ACT THREE: SCIENCE FANTASTIC

> To render oneself invisible is a very easy matter, [but] it is not altogether permissible, because that by such a means we can annoy our neighbour in his [daily] life… and we can also work an infinitude of evils.
>
> S. Liddell Macgregor Mathers, *The Book of the Sacred Magic of Abramelin the Mage* (1932: 147)

> And perhaps in this is the whole difference; perhaps all the wisdom, and all truth, and all sincerity, are just compressed into that inappreciable moment of time in which we step over the threshold of the invisible.
>
> Joseph Conrad, *Heart of Darkness* (1973: 101)

Scene

Complete darkness. Voices of Dr. Michio Kaku, Dr. Cy N. Salthaway, and Dr. Maura D. Scripshon are heard.[1]

Michio Kaku: Broadcasting live on Talk Radio Network on over 100 commercial radio stations around the country and live streaming on Talk Radio Network.com, this is *Science Fantastic* with Dr. Michio Kaku. As you know, listeners, on this show we talk about "black holes, time travel, higher dimensions, string theory, wormholes, search for extra-terrestrial life, dark matter and dark energy, the future of space travel, genetic engineering, the aging process, the future of medicine, the human body shop, artificial intelligence, the future of computers and robots, as well as topics from science fiction."[2] "When I was a kid, I used to love reading science fiction. I read about telepathy, that is reading minds. I read about telekinesis, moving objects with the

mind. I read about recording memories. I read about photographing dreams. Well, today we can do all of the above."[3]

And, today, listeners, our topic is invisibility. Yes, invisibility! As many of you are probably already aware, social scientists spilled from their offices and gathered on campus quads this week to celebrate the invention of the Fieldwork Invisibility Potion, or FIP. The culmination of decades of top-secret research funded by the Special Operations Branch of the Social Science Directorate of the National Science Foundation, the breakthrough invention of FIP allows for the first time the possibility of ethnographic field research uncontaminated by observer-observed interactions. Variously termed "bias" and "subjectivity" by leading social science practitioners, these forms of contamination have long plagued the quest for a replicable, objective, and systematic ethnographic method.

The project's current principal investigator, Dr. Popper Will Falsify, released a statement that reads, in part: "With FIP's invention, such sources of uncontrolled error in ethnographic method may very well join flat-earth theories, witch burning, and medical bloodletting in the dustbin of prescientific history. The observer effect has been a known feature of theoretical physics and quantum mechanics since Heisenberg first articulated it in the early part of the 20th century, so it's no small irony that it should be the social sciences that have now discovered the key to overcoming the Heisenberg effect in the quest for a truly scientific study of the social world. We believe FIP may very well usher in a new era of comity between the so-called social and natural sciences, one that finally secures social science's place as an equal partner, rather than an envious younger sibling, to the so-called hard sciences."

On our show today, I welcome two leading social scientists, Dr. Cy N. Salthaway and Dr. Maura D. Scripshon, to share their perspectives on the invention of FIP. Professor Salthaway is founder and director of the prestigious Institute for Cumulative Knowledge (ICK) and is widely known across the social sciences as a leading proponent of a more scientific approach to social knowledge. He joins us from ICK's headquarters at Harper University. Professor Scripshon is convener of the Political Ethnography Collective (PEC), an upstart group of social scientists seeking to challenge what they call an unproductive emphasis on scientism in the social sciences. She joins us on the phone from the high plains of Mongolia, where she is currently conducting research on equine–human relationships.

Welcome to *Science Fantastic Live*, Professors.

Dr. Cy N. Salthaway: Thank you, Michio, it's a pleasure to be here.

Dr. Maura D. Scripshon [static on the line]: Yes, thanks for having me on, Michio!

Michio: Before we get started with questions for each of the professors, I want to read for my listeners an excerpt from a recent book by Frederic Schaffer, entitled *Elucidating Social Science Concepts*. As a theoretical physicist, I must

admit to ignorance when it comes to the many differences inside of the social sciences. So, I did a little digging to try to understand one difference that I kept hearing over and over as I talked to my social science friends about the invention of FIP. And that is the difference between positivism on the one hand and interpretivism on the other.

[Dr. Salthaway and Dr. Scripshon chuckle in unison.]

Dr. Scripshon: Yes, that's a big one, all right!

