

Northwestern Journal of Law & Social Policy

Volume 13

Issue 3 *NORTHWESTERN LAW*

INTERROGATING ETHNOGRAPHY

CONFERENCE

Article 2

Spring 2018

PANEL DISCUSSION: ETHNOGRAPHIC EVIDENCE

Recommended Citation

PANEL DISCUSSION: ETHNOGRAPHIC EVIDENCE, 13 Nw. J. L. & Soc. Pol'y. 138 (2018).

<https://scholarlycommons.law.northwestern.edu/njlsj/vol13/iss3/2>

This Conference Proceeding is brought to you for free and open access by Northwestern University School of Law Scholarly Commons. It has been accepted for inclusion in Northwestern Journal of Law & Social Policy by an authorized editor of Northwestern University School of Law Scholarly Commons.

NORTHWESTERN LAW INTERROGATING ETHNOGRAPHY CONFERENCE

ETHNOGRAPHIC EVIDENCE

Northwestern Pritzker School of Law
East Chicago Avenue
Chicago, Illinois
October 21, 2017, 10:00 a.m.

PRESENT:

PROFESSOR DEBORAH TUERKHEIMER, Presider
PROFESSOR CLAUDIO BENZECRY
PROFESSOR PHILIP COHEN
MR. STEVE MILLS
PROFESSOR CHRISTENA NIPPERT-ENG
PROFESSOR MARY PATTILLO

MR. STEVEN LUBET: So before we begin I want to say what I should have said last night, which is to thank Gary Fine for actually proposing and organizing this conference. It was his idea. The Author Meets Critic is a format that we don't really do in law. Perhaps, Colin, for the better. But I do want to thank Gary.

MR. STEVEN LUBET: Now let me turn the floor over to my excellent and brilliant colleague, Deb Tuerkheimer—whose book about shaken babies, by the way, for those of you who study things like domestic abuse, is the single best book on the subject like that I've ever read.

MS. DEBORAH TUERKHEIMER: Thank you, Steve. It's a pleasure to be here. I'm humbled. I'm not an ethnographer—I do teach evidence—but it was very kind of Steve to invite me to be a part of this. I love the book, and I'm really very interested to hear what you experts have to say about it. So I think my role is basically to keep time and to make sure that everyone gets to say what he or she wants to say. I will briefly introduce the panel, and I will do so in the speaker order so that you know what is ahead.

So we will first hear from Christena Nippert-Eng, a sociologist and professor of informatics at Indiana University Bloomington. Her scholarly interests include cognition, culture, gender, privacy, time, space, everyday life, ethnography, user-centered design, and, most recently, the social behavior of nonhuman animals, especially the rest of the great apes. And I just want to mention her published books—*Home and Work: Negotiating Boundaries Through Everyday Life*, and *Islands of Privacy: Disclosure and Concealment in Everyday Life*. The newest book you should know about, *Watching Closely: A Guide to Ethnographic Observation* (2015, Oxford University Press) is an exercise-based approach to learning the skills of direct observation, featuring the work of former students in design, architecture, and the social sciences.

MS. CHRISTENA NIPPERT-ENG: Thank you very much. Thanks a lot, Gary, for inviting me to be here. And to Steve for the honest pleasure of reading this book. As Gary explained it to me, my job today is to talk about both Steven's book and the question of what should be the standard for ethnographic evidence. Before I do that, however, I want to spend just a little bit of time making sure we're all on the same page, since some of us have not had a chance to read the book yet, and we're a fairly diverse audience today. So I'll start with a sixty-second primer for the benefit of the non-ethnographers in the room.

At its core, ethnography is a methodological toolkit used by a variety of researchers to study populations and processes *in situ*. We do our research in the field. That research centers on three activities: observation, conversation, and participation. These three activities are used in various permutations with various degrees of formality.

There are a variety of people who use ethnographic research in order to do their work. These include anthropologists, sociologists, folklorists, primatologists, and other non-human animal scientists, social workers, designers and architects, to name a few. And ethnographers practice their craft around the world. Academic ethnographers sometimes write up the results of their research and publish them in monographs. These are single-subject books that are themselves called ethnographies. Ethnography can thus refer to both the practices and the product of researchers who do this kind of work.

I should also say that while I am a sociologist and a deeply committed ethnographer, I am not an urban ethnographer, the subject of Steve's book, nor do I write the kinds of classic ethnographies produced by a number of my esteemed colleagues who are here today. Rather, I use and help develop ethnographic methods to explore key cultural concepts, such as home and work, private and public, honesty and deceit. My current work is on everyday camouflage in both the face-to-face and digital realms. I also use ethnographic methods and teach them to others in order to help design new, more useful and sustainable products, experiences, services and policies for both human and non-human animals, and for interactions between living beings, as well as between living beings and objects, now including robots. Some of my best friends and students are urban ethnographers, while others focus on rural professional or hobbist populations, and still others focus on objects and spaces and animal behaviors. Each of us and each type of project has its own special challenges.

Which brings us to the book at hand. As we now know, in *Interrogating Ethnography*, Steven Luby, a professor of law, enters the section of the library dedicated to U.S. urban ethnographic sociology. As both a skeptic and a self-professed fan of this particular shelf of ethnographic work, he embarks on a study in which, "I read and studied over fifty ethnography monographs and a like number of journal articles, and I consulted hundreds more essays, reviews, textbook chapters and other sources. Ultimately my selection was both systematic and serendipitous, wide ranging but not all-inclusive. I followed wherever the research trail led, with occasional excursions away from U.S. cities into other materials that seemed especially interesting." His mission, he writes, is to "assess the use of evidence in ethnography in terms of sources, collections, presentations and dependability by comparing it to the standards that have been developed to determine the reliability of evidence in law practice." This "led me to ask a series of questions. How much have ethnographers tended to rely on rumors or hearsay? How rigorously have they fact checked their sources? Have they ignored or discounted contrary or inconvenient

evidence? Did they accept the word of undependable witnesses? Have they generalized or offered opinions that go beyond their factual support? Did they assume that the criminal law did not apply to their research?"

What follows in the book is, for the most part, a well-reasoned, highly readable overview in which Lubet takes to the next level Mitch Duneier's idea of field workers testing the validity of their work by hypothetically putting themselves on ethnographic trial. In the process, Lubet both teaches us about the nature, uses, and admissibility of evidence in U.S. courtrooms, and also reports the nature and uses and misuses of evidence that he finds in the collection of urban ethnographic work that he read. Lubet winds up saying the kinds of things that ethnographers often say about the people we study. There are insightful things that his subjects may not have noticed about themselves. There are the kinds of things that his subjects might prefer not to hear about themselves. And there are recommendations about the kinds of things his subjects might want to continue thinking about now that his study is over. Steve focuses especially on what he sees as a need for greater accuracy in ethnographers' claims, greater candor in what has and has not been observed firsthand, and greater transparency about the exact nature of our work being as clear as possible about the who, what, where, and when questions.

Overall, I believe the result is a provocative, important, and well-written work. I will be recommending it to all my students in the years to come. There is a little part of me, though, that might want to do that just so I can argue with Steve in front of my class. It turns out that Steve and I have quite a few shared observations and conclusions regarding the best ethnographic evidence. We and our professions are both products of the enlightenment. Yet, my standards are not those of an attorney or a professor of law. My standards are intentionally not those of some of my ethnographic friends either. My standards of practice are those of a scientist. I am, after all, a social scientist. When it comes to evidence, or anything else about what I do as an ethnographer, the standards of science seem to me most appropriate, rather than those of law or journalism or the fine or performing arts. Given what I know about all of these, I now know that my evidentiary standards overlap most with those of law, but they go beyond them too. Like other scientists, my commitment is to systematic observation. My commitment is also to the theories, methods, and bodies of knowledge that emerge from and further my commitment to systematic observation. As a scientist, my commitment is also to peer review. And I expect that my evidence, my conclusions, and the decisions I've made will be second-guessed by others who will help to determine the validity of my work.

In general, my personal desire to embrace the standards of science has focused largely on the same subject matters as Lubet on evidence, including the ways evidence is envisioned, collected, analyzed, and represented to others. In science, we tend to think of evidence as data and that our job certainly includes the mandate to be as precise as we can with and about our data at all stages of the process. Precision and systematicity appeal to me. I am by nature highly analytical and enjoy going deep into hundreds of snippets of conversation or observation data points to find the patterns within them. I enjoy a good mystery, too. And the idea that there is no such thing as inconvenience or anonymous data. If you want to get it right, this is precisely where you need to focus until those inconvenient outliers make sense too. I also enjoy the art of storytelling and the challenge of finding ways to let readers see my thinking while also letting my data be available to them for their own independent analysis.

