.

Qualitative software packages such as NVivo, ATLAS.ti, and Dedoose involve a serious time investment up front to learn the features and mechanics of data analysis and manipulation but are, in my assessment, worth the effort for large projects

containing files in multiple formats or for collaborative projects. These packages can simplify some tasks, such as transcribing, coding, cross-classifying data (e.g., allowing for comparison of themes or excerpts across attributes of participants, such as gender), and creation of visual displays. As noted below, the use of such software makes the analysis process more transparent, which can quell the fears of quantitatively oriented external evaluators. However, I maintain a critical stance toward the mechanization of analysis and, like others, advise against the use of auto-coding in place of manual coding. I still think I am most intimate with my data when I am coding transcripts the old fashioned way, by hand. Avoid the temptation to present software-based analysis as more valid than other methods.

Other technological innovations include Google Voice, a useful tool for recording interviews conducted over the phone; Skype, which can also be used to conduct virtual interviews and reduce travel time; smart pens, which allow for digital recording of field notes and attachment of visual or audio files to text; and Pinterest, which allows for organization of webpages relevant to field research.

Involve Participant Collaborators

With some training, participants can make excellent collaborators and provide access to contacts and data that might not be easily available if a researcher is working alone in a new field setting. Respondents can be given assignments, such as journal entries or time diaries that they complete on their own and which are later submitted to the researcher. Some use a method known as photovoice, where participants take photos of their physical and social worlds, documenting spaces to which the researcher might not have access and using it as a springboard for conversations with change agents about assets and challenges in their communities.

Respondents have also been effectively used as recruiters, particularly for hard-to-reach populations such as active offenders. Moreover, they can be trained to collect data, such as conducting interviews, taking surveys, or drawing maps of their activity spaces. For example, Alice Goffman (2014) teamed up with one of her informants to conduct a household survey of Sixth Street, a site she would not have had access to without the sponsorship of someone that residents of the block knew and trusted.

"Ethnographizing"

We can often integrate an ethnographic component into a larger research design that uses other methodologies to provide richness and context, a process I call "ethnographizing." For example, several interview-based studies read like traditional ethnographies because they use techniques that allow them to follow (all or some) respondents over time and incorporate field notes into the interview process. Andrea

Leverentz's (2014) *The Ex-Prisoners' Dilemma*, for example, incorporates limited periods of participant observation with repeated interviews with women returning from prison and living in a halfway house. Other examples include Edin and Nelson (2013), who conducted relatively large-scale interview studies, but followed a sub-sample over time to document how their lives unfolded and whether their stated plans came to fruition.

Public community meetings, such as meetings of a neighborhood civic association, offer several advantages to researchers who want to ethnographize a study in which the community's context is important to the analysis. First, they typically engender little resistance from university IRBs because they are public. They allow researchers to make contacts with community leaders and residents who can be activated to locate research subjects. They are typically held at night, making them more easily compatible with teaching schedules. Community meetings offer a good training ground for student researchers who are learning field research methods. Moreover, they are a rich source of data about the ongoing concerns of the community's residents, which can generate new research questions or help explain findings generated by other research methods.

Focus groups are another way of quickly capturing data that can be used to complement and "ethnographize" other methods. David Grazian's (2008) book on college hookup culture featured a chapter based on focus groups of Penn undergraduates. More recently, comedian Aziz Ansari and sociologist Eric Klinenberg (2015) teamed up and used focus groups all over the world to explore how we find love in modern society.

I have been lucky enough to have strong professional development opportunities early on in my career. The next section shares some broader career strategies for integrating ethnographic work into your research agenda.

Other Strategies for Dealing with the Academic Speedup

Constructing a Research Pipeline

Few ethnographers can meet modern standards for productivity by conducting ethnographic research alone. A practical approach is to diversify the research portfolio with a mix of products, some of which draw on original data collection and some which utilize secondary data. Young scholars can also fill their research pipeline with long-term and "one-off" studies, using the shorter-term studies as a bridge between lengthier projects. Finding secondary data is easy; hundreds of data sets exist on the ICPSR website for immediate download and use. The challenge of using widely available data, however, is identifying scholarly niches where unique contributions can be made. Moreover, using large numeric data sets requires the analyst to have kept up with statistical advances in the field.

As noted above, multiauthored publications are becoming more common because they allow for division of labor. Partnering with a co-author who possesses sophisticated statistical skills is a smart tactic. It is incumbent on the qualitative researcher, however, to keep abreast of new developments in quantitative research so that they can participate fully in peer review of quantitative manuscripts and communicate with quantitative co-authors about decisions related to the analytical plan. One may not need to know how to conduct propensity score matching, for example, but it is important to recognize when its use is appropriate.

Thinking strategically about collaborators is also important. A mix of collaboration types may be warranted. Student co-authors can take on some of the more onerous tasks such as constructing tables or figures and getting references in order in exchange for a co-authorship role. However, there is generally an inverse relationship between the benefit the student takes from a collaborative experience and the time invested in including them in the process and developing their skills. This is a valuable endeavor but must be taken on with the knowledge that this type of collaboration is less efficient than projects that do not take professional development into consideration. Moreover, as an untenured scholar, one might select collaborators who are also untenured, who are equally invested in the timely production of their contribution and who are less likely to be burdened with university service and other administrative duties as are more senior colleagues.

Institutional Review Boards

Navigating IRBs has become an especially onerous obstacle for scholars planning to conduct original data collection, as universities have become increasingly concerned with risk management (i.e., legal liability stemming from the risks of research). This is especially true in criminology and criminal justice, which often involve seeking out crime- or justice-involved individuals as research subjects. In fact, some ethnographers have objected vociferously to the "mission creep" of university IRBs, refusing to seek institutional approval for their research by arguing that they are doing art instead of science. This is a risky position for junior faculty and an untenable one for scholars seeking federal grants, which require proof of IRB approval.

Securing approval for an original data collection project is made easier with some advance planning. First, talk to your colleagues about their experiences with the IRB. Try to ascertain the nature of your university's IRB. Some are highly adversarial, requiring scholars to arrive in person and engage in a ritual similar to a dissertation defense (at one such meeting I was asked how I could sleep at night when I was "funneling money into the drug economy" by compensating drug sellers $25 for an interview). Many others are collaborative, working with scholars to reach a mutually agreeable position on protocols. Some predominantly teaching institutions do not have much experience in reviewing original research. Each of these types of IRBs will require a different approach. Next, seek out an IRB member,

especially one in a social science department, to discuss your specific protocol. Ask them to help identify "red flags" or places that are going to generate concern by the board. Strategize about how to address these concerns in advance. Identify the components of your study where compromises would weaken the quality of your research and mark those as the battles you're willing to fight. Reserve less important issues as potential concessions.

Be persistent in negotiating a satisfactory outcome for all parties. This may also require knowing IRB standards. For example, a former student who wanted to conduct research at a residential motel known to house sex offenders encountered a request to complete an "off-site" research form, attaching a letter of permission from the motel's owner. Knowing this would likely put the kibosh on the project, I pointed out that the definition of "off-site research" is that which is conducted at an institution that must grant permission to access its wards (e.g., schools, hospitals, and prisons). The motel, however, was in no position to grant access to its residents. This argument was eventually successful, and the IRB in question soon thereafter eliminated its "off-site research" form altogether.

The most effective IRB protocols employ the narrative to educate the board members—who may be in radically different fields, including the hard sciences—about the unique value of ethnographic research and about the specific epistemology and methodologies that will be used. Making a case for the value of ethnography is important because these boards must evaluate the potential benefits of the research vis-à-vis the risks it presents. Make liberal use of references to other ethnographic works conducted in the same substantive area or using similar protocols. Establish the pervasiveness of these methodologies, remembering always that these boards are at least as worried about university risk management as they are concerned with the welfare of human subjects.

Funding for Ethnographic Projects

As noted in the introduction, the academy has increased pressure for faculty to secure funding for their research. This has historically been a challenge for ethnographers, although it is somewhat less so than in the past. Federal grant agencies have expanded their definition of fundable projects and encouraged qualitative researchers to apply. As noted above, the National Science Foundation (NSF) convened a panel of qualitative experts whom generated a set of standards by which qualitative proposals should be judged. Although NSF is an excellent source of funds for smaller, more humanities-oriented projects, Howard Becker points out that the agency does not provide funding for the most important aspect of an ethnographer's research—time spent in the field—because it does not offer course buyouts.

One important aspect of establishing a research pipeline is identifying a "fundable" track of your research program. Applied skills such as program evaluation are especially useful here, as are substantive specialties that are aligned with federal priority areas—my "money" track is juvenile justice. Federal grant agencies are

increasingly calling for multi-method studies that lend themselves nicely to collaborating with more quantitatively inclined colleagues on a research proposal. Interviews or focus groups are a common supplement to larger-scale studies of secondary data or survey research. In addition to providing resources such as summer funds or course releases, these collaborative opportunities allow young scholars to become established in the world of grant funding, an important experience because it is often said that "it takes a grant to get a grant."

Soon after arriving in a tenure track position, junior scholars should scan their university environment for internal grants. These are often earmarked for early career scholars and are presented as "seed grants" to promote the development of larger or longer-term projects. At this point in the career, junior professors should be (1) publishing the dissertation as a book or series of articles and (2) identifying a shorter-term project that can serve as a "bridge" between the dissertation and the next stages of the research pipeline. Internal grants in amounts of $5000–10,000 are useful in either regard, as they can be used to hire a development editor (for book projects) or fund a graduate student to help with getting a new research project off the ground.

How to Publish: WWJMD?

One of the most challenging aspects of my move from sociology to criminal justice has been the marginalized status of scholarly monographs, or book-length treatments of research. As noted above, ethnographies have traditionally been written as books, largely because the context requires book-length treatment. My mentors, Elijah Anderson and Kathy Edin, were both book writers and thereby allowed me to bypass some standard dissertation conventions in the interest of turning it into book. Imagine my surprise when my mid-tenure letter referred to my plans for a book as "putting all your eggs in one basket!" Senior faculty advised against it, suggested I would get more mileage out of breaking it into articles, and warned that I *might* get tenure if I secured a reputable press and won some book awards. With a big gulp, I kept writing. (And be sure that I take no small amount of satisfaction at having won two national book awards after I left that department).

The most important lesson to take from my experience with books is to figure out the reward structure in your department, optimally before you sign a contract and accept the position. Ask faculty at all levels, the department chair, and the dean where books stand in the tenure and promotion (or merit) process. Ask for written guidelines to see what is codified and how much a book will "count." Look at the CVs of recently tenured professors in that department to see how they did it.

One thing you will probably notice is that tenured book writers also published articles in scholarly journals. As you are working on your dissertation, you will likely want to release one or two chapters as articles, to establish a track record of publications. (Note that university presses will likely be less excited about your proposal if you have already published more than that). The dissertation can also be

a good source of stand-alone pieces on methodology. Moreover, after it is published, material that didn't make it into the book can be rescued from the cutting room floor to comprise complementary substantive articles. As noted in the pipeline section above, having a mix of products and collaborators can help you continue to publish articles even as your ethnographic data collection and manuscript writing/ revision are being carried out.

I have tried, with some success, to publish stand-alone ethnographic pieces in journals. It is a good way to develop a thick skin. I've had some very silly comments by reviewers who complained about small samples or insisted that I create tables even when it wasn't clear what would go inside the cells. The same manuscript has resulted in comments from one reviewer that 20 cases were insufficient to draw conclusions and an editor that commented upon submission: "great data!"

This hints at the first rule of publishing any qualitative work in journals, which is to select your outlet carefully. Review the last 2 years of issues to see how many ethnographic manuscripts they have published. Talk to members of your network about where they have had positive experiences. See if someone has done a meta-analysis of publications in your field to see how many qualitative articles are published by specific journals. In criminology and criminal justice, one study found that rates varied from 0.3% in *Criminal Justice and Behavior* to 37% in *British Journal of Criminology*. (Note that journals published in the UK tend to be less committed to positivism and, as a result, are more likely to accept qualitative research papers). Look at CVs of scholars who do research similar to yours, to get ideas of where they have successfully published their studies. (You'll be stunned at how many journals exist). Think again about the reward structure of your department and what role impact factor, ranking, or disciplinary boundaries play in the tenure and promotion process.

When writing for mainstream American journals, I use the mantra, WWJMD? Or What Would Jody Miller Do? Her articles are models of how to sell qualitative work to largely quantitative audiences. What is her secret? She employs positivistic language (e.g., external validity, reliability) and explains every step of the data collection and analysis. What epistemological framework was employed (e.g., grounded theory)? Did multiple authors collect or analyze data and if so, how were reliable measured established? How specifically did you approach coding and how did the analysis lead to the results? How was bias addressed? What sampling strategy was used? Why should we not be concerned about small numbers of cases? [For this one, I find citing Small's "How Many Cases do I Need?" (2009) useful]. When reading her methods sections, which are rich with references to well-known qualitative methods texts, I imagine she has kept a file of all the critiques ever leveled by scholars who didn't understand qualitative research and used them to preemptively head off reviewers' concerns.

I feel ambivalent about presenting a final technique for speaking directly to quantitative reviewers, which I will call *numericizing* ethnographic research. This involves quantifying aspects of the research design, such as number of trips to the field, length of field notes in words or pages, counts of parent and child nodes used during analysis, or presenting counts of observations, as Alice Goffman (2014) did

when she reported how many times she witnessed police copters flying over the neighborhood or violent interactions between police and residents of the Sixth Street community. On one hand, it can credibly be argued that this strategy reinforces and reproduces inequalities between qualitative and quantitative research by creating expectations by reviewers for quantified findings. Some refer to this technique as "false precision," since it is impossible to say what proportion of possible observations they represent. Nevertheless, I have found that these methods—particularly reporting interrater reliability (which qualitative software will allow one to do)—do allay reviewers' fears.

On the other hand, I was heartened when I read the "methods" section of Matthew Desmond's article on eviction, which was the precursor to his Pulitzer and MacArthur Genius Award-winning book. He says:

> I did not rely on any qualitative data software. Rather, guided by the set of research questions that opened this article, I began listening to hundreds of hours of recorded interactions and poring over thousands of pages of field notes, reading and rereading, until, having become intimate with my data, the observations on which this essay's argument rests emerged and cohered. (2012, p. 1302)

I love this description because it resists the overly empirical methods of description and strikes the right balance between ethnographic method as art and science.

Conclusion

This chapter identified a number of ethnographic traditions that are challenging but worthwhile to keep alive in the context of the modern academy and provided a number of exemplars and strategies for how an early-stage ethnographer can honor its traditions while still navigating the reward structure inside higher education. In short, in order to keep ethnographic traditions alive, *we must keep ourselves alive*, earning tenure and all of the prestige and security that comes with it.

Once we have achieved that stage, however, we must continue to challenge the existing knowledge/power hierarchy that marginalizes ethnographers and our products. We must serve on tenure and promotion (or merit) committees and fight back against the devaluation of ethnographic research. We must serve as funding and journal peer reviewers and eventually, as journal editors, making sure that there are opportunities for qualitative researchers to publish in mainstream journals. We must insist that qualitative and/or field research methods are not only available in our graduate programs but are required courses for all students. We must speak up at defenses, job talks, and colloquia when qualitative research is denigrated or when students are asked to inappropriately apply quantitative techniques (e.g., tables, counting) to their studies.

In pursuing ethnography as our primary mode of inquiry, we are in some ways choosing to occupy a marginalized status within the academy. We may be automatically disqualified from certain academic posts. We will often be ghettoized into

low-impact journals or receive ill-suited demands from reviewers in more main-stream journals. We'll struggle to secure grant funding and suffer wage penalties when our work fails to meet bureaucratic merit standards. We'll fight tougher battles with our institutions' IRBs and often occupy token positions in our organizational units. We'll be asked to do field research in our "free" time and experience fewer firm boundaries between our personal and professional lives.

But we'll also have some advantages that our quantitatively oriented peers do not. We are "on the ground" to document emerging phenomena that haven't yet received coverage in the scholarly literature. Related, we never have to worry that we've been "scooped" in the same way analysts of secondary data sets do. Because of the interpretive nature of ethnography, no two researchers studying the same phenomenon are likely to carry it out the same way or focus on the same elements. Fieldwork forces us out of the ivory tower and into the real world, which can constantly reinvigorate our analytic frames. Because of this and the highly readable nature of ethnography, our work can directly inform practice and policy—in short, to actually be useful in change efforts. Finally—and maybe most importantly—ethnography is fun and creative. Once you are bitten by the fieldwork bug, there's no going back!

References

Anderson, E. (1999). *Code of the street: Decency, violence, and the moral life of the inner city*. New York: WW Norton.

Ansari, A., & Klinenberg, E. (2015). *Modern romance*. London: Penguin Books.

Barrett, C. (2012). *Courting kids: Inside an experimental youth court*. New York: NYU Press.

Berg, M., & Seeber, B. K. (2016). *The Slow professor: Challenging the culture of speed in the academy*. Toronto, ON: University of Toronto Press.

Black, T. (2010). *When a heart turns rock solid: The lives of three Puerto Rican brothers on and off the streets*. New York: Vintage.

Bourgois, P. (1995). *In search of respect: Selling crack in El Barrio*. New York: Columbia University Press.

Clark, K. B. (1965/1989). *Dark ghetto: Dilemmas of social power*. Middletown, CT: Wesleyan University Press.

Contreras, R. (2013). *The stickup kids: Race, drugs, violence, and the American dream*. Berkeley, CA: University of California Press.

Desmond, M. (2012). Disposable ties and the urban poor. *American Journal of Sociology, 117*(5), 1295–1335.

Duneier, M. (1999). *Sidewalk*. New York: Farrar, Straus, and Giroux.

Edin, K., & Nelson, T. J. (2013). *Doing the best I can: Fatherhood in the inner city*. Berkeley, CA: University of California Press.

Ferrell, J. (2006). *Empire of scrounge: Inside the urban underground of dumpster diving, trash picking, and street scavenging*. New York: NYU Press.

Fine, G. A., & Hancock, B. H. (2017). The new ethnographer at work. *Qualitative Research, 17*(2), 260–268.

Goffman, A. (2014). *On the run: Fugitive life in an American city*. Chicago: University of Chicago Press.

Grazian, D. (2008). *On the make: The hustle of urban nightlife*. Chicago: University of Chicago Press.

Kupchik, A. (2012). *Homeroom security: School discipline in an age of fear*. New York: NYU Press.

Lee, J. (2016). *Blowin' up: Rap dreams in South Central*. Chicago: University of Chicago Press.

Leverentz, A. M. (2014). *The ex-prisoner's dilemma: How women negotiate competing narratives of reentry and desistance*. New Brunswick, NJ: Rutgers University Press.

Liebow, E. (1967). *Tally's corner*. Boston: Little, Brown, and Company.

Mayes, L. (2017). *Law enforcement in the age of social media: Examining the organizational image construction of police on Twitter and Facebook*. Unpublished doctoral dissertation, Temple University, Philadelphia, PA.

Small, M. L. (2009). How many cases do I need?' On science and the logic of case selection in field-based research. *Ethnography, 10*(1), 5–38.

Stack, C. B. (1975). *All our kin: Strategies for survival in a Black community*. New York: Basic Books.

Sugie, N. F. (2016). Utilizing smartphones to study disadvantaged and hard-to-reach groups. *Sociological Methods & Research, 47*, 458. Online First.

Criminological Ethnography: Living and Knowing

Jeff Ferrell

I've been a criminological ethnographer for some 30 years now. Over that time, along with conducting various short-term ethnographic projects, I've engaged in four major ethnographic undertakings, each founded in long-term involvement with my situations and subjects of study and each producing a book. Five years as a member of the urban graffiti underground and as a target of the legal "war on graffiti" resulted in the book *Crimes of Style* (Ferrell, 1996). Roughly a decade of riding with radical bicycle activists, playing music with street buskers, going on the air with underground radio broadcasters, and otherwise immersing myself in conflicts over urban life and urban public space produced *Tearing Down the Streets* (Ferrell, 2001). A lifetime of Dumpster diving and trash picking, and in particular a year of living as a full-time Dumpster diver, led to the writing of *Empire of Scrounge* (Ferrell, 2006). The past few years of hanging out around rail yards with hobos and gutter punks, and hopping a few trains with them as well, have produced *Drift: Illicit Mobility and Uncertain Knowledge* (Ferrell, 2018).

"Twenty years of schoolin' and they put you on the day shift," Bob Dylan sang a long time ago. Well, 30 years of criminological ethnography, working the streets and worrying about the police, and they put you in a book titled *Doing Ethnography in Criminology*. More importantly, 30 years of street ethnography and you learn a few lessons, some of which I'll try to outline below—certainly not because they're lessons as to the "right" way to do ethnography, but instead because they're tentative understandings of how to orient yourself to the ethnographic enterprise. Above all, I'll try to communicate the central lesson I've learned: that ethnography really isn't a "method" at all, if by that we mean a fixed set of procedures to be deployed in criminological research. It's more a way of living in and knowing the world.

J. Ferrell (✉)
Department of Sociology and Anthropology, Texas Christian University,
Fort Worth, TX, USA
e-mail: j.ferrell@tcu.edu

The Thing Itself

Over these 30 years of criminological ethnography, I've increasingly come to think that the foundation for ethnography must be phenomenological. I don't mean by this that criminological ethnographers must be versed in phenomenological traditions—though there is much to learn there. Instead I mean that, before ethnography can accomplish anything else, it must begin with what phenomenologists call *the thing itself*. Now generally speaking, we think of education as the ability to understand people, situations, or events as situated within broader structures or patterns. So, as an educated person and an ethnographer, you might decide to investigate a local prison as an embodiment of contemporary trends in mass incarceration. You might choose to conduct an ethnography with homeless people, in part to show how they are increasingly criminalized by urban policing strategies. Or you might decide to study cybercrime to demonstrate how new forms of crime are spawned by new technologies.

In each of these cases, a phenomenologist would tell you—and as an ethnographer, I would tell you—that you're already off to a bad start. A prison may or may not embody contemporary incarceration trends, but before and beyond any of that, it is what it is, the thing itself, a distinct and uniquely configured complex of people, physical structures, smells, sounds, images, and attitudes. A homeless person may well be criminalized by urban policing, but she is never fully explained by it; she is instead a unique person occupying and negotiating a set of subtle, complex, often contradictory circumstances. Cybercrime may be spawned by new technologies, but it can't be subsumed under them; any ethnographic explanation for it must instead account for the distinctive values, actions, situations, and emotions associated with each cybercrime and each group of cybercriminals. To put it bluntly, a phenomenon exists not simply as an example of something else, as a category in a larger schema, or as a manifestation of some broader trend but as the thing itself and on its own terms.

As ethnographers, then, our job is to bring all we are and all we know to bear in our work—but equally so our job is to suspend all we know and much of who we are as we undertake ethnographic work. The phenomenologist Edmund Husserl called this "epoché," or the bracketing of one's assumptions—but again, the precise phenomenological term is less important than the attitude. That attitude must be one of genuine humility, acknowledged ignorance, eagerness to learn, and willingness to engage that which you study on its own terms. Likewise, the point of this attitude isn't to attain some sort of phenomenological purity where we completely lose ourselves and give ourselves over to the phenomenon; no one can suspend all they know and all they are this totally. The point is simply to start with this attitude of epistemic humility, to think it through and keep it in mind, and to embrace an openness to whatever the phenomenon has to offer.

Excuse my language, but in this regard I've come to realize that my first step in any ethnographic exploration of a particular topic is to remind myself: I don't know shit about it. Now maybe I've seen something about the topic in the media. But as

we all know, media reporting is susceptible to unexamined stereotypes, abbreviated presentations, ratings imperatives, and the mass appeal of moral panics—and all the more so for issues of crime, violence, and criminal justice.[1] Maybe I've read some scholarly writing about the topic—but given my ethnographer's sensibility, if that writing is based on a convenience sample survey or mostly made up of theoretical speculation from inside somebody's office, then I tend to think it tells me more about the author than the topic. (As my colleagues and I have shown, numerous juvenile delinquency textbooks misrepresent the most basic facts about graffiti writing—facts that any good ethnographer would know; see Ferrell, Hayward, & Young, 2015, pp. 103–104). OK, then, what about a previously published ethnography of the same topic? Well, yeah, I'd tend to trust this more than the other sources—but even here, this is not and cannot be an ethnography of the particular street gang or prison or cybercriminal group I intend to study, nor can it be situated in the same time and place. So really, there's only one starting point: I don't know shit. Over the past 30 years, I didn't know why graffiti writers wrote graffiti, how trash pickers lived from trash picking, and how train hoppers hopped trains. Humility in place and eyes wide open, all I could do was get started. Otherwise I would make the mistake of the rookie traveler who while still at home confidently plans out all he will want to see of a place he's never been and cannot yet know.

Knowing How to Know

If ethnography is about humility, it's also about epistemology – that is, it's not only a matter of humbly wanting to know about others' worlds but about knowing how to know these worlds. Survey research and other quantitative methodologies differ from ethnographic research in all sorts of ways, but certainly among the most pronounced is what constitutes knowledge of the worlds we wish to understand. Mailing a survey or mining a big data set develops a knowledge of the world that is in many ways shaped by preset categories and procedures; one might even say that this knowledge is confined within the categories crafted by the researcher. This discrete, a priori categorization is in turn necessitated by the ultimate goal of measurement and quantification, with quantification itself understood as a usefully precise way to capture and communicate knowledge.

For ethnographers knowledge develops differently. Undertaking to understand the thing itself on its own terms, ethnographers begin by paying attention to the subtleties of the groups and situations they encounter. To the extent that each group or situation constitutes a distinctive phenomenon, that distinctiveness can often be discovered in the sorts of nuances that non-ethnographers might dismiss as

---

[1] Sometimes non-ethnographic scholars critique ethnographic accounts for being overly romantic, or overly sympathetic, or overly dramatic in their depiction of research subjects—because these scholars are uncritically comparing such accounts to what they think they know of the subject matter from media coverage of it!

unimportant and not worth noticing. A spray paint can, for example, contains a small marble-like ball; when the can is shaken, the movement of the ball helps mix the paint for spraying. In *Crimes of Style* and in other of my ethnographic writings on graffiti, I've described the sound this ball makes. But why bother with such a small detail? Because for a graffiti writer working in the dark night, the sound of the ball differs with the amount of paint remaining in the can and so provides a sort of auditory measurement. Because the ball makes a different sound moving through thick paint than thin, it acts as tentative color identification. And most important, because the noise of a ball rattling in a shaken paint can may well alert others to your illicit presence—and so graffiti writers learn to gently, noiselessly roll the ball in the can to mix the paint, or forego using the ball to mix the paint altogether. A little ball in a disposable can of spray paint may seem a nuance not worth noting— except that it helps define and distinguish the thing itself, in this case nocturnal graffiti writing. So, a pro tip: If you don't know shit to start with, you won't know much more than that early in an ethnography, so carefully pay attention and record every little subtlety you can—you'll likely discover later that some seemingly meaningless detail in fact means more than you could initially imagine.

Subtleties and nuances matter as a form of ethnographic understanding for another reason, too: the phenomenon of *elegant knowledge*. This is a concept I learned from ethnomethodologists—a subset of sociologists who are concerned with how people accomplish the everyday social realities in which they live—and I have to admit that it is far and away my favorite concept as an ethnographer and the one that has long guided my own ethnographic research. Elegant knowledge suggests that, upon close inspection, human activities that might be dismissed as simplistic or uninteresting, or human beings who might be dismissed as unskilled or uneducated, in fact embody constellations of knowledge that are nuanced and sophisticated—and that are elegant as well in their graceful ingenuity. Further, this elegant knowledge is generally a form of situated knowledge, an ability to read, reference, and make sense of particular situations that mostly remain opaque to those outside them (this is similar to the ethnomethodological notion of indexicality). Ever walked by a graffiti mural full of unintelligible markings, for example? Well, for local graffiti writers, the markings are intelligible and in all sorts of ways. Particular colors used, the distinctive hand styles of different graffiti writers, encoded inside jokes and put-downs, and references to historical graffiti styles—all these are read and responded to by those who have the knowledge—and these signs and markings are further understood in the context of the particular setting ("spot") of the mural, the distinctive history of the local subculture, the current degree of police pressure, and the extent of conflict ("beef") between individual graffiti writers or crews. Ever passed a dumpster without much noticing it? Local dumpster divers notice it and know it. They know which stores deposit trash in it and on what schedule, how often neighbors use it to discard which sort of personal items, which security cameras point at it and from what locations, and which police or security guards patrol the alley—and dumpster divers know how to "read" the garbage bags in the dumpster, too, simply by touching them and feeling for valuables. Ever thought it might be fun to hop a freight train? Well, if you'd like to avoid having

your legs amputated by the train wheels or your freedom curtailed by the railroad police, then you'll need elegant knowledge of how to scout the rail yards and from what vantage point, without being seen, how to determine which trains are likely to depart in which direction and when, on what schedule the railroad police patrol the yards' perimeter, which sorts of rail cars provide which sorts of hiding places, and most importantly, how to run alongside a moving train and then jump and hoist yourself up into one of these hiding places without stumbling to your death.

The thing itself is more than it first appears, and to know it is to acquire something of the elegant knowledge that animates the lives of those who occupy it. It is also to retain, perhaps redouble, the humility with which an ethnographer begins; the more one comes to know about train hoppers or trash pickers, about cops or parole officers, and about the dynamics of drug raids or deportation hearings, the more one knows there is to know about them. Such an understanding usefully dispels any elitism that might come from a college degree or a published book, locating ethnographers instead inside a democracy of knowledge where people of all sorts merit investigation as to the sophistication of their understandings. And it reminds us again to value nuance and detail, and the particulars of situations, over the easy abstractions of generalization and grand theory.

In place of broad generalizations and abstract theory, then, ethnographers seek nuanced understandings and situated, elegant knowledge; over time an accumulation of such ethnographic investigations may well allow for some tentative generalizations but only to the extent that they remain rooted in ethnographic particulars. As regards theory, ethnographers seek this, too, and in a similar way. Rather than importing extant theoretical models into their research, or testing their ethnographic findings against external theories, ethnographers attempt to build modest, ethnographically specific theories from the nuanced knowledge that they discover. In a general sense, this is what Glaser and Strauss (1967) meant many years ago when they coined the notion of "grounded theory" – theory with a small "t" that remains rooted in the circumstances of its emergence. In my experience, such theory often emerges from paying attention to the linguistic and experiential categories utilized by those we study. If we listen carefully to police officers discussing a case or graffiti writers discussing a mural, we can often pick up on their categorizations of people, places, and situations—those kinds of suspects, that kind of spray paint, and this sort of situation—and so begin to see a grounded theory of policing or graffiti writing. As C. Wright Mills (1940) first proposed, we can also listen for the ways in which shared "vocabularies of motive" construct and communicate collective perceptions and motivations. Likewise, if we carefully observe situated interactions and behaviors, we can sometimes see categories in action—these sorts of suspects being treated differently than those, this sort of spray paint being kept aside for a more important project, this threatening situation meriting a different response than that one—and so see working theories of social life. A few years ago, Robert Weide and I (Ferrell & Weide, 2010) published the "spot theory" of how graffiti comes to be written in different places, or spots. But it wasn't our theory as academics; it was a grounded theory of graffiti writing that we had learned over many years as graffiti writers and as ethnographers of graffiti.

A final way that ethnographers come to know subjects and situations is perhaps the most powerful—and it's certainly the most dangerous. Early in sociology's history, Max Weber (1978) proposed that a full sociological understanding of the world could not be achieved without *verstehen*—without, that is, an empathic understanding of people and situations that would provide a degree of emotional accuracy while complementing more detached observations. For ethnographers, this means that attentiveness to subjects' shared emotions, even immersion in those emotions, must accompany descriptive ethnographic observations if we are to know fully our subjects and their lives (Ferrell, 1997). To put it bluntly, as ethnographers we have to learn with our eyes and ears, sure, but also with our hearts and our guts. After all, it doesn't do much good to record the physical details of a police chase or a night in the slammer or an illicit train ride if we don't know what such events feel like to those involved in them—if, that is, we get the emotional register wrong. In this sense *verstehen* is an essential component of good ethnography—but that doesn't mean it's easy or safe.

Part of the problem is that copresence alone isn't enough; you can't just haul your own personality and social status into a situation and expect to experience it emotionally in the same way that its inhabitants do. You have to listen attentively for the clues that their vocabularies of motive might offer, but more than that, you necessarily have to confront your own perceptions, your own fears and desires, and your own privilege as part of readying yourself for *verstehen*. That is, you have to ruthlessly examine yourself and your own emotions before you can usefully examine others. Dangerous work indeed—and it gets more dangerous when you realize that a good way to get past yourself and into the emotional worlds of those you study is to embrace the risks and vulnerabilities that they themselves face. This is another of the central ethnographic lessons that I've learned over the years: I can't jettison my own social standing, but I can put it aside for a while by exposing myself to some of the same vulnerabilities that shape the lives of those I study—and by doing so, I can know at least something of the emotions that animate their experience. So, over the years, I've shared graffiti writers' adrenaline-charged experiences of writing graffiti in the middle of the night, drunk or high or both, sometimes confronting gang members or paint huffers and sometimes more or less successfully running from the police. I've busked with other street musicians in the bitter winter cold, fingers poking out of cutaway gloves, playing fast to stay warm, looking out for cops, and nipping from a bottle stashed behind the guitar cases. I've lost myself in the collective excitement of a Critical Mass bicycle ride, blocking lanes of automobile traffic, and hearing the police sirens closing in. I've learned from dumpster divers a whole new sense of the city and its spaces and had my privileged understanding of time radically reoriented toward waiting, aftermaths, and temporal residues. And I've felt the train hopper's thrill of successfully jumping an outbound freight train, ridden the rolling thunder of a fast-moving train, and known the fear of being hunkered down in a rail car, trying to avoid the railroad police.

Notice just how dangerous this emotional knowledge is. To gain it you first have to confront yourself, your fears, and your privileges, and if you can make it through that, you then have to lose yourself in swirling currents of unfamiliar emotions and

physical risk. Because of this, you also have to come to terms with the fact that any good ethnographic project will leave you, the ethnographer, a different person upon its completion. Enlightened, angry, excited, scared, and outraged—you'll bring at least part of the phenomenon and its affiliated emotions back with you. On top of that, this isn't just ethnography we're talking about, it's criminological ethnography, inherently wound up with issues of law, illegality, crime, and enforcement—and as the above examples show, to achieve *verstehen* with one party to the law and not another is to put yourself on one side and not the other, not just intellectually but emotionally, viscerally, in your heart, and in your gut (Root, Ferrell, & Palacios, 2013).

On the Legal Margins

> They say in Harlan County,
>   There is no neutral there.
>   You'll either be a union man
>   Or a thug for J.H. Blair.
>   —Florence Reece, "Which Side Are You On?" (1931)

The intimacy of criminological ethnography—the way in which it necessarily immerses the ethnographer in particular situations, emotions, and ways of knowing—leaves little room for protestations of neutrality or objectivity as regards the law. Within the practice of criminological ethnography, "there is no neutral there," as Florence Reece said of a bitter miners' strike—the emotions are too strong, the situations too seductive, and the consequences too fraught—and so the criminological ethnographer is forced to consider and confront which side she is on.

As is surely obvious by now, I have over the past 30 years consistently put myself on one side: that of the outlaws, the criminals, and the criminalized. By dint of my politics and my personal predilections, I've chosen to immerse myself in groups who occupy the margins and who are the targets of legal enforcement and legal harassment. Partly this is personal. Since I was a little kid, I've been defying one form of authority or another, and a lifetime of such defiance has left me accustomed enough to facing, maybe even embracing, the consequences. Likewise, and no doubt relatedly, I've for as long as I can recall found comfort on the margins and a sense of belonging among the discarded, the downcast, and the down-and-out. But if this is personal, it's political and intellectual as well. Since my college and graduate school training in sociology, I've known enough about moral panics, moral entrepreneurs, media and political dynamics, and differential enforcement to suspect that, more often than not, those constructed as dangerous outlaws aren't what they're said to be—and that knowing and writing about them on their own terms therefore constitute an act of political subversion and correction. Over the years I've likewise learned enough labeling theory and anarchist political theory to suspect that often enough the real problem is not those defined as criminals but instead the inequitable forces that define them as criminal in the first place; in this sense as well, to study the criminalized is to confront power and its consequences.

In every case this ethnographic research has been complemented by careful, critical analysis of legal statutes, law enforcement campaigns, urban spatial dynamics, and the broader political economy of contemporary life; thirty years of ethnography have also been thirty years of interviewing city officials, reading legal documents, compiling media accounts, and looking up property records. I would argue that such research is essential for good ethnography or what some call critical ethnography; nuances of a group's elegant knowledge and shared emotion must be contextualized within larger social and historical moments and within the larger social and legal forces that in some ways shape this knowledge and emotion. But in my case at least, it's notable that this complementary research hasn't been ethnographic; I haven't undertaken to go deep inside the emotions of police officers conducting an anti-graffiti sweep or to master the elegant knowledge that city officials bring to their campaigns against the homeless. Partly this is again a matter of politics and personal predilection—I'll take streets and street people over office buildings and city council meetings every time—and partly it's a matter of focus; as a single, independent, grant-free ethnographer, I can only do so much. Besides, there actually has been something of an ethnographic dimension to my research on the authorities, anyway: the countless times in 30 years of street ethnography that I've been rousted by the cops, chased by the cops, ticketed or arrested by the cops, banned from locations by the cops, run off by private security guards, and otherwise been afforded the opportunity for firsthand research into the dynamics of private property, law, and enforcement.

Ah, good memories—but maybe for you they don't sound good at all, and so you'd prefer to conduct criminological ethnography within the confines of the law. This is certainly appropriate and an approach that has produced many good ethnographies—but it's also an approach that incorporates an alternate set of difficult decisions as to which side you're on. Obeying the law doesn't obliterate it; it only invokes it in a different direction. Sometimes, for example, ethnographers seek permission from legal authorities to conduct street research with criminal populations; but while carrying this sort of get-out-of-jail card may provide legal protection, it *a priori* puts the ethnographer in a distinctly different relationship with the law than that of the criminals under study. Further, it offers the ethnographer two unsavory choices: hide this arrangement from those being studied, thus introducing dishonesty into the ethnography, or have them find out that the ethnographer they're supposed to trust is in fact down with the cops. Other times researchers undertake ethnographies inside criminal justice institutions, or even ethnographies *of* these institutions. Such ethnographies are invaluable to the field of criminology—but to undertake them is inevitably to get caught up in and be made obedient to the regulatory structures that shape such institutions. Rules, regulations, guidelines, statutes, and permission forms abound—and this is not to mention Institutional Review Boards (IRBs). As part of a recent research project, Mark Hamm (2013; Hamm & Spaaij, 2017; see Ferrell et al., 2015, p. 191) set out to interview a handful of high-profile US prisoners convicted of terrorist violence. And he managed to do it, too—all it took was a little more than a year's work to get through the *eight* university, correctional, and Justice Department Institutional Review Boards that stood between

him and the interviews. Twenty years ago, writing on the necessity and inevitability of violating the law while conducting criminological street ethnography, I concluded that "for the dedicated field researcher who seeks to explore criminal subcultures and criminal dynamics, obeying the law may present as much of a problem as breaking it" (Ferrell, 1997, p. 9). Now, 20 years later, I realize that even for ethnographers working within the law, the same problem applies.

An insight offered by labeling theory complicates things further. As labeling theory has taught us, it's not only that we decide whether to break the law or to abide by it—it's also that those with the power to do so often decide *for us* whether we are criminal or conformist. An ethnographer can believe herself to be working within the law—but can then find herself charged with being an accessory to a crime, or discover that a lawsuit has been filed against her, or learn that her field notes have been subpoenaed as part of some related or unrelated investigation (e.g., Scarce, 1994). Criminological ethnographers by necessity stand all too near the fires of crime and enforcement, and they can get burned and burned badly even when they intend not to. Because of this, the utmost caution is required as regards those we study and the information we record about them; even when no legal danger seems apparent, I've learned to be meticulously careful as to what I write down or record, where I store it, and how I incorporate it in published work. Beyond this, I've gotten to the point that, when I give lectures on ethnography, I generally include a warning: Before you engage in criminological ethnography, consider carefully who you are in the world and what you can afford to lose—and just in case things go really badly, maybe have some alternative career options in mind as well.

Autoethnography Is Inevitable

At its most basic, autoethnography denotes the ethnographic exploration of the self. More broadly, and I think more usefully, it references the practice of accounting for oneself as an ethnographic researcher within the larger ethnographic process. Over the past couple of decades, criminological ethnographers have increasingly discussed, theorized, and undertaken autoethnography, and in this sense autoethnography constitutes something of a new development in criminological ethnography (Ferrell, 2012; Jewkes, 2011; Wakeman, 2014). In reality, though, autoethnography has been a part of ethnography all along—and it's been threaded through this chapter all along. In order to understand the thing itself, an ethnographer must first examine and confront her own biases and perceptions, lest they stand in the way of phenomenological appreciation. In seeking *verstehen*, further self-awareness and self-examination are necessary—and to the degree that *verstehen* is attained, the ethnographer's emotions now become entangled with the emotions of those under study. The nature of criminological ethnography in turn forces the ethnographer into uncomfortable confrontations with the law and her own sense of justice and injustice. Autoethnography indeed: the successful ethnographer will by necessity confront and account for her existing self-identity, will find that self-identity changing

during the research process, and will return from research a different person. As regards ethnographic research, autoethnography is inherent and inevitable. And because of this, ethnographers are increasingly aware that we'd best figure out what to do with it and how to develop its research potential while avoiding its narcissistic seductions.

The short answer to these questions is ethnography first, autoethnography second. We live in a contemporary society that fetishizes the individual and "the self," with massive machineries of advertisement and consumption marketing "self-awareness," "self-improvement," and the glories of self-indulgence. The last thing such a society needs—and the last thing contemporary ethnography needs—is one more moment of narcissistic self-involvement. So, if I might paraphrase from earlier, and again apologize for my language, as an ethnographer, I don't give a shit about your or my individual preferences, family background, childhood memories, personal achievements, or personal anxieties. Autoethnography is not autobiography; autoethnography is actually not your story or my story, not about me or you, but about ethnography. As ethnographers, our stories only matter in the context of our ethnographic work and to the extent that such contextualized stories contribute to ethnographic understanding. If we're going to make autoethnography a valuable part of ethnographic research, we must avoid the culturally bred-in tendency to prioritize ourselves and our own experiences and instead remember: ethnography first, autoethnography second.

Now this may sound funny coming from someone who's by turns been appreciated and criticized for writing himself into each of his criminological ethnographies. And it's true; if you read my ethnographic work, you'll regularly read accounts of my experiences and emotions along with those of others (Ferrell, 2012). But here's the key: I do my best to write about myself only when I'm *in situ*—that is, when I'm participating in the shared situations I'm studying, when I'm in the process of learning the elegant knowledge that animates the lives of those I study, or when, through *verstehen*, my emotions seem to have blended with the emotions of those I study. In these moments, to write about myself is to write about membership in shared ethnographic situations and about the process of becoming those I study. Such writing also embodies the humility of the initiate and the power of confession (Ferrell & Hamm, 1998), accounting as it does for the mistakes, misadventures, and dilemmas that emerge as the ethnographer is learning the cultural contours of an alternative social world. It is both autoethnographic and also a sort of sociology of the self: the self as situated in particular social settings and the self as emerging in the processes of new social interactions. I like to think that sometimes I achieve another sort of sociology of the self as well. When I write about how drastically different are people's responses to me as an on-campus university professor versus a disheveled back-alley dumpster diver or how differently local police treat me when I am in the company of graffiti writers, I'm also writing about what sociologists call *status inconsistency*—the often jarring mismatch between one aspect of person's status and another—and a mismatch that can reveal key social dynamics of power and perception.

If nothing else, then, autoethnography seems to provide for a more honest accounting of the ethnographer's role in ethnographic research—an acknowledgment that the ethnographer's own experiences are inevitably part of the research process and therefore an appropriate topic for description and investigation. Along the way, this sort of self-investigation can highlight important dimensions of the people and situations we study, to the extent that the ethnographer's own, often deeply felt experiences and emotions belong both to her and to those she studies. Beyond this, I would argue that autoethnography embodies some important methodological politics. It emphasizes just how different ethnographic methods are from the more conventional, positivistic methods of criminology. Where these more positivist methods seek to erase the researcher from the research process—to achieve, that is, "objectivity" and "replicability" that exist beyond the researcher's presence or identity—autoethnography makes clear that the ethnographer's identity and presence are essential aspects of ethnographic research. Autoethnography in this sense replaces positivist objectivity with informed, situated subjectivity. In doing so, it also positions ethnography more as a way of navigating situations and knowing the world—a form of personal and professional practice, perhaps—and less as a limited set of methodological procedures. I would add that it also confirms, at the level of methodology, the importance of diversity. If the ethnographic self does matter, then the more diverse ethnographers are in terms of gender and sexual identity, ethnic affiliation, and social class, the more eclectic, diverse, and informative the resultant pool of criminological ethnographies will be.

Finally, there's the issue of autoethnographic writing. In my experience, writing ethnography in the first person—putting yourself in the ethnographic picture—creates accounts that are not only more honest but more vivid and engaging than accounts written in a more dispassionate or distanced way. Any story is largely defined by the medium in which it's told, and first-person ethnographic narrative excels at communicating the lived sensuality and interpersonal nuance of ethnographic situations.[2] By way of example, here are two accounts of the same situation, the first version as I wrote it in *Empire of Scrounge*, the second version rewritten to exclude my presence:

> I see ahead on the right one of those big clean-out-the-house trash piles, and two people already digging in it, their half-full grocery cart sitting in the middle of the street. Cycling up, I offer greetings … The woman – older, white, gray hair parted in the middle – strikes up a conversation … Meanwhile, she's already pulling clothes and coats out of the pile and handing them to me … She assures me I should have them, and continues to pull clothes out of the pile for me … [Later] I'm thinking about the old lady who helped me scrounge the

---

[2] If you're interested in developing your ethnographic writing skills, I'd suggest reading novels, short stories, nonfiction reporting, classic ethnographies, and Agee and Evans' (2001 [1939]) *Let Us Now Praise Famous Men*—and avoiding traditional social scientific reports. I'd also encourage you to experiment with forms of writing, toward the goal of reproducing the rhythms of particular groups and particular settings in your writing about them. For me, this has meant experimenting with foreshadowing, vignettes, accounts that unfold beyond the limits of a single chapter, and other narrative devices. In short, think about ways in which you can make your style of writing homologous with its subject. See Clifford and Marcus's (1986) classic collection, *Writing Culture*.

big trash pile, taking the trouble to find clothes for me when she had few enough for herself. I'm thinking about doting grandmothers, sorting through the sale racks at Macy's or the strip mall, pulling out clothes for the grandkids – grandmothers who might or might not be sympathetic to the plight of an old homeless woman, digging in the trash. I'm thinking about my own grandmother, a central Texas farm woman known on occasion to haul home cast-off clothes or stray animals herself, an old woman that's a long time gone and sometimes still around.

While a variety of groups are engaged in street scavenging and trash picking, the group perhaps most typically associated with this activity – homeless individuals, pushing rickety shopping carts piled with scrounged items – is certainly present as well. Among this population of street scavengers are individuals not only marginalized by homelessness, but left especially vulnerable by age and gender; elderly women forced to survive by scrounging embody this problem especially. Yet it would be a mistake to understand these elderly women only as passive victims of social inequality; they are also active agents in engineering their own survival. Moreover, they often scrounge not just to survive, but in order to share with others as part of a larger if scattered community of those living on the streets. (Ferrell, 2012, p. 226).

Last, I'll reproduce a short autoethnographic excerpt from the *Drift* book (Ferrell, 2018, pp. 136–137). Here's the setup: Laid up in the Abilene, Texas, rail yards, a train-hopper buddy and I had just dodged detection while hiding in the "well" of a freight car; we had then made a run through the darkness for a six-pack of beer ("tall boys"), only to discover upon returning our freight train now ready to leave without us. I meant my first-person account of what happened next to embody a sort of narrative *verstehen* – to let the reader feel what I felt, and we felt. Can you feel it?

We grab our packs and beer, run and stumble back across the dark yard, and hop into our well just as the train is getting underway, headed for Sweetwater. Now I have to tell you, the forty miles of arid West Texas wasteland from Abilene to Sweetwater is nobody's idea of a scenic ride or preferred route. Nobody's…unless, that is, you just spent hours stalled in the Abilene rail yard. Unless you recently found yourself pressed to the floor of a railcar's back well, figuring you might be about to be kicked off the train or handed a criminal trespass citation or worse. Unless you thought to yourself, man, just let those boots crunch on by— and they did. Unless you now find yourself with four still-cold tall boys, snug back in your well, and picking up speed. Unless while you're downing those boys and barreling through the night you notice that lightning is beginning to filigree the sky, jumping from cloud to cloud, lighting up the route ahead of the train on all sides, and that big drops of rain are shooting up over the top of the car in front of you and swirling in the back draft behind you, and that the sweet rich smell of that rain hitting the weeds and fields of the countryside is all around you, and that the cool cloud charged air is sweeping by, and you find yourself whooping and hollering into the night all the way into the Sweetwater yards.

At which point the train rolls to a stop—and we're out of beer.

Ethnography Adrift

By way of introducing this chapter, I urged the reader not to take this essay as a list of ethnographic rules, but rather as a tentative orientation to ethnographic research. This isn't false modesty on my part; it is an honest humility, and one that lies at the

heart of ethnographic research. As already seen, ethnography demands of the researcher humility as to what she knows and doesn't know, a humble accounting of her biases and perceptions, and a humble apprenticeship in others' elegant knowledge. But more generally, I would argue that ethnography at its most sophisticated is innately modest in its informality and open in its approach—that it is a disorientation as much as an orientation—and that this untethered uncertainty is in fact its greatest strength.

Over the past 30 years, for example, I haven't selected my topics of ethnographic inquiry so much as they've selected me. *Crimes of Style* emerged from outrage and suspicion; doubting increasingly aggressive media coverage that portrayed graffiti writers as violent vandals, I decided that I had to find out about them for myself and so began the process of finding my way into the graffiti underground. *Tearing Down the Streets* wasn't planned at all but instead came together as I began to realize that my general interest in urban public space had led me deep into a variety of urban subcultures and urban conflicts. *Empire of Scrounge* was at first an afterthought; unemployed after resigning a university professorship, I turned to Dumpster diving, after a while began to take field notes on my experiences, and later still translated all this into a book. Unsurprisingly, I drifted into *Drift*. Sensing that more and more people were being cast adrift by contemporary circumstances, I actually put aside the projects on which I was working to think and write about drift. Then, halfway through this process, a chance encounter led me into the underground of train hoppers and gutter punks, and I drifted off again.

As an ethnographer, this lack of a research agenda—this disorientation toward one's own scholarly research—strikes me as entirely appropriate. Setting an *a priori* ethnographic agenda risks imposing that agenda on the world to be studied and likewise risks constructing the agenda as a filter through which the world is perceived. Agendaless drift on the other hand leaves the ethnographer open to what the world throws up and prepared to research subjects that emerge as particularly timely or important. This drifting disorientation doesn't imply passivity or imprecision on the part of the ethnographer; rather it suggests that the ethnographer's carefully honed skills remain ready for whatever tasks may emerge.

Once engaged in ethnographic research, the value of fluidity and uncertainty remains. Over and over again, I've found that ethnographic research has taken me beyond my habituated sense of time and space and has disoriented and reoriented me in ways that I couldn't have anticipated—and that my ability to flow with these new disorientations, to embrace the discomfort and discombobulation that accompanied them, was essential to the research. Ethnographers sometimes talk about this process in terms of mistakes and serendipity—a willingness to roll with missteps and unplanned encounters, an ability to integrate such phenomena into the research process—and suggest even a research approach that *promotes* missteps and mistakes in the interest of serendipitous insight (Kane, 1998; Root et al., 2013). Similarly, I've come to realize that one of my most valuable ethnographic skills is my ability to hang out and interact with most any sort of person I happen to encounter—to suspend judgment, put aside fear or discomfort, listen carefully for clues as to their world view, and then go where their world view takes me. As I and other

ethnographers can attest, this fluid approach sometimes leads to over-intoxication, missed meals, extremes of atmospheric discomfort, legal entanglements, and not a few moral compromises; but without this openness, I would argue, criminological ethnography is all but impossible.

Earlier I argued that "elegant knowledge" is for me the essential ethnographic concept and the discussion here of openness, drift, and uncertainty leads me to conclude with what I consider the essential ethnographic skill. Jazz musicians know this skill well; so do great documentary photographers (Ferrell, 2018; Lange & Taylor, 1969 [1939]). For them, decades of practice and training—decades of mastering the method that underlies their craft—don't lead to a technical straightjacket that rigidly dictates their endeavors. This rigorous training leads instead to moments of accomplished abandon—moments of letting go, of effortlessly responding to the immediacy of unfolding situations, of playing off the give and take of human interaction. Skilled musicians jamming and improvising among themselves, anticipating and echoing each other's flourishes, or documentary photographers drifting through the streets, ready to deploy their visual expertise as a moment of unanticipated urban drama unfolds—these I would argue are exemplars for good ethnography, their methods supple and fluid and their skills interwoven with instinct and intuition. Good ethnography remains grounded in the expertise of the ethnographer and the particulars of the situation; but it also remains ungrounded and adrift, an unfolding process of informed improvisation. In this it is once again distinct from more positivistic methods and once again less a technical procedure than a way of knowing and living in the world.

References

Agee, J., & Evans, W. (2001 [1939]). *Let us now praise famous men*. Boston: Houghton Mifflin/Mariner.

Clifford, J., & Marcus, G. (Eds.). (1986). *Writing culture: The poetics and politics of ethnography*. Berkeley, CA: University of California Press.

Ferrell, J. (1996). *Crimes of style*. Boston: Northeastern University Press.

Ferrell, J. (1997). Criminological *verstehen*: Inside the immediacy of crime. *Justice Quarterly, 14*(1), 3–23.

Ferrell, J. (2001). *Tearing down the streets*. New York: Palgrave/St. Martin's.

Ferrell, J. (2006). *Empire of scrounge*. New York: New York University Press.

Ferrell, J. (2012). Autoethnography. In D. Gadd, S. Karstedt, & S. Messner (Eds.), *The Sage handbook of criminological research methods* (pp. 218–230). London: Sage.

Ferrell, J. (2018). *Drift: Illicit mobility and uncertain knowledge*. Berkeley, CA: University of California Press.

Ferrell, J., & Hamm, M. S. (Eds.). (1998). *Ethnography at the edge*. Boston: Northeastern University Press.

Ferrell, J., Hayward, K., & Young, J. (2015). *Cultural criminology: An invitation* (2nd ed.). London: Sage.

Ferrell, J., & Weide, R. (2010). Spot theory. *City, 14*(1–2), 48–62.

Glaser, B., & Strauss, A. (1967). *The discovery of grounded theory*. Chicago: Aldine.

Hamm, M. S. (2013). *The spectacular few: Prisoner radicalization and the evolving terrorist threat*. New York: New York University Press.

Hamm, M. S., & Spaaij, R. (2017). *The age of lone wolf terrorism*. New York: Columbia University Press.

Jewkes, Y. (2011). Autoethnography and emotion as intellectual resources. *Qualitative Inquiry, 18*(1), 63–75.

Kane, S. (1998). Reversing the ethnographic gaze. In J. Ferrell & M. S. Hamm (Eds.), *Ethnography at the edge* (pp. 132–145). Boston: Northeastern University Press.

Lange, D., & Taylor, P. (1969 [1939]). *An American exodus*. New Haven, CT: Yale University Press.

Mills, C. W. (1940). Situated actions and vocabularies of motive. *American Sociological Review, 5*(6), 904–913.

Root, C., Ferrell, J., & Palacios, W. (2013). Brutal serendipity: Criminological *verstehen* and victimization. *Critical Criminology, 21*(2), 141–155.

Scarce, R. (1994). (No) trial (but) tribulations: When courts and ethnography collide. *Journal of Contemporary Ethnography, 23*(2), 123–149.

Wakeman, S. (2014). Fieldwork, biography, and emotion: Doing criminological autoethnography. *British Journal of Criminology, 54*(5), 705–721.

Weber, M. (1978). *Economy and society*. Berkeley, CA: University of California Press.

A Twitch or a Blink, an Ethnographer's Path to Understanding Culture: A Lecture

Mark S. Fleisher

Welcome

This course is entitled Ethnography. I can teach you field research methods, data collection techniques, and ways to analyze observational and narrative data. I can teach you how to display analyzed data. I can show you how to integrate narratives into descriptive scenes of your study site. I cannot teach you how to interpret what people do and what people say about what they did. Interpretation is the hard part. I cannot teach how to express your ethnographic understanding of the cultures you study. That you will have to struggle with yourselves. I'll help you and give you pointers I've learned.

On Ethnography: What We Know, How We Know It

Anthropologists ask questions about cultural behavior. Ethnography allows anthropologists to answer cultural questions. We gather data using two data collection techniques: participant observation and observing participation. We gather cultural data, say, on whale hunting by questioning Nootka whale hunters on the Pacific coast of Vancouver Island, British Columbia. You can sit in a Nootkan village asking whale hunters questions, assuming you can speak their Nootkan language or they can speak English (participant observation). Or, you can climb into dugout canoe 14 feet long and join in as a paddler in a whale hunt in 20 miles offshore (observing participation). If you've done your ethnography well, you can explain whale hunting to the satisfaction of Nootkan whalers. If you want a broader understanding of the social, economic, and ritual role of whale hunting in Nootkan

M. S. Fleisher (✉)
Jack, Joseph and Morton Mandel School of Applied Social Sciences,
Case Western Reserve University, Cleveland, OH, USA
e-mail: msf10@case.edu

culture, you'll need to hone your skills in descriptive linguistics. Ethnolinguistic methods can extract cultural knowledge embedded in the Nootkan language (Fleisher, 1984). Sociolinguistic analysis can describe Nootkan whalers' styles of speaking about whale hunting. You can read an example of an excellent descriptive ethnography in Margaret Lantis's 1938 article, The Alaskan Whale Cult and Its Affinities (1938).

Ethnography: Observing, Listening, and Participating

I've enjoyed ethnography. But: ethnography can be frustrating and difficult, especially if we must watch people struggle and suffer. Be warned: Full immersion in a culture outside your own can require months or years of closeness between you and the people you study. That closeness can become as depressing over time as it was exciting at first. My ethnography has leaned toward the depressing side of social life and fits best in the humanities.