Michio: Well, here is what Professor Frederic Schaffer has to say on the topic:

> *A widely shared methodological commitment of positivism, as I understand it, is a belief that social scientists can directly and neutrally observe a social world that is made up of entities (like families and classes and revolutions) that enjoy, or are treated as if they enjoy, a real existence independent of how people think about them. An interpretivist approach to social science, in contrast, usually starts from the dual premises that there are no "real" social entities, only culturally mediated social facts, and that social science is always perspectival and entwined with the pursuit of moral or material goods. The aim of much interpretivist inquiry, consequently, is to shed light on how shared meanings and their relation to power inform or structure the social world and the study of the social world.*[4]

Professors, how does the quote I just read resonate with your own views?

Dr. Salthaway: As a rough generalization, I think it does a nice job of summing up one of the key differences in how social scientists approach their work. My work, of course, tends to fall within what Schaffer calls a positivist logic of inquiry.

Michio: Professor Scripshon?

Dr. Scripshon: Yes, I agree with Dr. Salthaway that Schaffer's distinction provides a nice working definition. And, as you know, unlike Dr. Salthaway, my own work tends to be identified with an interpretivist approach.

Michio: Wonderful, so I got things right by inviting the two of you to comment on FIP. Dr. Salthaway, let's start with you. Those working within a logic of inquiry informed broadly by positivist commitments seem extremely optimistic about the invention of FIP. What's your take on it?

Dr. Salthaway: Unfortunately, politicians and governments often have a vested interest in portraying certain images of their societies. Ethnography, in-depth immersion, and participant observation are sometimes the only ways of getting a better handle on realities as they actually exist on the ground. But the obvious advantages of immersive fieldwork that gets closer to ground-level facts are diluted if not actually reversed by their "just anecdotal" quality, namely, that because the fieldworker is necessarily only observing interactions highly contingent upon her location in time and space, there is really no way to systematize and generalize the data she collects. Of course, another

important worry when it comes to researchers who rely extensively or exclusively on ethnographic research is that the data they collect are more an artifact of their presence than a reflection of what is actually there. Combined, these concerns make it exceedingly difficult for ethnography to justify itself as sufficiently systematic or replicable to qualify as science.

MICHIO: Could you give any examples?

DR. SALTHAWAY: We recently had a job candidate for a prestigious senior position with ICK. He had invested years to learn Malay and live in a remote Southeast Asian village of approximately seventy families. His job talk offered some remarkably vivid descriptions of pilfering, gossip, and foot-dragging that came out of this fieldwork, but when a respected senior member of our department interrupted the candidate halfway through his talk to ask whether the research amounted to anything more than an anthropological monograph about a specific researcher living in a specific village at a specific historical moment, it really put a damper on things. That, I think, is generally the problem that ethnography suffers from as a method.

MICHIO: So, prior to the invention of FIP, was ethnography really doomed to produce only anecdotal stories that, while interesting and occasionally entertaining, didn't amount to anything resembling science?

DR. SALTHAWAY: Well, Michio, I'm loath to throw the baby out with the bathwater, and it's why I advocate what you might call a three-legged stool approach to the scientific study of politics. Under this approach, ethnography is immensely useful for generating hypotheses, exploring peculiar residuals that appear in statistical analyses, or helping the researcher uncover potential causal mechanisms linking dependent and independent variables.

But, ultimately, to produce what I would consider truly valid scientific knowledge, ethnography must be subsumed within a broader research program in which the other two legs of the stool—statistical and formal analysis—serve to test, and ultimately verify or falsify, the hypotheses and hunches developed by fieldwork.

MICHIO: So has the invention of FIP changed your thinking on the role that ethnography might potentially play in the social sciences?

DR. SALTHAWAY: Well, it's an interesting question. On the one hand, by containing the potential to eliminate entirely the participant in participant observation and produce a pure observer *qua* observer, it does strengthen the capacity of ethnography to be more objective. On the other hand, FIP does not do much for the "just anecdotal" problem insofar as an observer, no matter how invisible, is still only observing highly specific interactions and settings. So, ultimately, I think that even fieldwork conducted using FIP would still need to be combined with statistical and formal analyses.