It's a good thing I like all of this, because science requires it of me. As a practitioner I'm sensitive to how challenging it can be to meet these requirements while conducting and writing about ethnographic research, as well as teaching how to do it. For ethnographers, a move toward science requires that we make constant corrective choices in how we envision our data, perceive our data, record our data, analyze our data, and then represent our data to others. Our ability to be systematic and to be precise can be enabled or disabled at any one of these moments of our process. Without question, as an ethnographer, there are times when I have felt that common practices in ethnography rest in tension with the standards of science. If nothing else, the ways we write up our work may lack the precision that a more scientific approach would require. Our culture overuses the words "many" and "most," for instance, without giving it another thought. Typically, we do this to strengthen an argument. On page fifty-one, Steve writes, for example, "Ethnographers are not the only professionals who must wrestle with the credibility of their sources, but many have expressed reluctance to question their informants." On page twenty-two he says, "Most readers would not consider stolen loan money to be earned." This is totally normal in our culture and totally allowed because Steve is neither a scientist nor an ethnographer, and not my student. Yet reporting many or most of something doesn't cut it in science, especially if you haven't done the representative survey first. My students quickly learn that this must be substantiated with precise data. At the very least, they'd better replace that "many" or "most" with a more modest and correct "some." If they write, "The gorilla hid from view behind the tree," they soon find out that I will be sure to write in the margin, "Implies intent to hide. Do you mean the gorilla went behind the tree hidden from my view? Or do you have evidence that the gorilla meant to hide, as she then started to play peek-a-boo with you or seemed to be trying to break line of sight with someone else in the troop?" Comments off of a homework assignment.

Of course, precision in storytelling and in how one represents one's findings to others matters more if one is adhering to scientific standards, less if one adheres to other standards. Language variability in ethnographic work makes perfect sense to me if only because of the very different traditions that ethnographers draw from. As John Van Maanen puts it, ethnography is "[t]his most humanistic of the social sciences and most scientific of the humanities." Ethnography is a remarkable convergence of approaches and standards. Certainly, because we come to ethnography from different directions and seek different outcomes, it may make it all the more difficult to discern whether, as Lubet puts it, malpractice in our use of evidence is even possible. As a practitioner, I'm deeply sympathetic to the ethnographers whom Lubet calls out for what he sees as—and what sometimes appear to be—less-than-excellent approaches to evidence and evidentiary claims. Yet, I'm also sympathetic to Lubet's demand that all U.S. sociological urban ethnographers take their evidence and uses of it more seriously, perhaps abiding by the systematic standards used in U.S. trial law.

But for those of us calling ourselves social scientists, isn't it most appropriate to hold ourselves to the standards of science? These standards appear to bring Lubet and me to the same place at times. Telling us, for instance, that we should be precise in the claims we make. That if we didn't witness something, we shouldn't pretend we did. That if we witnessed someone saying something, that is what we should claim, nothing more. That we should neither inflate, nor underreport, nor fail to report what we witnessed. That if we

infer that something happened, we must not only say that that is what we did, but we must also provide the grounds on which we did so. Steve and I have no argument on any of this.

Lubet and I also clearly agree, along with a number of you, I know, on the need for ethnography to continue to challenge some of its most fundamental assumptions. What does good data look like? How can we focus more on observation and possibly a bit less on conversation? How can we tell a good story but also an accurate one? How can we make our work available to others so that we can further science more effectively? And how creative can we get in all of this while still being hyper-intentional and systematic and ethical in the process? My goal is to not only encourage my students and myself to do this, but to realize that we're making choices about all of it all the time. The question is: can we be more intentional about those choices, relying less on tradition when that tradition no longer holds water?

And that, I think, is Steve's biggest gift in this book. As I made my way through Lubet's *Interrogating Ethnography*, I kept hearing Steve's own words from the preface echoing in my head. This time, however, I found myself applying them to his work, not Alice Goffman's. "It made a riveting read, but was it valid social science?" And, "Did it embody ethnography at its best?" Using the standards of scientific evidence, we would, of course, have to say no to both. Lubet does not offer us a systematic, scientific study in even the narrow collection of the Dewey Decimal System into which he delved, much less all of ethnography. We are not presented with a powerful, thorough, and meticulous data set. We are not presented with evidence that has been carefully, thoroughly, and meticulously analyzed. Meaning that the complete set of evidence has not been presented to us, along with his conclusions, for our independent review. This is the kind of scientific study that I would really love to see now, thanks to Steve's work. What we have instead is the kind of study that is often associated with the humanities, and indeed with humanistic ethnography. We have a smart professor of law advocating for his view of parts of the ethnographic landscape. Armed with a powerful analytical framework, Lubet applies it to his selective data set. He draws on carefully selected, illuminating examples, all designed to persuade us into accepting his conclusions. Although, I'm sure I would have advised you to kill one particular darling before publication, Steve; that dreaded elevator story.

MR. STEVEN LUBET: Not a chance.

MS. CHRISTENA NIPPERT-ENG: Not a chance. Too darling, right? Too darling. But in the end I must say I'm actually relieved to have Steve's book in hand now. I've been waiting for this level of conversation about our work for some time, and I think Lubet has done us a tremendous service in fostering this conversation in such an original and provocative way. Thank you.

MS. DEBORAH TUERKHEIMER: So let me introduce Claudio Benzecry. He's an associate professor in the Department of Communications Studies here at Northwestern, specializing in the sociology of culture. He is the author of *The Opera Fanatic: Ethnography of an Obsession*, which earned several American Sociological Association awards and was declared one of ten major books in sociological theory by *Contemporary Sociology*. His book *From Head to Toe: Everyday Globalization in a Creative Industry*, is forthcoming from the University of Chicago Press.

MR. CLAUDIO BENZECRY: Thank you. So I wrote this last night just because I thought all the comments by Colin, Shamus, and Mary were interesting and really valuable and good enough that I didn't have to replicate them. So, you know, I hope you can bear with me, and I actually probably am going to use the full minutes. Everybody keeps saying, "I'm going to use the twenty minutes," and they haven't, but I'm actually going to keep to that. Okay?

So the title of my talk is "Evidence of What?" In 1993, Gary Fine published an often-cited article about the lore of being a sociological ethnographer. In it he reflects about some of the inescapable tensions in the process of producing data, the way in which it is presented, validated, and circulated. He called our attention to the inescapable dilemmas of pretending to record reality as it is, while being cognitively limited by our location in time and space; the tension between the construction of an argument and the literary detail in which it is or is not presented; and the problems of data contamination contained in how we reveal ourselves to the subjects, problems of contamination which are at the same time the possibilities for data production. After all, like Mary said yesterday, we are tools. Ethnography is an anxiety-ridden practice in which a lot of the time practitioners do not have a clear idea of what they are going to find, what is relevant about what they found with respect to the case they might end up advancing, and in which we organize all those findings in a narrative way that works to rhetorically produce claims of causality beyond mere description. Even then, we attempt to legislate, regulate how we do this and pose certain demands as to curtail it, though we know the richness that comes from its uncertain, iterative character.

The book for which we have been convoked here today, like many other recent proposals and articles, is a valiant attempt to reduce the problematic complexity of generating valid qualitative data. But also, like a lot of those proposals, it's a recipe that fits mostly with one particular version of what ethnography is—the study of urban groups in delineated settings, recording mostly face-to-face interactions. We can actually ask, through like the meta question: what authorizes regulating ethnography via the very provocative comparison with a trial in court?

Part of the answer would be historical. Legal-judicial-criminological practices and sociology have been intertwined since the beginning of the modern state. If we look at the work, for instance, of Jacques Donzelot in the European context, we see the concerted work of legal and social services in the production of categories of people and the agents that were called to serve them. If we look at the work of Jorge Salessi in the South American context, we can see the formidable contributions of positivist criminology and sociology in fixating what were then identities in flux after the migratory flood of the late 19th and early 20th centuries. Replacing Bertillon's methods, the identification of people via fingerprint meant men could no longer pose for women, poor Italian migrants couldn't perform high culture mores for the camera. The work of containing that proliferation resulted in the production of an evidentiary method to classify the people and, at the same time, manage them.

Even earlier in history, if we look at the 17th and 18th centuries, we can ask what authorizes someone to be an author, and we will find the same role of the law, as that which called attention to the dissemination of texts and in its interest in punishing those producing libelous arguments, at first, and unauthorized copies, later, fixed them under one figure and

one figure only: the name of the author. So the author, a key figure responsible for the collection of evidence today, can actually be traced, as Adrian Johns and Michel Foucault have shown, to the legal work of producing the validity of texts via the attribution of authenticity to one identity.