I've used both ethnographic techniques. I was an observing participant working as a correctional worker in a federal penitentiary (Fleisher, 1989); a participant observer on city streets among homeless men and women, gang members, and active street criminals (Fleisher, 1995); a participant observer among adolescent males and females comprising a mixed-race, mixed ethnicity coed drug gang (Fleisher, 1998); and a participant observer in poor black community in Central Illinois community documenting everyday social life and collecting social network data (Fleisher, 2015a). If you want to dig in and look closely at the many personalities of ethnography, you'll enjoy reading a sociological and anthropological look at urban ethnography in Suttles' 1976 essay, Situational and Normative Accounts.

I cringe when researchers interview people off campus and call it ethnography. I led a research team into 30 high-security prisons in 10 states. We gathered thousands of pages of interview data on prison sexual violence from male and female inmates (Fleisher, 1989). Was that research challenging and enlightening? Absolutely. Was it ethnography? Absolutely not. But it wouldn't be ethnography unless I played the role of a prison inmate. If I did that, a month inside would be sufficient time to experience inmate social life and observe inmate social dynamics. The federal agency funding the research poo-pooed my idea. Agency officials thought I was a lunatic.

If you want to be an ethnographer, you must be willing to go places and settle into local life. There are no shortcuts. If you ask people questions using Skype or FaceTime, you cannot call it ethnography. FaceTime can get you face to face with missionaries in New Guinea Highlands a lot faster and cheaper than flying. They'll probably answer your questions about indigenous peoples' lifestyle adjustments after contact with the outside world. Maybe, if you can speak one of the New Guinea Highlands languages, you can get an interview with a Papuan. Is that interesting? Absolutely. Is it ethnography? Absolutely not. If you do want to do ethnography there, you'll need to learn one of the 800 or so Papuan languages. Before you fly to

New Guinea, you should read Pike's 1964 article, The Phonology of New Guinea Highlands Languages. After you pour over the complexities of those languages, doing ethnography at Whole Foods will look like a good option.

If you want to do ethnography, there's dark side we cannot control. Our research stands ready to be critiqued harshly by scholars who assert that ethnographic research findings are anecdotal, that is, fun stories about real people but not social science. The manuscript of my book *Beggars and Thieves* (1995) was reviewed by a scholar selected by the editor of a major university press. Back in those days, sad news came in the mail. The review was awful. The reviewer attacked my research, calling it nonscientific garbage. He couldn't believe that a major university press would even consider publishing it. That book went on to an award from the Society of Applied Anthropology, and now, 23 years after publication, folks still buy it, and academics still cite it.

Here's the problem. Some researchers believe that data collected with face-to-face interviews amount to no more than subjective blather; real social science, these critics argue, must be collected with survey instruments, where respondents fill in blanks and check boxes and then those fill-in responses and checked boxes are transformed into statistical findings. We can do that, too, with interview data. My prison sexual violence research (Fleisher, 2009) findings were displayed with histograms, charts, tables, circle graphs, and trend lines. The analysis necessary to generate those data displays helped clarify my thinking about the narrative data's dominant cultural themes. Statistics invoke reality and illustrate for our readers the real world we studied. If you want to become a twenty-first-century ethnographer, don't forget your statistics classes.

I prefer subjective, face-to-face research. Street criminals told me stories about selling cocaine and heroin. I've watched them do it. Critics of ethnography miss the kick of watching drug sellers dish drugs, listening to their rap, and learning what they do with their money. I've watched some heroin sellers use their cash to buy rock cocaine. I knew a heroin dealer who smoked rock cocaine, just to try it out, he said. In front of my eyes, he deteriorated physically and mentally and fell deeper into addiction. We can do both styles of research: ask people to fill out surveys, check boxes, and fill in the blanks and then do ethnography and watch the surveys come alive.

We are cultural anthropologists, ethnographers. The best countermeasure to a harsh critique of what we do comes when we have a strong methodology and a sound theoretical basis for our interpretation of behavior. Ethnography gets personal. You must think carefully about your research and how it affects you, the human side of you. It saddened me watching a guy I knew, a heroin seller or not, deteriorate smoking rock cocaine. When we come face to face with folks we're studying, research gets complex. Before you know it, your life gets mixed into their lives. Then, you can lose your focus on research. It happened to me. You can read my description of the effects that correctional culture had on me in a federal penitentiary (see Fleisher, 1989, chapter, Fieldwork, pp. 110–112).

Hanging out, laughing and joking with street hustlers, and listening to imprisoned convicted criminals recount adventures in folkloric tales of crime can be a

hoot. A prison inmate told me if I ever found myself standing in the street and needed to shoot at an oncoming police car, I should use 45 caliber handgun. Its bullets, he said, would pierce the car's windshield. A 38 will bounce off it. A bank robber said if I robbed a bank, I must carry a bag filled with ice cubes. Dye packs stuck into thick stacks of cash explode if they get too warm. Quickly put the money on ice. I wouldn't trust their advice on shooting at cop cars or bank robbery. They were both doing very long prison sentences. But our role as ethnographers requires that we go beyond listening to folkloric tales. Our ethnographic objective calls for analyzing culture. That requires gathering language data.

Language and Worldview: An Ethnographer's Way of Knowing

Language is an ethnographer's gold mine. I hope you've read or will soon read Edward Sapir's essay on the Sapir-Whorf (SW) hypothesis (Sapir, 1927, 1929). The SW hypothesis argues (in this sense, argue means to advance a point of view) that languages are the dominant source of cultural knowledge. It then asserts that language influences speakers' thought. American English speakers' perceptions of events depend in part on the passage of time. English speakers use tenses—the past, the present, and the future—that locate action in a continuum of time parsed into units. Weather forecasts require tenses. A blizzard is dumping snow on Chicago (now); the blizzard will hit New York City (future); the blizzard was a terrible snowstorm (past).

The Nootkan language, Hesquiat, was spoken by whale hunters on the west coast of Vancouver Island, British Columbia. I stayed on the Hesquiat reserve for a summer gathering linguistic data on kinship and family terminology and terms describing Hesquiat social structure (Fleisher, 1984). Hesquiat kinship has the term orphan. Its literal meaning is a person whose parents were killed by supernatural means. That single kinship term can lead an ethnographer to otherwise hidden domains of culture.

In the last 1970s and early 1980s, I taught a course, Language and Culture, at the Washington State Penitentiary, Walla Walla, Washington. My students were maximum-security male inmates serving long prison terms. One night on my drive home, I had a thought: inmates who spend decades behind prison walls must have cultural measures of time different from mine (Fleisher, 1972). Free world culture defines units of time and adds value to those units. We move through college and graduate school, semester by semester and finals week by finals week, until we get the prize, graduation.

Criminal courts sentence prisoners to a specific number of months in prison. If an inmate was sentenced to 36 months, weeks, days, and hours, as we think of these units of time, have little value. In men's prisons, inmates Do Time, as if time had been rolled out in front of them, and they must move along a time path. Prison cul-

ture adds personal qualities to inmates' time. Some inmates do Dear John time, the time when they feel emotionally upset when their wife divorces them or girlfriend stops visiting. When inmates abide by the rules and move along the time path without problems, they Do Easy Time. If an inmate violates prison rules, selling smuggled drugs, for example, he would be assigned to disciplinary segregation. There, locked down twenty-four hours a day, he's Doing Hard Time. An inmate who has a year or two remaining on his sentence calls himself a short-timer.

Male and female prisoners use a value-laden term, programming, for the time they spend doing constructive activities. An often-heard verbal interaction between a prison staffer and an inmate:

Staffer: "How you programming?"
Inmate: "Programming good."

Programming can last a week or a month or a year.

Participant observation prescribes our role as researchers: We listen to people speak and watch their behavior. But, we must do more than that. If we want to understand cultural behavior, we must dig deeply into the languages people speak. The methodology we call ethno-linguistics gives us the tools we need to reveal cultural knowledge embedded in language. My ethnographies have incorporated the speech of homeless folk, prison inmates, drug dealers, and teenaged adolescents in gangs as a mean to clarify how they think and how they express emotions. My ethnographies describe how they see the world, not how I think they see the world. If I wanted to do that, I could have done it in my office.

Ethnographic research can obtain all the data survey researchers gather and more: We are engaged in people's lives. Our research describes cultural behavior, namely, the quality of interpersonal relationships, actors' affective responses to daily interactions, ethnographers' empathy toward actors, and how actors respond to ethnographers. If we do our job well, we'll begin to see the world as the people we study do. We can learn the way people explain and describe the world around them, if we ask the right questions.

Worldview: Culture Analysis Requires Cultural Data

Ethnography requires a systematic methodology. We begin with ethnographic fieldwork. We hang out, but we must know why we hang out where we do, when it's best to do it, and what we hope to learn by hanging out watching and listening. And eventually we must ask questions, but not just any questions we think are interesting. We must ask culturally valid ethnographic questions. A culturally valid question makes intuitive sense to the people we study. I wouldn't ask a Nootkan whaler his technique for planting corn.

There have been decades of studies on prison sex and sexual violence. Few researchers' published methodologies describe listening to prisoners talk about their sexual behavior before they went to prison, how their sex lives changed inside prison, and then how they, the researchers, used inmates' worldview in writing

research questions. I learned culturally valid questions about prison sex and sexual violence by interviewing former prison inmates.

The content of questions we ask and the way we ask questions affects informants' (these are people we choose to answer our questions) willingness to tell us what we need to know. Long ago, I learned that sounding like a researcher bored my informants. They had been arrested, interrogated, investigated, and convicted of crime. Police interrogated them when they were delinquents in their budding criminal stage. Parole officers issued orders and threatened them with a return to prison if they broke the rules. Psychologists pried into criminals' early lives trying to dig out parental abuse they didn't want to remember, let alone talk about. Wardens told them to obey the rules, or else. I didn't want to sound like an employee of the criminal justice system.

When I prepared to do a study of prison sex and sexual violence, I needed culturally relevant questions. Yes, I had worked in a federal penitentiary and watched and listened to inmates for 14 months. That experience did not prepare me to write relevant cultural questions about inmate's social life and sexual behavior. Of course, based on my experience, I could have written dozens of questions. But those questions would have been grounded in my experiences. I knew too that years of interrogations gave inmates an arsenal of answers to all sorts of questions. I needed questions grounded in inmates' worldview.

Rock had done more than 10 years in prison. He asked 14 men and women to help me. Each of them had done more than 120 months inside prison. That was sufficient time, I thought, to have been exposed to inmates' and staff members' reports of sociosexual situations. They were my experts on prison sex. My first question to them was: "What questions would you ask if you were asked to study prison sex life and sexual violence?" Rock said he needed only one question: "Ask them [inmates]," he said, "how you get along with your cellie [cell mate]?" The panel of prison sex experts nodded their agreement. After more than a year of first-hand work experience in a penitentiary, I had no idea why Rock would use that question.

I also asked my experts how to ask questions. How you ask a professor for an extension on writing a paper sounds different from how you'll explain driving 20 miles an hour over the speed limit, when a cop pulls you over. Your vocabulary, pace of speaking, intonation, and body language will be different. You might argue with your professor when he denies your paper extension request. You should not, if you're smart, argue with a cop.

Rock and his friends agreed: If I wanted to get thoughtful answers, I should not ask questions sounding like a researcher. When my experts gave me questions to ask (here's the theory), those questions were based on their collective storehouse of knowledge of prison life and prison sex. Rock's question was predicated on his worldview, his body of knowledge learned through many years of living in prison. Informants' knowledge was accessible to me through their speech. I wrote dozens of interview questions predicated on the way my experts thought about and spoke about prison sexual behavior. My questions used their vocabulary and expressions. I asked the questions in a speech style unlike a college professor, a lawyer, a judge,

a cop, or a parole office. Be aware of the way you ask questions. It takes practice to find the best ways to ask questions when talking to your informants.

After interviewing 564 inmates, men and women, I understood the message Rock's question conveyed: If I want to know about prison sex, I should not begin by asking questions about sex. Rather, in a study of prison sex, I must begin by asking inmates about personal relationships. Sex comes later or not at all. If you want to learn about the ethnolinguistic method I used to dig into 564 inmates' worldview, read Fleisher (2015b), The Culture of Violent Behavior: Language, Culture, and Worldview of Prison Rape.

Culture: Collecting Ethnographic Data

The study of culture belongs to anthropology. To understand the gripping power of human culture and its influence on you, you must leave your comfort zone. My graduate school advisor told me I had to leave the country. I did. But you don't have to. If you grew up in a rural community, spend a summer mentoring kids in an inner-city, impoverished black community. That experience will disturb your cultural complaisance. If you grew up in an inner city, find a job on a rural Wyoming ranch. I guarantee you'll feel culture's impact when you awaken to find yourself far away from familiar surroundings, listening to local folks speaking dialects of English you've never heard. Learning American English dialects will be easier than learning a Papuan language, but you will need a thorough introduction to American English dialects and socio-cultural dimensions of dialect differences. I suggest you read Wolfram's 2004 essay, Social Varieties of American English.

Flying to Paris, France, or Amsterdam, spending a week at a Hilton in the Netherlands, enjoying fine cuisine, visiting museums, and then flying home do not count in the education of anthropologists leaving their culture behind and struggling to adjust to a different culture. International hotel chains transport your comfortable psychological home in western culture to places you want to visit. Flying from eastern Washington to Seattle, to Tokyo, to Hong Kong, I landed in Jakarta, Java, Indonesia, about eighteen hours later. I spent the night in an international hotel. If I had been dumped into that room without knowing where I was, I'd have guessed I was in Seattle or Chicago. When I awoke, I looked outside. I saw families living in makeshift huts and children playing yards away from a stream of raw sewage. That wasn't home.

I learned ethnographic research techniques outside the United States. Lessons learned there enabled my ethnographic field research here, at home, in America. I was an anthropology graduate student in a field school in central Mexico, among the Otomi people. At home one day, a few days later, I was in a tiny hamlet in the Central Valley of Mexico, among Otomi families living in cactus-walled huts, men hammering hard enough at rock hard dirt to open a spot where they dropped in corn seeds, and mothers raising their children in their culture's way.

My first night there, I slept on rusty sagging metal cot, hoping scorpions wouldn't crawl into my sleeping bag. I awoke praying that somewhere nearby I'd find a

shower. My prayers went unanswered. It was a shock to find myself in a hot, dusty desert village. No clean water. No shower. The toilet, a hole dug about six feet deep, was covered by thin boards. I prayed those boards would not break. No ice in the local *tienda*. The beer was warm. Early on, I prayed a lot. Day one: Life couldn't have been worse. Day four: Warm beer tasted great.

One of our assignments had us describe a cultural process. There were 15 graduate students; half described how tortillas were made and the other half *pulque*, a fermented beverage made from the sap of the agave cactus. That was a simple assignment basic to ethnographic fieldwork. Decades later, I relied on learning how to write descriptions of culture processes when I watched gang members cooking powder cocaine into crack (rock cocaine).

Many decades later, back in the United States, I spent 6 years doing three different ethnographic projects for federal agencies in a black neighborhood local folks called the North End (Fleisher, 2015a). There, I found showers and cold beer. I hadn't yet thought through how I'd collect the data I needed. I didn't know anyone there. I got lucky. In a few days, I met Burpee, a man in his early 50s who, a few months before we met, was released from prison the sixth time. Over the next 6 years, Burpee introduced me to families and young, middle-aged, and elder gang members whom I need to do my research. Years passed, I meet and befriended dozens of North End residents. The North End felt like home.

When we first met, Burpee didn't like me. I was asked by the producer of a local public television station to join a panel of middle-aged former gang members discussing gang intervention and prevention. I asked the producer to introduce me as a cultural anthropologist doing gang research. Instead he introduced me as a gang expert. I watched the men across the stage. I felt Burpee's anger. He was a dyed in the wool, certified gangster who survived on Chicago streets since he was a teenager and leader of a gang called The Burpees. That night, I knew he was playing the role of a mean old man and former gangster. When the ice melted, we chatted easily. He said I was the first white man he'd ever known who didn't want to buy his cocaine.

Burpee was my key informant. He was the key, the access point, to a storehouse of information. Enjoying his company; sharing meals; meeting his baby mama; holding Crystal, his newborn daughter; and spending lots of time hanging out brought us together. We were close. Even my son, Aaron, a high school student, got into the act. Burpee needed a bike, so he could stealthily creep up on youth drug sellers late at night. Aaron gave Burpee his bike. He and I visited folks' homes on holidays. Aaron insisted that we buy kids Christmas gifts. Months, then years passed, the thought that I was doing ethnography faded away. Neighborhood folks knew me, invited into their homes to share meals and watch television. Some were willing to share a joint with me. That I refused. Violating the state's criminal code while doing federally funded research could have terminated my research, my career, and my freedom. Truth: I enjoyed hanging out with Burpee and his neighbors far more than mingling with faculty colleagues.

The lessons learned among the Otomi came in handed, again. One of my favorite warm weather scenes was watching teenaged and adult women gather in the center green space of a public housing project where they styled one another's hair prepar-

ing for a weekend of get-togethers. They called that process "getting ready." Music blasted, joints were passed around, women and girls laughed, and stories were told over hours on Friday mornings. I watched and listened. I snuck in questions gathering terms for techniques fundamental to doing hairstyles. I laughed with folks who laughed at my bald head. Until that day, I had no idea the word weave referred to hairdos. When I first started watching them get ready, I asked what they were getting ready for. They just stared at me. I should have known. Eventually I did.

Blinks and Twitches: Interpretation of Culture

In Clifford Geertz' 1973 chapter, Thick Description: Toward an Interpretive Theory of Culture (p. 312), you'll read an explanation of a "thick description" as a threshold in cultural understanding. The term thick description does not refer to detailed descriptions, but detailed descriptions are necessary to describe the referent of a thick description. Geertz describes the difference between an involuntary nervous eye twitch and a deliberate blink, as a cultural sign with multiple meanings. When we've done good ethnography, we know the difference between a twitch and blink. Then, we must become skilled at discerning the messages blinks convey in particular social setting.

Early in my career, I shared office space with Margaret Mead in Columbia University's anthropology department. A large basement room atop the building's boiler had been partitioned into two spaces. Off the hall, a door opened into my office space, and then a second door in the partition wall opened into Mead's space. My space had a tiny window opening, which allowed a view of a parking lot guard's boots. I admit it. I was terrified sitting at my desk so close to Professor Mead. "What if she hears me say something really stupid?" The thought of Margaret Mead thinking of me as a bumbling new Ph.D. made me nauseous.

Mead arrived in early afternoon twice a week. Preceding her came an entourage, folks carrying her bags, books, bundles, and assorted stuff she required. Soon after their arrival, Mead walked in holding onto her staff, a walking stick. Upon her arrival, my personal ritual had me standing, offering a greeting in the manner deserving of a founder of American anthropology. I felt like I did when I was a Bar Mitzvah boy frightened by the Rabbi. During her office hours, students came in and sat in chairs lining the wall under the tiny window. One afternoon she thought her students were speaking loudly, disturbing me. I watched her stand up, walk slowly around her desk and through the door joining our offices. She stopped. I still recall the tone of her voice reprimanding her students. Then, she turned toward me and apologized for her students' noise. Back into her office she walked. When she was out of sight, I swiveled in my desk chair toward the students, smiled and nodded, acknowledging their terror and comforting them with a smile.

One several occasions, while I was standing, looking at books on my shelf, she came out and wanted to chat. (I was embarrassed to ask her to autograph her books. I still regret it.) As she talked she moved closer and closer to me, until we were face

to face separated by a distance reserved for a mom consoling her children. My mother didn't get that close when we talked. That narrow space separating us was disconcerting, I admit, but her involuntary tongue thrust at such close range caught me by surprise. Surprise, that's the wrong word. I was stunned, shocked. When my mind cleared, I realized her thrusting tongue was involuntary. It wasn't her unspoken way of saying, "piss off bud, you ignoramus."

That incident with Professor Mead brings us back to Geertz's article on thick description and his distinction between a twitch and a blink. Mead's tongue thrust was a twitch. She never blinked at me. In loud crowded fraternity parties, when someone winks at someone across a room, a clear message has been transmitted. Elle and Lucy, my little granddaughters, had full command of the cultural transmission of information in nonverbal messages, blinks, well before preschool. Elle knew when she stuck out her tongue at a friend, its intent was to tease her friend or show her friend she was angry at her. When Lucy stuck out her tongue at grandpa, the message was clear: "Chase me, Old Man."

Ethnography and Cultural Semantics

There's a critical difference between an ethnographer's understanding of a culture and survey researchers' interpretation of their data analysis. When ethnographers understand their culture, they know the meaning of a blink, its unspoken cultural semantics, the way a new mother knows the meaning of a newborn's expressions. New mothers seem to have coded into their DNA a paradigm of interpretations of a newborn's crying: Is it a call for food, or desire to be comforted, or time for a new diaper? Infants' facial expressions, wiggles, sniffles, whimpers, coos, giggles, and smiles, send a mom a message she understands.

When ethnographers leave their study community behind, if they did their job well, they know the difference between a twitch and a blink and know the meanings of blinks across social contexts. If we don't know a blink from twitch, we're not ethnographers. We're tourists.

Ethnography's Key Ingredients

My ethnographies are personal stories of real lives. Three elements are woven into my ethnographic understanding of people I studied: (1) social reciprocity, (2) empathy, and (3) intimacy. Social reciprocity refers to finding one's role in the community and joining in. Empathy just happens, but we must be aware of it. It means a personal attachment to the folks we study. If I can feel what teenagers on the street feel, I can write thick descriptions and interpretations of their lives. Intimacy means becoming a member of the community, seeing what outsiders cannot, participating in personal matters, and fully engaging in local life.

Sylvia, a 16-year-old, and her mother, Mama, stayed together in public housing on the North End. Together they watched over and cared for Sylvia's energetic 4-year-old son, Damien. Mama smoked cigarettes, one after another, all day. Selling dimes ($10) and dubs ($20) of weed out of the back door paid for food, clothes, and cigarettes. Mama's gravelly, mucosy, incessant coughs made me wince. Then the inevitable happened. Late fall, I walked into Sylvia's apartment. She was distraught. Mama was coughing and choking on bloody mucus. Mama didn't want to go but Sylvia insisted on taking her to a nearby ER. I drove them. I walked them into the ER, helping Sylvia hold up mama. Sylvia called a friend. Soon they appeared. I walked away. Days passed. Mama rested in a hospital bed; friends drove Sylvia back and forth. A few more days passed, her friends drifted away. Mama was near death. Sylvia knew it. Mama died. Sylvia didn't weep. Theirs had been a tumultuous relationship. When Sylvia was Damien's age, mama's abuse kicked off. Screaming, smacks across Sylvia's face eventually led to mama swinging a baseball bat, the bat hidden behind the kitchen door, a deterrent to home invasion. Sylvia knocked unconscious laying on the kitchen floor unattended. She told me about the bat incident standing in her kitchen. She pointed down at the spot where she fell.

Sylvia didn't have enough money to pay the cost of a mortician's services or buy a headstone or buy a burial spot or pay for cremation. Cremation, Sylvia thought, was an abhorrent practice. She asked me how she could get cash enough quickly and legally. It would have taken too long to pull together enough cash selling dimes and dubs. I told her of a similar incident that happened in Kansas City. A teenage boy I thought was a wonderful kid was shot in the head. Police found him sitting behind the steering wheel of his car, a hole in his right palm, a protective gesture. His mother was cash poor. She reached out to her friends. Her son's body lay refrigerated for weeks. I suggested that Sylvia work her friendship network, asking each friend for cash, until she had sufficient funds to bury mom.

My father died when I was a few years younger than Sylvia. She didn't know that. I understood her grief, though she tried to hide it behind her tough-girl persona. The intimacy of grief shared by adults who had lost a parent early in their life linked us, if only for a moment. She looked at me. There were no words. Eye contact was sufficient. Blink.

Even if I hadn't written books about my experiences in prisons and on the streets, hanging out among drug dealers, addicts, homeless folks, ex-convicts, and gang members, the experiences alone were a form of personal expression I found richly rewarding. I put myself in the middle of human dramas scripted by culture. Infant mortality among the Otomi was sky high. Seeing tiny burials, small piles of dirt covered by large stones, was a frightening sight for me; I had a baby daughter at home.

On the North End, I watched cultural adaptions of coping with scarce resources I saw in Mexican public markets. The Otomi sold handmade products. Some North End residents supplemented minimum wages, or no wages, with nickels, dimes, and dubs. Young mothers used that cash to pay their rent and buy food, diapers, and baby clothes. State criminal codes listed marijuana sales as illegal. On the North End, selling weed didn't violate local cultural rules.

Cultures have their scripts. Homeless men and women sharing a bottle of cheap enriched wine behind a dumpster were actors in a real-life drama, speaking and behaving in ways prescribed by their culture. My obligation as an ethnographer required me to learn and experience diverse ways of life. These experiences enabled me to describe those ways of life so readers who had never shared a bottle of enriched wine with homeless men and women understood the causes and consequences of homelessness, rather than rush to judgment and condemn them as lazy, shiftless, beggars, thieves, and alcoholics.

Ethnography: A Personal Challenge

Years after I wrote the *Dead End Kids*, I reread it. I was alarmed at my own words. How could I have done objective research when I was immersed in the painful lives of episodically homeless teenagers, whom the police called a violent drug gang? Spoken language is a personal form of expression. When it's used as the medium of research, then research cannot be objective, even if language has been reduced to script on surveys. I've conveyed in my books personal, intimate expressions of people lives. When I saw adolescents alone and lonely, pacing up and down city streets, sleeping wherever they found a safe spot, and injuring one another as adults injured them, I felt badly. I needed and wanted to reach out and help them. Helping the adolescents described in *Dead End Kids* wasn't something I did so I could write about it. I did it because I wanted to help them as I helped my children. I needed to help them, whether I would or wouldn't eventually write about them and our relationship. In a process psychologists label transference, I projected onto Fremont adolescents the loneliness I felt when I was an early teenager after my father died. I stayed away from home and spent way too much time on the streets in Manhattan, sleeping wherever I found a safe spot— the subway or hidden staircases at the Port of Authority bus terminal. I wandered around neighborhoods in lower Manhattan I knew nothing about. I drank too much beer. A few years later, I was in serious trouble with cops in my hometown. The longer I stayed among the Fremont Hustlers, my feeling awful about their lives intensified. I knew, or I thought I knew, how lost they felt. I felt it, too. I made a bad emotional situation worse by my own hand. As months passed, from June 1985 when I started my Fremont Hustlers research to February 1987 when I ended it, I felt badly hanging around Fremont instead of staying at home with my children. Their parents had neglected them. I felt I neglected my son and daughter.

Fremont teenagers drugged themselves into unconsciousness, a loss of self-control, where their emotional pain morphed into anger and violence, striking out thoughtlessly at a world they couldn't control. I watched them hit and then ignore their own children who were hungry, unbathed, and starved for nutrition and affection. I watched boyfriends slap and punch their girlfriends. I couldn't stop it. Someone had to. I called a close friend and told him I wanted to quit the research. I told him what I'm telling you now. He said, you have a choice: quit and walk away

in which case no one will know what you've seen or stick it out and write a story about their lives that's engaging and describes the conditions of their lives. No one will know about it, he said, if you don't write about it.

I tried to help myself understand the effect of watching child abuse, intimate partner abuse, parental neglect, and adolescents who went day to day without the warmth of adults. I emptied my emotions, feeling, and frustration in an essay: Counter-transference and Compassion Fatigue in Doing Gang Ethnography (Fleisher, 2000). My friend also said, you have another choice. You can write an academic book that 100 people read or write a good story about the lives of those adolescents that a 1000 people read. I took my friend's advice. I wrote *Dead End Kids*, a story read by thousands. I didn't have to spend a year writing that book. I didn't need to publish a third book. I had tenure. Professional advancement was not my objective. Writing about Fremont adolescents helped me express how helpless I felt watching damaged teenagers inflicting injury on one another. That book expresses my anger that no one in social services cared enough to reach out to help Fremont adolescents.

Years after I drove away from Kansas City the final time, a prosecutor there called me. She asked if I would testify in the prosecution of the mother of two pre-school girls and her boyfriend. Soon after I departed the neighborhood, the girls' mother and boyfriend (see Fleisher, 1998, Chapter, Misery) used the girls as sex slaves. I asked the prosecutor why social services didn't respond to calls reporting child abuse and neglect. Social workers, she said, claimed it was too dangerous to go into that neighborhood. Nonsense, I said. If a short, middle-aged, bald ethnographer can spend more than a year among those teenagers and walk away physically unscathed, surely social workers protected by well-armed police officers could have saved those children from a lifetime of pain.

Despite its challenges, I learned personal and professional lessons doing Fremont research. That popular book opened the door for all to see how badly social service agencies neglected children who need help the most. I was Fremont adolescents' social worker. I fed those little girls when their mother and others around them didn't respond to their cries of hunger. I didn't feed these kids and watch over them because I needed something disgraceful to write about. I did it because I am a father. That's what fathers do. They protect their children. I stand accused of subjective social research. I plead guilty.

The Art of Ethnographic Writing

Anthropological ethnography requires time, patience, and endurance. Done well, our ethnography can be a creative expression. Literary writing can transport readers into imaginary worlds' novelist envision. Creative ethnographic writing can transport readers into real worlds we experienced. I learned early on that a good description was its own analysis. The trick is learning how to write a good description using engaging prose.

Skillful writing requires more than drafting a series of grammar sentences. Chomsky's famous sentence "colorless green ideas sleep furiously" (Chomsky, 1957) conforms to grammatical rules. What does it mean? Be warned: You can write grammatical sentences absent of easily understood meaning. When you do that, you don't know the message you want to send your readers.

When I wrote *Beggars and Thieves*, I needed examples of creative prose that wouldn't tranquilize readers. Luckily, years before I had started research on *Beggars and Thieves*, I read James Lee Burke's early novels. He wrote beautiful prose drawing readers into dramatic sunsets, rainstorms, and mountain snows. If you read carefully you'll notice that Burke's backdrop scenes convey affective qualities contextualizing scenes. Lighting, thunder, and downpours convey angry characters' emotions.

If you do really good in ethnography and have a powerful story to tell readers, stiff prose will limit your reading audience. Soon after a semester ends, your boring books will be sold to the next generation of students who read bad writing. No one who does solid blink-interpreting ethnography should write monotonous narratives. Let your friends read your ethnographic essays. If they snore or give up in a few minutes, rewrite. My most recent book, *Living Black: Social Life in African American Community* (2015a), has about 63,000 words. To get to those final 63,000 words, I wrote about 175,000 words. Writing isn't easy. It can be enjoyable. It's always therapeutic.

You can avoid writing boring prose. To do that, you will need to find your voice. We use our spoken voice every day. Social contexts influence what we say and how we say it. Sociolinguists label these shifts in voice speech registers. My granddaughters and grandsons have never heard me speak in the speech registers I have used chatting up men, newly released from prison, standing in line awaiting a meal and a bed at a Salvation Army shelter. My use of obscenities and expressions borrowed from prison inmate speech registers create an instrumental voice, a hook, which allows me to hang out. After all, a guy who speaks like they do cannot be a stuffy, boring researcher, right? My persona changes, so does my voice.

Voice conveys your personality, your points of view. *Dead End Kids* was written in an angry voice condemning unresponsive social service agencies that ignored and failed to help Fremont adolescents. A friend and leading criminologist told me that I got too close to the Fremont Hustlers. I took his criticism as validation of the value of participant observation ethnography and that my empathic voice hit the bullseye. I wrote an ethno-semantic analysis of speech registers prison inmates use to describe sexual behavior (Fleisher, 2015b). I wrote it an academic voice. In *Beggars and Thieves*, my voice shifts street registers as I shift social scenes, hanging out with drug dealers to gang members to police patrol officers. I paid careful attention to accurately describing the speech registers patrol officers used when they spoke with teenagers selling rock cocaine. Theirs was the voice of concerned uncles.

When you write your ethnography think about the registers you choose. Finding your voice takes years of writing practice. If you're patient and deliberate in your writing style, you will find your voice. It'll surprise you. Practice. Be patient. But know this: If you don't read quality literature (in addition to text books and journal

articles), there's a good chance you'll miss an opportunity to share your work with a broad reading audience. I am the editor of a book series on violence intervention and prevention. I tell prospective authors their writing must, first and foremost, be easily read by folks standing in a grocery store line awaiting checkout. Use jargon, statistics, and theory sparingly, if at all. Remember the old adage: If you can't explain something clearly, you don't understand it. I require clearly written, plain English. We have an obligation to share what we've learned with the reading public whose taxes support our state universities.

We're out of time. Today's message: Find your ethnographic bliss. Take whatever paths you must to find your bliss. When you find your bliss, you'll know the difference between a twitch and blink. You'll have experienced the pleasures, frustrations, and rewards of ethnographic research. Share your work, and write creative ethnographic narratives. Readers will benefit from your experiences.

Good class. See you next week. Read.

References

Chomsky, N. (1957). *Syntactic structures*. The Hague, Netherlands: Mouton.

Fleisher, M. S. (1972). The possible application of ethno-semantics to prison argot. *Anthropological Linguistics, 14*(6), 213–219.

Fleisher, M. S. (1984). *Hesquiat* kinship terminology: Social structure and symbolic world view categories. *Anthropos, 1*(3), 243–248.

Fleisher, M. S. (1989). *Warehousing violence*. Newbury Park, CA: Sage.

Fleisher, M. S. (1995). *Beggars and thieves: Live of urban street criminals*. Madison, WI: University of Wisconsin Press.

Fleisher, M. S. (1998). *Dead end kids: Gang girls and the boys they know*. Madison, WI: University of Wisconsin Press.

Fleisher, M. S. (2000). (Counter-) transference and compassion fatigue in gang ethnography. *Focaal, 36*, 77–94.

Fleisher, M. S. (2015a). *Living black: Social life in an African American neighborhood*. Madison, WI: University of Wisconsin Press.

Fleisher, M. S. (2015b). The culture of violent behavior: Language, culture, and worldview of prison rape. In J. Miller & W. R. Palacios (Eds.), *Qualitative research in criminology: Advances in criminological theory* (Vol. 20, pp. 101–131). Abingdon, UK: Routledge.

Lantis, M. (1938). The Alaskan whale cult and its affinities. *American Anthropology, 40*(3), 438–464.

Pike, E. V. (1964). The phonology of New Guinea Highlands languages. *American Anthropologist, 66*(4), 121–132.

Sapir, E. (1927). Language as a form of human behavior. *The English Journal, 16*(6), 421–433.

Sapir, E. (1929). The status of linguistics as a science. *Language, 5*(4), 207–214.

Suttles, G. D. (1976). Urban ethnography: Situational and normative accounts. *Annual Review of Sociology, 2*(1), 1–18.

Wolfram, W. (2004). Social varieties of American English. In E. Finegan & J. R. Rickford (Eds.), *Languages in the USA: Themes for the twenty-first century* (pp. 58–75). London: Cambridge University Press.

The Ghost of Ethnography Future

Alistair Fraser

In Glasgow, Christmas is in the air. The night is biting cold, the darkness lit by festive tree-lights. Inside, Bill Murray's riff on *A Christmas Carol*, *Scrooged* is on TV. He is visited by three ghosts: the cranky cadaver of Christmas past, the violent fairy of Christmas present and the mute reaper of Christmas future. As I sit down to write, it strikes me that this chapter might be an opportunity to visit the ghosts of ethnography past, present and future in much the same way. Like Bill Murray's misanthropic character, the chapter will look backward in order to see forwards. No animals will be harmed in the process.

Twenty years ago, Adler and Adler (1998) did a fine job of sketching the ghost of criminology past. In a lyrical foreword to the text *Ethnography at the Edge*, they mapped the history of ethnographic studies of deviance onto the history of artistic movements: from the formative 'Impressionism' of the Chicago School of the 1920s and 1930s to the 'Renaissance' of the Second Chicago School in the 1940s and 1950s and the 'Abstract Expressionism' of 1960 countercultural ethnographies. The 1970s to 1990s signalled a return to the 'Dark Ages'—the rise of positivist methods, institutional review boards and tenure precarity drowning out previous waves of creative energy. There was evidence, however, of a new 'Enlightenment' in their criminological present. This new period was signalled by the publication of the text itself—studies of graffiti writers, motorcyclists, sky divers, fire-fighters—that represented a gasp for breath amid the stifling airlessness that had come to dominate criminological research.

Twenty years feels like an appropriate period to assess the extent to which this criminological present has come to pass. As this collection demonstrates, ethnographies of crime and deviance are in a fine state of health, in many ways challenging and developing the nascent 'Enlightenment' the Adlers spoke of. Critical, feminist and postcolonial ethnographies have shattered some of the idols of the criminological

A. Fraser (✉)
Scottish Centre for Crime and Justice Research, University of Glasgow, Glasgow, UK
e-mail: alistair.fraser@glasgow.ac.uk

canon (Deegan, 1988), and scholars have shifted their focus increasingly away from the 'dispossessed, marginal and the strange' (Burawoy, 2000, p. 12) that the Chicago School took as their focus and raison d'etre. As this collection demonstrates, ethnographic renderings of crime and justice represent the very best of the criminological canon, documenting complex lives in a deeply human way. Such studies offer access to the backstage lives of actors entangled with criminal justice systems, allowing insight into the social and cultural production of crime and harm in a way that recognises the complexity of power, culture and agency. In the denial of straightforward causative explanations of crime, ethnographies can directly challenge stereotype and stigma in a way that no other method can. When viewed through this lens, the present of criminological ethnography is in a state of health and vigour.

When viewed comparatively, however, the picture changes. In the two academic traditions most associated with ethnography—anthropology and sociology—there have been stark debates on the epistemological assumptions of ethnography, with subsequent periods of revision and renewal. In the aftermath of *Writing Culture* (Clifford & Marcus, 1986), for example, the narrative-driven text of the traditional anthropological account was skewered, resulting in a diversification and fragmentation of both fieldsites and approaches. In sociology, ethnographic work has sought to capture the shifting terrains of the global era through mobile, 'live' methods (Back & Purwar, 2012; Burawoy, 2000). In criminology, however, there have been few stocktakes or renewals of this kind—rather, ethnographers of crime and deviance have subsisted at the margins, burrowed deep in their fieldsites and only coming out for air to speak to one another or with confused colleagues.

Rather than focusing on the past or present, however, I want to focus on the future. In this chapter, I suggest three avenues for ethnography future—careering, teaching and moving—that seek to draw together existing knowledge into a coherent frame capable of responding to the exigencies of the contemporary era.

Careering Ethnography

Ethnographies of crime and deviance have a rich, if fragmented, history in the criminological imagination. We have heard further in this volume of innovative and boundary-pushing ethnographies across a range of areas, and it is clear that the study of crime and deviance continues to attract and inspire grounded, critical, engaged scholars who think deeply about their practice. These renderings feel all the more vital in the contemporary age, as the slow craft of ethnography is eroded by life in the accelerated academy. By and large, however, the demands of modern university life are such that the time, care and thought required for most—though not all—ethnographic practice are increasingly curtailed and regulated. But it is a practice with great potential to speak beyond the academy—to make robust critiques of the impact of social policy, to trace the multitude of cultural adaptations caused by shifts in social life, the webs of criminalisation that stick to individuals and the undersides of globalised change and migration. Taking the time to do

ethnography amid this increasingly constrained academic landscape feels tantamount to a political act.

To do so, I think, we need to think first about careering ethnography. This doesn't mean what you did on the way home from the bar last night, though it can be part of it. A half-remembered anthropology paper from a few years back talked about *participant-intoxication*, a study of a village where everyone took a form of fermented alcohol daily and everyone was inebriated more or less all the time. This form of participant-observation will be familiar to many who research deviant groups. As Dick Hobbs once wryly observed after a long night out with participants, he wasn't sure whether to write it up or sick it up. But not this kind of careering—what I mean is to reflect on what it means to develop an ethnographic career. As Hobbs again has observed, on an occasion of writing rather than vomiting, the best ethnographies are often done by PhD students. Some people manage to find a way to continue to think and work ethnographically at different points in their life—but for many others who are fortunate enough to have an academic job, the particular demands of academic—and family—life make the slow craft of ethnography challenging, to say the least.

Careering ethnography therefore takes two forms. The first is to consider examples of scholars that have managed to compose what might be called an 'ethnographic career': to move in and out of multiple fieldsites over a lifetime and balance this with the demands of work and life. These examples are few but powerful: Jeff Ferrell, Dick Hobbs and the Adlers. Reflecting on what fieldsites are open or closed at different life stages is important. The second form is to establish something more of a canon in the field. It's remarkable that there have been only one or two efforts to sketch the contours of ethnographies of crime and deviance to date (e.g. Hobbs, 2001) which means that most new ethnographers don't have an easy starting point in learning the literature and situating their own study in relation to it. Not quite a canon, but bringing to light the lineage of ideas and practice that informs our contemporary debate, situating these scholars within their particular historical context and, to some extent, rereading and reinterpreting them for the contemporary era. This can—and probably should—involve the rescuing and revival of ethnographers of deviance that have been forgotten, who were silenced by history. It would be fruitful I think to look backwards and to look forwards, examining the life courses, trajectories and influences on previous generations of ethnographers such that we might shore up the theoretical and methodological roots of contemporary practice.

Teaching Ethnography

In the UK, criminological ethnography is not really taught. The space for doctoral students to spend serious time on fieldwork is itself increasingly curtailed. The new postgraduate research training programmes now established in the UK make scant mention of ethnography; it is increasingly written out of the curriculum. The seminars in global ethnography or the workshops in writing ethnography that we read

about in the USA are not replicated in the UK; as a result, mostly, it is a craft learned on the job, with guidance from supervisors and peers. The ghost of ethnography future, then, is that the craft of ethnography becomes further marginalised and individualised.

The response to this, I think, is to think collectively about teaching and writing ethnography. During my PhD, I was the only one in my department studying criminology. Peers were carrying out ethnographic studies of UN Climate Change conferences, of rural communities in Southwest Uganda, community-building among refugees and asylum seekers in Scotland and voting preferences in Barking and Dagenham. As a result I learned at least as much, if not more, of fieldwork traditions and epistemological debates in sociology and anthropology than criminology. I was fortunate to attend a one-week anthropological training course, involving an intensive training workshop in field methods, in a big strange house in the Scottish Highlands. There were about twenty of us, mostly new PhD students about to embark on fieldwork—on military veterans, environmental disaster and religious politics—and we spent five furious days learning from leading lights in anthropology, on field methods, archival work, language skills and so on, as well as carrying out micro-visual studies of the community we were based in. A lot of drinking and a lot of smoking too. I think this was a secret anthropological technique to gain people's confidence, but it didn't really work as everyone was doing it. Rather than a keynote speaker, there was a keynote listener—the anthropologist Kit Carson— who applied an ethnographic ear to the week and then reflected at the end.

Around this time, I read with envy the collaborative publications of Michael Burawoy (2000) and his graduate students—all of whom, thirteen or so, were engaged in live fieldwork and were talking and reflecting on it on a regular basis, including one poor soul whose fieldsite was the sessions themselves. Connected to this, we don't really talk about writing ethnography. As I mentioned, for anthropologists, ethnography is not a method but a written product, and a great deal of debate has trained on the craft of writing. To quote Kit Carson, ethnography is a field science and a written art, and both require careful honing. I struggled to think about writing ethnographically and in the end used fieldnotes verbatim, with days and times attached, to reflect a particular observational moment; rather than, as is more customary in the USA, telling the story in the first person, tugging the reader close to the reality you seek to interpret and depict.

Moving Ethnography

Ethnographers are trained as 'professional strangers' (Agar, 1980) that inhabit a liminal world of in-between, and 'floating' between different worlds. Ethnography has long since been the approach of choice for investigations of crime and deviance, a rite of passage for critical scholars. Yet unlike in anthropology and sociology— wherein a critical questioning of the bases of ethnographic practice has resulted in reformulation of practice—criminologists, and I am one of these, have by and large

reenacted classic ethnographies. With notable exceptions, we have been driven not by theoretical questions of where new ethnographic sites might be found but by historical questions of where they were found in the past.

Within anthropology, commentators speak of the challenges posed by globalisation to the traditionally place-bound methodology of ethnography but also a 'postcolonial crisis of ethnographic authority' (Clifford, 1988, p. 8) that has emerged from a critique of the imbalances of global knowledge production. These critical developments form part of a broader intellectual current that explicitly critiques the structures of power through which knowledge is created and the intellectual legacies of 'telling stories' about peripheral populations. Within the field of criminology, ethnographic research most commonly involves observation of, and at times participation in, the activities of deviant populations—what Ferrell refers to as criminological *verstehen*—or that of police, prisoners or lawmakers. Despite recent efforts to establish a more global lens to criminological ethnography (Ferrell, 2012), however, there have been few efforts to engage meaningfully with the challenges posed by globalisation and postcolonialism within ethnographic practice. Ethnography tends to be local—or at best glocal—but seldom transgresses national borders or follows routes and connections. In an increasingly mobile, liquid era, where movement and migration are central motifs, researchers need to be mobile too. This presents a problem for ethnographers, as comparative ethnography is exceptionally rare. Long-term engagement with two or more fieldsites requires a bilingual cultural sensitivity and scholarly commitment that is as demanding as it is time-consuming.

Movement could mean shifting beyond a static fieldsite. The classical idea of the ethnographer is akin to that of the out-of-town stranger in John Ford films, arriving in a new place, figuring out who's who, the social dynamics involved, how these are shaped and how to make your way through them. I wonder, though, in an era that is at least as much about mobility as immobility, whether we need to think about moving ethnographies—more urgent, live accounts of events as they unfold (Back & Purwar, 2012). I'm thinking here more of the immediacy of recent films like Tangerine, a tale of humour and vengeance on the streets of LA, shot on an iPhone; or like Victoria, told in a single-shot, real-time, tugging the viewer urgently into and through the lives of others. This form of 'live', or 'liquid', ethnography (e.g. Kindynis, 2017; Raymen & Smith, 2015) is surely one of the ways of retaining the relevance and vitality of ethnographic methods.

Movement too could mean movement beyond the idea of ethnographic privilege. Ethnographers have traditionally ploughed a lonely furrow, with debate surrounding 'outsider' status and entering a closed field. Ethnographic practice is increasingly informed by a decentring of ethnographic authority and a realignment toward more collaborative studies involving participants. In this scenario, plural ethnographies have the potential not only to collectivise fieldwork but to challenge the very idea of the ethnographer as 'other'—rather to conceive of ethnographic practice as engaging with multiple researchers and participants in a way that is co-productive and reflective of the 'throwntogetherness' (Massey, 2005) of contemporary life. A few years back, I carried out an experiment in this approach through what we termed an

'ethnographic exchange' (Fraser & Hagedorn, 2018), with the 'home' researcher operating as a gatekeeper, guide and critical friend during the field visit. The purpose was to physically experience a different fieldsite and be confronted bodily with the similarities and differences with your 'home' research site. The beauty of the exchange was its simplicity and efficiency: the hard-won access of the other researcher is shared and collectivised, allowing the visiting scholar a sharp, penetrating insight into a social world that may diverge considerably from their own. Finally, virtual and digital ethnographies—and the hinterland between 'online' and 'offline' presence—represent a critical lens through which to engage the present. Such issues require ethnographers to think creatively and inventively about how we do 'real-time' sociological research into evolving social relations, practices and interactions whilst remaining attentive to the bigger historical and political frames (Back & Purwar, 2012).

Conclusion

It strikes me, then, by way of conclusion, that we might want to consider what is unique and distinctive about ethnographies of crime and deviance that distinguish our practice from the work of ethnographers in other fields and clarify that to better consider our collective ethnographic futures. For me it is understanding the social and cultural processes involved in behaviours labelled criminal, the group dynamics and community contexts and the structural levers and individual dispositions that add up to acting in a way that transgresses the law. But it is also the study of the machine itself; the bureaucratic mechanisms in which processes of criminalisation are embedded; the interaction between policing cultures and data processes, between legal reasoning and courtroom decision-making; and the interaction between complex structural forces and individual and group actions. In this sense it is not dissimilar from the study of state processes of migration and asylum processing or other instances in which state control is extensive and sharply regulatory. But it is also engaged in the study of feeling, emotion, affect and excitement—the vitality and spontaneity and unpredictability of social life in ways that diverge from these approaches.

It must also be recognised that ethnography is a broad church—that there are many ways to broach these subjects in a way that is authentic and real—and that confronting these issues might necessitate complex identity work in finding a position that is both personally and politically workable. Remaining engaged in the real-world whilst remaining relevant in the accelerated academy is undoubtedly the challenge of the ethnographic present. Perhaps the ghost of criminology future will be found skulking, not so much in the streets and alleys of urban areas but in the boardrooms of organizations. But thinking, teaching and writing collectively are ways to conceive of a future that is not spectral but concrete.

References

Adler, P. A., & Adler, P. (1998). Foreword: Moving backward. In J. Ferrell & M. S. Hamm (Eds.), *Ethnography at the edge* (pp. xii–xvi). Boston: Northeastern University Press.

Agar, M. (1980). *The professional stranger: An informal introduction to ethnography*. London: Academic Press.

Back, L., & Purwar, N. (2012). A manifesto for live methods: Provocations and capacities. *The Sociological Review, 60*(1), 6–17.

Burawoy, M. (Ed.). (2000). *Global ethnography: Forces, connections, and imaginations in a post-modern world*. Berkeley, CA: University of California Press.

Clifford, J (1988) Predicament of Culture: twentieth-century ethnography, literature, and art. Cambridge, Mass:Harvard University Press.

Clifford, J., & Marcus, G. (1986). *Writing culture : The poetics and politics of ethnography*. Berkeley, CA: University of California Press.

Deegan, M.J. (1988) Jane Addams and the Men of the Chicago School. London: Routledge.

Ferrell, J. (2012). Outline of a criminology of drift. In S. Winlow & S. Hall (Eds.), *New directions in criminological theory* (pp. 241–256). Abingdon, England: Routledge.

Fraser, A., & Hagedorn, J. (2018). Gangs and a global sociological imagination. *Theoretical Criminology, 22*(1), 42–62.

Hobbs, D. (2001). Ethnography and the study of deviance. In P. Atkinson, A. Coffey, S. Delamont, J. Loftland, & L. Loftland (Eds.), *Handbook of ethnography*. London: Sage.

Kindynis, T. (2017). Urban exploration: From subterranea to spectacle. *British Journal of Criminology, 4*(1), 982–1001.

Massey, D. (2005). *For space*. London: Sage.

Raymen, T., & Smith, O. (2015). What's deviance got to do with it? Black Friday sales, violence and hyper-conformity. *British Journal of Criminology, 2*(1), 389–405.

Becoming a Police Ethnographer

William Garriott

Since 2006, I have been engaged in the scholarly study of police and policing (Garriott, 2011, 2013). I am an anthropologist and sociolegal scholar by training. My undergraduate and early graduate work focused on religious studies. I might have stayed on this intellectual trajectory were it not for the time I spent conducting ethnographic research in the Appalachian region of the United States. This time "in the field," in communities in Eastern Kentucky and West Virginia, fundamentally changed the course of my scholarship and career. During this time I was confronted with questions for which I had no answers, questions that forced me to rethink my obligations as a scholar. It is a cliché that the questions ethnographers carry with them into the field invariably prove inadequate, if not completely wrongheaded (Faubion & Marcus, 2009, p. 22). But this is the beauty and challenge of ethnographic work. In my case, it set me unexpectedly on the path to becoming a police ethnographer.

Police ethnography is a field of interdisciplinary inquiry populated by scholars from a range of social scientific disciplines. Work in this area is united by the commitment to ethnographic inquiry—an approach which has traditionally been taken as offering a "view from the street" on the practice of policing (Manning & Van Maanen, 1978). Police ethnographies written several decades ago formed the foundation for social scientific research into police, even though the field is now dominated by quantitative studies (Marks, 2004; Moskos, 2009; Reiner & Newburn, 2007). These foundational studies encouraged a focus on the most visible manifestation of policing: the uniformed patrol units of municipal police departments.[1] This focus fundamentally shaped how police has been understood in scholarly circles,

---

[1] Though it is beyond the scope of this chapter, it is important to mention that there is a distinct urban bias in police ethnography as well.

W. Garriott (✉)
Drake University, Program in Law, Politics, and Society, Des Moines, IA, USA
e-mail: william.garriott@drake.edu

with significant implications (and, as I will argue, limitations) for our understanding of police power.

The tendency to focus on uniformed patrol officers is evident at the level of methodology. Though there is variation, the conventional approach has been for the ethnographer to embed him- or herself within a particular branch or branches of the uniformed police force of a given polity. The ethnographer then shadows the police officers—usually patrol officers—as they go about their work. This typically involves some combination of riding along with officers as they go on patrol, accompanying them as they make arrests, watching as they interrogate suspects, and/or otherwise following along as they engage in everyday duties. Some ethnographers have extended their research to include officer socializing "off duty" as well, at locations such as bars or pubs. Clive Norris, discussing his ethnographic work with a British police department, offers a typical description:

> I went out on routine patrol, both on foot and in cars; I sat in the station office and made tea for the shift; I helped chase suspects and, at times, arrest them, counselled the distraught, and administered first-aid to attempted suicide victims. I have felt a sense of relief when we have slipped out of the cold into a warm tea-hole, and shared the fear, humour, and boredom that are part of the everyday lives of police officers (Norris, 1993, p. 126).

Many ethnographers have been required to perform police work (guarding suspects, watching entrances, even driving a patrol car) (Rowe, 2007; Westmarland, 2001). Some ethnographers have actually become police officers in order to carry out their research (Moskos, 2009). Likewise, some police officers have become ethnographers, using their insider knowledge of policing to reveal the often cloistered world of police culture and practice (Young, 1991). But the uniformed police force is but one site through which the exercise of police power takes place, as I found in my work on methamphetamine.

From Religion to Oxy to Meth

I entered graduate school in anthropology in 2003 to pursue a project on the global spread of Pentecostal and charismatic Christianity. The shift to anthropology was unexpected. My undergraduate and graduate training up to that point had focused on religion, and I had presumed that any PhD work would be in religious studies. But, while working on my master's degree, I became increasingly interested in what was being called "lived religion" (Hall, 1997). The best work on lived religion was being done by ethnographers, and, among the disciplines, anthropology had the deepest commitment to ethnographic work. Of particular importance for my own intellectual development was anthropologist Robert Desjarlais' *Body and Emotion: The Aesthetics of Illness and Healing in the Nepal Himalayas* (Desjarlais, 1992). Besides being a superb ethnography of religious experience, this book exposed me to the wider subfield of medical anthropology, which would become an essential point of orientation as my work later shifted to questions of drugs and addiction.

Desjarlais, like many anthropologists, followed the disciplinary convention of conducting research outside his home country of the United States. However, I knew that I wanted to do something within the United States, on Appalachia in particular. I knew I wanted to work in Appalachia the moment I finished reading Kathleen Stewart's *A Space on the Side of the Road: Cultural Poetics in an "Other" America* (Stewart, 1996). The book remains one of the most significant on the region. It is also one of the most challenging—productively so, for even in those moments where I found myself disagreeing with Stewart's argument or interpretation, the book was performing the important role of breaking Appalachia out of the folkloristic cul-de-sac in which it had become trapped, demonstrating that it was equally well-suited for considerations of "high theory." There was also something personal embedded in the idea of studying Appalachia. Though I am not from the region, I grew up close by in central Kentucky. The Knobs, as we called the nearby foothills, were always visible on the horizon.

But making Appalachia the focus of my scholarly life was about more than staging a return home; it was about heeding the call coming from scholars such as Nader (1972) and Marcus and Fischer (1986): they argued that anthropologists should turn their gaze upon their own societies. This call was ethical as much as it was methodological. It was about moving forward in the wake of decades of postcolonial critique and adapting anthropological conventions to a changing world. The arguments were compelling, despite strong signs that it might not be the best career move (Gupta & Ferguson, 1997[2]), and so I began planning an ethnographic study of religiosity in Appalachia.

But in the summer before beginning my doctoral program, things began to change. I was exploring potential field sites and trying to get a deeper sense of day-to-day life in the Appalachian region. Part of this work involved accessing local newspapers. And when I did, I found something unexpected. I started to read stories about drugs, prescription drugs in particular. Some of these took the form of "news of the weird" stories: a man walking into the local pharmacy with a hunting rifle and demanding all of the prescription painkillers behind the counter. Finding one such story was one thing, but I kept encountering them. Not long after, a colleague put me in touch with a local activist in Eastern Kentucky. A brief visit in the summer of 2004 let me see firsthand all of the problems being set off by prescription painkillers, particularly a new drug, OxyContin (Garriott, 2010).

OxyContin is a potent prescription painkiller. It is an opioid, cousins with drugs such as heroin and morphine. Though there was a history of prescription painkiller use and abuse in the region, OxyContin was altogether new. I heard about it firsthand when I visited with local activists in Eastern Kentucky. I met a woman named Sheila. She was likewise in her 20s. Her husband had died from an addiction to Oxy, which he'd developed after beginning to use it to treat an old football injury. Sheila worked with Jeff, who was a longtime community activist in the area. Though Jeff had worked on several different issues over the years, prescription drug addiction

---

[2] "It remains extremely difficult for students who do their dissertation fieldwork entirely within the United States to get jobs at top departments" (Gupta & Ferguson, 1997, p. 14).

was now the primary problem facing his community. One of my most memorable experiences was when we went to a local restaurant for lunch. It was crowded. Jeff and Sheila knew everybody there. And they began pointing out every person in the restaurant that they knew had struggled personally with prescription drug addiction. This included our server, who was trying to get back on her feet after a stint in rehab.

Leaving Eastern Kentucky after my visit with Sheila and Jeff, I was overwhelmed by the scope of the problem. As I drove the winding roads out of the mountains, I found myself trying to make sense of what I'd just encountered. My previous work in medical anthropology was helpful. Certainly it provided a preliminary framework for thinking about both the political economy and lived experience of drugs and addiction. But it also provided something else. Arthur Kleinman writes forcefully about the inherently moral dimensions of experience. By moral, Kleinman means "what's at stake" for those individuals and communities that one encounters as an ethnographer and as a fellow human. For Kleinman, attending to what's at stake is at the heart of anthropology and the ethnographic enterprise (Kleinman, 1995).

And so my focus shifted, from religiosity to the emerging problem with drugs and addiction in Appalachia and other parts of the rural United States. Three years later, as I was beginning my work in West Virginia, the prescription drug problem was being overshadowed by yet another: methamphetamine. Indeed, while there was certainly a prescription drug problem in Baker County, West Virginia, when I moved there in 2006, methamphetamine was the issue of overriding concern to the community and to the nation.