I suppose you could say that one anxiety I have is that by making ethnography somewhat more rigorous without overcoming all of its limitations, FIP may give the dangerous illusion of strengthening arguments for

the stand-alone value of ethnography in the social sciences. If you'll indulge the extended metaphor, proponents of ethnography's stand-alone value have always seemed to me a bit like creators of one-legged stools. Now, I grant you that one-legged stools might be very aesthetically pleasing, they might make for wonderful conceptual or installation art, and it might even be possible to create an entire tradition or discipline of one-legged stools in which earlier styles are compared with later styles, different types of wood are employed for the stool, different varnishes are put on it, passionate debates erupt over whether this or that wood is more ethical and environmentally sustainable, over whether this or that kind of varnish better respects the underlying grain or "voice" of the wood, and so on and so forth. This kind of thing might continue to the point where these debates replace the actual making of stools as the primary concern of the one-legged stool school or tradition or discipline. But, ultimately, for those concerned with the advancement of *science*, all of this hyper-reflexivity and navel-gazing boils down to the rather straightforward question of whether you would ever want to sit on a one-legged stool. And just as no one would ever want to sit on a one-legged stool, no matter how beautifully crafted, so too would we be better off the sooner we abandon the fantastical notion that stand-alone ethnography, absent a kind of disciplining or stabilization by statistical and formal analysis, can serve to move the project of a scientific study of social relations forward? FIP undoubtedly makes the ethnography leg of the three-legged stool of science stronger, but it does nothing to eliminate the need for the other two legs of the stool.

Michio: It sounds like you're rather less sanguine than Dr. Popper Will Falsify about the implications of FIP for ethnography's capacity to become a true science.

Dr. Salthaway: I have the highest respect for Dr. Popper Will Falsify and what he has accomplished with the invention of FIP, but I just don't think we're quite there yet in terms of a truly scientific, truly replicable, truly systematic ethnographic capacity in the social sciences.

Michio: So, what, in your opinion, would we need in order to get there?

Dr. Salthaway [laughing]: Oh, a time machine, for starters. And a do-over button. And a hermetically sealed social world in which our publications had no chance of being read by those they analyze, since that too might alter their behavior.

Michio: Ah, you laugh, Dr. Salthaway, but "my *New York Times* bestselling book, *Physics Of the Impossible: A Scientific Exploration into the World of Phasers, Force Fields, Teleportation, and Time*, goes not just fifty years into the future, it goes 500 years into the future, when we might have starships, we might have teleportation, we might even have time machines.[5] And in this book I answer the question, according to Einstein's theory: is it possible to go back in time and meet your teenage mother, before you were born, and she falls in love

with you? Well, [laughing] if your teenage mother falls in love with you before you were born, you're in deep doo-doo."[6]

Dr. Salthaway [laughing]: Yes, I suppose if we do one day invent time machines and teleportation, people falling in love with their teenage mothers before they were born will be the least of our problems. Time machines would render our current notion of social sciences completely irrelevant!

Dr. Scripshon: Oh, I wouldn't be so sure about that, Dr. Salthaway! I'm not sure that the realization of any of those current impossibilities would fundamentally alter the core task of social sciences, which is understanding.

Dr. Salthaway: Understanding and not prediction?

Dr. Scripshon: Yes, Dr. Salthaway, understanding and not prediction.

Michio: Ah, thank you for that interjection, Dr. Scripshon! You know, although I am a physicist who writes about the future, and even though I believe "we can predict the evolution of the universe billions of years from now," I can certainly appreciate your reticence when it comes to prediction. "Let me quote from that great philosopher of the Western world, Yogi Berra. Yogi Berra once said, Prediction is awfully hard to do, especially if it's about the future!"[7]

[Laughter from both Dr. Salthaway and Dr. Scripshon.]

Well, Dr. Scripshon, let's now turn to your thoughts on the invention of FIP. You're widely recognized in the social sciences for employing what Frederic Schaffer calls an interpretive approach to social research. What's your take on FIP?

Dr. Scripshon: Like all revolutions in technologies of observation and analysis from the microscope to the telescope to the explication of the bell curve to the development of ordinary least squares analysis to the delineation of fuzzy set analysis, this so-called FIP will no doubt be heralded by many as a breakthrough of magnificent proportions and magical possibilities. Fundamentally, however, I do not think the invention of FIP or any other technology, including, I would add, time travel, obviates some of the basic, unavoidable questions facing the ethnographer and, by extension, all who take the social world as their focus of analysis.

Indeed, a central motivation for organizing the Political Ethnography Collective is the contention that the power of ethnography lies not only or even primarily in its capacity to get closer to the ground, to better "collect data" as if data were like so many rocks lying about in a field, but rather precisely in the way ethnography forces us to confront the question of how we as researchers are implicated in the social worlds we study, to confront the ways we actually co-generate rather than simply collect data, and to confront the ways the knowledge we produce with these data travels back and alters the very social worlds it purports to explain.

Michio: So, if I understand you correctly, you are saying that FIP actually eviscerates ethnography of one of its core strengths?