But there is actually a third entanglement—less at the level of history and more at the level of method—that I think convokes us today. Unlike the Galilean method, with its emphasis on every piece of evidence being directly observable and repeatable, the resolution of crimes and the work of sociology are actually conjectural. Following cultural historian Carlo Ginzburg, what I mean by this is that these are both methods of following and presenting clues. As such, the logic in place is one in which we are not looking for general laws, but rather for how a singularity can be explained. Looking for clues, for traces, becomes the common way to see how it is that a series of surface phenomena authorizes us to produce a deeper reading, far more revelatory than the description of just one detail. Our knowledge as ethnographers is conjectural, then, made out of signs and scraps of evidence. We use an interpretative method taking marginal and irrelevant details as revealing clues for what reality is really about.

Even with these historical and methodological entanglements, I would like to point to quite a few differences, following what Christena said. The first is the question that titles this little talk: “Evidence of What?” The work of the ethnographer in advancing the case is very different from the lawyer in a trial. The latter is focused only on the binary result: is someone guilty beyond reasonable doubt, or not guilty? Ethnography, on the other hand, is interested in producing a partial, fallible, and open-ended account of a particular parcel of reality, be it at the level of individuals, a community, the role of space in organizing sociality, how knowledge is validated by experts versus people, and whatever else you can think of. This difference in objectives organizes a collection and production of stuff as evidence or data in very particular ways. Ethnographers see documents, as Colin discussed for us yesterday, not as the definitive proof of something, but as the inscription, and sometimes the end point, of a collective process, usually attuned to organizational needs and in tension with the formal protocols.

We are also part of the world that we describe. Of course, there are different ways to imagine the tension between observing and participating in a social world, but we can’t escape belonging in one way or another to those worlds we study.

A third distinction is that ambiguity and puzzles are not something to be eliminated in our practice, but rather a central feature of ethnographic knowledge. We play with the ambiguity of social life in generating our explanations for it.

And the fourth, and probably the most important distinction, is that what kind of study object you’re building is something that we learn in the making, which makes it hard to privilege certain kinds of data over others from the get-go and decide what things we are going to foreground and call evidence and which things are going to be backgrounded. This results in different levels of commitment to certain parts of our data depending on how illuminating we think they are when we are deciding, “What is this a case of?”

All this is to say, I can see why it’s tempting to think of ethnography as a particular trial of credibility and truth. But the more I thought of the very provocative idea Steven advances in the book, the more I wondered whether the place we should go to think of ethnography is not the court, but rather the judicial archive. As historians of these archives have shown—and I’m thinking here of Natalie Zemon Davies, Arlette Farge, Carlo

Ginzburg, Lila Caimairi—an archive is a forest in search of some organizational principles that would be able to build it as a corpus of proof. It has several problems that are close to those that the ethnographer gets confronted with in collecting and generating evidence. It's incomplete, yet there is an abundance of detail. We can't work it all. It would be a sea of undifferentiated dates and places, so we end up isolating bits and pieces of the archive to make sense of it. We look for connections and contrasts between different pieces of data that have been collected. We actually build the archive through the accumulation of detail, but also and mainly through the work of classifying, discarding, narrowing down, of identifying what is essential versus what is useful. Much like historians, sometimes we need to trust indirect testimonies of the experience we're interested in recording. But while for historians these issues have led—I think Mary was pointing to this yesterday—to the silencing of marginal, poor, and illiterate populations, for us, the problem is the contrary. It is hard to get access to board meetings, state figures, and so on, and we go on in consequence by what Lubet will characterize as triangulated hearsay.

We also work on building our disparate materials as a relatively organized corpus that we can make sense of. Yet we have here a problem close to the one the historian confronts in the judicial archive. Unlike the pristine, easy-to-decode, absolutely-evocative-beyond-context-and-person character attributed here yesterday to field notes, what we find in notebooks is the lack of punctuation, of intonation, of reference of what they evoke, unless pointed at by the person who wrote it itself. The problem of how to go from the stuff we collect to the production data as evidence for an argument does not stop here. We need to write it somewhere and provide an account of it to our colleagues, and sometimes to an extended audience.

Unlike the court transcript or the scientific report, qualitative sociology has other issues to think about. As we know, thanks to Wolf Lepenies, sociology emerged as third term mediating the arts and sciences, and is positioned within a disciplinary space between the so-called two cultures. To establish its relative autonomy and to justify its scholarly status, it has been subjected to a double ordeal. It must pass the test of epistemological validity dictated by the empirical sciences, while also undergoing the trial of aesthetic appeal typical of the arts and humanities. To translate evidence into writing is, to a certain extent, to let some of the evidence go. We are not writing to exhibit some extraordinary piece of data, like the elevator story, but rather to justify our excursion by explaining what we learned and how we can communicate this to others. After the heroic moment of accumulation comes the ascetic and sober exercise of renouncing and winding down the data that we have. Being an author is something authorized by the data, of course, but it also means standing somewhere choosing which avenues to take and which ones to discard. So our last question is: how do we narrow it down?

The first answer to that question, being a Jewish kid from Argentina, is, of course, another question: what are you trying to know? The second answer, and related one, as advocated by my former colleague and friend, Andrew Deener, is to explain not the process of empirical inclusion—like we see in all of the methodology and appendixes that we write where we read the heroic character of the incredible amount of fieldwork we've done, resulting in thousands of pages of notes and transcriptions—as the only way to demonstrate the reliability of our data, but rather to focus on the process of narrowing down the case. If we were to do that, we would be able to better understand the research process not as an individual practice of the cowboy that Gary referred to yesterday, but actually as a

collective and institutional project that involves colleagues, students, reviewers, mentors, editors, and whatnot. And I want to quote Andrew verbatim here:

How researchers learn to exclude certain subjects, points of data, and alternative analytic themes is difficult to recount and assess, but it is of equal importance to constructing cases. Empirical errors occur when ethnographers misstate or misidentify basic facts as they relate to subjects, situations, events and locations. Empirical errors are different than the observational and interpretive omissions necessary to narrow down and hold constant the units and levels of analysis. Ethnographers should be very concerned about getting the facts right, but they should be equally concerned about getting the case right.

The advantage of ethnography might be precisely its incomplete, limited character. An admission of impossible closure in the business of producing a description about otherness in social life. And this happens by design. We don't record the totality of social life in a particular place. We organize a reality that is multiform, complex, and contradictory according to, and as such limited by, the questions we want to answer. It is this limitation, the fact that we are limited by theory, language, and selfhood, that actually allows us to produce the kind of knowledge we are presenting to each other. Learning to live, then, with a good enough ethnography, to quote anthropologist Nancy Scheper-Hughes, becomes a phrase that refers not just to the ethical standards of qualitative research like she wanted, but about the possibilities of knowledge and communication at large. Thank you.

MS. DEBORAH TUERKHEIMER: Our third panelist is Steve Mills, deputy editor at ProPublica Illinois. Steve came to ProPublica from the *Chicago Tribune*, where he spent twenty-three years as an investigative reporter and editor. At the *Tribune* his reporting largely focused on exposing flaws in the criminal justice system, from inequities in the death penalty to false confessions, and crime lab failures. This work contributed to several sweeping reforms, including Illinois Governor George Ryan's decision in 2000 to declare a moratorium on executions and helped free numerous innocent people from prison. His 2006 co-written series on the wrongful conviction and execution of Carlos De Luna became the subject of a 2008 documentary, *At the Death House Door*. Steve.

MR. STEVE MILLS: Thank you. I've got a confession to make, and my guess is that I'm the only person here who will make it. Before I got this invitation I had never heard of the word "ethnography." I didn't know what it was or is. And since I was invited, I've been trying to figure out how a journalist fits into this discussion and what my role might be. So I've been thinking about what ethnographers do and about what journalists do and whether there is any relationship. From what I can tell, we're sort of cousins. Although, I don't know that a journalist ever gets the luxury of sort of six years to work on a story. So in looking at that question I was trying to figure out where the differences are, where the similarities are. And looking at *On the Run* in particular, and how Steve looked at that book, I saw some flaws in what we do and flaws in what ethnographers do, and perhaps

some ways where we can come together and try to form a closer relationship where values might be shared.

In journalism, we are often writing stories in very short periods of time, taking very precise chunks of events and trying to make sense of them. We do much of that based on people we talk with, what we observe ourselves, documents we can gather that might help illuminate what might have happened. In some ways, unlike what ethnography is going through, journalism is very much under attack, and our methods and our standards are being looked at very closely. So when we work on a story, or any journalist works on a story, we first try to determine what that story might be. Is it about the flaws—in my case, the flaws in the criminal justice system, the death penalty system. When my colleagues and I did that in 1999, we decided that the best approach was to look at every case in Illinois. So that meant 275 cases in which somebody had been sentenced to death. To be comprehensive and to guard against attacks on our work, we felt we needed to pull apart every single case, read transcripts of every trial, look at the evidence in every case, follow the appeals process to know exactly what course the case had taken before somebody wound up on death row and, in about a dozen cases, was executed. In doing that we needed to get as close to those cases as we could. And to do that we singled out a handful or so that we thought best represented some of the issues with Illinois's death penalty.