Stumbling into Police Ethnography

But even as my focus shifted toward drugs, I did not go to the field thinking I was engaged in a study of police or policing. At the beginning my intention was to study the treatment experiences of methamphetamine users in the resource-poor setting of the rural United States. My background and continuing work in medical anthropology made this an obvious focus. I was also significantly influenced by the tradition of community studies in the United States, particularly the works of Varenne (1977) and Greenhouse, Yngvesson, and Engel (1994). Stemming from this influence, I was prepared to talk to a broad cross-section of local residents while participating in the everyday life of the community. I imagined that those struggling with addiction themselves would be a key focus of the study, as would treatment specialists, community activists, and members of the criminal justice system, just to name a few. Understanding ethnography as "the science of contextualization" (Greenhouse, 2012, p. 2), I imagined that gathering a broad swath of experiences from members of the community would allow exploration of the broader milieu in which individual experiences of methamphetamine addiction and treatment took place.

I began with those closest to the methamphetamine problem and its treatment. This included drug counselors at the local state-funded mental health facility, psychologists in private practice who worked with addicts, and a variety of profes-

sionals in the social service field, including social workers, mental health advocates, school guidance counselors, and the like. But interviews with individuals in these professions frequently ended with the recommendation that I speak with members of the criminal justice system—police in particular. Their experience policing methamphetamine had made them the primary experts on the issue. In addition, as I watched the wheels of the local justice system churn, I soon realized that local treatment resources, such as they were, could not be understood apart from the apparatus of drug enforcement. Carl Ferguson, the head addiction counselor at the local mental health facility, openly acknowledged this. He explained in an interview that the facility would have had very few patients were it not for the court system that frequently required participation in the treatment program as a condition of probation. The threat of incarceration kept people enrolled in the treatment program, succeeding where the individual desire to overcome the addiction seemed to fail: "The court is the hammer that keeps them in treatment," he stated.

The longer I stayed in the community, the clearer it became that the criminal justice system was taking the lead in the local response to the methamphetamine problem. This was due in part to the fact that there were few local treatment options apart from Alcoholics Anonymous and Narcotics Anonymous meetings and outpatient programs administered through the local mental health facility. The nearest inpatient treatment programs were upward of 100 miles away. They were also expensive—prohibitively so for many users of methamphetamine. But beyond this, the criminal justice system was the best-staffed and best-funded state agency in the area and the only one that seemed to enjoy a public mandate for dealing explicitly with the methamphetamine problem. Indeed, a community action group called Concerned Citizens United Against Crime focused their anti-drug efforts on monitoring the criminal justice system to make sure it performed according to a strict set of largely punitive standards. These standards included the targeting of drug offenders by police, the handing down of harsh sentences for drug offenses by judges, and the development of new policing strategies (such as increased use of the drug detection dog in the local schools) to ferret out drug dealers and users. Of course, this approach was in line with the governing ideology of the War on Drugs, four decades of which had solidified the idea that drugs were a problem to be managed punitively. It also reflected the broadened mandate of the criminal justice system in the United States which, over the same period, had been expanded to address the effects of social problems such as poverty, mental illness, and homelessness that had once been more explicitly managed by the welfare state (Simon 2007). So ingrained had this perspective become that, according to police officers I interviewed, methamphetamine users themselves would come to them requesting to be taken to jail in order to detox from the drug. Thus, even as I maintained my interest in the treatment experiences of those struggling with or recovering from methamphetamine addiction, it became clear that I could not understand these experiences without focusing explicitly on the criminal justice system, for it was in the context of this system— and its punitive orientation toward the management of drugs and drug offenders— that the treatment experiences of the majority of those caught up in methamphetamine were embedded.

The fact that my turn to police ethnography was unplanned had important impli-cations for my research. First, since I did not go in to the field thinking I was going to study police, the uniformed police force was not the primary focus of my research. This meant that, among other things, I did not approach my ethnography of police through traditional means (arranging ride-alongs, shadowing patrol officers, observ-ing life at the station, and so on). Rather, I negotiated a position as a researcher who wanted to simply speak to as many people in the community as possible with a perspective on the methamphetamine issue. My questioning of police officers and other members of the criminal justice system was thus not about police activity per se but about methamphetamine and the way methamphetamine was impacting police work. This meant that officers were not positioned as objects of study but rather bearers of a unique perspective and privileged knowledge regarding metham-phetamine.[3] Moreover, the forms of policing I highlighted were not limited to patrol work or investigation but included practices like community presentations and awareness campaigns.

The productive challenge here was that I had to maintain good rapport with both the police and the policed: police officers, judges, and attorneys, as well as those users and dealers they had arrested and convicted—no small feat given the suspi-cions and resentments that circulated between these various individuals in the com-munity, to say nothing of the constant surveillance that is part of life in rural and small-town America. Moreover, it is not uncommon for ethnographers researching criminality to themselves be suspected of being police operatives or undercover agents (Bourgois, 1995). Whenever I interviewed someone who I knew was a cur-rent or former user, or who had served time incarcerated for their use, I went out of my way to reassure them that I was not working for the police. Most dismissed this reassurance, telling me that they were not concerned. But for me it was important to be explicit on the point, particularly since there are some uncanny resemblances between the ethnographic method and police work. Of greatest significance, per-haps, is the reliance of both the ethnographer and the police officer on "informants."

Though I went to great lengths to reassure former meth users, it was often the police who were the more suspicious. Ethnographers are often denied access to police departments because the police are suspicious of their motives—or, at a mini-mum, institutional gatekeepers can find no incentive to allow an outside observer access (Moskos, 2009). In my own case, an officer who had worked for several years as undercover as part of a drug task force had a paranoid response when I called him to ask for an interview at the suggestion of another officer. He reluctantly

---

[3] Reiner (2000) has argued that a key virtue of ethnography as a means of studying police is its ability to study what police actually do, rather than simply what they say they do. Since my objec-tive was never to study police behavior per se, I was forced to simply take the police at their word—in the same way that I took everyone else I interviewed at their word. In the case of police, however, I could corroborate certain statements by checking it against court documents and others who experienced the same events being described in the interview. More importantly, perhaps, I treated what I was told as "official discourse": an example of what police thought I, as an outsider, should be told or wanted to hear.

agreed to the interview but demanded that I bring identification, stating, "You wouldn't believe what some people will do to get some inside information." Similarly, when attending criminal court proceedings at the county courthouse (which are open to the public), I was approached by the county sheriff and asked to identify myself. "You've got a right to sit in," he said after looking ambivalently at the business card I produced. "We just like to keep track of who's here."

The payoff for working with both police and the policed was that I was able to see the uniformed police force within the wider milieu of its operation. This allowed me to better understand its central but by no means exclusive role in the policing of the methamphetamine problem and therefore the community more generally. The police thus functioned as one node in the wider drug enforcement apparatus, just as the drug enforcement apparatus functioned as one node of police power. Thus, even as my project began to focus on police, it became an ethnography of *police power*, rather than simply of *police officers*.[4]

And this opened up an important perspective on the ethnographic study of police. It brought into relief the overwhelming focus of police ethnographers on patrol and patrol officers. Up to a point, this makes sense: they are the most visible manifestation of police power. They are what one thinks of when one hears the word "police." And yet, as scholars demonstrate again and again, the uniformed police force is just the most visible tip of a giant iceberg of policing. While police ethnographers continue to perform a vital role in highlighting what it is that police officers actually do when on patrol, an unanticipated negative consequence is that the uniformed police officer working "the streets" remains the unquestioned icon of police power. This leaves underexplored and undertheorized the rest of the "police assemblage" (Brodeur, 2010).

Since completing my ethnographic work, I have spent much more time becoming acquainted with the wider scholarly literature on police and policing. I have been privileged to be part of a small group of anthropologists working to establish police as a topic of anthropological inquiry. And all of this has taken place at a time in the United States when questions of policing have become central to debates over and within social and political life. Throughout this work, my time in Appalachia remains the touchstone. It leaves me perpetually dissatisfied with studies where either policing or police power is reduced to the actions of the uniformed police. This, in turn, has left me with the conviction that a key challenge for those engaged in the scholarly study of policing, both ethnographers and not, is to come up with theoretical and methodological conceptualizations that do justice to the full scope of police in the contemporary world.

---

[4] Reiner and Newburn (2007) note the increasing focus on policing rather than just police in recent police research.

References

Bourgois, P. (1995). *Search of respect: Selling crack in El Barrio*. Cambridge: Cambridge University Press.

Brodeur, J.-P. (2010). *The policing web*. Oxford: Oxford University Press.

Desjarlais, R. R. (1992). *Body and emotion: The aesthetics of illness and healing in the Nepal Himalayas*. Philadelphia: University of Pennsylvania Press.

Faubion, J. D., & Marcus, G. E. (2009). *Fieldwork is not what it used to be: Learning anthropology's method in a time of transition*. Ithaca, NY: Cornell University Press.

Garriott, W. (2010). 'Straight from the devil:' Contours of the 'public' in American public health. In J. Adkins, L. Occhipinti, & T. Hefferan (Eds.), *Not by faith alone: Social services, social justice, and faith-based organizations in the U.S* (pp. 231–253). Lanham, MD: Lexington Books.

Garriott, W. (2011). *Policing methamphetamine: Narcopolitics in rural America*. New York: New York University Press.

Garriott, W. (2013). *Policing and contemporary governance: The anthropology of police in practice*. New York: Palgrave Macmillan.

Greenhouse, C. (2012). *Ethnographies of neoliberalism*. Philadelphia: University of Pennsylvania Press.

Greenhouse, C., Yngvesson, B., & Engel, D. (1994). *Law and community in three American towns*. Ithaca, NY: Cornell University Press.

Gupta, A., & Ferguson, J. (1997). *Anthropological locations: Boundaries and grounds of a field science*. Berkely, CA: University of California Press.

Hall, D. (1997). *Lived religion in America: Toward a history of practice*. Princeton, NJ: Princeton University Press.

Kleinman, A. (1995). *Writing at the margin: Discourse between anthropology and medicine*. Berkeley, CA: University of California Press.

Manning, P. K., & Van Maanen, J. (1978). *Policing: A view from the street*. Santa Monica, CA: Goodyear Publishing Company, Inc.

Marcus, G. E., & Fischer, M. M. J. (1986). *Anthropology as cultural critique: An experimental moment in the human sciences*. Chicago: University of Chicago Press.

Marks, M. (2004). Researching police transformation: The ethnographic imperative. *The British Journal of Criminology, 44*(6), 866–888.

Moskos, P. (2009). *Cop in the hood: My year policing Baltimore's Eastern District*. Princeton, NJ: Princeton University Press.

Nader, L. (1972). Up the anthropologist: Perspectives gained from studying up. In D. Hymes (Ed.), *Reinventing anthropology* (pp. 284–311). New York: Pantheon Books.

Norris, C. (1993). Some ethical considerations on field-work with the police. In D. Hobbs & T. May (Eds.), *Interpreting the field: Accounts of ethnography* (pp. 122–144). Oxford: Oxford University Press.

Reiner, R. (2000). Police research. In R. D. King & E. Wincup (Eds.), *Doing research on crime and justice* (pp. 205–235). Oxford: Oxford University Press.

Reiner, R., & Newburn, T. (2007). Police research. In R. D. King & E. Wincup (Eds.), *Doing research on crime and justice, second edition* (pp. 343–374). Oxford: Oxford University Press.

Rowe, M. (2007). Tripping over molehills: Ethics and the ethnography of police work. *International Journal of Social Research Methodology, 10*(1), 37–48.

Simon, J. (2007). *Governing through crime: How the war on crime transformed American democracy and created a culture of fear*. New York: Oxford University Press.

Stewart, K. (1996). *A space on the side of the road: Cultural poetics in an "other" America*. Princeton, NJ: Princeton University Press.

Varenne, H. (1977). *Americans together: Structured diversity in a Midwestern town*. New York: Teacher's College Press.

Westmarland, L. (2001). Blowing the whistle police ethnography: Gender, ethnography, and ethics. *The British Journal of Criminology, 41*(3), 523–535.

Young, M. (1991). *An inside job: Policing and culture in Britain*. Oxford: Oxford University Press.

Using Prison Ethnography in Terrorism Research

Mark S. Hamm

Every ethnographer has an origin story. Mine began in the early 1970s when I went to work as a teacher for the Arizona Department of Corrections. I was a newly minted graduate of Indiana University's well-known criminology program, but I don't recall learning much about the problem I was about to encounter—violent prison gangs.

We had four gangs at Arizona State Prison, the state's main maximum-security facility, each organized along racial lines for self-protection: the Mexican Mafia, the Mau Maus (a black supremacist gang), the Native Brotherhood, and, the gang that would become the most violent of all, the Aryan Brotherhood. The AB was comprised solely of white inmates serving time for murder or armed robbery, many whom were heroin addicts. In 1975, the Brotherhood made moves to seize control of the drug trade and business rackets from the Mexican Mafia and the Mau Maus, leading to inmate-on-inmate attacks waged with makeshift weapons. Between 1976 and late 1977, these attacks led to 14 murders and 25 stabbings, culminating in a bloodbath that would change the course of Arizona prison history.

On November 3, 1977, the Aryan Brotherhood attacked five rival inmates with shanks. The inmates were repeatedly knifed as many as 20 times, leaving a river of blood and killing one. "It was the day the administration lost charge of the prison and the prisoners," noted an Arizona historian (Price, 2005, p. 48). Twenty-eight members of the Brotherhood were moved to administrative segregation, a high-security lockdown. The attack of November 3 escalated the prison's race and gang tensions, leading to outright war against the AB. The war brought a primitive racial tribalism to the inmate population, one in which whites, blacks, Indians, and Mexicans formed their own standing armies, "each exaggerated by a bizarre spiritualism that often accompanies secret-society crime networks," as a prisoner would trenchantly recall (Hartman, 2009, p. 71). The violence prompted an Arizona legislative investigation

M. S. Hamm (✉)
Indiana State University, Terre Haute, IN, USA
e-mail: Mark.Hamm@indstate.edu

of the prison and an official indictment of the administration, indicating that gangs controlled Arizona State Prison and that this gang warfare had spilled outside to the streets. Heads would roll, reforms were made, but the war went on.

By the early 1980s, I had been promoted to associate warden and was assigned to a Warden's Task Force responsible for reviewing security operations at prisons across the state. During this work, the Task Force learned of a plot hatched by the Aryan Brotherhood at Arizona State Prison to bomb a cellblock housing some members of the Mexican Mafia. Although foiled by guards, it was an audacious plan. The Brotherhood had somehow managed to dig up several feet of hard-rock desert next to the cellblock and had smuggled a load of dynamite into the prison to accomplish the bombing. What impressed me was the sheer determination that it took for a gang of prisoners to even attempt such an act of violence in the first place. And then there were the pragmatics. The gang had to steal shovels from the prison maintenance shop to dig up the caliche. They had to fool, bribe, or otherwise con the guards into releasing them from their cellblock during nighttime hours so that they could work on the hole. They had to acquire a treasure trove of master keys to the gates inside the walls. And most importantly, they had to obtain dynamite while confined to a maximum-security prison. This is what terrorism looked like to my uninitiated eye—highly-organized violence rendered with stealth. Had the AB members succeeded in their plot, every one of them would have faced the gas chamber.

Within the Aryan Brotherhood at the time was a skinny, red-headed, 27-year-old convict from the Arizona border town of Amado named Gary Yarbrough. Yarbrough had been discharged from the Marine Corps for committing robbery in the late 1970s before catching an Arizona burglary conviction in 1980, leading to a 3-year sentence at Arizona State Prison where he was pressed into the AB (Flynn & Gerhardt, 1989). Yarbrough was paroled in 1983; he referred to himself at the time as "worldly dumb but spiritually wise" (quoted in Turner, 1986, n.p.). Based on his previous reading of racist literature mailed to him at Arizona State Prison by the Church of Jesus Christ Christian outreach ministry, Yarbrough relocated with his family to the Aryan Nations compound near Coeur d'Alene, Idaho, where—given his military bearing and criminal bona fides as a tattooed warrior of the Aryan Brotherhood—he became sergeant-at-arms for the Order, or the Silent Brotherhood, in charge of the gang's criminal activities. The Order became the most dangerous domestic terrorist organization ever encountered by the FBI, responsible for assassinations, multimillion-dollar armored truck heists, and bombings across the American West, until their demise in late 1984 (Hamm, 2007).

Today, Gary Yarbrough is 62 years old, in ill health, and serving out a 60-year sentence for his role in the Order at the federal supermax prison in Florence, Colorado, where he is celled up next to the Underwear Bomber.[1] I hope he is granted parole at his next hearing; no longer a threat to society, his radical days are far behind him. Personally, I have much to thank him for. Yarbrough taught me something that I would have never learned from the textbooks of criminology, namely, that there is a potential relationship between an inmate's incarceration in a violent,

---

[1] Gary Yarbrough died in a hospital near the Colorado Supermax prison on April 2, 2018. He was 62.

gang-riddled maximum-security prison and the development of a rare fanaticism necessary for terrorism. This is Yarbrough's lesson: That a terrorist cell can emerge from a prison cell. Gary Yarbrough was not a neo-Nazi terrorist when he entered prison, but prison made him one.

Terrorism Research

In one way or another, every research project I have undertaken over the past 30+ years has drawn from this origin story. My goal has been to explain terrorism from the standpoint of the terrorists themselves through concise case study narratives about the behavioral antecedents and sociopolitical context of terrorism using the prison as a fieldwork setting for ethnographic research. While prisons were my primary setting for these studies, I often spoke with the terrorists' family members, friends, lovers, lawyers, and FBI agents who arrested them. I visited the neighborhoods where they lived and the places they attacked.

After earning a Ph.D. from Arizona State University, I began my academic career at Indiana State University, Terre Haute, Indiana, in 1985. As fate would have it, Terre Haute is home to one of the largest federal penitentiaries in the nation, and over the years I carried out various studies at the prison where I encountered a rogue's gallery of violent extremists, including Timothy McVeigh, members of the Order, and several high-profile jihadists of Al-Qaeda. I interviewed terrorists at other federal and state prisons, including America's original would-be suicide bomber at the Federal Prison Camp in Edgefield, South Carolina, and some two-dozen neo-Nazis and Islamists at California's Folsom Prison, including members of a gang known as Al-Qaeda of California. I also spoke informally with jihadist inmates in Britain and Europe and conferred with counterterrorism experts from around the world.

Like most criminologists, I have published my research in scholarly books, government reports, journal articles, and book chapters. This work has focused on skinheads and hate crime (Hamm, 1993, 1994), a historic prison riot (Hamm, 1995), the Oklahoma City bombing (Hamm, 1997), ethnography and field research (Ferrell & Hamm, 1998), America's terrorist underground (Hamm, 2002), terrorism as crime (Hamm, 2007), prisoner radicalization and terrorism (Hamm, 2013), and lone wolf terrorism (Hamm & Spaaij, 2017).

So, what lessons have I learned about using ethnography in criminology? A short list includes the following.

Ethnography Is Not Cool, Exciting, or Fun

In the tradition of previous ethnographers in criminology (e.g., Adler, 1985; Becker, 1973; Chambliss, 1973; Ferrell, 1993; Lindesmith, 1965; Polsky, 1967; Yablonsky, 1962), I conceive of ethnography as a method used to develop a deep understanding

of social processes, relationships, and meaning-making through direct engagement with criminal offenders. Such an understanding cannot be achieved through quantitative research methods, which is why criminologists turn to the ethnographic approach. Because of the human actors at center stage in the classics of criminological ethnography—wanted career criminals, pool hustlers, heroin addicts and dope smokers, gangbangers, graffiti artists, and cocaine smugglers—there has arisen a perception among some observers, including many students of criminology, that ethnography is a "cool" research endeavor, something exciting and fun. It is not. Ethnography is hard work, frustrating in the extreme. This is especially true for the ethnographic study of terrorists.

Ethnography in terrorism research is beset with problems, many of which are insurmountable. These problems range from researcher safety and the difficulty of gaining access to terrorists to the dreaded Institutional Review Boards (IRBs, more on that later) and the fact that fieldwork is an extremely time-consuming activity. For example, some intrepid researchers have attempted to conduct interviews with terrorists in conflict zones like Afghanistan, Chechnya, and Somalia. One is Adam Dolnik who concluded that: "The majority of time spent in conflict zones involves procrastination, frustration, and idleness, combined with a myriad of phone calls to contacts, and followed by endless hours and days of waiting for a phone call that may or may not be returned…This state of 'confusion' and 'uncertainty about anything' is possibly the greatest danger of field research on terrorism" (Dolnik, 2013, pp. 246–247). An Al-Qaeda expert put it more bluntly: "If you research terrorist groups you will likely kill your academic career before it starts" (quoted in Horgan & Stern, 2013, n.p.). A noted chronicler of research methods in terrorism studies has concluded that "very few published attempts have been made to systematically study terrorists outside of a prison setting" (Silke, 2008, p. 9).

In my research on terrorists in prison, I have experienced every frustration known to criminological ethnography and then some. I have been lied to by prosecutors, defense attorneys, inmates, guards, and prison officials. I have been threatened with physical harm by skinheads, verbally attacked by Al-Qaeda and white nationalist inmates, and denigrated by university gatekeepers for even attempting to interview terrorists because I might trample their rights as "human subjects" by asking questions about their murderous behavior. I was harassed and threatened by an off-the-reservation FBI agent. I sat face-to-face with an extremist in prison who experienced a full-blown psychotic episode. I stood in the rubble of the Oklahoma City federal building and spoke with a tearful 9-year-old boy who had lost his brother in the bombing. I had an existential crisis when—while tracking McVeigh's travels through the Southwest—I decided to sleep in the same bed that McVeigh had slept in at the Dreamland Motel in Kingman, Arizona, before he blew up the federal building in Oklahoma City. I was sued for libel by members of a bank robbery crew known as the Aryan Republican Army and spent a year in court fighting the case. None of this was cool, exciting, or fun.

IRB Approval Is a Necessary Evil

Anyone interested in doing terrorism research in prison must start with this fact: You do not simply call the warden of a maximum-security prison and ask to speak with a terrorist inmate or a radicalized inmate suspected of becoming a terrorist. Because of the heightened state of security in the United States since 9/11, these prisoners live under conditions of hyper-incarceration. To be interviewed, inmates must first be vetted by the Bureau of Prisons, the FBI, and the Department of Homeland Security, with advice from the CIA when foreign nationals are involved. Then written permissions must be obtained from inmates, their lawyers, and university IRBs. In one research project, it took me two years to gain permission to interview just two inmates. This torturous research process is why so few criminologists even try to contact terrorists. As I have written elsewhere, "Today, it may be easier for a convict to escape from an American prison than it is for a criminologist to enter one" (Hamm & Spaaij, 2017, p. 33).

But as torturous as they are, these procedures are necessary for the researcher's survival. I learned this lesson the hard way when I was dragged into federal court by the Aryan Republican Army. The ARA sued me for everything, including my bank holdings and university retirement fund. Because of administrative obligations integral to my IRB approval, however, Indiana State University provided me with excellent legal representation, and the case was put to rest.

Listen, Show Respect, and Be Honest

The objective in this type of research is to conduct face-to-face interviews with terrorist inmates or inmates who have become radicalized toward terrorism during their imprisonment. Inmate interviews are not freewheeling conversations; rather, they are semi-structured discussions intended to elicit information on how a person *evolves* into a terrorist. Discussions start with small issues that the inmate will likely be comfortable with and gradually proceed to larger matters concerning their radicalization. Therefore, the interviewer must be an active listener. Academics are not a group widely known for their interpersonal skills. As Dolnik points out, "most academics are not very good listeners," and, in interviewing terrorists, "academics will typically find themselves talking precisely to simple, not very well-educated men… who tend to have a very one-dimensional view of the world" (Dolnik, 2013, p. 242). Yet active listening is a requirement for empathy, the knowledge of the plight of another, and the basis for *thick description* (also known as emic interpretations), conveying in academic writing what experience itself would convey (Geertz, 1973). As noted elsewhere in this book, cultural criminologists working in the tradition of Max Weber refer to empathy as *verstehen*, denoting a process of subjective interpretation on the researcher's part or a degree of sympathetic understanding between researcher and subjects of study (Ferrell & Hamm, 1998).

It is within this context that the face-to-face prison interview achieves its superiority as a research method in terrorism studies. It allows the researcher to make a personal connection, not to a "human subject," but to another human being—in this case, a terrorist. Prison ethnography is more than interviewing, though, since fieldwork also allows the researcher to make contemporaneous observations on inmates' speaking style, sibilance, the way they handle both joyful and painful memories, the physical and psychic toll prison has taken on them, how they carry themselves around other inmates, and what guards say about them. These interviews and observations are the essence of primary data on terrorism because it represents information that has never been seen in open sources.

These ethnographic techniques only work, of course, if there is a rapport between the researcher and the inmates being interviewed, so let me offer two suggestions in this regard. First, appearance (or the "presentation of self") matters within the inmate society. It always has. I am not qualified to make a sweeping statement about appearance but can only say what works for me. I am a longtime admirer of the post-War bluegrass musicians who once ruled the roost at Nashville's Grand Ole Opry. None were more influential than the legendary Bill Monroe who insisted that he and his band, the Blue Grass Boys, always dress in suits, white shirts, and black ties when performing. Bill Monroe's intent was not to impress his audience but to show respect for the men and women who came out to enjoy music celebrating their rural heritage. So that's it for me. I dress like Bill Monroe out of respect for the convicts who agree to sit for interviews.

Second, rapport demands truth from the researcher, along with a measure of humility. It is no exaggeration to say that the average prison inmate has a finely tuned bullshit detector. Inmates can see shuck and jive coming a mile away. For the time it takes to do an interview, the researcher lives in the inmate's world. This is not a world of academic debate over contestable facts but a world of overcrowded cellblocks where prisoners have little use for social constructions of truth. John Irwin (2007) called this the warehouse prison, where convicts spend their time "wheeling and dealing" in contraband and displacing anger and rage against anybody who gets in the way. It is a miserable place where life is cheap. Strong warning is needed for anyone living there. As Bob Dylan famously sang about the penitentiary back 1966, "To live outside the law, you must be honest."

How Does It Feel?

Emotions are rarely displayed in the maximum-security prison. Emotions are better concealed in this environment because their expression might be read as a sign of weakness and weakness in prison can be fatal. As a prison ethnographer, I experience numerous emotions while interviewing terrorist inmates. Some of these inmates are incessantly angry, narcissistic, conniving, racist, and unrepentant for their violence. They are detestable human beings and I loathe them, but always in secret. Like anyone else in the maximum-security mix, I conceal my emotions.

Other terrorist inmates are remorseful for their crimes, vulnerable, easy to talk with, and exhibit an honest attempt to cope with their pains of imprisonment. They just want to do their time and go home. For them, I feel nothing but compassion since many of them are lifers who will never see the light of freedom again; but once more, I keep my feelings on the down low. Most of the inmates I deal with are damaged souls struggling with memories of childhood trauma, joblessness, divorce, drug addiction, and psychological trouble. In many ways, they are a product of the exclusive society. This is the work I have taken on and it has changed me. It has caused me to look at the world "through a glass, darkly" as St. Paul said to the people of Corinth. The Germans have a word for a poetic sensibility arising from such misery: *Weltschmerz*, or world-weariness. For me, writing is art therapy for world-weariness. Nietzsche said it best when he advised: "to live is to suffer, to survive is to find some meaning in the suffering."

My goal is to use such meaning-making to answer a basic question: What makes a young person adopt extreme political views, and how does that transformation lead to bombing and mass murder? It takes time to answer that difficult question, time for obtaining an endless series of official permissions, for traveling and building relationships with inmates, for listening to their stories about the world they lived in, and time for writing compelling narratives about their pathways to radicalization. People ask me why, given its many obstacles, I even bother with this kind of research. I do so because it is honorable work, something to be proud of because it sustains American penological values of redemption and rehabilitation. Consider the alternatives. To understand why terrorism occurs, since 9/11 government officials have subjected imprisoned extremists to such hidebound methods of penology as solitary confinement, interrogations in "black sites," and the use of torture (so-called "enhanced interrogation" techniques), often with dubious results and devastating geopolitical consequences as witnessed at Abu Ghraib and Guantanamo Bay and the subsequent revenge attacks against the West by Al-Qaeda and its affiliates. Yet if more criminologists were to use a relational approach that incorporates empathy and listening presence with extremists in prison, perhaps they could cultivate more informed explanations of radicalization, thereby reducing terrorism.

References

Adler, P. A. (1985). *Wheeling and dealing: An ethnography of an upper-level drug dealing and smuggling community*. New York: Columbia University Press.

Becker, H. S. (1973). *Outsiders: Studies in the sociology of deviance*. New York: Free Press.

Chambliss, W. J. (1973). The saints and the roughnecks. *Society, 11*, 24–31.

Dolnik, A. (Ed.). (2013). *Conducting terrorism field research: A guide*. New York: Routledge.

Ferrell, J. (1993). *Crimes of style: Urban graffiti and the politics of criminality*. New York: Garland.

Ferrell, J., & Hamm, M. S. (Eds.). (1998). *Ethnography at the edge: Crime, deviance, and field research*. Boston: Northeastern University Press.

Flynn, K., & Gerhardt, G. (1989). *The silent brotherhood: Inside America's racist underground*. New York: Free Press.

Geertz, C. (1973). *The interpretation of cultures: Selected essays*. New York: Basic Books.

Hamm, M. S. (1993). *American skinheads: The criminology and control of hate crime*. Westport, CT: Praeger.

Hamm, M. S. (Ed.). (1994). *Hate crime: International perspectives on causes and control*. Cincinnati, OH: Anderson.

Hamm, M. S. (1995). *The abandoned ones: The imprisonment and uprising of the Mariel boat people*. Boston: Northeastern University Press.

Hamm, M. S. (1997). *Apocalypse in Oklahoma: Waco and ruby ridge revenged*. Boston: Northeastern University Press.

Hamm, M. S. (2002). *In bad company: America's terrorist underground*. Boston: Northeastern University Press.

Hamm, M. S. (2007). *Terrorism as crime: From Oklahoma city to Al-Qaeda and beyond*. New York: New York University Press.

Hamm, M. S. (2013). *The spectacular few: Prisoner radicalization and the evolving terrorist threat*. New York: New York University Press.

Hamm, M. S., & Spaaij, R. (2017). *The age of lone wolf terrorism*. New York: Columbia University Press.

Hartman, K. E. (2009). *Mother California: A story of redemption behind bars*. New York: Atlas.

Horgan, J., & Stern, J. (2013). Terrorism research has not stagnated. *The Chronicle of Higher Education*, May 8. Retrieved June 16, 2014.

Irwin, J. (2007). *The warehouse prison: Disposal of the new dangerous class*. New York: Oxford University Press.

Lindesmith, A. F. (1965). *The addict and the law*. Bloomington, IN: Indiana University Press.

Polsky, N. (1967). *Hustlers, beats, and others*. Garden City, NY: Doubleday.

Price, T. W. (2005). *Murder unpunished: How the Aryan brotherhood murdered Waymond Small and got away with it*. Tucson, AZ: University of Arizona Press.

Silke, A. (2008). Research on terrorism: A review of the impact of 9/11 and the global war on terrorism. In H. Chen, E. Reid, J. Sinai, A. Silke, & B. Ganor (Eds.), *Terrorism informatics: Knowledge management and data mining for homeland security* (pp. 27–49). New York: Springer.

Turner, W. (1986). 5 neo-Nazis get stiff sentences for crime spree. *New York Times*.

Yablonsky, L. (1962). *The violent gang*. New York: Macmillan.

Searching for Glimmers of Ethnography in Jailhouse Criminology

Jacqueline B. Helfgott

Searching for Glimmers of Ethnography in Jailhouse Criminology

When I am faced with the question, "What made you want to be a Criminal Justice PhD/Professor/Researcher?" I am often at a loss as to how to reply because I can't decide which part of the answer I am willing to share. Usually my response is catered to the audience. If it is a criminal justice professional, I will say, "I read Joseph Wambaugh's (1973) *The Onion Field* and then was offered ride-a-longs with police while I was in college working as a waitress and became interested in policing," or "I was an intern for the Department of Corrections and worked with some amazing community corrections officers and became fascinated with corrections and prison subculture and reentry," or "I went on a tour of Walla Walla Prison and saw Kevin Coe himself right after he was sentenced and read Jack Olson's (1983) book *Son: A Psychopath and His Victims* and Cleckley's *The Mask of Sanity* and Rule's (1987) *Small Sacrifices* and was hooked on psychopaths because I couldn't understand how they could so blatantly hurt other people without remorse." Sometimes if I am feeling especially open, I will share that I grew up with a bunch of juvenile delinquents; and of that group I was the only one to graduate high school, let alone go on to get a PhD; or that when I was 21 years old, I really wanted to be a police officer, but when I had the application in my hands, I was too afraid with my background and the company I kept that I would not pass the background process; or that I had a boyfriend I lived with in college who I used to yell out to while riding my exercise bike and reading Yochelson and Samenow's (1976) *Criminal Personality*, "Hey, you're in here!" so naturally criminal justice was the discipline I chose. Or I will say, I didn't really want to be a professor at all; I wanted to work as

J. B. Helfgott (✉)
Seattle University, Seattle, WA, USA
e-mail: jhelfgot@seattleu.edu

a prison psychologist because I was fascinated with the criminal mind and prison subculture, but I got derailed. Or I will mention that I come from a long line of social activists including my mother who had me help her and her 1970s women's movement friends silkscreen the fist inside the women's symbol on T-shirts when I was barely 8 years old and my grandmother who was the president of the Gray Panthers of Seattle.[1] She and her friends were left-wing elderly activists, some of whom liked to lay down on train tracks and ended up in jail. My grandma used to say, when I would go to the prison to do research, "Please say hello to the prisoners for me." All of this is probably why I ended up a criminal justice professor. But it is safe to say that I have never felt comfortable in the halls of academia. I struggle in every professional academic event to fit in. Years ago (when I used to smoke cigarettes) when I was required to attend an academic event, I would find myself in conversations I had no interest in, hearing and using words I didn't understand or that didn't mean anything to me. I would have to go outside every 15 or so minutes to give myself a break to smoke a cigarette before I could go back in. By night I would go to the prison for my volunteer or research work, and there I would comfortably interact with the inmates and the officers who all couldn't wait to go out for their smoke breaks. We would talk about all kinds of stuff that mattered—real things, not just ideas. I have always felt more comfortable in police cars, courtrooms, and prisons whether I was interacting with the lawbreakers or the lawmakers.

I knew I wanted to do my graduate work on psychopaths before I entered graduate school at Penn State in 1989. The results of my dissertation—an empirical test of Gacono and Meloy's (1988) "levels hypothesis" that examined the relationship between unconscious defensive process and conscious cognitive style in psychopaths (Helfgott, 1991, 1992)—were published in two articles (Helfgott, 1997a, 1997b, 2003) and have been summarized in Gacono and Meloy's (1994) book *The Rorschach Assessment of Aggressive and Psychopathic Personalities* and Helfgott's (2018) *No Remorse: Psychopathy and Criminal Justice*. This research was an unusual focus in a criminal justice department at the time. In the late 1980s there was a great deal of suspicion regarding biological and personality theories of crime (unlike today, with the resurgence of interest in this area and with the new biosocial criminology section at the *American Society of Criminology*). I remember going to the *American Society of Criminology* and *Academy of Criminal Justice Sciences* meetings to panels on psychopathy and serial murder, and the same handful of scholars would be in attendance (usually Adrian Raine, Steve Egger, Erick Hickey, Ron Holmes, Philip Jenkins), and I was warned by the sociologists and legal scholars in my department of the dangers of locating criminal behavior in personality traits. After I completed

---

[1] In my first year as an assistant professor, I conducted a study that allowed me to interview my grandma and all of her left-wing activist friends. This was one of my early attempts to conduct research that captured the experiences of subjects that were grounded in my own subjective knowledge of the subjects. My grandma, her friends, and this habit of generating research from my own lived experiences have been a major influence in my attempts to incorporate the spirit of ethnography in my work. The results of this study and a subsequent study conducted on right-wing elderly activists were published in Kinney and Helfgott (2000) and Kinney and Helfgott (2004).

my dissertation defense, one of my professors shook my hand and said, "Congratulations, but I wish you would have chosen a different topic."

I can still remember the day I sat in my bedroom in my apartment staring at my data—the "0s" and "1s" entered into what is now called "antique software"—the black Turbo Pascal screen imported into the old windowless SPSS using syntax I was allowed to count to fulfill the requirement of my graduate foreign language. I lived in the largest room of a shared apartment and had my own door where I could go outside and sit under a tree. For my master's thesis, I administered the *Psychopathy Checklist—Revised* (Hare, 1991) and the *Rorschach* test to antisocial personality disordered prisoners in medium- and maximum-security prisons in Pennsylvania. For my dissertation I showed a sample from my first study the video clip of the rape scene from the film *The Accused* (Kaplan, Jaffe, & Lansing, 1988) followed by administration of a survey instrument designed to measure primitive borderline defenses. I used to go out my bedroom door and sit under the tree to read and review and score the Rorschach protocols. I still remember the day after I entered all of my data. I had gone under the tree to review my research notes and then went back inside to sit at my computer to run the analyses. I looked at the numbers on the screen excited to see whose scores would land them in the primary, secondary, and non-psychopath categories and was overcome with the awareness that I was putting real people in these boxes. Even though my subjects were violent men who had committed heinous crimes,[2] I couldn't shake the thought of how wrong it felt to reduce these complex human beings into these numbers on my screen. I believed in the research, but it just seemed wrong.

At the Edge of Ethnography

Because my area of research interest was psychopathy and the role the construct plays in the criminal justice system, I did not consider ethnographic field work an option for my work. I was tasked with measuring personality variables using psychometric tests and diagnostic classifications, some of which are the products of long discourse on the subjective nature of their development. Psychological research is in many respects the opposite of the sociological-anthropological ethnographic lens in the sense that it situates the researcher as an outsider looking in to identify features of psychopathology for the purpose of categorizing and classifying. Understanding is of course a primary goal as well, but that understanding is a much more distant sort of understanding than is offered through ethnographic research.

---

[2] The prisoners in my study were diagnosed with antisocial personality disorder and housed in medium- and maximum-security prisons. One had kidnapped a woman in the middle of winter, raped her, tied her to a chair, and set her on fire while alive, another had stabbed a man 67 times, one had tortured and murdered two people, and another had a swastika tattooed onto his forehead, to give just a few examples. I even had one subject who had tortured and murdered two people who told me that, if given the opportunity, he would choose to have brain surgery to fix him because if he were set free, he would kill again and would have no problem raping and murdering me.

However, primary data collection is a form of field work in and of itself[3] that helped me understand how, with a little methodological creativity, elements of the ethnographic lens could be embedded in any traditional mixed-method research design.

While I was in grad school, the professor I worked for as a TA and RA, Bill Parsonage, was married to an artist Sue Parsonage. Sue was a member of the Pennsylvania Prison Society and was looking for artists willing to do classes in the Bellefonte County Jail. She couldn't get any takers in the art program at Penn State, and when I heard about this, I told her that I originally had set out to major in art but changed my mind and that I would love to do it. I started facilitating art classes which was the beginning of what turned out to be a 20+-year habit of volunteering in prisons to do art that operated in a strange sort of parallel track alongside of my research.

My first teaching job was at Montana State University. One of the classes I taught was corrections, and my goal in teaching the course was to make sure the students had opportunities to learn from the prisoners themselves. I took them to Deer Lodge prison and the women's prison (which at the time was a small facility, a former mental hospital, that housed 55 women), and I assigned students to do creative projects that enabled them to learn the experience of prisoners in their own voices. Students were given license to do whatever they wanted, but it had to be creative. One group interviewed an inmate who worked in the Deer Lodge Hobby Shop and made a video of the interview. Another group acted out an execution including the inmate, executioner, and pastor, while reading poetry written by death row inmates, using a prop that was a homemade electric chair complete with a Chevy hubcap for the execution cap and horse harnesses for the arm and leg straps.[4]

---

[3] While it is beyond the focus of this study to elaborate on the many experiences I had while collecting data in prisons, it is impossible to ignore and, central to the spirit of ethnography, to acknowledge how much these experiences have shaped my research. For example, the first day I entered the maximum-security prison where I conducted my dissertation research, I dressed professionally in a skirt and heels. I was sternly reminded of my training on safety and contraband by the officer at the front door. I was escorted by the prison psychologist through cell blocks to the area I was designated to conduct my research interviews which can only be described as a dungeon in the sky. I had to be locked in when I had to use the bathroom and was given an emergency number I was to call on a phone with a red button. To this day I can remember the clanging of the doors, the cold smell, the hollers and sounds, the psychologist who told me stories about the recent Camp Hill prison riots being worse than he had seen in Vietnam, and the naked overweight man inside the last cell on the left who stuck his penis out of the cell bars as I walked by, and how strange it was that the first man I interviewed was so strangely different than the file I had read about him torturing, raping, and setting a woman on fire. I went home that night and made the mistake of calling my mom who then told me, "You will not go back into that prison!" I of course had to go back into the prison since it had taken me an entire year to obtain access, but I was shaken enough to call the psychologist I was working with to ask him if it would be possible to be in a different room where I was not alone and did not have to walk through that particular cell block. He then informed me that when I came in the next day, I should dress in the largest coat and bulkiest clothing I could find and he would find a room for me in the mental health unit. While I have become much more accustomed to all that comes with conducting research in the prison setting over the years, the subjective experience of the researcher as he/she moves through the process of navigating these and other criminal justice contexts cannot be underestimated in terms of the impact on research and the researcher role.

[4] This homemade "electric chair" traveled with me to Seattle University and to this day sits in my

I left Montana State University to go to my hometown to take a position at Seattle University in 1993. I was assigned to teach corrections and deviance courses. Whenever I could, I used prisoner writings in my classes, including Jack Henry Abbott (1991). I quickly got hooked up with the Concerned Lifers Organization at the Washington State Reformatory and started a program called the Creative Expressions Project that involved weekly classes where I would work with inmates and SU student volunteers on art and writing projects. I did the program at WSR for over 20 years and for a few years also at the Washington Corrections Center for Women. We did all kinds of stuff during those years—we got a grant to do a Pike Place Market Pigs on Parade; we constructed a Trojan horse-type giant pig inside WSR that had to go through all kinds of security checks because a person could hide inside. The pig was covered in 80,000 beads, was featured in the Pike Place Market "Pigs on Parade"[5] (MacKenzie, 2007; Pike Place Market Foundation, 2001), and was sold for $2500 donation to the Pike Place Market Foundation. We also designed metro bus stops and giant sculptures made of devil heads, published a prison "zine" called *Sounds of a Grey Metal Day* that ran for several years, and organized a prison art show at the Seattle University College of Arts and Sciences art gallery.

All during the 1990s, I taught our department's undergraduate deviance and social control course. I required students do mini covert ethnographies where they would become a part of a "deviant" subculture—they did projects on Fetish night at the Vogue, strip clubs, and S&M clubs, pan-handled, spent the night at the mission, and I became fascinated with the covert ethnography studies. During those years teaching deviance, one of my most memorable moments was discovering the book *Ethnography at the Edge* (Ferrell & Hamm, 1998) and reading about "jailhouse sociology" and criminological *Verstehen*. A few years before that, I had done a study examining the relationship between ex-offender needs versus community opportunity in Seattle (Helfgott, 1997a, 1997b). Every Friday I walked down the hill to the King County Jail and to a local ex-offender transition agency to interview inmates about their experiences. I remember thinking when I read the introduction to *Ethnography at the Edge*, "Oh no, I am a jailhouse criminologist!" I thought about my psychopathy research, reducing the inmates to those numbers that put them in psychopath boxes, and the study I had just done, conducting surveys in the jail. I was doing jailhouse criminology, not ethnography at the edge.

But then I wasn't. In a parallel track, I was also doing art in prison as an official volunteer sponsor. In 1993, during my first quarter at Seattle University, a student in my introduction to criminal justice course brought up in class that she worked with

---

office. When people notice it, it is a powerful symbolic reminder of the reality of capital punishment and the execution process. The use of physical artifacts such as this is yet another way to invoke attention to the realities of the subjects of criminological and criminal justice research.

[5] This was a citywide project where artists could submit proposals to be awarded the materials to create a pig. The program I facilitated—*The Creative Expressions Project* partnered with local artist Kathleen McHugh to apply for the project and received an award. This required considerable negotiation with prison administration and staff to bring a giant Trojan-horse sized pig (that was hollow enough to house a person inside) into the prison facility (For the final product, see: http://www.pigsonparade.org/HogHeaven/PigFinder/ and is featured in the book *Pigs on Parade* (Pike Place Market Foundation, 2001).

a man who was a former prisoner who had been fired from his position after his employer found out he had been in prison for murder.

I spoke with the student after class and ended up meeting her co-worker, who introduced me to Jon Nelson, the lead volunteer sponsor for the Concerned Lifers Organization at the Washington State Reformatory, who worked tirelessly on behalf of the incarcerated until his death in 2011 (Gilmore, 2011).

After that for over 20 years, every Friday night and for some years on Tuesday and Friday nights at both the men's and women's prisons, I made the one-hour drive through rush-hour traffic to do creative projects—art and writing with the prisoners, in a program I developed with the prisoners called the *Creative Expressions Project*. This was a completely volunteer activity that gave me a different perspective than I was experiencing in my role as a researcher. It's probably safe to say that I have learned more about criminal behavior and prison culture and the experiences of being incarcerated while working side-by-side painting prison murals with the prisoners than I've learned doing formal research surveys.

I was not concerned in this volunteer prison art program context with being an objective social scientist. However, I was a Washington State Department of Corrections (WA DOC) volunteer sponsor, and there was an entirely new set of rules and policies and challenges that went along with that. The training at DOC was straight out of *Games Criminals Play* (Allen & Bosta, 1981) emphasizing boundary issues, prison rules and policies, and the potential for the prisoners to engage in manipulation as a function of both their personalities and the prison environment. I had had this training as a researcher, but it was entirely different in the realm of volunteer programs where the interactions between the volunteer staff and the prisoners were much more informal and without the imposed boundaries of traditional (jailhouse criminology) research protocols.

My experiences as a prison volunteer sponsor, my interactions with the prisoners, and the courses I taught in my early years as an assistant professor naturally informed my research. The prisoners pitched idea after idea, and the more I became entrenched in the prison subculture, the more developed my ideas became about the programs and research I/we wanted to do. Ultimately, these experiences led to years of studies in corrections and reentry that were almost entirely informed from the ground up by my experiences with the prisoners—the things I heard them say, the ideas they had about what would make a difference for them, and the interactions between the prisoners and the correctional staff and administration.

In my role as a prison volunteer sponsor, I knew the prisoners were not my friends, except they felt like my friends. I was 8 months pregnant, going into the prison almost every night for both my art program and my research. I went to a Metallica cover band concert inside the prison only a few days before my daughter was born, a month early. The prisoners made my daughter a ceramic skull rattle, a Native American rattle, a turquoise ceramic teddy bear mobile for her room, a book with letters to her about how great her mom was, and a 6-foot-tall poster of my face signed on the back by all of them. They even wanted to make my daughter a crib,

but I told them it had to be up to current safety standards.[6] Also during those years, we engaged in public arts projects, such as the painted bus stops, murals for community centers, and the fiberglass pig mentioned earlier. It was a heyday of sorts in terms of the level of interaction with the prisoners and their families (who assisted in organizing the art show). I felt more comfortable interacting with the prisoners than I did interacting with the faculty at Seattle University during those years and probably still today. Over 20 years later, I am still in touch with many of these people, many of whom have been out now for many years.

During this time, it was hard to think about myself as a "jailhouse criminologist." I couldn't reconcile the side of me that made every effort to learn and understand the people I was studying from their perspectives with the side of me that administered psychometric tests and conducted traditional surveys and interviews. I had never done my own covert ethnography like the authors whose texts I was using (e.g., *Ethnography at the Edge* and an early edition of Adler & Adler's (2015) *Constructions of Deviance*). I had heard stories at conferences talking to colleagues who had done covert ethnographies. I met Randy Blazak at an ASC conference in the mid-late 1990s and heard all about his research on neo-Nazi skinheads. I read anything I could get my hands on by Jeff Ferrell and Mark Hamm and watched from afar the group of ethnographic researchers at the academic conferences who would play jazz in their hotel rooms at the conferences. As much as I admired these researchers, I did not know them and watched them with great admiration from afar. I couldn't for the life of me figure out how they had gotten their research projects through their university institutional review boards, not to mention that I had read Jeff Ferrell's articles about getting put in jail himself during the course of his graffiti research and that wasn't something I was eager to put on my CV as an assistant professor or as a new mom.

But I did want my research to capture *criminological verstehen*—"a researcher's subjective understanding of crime's situational meanings and emotions—its pleasures and pain, its emergent logic and excitement—within the larger process of research" (Ferrell, 1998, p. 27). I set out to figure out how I could incorporate the ethnographic lens and *criminological verstehen* in my traditional mixed-method research designs, determined to be just a little cool, while maintaining the traditional jailhouse criminology approach. I suppose rather than "ethnography at the edge" I was at the edge of ethnography. To this day, though almost all of my research has involved primary data collection in an attempt to immerse myself in the lived realities of my research subjects, I have never conducted an ethnographic study. I haven't even considered it. My research has been mixed-method designs collecting primary data through surveys, observation, participant observation, and interviews. However, in every study I have conducted, I have tried to keep a glimmer of the

---

[6] In case there is any question that I was violating the well-established rule "anything that goes in the prison must come out and anything in the prison must stay in," the prisoners had authorization from prison administration/staff to give me these items made in the prison hobby shop. During this time in the 1990s, there was an active prison hobby shop, and prisoners were able to sell and give items they made to their families, volunteers, and correctional staff.

ethnographic lens with hope that there is some merit in ethnography influenced jailhouse criminology. So, I ask: *Is it possible to conduct research using traditional methods that include creative elements that ensure that research subjects' voices are heard and that they are respected and understood as complex human beings? Can criminological Verstehen be accessed using a survey? Or in some other way besides becoming a member of a subculture to which I do not belong? Was I already a member of this subculture as a volunteer art instructor on Friday nights but then on Monday nights I went into the prison as a researcher? Is it possible to conduct ethnographic research using traditional mixed-method research designs? Do my own early experiences with delinquency influence my research and enable me to understand my subjects of study through my past while not partaking in deviant or criminal behavior in the present? Is there such a thing as ethnography-infused mixed-method research design?*

Incorporating the Spirit of Ethnography in Mixed-Method Research Designs

The ethnographic method is not suitable for all research questions, but there are ways to incorporate the spirit of ethnography into more traditional mixed-method research designs. What I offer here is a blueprint of sorts to encourage methodological creativity in traditional mixed-method research designs to maintain the spirit of the ethnographic lens. I offer experiences/stories illustrating successes and failures in my own attempts to incorporate the spirit of the ethnographic lens in prison and reentry-based research to highlight issues I faced such as maintaining boundaries while listening to and treating people with respect as human beings, struggles with prison security issues and logistics, research ethics, developing trust as an outsider, and the difficulties in measuring phenomenological experience through the traditional research process.[7]

I often find myself reading studies that utilize secondary data and/or focus on sophisticated statistical analyses using large data sets asking myself, these are just numbers, where is the person in this? Much of the research on psychopathy involves factor analyses and item response theory and study after validation study on psychometric tests. The go-to outcome variable in criminal justice is "recidivism,"

---

[7] I focus here on work I have done in corrections and reentry; however, I have incorporated the spirit of the ethnographic lens in all of my research including research evaluations of law enforcement training and community-police initiatives including qualitative methods to capture the more nuanced responses of subjects to supplement traditional survey methods (Helfgott, Atherley, et al., 2015; Helfgott, Conn-Johnson, & Wood, 2015; Helfgott & Parkin, 2017; Helfgott, Strah, Pollock, Atherley, & Vinson, 2018; Parkin, Helfgott, Collins, Messelu, & Krappen, 2015), in research on the experiences of women criminal justice professionals (Gunnison & Helfgott, 2018; Parkin, Helfgott, Collins, Messelu, & Krappen, 2018; Helfgott et al, 2018), and incorporating narratives and analysis of manifestos in my work on psychopathy (Helfgott, 2004) and copycat crime (Helfgott, 2014; 2015).

but no matter how many variables are used to measure recidivism—arrest, conviction, and re-incarceration—none of them tell anything about how people who engage in crime see themselves in relation to other people, how they see the world, or their phenomenological experience of crime and desistance and personal growth and change. With the bell and whistle advances in social science research, it is difficult to remember that crime is ultimately about one human being harming another human being and that the criminal justice processes involved in responding to crime are about human beings making judgments and decisions about other human beings. The human motivations, meanings, feelings, cognitions, interactions, situations that explain crime, and its response are messy and complicated and sometimes inexplicable, and the more we distance ourselves from the person or people we are studying, the more the results of our research will be academicized smoke and mirrors—fictions far removed from the real lived experiences of research subjects. Here are some of my attempts to capture some of these real experiences in traditional mixed-method research designs in corrections and reentry research.

Citizens, Victims, and Offenders Restoring Justice

In the late 1990s I received two grants from the Soros Foundations *Center for Crime, Communities, and Culture of the Open Society Institute* to develop and evaluate a program called "Citizens, Victims, and Offenders Restoring Justice." Results from the 3-year program implementation and evaluation are published in a series of articles, and the program was replicated in a dozen or more prisons in the United States (Helfgott, Lovell, & Lawrence, 2002; Helfgott, Lovell, Lawrence, & Parsonage, 2000a, 2000b; Lawrence, Lovell, & Helfgott, 2004; Lovell, Helfgott, & Lawrence, 2002a, 2002b).

The program was the product of discussions with prisoners at the Washington State Reformatory with whom I regularly interacted in my role as a volunteer sponsor with the Concerned Lifers Organization. It was also the product of some of the conversations I heard when I took my students on prison tours (Helfgott, 1997a, 1997b). For example, on one of the prison tours, a prisoner told one of my students that no one should get more than a five-year sentence for homicide because studies show that the family members of homicide victims stop visiting the graves of their murdered loved ones within five years. I had also testified before the legislature speaking against House Bill 2010, which passed and put forth major correctional reform in Washington State that removed opportunities for higher education in the prison and was struck by the polar perspectives of the victims, offenders, and correctional staff with the victims on one side with the view that prisoners should not have access to higher education and programs and the offenders and correctional staff on the other, with correctional staff in some cases supporting the offenders in terms of the need for prison programs that serve as a way to manage a safe prison environment. I attended a town meeting hosted by popular local television host where some of the lifers communicated via satellite where the victims and families

of victims were livid with the offenders. When I went back into the prison that night, the prisoners expressed shock at the outrage they received from the audience as if they expected the victims' parents to automatically embrace and forgive them.[8]

There was a powerful victim's advocacy movement in Washington State at the time led by Ida Ballasiotes whose daughter Diane was murdered in broad daylight in downtown Seattle's Pioneer Square by a sex offender on work release. After her daughter was murdered, she became a citizen activist and a five-term state representative who fought for get-tough laws. She and other victim advocates, including members of the organization Families and Friends of Violent Crime Victims (now called Victim Support Services), founded by relatives of the victims of Ted Bundy, had a powerful presence in Washington State[9] and led to the enactment of the 1990s get-tough laws (Helfgott, 2008, pp. 203–205).

All of this motivated me to develop a program that brought together the polar groups—the victim, offenders, and community members involved in the 1990s get-tough legislation discourse—and with the help of the prisoners, victim advocates, community members, and colleagues, I applied for and received grant funding to implement and evaluate a prison-based encounter program in which offenders, community members, and surrogate victims met, told their stories about how crime impacted their lives, and worked together to come up with concrete ways to repair harms and/or assist victims with whatever they needed. The program ran for 3 years including five 12-week seminars and was evaluated through pre-post surveys of all participants, participant observation.[10]

This project was an example of an attempt to integrate aspects of the ethnographic method in a number of ways. First, a large developmental committee was formed to design the program and its evaluation including prison administrators and staff, lifers, victim advocates, clinicians, community members, researchers, and student research assistants were included. This ensured that the program itself would be designed by its participants (all members of the committee also participated in one of the five seminars, and some participated in all of them). Second, the research design incorporated a mixed-method design utilizing a pre/post instrument that offered participants opportunity to answer Likert scale questions, yes/no questions, and open-ended questions, observation, participant observation, and post-program focus group. The program also provided participants opportunity to develop concrete ways to repair harms associated with the crimes that the participants discussed.

---

[8] This occurred at a very intense time in Washington State in the mid-1990s just after the passing of the Community Protection Act of 1990 enacting civil commitment for sexually violent predators and sex offender registration and the Persistent Offender Accountability Act of 1993 (aka "Three Strikes").

[9] See https://www.crimemuseum.org/crime-library/serial-killers/ted-bundy/.

[10] At one point during the three years of this program, Howard Zehr, widely considered the founding father of restorative justice whose work at Graterford Prison in Pennsylvania was in part the inspiration for the program, came to Seattle to visit and speak with the participants of the program. This was yet another example of a reflexive relationship between the research and subjects in an attempt to introduce the subjects of the study to the scholarly origins of the principles of restorative justice at the heart of the program and research endeavor.

Third, the participant observation component of the project attempted to measure phenomenological change in participants by noting in great detail changes in relationships, ways of thinking, and reactions of participants. Finally, the entire program grew from the lived realities of the participants and the developments that unfolded in Washington at the time.

Ex-Offender and Community Corrections Perceptions of Reentry

Another line of research in which I attempted to incorporate aspects of ethnographic lens was a series of studies on offender reentry. In the 1990s I conducted the study "Ex-Offender Needs versus Community Opportunity in Seattle" (Helfgott, 1997a, 1997b). This project grew out of my work as a board member for an ex-offender resource referral center called *New Connections*. I wanted to investigate the experience of ex-offenders as they returned to the community from an ecological perspective, by applying Robert Johnson's (2001) prison-based mature coping model to the community. This study involved surveys of employers, landlords, universities, community agencies, and interviews with former prisoners to understand the ecological environment to which prisoners were released in Seattle. This study and subsequent studies included interviews with former prisoners. This qualitative component allowed for the views and perspectives of the offenders themselves to be a critical component of the data.

One piece that was missing in the 1997 study was the voices and perspectives of community corrections officers. Subsequent studies included interviews with both former offenders (some of whom were prisoners at the Washington State Reformatory who had participated in the Citizens, Victims, and Offenders Restoring Justice study or Creative Expressions who had been released) and community corrections officers (some of whom were former Seattle University criminal justice students now employed as community corrections officers) to better understand the lived realities of both ex-offenders and community corrections officers throughout the release and reentry process (Gunnison & Helfgott, 2007, 2011, 2017; Gunnison, Helfgott, & Wilhelm, 2015; Helfgott & Gunnison, 2008). This research ultimately culminated in a one-year study involving interviews with ex-offenders and community corrections officers published in a book *Offender Reentry: Beyond Crime and Punishment* (Gunnison & Helfgott, 2013).