Dr. Scripshon: Not eviscerates so much as pushes underground. For researchers utilizing an interpretive methodology, the idea of neutrality or objectivity in fieldwork is an illusion because the researcher is always intervening in specific relations and networks of power. Being invisible does not change this. Take, as one example, a researcher who studies social relations of production on a factory floor. Not only *what* but *how* that researcher sees is going to be intimately tied to whether or not she enters the factory as a guest of management or whether she enters as an entry-level line worker, just to contrast two starkly different positional locations the researcher might take. Further, the ethnographer is always situated at the intersection of multiple identities—racial, gender, sexual, class, and so forth—and these impact both how people in the field interpret and therefore respond to her and how she herself filters her observations. So, arguably, the more fraught the power relations in the field, the more accounting for these sorts of positionality matters to the quality of the research.

Michio: Well, but wouldn't being invisible obviate the need for attention to what you call positionality?

Dr. Scripshon: Not at all! The researcher-specific positionality I was just talking about is only the most obvious and least avoidable center of a successive series of positions that every research project adopts. Ethnography's attention to researcher-specific positionality is nested inside other positions, such as the ways in which the underlying logic of inquiry used in the research channels a whole series of decisions of great import, beginning with the framing of the research question to the way the researcher counts certain things as facts or observations relevant to the research and others as coincidental or unimportant. And this position is itself nested within yet another that locates any given project within larger disciplinary histories connected to broader political projects, funding programs, and specific ideologies and interests. At this level, researchers reflect on the uses of research, on the kinds of discourses one's research legitimizes and is in turn legitimized by, and on the likely effects—intended and unintended—of those discourses on the subjects of research and the broader social and political worlds they inhabit.

Michio: I see. So, Dr. Scripshon, are you saying that all research must give an account of its position in these three dimensions?

Dr. Scripshon: I would say, Michio, that all research already does so, if not through explicit reflection, then through implicit silences. One unique feature of an interpretive approach to ethnography is that it surfaces these conversations for explicit commentary, rather than allowing them to be taken for granted, and that it does so because of the analytical and ethical gains that result.

Michio: So, from this perspective, the idea of invisibility is a bit ironic?

Dr. Scripshon: Ironic, yes, because part of the impetus of an interpretive approach is to make the position of the researcher at these different levels more visible.

Michio: I think I already know your answer, but would you ever consider using FIP in your own research in Mongolia?

Dr. Scripshon: Michio, let me just ask if after three years of slowly building rapport with this community and countless bruises and not a few broken bones from trying to master the impossible art of riding smelly horses across the tundra, you really think I would squander all that hard work by swallowing some potion that makes me disappear with a puff and a poof?

Michio: And there you have it, listeners. Radically different takes from Professors Salthaway and Scripshon on the invention of FIP. Clearly, the place and value of ethnographic research in the social sciences is rooted in longstanding disciplinary debates defying easy resolution. It seems that FIP will intensify rather than resolve these debates.

Meanwhile, social science departments and the research ethics committees called Institutional Review Boards, or IRBs, are scrambling to deal with the practical implications of FIP's imminent release. In an effort to attract the most competitive Ph.D. applicants, some top departments are already promising funding and specialized methods courses to support FIP-enabled fieldwork, while other departments are embroiled in debates over whether FIP ought to be reserved for tenured faculty, at least in its initial years of use. And in keeping with the patchwork system of university-specific IRB procedures, some IRB committees are all but requiring ethnographers to use FIP in the field, arguing that the risk of harm to subjects is radically reduced by the invisibility of the researcher. Researchers who can't be seen are less likely to harm subjects, they say. Other IRB committees have taken an opposite approach, equating invisibility with a kind of deception, which requires difficult-to-obtain exemption from standard informed-consent requirements.

For Principal Investigator Dr. Popper Will Falsify, the intellectual and pragmatic debates ignited by the invention of FIP only serve to underscore its revolutionary importance for the social sciences. We'll end this program with this confident quote from Dr. Popper Will Falsify: "We may very well be standing at an historic junction. In one hundred years, the history of the social sciences may be divided simply into pre-FIP and post-FIP. Real progress has been made. There will be no turning back."

Listeners! More on invisibility and time travel, as well as a special live question-and-answer session on air with our guests Dr. Maura D. Scripshon and Dr. Cy N. Salthaway after these short messages from our sponsors.

[Lights fade to darkness as futuristic music plays to end radio segment.]