If any of you are familiar with that work, or the history of the death penalty in this state, you know that the use of jailhouse snitches was a particular problem. The poor performance by some defense lawyers was a problem. The use of all-white juries to convict African American defendants was a big problem. But in doing all of that work and getting to the source of material, the trial transcripts, finding witnesses who had testified perhaps twenty or twenty-five years ago, finding witnesses who had not testified twenty or twenty-five years ago but perhaps should have, I think we did work that is not unlike what ethnographers do. We tried to recreate moments that, in this case, led to convictions.

Were there flaws in our work? Perhaps. Our work by any measure was somewhat subjective. We were able to very precisely define the number of, say, all-white juries that convicted black defendants. The number was I think a couple dozen. Or the number of cases where a jailhouse snitch had testified. I think it was several dozen. At the same time, we made subjective choices. And in doing so, we probably brought some of our own biases or prejudices or suspicions about the system to our work. I think that an ethnographer must do the same thing, and perhaps does it in ways that I find a little troubling.

Take the use of anonymous sources. I understand how it is necessary to the work, particularly when you're trying to get people to discuss very troubling aspects of their lives. At the same time, I try to stay as far away from using anonymous sources as I can. They're needed, but I think you need to look at them very skeptically. I know that I do when I read a story that talks of some person that is having some personal trouble, let's say somebody discussing their alcohol abuse or drug abuse, and they're telling their whole story but their name is disguised. Immediately a red flag goes up for me. So looking at the work in *On the Run*, I had the same issues. How do you believe this work when so much of it is disguised? I understand the need to do that. But it's just something that journalists would never do. Everybody, for instance, in that series on the death penalty was identified. Everything was backed by a document. It helped make that work as sound as it could be, I think.

That said, I think we miss things in journalism. By not being able to do that kind of broad work, we miss telling complete stories. There are a lot of stories now about—you

know, everybody is—reporters are going out into the field and they’re writing about how people feel about voting for Trump. It is work that is highly subjective. You’re selecting people who you think might be honest. Let’s say you go out and talk to a dozen people, and then you quote four of them. How did you decide which four to quote? Are they ones that are furthering your agenda in some way? It’s very difficult work if you’re trying to capture a very, very broad feeling—how do voters who voted for Trump feel about the way they cast their ballots? Yet, it’s so subjective and it’s hard to—it’s hard to find some real truth in stories like that sometimes.

So in looking at that in *On the Run* and looking at Steve’s work, which I found fascinating, I also looked for the places where he might have fallen down a little bit or where he made assumptions that journalists sometimes make as well. Looking at the story about the police officers going to the hospital and looking at visitor logs to determine where they might find suspects sounded entirely plausible to me. We know now that immigration agents are going to courthouses routinely and looking at the big sheets on the walls of dockets for who is going to be in court that day, looking for people who might be in the country illegally. And I know that, and in some ways I thought, frankly, that the police officers going to the hospital seemed entirely clever. So I found that anecdote in the story very illuminating because it cuts so many different ways. And Alice Goffman said she was there. Steve talked to, I think, defense lawyers and prosecutors who said that would maybe never happen. If I were reporting that, I think I would have probably gone to Philadelphia and tried to figure out—maybe gone to a hospital and talked to as many people as I could. I would have tried to get right to the root of what happened, and I don’t know that I would have ever figured it out. It seemed like a wash to me in some ways. So I still, having read *On the Run*, having read Steve’s book, I’m still not sure what to make of journalism’s role in all of this. If there is a connection, I would love to hear about it, and find ways to improve journalism if we can, and at the same time, improve ethnography. I think Steve’s book, that’s the aim of it, and I think that is to be lauded. Thank you.

MS. DEBORAH TUERKHEIMER: Okay. So Mary Pattillo and Philip Cohen have kindly agreed to respond, and then we will hear from the audience and take questions. I guess I want to take the opportunity just briefly to say a few words about the timeliness of the book and of this conversation. I want to maybe situate Steve’s work in the context of several fairly recent and ongoing developments that have been alluded to even this morning. So, first, I think we’re seeing a widespread movement toward evidence-based disciplines outside of ethnography. So evidence-based medicine; evidence-based convictions, right, in some sense; evidence-based journalism, greater scrutiny on journalistic methods; evidenced-based social psychology and a greater insistence on replication studies. All in some sort of an effort to divine a ground truth, but greater pressure on this notion of what the evidence can establish. So that’s going on even outside of ethnography. Second, and Steve alludes to this in the book, it’s become increasingly clear that we live in a world of alternative facts. So in the lead-up to the presidential election and its aftermath there is a sense of facts and its being highly contested, so the stakes might feel even higher. And then last, and I think complicating this effort, is that there has been growing awareness of the prevalence of cognitive biases that we all share. Ways in which these biases influence all that we do. And so you kind of put all of this together in the mix and it feels so important that we’re sort of shining a light in all different places and that we’re trying to figure out

exactly what is going to lead to disciplines—well, to the greatest integrity and, frankly, to the results that we’re all—I think that we’re sort of converging on some sense of agreement as to what, at least, these goals are. Although I think that you’ve complicated that for me a little bit this morning too, which is fabulous. So with that, thank you, Steve, for writing the book and for provoking I think really important thought and really important conversations. And I guess I’ll ask Mary and Philip if you want to say a few brief words, and then we’ll call on the audience.

MS. MARY PATTILLO: Sure. I do hope to be brief because I know many in the audience have lots of expertise. So just two points, in fact. The first is to just build a little bit off of what Christena said about the ethnographic method, and to elaborate on the idea that we start with data and then we rigorously analyze those data, which I don’t think we’ve talked about—kind of your definition of ethnography. So we have lots of data, which importantly on this question of sampling, for example, we try not to sample in the field. We try to write down absolutely everything so we have a full universe of information. And then we reread these field notes and we code them to get themes. And then that’s where our arguments come from. So I kind of made this point yesterday when I talked about inductive reasoning. And it’s important, I think, to talk about that coding process as part of the stuff that Claudio said about how we get to an argument, this kind of whittling down, which comes from, hopefully, a scientific, systematic process of coding, of rereading, of writing memos, of going back to the data and looking for facts that don’t jibe, and figuring out why—all in this kind of recursive process that gets to the argument and that also, then, leads us to perhaps leave out some of the other data because you can’t include all of those data because now those aren’t as relevant for what has bubbled up as an argument. So I just wanted to elaborate a little bit more on that. And then the second point is that—Steve, your point actually makes me think of this finding recently that we in Chicago always talk about New York like, “Oh, Chicago is really better than New York.” You ask New Yorkers, and New Yorkers are not thinking about Chicago at all. So your point makes me think of that. We ethnographers are always saying we’re not journalists, and journalists ain’t thinking about us ethnographers at all. For everybody in ethnography that is so scary. We think about you all the time.

MR. STEVE MILLS: We are now.

MS. MARY PATTILLO: Okay. So we’re thinking about journalists, we’re thinking about books like Alex Kotlowitz’s *There Are No Children Here*. Because you mentioned, you know, who all really has the opportunity to spend six years in places. Well, there are, obviously, books by journalists who do spend six years in places, and they outsell us and we use them in our classrooms, and we’re jealous, and so on and so forth. So that’s why journalists are relevant for this discussion of ethnography. But I will actually pose a question to you, because you were wondering kind of how this conversation could be helpful. One of the things that I’m very interested in is this question of anonymity. Because you all do seem to be able to spend six years with people who still let you use their names, and everybody else’s names and so on, and we seem to think that people won’t let us spend six years with them if we tell them we’re going to use their names. So I wonder about how you kind of broach that, how you negotiate that. But I also wonder if there are things that

you haven't been able to do, stories you haven't been able to break, investigations you haven't been able to publish, those kinds of things, because you wouldn't offer people anonymity because it's against the standards of journalism.

MR. STEVE MILLS: The way we do those stories is we probably spend most of the six years finding the people who will let us use their names. You go through many, many people to get somebody who will do that. You stake out territory and you ask and you ask and you ask. And you let people get to know you well enough that they trust that you'll tell their story accurately, fairly and honestly. I've done stories sort of like that. I spent time with a repo man out stealing cars, and it took a while to find somebody who would let me spend that time with them. I've done other stories like that where it's just a matter of finding the person who will let you do that, and spending time with your notebook in your pocket and your pen in your pocket where you're just getting to know somebody to where they trust you. I hate it.

MS. CHRISTENA NIPPERT-ENG: I'm sorry. So that story, when you write it, is actually fully informed by all the time that you've spent with those people who would not let you use their names, but you quote the one or two guys who would let you.