This research incorporated the ethnographic lens in three ways: First, it involved qualitative data collection that honored the experiences of the subjects. Second, it evolved through conversations and feedback with community corrections officers who expressed concerns that their voices were not heard in the earlier (1997) study. Third, the research ultimately led to a book-length qualitative study in which at least some of the participants were known by the researchers from prior prison programs and research and/or as students at Seattle University. Thus rather than an outsider

view where the researchers are outsiders examining their subjects, my colleague Elaine Gunnison (who interviewed the CCOs) and I (who interviewed the ex-offenders) had prior experience with many of our subjects, casting us as participant observers of sorts.

Evaluating the IF Project: IF Project Writing Workshop and the Seattle Women's Reentry

A final example of a mixed-method research design that attempted to incorporate the spirit of ethnography is a series of studies evaluating the Seattle Police Department's IF Project.

The IF Project, begun in 2008, is a crime reduction and crime prevention program coordinated by the Seattle Police Department Community Outreach Unit; it includes components that bridge law enforcement, corrections, juvenile justice, truancy programs, schools, and community agencies. It centers around a prison-based writing workshop in which incarcerated individuals are posed the "IF Question," "If there was something someone could have said or done that would have changed the path that led you here, what would it have been?" IF Project writing workshop participants respond in writing to the IF Question and discuss their stories and what they believed would have changed their path to incarceration. While it is unusual for a police agency to be connected centrally with prison programming, such partnerships are not unheard of—albeit most are with community corrections agencies (Jannetta & Lachman, 2011). The IF Project has received considerable national and international media attention.[11] Additionally, the Seattle Police Department received a Bureau of Justice Assistance grant to expand the IF Project to provide and evaluate reentry services for women with the development and pilot study of Seattle Women's Reentry (Fryer, 2016).

I (along with Seattle University colleagues) conducted an evaluation of the IF Project (Helfgott et al, 2017). This 1-year (2012–2013) process evaluation was designed for the purpose of developing a comprehensive evaluation plan including developing an IF Project "tool-kit"; describing the program structure, components, and content; and conducting a pilot evaluation to pretest tools and methods to determine the appropriate research design and methodology for future program evaluation. The evaluation sought to identify the program structure and to pilot a pre/post survey instrument to collect preliminary outcome data to understand how the program is perceived by participants. Other process evaluations in a range of criminal justice contexts have utilized similar types of approaches focusing on understanding foundational elements of a program and outcome (Davis et al., 2015; Densly et al., 2016;

---

[11] See, including *TED Talks*, the production of the *IF Project Documentary* (Horan, 2016) featured on NBC News (Yohannes, 2016) and has been replicated in correctional facilities and schools around the country (Guerzon, 2014; Yohannes, 2016) in Alabama, Arizona, Colorado, Florida, Kentucky, New Jersey, North Carolina, and Virginia (Bogucki, 2017).

Edmondson & Hoover, 2008; Miller & Miller, 2016; Scott et al., 2014; Thomas et al., 2016). The evaluation included developing and compiling program materials, administering pilot pre/post surveys to determine self-reported impact of the program on participants in terms of program goals and objectives, conducting observational analysis, and conducting focus groups with incarcerated individuals in the Washington State Department of Corrections. Evaluation measures were developed to investigate the extent to which the IF Project is achieving its intended goals—to identify the needs of program participants, promote prosocial behavior, and prevent crime. On an applied level, the IF Project process evaluation offers empirical evidence that can be utilized by the Seattle Police Department Community Outreach Unit, IF Project staff, and other stakeholders to inform future development and implementation of IF Project components and replications in other regions. In addition to the pre/post instrument used in the IF Project evaluation, 331 essays written by adult prisoners were analyzed for themes to identify self-reported aspects of life histories that "would have changed the path" that led the participants to incarceration.[12]

Research is also currently underway evaluating the IF Project's reentry program the Seattle Women's Reentry Initiative. This project will follow 98 women released from the Washington Corrections Center for Women in 2017 and 2018 including an experimental and comparison groups. Rather than utilizing recidivism as the sole outcome variable, the study involves pre-interviews with the women that range from two to eight hours in length, monthly interviews with the women post-release, and a subsequent interview 1-year post-release (Helfgott & Parkin, 2017). The hope for this study is that we are able to measure phenomenological change as the women make their way in the reentry process examining the relationship between demographics, personality, life events, programmatic elements, and reentry success.[13]

Reflections and Concluding Comments

Ferrell and Hamm (1998) explain that *Ethnography at the Edge* was the product of "creative tension between two research experiences, out of the shared methodological space which produced one field researcher's profound affinity with his subjects of study and another's profound emotional outrage and disgust." They suggest that in the "lived intensity and emergence out of fieldwork," these opposite orientations could not be more alike and note that the great distance between stuff of field research—the "cramped living rooms of ex-cons" and the "safe, suit-and-tie respectability" of the academic conference (pp. 3–4)—deserve attention and that

---

[12] This work is currently underway. Results will be reported in a subsequent paper entitled *If Someone Would Have Showed Me: Identifying Pivotal Points in Pathways to Crime and Incarceration Through Prisoner Self-Narratives* (Helfgott, Gunnison, Collins, & Rice, 2017). IF Project narrative responses of juveniles will also be analyzed for a subsequent paper.

[13] This is a multi-year Bureau of Justice Assistance-funded study conducted by me and Elaine Gunnison evaluating the IF Project Seattle Women's Reentry.

"Field work doesn't just risk existential disorientation, it all but guarantees it." (Ferrell & Hamm, 1998, p. 8).

As I reflect on my experiences, I think of the many ways I have been able to capture a glimmer of ethnography in traditional mixed-method research designs. Incorporating ethnographic elements in traditional mixed-method research also guarantees existential disorientation, –perhaps even more so than in traditional ethnographic research. In mixed-method research there is no place to discuss or navigate through issues that arise when attempting to merge the subjective meaning-making methodology of ethnography with the objective sterilized methodological restrictions of traditional non-ethnographic research designs. This absence of place to discuss the subjective experiences of the researcher has the potential to generate even greater existential disorientation which oftentimes may go unexplored and even unrecognized by the researcher.

Almost all of the studies I have conducted have involved primary data collection utilizing mixed-method approaches. I have tried throughout my career to employ primary data collection methods that would allow me to meet my subjects in person and whenever possible, to allow the subjects themselves to have a say in the process. I have operated on the fringe ethnographic methodology, reading about ethnographic work and then going back into my own studies to see what I can do to at least incorporate elements of the ethnographic lens.

There are a number of issues that can be raised in the research I have described here that may raise red flags for anyone examining the research methodology from a pure non-ethnographic perspective. How does it impact the research if participants are involved in the program development? How objective is the research if the research is born from issues and experiences of both the subjects and researchers? Is it methodologically and ethically acceptable for the researcher to know their subjects? In research involving prisoners and reentry, how can professional boundaries be maintained while attempting to understand and participate in the cultures and experiences and lives of subjects? Many of these issues are discussed as dilemmas in ethnographic research, but there is little discussion in traditional research methods literature to guide researchers in navigating these sorts of messy lines when attempting to insert elements of the ethnographic lens in mixed-method designs.

If nothing else, my hope in writing this chapter is to put out a call to researchers to continue to develop creative ways to incorporate the ethnographic lens in traditional mixed-method approaches. I am most certain that those involved in true ethnographic research might argue that the research I describe does not do enough to incorporate an ethnographic lens, and I agree. But my hope is to start a conversation about how traditional mixed-method designs can better incorporate the ethnographic lens so that research in criminology, deviance, and criminal justice can be more reflective of the lived experiences of our subjects of study. Suggestions for staying at the edge of ethnography with traditional mixed-method research designs are the following:

1. Collect primary data.
2. Include subjects on a developmental committee.
3. Include qualitative variables that collect in-depth narrative responses.
4. Measure creatively in ways that value phenomenological change beyond recidivism.

However, for all of the benefits of ethnography-infused mixed-method research designs, there are significant issues to consider that do not exist in traditional methodological designs. Maintaining boundaries, reconciling the ethics and methodological protocol norms with the subjective elements of ethnographic perspective that potentially violate traditional research norms and restrictions, practical issues such as the time/money it takes to do primary data collection let alone to incorporate qualitative methods, and IRB restrictions are issues that make it challenging and potentially prohibitive for researchers to incorporate the ethnographic lens in ways that honor the lived realities of their subjects of study.

When I was invited to write this chapter, the editors asked me to reflect on how the outcomes of one of my *non*-ethnographic studies might have been fundamentally altered if I had adhered to classic ethnographic method. I thought to myself, what would an ethnographic study of psychopathy, prisoner reentry, violent victimization, law enforcement training, and community perceptions of police look like? To me a true ethnographic study means becoming a part of that subculture you are studying, ideally covertly, but at minimum, as true participant where you are who you are studying. When I taught deviance to undergraduates and required them to do their "mini covert ethnographies," a primary requirement was that they had to study a group that they did not belong to. I wanted them to be faced with the challenge of how to reconcile the distance between themselves and their subjects of study. I at times had students who wanted to study groups that they belonged to—students with eating disorders who wanted to study eating disorder groups, students who themselves worked in strip clubs who wanted to do their research where they worked, or students who were police or corrections officers who wanted to study their workplace subcultures. While it may be ideal to study the group to which you belong, the challenge of research is ultimately to study phenomena you do not understand, and when faced with great distance in experiences and lived realities, is the ultimate challenge in social science research—how to study individuals and groups of people who you do not understand.

I am not a psychopath. I score a 12 on the PCL-R only because my negligibly checkered personal history leaves me scoring high on factor 2 items, the behavioral component of the PCL-R. I am slightly more psychopathic than the average person who scores a 5. I have been handcuffed and held and thrown on the ground by a police officer, but I have never been arrested and have never spent a night in a jail or prison cell. I have spent many hours and days in police departments and courtrooms and prisons interacting with criminal justice professionals, yet when I shoot a gun, I can't hit anywhere in the bull's-eye and have no idea what it feels like to be a sworn civil servant charged with the authority of maintaining the law. I have listened to story after heartbreaking story of extreme sexual and homicide victimization, and while I have had some victimization experiences and have had people close to me who have been sexually and violently assaulted and murdered, I cannot possibly know their or their family's pain. I am in touch with many former prisoners who have been successfully living their lives in the community after serving years in prison, but no matter how many questions I ask them or how much they are willing to share with me, I will never know what it feels like to make my way after being released from serving a 20-year prison sentence.

In other words, it is almost impossible to be a true participant or in many cases a participant-observer to study many aspects of criminology and criminal justice. However, it is important to always consider how to include the ethnographic lens in traditional studies to ensure that the human element of crime and justice research is not lost in the distance that is created by jailhouse criminology.

References

Abbott, J. H. (1991). *In the belly of the beast: Letters from prison*. New York: Vintage.

Adler, P., & Adler, P. (2015). *Constructions of deviance: Social power, context, and interaction*. Belmont, CA: Wadsworth Publishing.

Allen, B., & Bosta, D. (1981). *Games criminals play: How you can profit by knowing them*. Susanville, CA: Rae John Publishing Company.

Bogucki, K. (August 30, 2017). Personal communication.

Davis, K. M., Peterson-Badali, M., & Skilling, T. (2015). A process evaluation of Toronto's first youth mental health court. *Canadian Journal of Criminology and Criminal Justice*, *57*(2), 159–187. https://doi.org/10.3138/CJCCJ.2014.E10

Densley, J. A., Adler, J. R., Zhu, L., & Lambine, M. (2017). Growing against gangs and violence: Findings from a process and outcome evaluation. *Psychology of Violence*, *7*(2), 242–252. https://doi.org/10.1037/vio0000054

Edmondson, L., & Hoover, J. (2008). Process evaluation of a bullying prevention program: A public school-county health partnership. *Reclaiming Children and Youth*, *16*(4), 25–33.

Ferrell, J. (1998). Criminological Verstehen: Inside the immediacy of crime. In J. Ferrell & M. S. Hamm (Eds.), *Ethnography at the edge: Crime, deviance, and field research* (pp. 20–42). Boston: Northeastern University Press.

Ferrell, J., & Hamm, M. S. (Eds.). (1998). *Ethnography at the edge: Crime, deviance, and field research*. Boston: Northeastern University Press.

Fryer, A. (2016, June27). SCA grantee discovers new way of policing by asking women involved with the justice system 'what if?' Retrieved from https://csgjusticecenter.org/nrrc/posts/sca-granteediscovers-new-way-of-policing-by-asking-wom0000000en-involved-with-the-justice-systemwhat-if/

Gacono, C. B., & Meloy, J. R. (1988). The relationship between conscious cognitive style and unconscious defensive process in the psychopath. *Criminal Justice and Behavior, 15*, 472–483.

Gacono, C. B., & Meloy, J. R. (1994). *The Rorschach assessment of aggressive and psychopathic personalities*. Hillsdale, NJ: Lawrence Erlbaum Associates, Publishers.

Gilmore. (2011, July). The Rev. Jon Nelson, 'a man of action, not a man of words,' dies. *The Seattle Times*. Retrieved from https://www.seattletimes.com/seattle-news/the-rev-jon-nelson-a-man-of-action-not-a-man-of-words-dies/

Guerzon, T. D. (2014). 2014 superheroes for Washington families. *ParentMap*. Retrieved from https://www.parentmap.com/article/2014-superheroes-for-washington-families?page=9

Gunnison, E., & Helfgott, J. B. (2007). Community corrections officer perceptions of ex-offender reentry needs and challenges. *Journal of Police and Criminal Psychology, 22*(1), 10–21.

Gunnison, E., & Helfgott, J. B. (2011). Factors that hinder reentry success: A view from community corrections officers. *International Journal of Offender Therapy and Comparative Criminology, 55*(2), 1–18.

Gunnison, E., & Helfgott, J. B. (2013). *Offender reentry: Beyond crime and punishment*. Boulder, CO: Lynne Rienner Publishers.

Gunnison, E., & Helfgott, J. B. (2017). Critical keys to successful offender reentry: Getting a handle on substance abuse and mental health issues. *The Qualitative Report, 22*(8), 2152–2172.

Gunnison, E. & Helfgott, J.B. (2018). Women leading justice: Experiences and Insights. New York: Routledge.

Gunnison, E., Helfgott, J. B., & Wilhelm, C. (2015). Correctional practitioners on offender reentry: A missed perspective. *Journal of Prison Education and Reentry, 2*(1), 51–73.

Hare, R. D. (1991, 2003). *Manual for the psychopathy checklist-revised*. Toronto: Multi-Health Systems.

Helfgott, J. B. (1991). *A comparison of cognitive, behavioral, and psychodynamic assessments of psychopathy: Integrative measures of understanding the psychopathic personality in criminal and noncriminal groups*. Unpublished master's thesis.

Helfgott, J. B. (1992). *The unconscious defensive process/conscious cognitive style relationship in psychopaths: An empirical study of psychopathic dynamics in criminal and noncriminal groups*. Doctoral Dissertation, Pennsylvania State University, University Park, PA.

Helfgott, J. B. (1997a). Ex-offender needs versus community opportunity in Seattle, Washington. *Federal Probation, 61*, 12–24.

Helfgott, J. B. (1997b). The unconscious defensive process/conscious cognitive style relationship in psychopaths. *Criminal Justice & Behavior, 24*, 278–293.

Helfgott, J. B. (2003). The prison tour as a pedagogical tool in undergraduate criminal justice courses. *Corrections Compendium, 28*(8), 1–2.

Helfgott, J. B. (2004). Primitive defenses in the language of the psychopath: Considerations for forensic practice. *Journal of Forensic Psychology Practice, 4*(3), 1–29.

Helfgott, J. B. (2008). *Criminal behavior: Theories, typologies, and criminal justice* (pp. 203–205). Thousand Oaks, CA: Sage.

Helfgott, J. B. (2014). Fame, media, and mass shootings: Culture plays a role in creating these tragedies. *Crosscut*. Retrieved from http://crosscut.com/2014/06/08/crime-safety/120459/fame-media-and-mass-shootings-culture-playing-role/?page=2

Helfgott, J. B. (2015). Criminal behavior and the copycat effect: Review of the literature and theoretical framework for empirical investigation. *Aggression and Violent Behavior, 22*, 46–64.

Helfgott, J. B. (2016). Prison-based cognitive behavioral treatment programs: A mechanism of panoptic control. Commentary on Jennifer A. Schlosser's (2015) narratives and discursive discipline in prison: Rewriting personal histories through cognitive-behavioral programs. *Journal of Theoretical & Philosophical Criminology, 8*(2), 148–159.

Helfgott, J. B. (2018). *No Remorse: Psychopathy and Criminal Justice*. Santa Barbara, CA: Praeger/ABC-CLIO.

Helfgott, J. B., Atherley, L., Pollock, J., Vinson, J., Strah, B., Neidhart, E., et al. (2015, June). *Evaluation of the Washington State Criminal Justice Training Commission's "Warriors to Guardians" cultural shift and Crisis Intervention (CIT) Training final report*. Washington State Criminal Justice Training Commission Consultant Agreement.

Helfgott, J. B., Conn-Johnson, C., & Wood, N. (2015, May). *Seattle Police Department CIT Culture Survey Final Report*. Seattle Police Foundation/City of Seattle Police Department Consultant Agreement.

Helfgott, J. B., & Gunnison, E. (2008). The influence of social distance on community corrections officer perceptions of offender reentry needs. *Federal Probation, 72*(1), 2–12.

Helfgott, J. B., Gunnison, E., Collins, P., & Rice, S. K. (2017). The power of personal narratives in crime prevention and reentry: Process evaluation of the Seattle Police Department's IF Project. *Corrections Policy, Practice and Research, 3*(2), 1–24.

Helfgott, J.B., Gunnison, E., Murtagh, A.; Navejar, B. (2018). BADASSES: The Rise of Women in Criminal Justice. *Women & Criminal Justice*, *28*(4).

Helfgott, J. B., Lovell, M. L., & Lawrence, C. F. (2002). Citizens, victims, and offenders restoring justice: Accountability, healing, and hope through storytelling and dialogue. *Crime Victims Report, 6*, 3–4.

Helfgott, J. B., Lovell, M. L., Lawrence, C. F., & Parsonage, W. H. (2000a). Development of the citizens, victims, and offenders restoring justice program at the Washington state reformatory. *Criminal Justice Policy Review, 10*, 363–399.

Helfgott, J. B., Lovell, M. L., Lawrence, C. F., & Parsonage, W. H. (2000b). Results from the pilot study of the citizens, victims, and offenders restoring justice program at the Washington state reformatory. *Journal of Contemporary Criminal Justice, 16*, 5–31.

Helfgott, J. B., & Parkin, W. (2017, January). *Seattle police department's micro-community policing plans evaluation* (p. 158). Retrieved from https://www.seattle.gov/Documents/Departments/Police/Reports/SPD-MCPP-Implementation-Evauation-Final-Report.pdf

Helfgott, J. B., Strah, B. M., Pollock, J., Atherley, L. T., & Vinson, J. (2018). A qualitative approach to understanding guardian models of policing. *Journal of Qualitative Criminal Justice and Criminology, 6*(1), 93–120.

Horan. K. (Producer/Director) (2016). *The IF Project* [Motion Picture]. USA: Tinfish Films.

Jannetta, J., & Lachman, P. (2011). *Promoting partnerships between police and community services agencies: How coordination can reduce crime and improve public safety*. Washington, D.C.: Office of Community Oriented Policing Services.

Johnson, R. (2001). *Hardtime: Understanding and reforming the prison*. Belmont, CA: Wadsworth.

Kaplan, J., Jaffe, S. R., & Lansing, S. (1988). *The Accused [film]*. Hollywood, CA: Paramount Pictures.

Kinney, W., & Helfgott, J. B. (2000). Political activism over 60: Social stigma and the late-life liberal. *Sociological Imagination, 37*, 5–28.

Kinney, W. J., & Helfgott, J. B. (2004). Life satisfaction and late-life liberal activism: Activity, continuity, and the role of identity politics. *Politics of Change: Sexuality, Gender, & Aging Research in Political Sociology, 13*, 113–138.

Lawrence, C. F., Lovell, M. L., & Helfgott, J. B. (2004). The moral discourse of healing: Victims and offenders for restorative justice. *Journal of Societal and Social Policy, 3*(2), 49–64.

Lovell, M. L., Helfgott, J. B., & Lawrence, C. F. (2002a). Narrative accounts from the citizens, victims, and offenders restoring justice program at the Washington state reformatory. *Contemporary Justice Review, 5*, 261–272.

Lovell, M. L., Helfgott, J. B., & Lawrence, C. F. (2002b). Citizens, victims, and offenders restoring justice: Social group work bridging the divide. In S. Henry, J. East, & C. Schmitz (Eds.), *Social work with groups*. New York: Haworth Press.

MacKenzie, M. (2007, May). Pigs on parade to kick off market centennial celebration. *The Seattle Times*. Retrieved from https://www.seattletimes.com/seattle-news/special-reports/pigs-on-parade-to-kick-off-market-centennial-celebration/

Miller, J. M., & Miller, H. V. (2016). Validating program fidelity: Lessons from the delaware county second chance initiatives. *American Journal of Criminal Justice*, *41*, 112–123. https://doi.org/10.1007/s12103-015-9325-3

Olsen, J. (1983). *Son: A psychopath and his victims*. New York: Atheneum.

Parkin, W., Helfgott, J. B., Collins, P., Messelu, K., & Krappen, E. (2015). *Yesler terrace public safety plan final report*. Seattle Housing Authority.

Pike Place Market Foundation. (2001). *Pigs on Parade*. Seattle, WA: Marathon Group.

Rule, A. (1987). *Small sacrifices: A true story of passion and murder*. New York: New American Library (NAL Books).

Scott, C. K., Dennis, M. L., & Lurigio, A. J. (2014). Women's participation in a jail-based treatment program in a large urban setting: A process evaluation. *Criminal Justice Research Review. Offender Programs Report, 17*(4), 49–59.

Thomas, T. A., Collins-Camargo, C., & Boes, R. C. (2016). A process evaluation of a Kentucky court improvement initiative. *Family Court Review, 54*(3), 413–423. doi:10.1111/fcre.2016.54.issue-3

Yochelson, S., & Samenow, S. E. (1976). *The criminal personality* (Vol. 1–3). New York: Jason Aronson, Inc.

Yohannes, A. (2016). 'The IF project': New documentary asks inmates to reflect on missteps. *NBC News*. Retrieved from http://www.nbcnews.com/feature/nbc-out/if-project-s-kim-bogucki-working-inmates-asking-what-if-n647666

Just Who Needs Forgiveness? The Emotional Terrain of a Prison-Based Interview

Steve Herbert

When I first met him, Burt[1] was a 69-year-old prisoner in a medium-security facility in Monroe, Washington. He was about halfway through a 22-year sentence that he earned for murdering his wife. Because the Washington Department of Corrections expects Burt to die before his release date, he is designated as a "de facto" life-sentenced prisoner. For that reason, he was in a pool of 21 inmates I chose to interview for a book project focused on the steadily growing number of lifers in prisons in Washington and elsewhere in the United States (see Herbert, 2018).

As with most of those I interviewed, I talked with Burt on three separate occasions, each time for about an hour. Each interview was semi-structured; I had a list of questions to ask, but I allowed each interviewee to cover topics of special importance to them. I used part of my first interview with each individual to explore how he developed an understanding of the social order of prison. Here's an excerpt of my conversation with Burt on this topic, which occurred toward the end of our conversation:

Herbert   *So, in terms of the social structure of the prison, you've talked about race being a big divider, are there ways in which social groups are formed around here? Are there discernible groups?*

Burt   *Yeah. There's the real heavily religious groups. And there's almost a hierarchy, or a judgment thing, because my Christianity is not exactly theirs. I mean, when it comes down to how I look at Christ and that sort of thing. We're pretty much in agreement, but there's a group of them, and they're looked down upon by those who choose not to be at all religious. They say things like, "Oh those guys that hang out at the chapel*

---

[1]As with all prisoner names in this essay, this is a pseudonym.

S. Herbert (✉)
Department of Law, Societies, and Justice, University of Washington, Seattle, WA, USA
e-mail: skherb@uw.edu

> *are all sex offenders." And I don't -- I have no idea, because I pay very*
> *little attention to that. And I think one thing I don't -- I sit with and talk*
> *to guys that others won't because I, frankly, don't care. I know how bad*
> *my own crime was and …* (sobs) *and we all need forgiveness.* (silence)

What this transcript excerpt does not capture is the length of the pause before and after Burt uttered the words "and we all need forgiveness." During that nearly thirty-second span, I did not say anything. Eventually, after he gathered himself, Burt continued on with his sociological analysis, moving toward a discussion of prisoners who are focused on athletics.

As I think back on that interview nearly 3 years later—and on the dozens of other prisoner interviews that I completed—I wonder if I properly responded to Burt's silences. I said nothing and simply allowed him to compose himself. I can rationalize that as an act of respect, as a means to dampen any embarrassment he felt from crying in front of me. But the reality is that I was uncomfortable, and I was uncertain about the proper course of action. Was I obligated to say something to indicate that I appreciated the sadness he was experiencing? Should I have apologized for asking a question that unearthed deep emotions?

At a more broad level, I wonder how I justify conducting any prisoner interview that is explicitly designed to traverse complicated and fraught terrain. Upon what basis can I legitimate any such action? How can I ask questions that help reveal pain and sadness for Burt and other lifers?

The Emotions of Prison-Based Interviews

Prison-based interviews are likely to be laden with emotion. To be sure, whenever humans interact about issues of personal significance, emotions will likely be aroused. But the prison setting is a unique one, because prisoners are subject to abject reductions on agency and on contact with outsiders. Prisoners thus necessarily approach an interview with an academic in a peculiar and disadvantaged social location. This reality is compounded in situations like mine, where the interviews were meant to cover difficult ground. I planned to ask each interviewee about the many challenges that attend to a life that will likely end with death in prison. Indeed, I actively aimed for a discussion that would ensure their emotional discomfort.

This emotional exploration was unavoidable, but it also advanced my interests as a researcher. As Alison Liebling (2001, p. 475) once observed about qualitative work in prisons, "The more affective the research, in terms of shared feelings and experiences, the better the fieldwork gets done on the whole." So, I had clear motivations to drive the conversations toward the emotional.

Yet I was likely not alone in seeking connection. Many inmates seemed to enjoy talking about their travails. For that reason, I often believed that the inmates derived some form of therapeutic benefit from our conversations. Yewkes (2011, p. 66) is no stranger to this phenomenon: "Prison ethnographies frequently yield information of a deeply personal nature and, sometimes, rapport may be superseded by genuine, if

very transitory, friendship, as prisoner respondents seek to make emotional, experiential, and intellectual connections with the researcher."

Whatever the benefits that might result for either party, a prison interview is potentially treacherous, precisely because of the emotions involved. It behooves us to address these emotions directly (Yewkes, 2013) even if we might not like what we learn by doing so. There are clear benefits from prisoner interviews, but there are dilemmas, as well. Both the potential complications and potential benefits deserve to be catalogued and assessed, a goal I pursue here. I start with the complications.

On Complications

The project that led to my conversation with Burt and the other prisoners aimed to consider the sizable implications resulting from the growth of life sentences in the United States (On the rise in these sentences, see Nellis, 2017. On the resultant expansion of elderly prisoners, see American Civil Liberties Union, 2012). I sought to understand these complications for both prisoners and prison staff. From the prisoners, I hoped to learn how they came to terms with their sentence, how they worked to craft lives of purpose and meaning, and how they saw their futures. From the prison staff, I wished to learn about the challenges emerging from the growth in aging prisoners and how they anticipated such challenges would continue to develop.

The more emotionally difficult conversations were entirely on the prisoner side of the equation. This was hardly surprising. I asked each lifer questions about hardships they experienced, their challenges in maintaining connections with family and others on the outside, the privations of their daily life, and their reckoning with the prospect of death in prison.

Unsurprisingly, many inmates expressed strong emotions. For instance, some discussed what it felt like to have a parent die. Said Melvin about the death of his parents: "Just to know where I was when they passed, to know that I was in prison. My mom—that her son was in prison and was never gonna get out. And how much that had to have hurt her." Others talked about their spouses and the challenges they faced. All of those who were married expressed significant regret about their inability to assist their wives. Here's Leonard on that point:

> It's not easy. If I—as I look back on my life now, you know, I wouldn't recommend somebody getting married in prison because it's really hard. The spouse really deserves a lot more than that. If something happens right now, if my wife's in distress right now, can't do a thing about it. She's trapped on the side of the road or — you know what I'm saying? There's just nothing I can do. It's hard.

In each interview, I probed on the issues of decline and death. I asked how it felt to watch other prisoners age, lose bodily capacity, and eventually die. I wondered how inmates assessed themselves against such a scenario. Sean talked at length when I broached this subject:

> You know, I can still function. I have things to do – you know, I'm lifting weights five or six days a week; I'm going out and walking all the time. This morning, I went and did pull

ups – staying in shape is the kind of thing that I like to do. But then, you know, I look at – well, okay, so if this guy is in a walker. He's 82 years old and you know, my sentence takes me until I'm about 81. And so how am I going to feel if I'm in that condition? I can't do the same things – you know, how is he handling it? This other guy who I know – because my friends kind of take care of him – you know, he is about 78. He's in a wheelchair now – I'm not sure why. He has failing eyesight and he has a real hard time hearing. When we bring him to church, we try to put him in front so he can hear and maybe see a little bit of words for the songs. So anyway, I think about those things, so it comes up a lot. You know, a lot of other things – maybe they exist out there, but they're not thrown in my face, so I'm just not going to think about them. Why think about something that's, you know, on the depressing side? I'd rather think about happy things and so, anyway, that's what comes up.

Not content with that answer alone, I pushed Sean yet further and asked him to confront the possibility of a prison-based death:

Herbert	*So, your physical health seems to be good. Do you operate on the assumption that you will outlive your sentence?*
Sean	*I really don't think about it much.*
Herbert	*You don't?*
Sean	*No, I mean because it doesn't really do me much good to think about it. It's certainly true that it's probably a little bit depressing for me to think about I won't get out until I'm in my 80s, so I don't spend a lot of time thinking about getting out. It runs across my mind, but when it does, it's probably more a thought of being in a younger condition than in my 80s. So it's not something I focus on.*
Herbert	*So you don't think about the possibility that you'll die in here?*
Sean	*I don't dwell on it. I'm sure it's run across my mind.*

I could cite other examples of difficult conversations I worked to engender, but the point is likely clear: my subjects were asked to discuss some extraordinarily unpleasant circumstances. And, with each such question, I implicitly reminded them that I was differently situated. We both knew that after my tape recorder was switched off, I would leave and go home to my family. We also both knew that I envisioned a very different aging and dying process than they did.

To be sure, I did what I could to ensure that the men possessed agency during our conversations. The desire to maximize their voice actually began before any interviews were scheduled. At the outset of the project, I went to the prison to meet with all of those with life sentences. I used that group meeting to describe the research and to distribute copies of both the interview questions and the consent form that each interviewee would be required to sign. Prisoners who were interested in being interviewed were subsequently asked to send a note to their counselor. When they arrived at their first interview, each prisoner was asked to read and review the consent form before any conversation began. The form stated explicitly that interviewees could refuse to answer any question and that they could discontinue an interview at any time, two realities I reinforced orally. Once the interview began, the semi-structured format allowed interviewees to take the conversation in their own direction, which is precisely what each of them proceeded to do. One potential interviewee walked

away from the first interview before it ever commenced, and two stopped participating after one session.

However, the prisoners hardly possessed anywhere near the same level of agency as I did. They were escorted to and from each interview by a correctional officer. They were subject to a strip search before and after each conversation. I was not allowed to bring in copies of interview transcripts after the fact to allow them to modify their remarks. Nor was I allowed follow-up interviews at a later date to allow them to contest any interpretations I was developing about the significance of their remarks. In these ways, my project fell well short of a more collaborative approach to qualitative inquiry (see Pittway, Bartolemei, & Hugman, 2010; Rappaport, 2008).

There were complications arising from these conversations for me, as well, due to several factors: the work required to manage the conversation, particularly when the inmates got visibly emotional, the general discomfort I felt in probing on matters of such sensitivity, and my inability to verify anything that they said. Some prisoners told stories that seemed flecked with hyperbole. I could gently challenge such stories, but I could not disprove them. As a result, my analysis is perpetually susceptible to the critique that at least some of the data upon which it relies are unreliable. Finally, the interviews were often simply exhausting, particularly because I typically conducted as many as three in a given day. On several occasions, I left the prison wondering if the benefits of the project were worth the effort I and the prisoners were expending.

On Benefits

Yet perhaps the benefits were numerous and notable. Indeed, neither I nor the inmates would have engaged in any conversation absent a potential gain. For my part, the benefits are easy to list. At the professional level, I knew that the data I gathered would allow me to build an analysis that could result in publications and other markers of professional status. Beyond these measures of productivity, I had some hope that the work would influence wider conversations about punishment policy. Little attention is pointed in policy circles to life-sentenced prisoners and the challenges they pose; this was a lacuna I hoped to help fill. In addition, I was able to satisfy my curiosity about a world from which almost everyone is excluded.

I never asked any of the interviewees just why they agreed to sit down with me. For that reason, I cannot confidently catalog their motivations. However, our conversations allow for some educated guesses. Most of the prisoners unsurprisingly castigated the various sentencing policies that resulted in their death-in-prison sentences and hinted that they hoped that my work could help shift the public conversation in their favor. Others mentioned their intention at some point to seek clemency, an elaborate process that requires much support from outside community members. Although none broached the subject directly, I sometimes wondered if they hoped to recruit me as a potential supporter when that time came.

In addition, many prisoners told me directly that they enjoyed talking with me. I concluded from this that many were simply lonely. In fact, many talked about how hard it is to construct relations of trust inside prisons. They thus lacked opportunities to discuss their personal struggles as openly as I invited them to do. Whether there was indeed any notable therapeutic benefit for them is impossible for me to verify, but some suggested that this was the case. The semi-structured nature of the interview was likely a key contributing factor here. I would also like to believe that I listened to them with the requisite empathy to enable them to trust me. As Crewe (2013, p. 20) noted about his encounters with prisoners: "Since prisoners are so used to being disbelieved, un-recognised, and un-trusted, listening to their life stories in an active and attentive way is a powerful act. Because imprisonment almost always diminishes their sense of individuality, interviews that ask them who they are *as individuals*, not just as prisoners, communicates that their humanity is being taken seriously" (emphasis in original).

Conclusion: Pursuing Forgiveness

I certainly wish to believe that I took my interviewees' humanity seriously and that I communicated that to them. I take some comfort in the fact that many did in fact become visibly emotional. If I created a conversational dynamic suffused with enough trust to enable them to make themselves vulnerable, I am inclined to view that as a positive thing.

Still, neither I nor any other prison researcher can rest perpetually easy on this score. We should always keep at the forefront the clear-eyed language of Liebling (1999, p. 163), who notes that prison research "is an enterprise made complex by the human nature of the researchers and the researched. It is an intense, risk-laden, emotionally fraught environment." As with any encounter in which emotions are laid bare, the possibilities of harm are ever-present.

Beyond these emotional complications, any prison researcher necessarily possesses more agency than those they interview. In fact, it is their lack of agency that fundamentally defines someone as a prisoner, especially if their confinement is likely to end in an ignominious death. Further, the terms of outsiders' engagements with prisoners are always negotiated with powerful third parties, namely, the prison officials who structure the confinement regime. These rules of engagement may or may not allow for much agency for the prisoner, and thus the interview encounter may work to remind an inmate of his powerlessness.

Prisoners are also largely powerless to shape the analysis that results. Safely cocooned in an office, academic analysts get nearly exclusive license to interpret the interviewees' stories as they see fit. Such analysts may assert a strong desire to create work that furthers the interests of prisoners, but it is surely at least a little arrogant for anyone to presume to know just what those interests might be. As Sparks (2002, p. 558) notes, "If we wish to claim some civic and intellectual importance for

research in prisons, we may also need to accept that there is no entirely innocuous position from which to speak."

A similar note of caution should attend to any belief that an interview with an academic is actually therapeutic for a prisoner. I wish to believe that this occurred for some I interviewed, but I cannot verify that. Similarly, I cannot rest easy if some interviewees were disappointed to learn of my limited ability to help them with their clemency case or to pursue any other means to reduce or make more bearable their interminable sentence. I also cannot protect them from any misinterpretations of their words that may occur in my analysis or from any false hopes they may have about my ability to change public policy. And I cannot discount the possibility that my encounter with them served at some level to re-traumatize or re-stigmatize them.

My entrance into a prison to interview Burt and others was thus no innocent journey: I trod an emotionally fraught terrain. Like other prison researchers, I believe that, on balance, I did more good than harm in pursuing a qualitatively based analysis of life sentences in contemporary prisons. But it would be folly for me to gainsay the risks I asked the prisoners to assume. Burt may well need forgiveness, but so likely do I.

References

American Civil Liberties Union. (2012). *At America's expense: The mass incarceration of the elderly*. Washington DC: ACLU.

Crewe, B. (2013). Writing and reading a prison: Making use of prisoner life stories. *Criminal Justice Matters, 91*, 20–21.

Herbert, S. (2018). *Too easy to keep: Life-sentenced prisoners and the future of mass incarceration*. Berkeley and Los Angeles: University of California Press.

Jewkes, Y. (2011). Autoethnography and emotion as intellectual resources: Doing prison research differently. *Qualitative Inquiry, 18*, 63–75.

Jewkes, Y. (2013). What has prison ethnography to offer in an age of mass incarceration? *Criminal Justice Matters, 91*, 14–15.

Liebling, A. (1999). Doing research in prison? Breaking the silence. *Theoretical Criminology, 3*(2), 147–173.

Liebling, A. (2001). Whose side are we on? Theory, practices and allegiances in prison research. *British Journal of Criminology, 41*, 472–484.

Nellis, A. (2017). *Still life: America's increasing use of life and long-term sentences*. Washington DC: The Sentencing Project.

Pittway, E., Bartolemei, L., & Hugman, R. (2010). 'Stop stealing our stories': The ethics of research with vulnerable groups. *Journal of Human Rights Practice, 2*, 229–251.

Rappaport, J. (2008). Beyond participant observation: Collaborative ethnography as theoretical innovation. *Collaborative Ethnographies, 1*, 1–31.

Sparks, R. (2002). Out of the 'Digger': The warrior's honour and the guilty observer. *Ethnography, 3*(4), 556–581.

Respecting the Voices of Youth: Studying School Security and Punishment

Aaron Kupchik

Why This Topic?

I study how social institutions police and punish children, particularly youth of color. My focus is on schools as sites of rigid security and harsh discipline. I have no idea why I began to study this topic—I'm often asked, but have no good response. I wasn't in trouble with the law or in school as a child, nor was I exposed to rigid security or policing, so the topic doesn't resonate in any meaningful way with my background or experiences.

While I don't know how I initially became interested in policing and punishing children, I do know why I continue to be captivated with this topic: because it just seems fundamentally wrong to me how we treat so many children. As a sociologist, I am fascinated with how we selectively apply or deny protected category status to children, how we are simultaneously afraid for and afraid of children. In my earlier research I studied the prosecution of youth in criminal (adult) court, where the denial of childhood status—and the protection from harsh punishment that this typically comes with—is more than symbolic. But I also see myself as an advocate for social justice. As it relates to my work, I think of social justice as the practice of treating all youth fairly, reasonably, and with compassion, so that all children—not just those born with privilege—have an opportunity to grow, learn, and become productive adults. This is what really motivates me to study and write about policing and punishing children.

With compulsory education laws, we require that all children in the USA attend school. The vast majority of them go to public schools, funded by public money and governed by elected officials. To me it therefore seems like a collective moral obligation to provide a quality education for everyone who attends a public school and to treat children fairly and with care and dignity. But too often children don't receive

A. Kupchik (✉)
Department of Sociology and Criminal Justice, University of Delaware, Newark, DE, USA
e-mail: akupchik@udel.edu

such treatment. We over-police and over-punish youth, particularly youth of color. In my most recent book, *The Real School Safety Problem* (2016), I discuss the long-term and broad harms that come from how we treat children in schools. In addition to negatively and unnecessarily impairing the futures of the youth who are policed and punished, we also disrupt entire student bodies, hurt the families of punished youth, and even do damage to entire communities. These harms include increased racial inequality, bullying victimization of children who attend schools with unfair rules, and lower voting and community volunteering rates that follow years after students' experiences with school punishment.

We know better. Prior research on effective school discipline entirely predicts how the over-policing and over-punishment of youth would harm children, schools, and communities. And yet we do it anyway, treating too many children, particularly children of color, as threats to be managed and removed, rather than as young citizens in training or children to be loved and cared for. This frustrates me and motivates me to keep studying and writing about it.

I work hard at maintaining an empirical perspective with my work, to rely on empirical evidence rather than my feelings and opinions to form my views on policing and punishing youth. But more importantly, when I discuss policy issues related to school security and punishment with anyone—other scholars, undergraduate students, policy-makers, school staff, parents, and others who are interested—I try to base all arguments on evidence, not an appeal to ethics, morals, or my own sense of fairness. But my sense of what is right is what fuels my interest in getting to that point.

For me at least, a social justice-oriented motivation to scholarship is crucial, otherwise I don't know if I would be as passionate about my work. But it has costs, one of which is that it can lead to judgment. As much as I may try to be objective and influenced only by empirical data, research—*all* research, despite efforts by many to deny subjectivity—involves judgment. Given what we know, I feel strongly that it is wrong to treat children in schools as we do. As hard as I try to be empathetic, not just to mistreated students but also to overworked and under-supported educators, I have a hard time refraining from judgment about harm that they may do without meaning to, or about their acceptance of authoritative narratives that we know are misleading (e.g., that punitive discipline will help children learn better behavior).

Sometimes this judgment can be directed at students, the very group I most wish to identify with and help with my research. In one study I conducted, the research that resulted in my book, *Homeroom Security* (2010), I encountered such a problem when I found students who supported practices that, according to other sources of evidence, were harmful to them. After describing the research briefly, I will return to discuss this problem, how I responded, and the lessons I learned about both myself and doing ethnography.

The Project and the Problem

In 2006 I received an NSF grant to do a mixed methods study on school policing and punishment. A large portion of the project was an ethnography of four high schools, two in a southwestern state and two in a mid-Atlantic state. Within each state I chose one school with mostly middle-class white students and one school with mostly low-income youth of color to study. I worked with two graduate research assistants, one in each state. Together, we spent months shadowing school staff, including teachers and administrators, to get a sense of how they taught and ran each school. But mostly, because we wanted to understand how students were policed and punished, we spent our time observing police officers stationed at the schools (called school resource officers, or SROs) and deans of discipline or interventionists (titles given to staff members whose primary job is to respond to student misbehavior). We sat in on disciplinary meetings with students and spent hours upon hours walking the hall-ways with SROs or sitting in their offices as they chatted with students. We also conducted over 100 interviews across the four schools, where we spoke to students, parents, teachers, security guards, SROs, administrators, and disciplinary staff.

In addition to describing what contemporary school policing and punishment look like, I also wanted to evaluate how they impacted the school social climate and how students, parents, teachers, and others viewed these practices. Thanks to high-quality research that preceded my study, a body of evidence was quickly forming that showed the harms of over-policing and over-punishing students. Scholars such as Ferguson (2000) had demonstrated the ways that rigid security and harsh punish-ments in schools could hurt school social climates and criminalize youth, particu-larly youth of color. A report by the New York Civil Liberties Union (Mukherjee, 2007) that was released during my research time clearly documented the invasive-ness of police officers in schools, showing how their treatment of students resulted in unnecessary arrests, weakening of social and emotional supports for students, and sometimes even physical abuse. There were other studies as well, illustrating how the presence of harsh punishments and police in schools could harm students. These studies showed that students were increasingly viewed as potential threats to be policed and removed from school (i.e., suspended or expelled) and decreasingly as children to whom it is our obligation to provide social and emotional supports.

Naïvely, I expected that students would view their own experiences through this same critical lens, recognizing the harms of over-policing and unnecessarily harsh punishment. I expected this to be particularly salient when it came to police officers stationed in their schools. I was wrong. Just over half of the students we spoke to were happy to have police officers in their schools. Most were either ambivalent, not caring one way or another whether there was a police officer in their school, or pleased to have an officer in their hallways. Those who were pleased explained that the officer might be able to prevent crime, or that he (all four of the schools in my study had male SROs) was an adult who they could talk to and receive advice from. Among students who voiced some concerns about having an officer in their school,

all but one also voiced positive views. For example, I quoted one Latina student as saying:

> An officer in school, at times one feels like, like he's looking at you in a bad way and one feels, like, under scrutiny or bad, but one also has to realize that it's for our own safety, it's for our security that in case there is an accident or a person that has a gun. Then in that sense it's good. (p. 104)

Overall, students did not view police officers' presence nearly as critically as I had expected.

My Response: I

At first I was surprised and confused by this result, since the students' lack of critical response differed from what I judged to be the response they *should* have had. As I discuss above, I began this project with concern about what contemporary school security and punishment means for students' futures; as I continued with the project and observed how children too often can be mistreated, this concern turned to indignation. The fact that the students for whom I am concerned did not share my concern was troubling to me.

This surprise and confusion then started to turn into condescension. As a graduate student collaborator pointed out, asking students whether they like having police in their schools probably makes as much sense to them as asking them whether they like having a school principal; they have always had both as part of their educational environments and think of them as completely normal. They had been socialized from a young age to accept policing as ubiquitous within schools. I began to think of students as not having the capacity to critically analyze the invasive security to which they were exposed. This early response was condescending. I was assuming that students weren't angry (i.e., they didn't share my emotional response) because they weren't critical thinkers, since they'd been duped into complacency and needed to be woke to social injustice, to share my moral indignation. My reaction was to be a savior, to want these youth to be given the benefit of a proper education that would ignite their critical thinking of authority, so that they had the capacity to adopt my perspective

My Response: II

An important part of an ethnographer's job is to be reflexive and consider the extent to which their observations and interpretations of those data are influenced by their predispositions. For me at this time, this meant thinking carefully about my initial response to students' acceptance of SROs. I realized that my initial response was judgmental and condescending. But I still didn't know how I should interpret their

response; acknowledging my mistake might call into question my initial interpretation, but did nothing to provide a new perspective.

To this day I'm still stumped. I have no satisfying answers to explain why students tend to be uncritical of SROs, despite evidence that the presence of the SROs can result in arrests for minor misbehaviors and weaken social and emotional supports. But the question itself has been very beneficial to me, both in my research on this topic and my approach to ethnography, overall. It has spawned new questions and insights, and greater reflection on my own critical gaze, each of which makes me a better researcher (or so I hope!).

One question I began to ask was whether the growing body of research on school policing was too critical. I wondered whether it allowed ivory tower concerns about students' rights to overshadow students' and parents' demands for police officers to guard schools, or whether it was biased by the predisposition held by critical criminologists (like myself) that growth in policing tends to result in more punitive control. I reconsidered prior work with these questions in mind and continue to apply these questions as I read new research. Though I still view the literature, overall, as demonstrating how a greater police presence in schools can be problematic for youth, I believe I am now better at reading this work more carefully and critically.

A second question that I asked myself is why students who are exposed to SROs think so differently than me. What is it about their status as children, or as those immediately affected by, rather than observing, SROs, that might influence how they respond to the presence of police in schools? How might other status characteristics that make me see the world differently from them, such as sex, race/ethnicity, sexual orientation, disability status, etc., influence our responses to SROs in different ways? Finding results that differ from one's hypotheses do not represent failure and do not need to be a problem. Instead, they open up doors to new inquiries that should only enhance the knowledge that is produced.

Surprising results can also help stimulate one's growth as a scholar. Being an ethnographer is a privileged position, as one receives access to observe and interact with people, but usually without feeling the pain they may experience in a direct way. Being a straight, white male with a tenured position as a full professor offers me much greater privilege beyond that. It is vital to consider how my privilege influences not only the data that I collect but how I interpret the data. In this case, I realized that my first response, of judgment and condescension, was influenced by my privilege. I believe (I certainly hope) that the end result written up in my book, *Homeroom Security*, did not come across as condescending or convey such privilege. This experience certainly opened my eyes to how my privilege influences my research and offered me a lesson that I strive to incorporate in my work across projects.

Understanding that my reality and more importantly my experiences with social control institutions are shaped by my privileged status is important for my teaching as well. I always try to remember that my students might come to my class with very different experiences and perceptions than I have and to listen to and respect their views. For example, I have wonderful colleagues who teach undergraduates their rights when stopped by the police and encourage students to invoke these rights

(e.g., to not consent to a warrantless search of one's car at a traffic stop). I value these colleagues and their teaching, but I do not similarly encourage students to invoke their rights. Invoking one's rights makes sense to me, since I have little fear of being mistreated by police. But I simply do not know what it is like to be an African-American driver pulled over by the police, given the fear they may face, knowing about the fates of those who have died during or after such encounters (e.g., Philando Castile, Sandra Bland, and others). I agree that students should know their Constitutional rights, but the gap between my experience and theirs prevents me from asserting what they *should* do.

Conclusion

Above I describe a problem I faced during my research on school security and discipline, when I was surprised by students' overall acceptance of what I saw to be invasive school policing. I am still unsure how to interpret their response. But the experience taught me a great deal about myself as a scholar and about important questions I need to keep asking.

Ethnography is an important tool for critical analysis because it allows the researcher to probe into how authority and power are invoked. But it is very easy to slip into judgment when we do so. Simply describing what we observe, without linking these data to larger themes about power dynamics and providing policy suggestions, might be insufficient to make a contribution to the literature or get published in top journals. We need to show something unexpected, or describe a process or outcome that is either good or bad (usually bad), and as a result it is tempting to judge subjects. But it is important to avoid this temptation.

I will conclude with an example: James Forman Jr.'s recent book, *Locking Up Our Own* (2017). Forman tells the story of Washington D.C.'s history of policing and punishment since the 1970s. Criminologists are very familiar with the problem of mass incarceration that we have seen in this era. Given the racially disparate impact of mass incarceration, many scholars have focused on mass incarceration as a system of racial control (e.g., Alexander, 2012). Forman sees D.C. as an interesting case study because African Americans have served in most elected citywide positions during this era. These leaders responded to their citizens' calls for greater order and security amidst rampant drug use, crime, and disorder. The result was more police officers and more aggressive policing, a rigorous war on drugs, harsher sentencing practices, and substantial growth in incarceration. Rather than judging D.C.'s elected leaders for the harms of mass incarceration and their role in bringing these harms to their city, Forman explains their perspectives based on the problems they faced and the solutions available to them. By seeking to understand the position these individuals found themselves in, he avoids judgment and offers a tremendous analysis that makes a substantial and novel contribution to work in this field.

References

Alexander, M. (2012). *The New Jim Crow: Mass incarceration in the age of colorblindness*. New York: The New Press.

Ferguson, A. A. (2000). *Bad boys: Public schools in the making of Black masculinity*. Ann Arbor, MI: University of Michigan Press.

Forman, J., Jr. (2017). *Locking up our own: Crime and punishment in Black America*. New York: Farrar, Straus and Giroux.

Kupchik, A. (2010). *Homeroom security: School discipline in an age of fear*. New York: NYU Press.

Kupchik, A. (2016). *The real school safety problem: The long-term consequences of harsh school punishment*. Berkeley, CA: University of California Press.

Mukherjee, E. (2007). *Criminalizing the classroom: The over-policing of New York City schools*. New York: New York Civil Liberties Union.

A Taste of Ethnography

Peter K. Manning

Ethnography as a field should bring one face to face with one's self or selves. When speaking to graduate students about my work, I have been oblique and offered many versions or portraits of my work. That is, if there is real portrait of a person's work that is consequential for more than a brief time, it is rare, and this portrait is the only possible portrait. It's perhaps more than the blink of an eye, but substantially less. All of one's work, its reputation, the view of the audience. Who knows what might be said of one's work: it is merely a result of who casts an eye on it, when, and where. It is not quite true, as they say in Hollywood, that every knock is a boost. Careers often are shooting stars: that which counts early, little statistical essays based on secondary data, empty and atheoretical, do not stand the test of time. Fame leaks out of books. The issues chosen for fieldwork are often not really chosen, while others are stumbled upon or not chosen to one's dismay. Somehow, evidence of careers is cast "rationally," in spite of the obvious facts that no one can predict the future, let alone tomorrow. Will it rain? Will I be happy? *Que sera, sera*. The cloud of the future is one reason why evening prayers are still regarded as efficacious. Autobiographies drip with the details of mistakes, missteps, falls, tumbles, disgrace, and even failures. In other words, a career has many facets, most of which are unknown and unrevealed; thus every memoir or reflection is only partial. "Pick yourself up…" is a useful song. Memoirs and other reflections are reflexive, after all—what is written is what one wants the imagined other, the audience, to read. Modern order is not that of the preliterate. Graham Greene, one of my literary heroes, kept two diaries, one for his biographer and one for himself. I suspect there was a third, one revealed in a book he wrote late in life about his dreams. Reflections are designed chronologically as if life was lived that way. Life of course is lived forward and understood backward as if it has been written.

P. K. Manning (✉)
School of Criminal Justice and Criminology, Northeastern University, Boston, MA, USA
e-mail: pet.manning@northeastern.edu

I cast what I write about my work as a series of "lessons." The history of ethnography frames some of what is written here. Any serious discussion of fieldwork should begin with a consideration of Radcliffe-Brown and Malinowski, Bateson, Reo Fortune, and Mead,[1] but, alas, the sense of order and function that Malinowski and colleagues offered can no longer guide us. Modernity is complexity and bodes equality as a basis for order.

Clues to the Ethnographic Journey

Duke chose me, and my wife and I drove east to find it. She was accepted to finish her B.A. and I to undertake graduate studies in sociology. Graduate work at Duke was an uneasy combination of survey research and community studies with a touch of the Chicago School (Edgar Thompson and Donald Roy) and the Lewinian vision of Kurt Back's social psychology. My focus was organizations and occupations, and I did an interview study of doctors' politics, focusing on their views of the then threat of Medicare for my Ph.D. Prior to this work, I was touched by the writing, the hand of Goffman. Late in my graduate career—4 years only—I saw the blue bible of Alfred Schütz lying on a table in a colleague's office. In due course, I read of Garfinkel. These changed my perspective. I would never again do survey research as I found the context of answers was assumed, but in practice was a variable. When I arrived at Michigan State, I was welcomed soon by faculty and students in police administration, and I learned from them. I did some fieldwork in an emergency room and taught medical students. I had a position in the department of psychiatry in the medical school, and this sparked my interests in disease and illness. The fieldwork Horacio Fabrega Jr. and I did in Peru and Mexico shaped my interest in the rigors and seductions of fieldwork, and I learned the elegance of the techniques associated with the then-blossoming framework of cognitive anthropology. This work was on the one hand an exercise in eliciting and cognitive mapping and on the other a series of footnotes to Durkheim, with special interest in *The Elementary Forms of Religious Life* (1912).

Some Truncated Truths

I had a sabbatical after I earned tenure at 29 in 1969. The work I began in London in 1972 on policing was ill-developed, porous, and shaped by notions of honor and the police role. I had access via an officer from the Metropolitan Police who visited Michigan State and later sponsored me. This was not the formal and agonizing pseudo-legalistic matter currently in vogue. He called a friend on my behalf; I went

---

[1] I do not cite the works of these authors, but I suggest a book on the history of anthropology as a source for further reading.

to the subdivision and began fieldwork. I could ride a big Red London bus over and back in southeast London. I must say immodestly, perhaps, that I had not been taught fieldwork and I learned eliciting in field with Fabrega. The following will take something of the form of lessons.

The first lesson of ethnography is that the fieldwork is an opaque experience, shadowed with doubt and anxiety, and is ever so fragile—at any moment the work might crumble or you might. I did not use a tape recorder, I did not use shorthand, and I did not type up my notes. I only had this luxury later in joint research with Larry Redlinger and Jay Williams. I wrote notes in spiral notebooks which I still have. I relied on my then sharp memory to provide the continuity and the framework for the work. I had no theory per se, although I was much influenced by Durkheim via Goffman. I did attend the anthropology meetings in Oxford and presented a paper which was published in *Man*, the journal of the Royal Anthropological Society. The shadow of my work on police and policing was curiosity as I had had no experience with police or police officers until I came to MSU. I was intrigued by the workings of authority: how, when and where, and with whom it works and what the consequences were. This one-line summary I later learned in therapy—an analysis—was a way to weave together the brilliant nuances of my mother with my gentle and kind father's distant and sometimes amused attitude toward me. Authority was a mystery to me, not an abiding presence. I could step back and observe; I could see authority from "both sides." Authority is still a puzzle to me, and I find myself on one "side" or another of authority daily. My curiosity led my fieldwork in England for many years, and I did not have a "one-liner" about what I was doing or why. I did not have an agenda, a moral crusade or well-formed politics. The point of such a self-caricature is often missed—there were things totally out of the question and of no interest to me. I would not and did not study anything that did not at root have questions of power, of authority, and of resistance, perhaps even the shadows of the quest for equality. My criticisms of what I observed were rendered "between the lines."

The second lesson of ethnography as practiced is that detailed organized, systematic, and lengthy field notes are rare, perhaps a mythical idea. Clearly, many of the greats, including Gregory Bateson, for example, spent hours typing up notes in the oppressive heat of New Guinea, and Alice Goffman had more than a 1000 pages of notes before she destroyed them (I assume these were electronic). But as Jackson (1990) and colleagues have shown, the relationship between the field observations, the written notes, the analysis, and the writing up is problematic. Most fieldworkers do not begin with a theory, and few even have a formulated research question. They wander. The truth of field notes, that is, how much they reflect, interpret, distort, or omit some aspects of social reality, continues to be an issue for many sociologists. Know what you know and don't know and recognize what you have left out and why. At least two sociologists have gone to jail for refusing to produce their field notes, and the very virtuous and honest Alice Goffman was unfairly criticized in the media. In due course, she destroyed her notes of a published study and another work in progress to protect her informants. Increasingly, legal issues abide and are tangled with work on controversial topics.

A third lesson might be cast as one has to develop a style of listening, hearing, and writing out and up. I gave up fieldwork for several years, in part a consequence of a painful divorce; I could not listen well. My own emergent style, or ways of being in the world as a fieldworker, has features.

Field notes can be gathered and analyzed in various ways. Now, some initial cuts can be made with software. I recorded and had my field notes typed up and once I worked with colleagues and we taped all our interviews and shared them for writing the technical final report. Since then, I have not recorded my interviews or my impressions and leave them somewhat as iconic scrawls—modern-day hieroglyphics. This is a purposive diversion that makes my notes of little legal value.

Focused attention and memory are essential. I rely on my memory to shape the notes I have taken. I do not use index cards or software. These means do not produce the elegance of a scholarly articulation of the problem at hand. Evidence takes many forms. I rarely use quotes. I rely on detailed descriptions of events observed, vignettes that capture key events, e.g., raids, problematic traffic stops, and messages transmitted. I augment these with official records when needed. The narrative, after all, makes sense of events more than the often oblique comments of informants. The modern world of social media and smart devices, posing alternatively as cameras, phones, diaries, family albums, and software and electronic databases, requires rethinking of the techniques invented by Malinowski, Mead, Bateson, and the early fieldworkers in Oceania.

Fieldwork has many facets. Most fieldwork done by other than anthropologists until recently was done in the English-speaking world. The "culture" is not described; it is assumed given a location—a Los Angeles high-crime area or the inner city of Chicago or Philadelphia. My fieldwork on policing in Ireland in the Republic and in Northern Ireland (2008–present) required extensive reading of histories, biographies, and autobiographies; gathering of original sources, website materials, and official records; and reflection on told memories, folk culture, and music. The island, for better or worse, was a colony until 100 years ago. My aim was to understand, grasp, some sense of the cultural logic of the island. Listening to poignant songs from Ireland, songs of heroes and losses, death, hunger strikes, torture, and revenge, moves me—sometimes I cry, too.

In a sense, I see what I have written about in Ireland as a sedimentation, a filtration of feelings loaded onto words. The key ideas are emotional keys or springboards that touch off action and reaction in a kind of dialectic. Springboards resulting from a series of events stimulate, consolidate, and then shatter into revenge cycles. The dialectic of emotions, violence, and response cry out for explication.

A fifth lesson is while one copes and creates a path while walking on it, something like Heidegger's strolls in the Black Forest after his liaisons with Hannah Arendt, much of what is written is framed after the fact, writing with "style and polish" of course, and writing carefully of the third worst thing that happened in the field. This dramatizes one's humanity. The advice found in little handbooks on ethnography emphasizes the formulaic mode of "writing up." There is no one way to "write up," and most ethnographers come to see, late in the day, that they have a lot of data but not an identified problem. I set out to write *Police Work* (1977) while at

the Department of Justice in Washington D.C. in 1974–1975. I began to realize: I had no knowledge of the history of English policing except what I had read in England and seen on BBC in London was chauvinistic and misleading; the rational-bureaucratic picture of policing in the literature was false, deeply anomalous when confronted with facts, and that systems theory and structuralism, of which I had become recently enamored, was ill suited to explaining police organization, enactment, and police officers' behavior. Recall also there were a few soon-to-be classics in police studies and there as yet was no field of criminal justice. There was criminology, but I was and am not a criminologist. Finally, I thought that perhaps Goffman's idea of dramaturgy (with influences from Kenneth Burke) might be recast as a framework within which to analyze police organizations' actions-strategies, tactics, and the work of maintaining a mandate. I had entertained this idea earlier in general but not as a framework for analysis of my field notes.

Around this time, the late 1970s, I became aware of a number of matters concerning my fieldwork and framework. My theorizing was a kind of synthesis via Erving Goffman that recalled Durkheim, not Mead (1934), Blumer (1986), Becker (1963, 1998), or other symbolic interactionists. I was not a symbolic interactionist although I published in the journal and was a member of the editorial board for some years. I was not a pragmatist; I did not see the self as the center of my analysis (the organization as an actor is); I was struggling with the limits of a kind of existential sociology. I think this is an unexplored theme in Goffman's work. I was aware that I was struggling with something new in the sense that the combination of fieldwork and theorizing was a fragile combination, perhaps only done well by Erving Goffman and anthropologists I admired in the dramaturgical tradition. I wanted to frame organizational action as a kind of drama, a drama of power seeking and behind that secrets and secrecy. Unlike most others, I was a comparativist with an interest in history; all of my work is based on differences between organizations, usually in more than one country, and the differences between the differences of note.[2]

In later work, after 1979, while I was in Oxford on a research leave, I sought to refine my sense of what my scholarly quest was and why. In later work I was trying to refine the pieces of my work as a configuration and as a continuation of the question of the role of information in deciding. The question, which I raised in *Symbolic Communication* (1988), was what is the process by which, in a given surround or social context of politics and economics, a fact becomes information on which one can act? These actions are a product of interpretation from the call to the officer and subsequent actions. This imbedded action sequence or enactment was now imagined retrospectively as parallel to what I attempted in *Police Work*. The organization worked to establish the fundamental drama of policing—how to be authoritative, to display authority and maintain it through performances. This applies both to the organization and the officer as actor. In these performances, I was exploring the role of information as against ritual or redundancy in shaping police actions at the organizational level.

---

[2] Bateson (1972) argues that the difference that makes a difference is a fact. This is explored in anthropological detail in *Naven* (1958). This somewhat opaque book has had a continuing influence on my work for almost 50 years.

This meant also examining in time the extent to which the police organization created the information it used through its own processes of case-making and investigation. These three moves, from 1972 work to work in the late twentieth century, lead to a re-framing of policing using Goffman's much misunderstood *Frame Analysis* (1974). In effect, in my philosophical socio-political work, I built on my ethnographic work, but *Democratic Policing* (2010) was not an ethnography. The themes struck in *Democratic Policing* lead to a return to ethnographic work in Ireland. What are police good for? What features define democratic policing? Thus, a career may or may not have continuous themes, and the expressions of these themes vary in the work, and surface does not always "tell." My work, in short, is a systematic exploration of decision-making in the context of authority. This epigram covers the work on illness and help-seeking in Latin America, on nuclear power regulation and legal deciding, and on policing and its faces and guises. In arguing this as a career line, I willfully ignore the envy, rivalry, competition, and malice afloat in academic-life-as-a-business run by "senior teams," various executive officers, a pride of vice presidents and provosts, and a gaggle of managers seemingly free from the historic moral and ethical constraints of the university.

Yet another lesson or rule of thumb, the sixth perhaps, is that one's "theory" changes and is an adaptive cloak for one's work. The flexible version of cognitive anthropology I grasped became the basis for looking at semiotics, a related framework, and writing *Semiotics and Fieldwork* for a series I edited with John Van Maanen and Mark Miller. It is still in print. My work in England on nuclear power led me to try to understand semiotically the ways in which rhetoric and politics shaped views of the material world. Here was the anomaly: the reactors that produce nuclear power, the most dangerous engines of power in the world, are everywhere defined by governments as "safe." The work I published there was a combination of my wonder at such politics and machinery and my trying to refine a semiotic method.

Lesson number seven. Beware of making policy statements. Until the beginning of my work on democratic policing, I was not much concerned with policy although *Police Work* and *The Narcs' Game* have policy comments. My basic assumption, which I still hold, is that academics are very poor at shaping policy in their writings although they may be quite significant contributors as active policy-makers. Writing it up does not make it true or effective. In the work I did on nuclear power regulation, I was intrigued by the anomaly mentioned above, that the most dangerous machinery in the modern world other than nuclear warheads and bombs was deemed safe, defined as safe. I worked out the semiotics of this, not the reality of it. Similarity with the analysis of drug police. Does one want an efficient police organization? No. Could the drug policing game be made more "rational" and ends-oriented? Yes. Would this happen? No.

Lesson eight is about selecting a story. Although one story may be told in the text, this story is but one among many that might be told. Although ethnographers are devoted to discovery procedures, there remains a sense in which ethnography is storytelling, a modernist narrative, in which ethics become a later concern. I have written about this in an earlier chapter and have concluded there are no possible

written rules to guide comportment in the field. This is not very sociological. The efforts of IRBs (on which I have served) cannot possibly anticipate the complexity of ethical and moral decisions faced in the field. Doctors are trusted, social scientists are not, and ethnography cannot be guided prospectively. The net effect of IRBs is placing more emphasis on supervision of Ph.D. candidates, not on ethics or morals or the actual conduct of research. I have done unfunded research for the last 10 years or so. Formal statements of ethics provide no guidance to the where, when, and how of fieldwork. Students are taught the rhetoric of the IRB, the ritual chant about what one intends to and that certainly provides constraints. I have tried in my own work to observe several propositions. Don't lie about what you are studying—develop a brief rationale and stick to it: I'm writing a book about how police decide and what they view as the job. If they ask why, I say because I want to be a famous scholar. In my experience, these little descriptions are not believed in any case by police. I have several times returned to departments I studied and gave them copies of the book. My obligation in the field is to those who have trusted me, given their time, efforts, and attention. I honor that trust. My career rests in part on the kindness, toleration, and understanding of police in several countries, and I am forever grateful to them. This does not reduce criticism; it makes for honest critique based on knowledge of the practices of police. In any work, mistakes called many things are keys to understanding "good work" and call out remedies, excuses, and white and other lies; they are windows into the craft. I have written that not all that is counted really counts as good work and also that observing the police may require "not seeing" and using judgment in what is written. In some sense this means keeping some distance from events, e.g., by asking informants to describe scenes they view as mistakes or "not good police work." After all, most ethnography is not based on what is seen, but what is "out of sight." By this I mean the taken-for-granted, the assumptions, and the understandings at hand that frame observed incidents. Although ethnography is a certain kind of puzzle-solving, there is no "picture on the box" to guide assembly to guide decision by analogy.

A related lesson is that ethnography, more than other research, is sensitive to the current political and economic trends. The concepts, the concerns, the focus, and the narrative are all time-sensitive and affected by the current political climate. That is what ethnographers study. It is essentially existential and an act of creation or enactment. The situation in large part is a function of the age, gender identity, and career point of the researcher. Coping with this, many well-done studies show, is "part of the job." This leads to a second point concerning the situation—*ad hoc* problem-seeking means that the complexities of a given study are not reflected in cumulative knowledge. Very few studies have been re-studied, and the absence of a general theoretic framework in ethnographies means that they are "handicapped" in the "scientific race." There are a few of these sequences, but each tends to reveal anomalies. Very few ethnographers have set out to pursue an underlying theme, a cumulative pursuit, a life course of analysis within a framework.

Finally, as regards my "reading" of my own work in connection with career and the times and issues of ethics and policy, I must admit that fieldwork is a young person's game. Saying this begs the question of how it is possible to do extended

fieldwork in the current environment of valuing sterile metrics and "little statistical essays." Here, one needs two things: a 5-year scholarly plan for yourself, not an administrator, and a plan to write papers from the data, each of which is a chapter or important piece of the book. This both announces the forthcoming work to other and provides an ongoing view of the data, argument, and table of contents. Why do I claim nevertheless that ethnography is a young person's game? It requires patience in gaining access, working the field once in, writing up, and perhaps publishing; it takes a degree of good humor and tolerance in the field; it often requires travel and incommodious living; it may take considerable time before and during the research period to reveal an important issue to pursue; it can be exhausting, frustrating, and demanding emotionally for the fieldworker and intimate associates. It also seems to me that few important ethnographic works are done in late career, e.g., after 60. In addition, I know my memory is not what it was and perhaps even what I thought it might have been—I find notes less clear and can't interpret them (my sort of shorthand); I find I have forgotten parts of the notes I have written as I review them; I misplace bits of paper (envelopes, napkins, handwritten *ad hoc* notes) and, less so, computer files. I am not always clear on which of the two or three computers and flash drives I have stored materials on. These little stumbles and perhaps some little victories underscore the primary fact—fieldwork and ethnography is a series of acts that combines observation, insight, imagination, and memory. These are not all sharpened by age, but they may not have ever been present. Everyone urges scholars to be ambitious, seek fame, and do good works, but very few people will tell them to slow down or stop. They may confront this alone, in a dark motel room with the remote in hand, but hope still floats somewhere in the room. It lingers.

The future holds more complexity. The tools of the trade used to be few, but now the everyday equipment is more sophisticated than the computers of 20 years ago; GIFs, emojis, pictures, sound, and videos are almost essential in teaching and perhaps in modern fieldwork. Tweets may have a role in fieldwork relations, as might other social media such as e-mails and texting. There is a turn to the visual and to the affective. Police organizations, although wary of social media, use Twitter and have elaborate websites and crime mapping capacities. Perhaps fieldwork in the future will be working the images, like working the room, grip and grin, as in local politics. This is not so much laying out a linear argument as a wrestle with images, truncated, elided, threaded, and woven back, a loose rope. A literary example is Joan Didion's recently published *South and West* (2017). An analysis, some fieldwork might begin with some idea about light:

> In New Orleans in June the air is heavy with sex and death, not violent death but death by decay, overripeness, rotting, death by drowning, suffocation, fever of unknown etiology. The place is physically dark, dark like the negative of a photograph, dark like an X-ray: the atmosphere absorbs its own light, never reflects light but sucks it in until random objects glow with a morbid luminescence. (Didion, 2017, pp. 5–6).

What I intend to capture here is that pursuing insights, shades of light and dark, illumination of various sorts, is a metaphor for fieldwork. Here images, the

visual, beckon and the puzzle-solving job looks like mirror work; the task is how to separate yourself and the personae in the field from the light and the darkness that surround them.

References

Bateson, G. (1958). *Naven*. Stanford: Stanford University Press.
Bateson, G. (1972). *Steps toward an ecology of mind*. San Francisco: Chandler.
Becker, H. P. (1963). *Outsiders*. Glencoe: The Free Press.
Becker, H. P. (1998). *Tricks of the trade*. Chicago: University of Chicago Press.
Blumer, H. (1986). *Symbolic interactionism*. Berkeley: University of California Press.
Didion, J. (2017). *South and west*. New York City: Vintage.
Durkheim, Emile (1961 [1912]). The elementary forms of religious life. London: Macmillan.
Garfinkel, H. (1967). *Studies in ethnomethodology*. Englewood Cliffs, NJ: Prentice Hall.
Goffman, E. (1959). *Presentation of self in everyday life*. Garden City: Doubleday.
Goffman, E. (1967). *Stigma*. New York: Simon and Schuster.
Goffman, E. (1974). *Frame analysis*. Cambridge: Harvard University Press.
Jackson, J. (1990). *Fieldnotes*. Ithaca: Cornell University Press.
Manning, P. K. (1977). *Police work*. Cambridge: MIT Press.
Manning, P. K. (1988). *Symbolic communication*. Cambridge: MIT.
Manning, P. K. (2010). *Democratic policing*. Boulder, CO: Paradigm Publishers.

Doing Treatment Ethnography in Justice Settings: Reflections from Two Decades in the Field

J. Mitchell Miller

Introduction

Through a combination of choice and happenstance, I've been involved in some form of drug ethnography since entering graduate school in 1991. After writing a natural inquiry style master's thesis on asset forfeiture-oriented drug enforcement operations and then a more traditional methodological in-depth interview-based dissertation on confidential informant utilization, I fell into a co-investigator role on a National Institute of Justice grant during my first semester on the faculty of the former College of Criminal Justice at the University of South Carolina. This 1996 project was an implementation and process evaluation of the South Carolina Residential Substance Abuse Treatment program delivered inside a medium-security state prison. The research team reviewed program training materials and other official documents, observed treatment sessions, and conducted combinations of unstructured in-depth and semi-structured focus group interviews with prison administrators and staff, treatment team members, and the treatment inmates themselves.

Fresh out of graduate school, I still remember the sense of importance and excitement associated with this project. That I was collecting original data from drug offenders face-to-face to inform programming decisions in the context of a federal grant provided a sense of professional purpose that has long lingered. Since then, I've been fortunate to conduct program evaluation research across the country for various federal and state funding agencies at a fairly constant and sometimes demanding pace of as much as 12–14 days of site visits a month for several successive months. After working on three overlapping federally funded offender treatment program evaluations in central Ohio with each requiring quarterly site visits, for example, I

J. M. Miller (✉)
University of North Florida, Jacksonville, FL, USA
e-mail: mitch.miller@unf.edu

was presented a modest gift basket from the Hampton Inn in Sunbury about an hour north of Columbus off Interstate 71 for being the most frequent guest in 2013.

I've often thought that my life circumstances of marrying later in life, and then to a talented fellow criminologist understanding of and often along on grant trips as a research team member, and not having children to raise have afforded a greater degree of autonomy than had my choices led to more normal life obligations. Seemingly, there are numerous talented ethnographers and applied fieldworkers who simply can't be on the road frequently or for extended periods of time. Also likely, the gross underrepresentation of ethnography in grant work is because criminology and criminal justice, as well as anthropology and sociology, have historically researched drug ethnography topics (illicit drug use, drug subcultures, drug crime, and drug enforcement) with a critical anti-system tone at odds with justice agency agendas. Not surprisingly, most drug ethnography has been in the form of unfunded pure research.

The mixed-method program evaluation I've been involved in has, for the most part, multiplied into additional research opportunities through both development of existing partner programs and replication of effective treatment modalities into new jurisdictions. Successful research-practitioner partnerships often morph into continuation, enhancement, and similar sustainability projects, and successful work, particularly at the rural county level where I primarily work, can generate programming migration from an original to adjacent partner counties across funding cycles. In this way, the development of research-practitioner partnerships provides research settings for the discovery of effective practices that, when implemented in nearby settings, establish regional evidence-based treatment cultures.

Clearly, it has been the site visits and personal interaction with various criminal justice authorities and functionaries that have generated return business and not sterile statistical reports relating to program outcomes. I've often wondered to what extent "sales pitches" to potential practitioner partners (sheriffs, police chiefs, wardens, jail administrators, judges, etc.) over the years have been received as sincere and proven successful simply because the mixed-method studies I proposed involved recurring site visits—meaning we would be seeing one another on a recurring basis. Simply being there has made a great difference as it authenticates the importance of practitioners as important actual partners in a synthesized offender treatment and evaluation venture as opposed to minimizing their role to quantitative data transmittal. While I have great respect for the social betterment that can be derived from the predictive power of statistical analysis, social science that doesn't involve social interaction and appreciation of heard and seen data has never seemed very social in nature and less real to me.

My general characterization of applied fieldwork and what should be considered a strong, moreover an optimal, program evaluation design has changed considerably over the last 20 years as the form of applied fieldwork we practiced in the 1990s through the turn of the century contrasts sharply with the qualitative research I currently conduct, advocate, and refer to as *treatment ethnography*. When I started 20 years ago back in South Carolina, evaluation research designs typically neglected qualitative techniques that were almost categorically seen as serving supplemental

functions, particularly providing descriptive context of treatment environments so that statistically determined outcomes might be better linked to agency and other therapeutic settings. The objectives and nature of applied fieldwork have evolved considerably since so that the old goals of description, exploration, and contextualization are now seen as simplistic and limiting, if not insulting, as ethnographic work in program evaluations is serving more advanced and important functions.

With "outsider interloper" and "academic-know-it-all" stigmas still alive, salesmanship is requisite for would-be evaluators that too often must contextualize proposed research activity in a justificatory fashion. While power of persuasion will always factor into competitive research awards, the ongoing evidence-based practices movement should soon ease the longstanding tension between researchers and practitioners by ensuring program evaluation in virtually all new initiatives through grant requirements and norm propaganda reinforcing evidenced treatment practices and evaluation as the new normal. Most of the federal funding agencies maintain registers of evidence-based practices graded per degree of research evidence, most notably CrimeSolutions.gov which identifies rigorous research to orient juvenile and criminal justice decision-making in predictable approaches known to work.

The evidence movement presumably will bolster treatment ethnography because of the superiority of mixed-method research designs to singular-method alternatives. Qualitative techniques addressing program integrity and implementation intensity are necessary to meaningfully attribute observed statistical outcomes such as recidivism and relapse to treatment rather than modality variance or mere coincidence. In order for observed statistical findings specifying program outcomes to be optimally credible, validating qualitative process research first must be conducted so as to establish program fidelity and distinguish between theoretical (modality compliance) and implementation (quality of services delivery) failure. By developing a program fidelity instrument to gauge treatment dosage, participant engagement, counselor enthusiasm, program differentiation, and similar concerns (see the *Justice Program Fidelity Scale*, Miller & Miller, 2015) that are scored through observation and interviews, we hopefully have helped elevate the role of qualitative work in evaluation from description to confirmation and validation.

As do most of my qualitative criminology colleagues, I view ethnography, especially the natural inquiry covert participant style I've long advocated and defended, as a methodology themed by immersion into real-world settings where minimal intrusion in the research setting is a goal as "research noise" might prompt a Hawthorne effect masking the very social dynamics of research interest. Short of being sentenced into a treatment program or taking a job as a treatment counselor in a correctional setting, however, it is highly unlikely that any real immersion into jails or prisons is feasible even if there was a researcher willing to go to such extreme lengths. The idea of some type of covert participant observation study of treatment is far-fetched and even more so in the context of federally funded evaluations.

When I first realized that a purer ethnographic approach rooted in principles framed in early anthropological and sociological fieldwork was not possible, I falsely framed funded applied fieldwork as watered-down qualitative research that largely abandoned open-ended inquiry in natural settings for scripted data collection.

Some have characterized my qualitative program evaluation work as "selling out" to the interests of the justice system. For a long time, I viewed applied fieldwork as a routine data collection endeavor comprised of posing the same set of questions to multiple focus groups in succession over two or three days, confirming counselor credentials, scoring observed treatment sessions, and the like. Administering interview guides entailed fairly bounded dialogue with a lot of people, but this wasn't really speaking with them as in real ethnography. In some respects, applied fieldwork approximated the reductionist quantitative approach so disdained by ethnographers through prioritization of research *foci* predetermined before engaging research subjects or entering the setting.

In treatment ethnography, the usual challenge of immersion into the research setting (a nonissue when doing observational or interview work in justice agencies and correctional facilities where employees simply usher you to waiting subjects) is replaced with rapport development. Do the inmates see the treatment professionals (and me) as functionaries of the system, someone to break up the monotony of the day, a bother, or something more consequential? A traditional ethnographic approach as exemplified by participant observation isn't practical in jails and prisons as interviews are rarely unstructured. Set or semi-structured interview guides better align with evaluation data collection needs if not also correctional over-watch accentuated by controlled movement to interview sessions. Researchers are free to leave at the end, and treatment offenders are not so pretending we aren't in a correctional setting and that everyone is equal has always seemed disingenuous and apt to be seen as patronizing behavior that would make the offenders dislike me and hamper optimal data collection.

Rapport establishment with research subjects is always a concern but all the more so when the population of interest is comprised of drug-addicted offenders typically with mental health disorders. Though cognizant of their unfortunate and vulnerable situation, I simply engage them straightforwardly as though they were not in jail or prison and, until evidence suggests otherwise, rational actors. I try to keep introduction of myself and coverage of informed consent forms as brief as possible, as I assume these initial steps are deemed boring, before describing what my role as program evaluator is and what research activities will be forthcoming. And I try to suppress the urge to engage in search of finding common conversational ground that might augment rapport—in jail and prison evaluation work, there simply isn't time for that much anyway. Moreover, it violates the basic interview axiom that subjects, the source of the data, should be doing most of the talking.

Mostly, I try to treat subjects with respect, answer their questions honestly, and attempt to balance talk time across individuals during focus group interviews. Like the search for commonalities, I have to fight the tendency toward wanting to get them to like me and the presumption that doing so will get them to open up and tell me more about program realities. Over time, I've come to see such efforts as counterproductive as problems have stemmed from over-rapport. Demonstrating my familiarity with drug subcultures, recent drug use trends, and jargon can foster common ground (in part by showing I'm nonjudgmental) for more open discourse, but being seen as a friend has led to requests for legal advice and personal favors

including smuggling contraband (the most common item requested over the years has been alkaline batteries). Hence, I try hard to project a balance of professionalism (in hopes of keeping limited time with subjects as focused as possible on research topics) and relate-ability (so they hopefully see me as someone both interesting to dialogue with and that their input is important).

Today, I've largely reconciled the differences between ethnography and applied fieldwork that bothered me early in my career, largely by fusing the tactics and spirit of the former into the latter. Methodologically, replacing structured interview question lists with a more flexible semi-structured questionnaire approach has ensured systematic and linear coverage of program topics while allowing for follow-up questions and comments that enable clarification and exploration and provide more of an ethnographic feel. Whereas my earlier fieldwork questionnaires captured respondents' characterizations of program personnel performance and whether program components were deemed helpful, a more open and natural dialogue provides more holistic and better knowledge about program participation and influences on behavior ultimately impacting program outcomes. By establishing rapport and simply conversing, I've learned far more than would have been possible by sticking to the script. This is almost certainly because the respondents are actual actors in the setting of interest and experience the totality of life events rather than only the things researchers may think to include on a question list.

So, in a very literal sense, treatment ethnography is the most important thing I do professionally. The research-practitioner partnerships formed introduce substance abuse treatment into new justice settings, often in poor underserved very rural places. It is almost certain that some, perhaps many, of the drug offenders that volunteer for or are sentenced into treatment programs I've assessed, particularly in resource-scarce rural areas, would otherwise have remained untreated. This point assumes pretty heavy pragmatic bearing when the setting is in the epicenter of the nation's opioid overdose epidemic in central Ohio or in rural southern Georgia where treatment resources are virtually nonexistent to the extent that for some counties the only practical option for addressing offenders with drug and mental health disorders is transfer to the state line. Luckily, the current convergence of growing treatment needs and the rapid rate of program implementation present new "quality of intervention" focused research opportunities for qualitative criminology and drug ethnography by mainstreaming applied fieldwork into treatment success calculations.

Reference

Miller, J. M., & Miller, H. V. (2015). Rethinking program fidelity for criminal justice. *Criminology & Public Policy, 14*(2), 339–349.

Getting In by Being Out

Vanessa R. Panfil

A Path: Charted

My mother insists she knew the moment that I wanted to study LGBTQ people's experiences with crime, violence, gangs, and the criminal justice system.

I was about 5 years old. Around that age, my mom would spend special time with me by taking me to McDonald's for lunch. We would walk or take the bus, and on the days we walked, my legs felt every yard of the 2 miles it took to get there. That McDonald's had a very small merry-go-round that seated three children at a time, which I loved. I didn't really eat meat back then, so they would make a grilled cheese for me by sandwiching American cheese between two buns flipped outward and heating it. I also looked forward to eating a very tiny ice cream cone with orange sherbet. My mom always got a coffee with four creams and emptied only about a third of a sugar packet into it, stirring it slowly and sipping it still piping hot. We'd sit across from each other in the back booth and talk. I felt very grown up, like we were on a very important appointment. It was definitely a treat.

Thinking solely of what I would eat, and looking forward to what we would talk about, I was a bit startled when we walked in the door that day.

> "Ray-Jean! Ray-Jean!" That isn't my mom's name, but that's what he calls her. I'm not even sure how it should be spelled.
>
> Her eyes lit up. "Martez! We missed you! Where have you been?"
>
> "Oh gurl, my boyfriend and I got in a fight and I hit him in the head with a hammer! I was in jail, gurl!"
>
> Her reaction sounded shocked and guttural: "Oh my god, Martez!"

Martez was a manager and was always exceptionally nice to us, and we liked him. I don't think I'd ever given much thought to whether guys had boyfriends, or whether boyfriends ever hurt each other, or what it meant for someone I knew and

V. R. Panfil (✉)
Old Dominion University, Norfolk, VA, USA
e-mail: vpanfil@odu.edu

liked to go to jail. Weren't jails for mean people? Isn't it really bad to hit someone you like—or anyone for that matter—with a hammer? I was a bit confused, but they talked like this was something that happened, so I figured, I guess boys have boyfriends and sometimes they hit their boyfriends or other people.

Another 6 years passed, and my middle school required science fair projects. I wasn't all that interested in anything involving a light bulb, or a simple machine, or household chemicals. I wanted to find out what was going on in my classmates' lives. In sixth, seventh, and eighth grades, I conducted surveys of my peers. I created the surveys, tried to make mutually exclusive categories, and was guided by a research question. I went into other classrooms to get permission to survey students. I didn't have any incentives to offer them and certainly didn't go through any ethics boards. Now that I think back on it, I'm actually a bit surprised I was allowed to do these, especially since 1 year I asked about alcohol, tobacco, and drug use. Each year, my focus was on student performance outcomes, since I was talking to students, and I had an understanding of what it meant to be a student. One year I tested the effects of getting sleep and eating breakfast, another year I tested parental involvement, and another year I tested delinquent activities including substance use. (My parents had a program on the family computer that could make basic bar graphs, so these projects were mostly descriptive or about correlation, but I probably thought I was tapping into causation.) Although in seventh grade I was getting a D in health science for not turning in worksheets about cellular development, I won the science fair for my survey project about parental involvement's effect on student performance. I was much more interested in the latter. I didn't realize it then, but before I was even a teenager, I was already a social scientist who was interested in young people's outcomes. I also came out in middle school and found myself in all sorts of challenging interpersonal interactions as a result of that in middle and high school, but I got through them mostly unscathed, and ultimately succeeded academically. My science fair studies can help me make sense of why I survived: I had food to eat, a safe place to sleep, and parents who supported my identity. I knew other LGBTQ peers who faced many challenges beyond being gay—how did they deal with those? I started volunteering for an organization that provided trainings in order to improve school climate for LGBTQ students, primarily high school students. I carried around a small whiteboard and dry erase marker and went to workshops around central Ohio, and unsurprisingly, I talked not just about student safety, but about their academic performance as well, and what we could do to support students like me.

Another 6 years after the science fair, I was in college and volunteering for several LGBTQ advocacy organizations. I did a survey-based project for any class I could: a survey comparing attitudes of LGBTQ and straight/heterosexual students toward the use of identity terms that were reclaimed vs. those considered more universal (e.g., queer/dyke vs. gay/lesbian) for a linguistics course on semantics, a survey about knowledge of safer sex practices and HIV transmission for a "women and science" course (where I was shocked at how many people my age still thought you could contract HIV from "sharing a sandwich"), and a survey about students' attitudes toward transgender people and how they varied according to political affiliation for my research methods course. I always included open-ended questions on

those surveys and was truly more interested in responses to those, but I just wasn't able to conduct interviews instead of surveys for these projects. I really wanted to know what people thought or believed, in their own words, instead of being restricted to the few options I'd given them.

In college I also began volunteering for different kinds of LGBTQ advocacy organizations and taking on more responsibilities within them. I became very active with an LGBTQ youth drop-in center, first as a high school student seeking services and opportunities for involvement in advocacy and then as a college student and recent college graduate who volunteered for the center and mentored youth. I also held leadership positions with this center. During my later years with the center, the demographic was primarily urban, nonwhite kids who received additional services from other community agencies or the state. These youth were also in the process of questioning their sexuality and/or gender identity and, for many, coming out as gay, lesbian, bisexual, transgender, or another "queer-spectrum" sexual or gender identity. There were some who we would not see for weeks or months at a time, only to later find out that one had been arrested and spent time in a juvenile detention facility for assaulting a staff member at a group home. Another had been removed from the only foster parent he had ever lived with who was actually willing to acknowledge his gay identity. As a member of the Youth Advisory Board for the center, I was part of the first discussions the center had ever had about youth who came to the center with knives or guns. While it was imperative that we maintain a safe space, we knew some youth carried these weapons for personal protection, especially when moving through their neighborhoods, and we had to create policies to deal with this reality.

One young man who I met then and who I later interviewed for my study, Imani, told a harrowing story of walking home from school and being repeatedly harassed by a group of three men for being a "faggot." He was threatened with violence and chased, and he thought they were going to jump him. After two instances of their harassing him for being gay, following him, and attempting to beat him up, he felt compelled to fight back. During this fight, Imani ended up cutting one of the assailants with the blade he carried, and he was able to get away. His family moved from the area shortly after that. But I thought right then and there, what does it mean that gay young people are fighting back and joining gangs? How do they experience that? I kept thinking someone should find out their stories and in a more systematic way than hearing them occasionally during volunteer hours.

By then I had already declared my college major as criminology, due to a long-standing interest in violence prevention. But my time with the center made me think about how the class, race, and sexuality of these youth combined to shape their decisions, specifically regarding their choices to join gangs, fight, sell drugs, and sometimes even sell sex. Though we shared some experiences related to growing up gay, their involvement in gangs, crime, and violence was vastly different than my own life experience. I felt I needed to learn more and took it upon myself to do so. I wrote my applications to graduate school saying I wanted to study gay gang- and crime-involved men, more than 15 years after I found out why Martez hadn't come to work in a while.

"Go to the People"

Issues most of interest to me have to do with experiences and identities: the ways people think about themselves and their lives and the ways they interact with others. Those questions, like *how* and *why*, are especially amenable to qualitative methods such as in-depth interviews and ethnography. You have to actually ask people what they've experienced and how they think of themselves and their lives. Just like one source I read in my doctoral-level qualitative methods course suggested, "Go to the People" (Bogdan & Taylor, 1998). I have gone to the people and continue to do so. For my study of gay gang members, however, I wondered how (or if) they might receive me. I first started by calling up people I had known from the youth center a few years earlier and seeing if we could get together and if they would talk to me about their lives and maybe help me meet other gay men involved in gangs and/or crime. I used other strategies to recruit, but snowball sampling was how I recruited the vast majority of the men in my study.

Qualitative researchers often face challenges related to access—how to successfully recruit participants and gain rapport in order to produce rich and revealing data, especially about illegal and stigmatized activity. This was very relevant for my study, as their dually stigmatized identities (gang members and gay men) and their involvement in illicit economies meant that they would not have trusted a complete outsider. The vast majority of participants ended up being men of color, and some would-be interviewees declined to be interviewed, for fear that I was a police officer. My sex and race were factors I often had to negotiate in the field—often being the only white person and the only person assigned female at birth.

While most of my participants and I were different on many dimensions, including biological sex, race, education level, and current or former gang status, our shared experience of growing up gay allowed for us to have some shared understandings to build upon in interviews. Just as I wanted to study my fellow students when I was younger because I had some idea of what students experienced, I felt a related pull to find common ground as well as divergent paths in our interviews. I deeply understood their narratives of feeling "different" from a young age, that being gay means you have to tell people you're gay, over and over (with some announcements being more consequential than others, though many are with little fanfare or serious consequence), and the struggle to live as an openly LGBTQ person while steering clear of pitfalls. We all had lived and come out in a society where conservative political rhetoric compares even committed same-sex sexual relationships to incest and bestiality; some of us came out before the US Supreme Court struck down all remaining domestic sodomy laws in their 2003 *Lawrence v. Texas* decision. I too had lost friendships after coming out, been called a dyke or a fag from moving cars or people passing on the street, and felt unsafe more times than I care to think about. While Columbus was a gay mecca, certain parts of it and surrounding areas were decidedly unsafe, what one of my friends crudely referred to as "a hate crime waiting to happen." My initial sample of research participants and I even utilized the services of an LGBTQ youth drop-in center at the same time; it's precisely how we came to know one another.

As further indication of my gay "cred," I am well versed in "gay slangs" (in the words of one participant). Having some insider knowledge can assist in gaining rapport, asking effective follow-up questions, presenting plausible hypothetical scenarios, determining the veracity of responses, and providing a general sensibility for the topics discussed, such as not imposing stereotypes on participants. One indication that they felt a shared experience was the many times when they didn't think they needed to elaborate on a gay-related concept and would say things like, "You're gay, you understand." I understood the context, sure, but I almost always pressed for detail. I cannot presume that I do know what they mean—we had many differences that could account for different experiences. I also didn't want to assume that any particular experience was exclusively related to their sexual identity, when it was likely dependent on their sexual identity *and* other social statuses, such as their race. I would ask for them to explain it to me, so I could hear it in their own words, or sometimes I would ask for them to explain it "to my tape recorder." Interestingly, they would sometimes ask me about lesbians or what happens in the "lesbian community," indicating that we both had something to learn about the other's social worlds and networks. One way to retain rigorous data collection and analyses is to never assume that we know how our participants think, feel, and act as a result of our shared identification.

Being gay isn't the only identity important to me and to the men in my study, but in this case, our shared membership in the LGBTQ community facilitated my successful recruitment, rapport with participants, and ability to generate high-quality narrative and ethnographic data. It seems evident that my knowledge of gay culture and my own queer identity made my participants feel most able to relate to me. It was sometimes our only shared characteristic but seemed to literally transcend all other boundaries.

One moment that helped connect me to entire gangs actually hinged on my queer identity. It was October 2011. I had interviewed Imani (a very well-connected person) the day before, during which he told me that he'd talk to his posse about me. He said he needed to get them all in one place, so I could do a few interviews in one night, if they agreed. He called me with the address, which actually happened to be the family home of Aga, a member of Imani's gay gang. Aga's mother and several other extended family members were even there at the time. I entered the screen door to find a metal folding chair in the middle of the room, surrounded by Imani, seven of his fellow gang members, and Aga's mother looking at me from a horseshoe of couches lining the walls. A little nervous, I sat down and began to explain my study's goals. After about thirty seconds, Aga's mom interrupted me: "Is *you* gay?" All I managed to get out was, "Yes, I am," before the leader of the gang, Javier, loudly exclaimed, "Aww, she *family*, y'all!" This was followed by Imani exclaiming even louder, "I *told* you," and a lot of excited talking and deciding who should be interviewed first, based on everyone's plans that night and their schedules for the next couple of days. Over the next several months, they and other members of their extended friendship networks made me feel very much like family; my acceptance originally predicated primarily on my queer identity and my desire to bring the stories of queer young people out of the margins and into the larger consciousness. Other men recruited to the study from their networks similarly greeted my queer self-identification with encouragement, such as "Get it, gurrrl" and "Now that's wassup." I got in by being out.

I can imagine a scenario where a heterosexual/straight person could have gained trust and rapport with these particular research participants, but it would have been a challenging feat if that researcher had very little in common with them on other dimensions. I know this because a number of them told me this explicitly or acted as gatekeepers of their own narratives and referrals until they received direct confirmation that I was gay. Jeremy effectively summarized why a project about gay people conducted by a gay person was important and worth being a part of: "Because! It's about time for my voice to get heard. It is. I been waitin'. That's what I really did it for. … We hear like, other like, straight novelists that put they ideas of what gay mean, you know? And no, we want somethin' that comes from our mouth." In his view, me being gay and out provided the credibility to represent their experiences accurately and bestowed upon me a vested interest in presenting authentic voices of the members of LGBTQ communities, since I have one of my own.

Although I've focused here on my study with gay gang members, my other two original, qualitative studies similarly explore issues of import for diverse LGBTQ populations, and our shared identification in queer communities is always pertinent to our rapport and exchanges. In Newark, NJ, I and my colleagues conducted surveys and in-depth interviews to investigate how urban LGBTQ youth of color navigate risk and enact resilience in their schools and communities, as well as their interactions with families, peers, police, and the juvenile justice system. In Sri Lanka, we conducted focus groups to explore how gay and transgender adults build community and construct identities to promote their own and others' right to exist in a criminalized state, with a particular eye toward continuity and change and their consequences, such as they relate to political participation or interactions with NGOs. In both of these studies, research participants were also interested in my own experience as a gay person, such as the youth in Newark asking me about things I had experienced in school and growing up or the Sri Lanka participants asking me about gay culture in the USA—were there gay bars, gay beaches, and gay churches, for example. They were interested in my life, just as I was interested in theirs. And, in the interest of basic reciprocity, when they ask me a question, I answer as honestly as I can. The same was true when I was in the field with gay gang members, who laid their lives bare for my eyes and ears. It was only fair to also give of myself. I wouldn't have any findings, contributions, or a career without the people who agreed to talk with me.

Balancing Acts

Ethnographic studies require many decisions during the conduct of the study that have to be informed by ethics and emotions and not just by practical concerns related to study design. Often, these begin with issues such as study scope and recruitment, continue to influence interpersonal interactions in the field, and affect decisions related to data analysis and dissemination of findings. In my study with gay gang members, I continuously encountered unexpected findings that expanded the scope of my project but simultaneously helped to clarify the potential

contributions of my study overall. Similarly, although I often negotiated my queer identity and other factors such as my race in the field, I had an overarching concern with the ways I would eventually represent my participants in my published works. I discuss these two issues in this section.

Before conducting a study, researchers consider the main concepts or research questions guiding their investigation. Because I had read very little about gay gang members, I wanted to conduct a fairly expansive study that would tap into the ways they construct identity, especially within contexts where their identities, particularly being gay and gang involved, seem to be so at odds with one another. Interview domains for my study were related to each participant's: life history, relationships and sexual identity, gang and/or criminal experiences, experience with the criminal justice system, and what it meant for him to "be a man," along with other topics. I borrowed some questions from or took inspiration from other existing study instruments with men, active offenders, and gang members but knew I'd learn many things that wouldn't line up with those questions. I went into the field knowing that I needed to trust the inductive process, to let important themes unfold as I did more interviews and spent additional time with the men in my study.

Consistent with this, some of my most compelling findings were those I didn't anticipate. By the end of my study, about half of my participants were or had been members of primarily gay gangs, with the other half being current or former members of primarily straight gangs (though, some primarily straight gangs had a critical mass of gay, lesbian, or bisexual gang members, which I refer to as hybrid gangs). I was thus able to focus much of my book, *The Gang's All Queer: The Lives of Gay Gang Members* (Panfil, 2017), on a comparative portrait of gay men in different types of gangs. My findings were able to reveal organizational differences that affected men and differed by gang structure. For example, the men in straight gangs were much less likely to come out to their gangs, as they were expected to be hypermasculine, tough, and willing to fight, and to date or have sex with women. Being openly gay could threaten their gang status and their personal safety; the handful of men who came out to their straight gangs were sometimes faced with serious negative consequences. But in contrast, members of hybrid and gay gangs were much more likely to come out to their groups and to be what they called "the real me." While they were still expected to fight and were discouraged from being flamboyantly gay or feminine in public, men in gay gangs sought to build a public reputation as a gay man, what they called becoming "known." Being "known" meant they were able to achieve many masculine ideals—making money, being taken seriously, gaining status, looking good (similar expectations for men in straight gangs)—but as an openly gay man. I could not have predicted the rich and revealing comparative study I would be able to undertake when I first entered the field, but I stayed attuned to these emerging differences and tried to flesh out more and more of them as I went. There were many other convergences and divergences across gang type that I discovered while conducting my study and which I describe in detail in my book.

Similarly, one of the most compelling findings of my entire study was on a topic directly relevant to what happened to Imani years ago but which I had no idea how common it was—fighting back after being insultingly called a "fag" or a "faggot" by straight men on the streets, on buses, in schools, or in bars. Fully 70% of the men

in my study had physically fought because of this. These narratives came up after a variety of prompts, and although I had been inspired to conduct this study partly by a scenario of this sort years earlier, I did not have a question pertaining specifically to this on my original instrument. What an oversight that was! But, luckily, I trusted the inductive process, and, once these narratives began to arise, I made sure to address the issue with subsequent participants. Such unexpected findings can even challenge or refine existing criminological theory (Panfil, 2015). For example, one theme participants talked about within the context of fighting back was what they called "fagging out": acting in aggressive and flamboyant ways simultaneously in order to respond to an antigay epithet by drawing on both normative and nonnormative resources (physical toughness + gay identity). Some of the best sound bites from my study were on this topic, such as "I will fight you like I'm straight" and "I'm gonna show you what this faggot can do," which directly address and refute negative assumptions about gay men while responding to antigay harassment (Panfil, 2014c). This theme also gave me an avenue through which to explore how school-based bullying and harassment combined with other life circumstances to contribute to gang membership, helping me return to another long-term advocacy and research interest of mine: LGBTQ youth in schools (Panfil, 2014b).

Uncovering such findings that revolve around gay identity, in a project undertaken with gay and bisexual men, by a queer person, also struck me as potentially incendiary if not addressed with care. This was similarly true since I am a white woman who interviewed and spent time primarily with gay men of color. As mentioned earlier, qualitative methodologists are often concerned with the politics of representation—a commitment to making sure the ways we discuss our participants are as ethical, nuanced, and accurate as possible. A concern that has dogged me for years is whether talking about LGBTQ people's crime, violence, and gang involvement is better left unsaid—will me talking about these phenomena fuel efforts to take away rights from LGBTQ people (Panfil, 2014a)? While I was concerned about this as I was preparing the first publications from my gay gang members study, I argued that it is better to show that LGBTQ people are capable of the full range of human experience, including gangs, crime, and violence. However, it seemed much more comfortable to do so when it appeared there was victory after victory for LGBTQ people's equity in 2010–2015, but now more than ever, I find myself attuned to this issue, as violence against queer people increases and states ramp up efforts to take away gay and transgender people's rights. These ethical issues will continue to be salient for me moving forward but certainly will not shake me from my path.

"Professionally Gay"

Among people who work in many fields, such as human services, nonprofit organizations, politics, and higher education, someone who self-identifies as LGBTQ and works on LGBTQ-related issues is sometimes jokingly referred to as "professionally gay" (or, among some older generations, as a "professional homosexual"). That is, their personal characteristics and professional commitments end up so intimately

tied in other people's perceptions, that they are regarded as inseparable. While this can unfortunately result in tokenism and being asked to speak for an entire (hetero-geneous) population based on one's own experience, it is also a way for those indi-viduals who want to make contributions informed by their experience to help others in the community. I knew that becoming involved in queer criminology as an openly queer person would entail certain implications and assumptions: that what I did was activism or advocacy and not scholarship; that my analyses were inherently "biased" and could never be "objective"; and concerns about rigor and transparency depend-ing on what my findings showed. I do not see advocacy and scholarship at odds, and I do my best (and have been recognized for) nuanced, rigorous/thorough, and ethi-cal analyses. And, of course, within qualitative methods, we believe the subjectivity of the work is actually its strength, provided that our position in relation to the study and our participants is made visible and analyzed just as the data would be. In my book on gay gang members, for example, I too am a character in the story and am constantly visible, if for no other reason than the book is written in first person.

Such concerns have materialized in reviews of my manuscripts. Years ago, a manuscript of mine was rejected from a top-tier journal for what the editor deemed "significant theoretical and methodological weaknesses" raised by a reviewer. The methodological concern was that, despite me saying that my interview instrument was informed by my conversations with *other* queer people over the last decade, a reviewer questioned whether or not I was gay, said I was being "coy" about it (which they said "isn't helpful and in fact is distracting"), and that I needed to discuss it "more directly so that this information can be properly digested by readers." On its face, this critique is minor, and indeed, it was fixed by me adding just a few words ("and my own lived experience as a queer person") to clarify; but it was evidently seen as communicating a larger constellation of critiques about the process by which people from a marginalized group study others from the same marginalized group. Recently, in a separate paper on a different project, my statement about being a feminist, queer researcher (as it related to analyzing complex and sometimes dif-ficult material) was critiqued by a reviewer that it "feels more a contrivance to deflect criticism" than "a moment of reflexivity." But, I *had* included it for greater transparency and so that a reader could consider my position alongside my analysis. Another reviewer on yet a different manuscript was conflicted about whether or not it would be fair to ask me to include materials critical of the LGBTQ community in order to have a more "objective" perspective but ultimately concluded that was not necessary (just necessary enough to raise).

As with the concept of being "professionally gay," scholars who utilize qualita-tive methods may be more likely to be seen as a qualitative methodologist more specifically, such as being interested in epistemology (the ethics, validity, and pro-cesses involved in conducting qualitative studies). Although many qualitative meth-odologists I know are intimately interested in the ethics, theory, etc. of qualitative methods, people do not necessarily specialize, though they are assumed to do so, "professionally qual," as the case may be. This is partially attributable to the relative lack of qualitative vs. quantitative studies. As with explaining one's positionality in a queer-themed study, misunderstandings of qualitative work can similarly result in unfounded critiques by unfamiliar scholars. For example, one of my mentors, a

leader in qualitative methodology, was once chastised by a reviewer for her "fail-ure" to use data analysis software to produce her qualitative analyses. As if it wasn't a person who has to read the words, code the data, and conduct the analyses—the brain is in the researcher, not in the computer program used to help organize and code the data.

Conducting qualitative studies, especially in graduate school, is sometimes seen to come with opportunity costs. Spending several years in the field to do an ethnog-raphy takes, well, several years, plus the analysis and writing time, which may be unattractive to students seeking doctorates and to doctoral programs who are con-cerned with their average time to completion. Appeals of various sorts are made to students with the intention of steering them away from qualitative research, which may regard their future abilities to publish qualitative work in top-tier journals, receive external grant funding for qualitative studies, secure a job at a research-intensive university upon graduation, and others. And unfortunately, some of these warnings are true for the most part. For example, a recent review suggested that between 5 and 10% of publications in leading criminology and criminal justice journals utilized qualitative methods (Copes, Tewksbury, & Sandberg, 2016), though the percentage of qualitative pieces in many journals seems to be changing. Even book writers may advise students and junior colleagues to give careful consid-eration to *not* writing a book, since the reward structure for publishing books as compared to articles is often very different, or they may be weighted quite low for tenure purposes (e.g., a book equaling only one article) or not count at all.

Nonetheless, there are distinct and immense advantages to collecting your own qualitative data as a doctoral student or early-career scholar: when you do your own data collection, you "own" those data. They are of your own design that can serve your specific goals and interests. When you publish from your project, you don't ever have to make a case to move up in authorship order, no one gets to prevent you from publishing what you want, and your findings are yours to build your career on and do with what you will. When publishing from large, publicly downloadable data sets, it may be hard to find an original question that remains unanswered from those data. But when you design a project based around an unanswered question, and col-lect your own original and primary data, that issue clearly does not exist. Additionally, conducting a project that is consistent with a deeply held curiosity or passion can be very sustainable in the long term and keep you excited about the work you're doing.

It may seem scary and daunting, especially if you're still a graduate student or considering becoming one. Doctoral students are often able to carve out more unstructured time than scholars on the tenure track or in later career stages, so these projects are certainly feasible then. There's also built-in help from mentors and dissertation committees! There is truly no classroom experience that can approximate collecting your own primary data, especially qualitative data. Making mistakes is inevitable but can be invaluable learning experiences after you dust yourself off and recover. Remaining flexible and trusting the inductive process, while being persistent and willing to problem-solve in the field, will lead to proj-ect success in both anticipated and unanticipated ways. Those unanticipated find-

ings can sometimes be the most compelling things you learn in the field, as was true for me.

I have been accused of being a Pollyanna (sometimes by close friends and colleagues), especially related to job placement and overall success of people doing qualitative queer, feminist, and/or critical work. It may take a couple of tries, but a good fit can happen. It is true that some institutions give increased weight to certain kinds of scholarship, in terms of the product or the process. But the core concerns for producing quality scholarship are often the same. Our discipline and the scholars within it value compelling and timely projects where the data collection and analysis are rigorous and thorough. Just because someone hasn't studied a topic doesn't necessarily mean it's important, but even when it is important, sometimes people need to be convinced if it's outside of their typical purview. Although we may be resistant to seeing our work as a product that needs to be "sold" to consumers, it is true that we as scholars have to figure out how to market ourselves and our work, especially if we're going down new paths. You may be critiqued for studying a "niche" or "narrow" topic, so one strategy to get in front of this is to learn how to describe an allegedly "niche" topic in ways that show it is of interest to a broad criminological audience or to interdisciplinary audiences. Again, some appeals are less convincing (e.g., "No one's ever studied this before"), while other appeals are much more descriptive, convincing, and don't sell your project short. For example, projects that challenge or refine existing criminological theoretical traditions, or have direct policy implications, or investigate pressing social problems (all of which queer-themed projects typically do, by the way) are often quite compelling. I have faced plenty of opposition to my course of study, but have also found many unexpected allies, and have been able to achieve various normative markers of success in terms of recognition and publications. Some indicators that my expertise in queer-related concerns and scholarship is taken seriously include being asked to participate in service to the discipline, such as serving on relevant committees, and to mainstream journals, such as reviewing papers with these themes or serving on editorial boards.

For me, getting in by being out doesn't just pertain to my various studies with LGBTQ people as a queer person. It has also entailed getting in to the discipline by being "out" and proud of my queer and qualitative accomplishments as well. I'm effectively professionally gay and professionally qual, and that works for me. It's exactly who I am, and it represents two of the main ways I want to contribute.

References

Bogdan, S., & Taylor, R. (1998). Introduction: Go to the people. In S. Bogdan & R. Taylor (Eds.), *Introduction to qualitative research methods* (pp. 3–23). New York: Wiley.

Copes, H., Tewksbury, R., & Sandberg, S. (2016). Publishing qualitative research in criminology and criminal justice journals. *Journal of Criminal Justice Education, 27*, 121–139.

Panfil, V. R. (2014a). Better left unsaid? The role of agency in queer criminological research. *Critical Criminology, 22*, 99–111.

Panfil, V. R. (2014b). Gay gang- and crime-involved men's experiences with homophobic bullying and harassment in schools. *Journal of Crime and Justice, 37*, 79–103.

Panfil, V. R. (2014c). "I will fight you like I'm straight": Gay gang- and crime-involved men's participation in violence. In D. Peterson & V. R. Panfil (Eds.), *Handbook of LGBT communities, crime, and justice* (pp. 121–145). New York: Springer.

Panfil, V. R. (2015). Queer anomalies? Overcoming assumptions in criminological research with gay men. In F. Adler, W. Laufer (Series Eds.), J. Miller, & W. R. Palacios (Vol. Eds.), *Qualitative research in criminology: Vol. 20. Advances in criminological theory* (pp. 169–189). New Brunswick, NJ: Transaction Publishers.

Panfil, V. R. (2017). *The gang's all queer: The lives of gay gang members*. New York: NYU Press.

A Bronx Tale: Lessons on Community and Police from 10 Years of Systematic Social Observations

Andres F. Rengifo, Cherrell Green, Lee Ann Slocum, and Aaron Ho

Introduction

Our project originated as graduate-school work that one of us (Rengifo) developed in collaboration with his advisors at John Jay College of Criminal Justice. In 2003, Professors Todd Clear and Elin Waring received a small grant to examine community life in high-incarceration neighborhoods, and the idea of conducting some form of neighborhood-based fieldwork quickly gained traction as part of the exploratory component of the study. Back then, we were all aware that Systematic Social Observation (SSO) had been used in the Project on Human Development in Chicago Neighborhoods (Sampson & Raundebusch, 1999), and we knew about similar efforts elsewhere (Taylor, Gottfredson, and Brower, 1984). We wanted to broaden these perspectives to capture not only visible evidence of disorder, or incivilities, but also to observe their apparent flip side—community organization, supervision, and the regulation of public areas. We also decided that fieldwork would be more fruitful if focused on a single area of the city with high levels of prison admissions/releases *and* varying patterns of land use and neighborhood context (new high-rises, old brownstones, mixed commercial/residential, etc.). After some discussion, we picked Community District 1 of the Bronx—a section at the southernmost tip of the borough encompassing about 75,000 people in a three-mile radius spread across

A. F. Rengifo (✉)
School of Criminal Justice, Rutgers University, Newark, NJ, USA
e-mail: arengifo@rutgers.edu

C. Green · L. A. Slocum
Department of Criminology and Criminal Justice, University of Missouri-St Louis,
St. Louis, MO, USA
e-mail: cg9qd@umsl.edu; slocuml@umsl.edu

A. Ho
Department of Criminal Justice, Jersey City University, Jersey City, NJ, USA
e-mail: aho1@njcu.edu

several neighborhoods—Melrose, Port Morris, and Mott Haven among others. With this in mind, we were ready to write our own Bronx tale!

To maximize the potential for the observation of street activity, data collection was conducted in the summer/fall of 2005 by seven fieldworkers that worked in rotating pairs during daytime hours. Some of these observers were local residents and others were students/researchers. Overall, 90% of the area's "street blocks" (both faces of a street segment) were assessed ($n = 606$ blocks across 22 census tracts). The results of the fieldwork were not studied systematically as these were seen as exploratory and linked to the broader project about incarceration and neighborhoods, but those of us on the ground learned a great deal about teamwork, observations, and ultimately, about the Bronx. Luckily, we would have a chance to return armed with new ideas and research questions.

Our current team was assembled at the University of Missouri—St. Louis (UMSL)—where the two co-PIs (Rengifo and Slocum) started working in 2007. With funding from UMSL, we went back to Community District 1 in the summer of 2010 with a revised strategy that featured an expanded SSO instrument and more explicit protocols for fieldwork and coding. We also doubled the study area to 1172 blocks (48 census tracts) to accommodate an even more diverse set of neighborhoods. We returned for a third time in the summer of 2015 and completed a new wave of data collection that replicated these protocols and sample.

In this paper, we discuss why we kept returning to our original target site every 5 years to conduct observations. More substantively, we reflect on the promise and peril of using SSO to examine policing practices. We focus on the 2010/2015 waves of data collection as these explicitly dealt with the rating of law enforcement presence/activity; however, we also draw on the broader 10-year project to identify the lessons we learned about the practice of observational research and its potential contribution to the study of police and policing and to the epistemological toolkit developed by urban ethnographers. Consistent with Reiss' (1973) pioneering use of SSO in criminology, we think of this approach as a strategy that bridges the hallmark of conventional ethnographic work—the direct observation of social phenomena—with other criteria more closely associated with quantitative research such as attention to threats to replication and validity through the use of independent observers and close-ended instruments. Below, we first describe our study, the specific tools that we employed for fieldwork, coding, and data analysis. We then summarize five "lessons" learned about methods and the nature of community organization and police, and we conclude by reflecting on how SSO contributes to ethnographic approaches, especially those examining police behavior.

Dispatches from the Field

Our SSO project was pretty low-tech: we did not use an SUV to drive us around like in Chicago or try to set up video recordings of streets, like in Detroit. Instead, we walked, and we walked a lot (over 1100 streets!) and used old-fashioned clipboards and paper forms. This strategy got us closer to the environment we sought to observe

and allowed us to unpack new dimensions of observational work, some of which are described below. The SSO instrument we designed was similar to those used in previous studies. Our checklist, which we called BIP (block inventory protocol), was rather short (two pages) and largely close-ended and featured a combination of items that enumerated physical features of the block, as well as specific (public) behaviors. However, it expanded these conventional areas of focus with additional prompts on formal and informal controls that noted, on the one hand, the presence, relative volume, and specific activities of the police and, on the other, unscripted interactions involving raters and local persons. It also captured a wide range of community organizations including some "usual suspects" in neighborhood-based research—churches, schools, police/fire stations—as well as other locations that signaled community life such as community gardens, childcare centers, and even some off-hour restaurants that doubled as improvised neighborhood committees. Lastly, the BIP also included open-ended fields so that observers could document unanticipated settings, odd block geographies and events, and the "feel" of blocks. Reviews of these notes during data collection sometimes pushed us to pursue additional areas of focus, such as the nature of gentrification in the area.

We learned a lot during our time in the Bronx, including some things that we didn't want to know. Below we describe five specific lessons related to the nature of fieldwork and the benefits and limitations of SSO for knowledge building.