MR. STEVE MILLS: The story would be—yes, that's information that you take in, it informs how you view whatever world you're looking at. But your story is focused on the person who will let you. You know, when I see those stories where somebody is not named, I immediately am skeptical. But there is a real need for it. And if I see a story that says, you know, according to thirteen or fourteen people in the White House this is what is happening, I trust that. In part because I know the reporters, I know the standards at the *New York Times* or ProPublica or the *Washington Post*, and I know that you can't get that story into the paper until your editors know who the sources are, that there is more than one person who is saying the same thing. We don't write stories based on just one person who is anonymous for that very reason. At the same time, if you were going to do a story about an entire community, it's going to be hard to get everybody to go on the record. That's what we try to do, and it sometimes takes a long, long time to find those people who will do that. You ask a lot of people, you get a lot of no's, and you just keep moving along until you find somebody who will say yes.

MS. MARY PATTILLO: You don't know how valuable you've been on this panel just with that statement. Well, I think we can have a discussion about it later, but I think it is incredibly telling that then ethnographers get to tell all the stories that you all aren't telling because we promise people anonymity. And that's one point. But then the second point I'd say is the ultimate person whose story journalists are telling, what are their interests? Why are they willing to use their name? How then—the word that is coming to mind is how freakish are they that they are going to use their name and so many other people won't?

MR. STEVE MILLS: Everybody does have some interest. But in Alex's book, for instance, Alex knew those people. He spent a lot of time there. He knew them. He got to know them really, really well. That's how that happened. They trusted him. They trusted—

MS. MARY PATTILLO: Right.

MR. STEVE MILLS: And when I've done stories like that it's because people—I got to know the people. I still am in touch with them. I got a phone call from a guy I wrote about almost twenty years ago. You don't do—you try not to do hit-and-run journalism where you go and tell a story and then you disappear. It's a relationship.

MS. CHRISTENA NIPPERT-ENG: I do want to follow-up for just one second on that. You know, in terms of overlap, then, I mean one of the things we're seeing, and this is an argument that Colin has made, and what Mary did, we found out in her book last night this is what she was referring to. That you can actually—and that there might be people who would be very happy to have their names in what you do, and other people who want to remain with a pseudonym. So maybe what that does is it blows the traditional standard of assuming and putting in your IRB that, you know, everybody will be anonymized and this is how I'm going to do it. Maybe that pushes us in a more highly accountable and interesting space.

MR. STEVE MILLS: One of the best stories the *Tribune* has run over the last ten years and one of my favorites is a story by a very good friend of mine named Barbara Brotman. She wrote of a man in hospice care and basically followed his last, I don't know, six months. As intimate a setting as you can imagine, someone dying. And it took her a while to find somebody who was willing to let her be there for all of that. So it took—as journalists we hear “no” a lot. And you just have to be willing to walk past it until you get to “yes.”

MS. DEBORAH TUERKHEIMER: So just briefly, Philip, and then Gary, I do see your hands up. Just a couple minutes to comment and then to the audience.

MR. PHILIP COHEN: Okay. I just want to pull that question of the anonymity back to Claudio's question about science a little bit. This is sort of a generalization, but every time you get close to pinning an ethnographer down on this question, they start to get all philosophical about what science is.

MR. CLAUDIO BENZECRY: I think it's very empirical. Science doesn't have almost anything to do with the scientific method as we talk about it. If you—I mean, there is a whole section of the social sciences that is called social studies or science of technology that shows you that the work of everyday scientists is absolutely unrelated to the way in which we purpose it to be. So it's not a philosophical question. If you have friends who are biologists who work at NIH, who work for pharmacies, big corporations, the way they work doesn't have anything to do with the textbooks that we read about, as in positive science. So then the question becomes why are we subject to standards that essentially are not the standards—especially not the standards of how laboratories operate. So to me that's not a philosophical question, it's a very empirical one. Where is this science that we keep alluding to? Because it's not in the physical sciences, it's not in the natural sciences, it's not in the applied sciences, and it's not in medicine. So who does it? And I'm being super serious about it. So it's not a philosophical problem, it's an empirical problem. And if you

try to find it, you're not going to find it. So it's fantastic to have that on the horizon of intelligibility, on the horizon of "we should be doing this," but the reality is that nobody does it. So I just want to be very clear about it and less philosophical.

MR. PHILIP COHEN: So rather than arguing about what science is, we'll just agree that science doesn't exist.

MR. CLAUDIO BENZECRY: No, that is—I mean the production of valid scientific knowledge exists, and I think we all agree on that, right? That follows protocols, right? That follows standards for how we actually build our data. And, of course, what's real resists, right? The standards, the routines, the patterns are there. So there is a limit to how much you can pull. Let's say Colin is going to his fracking community. At first he might be a disturbance, a problem being there. After a long time people just do what they always do. You become something—you know, you're still being seen a source of data or as a tool for the production of knowledge in the way Mary described it. You're there. I mean, I work with people that do the development and design of shoes. They don't have time to waste on me. So, yeah, of course at first it's odd. "Oh, what's this guy doing? Why is he hanging out with me?" Then by the third time they have products they need to deliver, right? They have things to do. I'm not contaminating anything because if I would they would get fired. They would get in trouble in their job. And so I try to think, you know, when you're thinking of these questions of validity, contamination and evidence. And to me the questions that are really dancing around this, and I think like in the way Christena refers to this, it has to do with a valid standard for the production of knowledge that we think to be truth. So journalists have particular standards, lawyers have particular standards, and we have particular standards too. Right?

MR. PHILIP COHEN: So if I can just finish my point. I think my question about that goes to the question of verification and the methods for that, and I think this ties into the question of anonymity. And I think that one of the differences between the people who claim they're scientists and ethnography is a standard of verification, at least as a goal. And for whatever reason, historical reason or whatever, that is not a standard that ethnographers aspire to.

MR. CLAUDIO BENZECRY: Well, we know that, right? It's not the same community.

MR. PHILIP COHEN: Hold on a second. Okay? I'll wrap up in a second. So I just think that it's worth—I just think that it's worth pausing on. I think why I'm interested in the difference between journalism on this with the question of naming the sources is there is sort of—you can see the historical path dependence that led to these two practices, two sets of practices. Colin was saying yesterday, and Colin can elaborate on this if he wants, that the practice of masking people in ethnography predates the existence of IRB. It's not about an ethical standard imposed by the bureaucracy, it's a tradition that comes from the practice of ethnographers. I think it draws from the—it's related to the construction of composites and the use of people—of descriptions of people as types rather than as individuals. And so it's a pretty different model from what a lot ethnography is now doing, and definitely from what journalism is doing. And I just would like to have—I mean, we all read the—we all read the work of journalists and accept it with some kind of truth value. Like you

say, if it says thirteen sources in the White House say this. We all agree in ethnography we think we've learned a lot from that also. So I'm really interested in somebody on one side or the other of that convincing me—I don't see a reason, a good reason, why one should have the standard where everything is named and another should have the standard where people are not named. And I'm just—so I would like to hear the theory that says this kind of work cannot—and I think Mary's come close to it by saying you can't build that relationship. But so that would be—that's my question to pose. I'll give one example. One reason they might sell more books about this is because they name people. I'm not sure about that, but it's possible.

MS. MARY PATTILLO: Because they're trained writers.

MR. PHILIP COHEN: No, no. I think it's one thing also that you put it right in that real person. But anyway. One thing—I mean, we got some survey research. And the funny thing about survey research, all the survey responses, of course, are anonymous. So you do a survey, and if an ethnographer talks to fifty people and doesn't name them, that's a big controversy because they're not naming their people. But if you do a survey of fifty-one people, of course you would never name them because it's a survey and you don't name people, you're doing a survey. So I think that's pretty interesting. But there is one example from survey research that goes to the issue of: can you build trust? About sexual orientation. And the conventional wisdom in survey research for a long time was you cannot ask about sexual orientation or everybody will hang up on you, they won't know what you mean, it better be the last question on your survey. That was really the conventional wisdom for a long time. And it just is not true. So maybe the world changed and it's okay to ask, or maybe we were wrong all along and it would have been okay to ask. But maybe it depends on the context, on how the needs of the survey are framed. But you can ask that question in America today. And even though homophobes will have a way to answer it, they're not surprised and they've heard of it and they're ready to answer the question. It really changed things in survey research. So I'll completely stop talking now.

MS. DEBORAH TUERKHEIMER: So I have a long queue, so if we can try to keep the questions short and the responses short as well. Lots of people have lots to say.

MR. GARY ALAN FINE: Steve, let me address the problem that I see in what you've suggested. You suggest that you go out in the field and you're meeting a lot of people and most of them are saying I don't want you to tell my story with my name attached. And finally you find that one person, or two people, who are willing to do that. And then you are making the argument that they are typical. Well, they seem to be atypical.

MR. STEVE MILLS: The question was if it takes a while to find somebody willing to share their story, are they typical or not. I don't know that they're typical or if they're not typical. But they're willing to tell their story. And what you find from everybody else may—in talking to them without using their name may help inform what you ultimately write. They are just that story.