Lesson #1: As Neighborhoods Change, so Must Data Collection Protocols

As mentioned earlier, the selection of the study site was initially driven by an interest in observing community life in areas where social control is shaped by high levels of prison admissions and releases. However, as we made progress with fieldwork, it became apparent that other features of the local community justified this choice and eventually prompted our UMSL-based team to consider the collection of follow-up data and the use of a more extensive set of markers of public life. The new checklist attempted to capture (a) changes in types of order/disorder, particularly in connection to broader issues of rezoning/gentrification; (b) variation in forms and strategies of social control, from police presence to CCTV cameras to community organizations; and (c) rater-based interactions with local persons as indication of informal regulation. All of these elements seemed to us particularly prominent in the Bronx and fit squarely with the evolution of the broken windows policing from Zimbardo's 1969 experiment to stop-question-and-frisk and with current discussions surrounding gentrification and not-in-my-backyard mobilizations.

Consistent with other place-based SSO instruments, the BIP included several modules intended to capture the overall characteristics of a given street block (e.g., general attributes such as traffic of persons/vehicles and land use), as well as counts of more specific markers of order and disorder, including abandoned lots, broken sections of sidewalks, etc. (see the BIP in Appendix). In the design of the instru-

ment, we redefined items employed by earlier studies in order to account for evolving forms of urban life (from counts of "stray dogs and cats" to "dog waste" and from an exclusive focus on "undrivable cars" and "damage on private property" to "illegally parked cars" and "damage on public property"). We also made sure to consider forms of disorder that are generated through different processes. For example, some types of disorder originate in poor city maintenance (e.g., broken sections of sidewalk/street) rather than the alleged neglect of local residents or others (e.g., damage on private property or tag graffiti). Capturing a wide range of manifestations of disorder also was critical because, when considered in isolation, some items may provide an incomplete picture of the local environment (e.g., "streets with litter" signal something different when one also observes "overflowing trash city cans"). The next section focused on observed behaviors, which we also expanded from prior SSO instruments to account for foul language and loud music and street sales (different from drug sales), in addition to more conventional indicators such as prostitution and panhandling. The fourth module of the BIP focused on the extent and nature of police activity (car passing/no siren, car parked, on foot, frisking, etc.) in the block, as well as other forms of surveillance and social control (CCTV cameras, signage). The final two sections of the instrument documented local organizations and specific interactions between raters and the local public. The module on unprompted resident-rater encounters was added to the 2010/2015 versions of the BIP because during fieldwork in 2005, it was not uncommon for local persons to ask us why we were on their block taking notes.

Lesson #2: Counting Things Is Tricky

The BIP instrument was set to be coded independently, but simultaneously, by pairs of fieldworkers. We used multiple raters to assess observer effects and interrater reliability but also to help ensure the safety of the raters. Extensive care was taken to put together a protocol that would ensure that different raters counted the same things in the same manner. For each item in the protocol we developed specific coding instructions included in a 30-page coding manual that included descriptions of the item, pictures, and guidance on common problems or challenges. In connection to policy activity, for example, we said:

> **Police activity (stop and frisk, foot or car patrols)**: Marked and unmarked cars/officers should be counted. Includes moving or parked vehicles, foot patrol (standing or walking), bike patrol, fixed posts, etc. Cops may be eating lunch, arresting someone, etc. Count NYPD traffic police here. Do not include private security, other non NYPD law enforcement (Sheriff, Parole), or school crossing guards. In addition to marking the presence of law enforcement, also indicate the number of police officers and/or police cars present in the space marked "#". Also code the type of activity you observe the police officers engaging in using the codes provided on the BIP. P=car passing by no siren/lights; S=car passing by with siren/lights; E=empty or parked police car; F=on foot (standing or walking); A=frisking, arresting; C=talking with resident/suspect; D=directing traffic; V=vehicle stop; T=ticketing car/person; O=Other (describe)

Raters received approximately a week of training before setting off into the field. Training began remotely using Google "Street view" images of the South Bronx in order to get observers accustomed to the protocol. More intensive on-site training took place over the span of a week and primarily consisted of coding a block and then comparing ratings and discussing inconsistencies. Once we aligned protocols and addressed all pending questions, raters began conducting observations in rotating pairs based on availability and prior assignments.

The coding of some items, including police activity, was straightforward. Most instances of police activity involved marked cars/uniformed personnel, although at times we were surprised by other sightings, including a livery cab that suddenly morphed into a police car (strobe lights on, full siren, speeding, and running red lights), or a resident washing his vehicle using water from a broken hydrant who, upon closer inspection, had a badge hanging from his neck and a radio inside his car. These were rare events though.

Despite our detailed manual and intensive training sessions, other street features were more difficult to code consistently. For some commonly used markers of "social disorder," it was harder to develop a shared view on what constituted evidence of the behavior. Should observations of women approaching cars, speaking to male drivers, constitute "evidence of prostitution"? What if they were in an area known for this activity, like Hunts Point? The issues surrounding the identification of "drug sales" were even murkier, especially in 2015 when many exchanges had moved indoors and product was delivered via bicycles. Ratings of these behaviors could have benefited from longer observation periods—ours were about ten to fifteen minutes per block. Other items on the BIP could have also used more precision in our definition. When to count someone as "homeless"? How to differentiate an abandoned building from a building with a slow/stalled renovation? What makes a lot "abandoned"?

We addressed some of these issues by encouraging fieldworkers to use the open-ended section of the BIP to account for more detailed descriptors of observed features of the local environment and associated public behaviors (e.g., artistic graffiti in the form of RIP murals or commissioned/institutional works) and to document forms and strategies of engagement of local community organizations beyond their physical location in the neighborhood (e.g., a community garden boycotting whole foods).

Lesson #3: Fieldwork Is Not for Everyone

We increased the number of field workers from 7 in 2005 to 12 in 2015 to accommodate the expanded area of study and to concentrate all observations in a shorter time span. This helped us to minimize the potential impact of external influences (weather, holidays, turnover, etc.) on observations. It also limited staff turnover and gave us some breathing room in terms of the pace of the work and pairwise assignments. Recruiting observers was challenging—Fieldwork is not for everyone. All potential raters were warned of the reality of the work; it is hot, noisy, odiferous, and

physically and mentally exhausting. Assessments required persons to move in uncomfortable spaces (next to highways, in industrial areas, on crowded sidewalks) and to count unsavory things (e.g., litter, dog waste, hypodermic needles, and used condoms). Work typically started at 9 AM and ended at 4 PM (add one to two hours each way for commuting, repeat 4–5 days a week for a month), and assigned routes involved between 20 and 25 street segments or blocks. Priority was given to hiring fieldworkers who lived in the Bronx because they offered local knowledge that we did not possess. For example, one observer who lived in the area helped shape our measure that captured indications of drug use (dime bags, cigar packages, burnt spoons, syringes, etc.).

As expected, we found great variation in the size, composition, and micro-context of the streets we observed. Some street blocks were in well-cared for locations, shady residential areas or adjacent to parks—raters who were assigned these blocks were viewed with envy. Others, however, were shadeless, crowded with people and businesses, or next to or under highways. There also were blocks that were filled with weeds, mounds of trash, and broken glass or excrement (mostly, but not always, from dogs). One set of routes zigzagged in front of a waste transfer station, whose smell in summer heat led one new fieldworker to gag. Shade, corner stores, and restrooms were valuable commodities, and fieldworkers often planned their routes around access to clean bathrooms and cool environments in which to rest or eat lunch. Fast-food restaurant chains were particularly popular because they had bathrooms *and* air conditioning.

Fieldwork conditions were less than ideal on some days. When afternoon storms came, raters sometimes had to take cover under underpasses if they had not planned their routes to get them near a business. On the first day of data collection in 2015, one observer experienced a shoe malfunction that required a hefty round of duct tape to fix. The fix held for the last few blocks of the day. Another day a fieldworker complained of exposure to UV rays, and one more quit after the first day of training—"too scary" they said, "I would rather do police ride-alongs."

Lesson #4: You Are Not Invisible; You Will Be Noticed

For many residents of the South Bronx, the street is part of their homes, especially in the summer, so even though we were examining public areas and behaviors, they may have felt we were studying their "living room." Unlike other types of fieldwork in which researchers seek to blend in with the community they are observing, we aimed to be "forgettable" ("invisible") in the local scene. In reality, however, more often than not, we were seen as outsiders, distinguished from locals by our appearance, behavior, and accessories (purposive walking, clipboards, hats, handbags). While our intent was to observe people's ordinary behavior, at times our presence appeared to cause people to alter their posture—some groups would disperse while others would suddenly get quiet. Only in a handful of occasions did our

presence evoke hostility. Notably, rocks were thrown at one pair of observers from a high-rise roof, and on another occasion, a fieldworker was followed by an older man who kept demanding to get a copy of the instrument. In one of the more serious of these incidents, a private security guard in a relatively white and affluent section of the Bronx asked our observers, a young black man and woman, to leave the area and threatened to call the police because he felt that they were "loitering" and "soliciting."

More commonly our presence was met with curiosity. On about 1 out of 10 blocks, community members proactively engaged raters. Most of these encounters were relatively innocuous. On one occasion a neighbor wanted to know who we were and if our work would have an impact on him: "Who do you work for? Will your project improve our neighborhood? Are you here to fix things or just to count?" Some people complained to us about poorly maintained streets and sidewalks. One person told us, for example, that her friend had tripped on a sidewalk and broken her hip, adding "she was sober then!"; another person tripped on the sidewalk across from a church and considered suing them but felt that it would bring him "bad mojo." Residents also came to us for help, typically directions. But, once we were stopped by a young teenage girl who asked us to call the police since she had just been robbed of her cell phone.

Residents, organizations, and businesses were surprisingly welcoming. Staff from several organizations invited us inside their building to learn about programs and services, while others offered up flyers, armbands, and, on one occasion, condoms. Firefighters provided access to the fire house bathroom and water. At least three different residents offered unsolicited histories of their local area, and, in the context of these or other similar encounters, people invited us into their houses to chat or to rest (we politely declined).

Outright harassment was uncommon, but, as expected, female raters were more frequently hassled by men. This ranged from the subtle, "God bless you ladies" remark to whistles or being asked to take a resident's phone number. In the most egregious case, a rater stated that she was followed halfway down the block by a man fondling himself. But, even in areas where our presence was not openly questioned by residents, it often was made clear that we were out of place. For example, it was common to look up into an apartment building and see the curtains cracked and a face peering out, the observer to become the observed. When this occurred, we marked it on the BIP ("person watching the street") and moved on.

Interactions with the police were very different. While police were often visible on the daily routes of observers, our behavior did not appear to capture their attention, and we had few personal interactions with them. Perhaps, like the residents, they thought we worked for the city and thus were part of the normal block environment. Being under the radar gave us a unique vantage point from which to observe them. Relative to systematic observations of police behavior recorded by researchers embedded in law enforcement agencies, we were able to observe officers' "natural" behavior because officers were unaware that their actions were being recorded.

Lesson #5: Be Open to New Ideas and Unexpected Findings

We observed over 1100 blocks of the Bronx in 2010 and a similar number in 2015. In each wave of data collection, we used roughly the same instrument, applied it to the same area, and did so during the same time of the year. We even had three field-workers who coded in 2010 return for more 5 years later! With each day of work, we learned a bit more about the local community, its regular actors, and patterns of street life. We first coded police behavior in 2010. At that time we expected to observe quite a bit of police activity, especially given the popularity of proactive policing strategies based on stops, tickets, and arrests. When we got back to the field in 2015, we assumed that these visible forms of police behavior would be relatively dormant given the heightened scrutiny of the NYPD due to allegations of bias and abuse. We were wrong both times.

In 2010, police behavior was noted on 154 different blocks (i.e., 13% of all blocks observed). Too little? Too much? We had a hard time coming up with a benchmark—Add this to lesson #2 above. But what was more surprising to us was the breakdown of observed law enforcement activities: The vast majority of police behaviors involved a "car passing by without sirens" (8%), followed by a police car "parked" (3%) and police "on foot" or on patrol "with the siren on" (1–3%). Specific police-citizen encounters in the form of ticketing, searching, or more unstructured contacts were even less frequent (<1%). In one such instance, an officer appeared to be lecturing two 10-year-olds on the block, and in another, a police officer was seen speaking in Spanish trying to comfort a young Latino boy who was crying.

Contrary to our expectations, our sightings of police increased from 2010 to 2015 when law enforcement was observed in 19% of all blocks, although they showed a similar breakdown by the type of activity. Further, when raters did observe the police, it was usually in connection to a single police officer (usually passing by in a patrol car instead of multiple cars or vans moving around in "packs" as in 2010). However, there were several cues that suggested that proactive law enforcement remained part of the local ethos above and beyond the seemingly passive yet ubiquitous presence of the NYPD. Raters described witnessing several arrests. They also noted the presence of large mobile command centers and towers retrofitted with cameras and spotlights, CCTV cameras, shot spotter devices, police officers interfering in disputes on the street, the apprehension of a suspect with a gun, reward signs, and signs for "Operation Clean Halls," "Operation Weed and Seed," and "Drug-Free Zones." In one instance, an officer was holding up traffic, and a resident expressed her frustrations with us, "they do whatever they want." Taken together these findings suggest heightened, yet more passive, policing and surveillance of community residents.

Our study also provided a number of other insights about police practices in the Bronx. First, we confirmed that this area has regular presence of the police. Per our more recent set of observations, on average about 1 in 9 blocks had a sighting of law enforcement, suggesting that for local residents, seeing the police in their immediate area is a daily occurrence.

Second, we note that police sightings increased from 2010 to 2015, in a period marked by heightened controversy about the NYPD. Given that the number of recorded stops during this time had already dropped—from about 685,000 in 2011 to 45,787 in 2014—and consistent with the police seeking a low profile, we expected police presence to drop. It had not. The actual recorded increase may be tied to a different model of law enforcement (less activity, more visibility), or perhaps changes in manpower or deployment patterns (less at night, more for anti-crime units or patrol). It is also possible that the gentrification of the South Bronx, albeit in its nascent stages, had brought with it the demand for a greater police visibility. An additional possibility is that the raters may have followed slightly different observation protocols, for example, the coding of patrol cars in surrounding streets vs. target streets.

Third, deployments in 2010 seemed more intense (more officers per sighting, perhaps more sirens). For example, during training for this wave, the research team observed a fleet of approximately ten patrol cars aggressively descend on a street altercation and then, just as suddenly, peel away. In addition, in 2010, when the police responded to our 911 call for the young girl with the stolen cell phone, they arrived in a passenger van containing 6–8 officers. In 2015 we saw more police cars but did not necessarily encounter other forms of enforcement activity.

Final Thoughts

We conducted three waves of SSO in the same area of the Bronx every 5 years since 2005. Although we did not come to form intense relationships with neighborhood residents like traditional ethnographies, by walking over a 1000 blocks in each of the waves, we did come to develop a specialized knowledge of the physical space and the more general social environment. This long-term, albeit sporadic, engagement with the community gave us a good vantage point from which to observe long-term changes in the South Bronx, something that is not possible with ethnographic research based on more intensive but shorter-term engagements. By 2015, we witnessed how several portions of our research site showed signs of an impending transformation. Colorful murals had appeared accompanied by tourists on buses taking photos; fewer abandoned lots, more people in the streets, more coffee shops, and a greater police presence. We even saw "hipsters."

Our experiences in the Bronx also taught us lessons about research methods and SSO. We learned that observations of the police can be conducted effectively by researchers embedded in the community. These data are less likely to be influenced by the presence of the observer in terms of the behavior of officers and residents, or selection into incidents. Although they were more apparent for observations of community members as opposed to the police, observer effects remained an issue. It is not clear, however, if our presence influenced police behavior any more than that of a community resident. This may be particularly true given the proliferation of technology designed to capture police behavior (e.g., body cameras, "cop watch" apps).

Unlike studies of the police in which the researcher is embedded within a police agency, SSO also is useful because it can be used to document non-contacts—the times when police pass by illicit behavior (e.g., gambling and drinking on the street) and choose to do nothing.

Reflecting on our data collection experiences has also taught us ways that this type of research might be adapted to provide a more complete and nuanced examination of police behavior in the community. For example, to explicitly examine police activity and contacts, additional sampling of streets or times of the day would be required even in a hyper-policed place like the South Bronx. In addition, contact-specific observation protocols could be developed in order to collect more detailed data about participants in police encounters, for example, their sequenced actions and exchanges.

While certainly not a replacement for traditional ethnographic methods, SSO, when sustained over long periods of time and by the same core group of researchers, has the potential to produce intimate knowledge of a place akin to that generated by qualitative research. To do so, however, requires that the data collection instrument be designed with this purpose in mind and that the importance of unstructured observations be communicated to field workers.

Ethnographies often rely on different qualitative methods—e.g., observations, interviews, diaries—to describe otherwise "invisible" problems and their social contexts and to document how actors adapt by recreating meanings or charting strategies for action. When considered part of an ethnographic project, the potential of any of these methods to uncover underlying issues is amplified not only due to the use of parallel approaches but also, and perhaps more critically, due to the researchers' long-term engagement with subjects and settings and by the expansive role that they negotiate as active participants in fieldwork and as reflective commentators of method and substance.

Appendix: BIP Instrument (2015)

Team		Street name	
Rater		Starting street	
Date (mm/dd)		Ending street	
Time started		Raining? Yes No	Approx. Temp 70s 80s 90s 100s

I. General block characteristics

1. Typical height		2. Traffic volume		3. Litter/glass		4. People	
Flat		None		None		None	
1 story		Very light		Very light		1–5	
2 stories		Light		Light		6–20	
3 stories		Moderate		Moderate		21–50	
4–6 stories		Heavy		Heavy		51–100	
7–10 stories		Very heavy		Very heavy		More than 100	
11–20 stories		Temporarily blocked					
More than 20 stories		Pedestrian only					

II. Block Environment

5. How many of the following?	0	1	2–4	5–6	>7	6. Indication of...	Yes	No
a. **Undrivable**/damaged **cars** on street						a. Prost.		
b. **Damage** on **street** prop. /broken lights						b. DD		
c. **Damage** on **private** property						c. D. use		
d1. Number of distinct **pieces** of **graffiti**						d. Public alcohol drinking		
d2. Number of **entities** with **graffiti**						e. Panhandling		
e. **Boarded**/abandoned **buildings**						f. Homeless people		
f. **Abandoned lots** (with garbage, etc.)						g. Children in street*		
g. **Broken** sections of **sidewalk** (potholes, etc.)						h. **Teens** hanging out*		
h. **Broken** sections of **street** (potholes, etc.)						i. **Adults** hanging out		
i. Evidence of **d.** consumption						j. People **eating** outside		
j. Evidence of **alc.** consumption						k. People **playing** outside		
k. Lots/buildings under **construction**/renovation						l. Cursing/vulgar language		
l. **Dog waste**						m. Physical/verbal fight		
m. Overflowing **garbage cans**						n. Loud music		
n. **Condoms**						o. Str. sales (not food)[a]		
o. Pairs of **shoes** on wire/street sign						p. Person(s) watching street		
p. **Illegal parking**						q. Cat calls/harassment		
q. Pay day **loan**/check **cashing**						r. **Car repair** in street		
r. **Pawnshops**						s. People **cleaning** street/walk		
s. Money **wire** services						t. People **gardening**		
t. **Signs** in **foreign** language						u. Non-US flags		

[a] In comments include details on type of street sales (esp. cigarettes) + police activity   (use * if supervised act.)

7. Indication of...			Yes	No
a. **Police activity** <u>P</u>=car passing by no siren; <u>S</u>=car passing by with siren; <u>E</u>=empty or parked police car; <u>F</u>=on foot (standing or walking); <u>A</u>=frisking, arresting; <u>C</u>=talking with resident/suspect; <u>D</u>=directing traffic; <u>V</u>=vehicle stop; <u>T</u>=ticketing car/person; <u>O</u>=Other (describe)[a]	#	Activity code(s)		
b. **Surveillance cameras** pointed at street (or signs indicating presence of cameras)				
c. **Signs/flyers** advertising community meetings, parties, or block parties				
d. City street signs: **Neighborhood Watch," "Weed and Seed," or "Drug-Free School Zone"**				

e. City sign on building:"**Operation Clean Halls"**			
f. City street sign:"**Slow Children at Play**"			
g. Signs on residences announcing security alarms, "Beware of dog"			
h. **Interaction** b/t resident and city/fed/local worker (firefighters, crossing guard, etc.)			
i. **Signs/fliers** posted by *residents* (not businesses) that regulate public space (curb your dog, etc.)			

III. Social Interactions with Block Raters

	Yes	No
8a. "Hey, Honey" or "Hey, Man" hassles		
8b. Questioning of rater activity		
8c. Other ( )		

IV. Inventory of Organizations

	Name and address	S	S	S
1				
2				
3				
4				
5				

1. Church, temple, or other place of worship	14. Employment center
2. Church-connected organization (such as a charity)	15. Shelter (homeless, women, family)—specify below
3. Stand-alone leisure/sports facility (gym, martial arts)	16. Food program, pantry, soup kitchen
4. Child care/day care center	17. After-school program/education for youth
5. Community center	18. Adult education, including universities and colleges
6. Youth organization (scout leaders, explorers, etc.)	19. Economic development
7. Political groups or organizations	20. Community gardens
8. Government agency (includes probation, parole, etc.)	21. Halfway houses, other post-release residential centers
9. Senior citizens association	22. School
10. Labor union	23. Bar/restaurant, etc used as community forum, specify below
11. Clinic/health-care organization, family planning, counseling	24. Other (describe):
12. Mental health	25. Other (describe):
13. Substance abuse and rehabilitation (includes clinics)	26. Other (describe):

V. Comments: special buildings; unusual block features (i.e., block located under bridge, divided by entrance ramp); artistic graffiti and public art; unusual activity; further description of police activity and street sales; unusual items found on block (e.g., weapons); "other" uses of land, etc.

Additional info on street sales:

Additional info on police activity:

References

Reiss, A. (1973). *The police and the public*. New Heaven, CT: Yale University Press.

Sampson, R. J., & Raundebusch, S. W. (1999). Systematic social observation of public places: A new look at disorder in urban neighborhoods. *American Journal of Sociology, 105*, 603–6521.

Taylor, R. B., Gottfredson, S. D., & Brower, S. (1984). Block crime and fear: Defensible space, local social ties, and territorial functioning. *Journal of Research in Crime and Delinquency, 21*, 303–331.

The Scream: Insider Access and Outsider Legitimacy in Danish Prisons

Jennifer Sumner, Lori Sexton, and Keramet Reiter

The Scream

What we will refer to as "the scream" happened during our first week of fieldwork in 2015 in one of Denmark's closed[1] prisons. As is common in ethnographic work, critical points that help us understand the field often occur in the context of deeply cringe-inducing interactions—those you are embarrassed to report until you realize what they reveal and until there is enough distance between you and the incident to soften the blow of embarrassment (see, Bourgois, 2003; Goodman, 2011; Jenness, 2010, 2011; Venkatesh, 2008). The incident we describe here and the subsequent discussion are part of an in-depth, qualitative study on punishment in Denmark for which we each conducted 10 weeks of fieldwork in (and around) the Danish prison system over the course of 3 years (2015–2017). Data collection consisted of ethnographic fieldwork in two open and two closed prisons, including observations of prisoners and staff working, eating, participating in activities, and otherwise going about their daily routines and some degree of *deep hanging out* (Geertz, 1998); frequent informal conversations with prisoners and staff throughout each day; and

---

[1] In Denmark, prisons are designated as open or closed. Open prisons are minimum-security facilities without a perimeter wall and in which prisoners are generally able to move freely and unaccompanied by staff throughout the day. Closed prisons are maximum-security facilities with a perimeter wall and greater restrictions on movement.

J. Sumner (✉)
California State University, Dominguez Hills, Carson, CA, USA
e-mail: jsumner@csudh.edu

L. Sexton
University of Missouri, Kansas City, Kansas City, MO, USA
e-mail: sextonl@umkc.edu

K. Reiter
University of California, Irvine, Irvine, CA, USA
e-mail: reiterk@uci.edu

in-depth, semi-structured interviews with them about their experiences with incarceration and punishment in Danish prisons. Beyond these primary sites, we also conducted 1- to 2-day-long tours of six other prisons and additional in-depth interviews with prison experts working in the headquarters of the national prison system or who had related expertise. This fieldwork resulted in interviews with 142 participants (76 prisoners, 49 staff, and 17 experts). All interviews were conducted face-to-face and in English (a language staff are required to speak and the go-to language for Danes communicating with non-Danes, even other Scandinavians). All prisoner interviews took place in confidential settings, without the presence of staff. In most cases, interviews were recorded and transcribed; otherwise, detailed notes were taken. Interview notes and fieldnotes were taken by at least two of the three research team members and layered in final form.

When the scream occurred, we had just embarked on the first of our three rounds of fieldwork in Denmark and had already spent four long days at our first prison site conducting ethnographic fieldwork. At this prison, we were assigned one key point person, a prison guard who was asked to act as our guide because of his particularly sharp English language skills. He was very professional and enthusiastic about facilitating what we perceived (and communicated to him) to be relatively unique access to the prison. He seemed to take it as a personal challenge to show us things we didn't expect to see, such as prisoners and guards sharing meals together, the nightly lock-in routine in the special security unit, and the basement storage area where prison riot gear was kept (which Keramet willingly tried on). In fact, as we neared the end of the week, our guide repeatedly asked us, "What else do you want to see?" as though hoping for a chance to one-up his performance on previous challenges. Treading lightly at first, accustomed to repeated denial of access to various aspects of US prisons (Goodman, 2011; Reiter, 2014), we became increasingly comfortable pressing further about what we wanted to see and do in the prison as part of our fieldwork. This dynamic strengthened the rapport between our guide and us: a comfortable familiarity that extended to other staff as we interacted with them.

On our fourth night at the prison, as part of his increasing determination to wow us with our unfettered access to the prison, our guide invited us to stay at the facility until prisoners were locked in for the night—a process in which guards bid prisoners goodnight and individually lock them in their "rooms" (the term prisoners and guards used for what we would call "cells") until morning. Before this routine, we shared a takeout dinner with guards in the high-security unit, feeling pleased we were able to stay for extended hours that day—an uncommon occurrence in US prisons, where we were often required to leave before afternoon shift change or evening count procedures. After dinner and before observing the nighttime routine, we accompanied three guards (one male and two female) outside, to the small yard behind the unit, so they could smoke. It was cold and we were shivering; perhaps it was this distraction that kept us from noticing that the male guard had disappeared, even though he had been with us just moments before. We walked farther toward the back of the unit, closer to the perimeter wall, listening to one of the remaining guards tell us of a time some years ago when staff discovered a collection of unauthorized cell phones right near where we stood. She explained that prisoners'

contacts outside would stand on the other side of the perimeter wall and throw cell phones into the yard. This happened so frequently that one guard was even hit by a flying cell phone while walking in this area. She went on to explain that, after additional security measures were implemented (including a layer of netting hung over the area), the problem abated.

Once the guard we were talking with finished her cigarette, we walked back toward the door to the unit, passing along one side of a large concrete wall dividing the lawn near the door to the staff kitchen. As we approached the end of the wall, the guard who had been missing from the group jumped out from behind the wall, yelling loudly in order to startle us. Jenn tends to startle very easily, often screaming or jumping disproportionately in the moment. In this moment, Jenn responded in character by screaming *very* loudly. (Perhaps it was not the existential scream depicted in that most famous Scandinavian painting by Edvard Munch; but it was piercing nonetheless.) The group erupted in laughter. Keramet and Lori, who had been trailing behind, were also startled and jumped a bit. The younger female guard told us she even grabbed for her pepper spray in response to the surprise. But no reaction was as extreme or as loud as Jenn's. We found out later that the male guard had been waiting there in the cold for a good fifteen minutes and that the female guard recounting the cell phone problems was in on the joke the whole time.

For Jenn, the laughter and heart pounding was followed by embarrassment, which arose from a sense that her reaction disrupted the field considerably. She was concerned that her behavior disrespected the environment, the space, and most importantly the people who live and work there. Our guide assured her that she shouldn't be embarrassed, because neither she nor the team had bothered the prisoners. As much as we appreciated our guide's insights, we did not think a prison guard was necessarily in the best position to determine whether or not we actually had disturbed or disrespected the prisoners, whose cell windows were adjacent to where the incident occurred. Jenn, especially, remained uncomfortable: both her own scream and the behavior of the guards in enacting the practical joke represented extreme departures from anything she (or Keramet or Lori) had ever experienced conducting research in US prisons. The scream in particular would have been considered highly inappropriate in a US prison; it may have even been a cause for termination of access, signaling to staff that we didn't know how to conduct ourselves in the tightly controlled facility in which we were guests. In the Danish prison, however, the scenario was instead a key indicator that we were being treated as insiders—perceived to be "at home" enough in a Danish prison to be the butt of a good-natured but disruptive joke played by the guards.

Outsider Status

Despite our inclusion in both formal routines and informal camaraderie by prison staff, our outsider status was irrefutable in many ways. Most obviously and directly, we did not speak Danish, and our lack of Danish language skills was a prominent

feature of our interactions. People regularly searched for words in English, asking others to translate when those words could not be found, and they often spoke Danish with one another when not speaking directly to us. We repeatedly assured our hosts that their English language skills were impressive (and they were) while apologizing that we couldn't offer any Danish in return. Meanwhile, people frequently attempted, brows furrowed, to decipher which Danish words we were hopelessly trying to produce as we apologized profusely for our pronunciation, and they continually reassured us that the Danish language is notoriously difficult for outsiders, even other Scandinavians. These exchanges, though, proved to be a regular source of banter and helpful rapport building. Prisoners and guards alike schooled us in pronouncing the names of prisons we were visiting and the ever-important words for different Danish foods—including *flodebøller*, a marshmallow-like treat enrobed in chocolate which featured prominently in a prison "Olympic" competition in which we participated. And, unlike in other experiences we have had traveling internationally, we quickly learned that beginning interactions by asking Danes if they spoke English was more insulting than simply assuming that they did and proceeding accordingly.

Our status as outsiders was also evident in the extent to which people worked to show us their system, rather than letting it speak for itself. This was not done through any sort of elaborate performance; such a showy effort would offend cultural values prioritizing humility and equality. Rather, prison personnel and prisoners alike served as gracious hosts, taking care to ensure that we, their guests, knew the lay of the land—both with regard to system functioning and Danish culture more generally. As with our language lessons, food was often central. Our first day at each prison invariably began with a meeting with prison administrators around a conference table; coffee, tea, rolls, and pastries were almost always served. The first lunches at more than one prison included traditional Danish *smørrebrød* (open faced sandwiches), and one of our assigned guides at an open prison made an extra stop after picking us up at our lodging to purchase a traditional warm liver paté, which she served us upon our arrival at the prison. One prisoner even prepared crepes and coffee to share with us at the start of an interview in solitary confinement at a closed prison (see Reiter, Sexton, & Sumner, 2018). Further, as we discussed briefly above, we were regularly invited to take part in, rather than just observe, prison life. This included participation in a hip-hop dance class (with both prisoners and guards participating together), sharing meals with prisoners in their housing units (that the prisoners had cooked), and participating in a prison Olympics day at one facility.

Although prisoners and guards welcomed us into the daily routines of their lives, we were aware that, as outsiders, we were experiencing "front stage" (Goffman, 1961; Piché & Walby, 2010) versions of their routine lives that were being performed explicitly for us, the foreign visitors. As we have argued elsewhere (Reiter, Sexton, & Sumner, 2017), from a constructivist perspective, these very performances are instructive for making sense of the culture and values of a system. What people chose to show us—from the words they taught us, to the food they shared, to the practical jokes they played—reveal the aspects of their culture that they wanted

to share and wanted us to witness, and the things they were proud of or that they perceived as representative.

Our observation that our identities—in this case as cultural outsiders—influenced our field research, both in terms of what we were shown and how we interacted in the field, aligns with a robust body of scholarship on reflexivity in closed institutions generally (e.g., Monahan & Fisher, 2015; also, Doykos, Brinkley-Rubinstein, Craven, McCormack, & Geller, 2014) and correctional facilities specifically (e.g., Damsa & Ugelvik, 2017; Goodman, 2011; Jenness, 2010, 2011; McCorkel & Myers, 2003; Ugelvik, 2014, 2018). In our collaborative research in Denmark, however, we experienced a dual status as cultural outsiders, who were at the same time warmly and jocularly welcomed by prison staff as institutional insiders, based at least in part on our established knowledge of incarceration through our research experience in prisons across the United States. The specific nature of the influence of this outsider-insider duality on our field research surprised us. On the one hand, our dual roles opened up access—as prisoners and guards alike sought to give us an in-depth experience of Danish incarceration. On the other hand, these roles also served to legitimize the practices, policies, and values of Danish incarceration in complicated (and potentially troubling) ways.

Insider Access

From our first request for research access, we were immediately and continuously welcomed as researchers in Danish prisons. Officials at headquarters and at individual prison sites invited us to return to institutions on future trips; we were regularly asked what else we wanted to see and do; and we were rarely denied requests to see additional sites or talk to additional people. In fact, we were consistently provided greater access than expected, and we even had the occasion to decline further access (when appropriate given the demands of multisite ethnographic research and careful assessment of data saturation, of course).

At one open prison, we were handed our own set of keys so that we would have unfettered access to the warden's meeting area (our research headquarters for the week) and so that we could come and go as we pleased, moving throughout the prison unaccompanied (just as prisoners do in these facilities). We were surprised to discover that these same keys even allowed us entry to additionally secured spaces on the prison grounds, including an enclosed section used to segregate sex offenders from the larger prison population. While other prison scholars have written about how the possession of keys can be a fraught display of power, indicating a clear alignment with staff and away from prisoners, the fact that prisoners in these facilities also had keys to their own rooms (or cells) made us more comfortable with "holding the keys" (Scott, 2015, p. 54).

At that same site where we were given keys, we were permitted to take photos of the prison (but not of people), including inside buildings, without staff oversight. At another open prison, we were invited to participate (and did) on the staff team dur-

ing an all-day annual event dubbed the "Prison Olympics" in which all staff, from guards to the warden, competed against teams made up of prisoners in different housing units. Most recently, we were provided access to Denmark's newest and highest security prison, Storstrøm, which opened just days before we arrived. At that point, the facility staff were still ironing out many logistical kinks, including learning to use the new scanners and X-ray machines for screening visitors and determining who would make runs to the local grocery store in advance of the prison store becoming operational.

Lori and Keramet participating in the Prison Olympics

We soon began to wonder if what we perceived to be extensive access—certainly a departure from the norm we had experienced in the United States—was simply a function of Danish prison system principles of transparency or whether this access had something to do with our status as US researchers. Not until our second visit did we begin to appreciate how unique our access was, even among researchers studying Danish prisons. This realization became most clear once we had scheduled a

tour of the live construction site for the new Storstrøm prison. Through the course of this planning, we learned that we were among a select few researchers permitted to visit the site, even though other Danish prison researchers we met had also requested site visits and had been either directly denied or indirectly ignored. This realization prompted us to more deeply examine the factors that *enhanced* our access, rather than restricted it—the latter having been a more common analytic focus up to that point in light of our US-based prison research experiences.

We already regularly asked ourselves many questions about how our outsider status may have affected the research: What data were we missing due to the language barrier? How were our non-native identities and subsequent limitations to understanding the country's criminal justice system affecting our interviews? And, as prison researchers more generally, what were we overlooking as institutional outsiders, being neither staff nor prisoner? But when we learned how exclusive our access to the new, high-security prison site was, we had reason to shift our attention to how we may have paradoxically been accepted as *insiders* within a culture that was not ours—a culture often characterized as highly exclusive (Booth, 2014).

Denmark, and Scandinavia more broadly, is widely described as a homogenous nation with strong social cohesion (Booth, 2014; Pratt, 2008). These characteristics often crop up as explanatory factors in making sense of the nation's social welfare system, which provides a robust safety net to its citizens. Over the past few decades, however, Denmark's population has become more diverse as waves of immigrants and refugees from Eastern Europe and the Middle East seek a better life there. In response to these changes, Denmark has introduced increasingly restrictive, and in many cases punitive, immigration policies focused on strengthening the distinction between those who are Danish and those who are not (see Delman, 2016; W, 2017). This is evident in increased security along the country's southern border; changes in access to social benefits for refugees and immigrants; the use of a deteriorated closed prison to detain refugees; the overrepresentation of nonethnically Danish people among prisoners, particularly in the country's higher security prisons; and increasing separation of these prisoners from those who are ethnically Danish (see, e.g., Haller & Kolind, 2017). During our most recent research visit in November of 2017, for instance, we learned that the Minister of Justice announced publicly that prisoners who were scheduled to be deported should not be moved to the new prison at Storstrøm as originally planned by the Prison Service. According to one prison official, the Minister said: "This is too nice and too new. They are not Danish. Why spend our luxury prison on them?" Likewise, prisoners we interviewed recounted stories of differential treatment between Danish and nonethnically Danish prisoners.

The resistance to "outsiders" (ethnic and otherwise) is often explained within the context of a culture of conformity, which some have argued is the brick and mortar of a successful social welfare system (Barker, 2018). As university professors and researchers from the United States, however, we realized we were not perceived as outsiders. While Monahan and Fisher (2015) note that "secretive" and "guarded" organizations tend to perceive researchers as a potential threat to organizational operations, in Denmark, we instead observed that researchers are an integral part of the organizational fabric of the prison system. Prison staff explained to us that our

research visits had to be carefully coordinated with the many other requests for access (both from within and outside of Denmark) made of each facility. Thus, we may have also been perceived as insiders—just one more coterie of prison researchers traversing Danish prisons, solidifying a culturally accepted nexus between research and penal practice. While our identities remained a key point of difference, the nature of this difference is crucial. As educated, professional, white women from the United States, in some ways we "fit" into Danish culture better than those who fall into the categories of outsiders most strongly resisted in Danish culture and policy. We may, indeed, have been perceived, in contrast to ethnic outsiders, both as nonthreatening and as potential insiders.[2]

Legitimizing Through Comparisons

Throughout our fieldwork, we increasingly understood that our insider access was conditioned both by the transparency of Danish prisons and by our status as a particular kind of acceptable or familiar outsider. This realization, in turn, had important implications for how we understood our interactions with Danish prisoners and guards, as well as for the role our own presence (as a specific category of outsider) and ongoing access played in legitimizing the Danish system. This became clearest in a pattern of interactions we experienced in the interviews we conducted with prisoners and guards alike. Prisoners and guards frequently engaged us in direct comparisons between the United States and Denmark, especially when we asked if they had questions for us at the end of our interviews. Through these conversations, we gradually learned that those who work and live in the Danish prison system perceived us, the US prison scholars, as representatives of a failing prison system. Our interview participants and hosts, on the other hand, perceived themselves as representatives of a *better*, more functional system. This relationship to one another set the foundation for all of our interactions with prisoners, staff, and even Danish punishment scholars, from start to finish.

Over the course of our research, we were regularly asked "why Denmark?" We responded with several reasons, especially referencing the increasing interest in Scandinavian prisons in the United States, including the frequent depiction of Scandinavian prisons as "ideal" in US news. At the end of our interviews or visits, respondents commonly asked what we hoped to do with what we learned from our work. In response, we frequently mentioned that we hoped we might learn from how punishment was done in Denmark, in order to improve the US system. Further, respondents repeatedly asked specific questions about the US system, such as: Do you *really* keep people in prison for life? Are there *really* three-strikes-and-you're-out laws? Do the cells *really* look like what we see in movies? And what about the

---

[2] Of course, other factors such as gender may have also played a role in this perception. Our guide at one prison site told us that we were unexpected to him: three relatively young women, as opposed to three older men, whom he told us he pictured as "typical" scientists.

death penalty? What could you get that for? Participants revealed a perception of the US social, economic, and prison systems gleaned from international news and entertainment sources and frequently asked: "Is that *really* how it is?" Most often, their perceptions were fairly accurate. Once we confirmed the accuracy of these perceptions, participants frequently asked us for our own assessments: "Are those policies effective? Do you agree with those policies?"

We were unaccustomed to this pressure to make normative judgments in the midst of data collection, even as we understand the "inevitable" "normative aspects" of comparative research more generally, which require focused reflexive analysis (Ugelvik, 2018, p. 8). At first, we had contentious discussions about how to handle these interactions. One of us was more reserved, sharing less of her perspective. Two of us were more forthcoming, happy to either agree that the Danish system was better or to engage in a debate over the relative strengths and weaknesses of the two systems. Increasingly, all three of us found ourselves engaging in these cross-national comparisons and agreeing with our participants' implicit and explicit critiques of the criminal justice system in the United States. As we grappled with how to engage our participants' requests for our assessments of the relative value of our respective justice systems, we increasingly understood that this contrast between our systems framed the research at many stages—particularly our access.

In one case, the perceived inferiority of the US criminal justice system threatened to thwart our access. On our first day in the field at a closed prison, we attempted to begin interviewing in the unit housing gang-affiliated prisoners. However, those living in the gang section initially rejected our invitation to participate in an interview. Within a few hours, prisoners and staff had explained to us that the prisoners in the gang section feared that our purpose was to find ways to implement US policies in Danish prisons. Learning this, we assured the prisoners that this was not the purpose of our research; instead, we explained that we hoped the United States might actually have the opportunity to learn from the Danish system. Once this was explained, prisoners expressed interest in participating in interviews. In this way, our explanation opened the door to later conversations assessing the relative value of our respective systems.

In a broader sense, though, the perceived inferiority of the US criminal justice system actually facilitated access. Because our prison system was considered by all of us (whether or not we were initially comfortable saying so) to be "worse" than the Danish system in a very general sense, our presence did not pose a threat to existing policies and practices in Denmark. Further, it may have even served to shore up the legitimacy of the Danish system against an unacceptable alternative as we implicitly sought empirical refuge from the failures of our own criminal justice system. We grappled with this throughout our time in the field, and we continue to grapple with it with every page we write. Did our own presence (comparative by default and, therefore, inevitably tied to normative judgments (Ugelvik, 2018)), and our continued positive analysis of the Danish prison system relative to our own, reaffirm and legitimate that system? We think it did and does. By welcoming representatives of an inferior system, Danish prisoners and staff alike both maintain and perform their relative superiority with minimal effort and negligible boasting, in

keeping with traditionally Danish principles of "parsimony, modesty, [and] a disapproval of individualism or elitism" (Booth, 2014, p. 81). As Goodman (2011) has argued in the US context, academic researchers risk "inadvertently confer[ring] on a manifestly unjust system some degree of legitimation inherent in the visible cooperation of outsiders" (p. 601). Although the injustices of the Danish system are not always immediately obvious or manifest in the way Goodman saw them in the US system, we remain sensitive to the legitimacy we conferred through our highly visible status as US researchers in Danish prisons.

Insider Access and Outsider Legitimacy

Negotiations of identity are central to both access and analysis in qualitative research. We present here an analysis of the surprisingly extensive access we were granted in Danish prisons, from being the subject of practical jokes to visiting new facilities to which even some Danish researchers were denied access. We argue that our insider access relates, counterintuitively, to our status as a particular category of nonthreatening cultural outsiders. While our status as prison researchers from the United States certainly enhanced our access to a system that many (including both journalists and researchers) have assessed as humane and laudable (e.g., Larson, 2013), this same status also potentially obscures aspects of the Danish system that are less worthy of imitation. Indeed, policy experts and prison administrators from the United States who tour Scandinavian prisons regularly return with glowing reports about somewhat surface-level differences (Reiter et al., 2017). We get it. The sharp knives in the prisoners' kitchens were shocking to us at first, too.

Yet, this comparative orientation—and particularly the limited nature of the relative comparison where one system is cast as ideal and the other as shameful—could create obstacles to identifying central aspects of the Danish system that are most deserving of critique. These include the system's extensive use of solitary confinement; restrictive pretrial detention practices; and increasing hardening across the system overall, especially in the treatment of immigrants, noncitizens, and nonethnic Danes. This hardening, in terms of use of solitary confinement and harsh treatment of immigrants, echoes some of the harsh practices characterizing corrections in the United States. Yet, even as the Danish system hardens, the Danes seem to take comfort in the idea that no matter how hard their system becomes, it is in no danger of resembling the US system. In the end, our status as representatives of, and experts in, the notoriously harsh and ineffective US system facilitated our access as especially nonthreatening outsiders, another group of American visitors likely to be impressed by the apparent openness and humanity of the Danish system. Moreover, despite the substantive injustices and systemic failures revealed by our data, our presence gave Danish officials an opportunity to point out that, however, harsh their prisons were, US prisons were harsher.

References

Barker, V. (2018). *Nordic nationalism and penal order: Walling the welfare state*. New York: Routledge.

Booth, M. (2014). *The almost nearly perfect people: Behind the myth of Scandinavian Utopia*. New York: Picador.

Bourgois, P. (2003). *In search of respect: Selling crack in El Barrio*. Cambridge: Cambridge University Press.

Damsa, D., & Ugelvik, T. (2017). A difference that makes a difference? Reflexivity and researcher effects in an all-foreign prison. *International Journal of Qualitative Research, 16*, 1–10.

Delman, E. (2016, January 27). How not to welcome refugees. *The Atlantic*. Retrieved from https://www.theatlantic.com/international/archive/2016/01/denmark-refugees-immigration-law/431520/

Doykos, B., Brinkley-Rubinstein, L., Craven, K., McCormack, M., & Geller, J. (2014). Leveraging identity, gaining access: Explorations of self in diverse field-based research settings. *Journal of Community Practice, 22*(1/2), 130–149.

Geertz, C. (1998, October 22). Deep hanging out. *The New York Review of Books*, *45*(16), 69. Retrieved from http://www.nybooks.com/articles/archives/1998/oct/22/deep-hanging-out/

Goffman, E. (1961). *Asylums: Essays on the social situation of mental patients and other inmates*. New York: Doubleday.

Goodman, P. (2011). From "observation dude" to "an observational study": Gaining access and conducting research inside a paramilitary organization. *Canadian Journal of Law and Society, 26*(3), 599–605.

Haller, M. B., & Kolind, T. (2017). Space and ethnic identification in a Danish prison. *Punishment & Society*. Advance online publication. Irvine, CA. https://doi.org/10.1177/1462474517722541

Jenness, V. (2010). From policy to prisoners to people: A "soft mixed methods" approach to studying transgender prisoners. *Journal of Contemporary Ethnography, 39*(5), 517–553.

Jenness, V. (2011). Getting to know 'the girls' in an 'alpha-male community': Notes on fieldwork on transgender inmates in California prisons. In N. Jones & S. Fenstermaker (Eds.), *Sociologists backstage: Answers to 10 questions about what they do* (pp. 139–161). New York: Routledge.

Larson, D. (2013, September 24). Why Scandinavian prisons are superior. *The Atlantic*. Retrieved from https://www.theatlantic.com/international/archive/2013/09/why-scandinavian-prisons-are-superior/279949/

McCorkel, J. A., & Myers, K. (2003). What difference does difference make? Position and privilege in the field. *Qualitative Sociology, 26*(2), 199–231.

Monahan, T., & Fisher, J. (2015). Strategies for obtaining access to secretive or guarded organizations. *Journal of Contemporary Ethnography, 44*(6), 709–736.

Piché, J., & Walby, K. (2010). Problematizing carceral tours. *British Journal of Criminology, 50*, 570–581.

Pratt, J. (2008). Scandinavian Exceptionalism in an era of penal excess; Part I: The nature and roots of Scandinavian Exceptionalism. *British Journal of Criminology, 48*, 119–137.

Reiter, K. (2014). Making windows in walls: Strategies for prison research. *Qualitative Inquiry, 20*(4), 417–428.

Reiter, K., Sexton, L., & Sumner, J. (2017). Negotiating imperfect humanity in the Danish Penal System. In P. Smith & T. Ugelvik (Eds.), *Embraced by the welfare state? Scandinavian penal history, culture and prison practice* (pp. 481–508). London: Palgrave Macmillan.

Reiter, K., Sexton, L., & Sumner, J. (2018). Theoretical and empirical limits of Scandinavian Exceptionalism: Isolation and normalization in Danish prisons. *Punishment & Society, 20*(1), 92–112.

Scott, D. (2015). Walking amongst the graves of the living: Reflections about doing prison research from an abolitionist perspective. In D. H. Drake, R. Earle, & J. Sloan (Eds.), *The Palgrave handbook of prison ethnography* (pp. 40–58). London: Palgrave Macmillan.

Ugelvik, T. (2014). Prison ethnography as lived experience: Notes from the diaries of a beginner let loose in Oslo Prison. *Qualitative Inquiry, 20*(4), 471–480.

Ugelvik, T. (2018). *Apples and oranges and all kinds of prisons*. Presented at the Comparative Penology, Nordic Exceptionalism and the Prisoner Experience Conference at the University of Cambridge, England.

Venkatesh, S. (2008). *Gang leader for a day: A rogue sociologist takes to the streets*. New York: Penguin Press.

W, R. (2017, January 26). Danish government will pull benefits from immigrant families whose parents refuse to send kids to language school. *CPH Post Online.* Retrieved from http://cph-post.dk/news/danish-government-will-pull-benefits-from-immigrant-families-whose-parents-refuse-to-send-kids-to-language-school.html

Doing Ultrarealist Ethnography: Romanticism and Running with the Riotous (While Buying Your Round)

James Treadwell

This section concerns the praxis of ethnography, or my own doing of ethnography in precarious inner-city locations in central England. When criminological ethnographers write of their own experiences, they often use travel metaphors, and I suppose that I could dress this up in some sort of personal voyage or journey through ethnography, but I hope that the trip isn't over yet, and the problem with such travel metaphors is that they suggest a road travelled, and well, I am not sure that I have gone that far.

I still live in the area in which I have spent most (though by no means all), of my life in. I'd like to think of myself as a sort of pragmatic realist, the sort that recognises paradoxically how crime can be full of wit and humour but also full of viciousness that turns on a knife edge. I have seen the banter of the prison wing turn to a bloody and shocking scene and been laughing watching a football match only minutes later to seeing those I was with kicked unconscious in the street. I have smelled the sweet and pungent aroma of a cannabis factory and heard men laugh and joke as they recount the most horrible acts, and I have sat and heard people's inner fears, thwarted dreams, lost chances and personal tragedies, and occasionally, some of that experience and those stories have been converted into aspects of ethnographic studies. For some, there is no real value in this, but for me I feel 'ethnography' puts me in a unique position. As a researcher I feel I was talking about a number of crime shifts and mutations before others necessarily noticed. My work on eBay and Counterfeits, with the benefit of hindsight, now seems strangely prophetic as there is an acceptance that drops in recorded crime statistics were overplayed and simplistic, simply failing to account for the changes in criminal practice. I have also been able to generate a reasonable publication profile and publish reasonably well based on undertaking ethnography, but I can claim to be no great authority. I certainly have

J. Treadwell (✉)
Staffordshire University, Stoke-on-Trent, UK
e-mail: James.Treadwell@staffs.ac.uk

not had the success of Alice Goffman or Sudhir Venkatesh, but I keep plying my trade.

That is why here I want to issue a few disclaimers. As I was initially asked to, I have tried to write this in a conversational style, and not be too verbose or grandiose, but it was not a comfortable piece to produce, in part because I still have lingering doubts about my own expertise or my ability to say anything all that profound. That is perhaps why, when I was asked to write this piece by the editors, I suppose I was enthusiastic on one level, but I was, and perhaps remain, a little reticent on the other. The fact that it arrived at all is probably due to the tenacity and persistence of Stephen Rice (some of the best ethnographer characteristics). I held onto it, because as humans we sort of seek comfort in the familiar, what is new to us can be alarming, anxiety-inducing, and dare I say it troubling. If that is the case for writing and handing over a book chapter that moves academics away from the tried and tested conventions of using big words, multiple references, assuming a written style that narrowly conforms to a range of conventions, that balancing of achieving an objectivity and formality that many who write professionally do not feel constrained by. That a line in an email 'Please remember that we're looking for a tone/tenor foundationally different from most books in CCJ. That is, a conversation' can cause consternation and force me away from perhaps where I have started to become increasingly comfortable in the academic ivory tower. But I suppose that several projects now and good field data are testimony to the fact that as an ethnographer I am alright, and I think I can hold my own in a conversation, so I will try and do that as much as I can in this piece. The instructions to me in email 'we want to give the readers a look at the relationship between the research and the person behind the research. Towards this end, several scholars who have submitted their papers have included very few (if any) in-text citations' from the book's editors again cause a discomfort. Is this proper academic work, is it just purely self-indulgent or what use is this to anyone interested in ethnography and qualitative research: I still ask myself?

As cultural criminology acknowledges, beyond 'true confessions', qualitative fieldwork results from, and at the same time reproduces, the researcher's own gendered identity, and our reflections here would seem to lend support to that assertion. Arguably this is true of all manner of experiences and facets of the criminological researcher's background. The research we undertake is both a part of us and a part of making us what we will be. We make our research and our research makes us.

Certainly, ethnographic research has been the making of me. Without it, I do not know what I would be doing now. I think there is a good chance I would be in prison. I am a pretty average, white English male; I am just above-average height and well built, and I'd say I am probably about average intelligence in an IQ test, but that might be a stretch. I read a bit, like to have fun and like talking; some people would describe me as hard to shut up, and I prefer indefatigable. My salary and status now are certainly not 'working class'; my mindset and attitudes put me somewhere on a continuum between there and middle class. US readers are probably less alert to those distinctions, framed as they are between blue and white collar, and perhaps regardless of nation, these are just passé when some in sociological circles

are willing to declare the death of class. Yet class occupies a vital part of English experience, and, some ethnographers would argue, shapes who we English are (Fox, 2004). Of my person I guess some people would say I am alright, and I am sure that some would say I am a total bastard. I can be selfish and massively unswayable and fixated. My wife, who probably knows me better than anyone else in the world other than me (after all I am the only one who spends twenty-four hours a day every day in my own head), often says I have something of an addictive and totalising personality, and I can see that. I am naturally curious, occasionally perhaps a little bit paranoid and defensive, and I don't always deal well with criticism (I am trying to get better at it, but I am certainly not great). I also tend to identify myself as an academic, and my professional working job title is 'professor of criminology', which doesn't get you free upgrades as much as I would like and comes from a post-1992 university (i.e., not posh or elite) that I am fiercely proud of, not least because it is located in one of the most disadvantaged and precarious locales in the UK and draws in a non-typical undergraduate student cohort. For fifteen years I have worked and researched in the university sector, at several universities, and have undertaken several ethnographic projects, and I think overall my research is reasonably well received, and those academics I have worked with would be willing to say I am a decent ethnographer, but if I am on a journey, it is one of uncertainty, and I feel slightly uncertain about claiming any expert status.

As a long-term proponent of ethnography and qualitative research, I think I understand at least in part where such insecurity comes from. In public discussion, uncertainty is often presented as a deficiency of research. It's an essential part of scientific research, and the social sciences are no different, except that their matters are human behaviour, with all the nuance, contradiction and complexity that that field brings. Yet it may not come as a surprise to find that while I remain convinced in the scientific merits of my qualitative practices, and will argue that ethnographic and small qualitative research can provide every bit (or more) of a contribution to criminology's theoretical elaborations and ties to policy as can ICPSR mega-downloads, big data sets and quantitative numerical analysis and data modelling, to assert that and make that argument remains complex. One does not need to be an ardent social constructionist or symbolic interactionist to see the 'running conversations' about criminological method remain, as do dominant ideas that science is about numbers. Only a tiny proportion of US and UK criminology is qualitative and features people as complex than them transformed into numbers, despite the fact that crime is every bit a human behaviour that cannot be understood quantitatively or with diagrams and regression analysis alone. Criminology is a discipline with its orthodoxies and its disagreements. I have always seen the likes of Jason Ditton and Dick Hobbs as something of a guiding light and inspiration in that way. They always saw the shifts and changes from the ground and built from there, seeing the world change and issues emerge before the quantitative researchers and policy makers notice, the streets drive the stats, and qualitative and ethnographic researchers have used that intimate connection to social life to drive their own observations, which, while easily dismissed as small scale, micro, subject to regional variation and not representative, also, when taken collectively together showed the very social

transformations and changes in human behaviour that then, subsequently becomes the stuff of trends and fluctuations.

So, ethnography *can be* a uniquely useful method for uncovering the realities and meanings of human life. Humans create their social, enacting meaningful processes. Because ethnography provides insight into these processes and meanings, it can most brightly illuminate the relationships between structure and agency and can shine light in deep corners, and it can give us the raw material which we can both build and test theory. At a deeper level, it might shed light on deeper processes too, but given my desire to avoid descent into more complex arguments about the nature of the world and foregoing those here, I simply appeal to those considering ethnographic methods to alert themselves to a theoretical vista beyond social constructionism emerging from the British ultrarealist movement (Winlow & Hall, 2015). For now, it is worth noting that a shared unity in the purpose and validity of the method unite as much as distinct theoretical arguments divide, and at least a shared recognition in broader critical criminology is something to be celebrated. After all, in both the USA and the UK, criminological research relying upon participant observation remains relatively rare (as it should! It's better to study serial killers from a distance than from personal experience). Ethnography's place in the criminological method persists as a peripheral.

Yet also as a note of caution, I have long felt that as academics we ought to take care not to conflate ethnography with other qualitative methods, such as interviews. This is something that concerns me, because ethnography is unlike any other method and is about 'living' the research. I still have some reservations about the term ethnography as applied to carceral settings. I often now find myself undertaking participant observations; while qualitative experience generally might benefit from having something of an ethnographic sensibility, ethnography is not simply qualitative research but is a positioning that uniquely explores lived experience in all its richness and complexity and requires that aim as function.

Secondly, by focusing on my own research journey, I hope to explain what I think ethnography can contribute to criminology and the academic enterprise more generally. Here, I focus on how both overt and covert processes and meanings structure life. Ethnography allows for both intensive and extensive analysis, and deep details provide the optimal way to illustrate and explicate the oft-stated connection between the life world of a social group and the world they construct, but it is never enough to simply look only over the surface explanations of such constructions as if they are the be-all and end-all. How else better to determine how place and agency intertwine and recreate each other than in examining how different social individuals and groups inhabit, manipulate and articulate their life world, but that articulation does not negate the need to dig deeper. What good ethnography does is to dig deeper.

Yet despite the obvious recompenses of ethnography, it is subject to a continual and ongoing critique, and, indeed, this is healthy and necessary. In the final section of this work, I explore three common critiques levelled at ethnographic work, reflecting upon my own journey as an ethnographer: that it is overly subjective and hence 'unscientific'; that it is too limited to enable generalisation and broader theory construction; and that it ignores the conditions of its own production and thus

unquestioningly reproduces power disparities and representational practices that deserve interrogation. Although these critiques do not lack merit, I want to mount what undeniably is both a defence and a call to arms that we do ethnography better, in both a more real and more honest way. In many ways then, I hope this chapter, based on my personal experiences in the field and outside, helps to that end. The focus and topic of this book, which concerns using ethnography in criminology and the process of discovery through fieldwork, is one which I can relate to. I am no statistician, I balk at regression analysis, but it is ethnography and its methods that frame my career, and in outlining my own personal research journey, I hope to make some broader points about the value and function of ethnography.