MR. GARY ALAN FINE: But the claim is that the way you get an editor to be persuaded to publish is because somehow that story tells you something about society. That is, that story is typical in some important way.

MR. STEVE MILLS: It may not be typical, but it might still tell you something about society. It might be an outlier, perhaps, and that's how you write the story. You write—hopefully you write what you find. Whether it's typical or not, I don't know. But it hopefully provides some slice of the world we live in, and hopefully tells readers something about the world they live in that perhaps they don't know. But I don't know that it is necessarily typical.

MS. DEBORAH TUERKHEIMER: Emily.

MS. EMILY KADENS: So I want to do I think what Steve said. I'm an historian and a lawyer, and I really kind of want to challenge Claudio on his—I think somewhat simplistic—statement that what the lawyers are doing is not the same, that we just are interested in guilty or not guilty. I'm teaching a seminar this semester in which my students are using the case file from an English case. It's about 550 pages of depositions. And what they're doing, and we're going to ultimately try this case, is exactly what they will be doing as litigators. They are gathering the data, they are evaluating the data. I have to ask them questions that I would ask myself as a historian, and I assume that you ask yourself as ethnographers. Do you want somebody to win when you're reading this or are you being open-minded? What do things mean that you read? Because, of course, the language and the assumptions are different from their language-and-assumption world. What assumptions are you making about somebody's statement? Do you think that they are lying? Do you think they are telling the truth? How do you evaluate that? And then ultimately they have to be able to stand up in front of the finder of fact and tell a story. Having gathered this data, tell the story, which is the process of: I have to account for all the data, what am I going to foreground, what am I going to background. It's not just a simple matter of, oh, they're guilty, they're not guilty, I have to convince someone. And I also simultaneously am being challenged on every single one of my assumptions, on the story that I am telling. So I have to produce a story where I can say, okay, how could somebody see this differently. So I think it is very much what I do as a historian when I write my history. This is what I tell the students. These are exactly the same skills. And it sounds very much like what an ethnographer does, or should be doing, when they are putting their stories together. So I really think that the way Claudio sort of dismissed it is far too simplistic.

MR. CLAUDIO BENZECRY: Thank you. What was your first name again? I'm sorry.

MS. EMILY KADENS: Emily.

MR. CLAUDIO BENZECRY: Emily. Thank you. So let me say two things. And I actually found your comment really, really fascinating. But the objectives are very different. I think that organizing—again, you are speaking as this—this is part of the training, right, and as part of the training people are looking on whether they want someone to win or not. But if

you are trying someone, and if you're defending someone, you have already a predesigned condition about what kind of data you're going to try to prioritize and which ones you're not, right? So that makes for a very different objective than—and I think like there is a small I wouldn't say confusion, but conflation in what ethnographers do and the fact that people like Christena and me don't do anything remotely close to urban ethnography is might be helpful here. We keep talking as if we just go, "There are a bunch of people doing things," and we picture them, right? And that's—you know, but the reality of what we do is very different, and that's why modeling and types and process are very important to us. We're trying to find explanations about particular processes in social life, in villages as people here like to say. Of particular kinds of processes, not just the life of those people there. And that's a huge difference. If that makes sense to you. I understand the part about gathering the data, right? I understand the part about assessing it. And of course I understand the part about persuasion. There is no doubt whatsoever. But what we're trying to do with that data is completely different. And, in fact, the fact that you pointed to an archival case was sort of fascinating to me because I didn't say there is no relationship to judiciary practices, but if anything we look much closer to the person who is doing like a historical study of the archives than to the actual court, lawyer and trial. Those are the—

Ms. EMILY KADENS: But what I think is interesting—I am a historian, that's why I teach this case. But what I think is interesting is how almost exactly parallel what a litigator or a prosecutor does is with what I do. I actually had a litigator come in and talk to my class at the beginning of the semester to make that exact point. And you say that we start out with a position, guilty or not guilty. But if you do that you will not see the evidence in the light you need to see it. You have to start out by saying how objectively can I evaluate this and then build my case. Which I think is exactly the process I do as a historian. It's got to be the process you do as an ethnographer, because ultimately you're telling a story. But if I as a litigator start out and say, okay, I have to make the assumption that my client is guilty or innocent from the beginning—

MR. CLAUDIO BENZECRY: No. Can I quote myself? Because I actually have what I said in front of me. I said, "It's very different to the lawyer in a trial. The latter focused only on the binary result." So I didn't say anything about (inaudible). "Is someone guilty beyond reasonable doubt or not guilty?" That's very different from why we gather data and what we do with it. So I hope you understand it doesn't mean it's a presumption from the get-go, but essentially what is it you're going to be doing with the data and what does that data say about what you're trying to understand. You're trying to understand one action, right? One individual or a series of individuals did this or didn't do that. Is that a correct way of describing or am I making that up?

MR. EMILY KADENS: Ultimately you're telling a story where you have decided that the arc of the narrative is X. Ultimately a litigator is telling the story where they have decided that the arc of the narrative is X.

MS. MARY PATTILLO: But can I ask? If you're hired as the defense attorney and you objectively assess the data and you're, like, this person is guilty as hell, but I'm a defense attorney. So then you're not telling the arc of a story of what you objectively assess, you're

telling—you're defending. You're foregrounding a lot of data that you think are not the data that's most compelling in your reading.

MS. EMILY KADENS: Yes. But there is a step in there, right, where we are still doing the same work.

MS. MARY PATTILLO: Yes. We agree with—I think that's where we all agree. It's then the final telling is where I think is the point that Claudio was making.

MS. DEBORAH TUERKHEIMER: In the back.

UNIDENTIFIED FEMALE SPEAKER: Okay. So I want to talk a little bit about the difference between standards and practice. Not in terms of science, which I think is a really important and well-taken point, but in terms of the law. So I guess my question is given—and it's for the whole panel and for Professor Lubet as well. But given the fact that we see enormous amounts of plea bargaining and lot of explanations for this proliferation of plea bargaining in the complexity of the laws of evidence, why should ethnographers adopt these standards that have been argued as criminal in the various institutions they were designed for, the trial?

MS. DEBORAH TUERKHEIMER: Okay. So the point is that 98%, 95% of cases plead out before trial, so does it really make sense to take the standards of evidence that apply to trial and think more globally about their application to ethnographers?

UNIDENTIFIED FEMALE SPEAKER: Well, they don't work necessarily for trials because we don't see many trials.

MS. DEBORAH TUERKHEIMER: Anyone on the panel?

UNIDENTIFIED MALE SPEAKER: That's a Lubet question.

MR. STEVEN LUBET: Plea bargaining has been analogized to torture. You know, it's a method of extracting admissions from people by threatening them with an even worse punishment. I think, in my opinion, that is a fair description of plea bargaining. Prosecutors say, you know, we'll put you in prison for twenty-eight years unless you plead guilty. It's not that different from the thumb screw. So I think that's an extremely fair criticism. On the other hand, even plea bargains are extracted in the shadow of evidence. So even in the course of plea bargaining there is a weighing of evidence in which each side determines whether the case is stronger or weaker, and from that balance comes the negotiation, and from that negotiation comes the outcome. But as I said yesterday, I'm not proposing that ethnographers mimic trials. I don't think that's—I don't think that would be salutary, I don't think that would be an advantage. I do suggest that ethnographers weigh their evidence more critically and that the adversary process as it once existed or if it were the idealized adversary process—

UNIDENTIFIED FEMALE SPEAKER: Well, that's the thing, it's different now.

MR. STEVEN LUBET: —we imagine it is helpful in the process of evaluating the strength of evidence. So you don't have a plea bargain unless you first determine the strength of your evidence, and that's a way of assessing it. But I think Deb might have something to say about that as an evidence teacher.

MS. DEBORAH TUERKHEIMER: Well, I take the question and I take the answer right, too. The evidence at trial does, you know, bear at least somewhat on the likelihood of a plea. There are many instances that will be a suppression ruling, ruling on a suppression motion before the defendant decides whether it's in his or her interest to plead guilty. And that goes to the point that trial lawyers are thinking about what is going to probably happen in the courtroom if the case were to go forward. It's imperfect, and that to me is the crux of the point that he makes. Shamus.