The Makings of This Criminological Ethnographer

My personal journey into academia was perhaps unconventional, well if there is a conventional pathway into academia. I grew up in a small area of South Birmingham, England's second city largely in the 1980s in what was a rather typical working-class area. It was by no means the most impoverished part of the city or country but was largely comprised of working-class people (employment principally in Birmingham's manufacturing centre). That time in British history was a turbulent one, both socially and politically. Our neighbours included a police sergeant and his wife and children, and we would play in the streets together, although this ended during the 1984–1985 miners' strike, an epochal moment in many public police relations when the police started to make money and many other people started to regard them as government boot boys. The working-class men on my street begrudgingly tolerated the police, especially the more respectable working class, yet when the police officer bragged about the overtime money he was making, he was so ostracised he had little choice but to move his family to a detached house in a more salubrious area; he became a figure of hate. For most working-class children, the path towards adulthood was standard, primary followed by local comprehensive school; the brightest might study for advanced-level qualifications to better prepare for a well-paid job, aiming towards some better employment outcome, but the university was not really talked about much.

I learned a range of other skills though; as I grew into adulthood, I knew clothing brands and fashion fads, people and faces and how to navigate entrepreneurially. That was expected and normal. Of course, what is more apparent now, in hindsight, is just how much British society was changing in that very period. Those changes might be captured in a plethora of graphs and figures, but really that time was a high point for the Thatcher government at the 1983 general election, despite unemployment doubling to some 3 million, and went on to win a landslide victory, thanks in large part to labour's divisions and its left-wing policies. That perhaps set the early seeds for my journey into the heart of violent English protest and nationalism decades later, but it also set the stage for much of my early life. Certainly, of my

early life in the midlands, I recall that the middle classes were not that distinct socially, culturally or economically from working-class peers.

We all largely attended the same (or similar) schools and lived on the same streets or at least nearby, at least in part because of the kind of egalitarianism of the late 1970s held sway and British people were statistically about as equal as they had ever been (and potentially, will ever be). After this period, the post-war consensus began to be eroded most notably by Thatcherism, deindustrialisation gained pace and poverty rose, as did crime. The stage was being set for the criminologists of the future, and while I played football on game fields looking at the stolen and burned-out Astros, Metros and Escorts, I was schooled alongside and amongst many that would go on to serve time in Her Majesty's prisons.

The social and economic uncertainty wrought in inner cities such as Birmingham, Manchester and Liverpool during that turbulent period connected with my inquisitive mind, as during my early years, unbeknown to me, I undertook what was arguably the perfect process of apprenticeship for criminological ethnography in English inner cities. This thing, which some academics tend to suggest of as some mythical process that involves learning the local language and argot, participating in daily routines and becoming sufficiently part of the local social environment to be a convincing insider and outsider, is also just the process of growing up. What you do not get told much in reflexive accounts is which criminologists went to poor schools and who had their education paid for privately and if their father taught them the workings of the old boy network or how to roll a spliff (in US parlance a joint). Yet if we accept that crime as conventionally understood and defined is a phenomenon that tends to be associated with and constructed as predominantly the preserve of the lower social strata of men and the lower social classes, then those factors are important. My own education yielded a couple of good GSCEs (i.e., high grades) from a comprehensive school that had a reputation in the 1990s not for turning out finished young men, but the best TWOCers (taking without owner's consent, i.e., joyriders), ram raiders and robbers in England's second city. I first got into trouble on my first day of secondary school, and I don't think a week passed where I was not in some sort of trouble. My school fed the ranks of the infantry and the last vestiges of the car production line at Rover, but it largely did not send its alumni into university. It sent more to the region's prisons.

Alice Goffman's recent experiences may show that it is not always prudent to confess to a crime in print, but it is sufficient here for me to say that between my teen years and my twenties I was myself no angel but had a gift of sharp enough intellect to remain largely uncaught and undetected when I did commit offences. While ethnography tends to self-flagellate a little about the ethical and legal implications in general of breaking the law or acquiring 'guilty knowledge' during fieldwork, we think little about what the necessary attributes are to do good ethnography in the first place. [And besides, it's beyond the statute of limitations.]

Let's be honest for a moment; some people will make awful ethnographers in some contexts, perhaps removed and placed in an alternative setting they would function, but ethnography is perhaps more art than science, that art of balancing ability to understand the emic and etic, the inside and out and of talking to people not with a condescending tone or full of insincere sympathy but reasonably. That

making of personal bonds is easier, arguably if there is something that is shared. Yet as Goody long ago suggested 'criminology, as a social science discipline, has never embraced the idea of research that is based on the study of the individual. There remains an unhealthy intellectual suspicion of what "the individual" or, more damningly, "the personal" has to offer criminology' (Goody, 2000, p. 474). My first experiences of doing ethnography involved MA and then PhD research, the latter of which involved an extensive study of a football hooligan group in the North of England using ethnography in the form of participant observation. Inspired by the changes I saw on the streets and amongst my own friends on 'the scene' that arose out of the socio-economic culture of football in the early 1990s, I was keen to pick up where Gary Armstrong's inspirational ethnography (1998) left off. My participation over more than 2 years relies upon contacts with the firm's older 'main faces' and top boys, many of whom I counted already as contacts. I went to matches, got stopped and searched and behaved as if I was back in a period of school-like adolescence while remembering to make field notes and write up. I couched the study academically in that complex language of the individual biographies of those men involved in football hooliganism, examined against the socio-structural conditions of late modernity and post-industrialism that have given rise to new specific understandings of masculinity and crime, but really what I did was to translate my own understanding of what was going on into an accessible form of relevant, real-world criminology of the sort that I had read that had inspired me. I had no doubt that they had really met real, active proper criminals. I doubted that a great many academic criminologists had, and even when they did, I doubted their ability to separate the reality from the bullshit. I felt I could and, in the first instance perhaps, was too guilty of regarding ethnography merely of being the process of showing what was happening but without that wider meaning. However also I knew, but lacking the academic words to show it, that also I wanted specifically to say to criminologists that what seemed to be happening was that the concern with violence and team loyalties that once underpinned football hooliganism was mutating into a more orchestrated and organised form of violence, which in turn seemed to be serving as something of an apprentice to the world of more serious instrumental and organised crime. Yet, even then such an aim troubled me. It did not necessarily fit with the academic discourse constructed around social research that so frequently stresses the essentiality of 'grounded theory'. Surrounding me in academia were far more intelligent people, people schooled and knowledgeable in the conventions and terminology of social research. I am not so sure now though that those people would have coped quite so well with having a pint glass thrown at them or would have fared well in a violent confrontation. That is not to try and celebrate the more machismo elements that sometimes arise out of ethnographies' tendency to tell glorious and exciting war stories. Anyone with enough modicum of intelligence to produce good ethnography in unconventional and sometimes risky settings will know full well that violence is not glorious, and they might know that real pain hurts and that real violence is not merely a social construction. Such experiences tend to confound the point that, as qualitative researchers and ethnographers, what we learn in the field and report back in the academy is not 'objective fact' but rather is

perspective informed opinion. As Altheide and Johnson (1998) suggest, 'all knowl-edge is'. This 'perspectival nature of knowledge' must – it follows – be considered 'an obdurate fact of ethnography' (Altheide & Johnson, 1998, p. 490).

The point I make here about method, and perhaps the best lesson I can teach anyone, is that ethnography, while seemingly democratic, inclusive and open, is put simply 'not for everyone'. The field isn't for everyone, some won't like it and some won't get it. They will see the surface, but not the deeper issue. That said, I do not think I elected to use ethnography out of some zealous drive to campaign or show resistance and reliance in working-class communities. So often, academic research-ers do not question in any depth why we select our areas of interest. What draws us to certain topics? Can something be inherently 'interesting', or must we be drawn to it for a reason? Is it just familiarity and comfort, and if the latter, how does that impact on seeing the bigger picture?

It is only recently that criminologists have attempted to enter a somewhat uncom-fortable position of trying to ascertain why we choose and have chosen these topics to fill several years of our lives. It is notable, for example, that I might have been as usefully employed as an ethnographer if in the 1990s I moved to London, took a job as a trader and awaited the onset of a global financial crisis. If you want to under-stand the harms wrought by crime, then political special advisors might be as wor-thy a group of study as street gang members. That raises the additional question of how much criminological research is truly objective in acknowledging the under-pinning of the social, economic, political, moral and cultural standpoint of the researcher open to real, proper scrutiny. How much can we say that of any social research? While in the world of postmodernism and identity politics, standpoint sociology is coming a little cleaner about the values of the producer. Yet paradoxi-cally a drive towards ever-greater 'reflexivity' in ethnographic work has not neces-sarily produced more honesty or transparency, so too there is a horrible certainty and very intolerant, zealous and conviction-driven agenda that drives some of the supposedly more liberal criminology and sociology, but that may be a matter for elsewhere.

For me, as an ethnographer of lower-strata criminality in deindustrialised and precarious locales, a core theme that has united much of my research, a thread that runs through and unifies it, I suppose, is physical violence or the latent threat thereof. By my teens I had learnt that violence was not glamorous but painful, especially if too much on the receiving end. I learned to box and kickbox from my early teens and engaged in numerous heavy sparring sessions. I was thrown in with older and bigger boxers, and while I learned to like the experience, seeing your face smeared in blood and feeling the pounding in the skull that comes afterwards, I also devel-oped a reputation for having a fair amount of tenacity and courage that could open doors for me and put money in my pockets. In those settings, in low-lit gyms in backstreets and under swimming pools and on industrial units, I also had learned a range of other skills that were equally useful: how to crack a joke, how to navigate 'banter' and when to speak up and shut up. I had learnt that violence did not particu-larly bother me particularly, whether it was witnessing or doing it, but I had also learnt how to be around hard men and not to 'take the piss' or, as importantly, have

people take it. I still do not mind being around threatening situations and people, but I am also realistic and grounded; I have seen things turn quickly, and sometimes being a good ethnographer is knowing when to walk away, at any stage in observations. While I am comfortable in my own abilities as an ethnographer, I am not complacent when in places and spaces marked by precariousness. That closeness also brings with it a real consideration of ethics, because I have long believed that true ethical practice is more likely when relationships are meaningful and when research subjects can't be left in the field, because going native is not a fear if you are born and raised amongst the group.

For my part, I feel that I have always been relatively grounded in my experiences as a criminologist; rather fortuitously my biography had opened a rich field of potential research topics (in criminological terms), and moreover, the milieus in which I have moved have never been totally alien. Certainly, before embarking on a criminology degree at university, I was the product of some similar experiences to those that criminologists describe amongst their offender 'subjects'. The wasted hours I spent in gyms and pubs I cannot count. Occasionally, I hit people or engaged in instrumental criminal practices (which only sometimes proved profitable). I had been arrested but had been lucky to navigate without the wholesale stigma of a serious criminal record, less lucky when it came to acquiring good tattoos, but my background and experiences taught me a bit of humility, and as I realise that I might as easily have been the subject a criminological study, I have attempted to present the worlds I describe realistically and fairly.

That reference to realism is an important one, as theoretically and empirically I now find myself perhaps more sympathetically orientated towards ultrarealist ideas than ethnographies' familiar place with social constructionism. For me, rather than just an abstract and highbrow theoretical argument, it has become evermore pressing to resist the worst excesses of a constructionist narrative that stresses resistance against conformity, freedom against narrow restrictions, because put simply some things might be more important than social construction, and having spent years in lower-class milieus, political economy seems just that. When some Marxists criticised ethnography for documenting only the surface of events in local settings, rather than seeking to understand the deeper social forces that shape the whole society and that operate even within those settings, they may have had a point. Good criminological ethnography for me is that which shows not only the action but the mundanity of crime that captures its nuances and contradictions but does not reduce everything of our ethnographic efforts to mere grounded analysis of everyday experience. Good ethnography aims to provide a convincing written and textual analysis and description that provides insights into the relationships between human actors, their motivations, daily practices, structural conditions and meaning. The profoundly unsettling transformations that have reverberated throughout the social, economic, political and cultural world and a well-developed theoretical and analytical context that capture a world in flux are needed to make good ethnography great. If both the powerful and those at the lowest points in the social strata experience important real and universal forces and these impact upon psychosocial drives,

human behaviours and cultural-economic conditions, then ethnographers can show this; good ethnography links the theory to the practice.

Going Right, After a Riot

Occasionally though for all that background might count, the aspirant ethnographer may be presented simply with an opportunity too good to miss. If opportunity 'makes the thief', it also sometimes might make the ethnographer. It was just such opportunism that underpinned the ethnographic work that I undertook on the English riots with Dan Briggs, Simon Winlow and Steve Hall, which also became part of the empirical base of the book *Riots and Political Protest* (Winlow, Hall, Treadwell, & Briggs, 2015) alongside the work on the English Defence League (EDL).

The riots research was also used for the article *Shopocalypse Now: Consumer Culture and the English Riots of 2011* (Treadwell, Briggs, Winlow, & Hall, 2013). That research happened because I just happened to be in Birmingham City Centre at the right time to watch things kick off, but I also suppose that, by that time, I had developed something of an instinct for searching out violent disorder. Having spent a few years previously largely avoiding police attention during public disorder around football helped me mingle and talk to people. Of course, such research and the immediacy of the decision to stay and watch were never going to pass before an ethics committee and are risky; I could hardly be sure that my own front door would not be opened by what police call their 'big red key' (i.e., a battering ram) in subsequent days. Certainly some of those I spoke to experienced that and subsequent jail terms. For my part, I did no violence, stole nothing and largely watched, the lowest risk strategy I could adopt, but that does not mean that a riot is a good place to be. I saw others around me robbed at knifepoint, but fortunately seemed to carry enough presence to not be directly threatened myself. A blackberry mobile phone and voice notes, pictures and text notes did the rest, an approach to data collection that I had practised well in my PhD studies. Yet this brings me to another important aspect of ethnography and perhaps the revision of prior made points.

My time undertaking covert ethnography with the EDL was similarly connected to personal contacts and focused on the uncertain state of the social world. The English Defence League (EDL) is a far-right street protest movement which focuses on opposition to what it considers to be a spread of Islamism and sharia in the UK. It describes itself as an anti-racist and human rights organisation, but its longstanding ideology is a belief that the religion of Islam 'challenges an English, Christian way of life'. The group has had confrontations with various groups, including Unite Against Fascism (UAF), and for several early years I was at the centre of several of its branches undertaking ethnographic research. Again, the data I gathered has been used in several places but perhaps most significantly as part of *Rise of the Right* with Simon Winlow and Steve Hall (Winlow, Hall, & Treadwell, 2017).

What becomes apparent during that research was that much of what had drawn me to ethnography now lacked appeal. In the EDL I did not see a kindred identity

with which I felt any unity. Rather I saw something that made me profoundly uneasy. Claims to be 'non-racist and non-violent' made by many of its early rank-and-file supporters were utterly transparent. It also highlighted for me another issue which perhaps ethnography ought to become better at acknowledging openly. There is extensive literature that talks of the risk of over-familiarity or 'going native' (a term I dislike for all its imperialistic overtones' and for reasons I have already begun to outline). The negative occurrences that exist in ethnography when there is biographical congruence and proximity between the researcher and the researched, similarity and proximity do not always create solidarity or shared sentiments. More compelling accounts of 'the self' in criminological research have tended to show how the individual impacts upon research processes and the production, and it is sometimes noted how orthodox criminology and its focus on 'objectivity' in 'methodology' and 'restrained language' effectively 'discourage any form of biographical or emotional intrusion by the researcher' (Jewkes, 2012, p. 65). Of course, these biographical processes can be heuristic, but not only in creating a congruence. I grew up like so many of those who I encountered in the EDL; I lived on the same sort of former council estates in Birmingham. Was I ever at risk of going native? I do not think so. I didn't like them. Much of the guidance on undertaking ethnographic research generally will stipulate that the basic ethical principles which are to be adhered to and maintained throughout the process of fieldwork include honesty and transparency, and the ethnographer should aspire to doing good, not doing harm and protecting the autonomy, well-being, safety and dignity of all research participants. They will stipulate that researchers should be as objective as possible and avoid judgements. What then of the ethnographer faced with a group of regressive, violent racists willing to make sweeping statements and pour out simplistic, narrow platitudes? While occasionally I met those who I could separate from their narrow racism, the fieldwork dragged me down further; being in a dark place too long makes ones' outlook on life dark. In and amongst the EDL, I lost weekends and evenings to dimly lit drinking holes, sessions of alcohol and cocaine and hours of racist, angry diatribes. While many talk excitedly of fieldwork and it becomes the stuff of conference talk and ethnographic stories, told and re-told, the field can be a place that is hard to leave, especially if one starts close to it.

We often do not discuss what happens when a researcher does not like his or her participants, but perhaps we should, especially in criminology. After all, it is likely to be the biggest factor that impacts upon the subsequent framing of the presentation of the study. Most ethnographers are at least willing to admit that they are not value-neutral and perhaps not all are willing to admit to their own prejudices, yet in subjectively focusing in too much upon the self, to look at either one's personal history or the emotive processes that arise outside of and inside of fieldwork, there is the clear risk that the researcher disconnects themselves from their wider social setting. I didn't like the EDL, but I knew where they came from; I also knew that they were not representative of many white and ethnic minority working-class men. I hold strongly a conviction that any move towards the subjective must not render the researcher blind to the often very objective and real social suffering that criminological research is frequently concerned with, and the micro and macro social

processes that drive action, and while I found I disliked the EDL, their complaints, their directionless anger, their ignorant prejudices, racism and insularity, I could see the drivers of it; they were all too apparent, again because of the congruence with my own experience and biography and background. I am pretty sure Steve Hall and Simon Winlow felt the same, and we captured all the essence of that in *Rise of the Right* (Winlow et al., 2017).

But where I certainly was by the time that my work with the EDL was ending was somewhat adrift. I had a young daughter at home but had spent weekends and evenings not at home with my beautiful wife and child but in pubs and clubs, at angry demonstrations full of sweat, testosterone, flying spit, punches, lager and cocaine. I was depressed, I had put on weight, I wanted out and I headed back to the gym. When I first stepped back through the doors of a mixed martial arts gym at the end of the EDL work, three minutes of vigorous exercise nearly killed me. Within a year, I had lost what I estimate to be about 25 kilos in weight and was fighting in MMA bouts, my mind was clearer and I was again enjoying ethnography, back in the field, but this time in the context of a prison and even enjoying being hit in the face and keeping a reflexive journal for an ethnography of the less salubrious side of British MMA which I hope may yet come to see the light of day. The darkness had lifted, and I felt ready to return to the field. That setting was prisons, again looking at violence with Kate Gooch, and it is prison research in an ethnographic manner that has been the basis of my most recent research projects. If ethnography needs to be integrated into or combined with other kinds of new theorising and new theoretical perspectives that are better suited to studying whole institutional domains, national societies and global forces, as ultrarealists have started to suggest, then the USA and UK as places share much, and yet the UK does not, in recent years perhaps, share quite the same commitment to a political ethnography built at and around imprisonment, and that is where my journey seems to be taking me next. I suppose in those lines I betray my view on the issue of whether ethnography ought to be theoretically neutral or whether it has an essential affinity with particular theoretical orientation, but that is perhaps a longer story in the making and one that still lacks a conclusion as yet, but the road travelled so far has taught me a few lessons for the aspirant ethnographer that I think might be well worth sharing.

Don't be afraid to call bullshit on your participants: you do not have to be aggressive or disrespectful if you do this, but a lot of people will have preset narratives, and it would be remiss to just take these on face value and accept them. Do not be afraid to challenge people or dig deeper in terms of what they are saying; sometimes people will get a little animated, or even possibly angry, so as and when you do this, tread carefully, and you might want to be respected in the field, but ethnography is not about just hearing what people tell you.

You have two ears and one mouth, normally best used in that ratio, but don't be afraid to talk about yourself, if you expect people to open to you, you must be willing to do them the same courtesy. Prepare to be bored, frustrated, angry, annoyed, disappointed, frustrated, upset, anxious and perhaps occasionally depressed and frustrated. The ethnographic adventure is often presented as very exotic and exciting, but a lot of it is repetitive, monotonous and just, well, a bit dull. Be ready for

this. Be prepared to not like all or some of your participants: some ethnographers have a habit of puffing up their own credentials or ability as researchers by emphasising the close relationships they had with their participants and how friendly they were and the bonds they formed. What they don't mention is that this can be and sometimes is exaggerated, and they often thought their participants were arseholes and did not like them at all. You will think this about research participants just as you do about colleagues, family members and friendship groups. Not having a close emotional connection with your participants does not make you a bad ethnographer, a close one doesn't make you good. The most important part is that you are there and there consistently and that by being there you get the chance to speak to people. Sometimes it is better if you just go with the flow. Don't waste your time and energy defending what you do too much to doubters and detractors, critics and naysayers. The best ethnographers have spent plenty of time dealing with these folk, but then they also come to realise that not only are their opinions as unwarranted and unneeded as they are unsolicited, but to deal with them too much expends better time that you will never get back. If they want to challenge you, tell them to do it in print. You might get a citation that way. Get into the field; lots of people can offer you advice, but the best way to get better at anything is through practice. And finally, just as my dad taught me, always buy your round.

References

Altheide, D., & Johnson, M. (1998). Criteria for accessing interpretive validity in qualitative research. In N. Dezin & Y. Lincoln (Eds.), *Collecting & interpreting qualitative materials*. London: Sage.

Armstrong, G. (1998). *Football Hooligans: Knowing the Score*, Oxford: Berg.

Fox, K. (2004). *Watching the English*. London: Hodder & Stoughton.

Goody, J. (2000). Biographical Lessons for Criminology, *Theoretical Criminology, 4* (4), pp. 473–498.

Jewkes, Y. (2012). Autoethnography and emotion as intellectual resources: Doing prison research differently. *Qualitative Inquiry, 18*(1), 63–75.

Treadwell, J., Briggs, D., Winlow, S., & Hall, S. (2013). Shopocalypse now: Consumer culture and the English riots of 2011. *British Journal of Criminology, 53*(1), 1–17.

Winlow, S., Hall, S., & Treadwell, J. (2017). *Rise of the right: English nationalism and the transformation of working-class politics*. Bristol: Policy Press.

Winlow, S., Hall, S., Treadwell, J., & Briggs, D. (2015). *Riots and political protests; Notes from the post political present*. Abingdon, Oxon: Routledge.

Winlow, S., & Hall, S. (2015). *Revitalising criminological theory; Towards a new ultra-realism*. Abingdon, Oxon: Routledge.

Shots Fired: Navigating Gun Violence and a University's Intervention While in the Field

Check for updates

Marta-Marika Urbanik

> Given the current heat in Regent Park, the killing that occurred last night and the potential for continued retaliation (more shootings, including more drive-bys), I am seriously concerned about your safety. I realize that you think you know what you are doing (as you have expressed to me), and I do not doubt your street smarts or relationships with the guys. Unfortunately, this won't help saving your life if you happen to be in the line of fire during the next drive-by shooting. We both know that enough people die of senseless violence each year, including in Regent Park, even though the victims are usually keenly familiar with the "street code" and "know what they are doing". Further, in the wake of last night's homicide, there will be an intense police presence and related investigation, and you will become known to them as someone who might have information (if you aren't known to them already). You might also become a target yourself, given that you are known to be associated with that group of young men…
>
> (Excerpt from an email sent from my Ph.D. Committee,[1] August 19, 2015).

Introduction

Given the sensitive and "risky" nature of many criminological ethnographic studies, the relationship between criminological ethnographers and risk-averse research ethics boards (REBs) is often characterized by reciprocal suspicion. While REBs view criminological ethnography as *exceptionally* risky for research participants, crime ethnographers—among others—often see the review process as intrusive, superfluous, tyrannical, and largely disconnected with what ethnographers actually *do* (see: Fassin, 2006; Feeley, 2007; Haggerty, 2004; Katz, 2007). For many, the review

---

M.-M. Urbanik (✉)
Department of Sociology, University of Alberta, Edmonton, AB, Canada
e-mail: urbanik@ualberta.ca

process is considered to be an unsubstantiated burden, to strategically overcome or to avoid all together, thereby affecting our research decisions. Managing these bureaucratic hurdles may be particularly challenging for graduate students and junior faculty who have limited institutional authority and are less adept at standing up to REBs (Feeley, 2007). While the focus of REBs and our frustrations with them has typically relates to the consideration of hypothetical harms to research subjects, also embedded within REB protocols are deliberations about *researcher* safety. Yet, concerns about the possibility of researchers experiencing fieldwork harms and strategies for managing REB concerns about researcher safety continue to receive limited scholarly attention—particularly in relation to *ongoing*, as opposed to prospective, fieldwork.[2]

This chapter will convey my experience of navigating the on-the-ground complexities of conducting criminological ethnographic fieldwork in an allegedly "dangerous" place, alongside "dangerous" people, during a particularly "dangerous" time. In particular, it will illuminate a situation when the University deemed my ongoing fieldwork as "high-risk", almost suspending my REB approval and forcing me out of the field immediately. I will analyse the extent of REB's understanding of crime ethnography, REB "power" in affecting the course of a graduate student's research, and the real and potential consequences of this intervention for my physical safety and research project. Given that many of us conduct research that REBs might consider exceptionally risky, my story is likely not an idiosyncratic oddity, and I hope that it will illuminate an important challenge that is often discounted when we consider ourselves atomized researchers, instead of members of a broader research community struggling to resist restrictions on our research endeavours. First, I will describe my research project and fieldsite and explain how a series of shootings in the field prompted a University intervention. I will provide an honest and barefaced account of what happened,[3] describing where the University's intervention went wrong, how it affected me, potentially jeopardized my relationships and my project, as well as the mistakes that I had made along the way in trying to resist the University's interference. Second, I will argue that despite the inherent risks presented by many criminological ethnographic studies, crime ethnographers are still best-placed to determine the risks that they are exposed to. Here, I will problematize the assumption that outsider perspectives on research risks are more "objective" than the perspectives of those immersed in the fieldsite. Lastly, this chapter will draw attention to the silver lining of the University's intervention, providing hope that we *can* participate in effectively moulding how our REBs understand and treat criminological ethnographic projects.

---

[2] For an exception, see Moskos (2015).

[3] This account is informed by my own memories of what occurred, notes I had taken at the time, email exchanges, and informal conversations with my supervisor. My rendition of meetings or conversations that I was not privy to stems from formal and informal accounts of my supervisor, my Ph.D. committee members, and representatives of risk management services at the University of Alberta.

Fieldsite and Research Project

My doctoral research was based in Toronto's Regent Park neighbourhood, which is Canada's oldest and—prior to its ongoing revitalization—was Canada's largest social housing project, with 100% of the neighbourhood's 69 acres devoted to social housing. The neighbourhood has a notorious reputation as one of Canada's most violent and crime-ridden neighbourhoods, and media portrayal conveyed the neighbourhood as a "ghetto" and as "a haven for single mothers, welfare families and deviants… a magnet for crime and drug problems" (Purdy, 2005, p. 531). I originally entered the neighbourhood as a master's student[4] in 2013, when I worked as a research assistant on a project that was exploring the effects of its ongoing neighbourhood revitalization. Although the vast majority of neighbourhood residents that I interviewed were law-abiding citizens, I increasingly got to know some of Regent Park's major criminal players—prominent gang members and those involved in the neighbourhood's informal economy (i.e. drug trafficking). I soon became interested in their experiences of the revitalization, and in the summer of 2014, I made Regent Park the fieldsite of my own doctoral work, undertaking an ethnographic examination of *how* and *why* revitalization efforts were affecting criminal *processes* and *structures* within the neighbourhood. I genuinely enjoyed every minute of my fieldwork; I came to love the neighbourhood, its residents, and grew especially close to a number of my research participants. I spent time "deep hanging out" (Geertz, 1998) in the neighbourhood's parks, on benches, on resident porches, at its basketball courts, in school yards, in resident homes, in backyards, and in the community centre. Despite how much fun I was having and how often my participants and I would pass the time laughing, telling jokes, sharing meals, and otherwise enjoying the warm summer days, I was acutely aware of the dismal realities that plagued many families in the neighbourhood: poverty, joblessness, relocation, victimization, health concerns, incarceration, addictions, and mental health issues. Thus, although my fieldwork was enjoyable, this was often overshadowed by the injustices that many of my participants struggled with.

---

[4] At the Centre for Criminology and Sociolegal Studies at the University of Toronto.

Fieldwork: 2015

In June of 2015, I returned to the field to recommence my fieldwork.[5] By this time, I had already spent two summers conducting ethnographic research in Regent Park and had conducted over 100 interviews with neighbourhood residents. I was spending almost all of my time in the field with the neighbourhood's major criminal players—a group of about 20 men (18–47 years old)—at their favourite hangout spot: a basketball court colloquially known as River Court. I "hung out" with my participants approximately five to eight hours a day at River Court, where they would play basketball, sell and use drugs, drink alcohol, gamble, and otherwise just "chill" with friends and neighbours. I felt particularly excited and comfortable in Regent Park. My participants seemed exceptionally content; the summer was a hot one; the neighbourhood was buzzing with summer camps, neighbourhood BBQs, parties, and sports programmes, with many residents enjoying their front yards and communal neighbourhood areas well into the late evening. The neighbourhood was also remarkably "calm" this summer; my participants were more relaxed than I had ever seen them before, police presence was minimal, and there were no active "beefs" that my participants seemed weary about—a significant mood shift from the previous summers.

By August, things changed precipitously. During the course of 2 weeks, five shootings occurred that were either within the neighbourhood (three shootings) or were allegedly related to Regent Park (two shootings). Although the first shooting did not significantly contribute to neighbourhood tensions, the following shootings most certainly did. The last shooting resulted in a homicide. I had narrowly missed two of these shootings, where I would have been with the individuals who were shot at, at the place where they were targeted, and at the time they were targeted, had I not coincidentally—and luckily—either just left the location or was just minutes away from arriving there. During one of these instances, a masked man on a bike approached some of my key participants who were just spending their Friday evening hanging out at River Court and started indiscriminately shooting at them. On this day, I had already spent the afternoon and early evening in River Court and had left just ten minutes before the shooting occurred. The second shooting that I narrowly missed was especially brazen and, given its nature, shook up the entire neighbourhood. It occurred the following week, on a hot Friday afternoon, on the boardwalk, adjacent to River Court. The boardwalk served as passageway for pedestrians and vehicular traffic and was lined with a few benches where my participants would sometimes hang out. That day, I had stopped in at a community centre just one hundred meters away from River Court, to check in with some local residents. I was just about to make my way back to River Court when one of the youth counsellors invited me into one of the summer camp sessions, where local youth were

---

[5] Given that my fieldsite (Toronto) was located across the country from my doctoral programme (Edmonton), I conducted my fieldwork during the summer months and would return to the University in autumn.

playing drums. I was chatting with some of the youth when about fifteen minutes later, a camp worker came into the room and notified us that a drive-by shooting had just taken place at the boardwalk and police were securing the scene. I headed towards the boardwalk which was now lined with police tape and, after speaking with neighbourhood residents, found out that a car had sped down the busy boardwalk, firing a gun at my participants. These shootings unsettled me, and I was particularly worried for my participants.

Unfortunately, gun violence remains a lived reality in Regent Park. However, despite the relative frequency of gun violence, most Regent Parkers had told me that they felt relatively safe in their neighbourhood. When I asked how they could feel safe in an environment that seemed so dangerous and chaotic to outsiders, they attributed their feelings of relative safety to the *nature* of violence in the area. Certainly, all shootings had a negative and disruptive impact on the neighbourhood. However, both prosocial and criminally involved neighbourhood residents told me that the *modus operandi* of most neighbourhood shootings allowed them to isolate neighbourhood violence to dangerous spaces, people, and times. Many Regent Parkers described that most shootings were rarely indiscriminate and generally occurred late at night in corners of the neighbourhood, thus mitigating the chances that innocent bystanders would be victimized. Further, they alleged that gun violence was rather predictable; residents would often catch wind that violence against a specific person was likely and often knew why a specific person was targeted. At the same time, residents acknowledged that there was always the possibility that an uninvolved individual could get hurt. Many residents, especially those who were criminally involved, would emphasize that "bullets got no names". Thus, although *all* neighbourhood shootings would spark resident anxieties, when gun violence deviated from the "norm" and endangered uninvolved residents, the shooting would have a greater negative effect on the community. The nature of the drive-by shooting at the boardwalk, coupled with the high frequency of gun violence within just two weeks, was uncharacteristic of neighbourhood violence and consequently unsettled many residents. Regent Parkers saw this as a sign that the neighbourhood

was particularly "hot" and expected further violence. Given local tensions, there was a significant police presence, and my participants—when they did spend time outdoors—relocated their hangouts from their usual locations (River Court, the benches at the boardwalk) to areas much closer to building entrances, where they could seek immediate refuge if more shots were fired.

The physical dangers of my research were threefold. First, I was spending time in an allegedly "dangerous" place. In addition to intra-neighbourhood violence, Regent Park also had serious and long-standing "beefs" with other social housing projects across the city. These "beefs" were often tied to gang activity; though similarly to American cities, gang rivalries could implicate entire neighbourhoods in their beefs, thereby subjecting neighbourhood residents to inter-neighbourhood violence—most commonly in the form of drive-by shootings. Second, I was spending time with "dangerous" people, as some of my participants had lengthy criminal records for violence (assault, robbery, gun crimes, murder) and drug offences. Some of my participants carried knives, guns, baseball bats, and chains, which was particularly risky given that they regularly drank and consumed and sold drugs in my presence. My participants were also potential targets of violence from rivals within, but also from outside the neighbourhood (often tied to the aforementioned neighbourhood "beefs"). In this sense, the neighbourhood itself and my participants were also *endangered*. While I trusted that my participants would not harm me, there was always a possibility that I would be present when gunfire erupted. Third, the timing of my fieldwork presented additional risks; Regent Park was "hotter" than usual, and residents expected retaliatory violence. Consequently, by spending time hanging out in Regent Park, I was potentially endangered by the space itself, by my participants, and by those who sought to harm my participants, during an exceptionally "dangerous" and tumultuous time. Further, the recent controversy surrounding Alice Goffman's work (2014) reminded me of the legal and ethical risks surrounding research projects like mine. The public backlash reinforced how carefully I would have to navigate the field, secure my fieldnotes and interviews, and determine when to stay/participate and when to leave/excuse myself from activities.

Given heightened neighbourhood tensions, I communicated with my supervisor daily, keeping her abreast of what was happening in Regent Park. I informed her about the shootings in real time, letting her know that I was safe, especially since I knew that she was monitoring news reports. She was concerned for my safety, and I promised her that I would "lay low" (spend less time in the neighbourhood, be extra careful, leave when I felt things might escalate, etc.) until the tensions eased. However, the concentrated shootings, the homicide, and resident predictions that more violence was imminent unnerved my supervisor (untenured at the time), who reached out to other members of my Ph.D. committee for guidance. They shared her concerns, and on the morning of August 19, I received an email documenting their trepidations and supporting my decision to lay low. That same morning, unbeknownst to me or my supervisor, one of my committee members sought advice from legal counsel at the University, who informed the University's risk management services (RMS)—the division responsible for managing research-related risks—of the dangers of my fieldwork. Immediately, RMS formed an incident response team to assess

the risks pertaining to my fieldwork. The incident response team comprised representatives from the Provost's office, General Counsel, protective services, the Office of Emergency Management, and Insurance and Risk Assessment. The incident response team held its first of several emergency meetings just hours after becoming informed of the situation. Although my department chair was present at this meeting, my supervisor was not yet informed of the situation (and neither was I).

My supervisor only became aware of RMS's involvement when she received an email requesting her presence at the *second* emergency meeting pertaining to my fieldwork—just hours after sending me that first email. This took her by complete surprise, and as an untenured faculty member who had never encountered RMS before, she turned to a senior colleague for support, who then personally invited the Graduate Chair. At this meeting, the incident response team identified several risks of my fieldwork: personal safety (due to the location of the research, perceptions about my research participants, and the homicide), legal risks (if my research became implicated in a homicide investigation), the University's reputation (if I was injured or killed), and legal liability on behalf of the University (if the University did not adequately "inform" me of the risks associated with my research and did not take "appropriate" action to keep me safe). Given these concerns, the incident response team maintained that I was in imminent danger and was pushing to suspend my research ethics approval, forcing my immediate removal from the field. To absolutely guarantee my exit from the field, they wanted me to present myself at the University (across the country from Regent Park) the *following* day. My supervisor was then allowed to speak on behalf of my project, defend *her own* supervisory and research qualifications, and try to quell the University's concerns. The other members of my department supported my supervisor's claims that although my Ph.D. committee shared similar concerns, they had already encouraged me to lay low and were quite confident in my ability to remain safe in Regent Park.

Despite the response team's strong disagreement with this claim, thanks to my supervisor's and committee member's immense efforts to try to preserve my research, the response team decided to temporarily hold off on suspending my research. Instead, the team decided that two steps were to be taken. First, my supervisor would formally contact me via email, express the University's concerns, and inform me of the immediate steps that the response team insisted I took to mitigate risks to my safety. As part of this, I was prohibited from entering the field until further notice and was expected to formulate a detailed safety plan (described below) that I would propose to the response team. Second, the University's protective services unit would obtain "ongoing monitoring" and an independent risk assessment from the Toronto Police Service. If I did *not* produce a safety plan, if the University considered the safety plan *inadequate*, or if the level of danger in the neighbourhood *escalated*, the University would immediately suspend my research ethics approval and force me out of the field. Ironically, while senior University officials were holding yet another emergency meeting pertaining to *my* safety and ethical and legal obligations that the University assumed on behalf of my research, I was sitting with my participants across in Regent Park, completely unaware of what was happening in Edmonton.

Defence Mode

When I received the official email from my supervisor, I was shocked. Who were these people? Who called them? Was I in serious trouble? I saw myself as a dedicated, trained, and experienced researcher who had followed all research ethics guidelines. I was hurt, scared, and furious at everyone. I was upset at the committee member that had brought this situation to legal counsel. I was angry people I had never even heard of thought I was a damsel in distress who had to be saved from dangers I believed were exaggerated. I was irate that individuals completely unfamiliar with Regent Park assumed they could assess the level of risk to which I was exposed. I was also frustrated that the response team failed to recognize that I was a relatively experienced researcher at the time; this was my third summer in Regent Park, and I had been involved in a number of other research projects where I was alone with participants in potentially "dangerous" places and had conducted close to 200 interviews. Although I was still a Ph.D. Candidate, I had a demonstrated record of being "street smart" and had proven my ability to handle myself in risky situations. Further, I had not participated in any illegal activities; I did not have evidence that could incriminate my participants and had taken appropriate steps to secure my fieldnotes and transcripts if they were ever subpoenaed by the courts. Despite the heightened dangers of my fieldwork, I did not understand why a group of unknown others thought it was necessary to interfere with and potentially suspend my research.

Admittedly, even writing this chapter brings back the flurry of anger and resentment that I felt at the time. Trying to get my thoughts on paper has been challenging; I had the printed email correspondence on my desk for a while, and I postponed reviewing it for as long as I could. Chatting about the intervention with my committee members unsettled me. I know that I may come off as bitter about the situation, but I saw this official intervention as an attack *against me* and believed that my judgement as a person and my integrity as a researcher were being questioned at best, and doubted at worst (Moskos, 2015 cites similar frustrations). It seemed that my efforts to build rapport, collect data, and maintain professionalism while in the field and at the University were futile. I felt disempowered and betrayed by the University and by my committee members. I even felt betrayed by own supervisor whom I greatly admired and respected. I did not understand why she allowed the response team to exaggerate the situation or why she was so bizarrely formal when she communicated with me. Never before had the power differential between University actors and myself been so apparent, and never before had I felt like the University and my committee were failing me. Immediately, I regretted being honest and forthcoming with my supervisor; I saw my transparency of the situation as the catalyst to this bureaucratic commotion I was subjected to and also somehow responsible for! I was also shocked at the velocity at which this was all occurring. How could several senior University members have attended this morning's meeting, if my Ph.D. committee just affirmed my position to lay low a few hours ago? How was I expected to submit a formalized "safety plan" by tomorrow? I could

barely process my feelings or take fieldnotes; how could I create a formal document petitioning the continuation of my fieldwork on such short notice? And if the response team did not consider the safety plan sufficient, they would suspend my research immediately?!

I was particularly upset that being pulled out of the field would have affected the *quantity* and *quality* of my data and could have impacted the completion of my Ph.D. Although, by this point, I had collected "enough" data that I could have exited the field and started writing my thesis, I wanted my scholarship to be as comprehensive and nuanced as possible and believed that terminating my data collection would negatively affect the quality of my work. I felt like there was so much more I needed to learn and believed that my job as an ethnographer was to gather as much data as possible, to get the fullest story possible, to be as accurate as possible, and to uncover as much about the sociological and criminological phenomena that I was studying as possible. The recent spate of violence had uncovered a new and critically important area of academic investigation that I was well positioned to pursue and felt obliged to do so. I also felt a sense of duty to my participants to continue my data collection; they considered me their "minor historian" (Scheper-Hughes, 1993, p. 22) and placed hope in the fact that I would serve as the mouthpiece of their struggles.

Many of these feelings were attributed to the fact that the incident response team *did not* communicate with me directly and my supervisor was *not permitted* to privately communicate with me, so I had limited knowledge of what was happening at the University. I was not informed of the formation of the response team, their conversations, or their meetings, and I was not given the opportunity to communicate with them directly. Thus, I felt that the University completely disregarded my expertise and judgement and saw me as vulnerable, defenceless, and/or foolish. I did *not* need a response team meddling in my research, and I certainly did not need a group of powerful University representatives debating my capability to conduct myself safely, ethically, legally, and professionally in the field.

The University's involvement also significantly contributed to the emotional turmoil I was already experiencing. Having just missed two shootings and knowing that residents expected future violence, I was undeniably anxious about the situation and couldn't even articulate my feelings into fieldnotes. Despite these feelings however, I still believed that I could manage the situation on my own, and rightly or wrongly, believed that the University's concerns were exaggerated. Further, I felt that the only people who understood what I was experiencing were other urban ethnographers and my supervisor and another colleague—both of whom had also experienced their participant's victimization first-hand. My family never really understood the nature of my work or my relationships with my participants. They learned about the shootings through the news media, which further fuelled their opposition to my presence in the neighbourhood and my association with "such" people. I also didn't feel that I could tell them about the University's involvement in the situation, as they would have been upset that the completion of my Ph.D. was in jeopardy. My friends (nonacademics) were similarly disinterested; though they were concerned for my safety, they were not cognizant of how the shootings affected

me, what this might mean for my research, and how the University's intervention could affect my doctoral programme. So, when my relationship with my supervisor was briefly compromised, I was left to navigate the emotional turmoil and the response team on my own.

Reflexivity

As ethnographers, we are always pushed to be as reflexive as possible about what we are seeing, assuming, feeling, writing, and experiencing. Reflexivity enhances our scientific rigour, understandings of our data, and enriches our own methodological reflections. It also helps with our own psyche and supports our ability to process the difficult truths of our research and our own positionality. However, sometimes our positionality smacks us in the face and *demands* recognition—when academic niceties and protocols relating to being "reflexive" are not sufficient. Many of these moments happen during our experiences or traumas in the field when we recognize our own privileges in relation to our participant's lives. But there are also times when we are uncomfortably reminded of our privileges by dynamics *external* to the field.

The University's response to the situation in Regent Park was a stark reminder of my privilege—a necessary truth that we sometimes lose sight of when we are immersed in our fieldsites and increasingly identify with our participants. Although this may seem preposterous, the University's willingness and ability to pull me out of the field nauseated me. I had a multi-billion-dollar institution charged with protecting my safety. I had a room full of senior University representatives that would mobilize any and all available resources to assist me in any way that I wanted, or in any way that they saw fit. They would have arranged an immediate "escape" from the "dangerous" area, booking me an immediate flight to safety, and would remain on "standby" until I returned from the field. In this moment, I was paralysed by researcher guilt or, more appropriately, privileged guilt; I could leave the field at any moment, and though my departure might be inconvenient, or undesired, it would have a limited impact on my life. My participants could not be pulled to safety. This was their life and their everyday reality. Put simply, it appalled me that my participants did not have a similar escape route. I was terrified for my participants, some of whom may had been the direct targets of, and/or participants in, these shootings. I was aghast at how scared they must have been. Despite being in far more immediate danger than myself, my participants had nowhere to go, and no one who would save them. Looking back, I realize that I was not adequately reflexive about this. I did not appreciate that although this situation was new and chaotic for me, it was not that unusual for my participants—it was an unfortunate fact of their lives. While I knew that they were shaken, they did not experience the shootings as profoundly as I did—despite being in significantly greater danger—because they had unfortunately been in this situation before. I was imparting my own perceptions and feelings of the situation unto them, similar to what the University was doing to me.

Thus, producing the safety plan was a sobering moment—a stark reminder of my own privilege and the injustice of the situation for my participants.

I also struggled with whether I should tell my participants about the University's response. I wondered how they would respond to the fact that I had a multi-billion-dollar institution willing to take drastic action to keep me—just a graduate student—safe and was concerned that this might influence their perceptions of me. Would this not be a screeching "reminder" of their own disadvantage and their comparatively dismal alternatives? Would this contribute to their feelings of helplessness? A "student" status often grants ethnographers with a "hall pass" to conduct research with many difficult-to-access populations. Even though many graduate students are far more advantaged than many of the populations that we study, referring to ourselves as "students" conducting some version of "school work" often works to minimize these privileges in the eyes of our participants. Had I initiated my fieldwork as a professor, I doubt that I would be awarded the same benefits that I received from neighbourhood residents (ironically, my student status worked to my detriment at the University, which considered my position as an additional liability). Thus, in addition to not wanting to "remind" my participants of their limited alternatives, I also had a self-interest in not increasing or further illuminating the social distance between us. I believed that telling them about the situation would contribute to their nihilistic worldview and generate greater stress and might jeopardize our relationship. I was particularly worried about the fact that the University was, allegedly, in contact with the Toronto Police Service. I cannot think of a quicker way to lose access to my core participants than this causal exchange: "Hey, my University is worried about my safety and they are actively monitoring the situation alongside 51 Division". Given these fears, I did not tell my participants about the University's interference, though I am still unsure whether this was the right decision. Although I had always been truthful and forthcoming with my participants previously, I writhed with balancing the harms of full disclosure and non-disclosure in this situation.

Writing the Safety Plan

I originally viewed the safety plan as a pro forma hoop I had to go through in order to continue my fieldwork, and so I wrote the first iteration in a frustrated and even a condescending tone. I saw it as a cumbersome formality being imposed on me primarily because of the University's concerns about their legal liability had I been injured or killed in the field. I reckoned that if I signed off on the dangers of my fieldwork and absolved myself or my family of any legal recourse against the University, the University would be satisfied and this bureaucratic mess would dissolve. Hence, I believed that the best course of action would be to downplay the dangers of my fieldwork, and so I characterized the situation in Regent Park as "low risk" and "a bit volatile". I suggested that I would minimize my exposure to harm in the field by limiting the amount of time I would spend with my criminally involved

participants, leaving the neighbourhood by 9:30 p.m., and only conducting scheduled interviews for the next few days. Although I understood that the situation in Regent Park was riskier than it was typically, I *still* believed that my victimization was unlikely. While I did not feel as safe as I normally did, I was already "laying low," being more vigilant, heading home when I had a bad feeling, and passing through River Court quickly. Thus, my characterization of the risk level in Regent Park was consistent with my feelings that the University's concerns were overstated and with my desire to remain in the field.

I can readily accept that to an outside observer, I was at an increased risk of danger. Some may suggest that outside observers or the response team "experts" were *better placed* to judge the immediate hazards of my fieldwork, given that they were unbiased and emotionally removed from the situation and had no stake in my data collection. Although these are all valid points, this position is also quite paternalistic, as it discredits my *first-hand* knowledge and experience of the situation. My belief that I was relatively safe from harm may seem absurd. However, as criminological ethnographers, aren't most of our perceptions and decisions about safety, fieldwork enjoyment/fulfilment, and data collection efforts typically perceived as outlandishly risky by those who do not conduct similar types of research? Have many of us not been charged with taking foolish risks by "hanging out" with "unsavoury" and "inherently dangerous" individuals, in "dirty" and "hazardous" places? Looking back at our own first encounters with criminological ethnographies a la Bourgois (1995), Maher (2000), and Venkatesh (2000), did we not have similarly visceral reactions to the risks that they took in the name of research? As outsiders, we immediately recognized the "risks" of their fieldwork, and yet these ethnographers felt comfortable enough to continue their research and are now celebrated for their ability to build rapport, immerse themselves in environments that most individuals would not dare to enter, and uncover important criminological findings. Thus, though some may accuse me of being naïve of the inherent risks of my research in Regent Park—especially during this tumultuous time—or worse, of trying to appear bold, valiant, or heroic by "minimizing" the risks to my safety, can outsiders truly have a more objective understanding of the risks at hand than an experienced ethnographer who has already managed to safely navigate the area? Aren't they as outsiders, and didn't we as bright-eyed students, merely make judgements based on "external representations" of such places, instead of recognizing the immense disconnect between these outsider perspectives and their "internal representations" (Purdy, 2005; Wacquant, 2007, p. 67)? Despite our best intentions, we—as criminological ethnographers and others in the academy—may be drawing from and perpetuating the very same stereotypes that many of us try to quell. Further, though some may suggest that criminological ethnographers sometimes place themselves at unreasonable risk in the pursuit of data collection, given that the vast majority of urban ethnographers conducting research in "dangerous" settings with "dangerous" people *are not* subjected to serious harm or injury, we need to acknowledge urban ethnographers' capacities to balance the potential risks and benefits of their research, all while remaining safe in the field.

Making Mistakes

Upon receiving the safety plan that I had so despondently crafted, my Ph.D. committee sternly advised me to change my narrative and present the situation in Regent Park as an "elevated risk". They maintained that my "low-risk" and "a bit volatile" characterization was inaccurate, suggested that I did not have an appreciation for the gravity of the situation, and would lead the response team to further doubt my personal judgement. They recognized that my frustration with the response team clouded my willingness to acknowledge and appease the University's concerns. In addition, they proposed that I keep a 4:00 p.m. curfew, given that two of the recent shootings occurred in the late afternoon and early evening. They also found my proposal to limit my interactions with my criminally involved participants as unsatisfactory, expressing concerns that I may be the target of retaliatory violence vis-à-vis my relationships with my participants or my presence in the area. Understanding that my sudden disappearance from the neighbourhood or ceasing communication with my participants could instil suspicions that I was a "snitch" or an undercover police officer (thereby potentially leading to my victimization), they proposed that in the interim, I would not actively seek out contact with these individuals but would politely and briefly interact with them when they approached or contacted me. The committee also recommended that I would not conduct any interviews until my supervisor and I reassessed the risk situation in Regent Park.

This pushback further agitated me. I had already done what was asked of me and did not understand why I had to adopt the response team's risk classification. I did not feel comfortable supporting that I was in a high-risk situation, since I did not truly believe that I was. I worried that if I did describe my fieldwork as "high-risk" in an official document, the response team would be legally mandated to immediately remove me from the field. In addition, drafting the safety plan had already taken up considerable time, and given my concerns for my participants, I felt that my time would be better spent processing my feelings and staying abreast of developments in Regent Park. In hindsight, my first shared draft was utterly foolish. I should have known better than to characterize a situation where gunfire was increasingly unpredictable and frequent as low risk. Irrespective of my feelings, I should have had the foresight that the incident response team would not be satisfied with my haphazard approach. My research experience aside, I had limited experience navigating such serious University formalities. Admittedly, my characterization of the situation as "low risk" was also an attempt to resist the response team's authority and uninvited "meddling" in my research.

Playing Along: Writing the Safety Plan

Thanks to a stern phone call from a professor, I realized that this was an incredibly serious situation and I needed to "play the game" in order to preserve my project; the stakes for noncompliance were just too high. I had to be strategic with the safety

plan, since I was *not* in a position to bargain or dismiss the response team's concerns. Thus, I mobilized the safety plan for *my* benefit. I recognized that the response team was not comprised of ethnographers or criminologists and likely considered the study of criminal activities as inherently risky. Indeed, a young woman "hanging out" alone with "gangsters" in a Toronto "ghetto" learning about their lives and criminal activities was in and of itself a hard sell to the Ethics Board. I acknowledged that the response team likely had a very limited understanding of the realities and inner workings of disadvantaged neighbourhoods and that their perceptions were likely clouded by sensationalized media narratives of "ghetto life" which convey violence as imminent and indiscriminate. I began to accept that the response team had honourable intentions in trying to protect me—and the University—from harm and realized that I had to give the response team more complete and more specific information.

In the safety plan, I presented more information about Regent Park, the general nature of neighbourhood violence, as well as the neighbourhood's internal dynamics. I highlighted that Regent Park spans several city blocks, that the violence was secluded to one small area of the neighbourhood, and that every day, thousands of residents safely navigate the area. I outlined that although violence had increased in the 2 weeks prior, it was generally not indiscriminate; it was usually confined to specific concerns, at specific times, when certain individuals would congregate. Thus, criminally involved residents were at the greatest risk of victimization, and the heightened risk was temporary as violence usually occurs in brief clusters and then returns to its usual levels. Second, instead of minimizing the response team's concerns, I conceded to their position that the recent shootings had placed me at an increased risk. Third, I emphasized how my extensive experience in Regent Park, my intimate knowledge of its inner workings, and my honed "street smarts" enabled me to remain safe in the field from the onset, and how I would continue deploying this knowledge going forward. Lastly, I outlined specific ways that I would further reduce my exposure to risks. These included significantly reducing how much time I would spend in the area, what time I would conclude my research, the areas that I would avoid, and who I would be in contact with. Some of these rules are outlined below:

- For the next 3 days, I will not interview any residents.
- For the next 3 days, I will not frequent river court whatsoever.
- For the next 7 days, I will not be present in Regent Park past 5:00 p.m.
- For the next 7 days, apart from a fleeting greeting, I will have no contact with individuals that are believed to be involved in current neighbourhood tensions. I will not "hang out" with these individuals during this time.
- For the remaining duration of the research, I would be in constant contact with my supervisor, letting her know when I entered and left Regent Park.

Foreseeable Unintended Consequences

However, the *most important* component of my safety plan was an explanation of how, despite their best intentions, the response team's decision to pull me out of the field could possibly place myself and others in *greater* danger than allowing me to remain in Regent Park. I explained that my participants were aware that I had intended to stay in the field for another 6 weeks and that I was hoping to conduct another 20 interviews. I had been in Regent Park almost every day for the past 7 weeks, thus becoming an "expected" presence in the area. My participants would immediately notice my sudden disappearance from the area and would be especially suspicious if this departure directly followed a series of shootings and an active police investigation, largely directed at my participants. I had spent months building rapport with my participants and trying to convince them that I was neither a police officer nor an informant, so that I could secure access to this group of men. My sudden disappearance could undo all my efforts and suggest that I had been sharing intelligence with the police, thereby entirely terminating my access in the near and distant future.

Further, similarly to other disadvantaged neighbourhoods across North America, many Regent Park residents adhere to a "street code" (Anderson, 1998; Urbanik & Haggerty, 2018; Urbanik, Thompson, & Bucerius, 2017), which deems violence and retaliation against "snitches" as acceptable and even mandatory—placing me at a significant risk. I also clarified that although my removal from the neighbourhood might reduce the *immediate* risks to my safety, these dangers may materialize in the future. As a native Torontonian, I frequently visit the city, and given Regent Park's proximity to its downtown core, it is likely that I could run into my participants during a subsequent trip. If my participants labelled me a "snitch", future encounters could be dangerous, especially if my participants somehow associated the police scrutiny they were subject to with my presence in the area. In addition, I was particularly concerned about the fact that the street code allows for retaliatory violence against a family member or close friend of the suspected "snitch". Although I may have been safe from retaliation while being on the other side of the country, my family—who lives in Toronto—was not awarded the same protections.

This possibility aside, I also outlined that my participants would have likely seen my abrupt departure from the neighbourhood as offensive in a number of ways. First, it could insinuate that I feared for my safety, which could have disrespectful undertones to the participants I was particularly close with and those who saw it as their duty to protect me while in Regent Park. Second, as the only woman who associated with this group of men on a daily basis, I was the go-to person for moral support. My participants allowed themselves to be vulnerable with me; speaking about their problems, trusting me with their feelings, and sharing their fears of violence. Some of them told me that they could not be as open with their male counterparts, out of fears of appearing "soft" and/or "weak". The street code necessitates that men present themselves as tough, capable, emotionally and physically strong, and always ready and willing to use violence. As a woman and as a "trusted outsider"

(Bucerius, 2013), my participants saw me as someone they could be authentic with, even when it counteracted their status as "gangsters". Thus, suddenly disappearing from the lives of my participants—especially during such an emotionally tumultuous time—was unconscionable, insensitive, and, frankly, disrespectful. Not only would this be morally reprehensible on a personal level, but it could destroy any possibility of research access in the future and could also subject me to victimization in the future.

Upon reviewing my safety plan, the response team ultimately decided to allow me to continue my research, though some members still believed that I was in immense danger and should be removed from the field immediately. Thus, the allowance was conditional; I had to strictly follow the rules outlined in the plan, and the response team would continue monitoring the situation and would immediately intervene if tensions escalated or the violence continued. Fortunately, the violence had ceased, and I was able to spend the next 6 weeks in the field, gathering more data on the themes I was originally pursuing and now also exploring the aftermath of neighbourhood gun violence.

Deviating from the Script: Rule Breaking

While I designed the rules in my safety plan to keep me safe while in the field (and mostly to appease the response team), in some respects, these rules were more problematic than they were helpful, and thus I broke several of them. For example, one of the rules stated that I would not be present in the neighbourhood past 5:00 p.m. One afternoon, I had spent the past several hours at a makeshift rap studio with some of my participants who were recording music. It was approaching 5 p.m., when one of my core participants from the past summer arrived at the studio with his friends. Our relationship was somewhat strained since he had come on to me in a sexual manner,[6] and I made it clear that I did not appreciate this. I knew that if I instantaneously left to adhere to my curfew, this might suggest that I was still upset with him and could provoke an unpleasant exchange. I believed that I needed to remain at the rap studio and behave as normally as I had before his arrival, to signal that we were still "cool" with each other. Given our previous relationship, his identity, and the sway he had with other Regent Parkers, upsetting or disrespecting him

---

[6] As a young woman studying a hypermasculine group of men, I was immediately sexualized by my participants, although the nature and extent of this sexualization changed during the course of my research. My initial interactions were largely dominated by the men seeing me as a sexual conquest, which I worked to try and overcome through insisting that I was not interested in a sexual or romantic relationship and politely rejecting their advances, as well as through trying to dress and behave in a desexualized way (wearing baggy clothes, minimal makeup, not fixing my hair, sitting/ standing/speaking in less stereotypically "feminine" ways, etc.). Although this was strategic on my end, dressing down also allowed me to be comfortable during long hours in the field—mostly outdoors—and I would often consciously pick my outfit to ensure that I was able to run for cover if I ever had to (running shoes, no jeans).

(even unintentionally) would not be a smart decision in terms of my safety or research access. I was also worried that my participants might think that I was uncomfortable or fearful being in a closed setting with 9 men (or those men in particular). I had established mutual trust with my participants, and they had never given me a reason to fear them. Again, in a neighbourhood which is largely governed by the street code, showing this type of "vulnerability" or "fear" *makes* one vulnerable to victimization, as others capitalize these vulnerabilities in their own gendered performances. Had I left immediately or soon after their arrival, I might have undone all my efforts of appearing as unafraid. It might also suggest that although my participants trust *me* enough to let me into their lives and grant me access to potentially incriminating observations, experiences, and conversations, I did not trust *them* not to harm me. This was further exacerbated by the fact that many of my participants (including the individual in question) had told me that they saw it as their *duty* to protect me. They were "cool with" my presence in the area and said that I could roam the neighbourhood as I wished, and if anyone gave me trouble, they would "handle it". I was not about to jeopardize relationships that served a protective factor during my fieldwork, so I remained in the rap studio forty minutes after my curfew.

In another one of my rules, I stated that I would not communicate with any criminally involved individuals for a few days, which turned out to be quite problematic. "Criminally involved" is such a vague description, and since I did not have access to police information and I did not collect data on criminal involvement, I had no *official* knowledge about anyone's criminal involvement. This rule was also absurd given that my core sample consisted of "criminally involved" residents. An ethnographic analysis of the effects of neighbourhood revitalization on criminal structures and processes almost *mandates* that my core participants would be involved in illicit activities. Further, it wasn't clear what "communicate" referred to—was I not allowed to accept phone calls from my participants? Could I not respond to text messages? If I was in the field and they wanted to talk to me, did I have to avoid them? Given my relationships with my participants, I creatively worked *around* this rule—not necessarily breaking it, but also not adhering to it.

Thus, had I strictly adhered to all the rules outlined in my safety, I would have jeopardized my research access by coming off as afraid, disrespectful, or uncaring, which could have also placed me at an increased risk. Our relationships in the field can change from minute to minute, and, since many of us depend on these relationships, most ethnographers go to immense lengths to protect them. Given the complex interpersonal dynamics at play in fieldwork, stringent rules are essentially useless, since they do not leave room for fieldwork complexities to shape our decisions. What is further problematic about this situation is that I *could have been reprimanded* by the University for breaking these rules, even when I believed that it was safer for me to do so. What would this reprimand look like? Could the University suspend my project on these grounds…? In addition to trying to navigate the fieldsite's uncertainties, I now also had to worry about breaking rules that I considered constraining and not beneficial.

The Ugly Truth

Unsurprisingly, the University's intervention was severely detrimental to my relationship with my supervisor (though thankfully, only briefly). I want to clarify that it was not the intervention per se that compromised this bond but *how* the intervention was handled. Prior to the incident response team's intervention, I had an exceptionally strong relationship with my supervisor. Hence, when the response team appointed my supervisor as their mouthpiece during this entire ordeal, and my supervisor was being so bizarrely stern and formal in our communications—atypical of our relationship—I was completely blindsided where this was coming from. Unbeknownst to me, my supervisor was instructed *not* to communicate with me apart from these formal procedures, and any of our communications had to be forwarded to the response team. Though frustrating, this is consistent with the lack of transparency that operates at the REB or risk management level (Katz, 2007). Thus, while I was in Toronto, confused, angry, and already emotionally charged because of the shootings in Regent Park, I was not aware that my supervisor was experiencing similar feelings in Edmonton. As an untenured faculty member, she was also pulled into a situation that she did not wish to be part of nor one that she was familiar with navigating. I also did not know of the great lengths that my supervisor and another committee member went to trying to convince the response team that I should be allowed to remain in the field nor that the response team was also reassessing *my supervisor's* research ethics approvals. While I felt attacked by the response team *and* my committee, my supervisor was fighting for my research project while simultaneously having to justify and demonstrate her own capacity to supervise these kinds of research endeavours. By not being tuned into what was occurring at the University, I could not appreciate how stressful this situation was for her. My ignorance allowed me to paint the entire response team and my Ph.D. committee with a black brush. Had my supervisor and I been allowed to communicate freely, we would have both had a better appreciation for what the other was experiencing; more specifically, I would not have felt the need to be so defensive, and in turn, I would have been much more receptive to the University's concerns. This was a time when I arguably needed my supervisor the most, and the University had jeopardized this relationship.

Undeniably, a student's relationship with their Ph.D. supervisor is one of the most integral, influential, and consequential relationships within academia, and beyond. A supervisor can significantly alleviate or allay a student's concerns and anxieties and, by extension, can also guide the course of the student's career. Some students are lucky to have strong relationships with caring, engaged, and supportive supervisors—much like I did—so this a sudden and unexplainable change resulted in additional emotional and professional turmoil. Thankfully, my supervisor and I were able to entirely repair our relationship soon after the University's intervention, which involved a number of honest and long conversations about what transpired. Although we were able to successfully traverse this bump in our supervisory relationship, I recognize that not all supervisor-student relationships are strong enough to withstand such emotional and professional turmoil.

Hindsight Is 20:20

Of course, I *now* recognize that the incident response committee was genuinely concerned for my well-being (in addition to having more self-interested concerns like the University's reputation and legal protections) and had no intention of appearing punitive or patronizing. While writing this chapter, I re-examined all the email exchanges pertaining to the situation and the University's response. Reading these exchanges long since the dust has settled and with the hindsight that my research project was salvaged, I have a different interpretation of the situation. I recognize that the committee member who sought the University's legal counsel did not betray me; they were genuinely concerned for my safety and understandably drew on a resource specifically mandated to manage such circumstances. I also understand that they did likely not intend for RMS to become involved. Further, I acknowledge that had RMS not been notified and had something happened to me, likely all the members of my Ph.D. committee would have faced significant professional scrutiny. Though I still vehemently oppose the fact that my supervisor was not allowed to privately communicate with me, I now understand that all of our correspondence was so formal given that it could be subject to future legal scrutiny if the situation went awry. Further, the incident response team's decision to ask me to produce a safety plan and then re-evaluate the situation based on my safety plan demonstrates that the response team did indeed respect my expertise, to some degree. Thus, although this chapter documents my *perceptions* of the University's intervention at the time, it also reveals that ethnographer "reflexivity" outside of the field can further our growth as researchers and scholars.

The Silver Lining

Fortunately, this story has a silver lining. Once I returned from the field, my supervisor and I were invited to speak about the incident at an event hosted by RMS, with several University stakeholders in attendance. Initially, we were quite sceptical about this invitation, as we did not feel that we could speak positively about the situation. However, we eventually decided that we should share our experiences with the broader University community, in hopes of illuminating the complexities of urban ethnography to those who may comprise future incident response teams. Deciding how honest we would be about what occurred and how RMS responded was tricky, but we decided to be forthcoming yet professional in describing how the situation was handled and providing specific suggestions for the University going forwards. Our original fears that RMS would be displeased with our full-disclosure approach were unfounded, as senior RMS representatives commended our honesty and committed to using our experience as a learning opportunity. Surprisingly, the University's willingness to learn from this situation and commitment to *assist* researchers in their fieldwork, instead of *inhibiting* their research endeavours, was

not a bureaucratic nicety. We had a series of productive conversations which ultimately resulted in the development of an improved system for identifying, and responding to, risks to researcher safety in urban ethnography. We were also invited to share our experiences at the Canada Research Ethics Board annual meeting and wrote about the situation in relation to RMS's mandate for assessing risk (Urbanik, Stack, & Hui, 2016). Despite the unfortunate circumstance of this intervention, I consider myself very fortunate to have had this instance dealt with by representatives from the University of Alberta. Thus, although this chapter brings attention to the power dynamics at play in evaluating expertise and judgement and allowing or discontinuing research, it is imperative to highlight that power dynamics at the bureaucratic level can shift if we first *play along* and then subsequently try to change processes from *within*.

Conclusion

Put frankly, Research Ethics Board applications often require that crime ethnographers hoping to conduct "risky" research are skillful creative writers. We are expected to anticipate and mitigate possible harms to our participants—no matter how hypothetical—well before we are familiar with our fieldsites, attuned to fieldwork dynamics, and even have a solid grasp of our data collection efforts. What often gets lost in this tedious speculation is a critical reflection about our safety as *researchers*—on our behalf and on behalf of REBs. Thus, we often have to "handle" risk to our *own* safety on the fly. The long history of criminological ethnographies demonstrates that we do this quite well, since the majority of us successfully carry out our research unscathed. However, even a cursory glance at recent and dated crime ethnographies reveals the significant potential for things to go awry in the field and come to the attention of University REBs. Thus, instead of just sharing our frustrations with and strategies for securing ethics approval, we need to also examine how REBs and University risk management can shape or entirely suspend our *ongoing* research initiatives. My reflexivity in this chapter pushed me away from the protective barriers of objectivity that so many of us hide behind (Etherington, 2007). However, I hope that it has shed light on my own messy experience of learning how to conduct a "risky" crime ethnography while simultaneously navigating University concerns and bureaucratic hurdles. Although we might dismiss such experiences as *individual* problems—seeing ourselves as atomized researchers—clashes with REBs are *collective* struggles. Thus, if we wish to continue conducting important, albeit potentially "risky", criminological ethnographic research without jeopardizing our projects because of University intervention, we should not look at REBs as "foreign regimes foisted on us; they are administered by us and by our peers. Join them; subvert them—or at least curtail them. Serve on them and do all you possibly can to facilitate the research of your colleagues rather than act as a censor" (Feeley, 2007, p. 773).

References

Anderson, E. (1998). *Code of the street*. New York: Norton.

Bourgois, P. (1995). *In search of respect: Selling crack in El Barrio*. Cambridge: Cambridge University Press.

Bucerius, S. M. (2013). Becoming a 'trusted outsider': Gender, ethnicity, and inequality in ethnographic research. *Journal of Contemporary Ethnography, 42*, 690–721.

Etherington, K. (2007). Ethical research in reflexive relationships. *Qualitative Inquiry, 13*(5), 599–616.

Fassin, D. (2006). The end of ethnography as collateral damage of ethical regulation. *American Ethnologist, 33*(4), 522–524.

Feeley, M. M. (2007). Legality, social research, and the challenge of institutional review boards. *Law & Society Review, 41*(4), 757–776.

Geertz, C. (1998). Deep hanging out. *The New York Review of Books, 45*(16), 69–72.

Goffman, A. (2014). *On the run*. Chicago: University of Chicago Press.

Haggerty, K. D. (2004). Ethics creep: Governing social science research in the name of ethics. *Qualitative Sociology, 27*(4), 391–414.

Katz, J. (2007). Toward a natural history of ethical censorship. *Law & Society Review, 41*(4), 797–810.

Maher, L. (2000). *Sexed work: Gender, race and resistance in a Brooklyn drug market*. Oxford: Oxford University Press.

Moskos, P. (2015). Observations on the making of a police officer. In M. Maltz & S. Rice (Eds.), *Envisioning criminology: Researchers on research as a process of discovery* (pp. 33–42). Berlin: Springer.

Purdy, S. (2005). Framing Regent Park: The National Film Board of Canada and the construction of 'outcast spaces' in the inner city, 1953 and 1994. *Media, Culture & Society, 27*(4), 523–549.

Scheper-Hughes, N. (1993). *Death without weeping*. Berkeley: University of California Press.

Urbanik, M.-M., & Haggerty, K. D. (2018). '#IT'S Dangerous': The online world of drug dealers, rappers and the street code. *British Journal of Criminology*, azx083. https://doi.org/10.1093/bjc/azx083

Urbanik, M.-M., Stack, P. G., & Hui, L. (2016). Episodes of violence during ethnographic fieldwork in the 'hood': A case-study exploring a University's response to increased risks. *URMIA Journal, University Risk Management and Insurance Association, 20*, 51–59.

Urbanik, M.-M., Thompson, S. K., & Bucerius, S. M. (2017). Before there was danger but there was rules. And safety in those rules. *British Journal of Criminology, 57*(2), 422–440.

Venkatesh, S. (2000). *American project: The rise and fall of a modern ghetto*. Cambridge: Harvard University Press.

Wacquant, L. (2007). Territorial stigmatization in the age of advanced marginality. *Thesis Eleven, 91*(1), 66–77.

Doing Criminological Autoethnography: Learning from Conversations with Ourselves

Stephen Wakeman

Introduction

This chapter is all about autoethnography—*what* it is, (and what it isn't), *how* to do it, and *why* it might be useful to criminologists. In its purest form, autoethnography is simply the ethnographic exploration of the self. It is, as Tessa Muncey (2010, p. 2) has neatly claimed:

> [A] research approach that privileges the individual. It is an artistically constructed piece of prose, poetry, music or piece of artwork that attempts to portray an individual experience in a way that evokes the imagination of the reader, viewer or listener.

In this respect then, it's fair to say that autoethnography is somewhat *distinctive* when compared the usual criminological accounts of crime, deviance, and the agencies of their control that we read about in our journals. In essence, autoethnography is about doing research a little bit differently—it's about trying to understand the social world through observing it ethnographically but, at the same time, *critically reflecting upon our place within said world*.

In the spirit of doing things a little differently, this chapter is written in an informal, conversational style.[1] Some readers may not like this and may find the first person narrative somewhat uncomfortable in a field like criminology. It is suggested that these readers skip this chapter. This is because the conversation presented here is unlikely to have much in the way of 'policy relevance'; it is not likely to aid in grant applications; and it doesn't really contain anything that might evidence

---

[1] I am most grateful to the editors for both my invitation to contribute to this volume and being given the stylistic freedom to write in this way.

S. Wakeman (✉)
Centre for Crime, Criminalisation and Social Exclusion, Liverpool John Moores University, Liverpool, UK
e-mail: s.j.wakeman@ljmu.ac.uk

'impact'. Yet all of this is ok with me. In addition to this break with these somewhat crude but nonetheless accurate characterisations of mainstream criminological motivations, the conversation presented here is also less than 'conventional' itself—it is actually a conversation *with myself*. Or, more accurately perhaps, it is a conversation *between* my 'selves': one about to start an ethnographic study of heroin use in a disadvantaged area of the United Kingdom and the other having completed the said study and begun to reflect upon it critically and in the process start to make sense of the ways in which others might employ autoethnographic methods in their work.

In essence then, the aim of the chapter is pedagogic in nature—the goal is to help others learn to incorporate autoethnographic principles and practices into their work, *if* they are willing and open to doing so. This is because an increased focus upon the self has great potentials for the field of criminology (see Ferrell, 2012; Jewkes, 2011; Wakeman, 2014, 2016a); yet at present, these remain somewhat undeveloped. There are a number of well-established reasons for this reticence around exploring the self: Young (2011) wrote of criminology's 'physics envy'; Maruna and Matravers (2007) talked of criminology's general discomfort around the 'individual'; and to this list I have contributed the discouraging influences of the field's administrative functions and funding (see Wakeman, 2014). In short, it appears that there are many reasons why criminologists don't like to talk about themselves and their feelings. Yet, things are starting to change here; a small but significant body of literature is developing around some of the ideas covered below: Lumsden and Winter's (2014) edited volume is a great example. If nothing else, it is hoped that this chapter will add to these arguments and in the process convince more scholars to try and incorporate some of the principles of autoethnography into their studies of crime.

Question One: *What* Is It?

> So, I've read some stuff about autoethnography, but if I'm honest I don't really get it. I'm just not sure what it is exactly, and because of this uncertainty I'm just not sure how I could ever use it in my research.

Well if you're asking: 'what is criminological autoethnography', then that's a great question to start with. Unfortunately, I don't have a simple answer to it. The fact is autoethnography has somewhat porous boundaries, to say the least. There is a lot of discussion around what does and doesn't constitute autoethnography, and this is compounded by the fact that there are many different variants of it. Denzin (2014), for example, identifies at least 15 different 'strands' of the method. For me though, the best definition is usually the simplest one. Autoethnography is ethnography but with a strong focus upon the self. That is, it's ethnography but practiced from a highly reflexive position whereby I (you) place myself (yourself) at the core of the process. It's a way of doing ethnographic research whereby I think about myself and my emotional reactions to the things I observe, just as much as I think about those observed things themselves.

As I've argued elsewhere (Wakeman, 2014), we can understand criminological autoethnography as existing at the intersection of three things: fieldwork, biography, and emotion. In other words, to do criminological autoethnography, you just need to go out into the field and do some research (I suggest this is best undertaken using the ethnographic method of participant observation, but it by no means has to be). Then, what you find needs to be placed in the context of what you know about it from your background—*your biography*. What I mean by this term is basically just 'who you are'. And finally, you record your reactions to the intersections of the former two *emotionally*. That is, you use the emotional reactions your fieldwork and biography elicit as data. That, in a nutshell, is doing autoethnography.

> Ok, but isn't that what they call 'navel-gazing'? I'm with you I think, it sounds promising, but give me an example. Like, how does it work in practice?

Well, how about we hold fire on the example for a minute; I'll get to the 'how' questions below. Let me address that point about navel-gazing first. *That isn't what this is*. Some people might see it that way, and truth be told, I've given up trying to argue with them. Young's (2011) physics envy runs strong with some in criminology, there's little point trying to convert people so set in their ways. Yet, your concern is here is valid. In fact, one of the best examples of criminological autoethnography to date comes from Richard Sparks (2002) who wrote of his unease at the thought of engaging with deep reflexive practice like this. He went so far as to suggest that it could even be 'ethically dubious' of researchers to focus upon themselves in the face of others' suffering. He made a good argument. But, as he went on to stress, if there's a critical component to such introspection, then it's more than warranted. I built upon his ideas to claim that it's more than warranted; it's actually an imperative if we are to properly understand our subjects. The core of the claim only stands up here though if there's a critical and/or analytical 'edge' to the practice of autoethnography.

You see, in essence, the criminological autoethnography I advocate here is an *analytical endeavour*. It is about asking deep and wide-reaching questions of the social world in which we all live. I hope that I can convince open minds of this by stressing the distinction between 'evocative' and 'analytic' autoethnography, best rendered clear in the work of Leon Anderson (2006). Anderson's work was (and still is) important to me, because it was here that I learned that the key critique of navel-gazing could—rightly or wrongly, I'll let you make your own call on that one—be directed at *evocative* autoethnography much more convincingly than it could *analytic* autoethnography. This is mainly because evocative autoethnography (for a fantastic example of which, see Catherine Ellis's work, e.g. Ellis (2004)) is primarily about the self. It's about the author being both the subject and architect of the story being told. In fact, as Ellis (2004) has argued, this type of autoethnography is too complex to be considered a research method by our regular social scientific standards—it is positioned in an unidentifiable yet distinguishable space, somewhere *between* the academic journal and the novel. Now I'm not knocking this approach; I think it's fantastic. But, it wasn't for me. I wanted my work to be about more than just me; *analytic* autoethnography made that possible. Criminological autoethnography is about more than just the autoethnographer. It's about the social as much as it is the self.

To demonstrate the distinction between analytic and evocative autoethnography, Anderson (2006) notes that the goal of an analytic autoethnography is to go beyond that of its evocative counterpart. The rejection of objectivity is retained, the reliance upon subjective meaning and interpretation is too, but the rationale changes slightly. The focus is now upon using autoethnography to critically dissect the social world. As I've argued before, in analytic autoethnography:

> [T]here is a greater commitment to the critical and analytical spirit of realist ethnography. That is, there is an imperative placed upon conducting autoethnography as an analytical strategy. The goal is not *just* to capture emotional and evocative content but rather to develop a broad critical analysis of any given social phenomenon through it. In this respect, analytic autoethnography—or what I term here 'biographically-attuned autoethnography'—is not so much a method of self-investigation, but a technique of *social* investigation conducted through the *self*.
> (Wakeman, 2014, p. 708 original emphases)

I think this negates the accusations of self-absorption and navel-gazing. I'm not doing research like this for these reasons—I'm doing it to better understand the sociological issues that I study. And I think in this respect, a truly analytic autoethnography is a unique and valuable tool for the criminologist to possess.

So in answer to your question then, what is autoethnography? Well, it's complex, but in its purest form, it's a way of doing research that puts the researcher at the centre of the process, alongside the participants and their experiences. I think of it now more in terms of a sensibility than an actual approach or method. It's a way of doing your research that requires you to think about who you are and how you came to be that way in the context of what this can bring to the research project.

> Ok, that kind of makes more sense now. It's just ethnography, but I incorporate myself into the analysis, yes? I just do my research the way I was planning to do it all along, but now I think a little bit more about who I am, and how this is going to impact upon what I learn in the research, yes? That sounds achievable to me.

Question Two: *How* Does It Work?

> So I think I'm with you now—I think I get what autoethnography is. But, my question from above still needs answering. How do I do it exactly? It's alright just saying I need to think about myself and my emotions/biography a little more, but what does that actually look like in practice? How do I do autoethnography?

Again, this is a good question. And again, it's hard to answer in a simple form. The truth is, there is no one way of doing autoethnography in a simple, linear manner. The method is too complex for that. In this respect, there's something useful to be gained from thinking about it as existing somewhere above and beyond methodology in a kind of transcendental state; autoethnography is more than just a research method; it is perhaps best thought of more along the lines of a sensibility than it is a method/methodology. What I mean by this is it can be achieved by adapting ones thinking slightly more towards their own role in the collection, production, and

distribution of the knowledge that is gleaned from research. Whether this is from surveys, interviews, observations, or any other technique, the autoethnographic sensibility idea holds. Having used this model myself in my own work, I'm convinced that this is true and that more people could do it if they tried. That said, I understand how a lack of clear-cut guidance can prove daunting, especially for graduate students and people at the earlier stages of their careers. So, with this in mind, I've always found the previously alluded to ideas of Anderson (2006) to be useful.

Anderson (2006) offers five key principles of analytic autoethnography, and my feeling is that these can help the willing researcher to piece together a plan of action for doing autoethnographic criminology. They are as follows:

1. Complete member researcher.
2. Analytic reflexivity.
3. Visibility of researcher in the text.
4. Dialogue beyond the self.
5. A commitment to an analytical agenda.

I think running through these in detail is worthwhile. To begin with, complete member researcher just means that, either now or in the past, the autoethnographer needs to be a member of the group studied. Obviously, there are some limits to the degree of membership required, but it does hold that autoethnographers need to be involved in the worlds that they study. If, for example, you're studying skydiving, then you need to jump out of some airplanes. This is achievable (potentially at least) for most people. Criminologists, however, have a discipline-specific issue here in that they may be unable legally to partake (and/or unwilling, ethically/morally too). For example, I'm able to research drug addiction using autoethnography as I have a history of addiction; this earns me my 'complete member' status. However it need not preclude anyone who doesn't have this history from doing the same—it just means that they would need to conduct their research in very close ethnographic proximity to active users and then practice the principles of reflexivity outlined below in relation to themselves. In short, you don't need to actually *do* something to write about it autoethnographically, but you do need to be very close to it as it happens and to document what that feels like to you.

> I'm glad you've said this—it was one of the things putting me off the method. So if I understand you properly then I don't need to actually do something to be an autoethnographer, I just need to be close enough to it to have some sort of membership status?

That's exactly right. This opens up the door to a whole range of possible applications—all the intersections of our identities and life experiences provide us with potential autoethnographic tools. All we need to do is embrace them and then work with the remaining four principles from above.

The second of these is 'analytic reflexivity'. Here Anderson talks of 'self-conscious introspection' (2006, p. 382), and I think this is a useful starting point. What this means is the autoethnographer turns her eye inwards as much as casts it outwards. Therefore, while I'm in the field, I'm asking questions of myself as much as I am of that which is occurring around me. Why is it, for example, that some of

the heroin users I met in the field scared me? Why did I feel pity and empathy for others? There's a story I often tell about the discomfort I felt watching a woman inject heroin and crack into a vein in the side of her neck. She did it in a mirror in the exact same way she had been doing her hair and makeup just a few minutes previously. The discomfort here was grounded in the ways in which the juxtaposition of the two images ruptured *my* understandings of femininity. This is important, crucial I would say, to forming a properly critical analysis of what I was seeing. My reactions matter just as much as what was visible to the outside world. Thus, to practice autoethnography, I must monitor them and record them as and when I can. This is linked to the third principle—visibility of the researcher in the text—in that I must write about them too. To be blunt, autoethnographers must write about themselves as well as others. I must be a visible presence in my written output. Importantly here though, this must be *emotive* in nature. As I've stressed before (see Wakeman, 2014), foundational ethnographic criminology often contained reference to 'mechanical' selves. That is, a researcher would talk about 'going here', and 'doing xyz', but never really what it felt like. Analytic autoethnography is about the emotive self—it is this self that should be visible in our textual outputs.

The above considered, the fourth principle notes that there must be 'dialogue beyond the self'. Here Anderson (2006) was looking to firmly establish a distinction between evocative and analytic autoethnography, and I think that it is worth reasserting this here: while there must be a focus upon the self in autoethnography, it must go beyond it too. The goal here is to ask questions of the social world through the self, and as such, to do autoethnography, one must *do* some research. It is in fact an imperative of the method—as Jeff Ferrell sensibly counsels: 'first an ethnographer, and only then an autoethnographer' (Ferrell, 2012, p. 219).

> So again if I'm following you, this doesn't actually sound that complex—it's just about doing the research I was planning to do, using the methods that best answer my research question, but thinking and writing about myself and my roles in the evolution of the project as it unfolds?

Yeah, in a nutshell, I'd say that's pretty much it. Nicely put. However, there is one more important principle to think about as well.

The fifth and final principle outlined above is 'a commitment to an analytical agenda'. This, for me, is where it starts to get really good. A key guiding principle of autoethnography ought to be a commitment to critical analysis: a desire to push knowledge forward in new, innovative, and exciting directions. Anderson argued that autoethnography should involve 'a broad set of data-transcending practices that are directed toward theoretical development, refinement, and extension' (2006, p. 387), and in my own work I have been at pains to embrace and extend this line of thought—autoethnography *must* be about critically enhancing knowledge. You see, through this method, there's a real possibility of pushing the boundaries of what we know about our subjects in that it lets us think differently about our data. That is, autoethnography is 'data transcending'—it allows us to go over and above standardised 'as is' readings of the data we collect in the field.

Question Three: *Why* Does This Matter?

> This is all good so far, I'm now kind of confident that I know what autoethnography is, and I'm convinced that I've got everything I need to give it a go. But, my final question has to be around why. I mean, why exactly should I go down this route? What are the concrete advantages of 'being autoethnographic'?

My initial response to this question would be quite simple in that I've just stated it: *this method allows us to transcend our data and develop new understandings of what it might mean.* It allows us to think a little differently about what we see in the field, and in this respect, it can help us learn a little differently too. Thus, it can play a key role in the development of progressive theory in a field like criminology. The best way I've found to convince people of this is to show them how it works in practice. I do this through telling a story from my work on heroin use in a marginalised community in England's North West (see Wakeman, 2016b). Through this research, I got to know an individual called 'Tony'[2] whom I watched get 'clean' and then 'relapse' back into addiction several times while I was in the field. His case is interesting in that I understood it well through my own experiences of trying to get off—or, more accurately put, *stay off*—heroin. That is, if I think about what I saw in the field here from the objective, detached position of a social researcher, it looks quite different to what I saw from the perspective of an ex-heroin addict autoethnographer. Again, the theory that stems from my research looks very different depending upon the extent to which I think about myself in its formation. The following story hopefully exemplifies this.

On the second or third occasion that I meet Tony, we're sat in another user's flat and he's telling me how well he's getting on today. I'd seen him the week before in full on withdrawal, and this is not a nice sight. Withdrawal from heroin is anything but pleasant, and this is especially true for a user like Tony who was taking a lot of heroin every day. He wasn't in a good place. Today however, just over a week later, he seems fine. He tells me earnestly that he's finished with heroin—that he's done with it and won't be getting addicted again. He then somewhat sheepishly slips in that he had a little bit yesterday, and also a little 'taste' the day before too. But, he maintains that, as he's not experiencing withdrawal symptoms on days when he's not had any heroin, he's doing fine. He means this too; he's at great pains to tell me how it's ok to just take a little now and again, that it's perfectly possible to do so without developing a habit. He is very sure of himself here, and he provides a perfectly plausible account. So, if I read it 'as it is', what I see here is Tony controlling his heroin use. I see someone making agentic decisions about when to and when not to use heroin. It follows on from this that my research would underpin and support theory that flows in this directions too. Thus, it would support the 'rational addict' thesis of Becker and Murphy (1988) and the numerous other who have followed them since to document infrequent and controlled heroin use that challenges the common misconception of this drug's capacity to enslave its users and relieve them

---

[2] I call him 'Tony', because this isn't his name.

of choice. This line of theory seems to fall naturally on from the proximity of the researcher to the subject in question. In this instance, I am detached as a researcher. However, if I centralise myself, if I bring my own experiences, thoughts, feelings, and inclinations into the picture—if I practice the principles of analytic autoethnography—it starts to read quite differently.

This is because the whole time I was sat listening to Tony speak that day, I just knew he was lying to me. Not because I'm some kind of super detective, or because he's dishonest or disingenuous, but because of a faint but meaningful look in his eye that I understand because of who I am and what I once was. Our shared history of addiction facilitates an empathic connection between us as he talks—I feel again what I felt years previously when it was me who sat on the sofa telling someone else earnestly how I really wasn't going to use today, how I had it under control. You see the thing is, on the numerous times I told this story, I always believed it, and I always meant it, but at the same time I always knew deep down on the inside that I was going to use. That this thing inside of me that I now call addiction would wake up, take hold of me, and compel me to go and get the drug I wanted so badly. It's a strange space—not wanting to do something as you know the consequences that will entail but concomitantly knowing that there will come a point where you will do it, almost despite yourself. It's unquantifiable, but this doesn't mean it's not real. What really matters here though is this: when I work with this feeling of empathic identification with Tony, the whole scenario reads differently, and then as such, the theory that flows from my research *challenges* rather than supports dominant works such as the rational addict thesis.

What's evident in Tony's story from that day is not his ability to *control* his use but rather his *inability* to control his compulsion to use. These are two fundamentally different things. And again, the only real thing that's separating them is a division in the extent to which the researcher applies their own biographical knowledge to the analysis process. While he was talking that day I just knew he would use, and I have to stress again that I take no pleasure in finding out I was right. I saw him just a few hours after this conversation had taken place waiting next to a phone box for the heroin dealer he had just called. The difference in the way this story can be interpreted is located in the extent to which I allow myself as a researcher, and human being, into my analysis. So hopefully it's clearer now what I mean when I claim autoethnography to have the ability to transcend data? This method allows us to go over and above our data. Recall the line from Anderson (2006, p. 387) cited above, the one that argued autoethnography can facilitate 'theoretical development, refinement, and extension' of our ideas—is this not evident in the above? Through centralising myself and my history of addiction within my research, I am able to push theories of addiction in new and progressive directions. Surely, this ability to reshape and redirect established debates is enough to answer any 'why' questions around the use of autoethnography.

> Ok, you've made your case well here—I'm convinced. But I've got one last question for you… What about the 'why nots', if there are any? I mean, you've talked a lot here about the potentials of autoethnography, what about its pitfalls? Are there any reasons I should avoid it, or anything I should be careful of?

That, again, is a really good question. And in drawing things to a close here, a really important one too I think. While there are ample potentials to be found in the use of autoethnography in criminology, I think it's really important to stress the fact that it's far from perfect at the same time. There are some issues here that need careful consideration. Not least of these is the question of ethics—more specifically, what we could call here the 'ethics of representation'. What I mean by this is that autoethnography ultimately produces some sort of representation of its subject matter, be this a book, an article, a film, poem, or whatever. The point is representing social lives outside of our own is an ethical minefield. When we try to do it *through* ourselves, this becomes even more complex. It's not just about representing peoples' lives honestly and accurately, but it's also about doing so with intellectual conviction too. There's a fine line here between using ourselves and our experiences to guide our analytic investigations and then becoming blinded by our own experiences. I've made this point multiple times and it doesn't always go down well, but I think it's true: suffering a heart attack does not make someone a cardiologist. Just because we have experience of something, that does not automatically equate to expertise. This is something would be autoethnographers ought to think carefully about.

There are, for example, lots of people with histories of drug addiction—their stories are important and we can learn a lot from them. Likewise, there are lots of scholars who have studied addictions—their works are important and we can learn a lot from them too. The point is that both alone are great but also somewhat limited. Autoethnography provides a bridge between the two forms of knowledge that has significant potential, but it must be used carefully as if the balance between the two is off, then these potentials might be lost. This is a matter of ethics as much as it is any sort of practice. The question around the extent to which I bring myself into my academic work is ethical at its core. In his book on Dominican 'stick up kids' (young men who rob drug dealers) in New York's South Bronx, Contreras (2013) talks of what he called his 'representational dilemma'. By this he meant the ethical dilemma posed by using his own knowledge and experience of this type of activity to complement what he was witnessing and hearing about as a researcher. The issue here was that if he revealed too much, he runs the risk of feeding populist discourses around young Black people from poor backgrounds as being violent and criminogenic. Yet if he doesn't reveal himself and his experiences, he loses a unique layer of complexity from his study. This is the core of the balancing act.

In my case, I need to bring myself into my analysis to properly challenge theory and maximise the potentials of analytic autoethnography; yet if I centralise myself too much, I run the risk of pathologising and/or marginalising my participants by presenting myself as *the* magic case through which the proper understanding of addiction is to be reached. It should go without saying that this is not in the spirit of good autoethnography.

> So it sounds like a bit of a struggle then to be honest? Constantly negotiating the extent to which I'm at the core of the study, constantly re-evaluating my position as a researcher in relation to my participants? This isn't going to be easy, is it?

Easy is one thing this won't be, no. I can guarantee you that. But, nothing meaningful ever is, is it? The beauty here is in the complexity; *the potentials will only be realised through the struggle*. If you want easy, then just don't write yourself into your work. Go out, do your research, write it up, publish it, and then get on with the next project. There's nothing wrong with this approach, but it does seem to me to underpin a lot of studies that seemingly just reproduce what we already know about stuff. If you're up for the challenge however, and you're ready to push yourself to see if we can expand what we know, then the autoethnographic route is the one for you.

Towards Some Conclusions

In terms of drawing this conversation to a close, I hope more than anything else that it's been useful to any readers with a curiosity around autoethnography. Moreover, it would be fantastic to think that this brief account may have played a small role in shifting such curiosities into intentions to give the method a go. What I've tried to do here is capture the internal dynamics that underpin good autoethnography and reveal the ways in which they can enhance criminological research. Again, this isn't just about introspection—it's not simply a method of analysing ourselves. It's about addressing our role in the research process, no matter what set of methods we use to answer our research questions. I firmly believe this to be true: there's room to think more about the role of the self in criminological research no matter what form this research takes. There is for my mind, at least, no reason why researchers who write surveys cannot reflect upon their role in that process; the same is true of the researcher who invites participants into university offices to interview them. I'm sure this will not be agreeable to all readers, that there are those who will still view this call as 'unscientific', 'unnecessary', and of being marginal in importance to their quests to influence government policy and practice. Once more, this is ok by me. I'm not here to try and convert people to this method or way of thinking—I simply want to give permission to the already curious to try it, just a little, to see what happens.

The mainstay of the argument here can be summarised as follows: we are complex human beings before we are scholars/researchers. We have multiple factors affecting who we are as people from our past and present circumstances, and these will impact upon how we can do research and the ways in which we can learn from it. In some instances, the intersections of fieldwork, biography, and emotion might reveal new and innovative ways of thinking about things that have criminological significance. This alone must surely validate further consideration of the self in the research process. Hopefully I've shown above how this can work in practice using my experiences of drug addiction to challenge and potentially transcend some established theory in this field. The data-transcending properties of autoethnography are key to its use for me. I hope others will view the potentials of the method in this light and seek to harness them in their research.

As a final note however, I'd like to stress the importance of self-care in all of this too. The ethical issues inherent in this method are multiple and complex, and it has been impossible to address them all in the requisite detail here due to the confines of space. It is worth noting the following paradox of autoethnographic research in closing though—*analysis of the self should not be undertaken alone*. A very wise man I once met asked me an important question while I was in treatment for my addiction: 'why would you want to learn more about yourself' he asked, 'you might not like what you find'. He had a point. Real and honest introspection, the kind that facilitates good autoethnography, can be unsettling. It can be isolating, and it can even be painful. As such, a good support network is an important prerequisite of this type of work. If this is in place however, then there is significant potential to be found in this type of work. In this respect, let's hope that the end of this conversation actually turns out to be the starting point of many more.

References

Anderson, L. (2006). Analytic autoethnography. *Journal of Contemporary Ethnography, 35*(4), 373–395.

Becker, G., & Murphy, K. (1988). A theory of rational addiction. *Journal of Political Economy, 96*(4), 675–700.

Contreras, R. (2013). *The stickup kids: Race, drugs, violence and the American dream*. Berkeley, CA: University of California Press.

Denzin, N. K. (2014). *Interpretive autoethnography* (2nd ed.). London: Sage.

Ellis, C. (2004). *The ethnographic I: A methodological novel about autoethnography*. Walnut Creek, CA: AltaMira Press.

Ferrell, J. (2012). Autoethnography. In D. Gadd, S. Karstedt, & S. F. Messner (Eds.), *The sage handbook of criminological research* (pp. 218–230). London: Sage.

Jewkes, Y. (2011). Autoethnography and emotion as intellectual resources: Doing prison research differently. *Qualitative Inquiry, 18*(1), 63–75.

Lumsden, K., & Winter, A. (Eds.). (2014). *Reflexivity in criminological research: Experiences with the powerful and the powerless*. Basingstoke: Palgrave Macmillan.

Maruna, S., & Matravers, A. (2007). N = 1: Criminology and the person. *Theoretical Criminology, 11*(4), 427–442.

Muncey, T. (2010). *Creating autoethnographies*. London: Sage.

Sparks, R. (2002). Out of the 'digger': The Warrior's honour and the guilty observer. *Ethnography, 3*(4), 556–581.

Wakeman, S. (2014). Fieldwork, biography and emotion: Doing criminological autoethnography. *British Journal of Criminology, 54*(5), 705–721.

Wakeman, S. (2016a). The case for criminological autoethnography. *British Journal of Community Justice, 14*(2), 75–78.

Wakeman, S. (2016b). The moral economy of heroin in 'Austerity Britain'. *Critical Criminology, 24*(3), 363–377.

Young, J. (2011). *The criminological imagination*. Cambridge: Polity.

Making Use of a Biased Eye: Photographic Studies of Neighborhoods

Danielle Wallace

"Put your gloves on!" an older Black man sternly told me as I was walked along 53rd street toward Woodlawn in Chicago. I was having some strong cognitive dissonance. Why was a Black man telling me, a young White woman, to get her gloves on? A few moments ago, this approaching man had put me on alert. American culture tells us that a Black man on the street is someone to be incredibly cautious of. Black men have long been stereotyped as both hostile (Devine, 1989; Devine & Elliot, 1995) and dangerous (Welch, 2007). The perception of Blacks as highly criminogenic is so complete that the race of a criminal does not need to be mentioned for individuals to assume the actor is Black (Welch, 2007). Conversely, as I was experiencing here, when one sees a Black man, many people jump to feeling hesitant, guarded, vulnerable, and fearful. I was certainly not exempt from employing these stereotypes, but after having just been scolded (by what felt like an elder), I was confused.

It was November in Chicago. I had taken off my gloves because I was hot. I had just moved to Chicago from Southern California, and being overly conscious of the cold, I layered up. Walking to campus, however, almost always made me regret my layers. While I would start off cold, three or four blocks in, I started peeling off layers. First the gloves, then the beanie, and, soon, my jacket would be unzipped.

He said again, "Put your gloves on, young lady! It's too cold to not wear gloves. Now smile and get to school." This man just scolded me for not wearing my gloves like my mother would. I put on my gloves, we exchanged pleasantries, and I was on my way.

His kindness to me was a wake-up call. Like many Americans, I had initially viewed him as a threat. Conversely, he saw me as a naïve youngster incapable of taking care of herself in the cold Chicago winter. This juxtaposition—where I perceived him as criminal/disorderly while he saw me as violating the cold weather

D. Wallace (✉)

School of Criminology and Criminal Justice, Arizona State University, Phoenix, AZ, USA
e-mail: danielle.wallace@asu.edu

norms of the city—can provide insight into how structural and cultural processes shape individual crime/disorder perceptions. I don't know who he was, but I was and still am very thankful for him. He broke the universality of the Black male stereotype for me. Not every Black man on the street is a criminal. He showed me it was time to change and let go of stereotypes. I couldn't live in my neighborhood for the next several years always being afraid. I was acting like someone who didn't live in Hyde Park. I saw every flaw, every piece of disorder, present in the neighborhood; I was fearful. In reality, Hyde Park had crime, but I was more at risk for theft on campus from fellow students than I was from any crime in the neighborhood. From then on, I knew I had to learn the street dynamics of my neighborhood and, importantly, learn not to assume.

Why is this story important to explain? While certainly not emblematic of everyone's experiences, my encounter above shows the positionality inherent in perceptions of disorder. Perceptions of disorder are shaped by who you are, your previously experiences (or lack thereof), and the stereotypes you hold. Next, what does this teach us about structural and cultural processes shaping individual crime/disorder perceptions? To facilitate this discussion, let me tell you more about my personal background and how it shaped how I initially approached disorder.

One does not need a survey to figure out whether disorder is in a neighborhood: just look around. There may be trash and rundown, unkempt buildings, people hanging out on street corners, and graffiti. Being in a disorderly neighborhood makes us lock our car doors, hold our bags a little closer, or keep to paths that are well lit. In sum, disorder makes us fearful. But understanding the ways in which people look for, see, and respond to disorder is not as simple as how I portray it above. Disorder perceptions are linked to race and class biases (Sampson & Raudenbush, 1999, 2004; Wallace & Louton, 2018), as well as gender differences (Sampson & Raudenbush, 2004; Wallace, 2015). Objective measurements of disorder are not exempted from these biases either (Hoeben, Steenbeek, & Pauwels, 2016). I had to face a number of my own personal biases to become an established disorder scholar. As scholars, we come at our research questions and methods with a host of opinions, biases, and positions. Luckily, when done carefully, there are ways to use your position in your research, whether it illuminates your questions (though hopefully does not bias hypotheses), helps you "get in," helps construct a survey, or simply allows you to navigate a social world that's not yours.

Home vs. Chicago

In 2002, I moved to Chicago to start a PhD program in Sociology at the University of Chicago. Mid-October, I packed up, threw my best friend, Aaren, in a white rental van, and drove from my home in Southern Orange County, CA, to Chicago, IL. As Aaren and I pulled off I-90 on the 47th Street exit, we sat quietly in the van as nervousness came over us. Aaren was polite; she didn't want to say anything to "make it worse" so she kept quiet. We were scared.

We all know Chicago. It is one of the most studied places in the United States. Chicago itself is a contributor to field-defining literature in sociology and criminology. The scholarship about and in Chicago helped to define how our fields understand poverty, race, disadvantage, segregation, ghettos, gangs, urban life, urban transitions, and countless other concepts. Louis Wirth, Robert Park and Ernest Burgess, Clifford Shaw and Henry McKay, St. Clair Drake and Horace Cayton, Robert Sampson, Gerald Suttles, William Julius Wilson, and Douglass Massey, to name only a scant few—the pivotal works of these great scholars are what we all read, and what we teach our students is the foundation of our fields. So, we, as scholars, know Chicago.

But for those of us who *know* Chicago, who have lived there, worked there, and studied there, you know what I mean when I say Aaren and I were scared. In 2002, the area right around 47th Street and the freeway was desolate and decayed. Few people were on the street, and those people who were on the street were Black men. Most of the shops along 47th Street were boarded, boarded in sheets of plywood that looked like they had been there for years. Litter made its home on the sidewalks, and the trash cans were full. There were only check-cashing shops, a Kenny's Ribs and Chicken, and a corner grocery shop for a few blocks. It was only 10 or 15 blocks, but for me, it felt like miles. As we crossed Cottage Grove Ave, the buildings started to change. We were in Hyde Park, a little island of a diverse, middle-class neighborhood protected by the University of Chicago police force.

"Well, this is nice," I said as we looked for parking. My nervousness had ebbed, but it wasn't gone.

Aaren replied, "I didn't want to say anything, but I'm happy you're not living back there."

We all know what "back there" meant: the ghetto. I began my time in Chicago scared and unsure of how to traverse the city and my neighborhood.

My frame of reference for Chicago and Hyde Park was Orange County, CA, where I had been born and raised. In the 1980s and 1990s, during the bulk of my childhood, Orange County, especially the southern aspects of the county, was still developing. Intertwined with housing developments were rolling hills, big trees, and lots of green. I lived in the back of the county, closest to the mountains, but still a short twenty-minute drive to the ocean. The county was a primarily white area, but to say the county is not diverse is wrong. Plenty of Hispanics and Asians of many nationalities live in Orange County. Just not where I lived. There were a few Hispanics who were employed as day laborers, in service jobs, and as landscapers. A few more ran the local Mexican restaurants we would eat at when hung over. There were a handful of middle- and upper-class minority families: Hispanics, Blacks, Asians, Iranians, and Indians. Many of them were my friends in high school. But my Orange County was relatively free of "scary" minorities: urban Blacks.

As more housing developments came, Orange County, while retaining its beauty, was investing in master-planned communities. The County was already rife with homeowner associations (HOAs); it was near impossible to get a house that was not embedded in one. Master-planned communities are those where one large HOA governs several subdivisions of HOAs underneath them. A master-planned

community could assure that a large swath of a city could retain a designed, even homogenous, look and protect property values through a set of rules and guidelines for what homeowners could do to their property (i.e., no one had to worry that their neighbor would paint their house purple or build an addition). A master-planned community also offered community parks, pools, and walking trails. Sometimes, there even was a lake. Even the plants and vegetation in street medians were managed by the master HOA.

I lived in one such community: Rancho Santa Margarita, CA. In college, while commuting to the University of California, Irvine, from home, I worked at the governing HOA of Rancho Santa Margarita. During the busy months, one of my tasks was to check up on various parks and trails; once a week I would have to visit them, walk them, and write up any work orders needed to keep them looking and functioning up to standards. I spotted important issues, like faulty playground equipment or tripping hazards. These issues arose less frequently. The bulk of my work was addressing minor disorder. I spent countless hours writing work orders for shopping carts brought home from the store to be removed, the removal of "graffiti" (only rarely would we see tagging; often times it was children's doodling on park benches, etc.), and gum to be power washed off the sidewalk. I was responsible for protecting people's housing investment and their public spaces, but more importantly, I maintained the definition of "orderly" in Rancho Santa Margarita.

With this hypersensitive eye, I came to Chicago. Unwittingly, though my job, I was already trained in disorder scholarship, just a different kind than what was occurring in Chicago. The stark differences in the environments brought about curiosity but also a deep unease.

What Changed?

Over a decade ago, Sampson and Raudenbush (2004) demonstrated an association between neighborhood racial composition and perceptions of disorder: large Black populations within a neighborhood were related to higher disorder perceptions among individuals. Sampson and Raudenbush (2004) quantified what Bernard Harcourt (2001) had been saying for some time: you cannot define orderly without defining disorderly, and often this definition includes people. Very simply, due to histories of policing minorities and stereotyping, Black people now represented disorder.

Upon arriving in Chicago, I was the perfect embodiment of this process. It is not that I was explicitly racist, but I was not exempt from implicit bias, the more nefarious type of racism that lives in most, if not all, of us. Hyde Park's primary demographic was African Americans and individuals who were "university-associated." While it took time, the first instance that challenged my perceptions of disorder and how American culture defined it was the man on 53rd Street who scolded me about not wearing gloves.

The other major turning point in my studies came from an interaction with a friend. Jamilyah was the only other criminologist in my cohort. We recognized immediately that we needed to stick together. I loved talking to her on the bus, or in class, or walking home. She was funny and insightful and, like me, loved to think about crime. One day we were talking about fear in the neighborhood. I mentioned my feelings on the topic and that I didn't feel completely safe in the neighborhood. Jamilyah was from South Philadelphia, specifically from a neighborhood with gangs. She was fearful too. Stupidly, I assumed she would be comfortable in Hyde Park; after all she was from a tough neighborhood that was primarily Black. Jamilyah was Black and I made the mistake of thinking she would fit into Hyde Park, that someone who is Black would feel comfortable in any Black neighborhood. Thankfully, I had the insight not to say this to her (though not much made it by her and I'm sure she figured it out and was being polite), but I did ask her why. She told me that she didn't know anyone in the neighborhood. She said, while she grew up in a neighborhood with gangs, she knew everyone, and everyone knew her. People in her old neighborhood would protect her. Moreover, while people were out and about in Hyde Park, no one sat on stoops or just hung out on the stairs of their building. There was no informal monitoring system in Hyde Park: the neighborhood was highly transitory with students moving in and out all the time. In sum, the people of Hyde Park had no investment in Jamilyah or her safety.

Outside of feeling like an idiot, yet again, for making assumptions, during this conversation, something struck me that ultimately became the core of my research agenda. On a survey, Jamilyah and I would score the same on a disorder scale or on fear of crime measures but for completely different reasons. Our motivations for feeling fearful or noting disorder were different, and current forms of measurement could not take that into account. Soon, I was going down a hole with disorder: Do disorder cues vary by neighborhood? Does disorder mean the same thing to all people? If no, then what does it mean? And who does that meaning apply to? Blacks, Whites, women, or men? Do residents see disorder the same way that a neighborhood outsider would? Can disorder perceptions change like mine did when individuals learn their community or gain personal insight?

My Research

Since experiencing the above interactions, my research has surrounded pushing the boundaries of the construct of disorder, particularly how individuals perceive and attach meaning to disorder. Borrowing from my experience when I initially moved to Chicago, I wondered how nonresidents, or neighborhood outsiders, saw disorder in unfamiliar neighborhoods. When faced with little information, what disorder cues do individuals see, and what meaning do they give disorder cues? Sampson and Raudenbush (2004) hint at this as a Bayesian process (i.e., based on their own prior experience) but focus on residents' perceptions of disorder. Neighborhood outsiders are not often considered when studying disorder, but that does not make them less

important. Because disorder shapes individuals' behavior within neighborhoods, understanding outsiders' perspective is important. When fearful, individuals will not partake in neighborhood amenities, like stores and restaurants, and generally limit their time there. Thus, nonresidents have the capability to influence the flow of people, money, and resources into neighborhoods. Their racially encoded disorder perceptions may have the unintended consequence of entrenching neighborhood issues like segregation, concentrated disadvantage, or unemployment that are common in minority neighborhoods (Sampson, 2012).

To understand how outsiders would perceive and ascribe meaning disorder, I designed a study where individuals would report on a neighborhood that is not their own. I embedded three photographs in a survey and then asked respondents to complete the following statement: "Based on this photograph, I get the sense that this neighborhood is…" The open-ended nature of the question enabled respondents to portray how *they* perceived and interpreted the neighborhoods shown in the photos. Together with a team of coders, I began an iterative process of coding to determine themes in the responds. Responses were then sorted into thematic categories (more details on the study and its methods can be found in Wallace & Louton, 2018).

How I chose the photos to be in the study is where my previously "biased eye" would be of great help. Before taking the photos, I needed to think about my respondents and their backgrounds. My respondents were young (late teens and 20s), Internet savvy, college students, from a number of different race and ethnic backgrounds. Arizona State University (ASU), where my respondent pool went to college, is a diverse university, with about 40% of incoming freshmen being minorities. That said, while Phoenix and ASU had some diversity, the physical environment in Phoenix was more like Orange County, CA, with its housing tracts and sprawl than a dense urban environment like Chicago. My suspicion was that my respondents would see an urban environment much like I had when I first arrived in Chicago.

I next needed a good understanding of what disorder cues and interpretations I wanted to elicit from my respondents. While disorder certainly has differential meanings, definitionally, it has been consistent for some time. I knew I wanted to cue for those ideas; indeed, this would be a validity check on the image. Next, I wanted my photos to pick up on the ideas of fear, crime, and an unwillingness to use a neighborhood. I also wanted to hint at the race of a neighborhood without placing people in the photos. This photo was perhaps the easiest to select. I knew what cued my fear previously, and I need to embed decay and some sort of disorder into the photo. I ultimately decided on Fig. 1. Taken on the West side of Chicago, the neighborhood in Fig. 1 was on the edge of Little Village, a primarily Hispanic neighborhood with a moderate crime problem. The crime in Little Village did not dissuade people from traveling there for the great Hispanic restaurants.

As a counterpoint to Fig. 1, I wanted a picture that contained a more traditional downtown scene that included shopping, activity, and people. Figure 2 is a photograph of downtown Seattle during the work week when people and tourists were out and about.

Fig. 1 Photograph of various forms of graffiti, murals, and street art

Finally, I wanted to show my respondents a picture that might speak specifically to their environmental contexts in Phoenix. At the time of the survey, around 2011, the Phoenix Metropolitan area had just gone through the housing crisis and recession. The metro area was particularly hard hit by the housing crisis (Chamberlain, Wallace, Pfeiffer, & Gaub, 2018; Pfeiffer, Wallace, & Chamberlain, 2015), particularly by housing investors (Pfeiffer et al., 2015). Foreclosures and vacancies were a common sight in many neighborhoods, especially those in suburbs and exurbs of Phoenix (Chamberlain et al., 2018; Pfeiffer et al., 2015). Using some of this imagery, I was hoping to tap into my respondents' experiences with foreclosures and vacancy. To do this, with the help of David Schalliol, I used a photograph of an abandoned housing development (see Fig. 3).

The results of the study were shocking to me (see Wallace & Louton, 2018 for more information). While I did a decent job at estimating what some respondents would say about the neighborhoods in the photos, many times, I was completely off base. I did not anticipate the depth and potency of the stereotypes that respondents commented on in Fig. 1. Respondents described the residents of the neighborhood in Fig. 1 as "gun-toting assholes" or "guilt-ridden Hispanics." Nor did I anticipate that Fig. 2 would be seen as "American." Figure 3 had the most perplexing responses, and my team and I started calling it the creative photo. Some respondents made up

Fig. 2 Photograph of Downtown Seattle street scene

Fig. 3 Photograph of abandoned building development

stories around the house in the photo, saying a "creeper" lived there or it was a drop house for "coyotes" (i.e., people engaged in human smuggling across the Mexican-American boarder). The best thing, however, that came out of the responses to the photos was the variability in the interpretations. Sometimes the responses lined up with traditional interpretations of disorder, and, just as often, they did not. Being open to the meanings ascribed to disorder, or even the idea that one did not see disorder in any of the photos shown, allowed me to push the concept of disorder for individuals viewing a neighborhood from outside.

Conclusion

Visual methods are powerful for many types of researchers throughout the social sciences. Visual methods aren't simply a method for those using "mixed methods" but can sure many purposes. Quantitative researchers can employ photographs or videos in surveys or experiments to understand how people respond to a stimulus. Conversely, ethnographers can use photos and videos to understand the space they are studying. Ethnographic topics are limited to people but can encapsulate concepts like spending, consumption, and food. Visual methods are invaluable for added information. For all researchers, though, visual methods tell stories to both our subjects and our audience in ways that even the best writer cannot replicate.

But before considering visual methods, or really any method or question, take to heart the lessons from my examples. How we approach scholarly work is not just about our studies but how we perceive where we are at the time and where we are ideologically. Even if you're data mining, you have to recognize your position. What is in your mind that shapes your questions? What is your position coming into this question? While we may believe it, quantitative methods are not objective when our positions leak into the questions we ask and how we frame our questions and construct our methods. Those positions can have both positive and negative effects on our research. I changed a biased position toward neighborhoods and race into something more open and exploring. I used that change in position to shape my agenda and my methods. I am eternally thankful to the man on 53rd Street and Jamilyah for challenging my perceptions and the bias that laid within them. They didn't know it at the time, but they defined who I am as a scholar.

References

Chamberlain, A. W., Wallace, D., Pfeiffer, D., & Gaub, J. (2018). Housing disinvestment and crime in a phoenix suburb: Exploring the differential effects of investors and owner-occupants. *Urban Affairs Review, 54*(1), 190–224.

Devine, P. G. (1989). Stereotypes and prejudice: Their automatic and controlled components. *Journal of Personality and Social Psychology, 56*(1), 5–18.

Devine, P. G., & Elliot, A. J. (1995). Are racial stereotypes really fading? The Princeton trilogy revisited. *Personality and Social Psychology Bulletin, 21*(11), 1139–1150.

Donohue, J. J., & Levitt, S. D. (2001). The impact of legalized abortion on crime. *The Quarterly Journal of Economics, CXVI*(2), 379–420.

Harcourt, B. E. (2001). Illusion of order: The false promise of broken windows policing. *Harvard University Press*.

Hoeben, E. M., Steenbeek, W., & Pauwels, L. J. R. (2016). Measuring disorder: Observer bias in systematic social observations at streets and neighborhoods. *Journal of Quantitative Criminology*.

Pfeiffer, D., Wallace, D., & Chamberlain, A. (2015). Is investor purchasing of foreclosures related to neighborhood crime? Evidence from a phoenix suburb. *Housing Policy Debate, 25*(1), 67–90.

Sampson, R. J. (2012). *Great American city: Chicago and the enduring neighborhood effect*. Chicago: University of Chicago Press.

Sampson, R. J., & Raudenbush, S. W. (1999). Systematic social observation of public spaces: A new look at disorder in urban neighborhoods. *American Journal of Sociology, 105*(3), 603–651.

Sampson, R. J., & Raudenbush, S. W. (2004). Seeing disorder: Neighborhood stigma and the social construction of "broken windows". *Social Psychology Quarterly, 67*(4), 319–342.

Wallace, D. (2015). A test of the routine activities and neighborhood attachment explanations for bias in disorder perceptions. *Crime & Delinquency, 61*(4), 587–609.

Wallace, D., & Louton, B. (2018). The disorder perceptions of non-residents: a textual analysis of open-ended survey responses to photographic stimuli. *City and Community*.

Welch, K. (2007). Black criminal stereotypes and racial profiling. *Journal of Contemporary Criminal Justice, 23*, 276–288.

Index