MR. SHAMUS KHAN: Yes. I just wanted to push on the anonymous and verified sources a little bit. While it's true that we can respect some journalism for the fact that it relies upon the verification of sources, we might remind ourselves of a lot of journalism that relies upon the verification of sources that, say, led us into the Iraq War. So the work of Judith Miller and Michael Gordan, which was verified extensively by the very administration that produced it, and later verified by Chalabi. I mean, these were verified sources. And what it raises is just the point that I was trying to raise yesterday, which I will hit again, which is the relationship between knowledge, verified information, and power, and its institutional location in terms of who is likely to be trusted. This exists both in relationship to things like media, but also in relationship to things like law, which may well explain why it is that we have such systematic disparities in, say, conviction rates by what sociologists would think of as variables deeply associated with power. That is, whose stories are verifiable and trusted; who, in our institutions, is situated so as to be considered legitimate. I mean, these are very, like, important things. And I want to push, especially in the Lubet text, to say that the word "power" doesn't really appear. That the idea that evidence would somehow be located within a matrix of relations where certain things were privileged over others because of the institutional location of a person. We don't hear about this. What we get is a little bit of the view of justice as blind, the view of evidence as kind of blind, and ethnography very much pushes against this. I am also not an urban ethnographer. I have been very critical of urban ethnography, saying it's been writing the same book for sixty years.

MS. MARY PATTILLO: We're replicating.

MR. SHAMUS KHAN: You're replicating. That's a very good way of saying it. But I do think that this is one of the things that urban ethnography is very good at and very good at sort of thinking through how it is that stories from people without institutional locations that allow them to be seen as verifiable can actually be spoken about in academic and public discourse.

MR. PHILIP COHEN: What does "seen as verifiable" mean?

MR. SHAMUS KHAN: Well, I mean, I would say—I'm not really a Foucauldian so it's a little bit hard for me to push the hard Foucault line, but I would say that my institutional location makes the fact that I make a claim more likely to be verified than some other person. And we can, you know, go all the way down with something like that. So if I make a claim to truth, I would opine that that's more likely to be seen as verified than if someone else does who, say, is homeless and lives outside of—the guy who lives outside of my apartment building in New York who lives in an RV.

MR. PHILIP COHEN: I ask that because that's exactly what Steven does in a number of cases. Sort of he takes something told by a powerless person and goes and asks a person in authority if that's true. So I'm just—like Alice Goffman for a second in our conversations. Or he maybe asks five people. I mean, it's not so simple as that, but there is a little bit of that in there.

MS. DEBORAH TUERKHEIMER: Just on the rules of evidence, sort of thinking about whether the framework is useful in this context. What I tell my students is that the rules of evidence construct an alternative reality, right? If this case goes to trial, this is what you need to be thinking about. Even if you never set foot in a trial courtroom, right, you need to understand what would be admissible and what would be excluded. But to the point you make, ultimately it's for a trier of fact to credit or not credit the testimony that comes in. And that's where I think your point about hierarchies, social hierarchies, social structure, powerlessness very much comes into play. It's not just the rules of evidence that decide how the case goes, were it ever to go to trial, right? It's what the jurors think about this witness, about this evidence. And in that regard power of course matters and social position matters, you know, all the way down, all the way down. So trial lawyers know this, and hopefully in the way they are teaching their evidence classes this comes through as well. I think it's a really important point. It's not just the rules that matter, it's what happens once the evidence is admitted, what the trier of fact does with that, and that's where credibility ultimately is not a question for the judge, it's a question for the jury. Tim.

MR. TIMOTHY HALLETT: Okay. So I'm going to start by responding to Phil's comment about ethnographers squirming when we're pinned down on the issue of science. So first it is correct to recognize that there actually is a power dynamic there. In sociology most of the discipline is quantitative. Most quantitative sociologists have a lot riding on the decision-making (inaudible) of science, whether or not it's actually science. They have a lot riding on that. So there is the act of pinning down. So ethnographers resist the act of being pinned down in that way. The other reason that ethnographers resist being pinned down is because we—in ethnography, in the broader discipline of ethnography, we've had serious post-modernist critique of realist ethnographies and we've had serious feminist critique of those sorts of realist approaches. So even the most realist ethnographers among us, Mary, Gary, I would put myself in that same category, what we strive to do is to not represent *the* truth but *a* truth, or many truths, in the context of our projects, without going to the ground truth or truth with a capital T. In doing so what we strive to do is to be multi-vocal in the accounts. To present multiple voices in the accounts. Now, interestingly, what happens is if we follow that strand of what is the one person who will let me say their name, that multi-vocal account becomes an individualist account. And it actually becomes more

subjective; it becomes less intersubjective. So to come back to Steve's point. If the ethnographer collects all of the data on all of those different repo men and women, they can have a multi-vocal story, and they can tell that multi-vocal story if they also include the anonymous voices. But if they only ultimately report on the one voice that's willing to have their name shared with the broader public, that becomes a subjectivist account. It becomes a positional account. And it becomes far less intersubjective or objective with a small O or truth with a small T.

MR. PHILIP COHEN: Could you take that named person—could you spend a bunch of time in the field, map the whole thing out, find the person—obviously I know you did it. Find the person who is willing to be named, get their story on the record, and then say things like Steve would like you to say? I found twenty other people who didn't want to be named but said it works sort of like this for them too. Or I checked the legal records, or something like that. Is there a way to include the unnamed people also?

MR. SHAMUS KHAN: If I could jump in here just a little bit. Colin and I have written about this a little bit. Ethnography doesn't always select as its units of analysis individuals. So that is a survey researcher's approach. Often what we select is situations, which we sample over and over and over and over and over again. And part of the point is that the survey research has really good representative information about lots of people in one situation—which is to say a research situation wherein they're filling out a research instrument. And ethnography tends to gather not lots of people but instead situational context. So, I mean, in some ways you would be asking ethnography to meet a standard which it's actually not really set up to do. And, you know, I could ask the alternate. Like could you give—you know, could the PSID be done in 500 different locations, meaning, you know, like sometimes at a coffee shop, sometimes when people are sitting down at dinner with a family, sometimes in these other contexts, so we know that the results and the responses that individuals are giving are more verifiable to the different context of their life.

MR. TIMOTHY HALLETT: Well, the other response to it is, as Mary said yesterday, most ethnographers already are in effect doing those sorts of things. And so, sure, we could say, "And here is this one person who said that we can share their name." But if we shared that one person's name, it may well lead the Slate reporter to easily find the other people who didn't want us to share their names. It may lead other people in the discipline to say, well, you shared that person's name, why don't you share all these other person's names? So what happens is we can do all these things and we could make our data publicly available to everybody in the discipline if IRB would allow us to do that. Regardless of whether that was initially from ethnographers, it's now into the IRB standards. There are good reasons why IRB wants you to destroy your records after so many years so they can't be subpoenaed and you have to hire a lawyer. But if we were to do those things and to go into the field—my most difficult moment in the field is when I'm doing an interview when I have to be like a lawyer. That's when I have to present them with the informed consent sheet and that's when people get worried or freaked out. My other most difficult moment in the field is when I'm just gaining entry and people say, "Are you a journalist?" And I have to explain, no, I'm not a journalist. I publish all my stuff in obscure academic journals.

Those moments of lawyer-ishness and those moments of journalism-ishness make the relationship very difficult. If I were to go in and they would say, well, “Who else is going to see this?” And I were to say, “Well, anybody who is trained in sociology is going to have access to this data when it is all said and done,” people would walk away. And what it would do is it would create a lot of transparency. But it would tell me as an ethnographer to write my field notes in such a way that I would be protecting my back because somebody is going to be coming in and they may have, you know, different sorts of motives. As opposed to writing my field notes in a way of, “I’m trying to figure out what is this case.” We could do those sorts of things, but in effect they would make ethnography unworkable as a research method. I think in a way I’d be interested to hear from the young scholars here. I’m concerned the implicit message—you know, the goal and the motivation is to make ethnography better, and that is an important goal. But I wonder to what extent the implicit message to young scholars is “This is unworkable. I’m going to do something else. Sign me up for doing interview research, even though talk is cheap. Sign me up to do survey research, even though it’s not science in the way that someone in a lab does something.”

MS. DEBORAH TUERKHEIMER: Does anyone want to speak to the cost of transparency?

MR. STEVEN LUBET: Well, I just want to ask a question. When you say people would walk away, to me that raises the question, “How do you know, since the great practice in ethnography is not to ask?” I know Mitch Duneier was able to get, I think, right, everybody to let them use their real names in *Sidewalk*, and Philippe Bourgois was able to take photographs of drug injectors in *Righteous Dopefield*. So I think there is an empirical question about whether you could get more cooperation than you have gotten just by trying. And I don’t know the answer, but I think it’s an empirical question.

MS. DEBORAH TUERKHEIMER: Is that Dan?

MR. DANIEL MENCHIK: Yes. I do want to speak to this because, you know, we talked about the model of science, the model of ethnography, and a little less about the moral responsibility of ethnographers. We’ve talked a little bit about our responsibility to subjects, maybe making them anonymous, maybe making them non-anonymous, even if they—or making them anonymous even if they don’t want to be. We do have some examples in ethnography where subjects have wanted to be named and they come to regret that down the road. And I wonder if there is a responsibility of ethnographers to look out for them on their behalf? Because just like with journalism, you don’t know what’s going to happen to your account several years down the road, tens of years down the road. And if we’re looking out—because this is going to get used in unintended ways, of course it hurts our subjects, but it also hurts us as ethnographers because our doors could get closed. So the question I guess is if we’re thinking about science as an institution or an enterprise that we’re all a part of, it may be unscientific to use these standards of evidence, masking and such, but it’s more pro-science to actually use these.

MR. TIMOTHY HALLETT: In terms of empirical—in terms of how we know, and it’s sort of an empirical example. So I’m an organizational scholar. So there is demonstrated organizational theory that as organizations, you know, start to try to achieve their goals they get distracted from their goals and they end up doing something else. It’s called goal displacement. And this is exactly what’s happened to IRBs. IRBs were established to protect the ethics. But as Colin has talked about—I mean, providing anonymity isn’t necessarily what your subjects want, it’s not necessarily an ethical sort of thing, but IRBs want to protect their universities from being sued.

MS. DEBORAH TUERKHEIMER: Claudio has some comments, and Christena has some comments as well.

MR. TIMOTHY HALLETT: So that’s how they operate. If we were using organizational theory, it’s a fact of organizational theory that we can anticipate that if a certain standard of transparency were put on to ethnographers it would change how they would do their work and it would change the product of their work and it would result in goal displacement.

MS. DEBORAH TURKHEIMER: Okay. There are a couple of comments from the panel, and then we’ll go back to the audience. Claudio and then Christena.

MR. CLAUDIO BENZECRY: I just wanted to follow up on something Tim said, and then I think a clarification that Dan had made, which is about the moral responsibility. So there are two ways of thinking about this. A lot of time we study populations, like Shamus mentioned, that have relatively low credibility, there is a strong chance—in some cases they want to be named. So you do get to see a lot of ethnographies where a key part of the process of achieving trust and giving back is actually the recognition that you’re giving back to your subjects, that they’re going to be able to tell a particular kind of story. (I’m assuming most people know where Shamus did his book.) On the other hand, most of the cases of studying-up would be absolutely impossible if you were not going to anonymize your sources. So it’s very easy to go to a corner and observe people and say I’m in the corner of X, Y, and Z, then deal with the issue of whether you actually want these people or ask them if they want to be anonymized or not. But let me put it this way: I work in sample rooms and factories in Guangdong, China. So I have a lot of access. But I don’t have access because the companies are giving me access, I have access because I know the technicians, the managers, some of the workers, and they give me access. So if—when the book comes out and people go and check and say, “Who the fuck allowed this person inside this building to begin with?” I’m risking getting everybody on the ground fired, right? People that gave me their trust, the ability to enter, the ability to see the process from the inside. And so all of a sudden we end up with a process in which we just do ethnography on people that cannot defend themselves from our presence. So I think it is problematic to think that anonymity is such a big problem. And, again, I feel something Colin mentioned yesterday implies that, you know, we are hiding ourselves in the anonymity of our sources as a way of saying whatever we want. As if this wasn’t a collective process. As if what I do, colleagues like Gary doesn’t, Colin doesn’t, read or review, maybe without me knowing. As if I don’t circulate that with other people that say this sounds absolutely idiotic

or implausible, or I think I have an alternative explanation for what you have to say. So I feel again, this is something where we end up with (inaudible) our tradition of responsibility for something that goes back to the organizational issues and collecting process.

MS. CHRISTENA NIPPERT-ENG: Dan, I think you put that really well. We continue to struggle all the time, I think any time that we think there is a single correct answer to what we ought to be doing with this. You know, we live in an age of trolls and worse, and I am constantly thinking about protecting people from what their life experience may not prepare them for in the decision to let their identity be known. People who, in fact, if they give you permission for their own identity are talking about their loved ones, are talking about their coworkers who have just been outed as well if you do that. I struggle all the time. I'm doing all this stuff on camouflage now, so, you know, fake news, fake ads, fake people, social bots who are doing all this. Thinking about the range of ways in which people are setting up unwittingly those around them for trouble. I don't know how many parents I know who they start posting stuff about their kids and pictures about their kids from in utero, right? You know, that kid has no choice about their privacy growing up as a child. So I think about those things. On the other hand, I think about quite a few of the elderly who I have interviewed and spent days with who are in their mid-80s, late 80s, some of them in their early 90s, who ask me so sweetly afterwards could I get a copy of the hours of conversation that we just had, I would like to share it with my children. You know, would you name me? It turns out that this is something that—we hope that something about our lives will be documented and will be known when we leave this world. So to say absolutely not, I'm not going to name you, and I'm not going to. So I err on the side of protecting identity, but I would also like to be able to ask. And if it seems that this would not be doing more harm by letting it out, right? There is no one answer for any of this. That's kind of what I like about questioning those basic assumptions that we've been handed down. Does this standard—not the practice of science, right. As organizations, people, we know that the practice, plea bargaining, whatever it happens to be, is totally separate and we're very comfortable with ideal types and thinking about things that don't exist. But do the standards, do the goals, does the orientation and the world view require that we always identify, require that we always mask? Probably not.

MS. DEBORAH TURKHEIMER: Steve Mills.

MR. STEVE MILLS: In journalism I think we probably share some values here in that you wouldn't just go to somebody and ask them to open up and tell their life story without letting them know what they might be in for. And—I mean, I certainly wouldn't do that. And depending on how sophisticated the person was, you wouldn't want to take advantage of them. And I think we try to be very careful, at least we should be very careful about that. The same way that if you wanted somebody, say, to leak you something. The conversation that you might have with a much more sophisticated lawyer or an official as opposed to somebody who is very low on the food chain at some government agency. That person who is far less sophisticated I would have a very different conversation with them, perhaps, than I might with some very top official who knows exactly what they're doing and what the

ramifications might be. I wouldn't want somebody to, say, leak me something or tell me their story without them knowing what might happen afterwards.

MS. DEBORAH TUERKHEIMER: Gary.

MR. GARY ALAN FINE: Yes. I wanted to clarify something that I hear about the IRB, or Institutional Review Board. It is not the case that the IRB demands anonymity for subjects.

MS. ANYA DEGENSHEIN: It is or is not?

MR. GARY ALAN FINE: It is not. No—well, I don't know your particular case. But I served as chair of the Evanston IRB for three years. The rule is informed consent. That if you're going to use someone's name, they have to give you approval with the understanding of what that entails. So, you know, maybe—

MS. MARY PATTILLO: Anya, can you tell us?

MS. ANYA DEGENSHEIN: Yeah. Sure. So I went through the IRB process for my dissertation, and the dissertation entails interviews. And I asked, or I put in my application, I said I would like to make it an option for people to be anonymous or not. And, of course, I would go through all the steps where you have to say what the project is about and say what the potential risks are, but I wanted my informants to be able to opt in or out, especially because I was talking to sophisticated actors, many of whom work in the law and who have a lot of experience going on record. So I thought these are the types of people who would probably make an informed decision about whether or not they would like to be named. And the woman who was in charge of the IRB told me I couldn't. She said the standards of the IRB are such that you must—it is absolutely necessary to protect the identity and the well-being of your informants at all costs. You cannot. So I e-mailed them back, and I cited Jerolmack and Khan. And I said, well, actually, that's interesting, because in my field there are actually arguments for the desirability of allowing people to have their names attached if they so choose. And so, you know, are we saying that my colleagues who are working in this field and who are tenured professors at the most prestigious departments are wrong or are, you know, not doing IRB correctly. And she came back and she said no, that's not the standard of IRB. You cannot do that. You cannot do it. I said. Okay. Well, you know, it's also interesting because one of the members of my dissertation committee, Mary Pattillo, has in her book named individuals. And she said no, the only instance in which someone can use their name is if they are on record, basically. If they're a public figure or a quasi-public figure and on record already with the particular thing you're asking them about. And the reason for that is because then you can't reasonably promise them anonymity. You can still ask them if they would like anonymity, and maybe we should still give that to them, but you can't guarantee it.

MR. GARY ALAN FINE: This is a staff member?

MS. ANYA DEGENSHEIN: A staff member.

MR. GARY ALAN FINE: Okay. So she's wrong in two ways. First of all, the IRB decision making is the faculty committee. The staff doesn't make those decisions. And the second thing is that the argument that you can never use names is straight-out wrong. I mean, it could be—I take your claim, the evidence that you're presenting us as true evidence. But it is a violation of the IRB rule, unless it has changed.

MS. MARY PATTILLO: And it's an illustration of the three things that we—clear things that we have on the table. Science as standards, science as practice; law as standards, law as practice; and now IRB as standards, IRB as practice. Which has led, as a result, ethnographers to often say I'm not going to have that long conversation. I need to get in the field and just promise anonymity upfront.

MR. STEVEN LUBET: So interestingly, that's the subject of the afternoon panel. And lunch is ready.

MS. DEBORAH TUERKHEIMER: Thank you all.