End of Act Three

Notes

1 My thanks to Lee Ann Fujii for helping name Drs. Salthaway and Scripshon.
2 Kaku 2016a
3 Kaku 2016b
4 Schaffer 2016: 2
5 Kaku 2008
6 Kaku 2016b
7 Kaku 2016b

ACT FOUR: ETHNOGRAPHY AND POWER

The political organization of a native tribe is obviously one of the first things to be known clearly. Now the political organization of an African people may be of an advanced kind, implying a sort of monarchy, with extensive traditions and genealogies, with great ceremonial and ritual, a developed system of finance, military organization and various judiciary functions. Such native states can be allowed to run on their own lines but they have to be first expurgated and then controlled. Now it is essential to touch as little as possible of the established order, and yet to eliminate all elements which might offend European susceptibilities or be a menace to good relations. Such knowledge obviously ought to be obtained.

Bronislaw Malinowski, "Practical Anthropology" (1929: 24–25)

Cultural knowledge of adversaries should be considered a national security priority.

Montgomery McFate, "The Military Utility of Understanding Adversary Culture" (2005a: 43)

There are armies, and armies of scholars at work politically, militarily, ideologically.

Edward Said, "Representing the Colonized: Anthropology's Interlocutors" (2006: 373)

[F]ieldwork itself reproduces modes of knowing straight out of plantation slavery, plantation management, and plantation geographies that were laboratories for black subjection and black resistance.

Christina Sharpe, "Black Life, Annotated" (2014)

I have never approved of the very public manner in which some of our western friends have conducted what they call the underground railroad, but which I think, by their open declarations, has been made most emphatically the upperground railroad. …I see and feel assured that those open declarations are a positive evil to the slaves remaining, who are seeking to escape. They do nothing towards enlightening the slave, whilst they do much towards enlightening the master. They stimulate him to greater watchfulness, and enhance his power to capture his slave.

Frederick Douglass, *Narrative of the Life of Frederick Douglass, an American Slave* (2017 [1845]: 86)

Scene

Back in the Finger Lakes Barn. Séverine Autesserre, Katherine Boo, Mitchell Duneier, Alice Goffman, Karen Ho, Timothy Pachirat, James C. Scott, Anna Tsing, Piers Vitebsky, and Loïc Wacquant mingle, the curled-up wolfdog nearly lifeless at their feet.

Loïc Wacquant [impatient]: The complication of this dog aside, I would like to state from the outset my opposition to this ridiculous idea.

Mitchell (Mitch) Duneier: I agree with you, Loïc. This is just not a good idea.

Alice Goffman [sighing]: So I assume all of you received the same letter I did?

Anna Tsing: The one from someone calling themself The Prosecutor that summoned us here to hold an ethnographic trial of your book?

Alice: Yes, the one that opens with, "In the name of the public interest and the integrity of science, I demand that you present yourselves for an ethnographic trial to be held on the eleventh of May in the old barn off Lake Keuka." Did everyone else receive it as well?

[Nods all around.]

Loïc [looking around]: So where is this so-called prosecutor?

Alice: Doesn't look like he or she is here yet.

[Pauses and looks around the room.]

Unless it's one of you?

[Everyone shakes their heads no.]

Katherine Boo: Well, The Prosecutor's letter of summons did mention that they would be wearing "A flowing black cloak woven with the strands of truth and carrying an oak staff fashioned from justice." Doesn't look like anyone here is dressed like that!

Séverine Autesserre [smiling]: I laughed pretty hard when I read that. I mean, it is a joke, right?

[Silence and serious faces.]

Right?

[Her smile fades.]

Alice: Well, this person who calls her- or himself The Prosecutor *has* been racking up some very public victories against a whole series of junior scholars lately, most of them ethnographers. It's resulted in the early end of many promising intellectual futures and intimidated many other young scholars from pursuing the kind of work we all do.

Anna: Yes, I've seen that happen, too, Alice, and I thought it was serious enough that I should come.

[All nod glumly.]

James C. (Jim) Scott [smiling, trying to break the tension]: Hey, speaking of jokes, I assume you all know the one that goes, "three anthropologists, three sociologists, three political scientists, and a journalist meet a hooded prosecutor in an old barn to hold a trial?"

[General laughter breaks the tension.]

Anna: Don't forget the sleeping dog.
Timothy Pachirat: Wolfdog.
Jim: Oh, right. "Three anthropologists, three sociologists, three political scientists, one journalist, *and* a wolfdog meet in an old barn to hold a trial…."
Karen Ho: The dog doesn't actually look so hot.
Anna: Wolfdog.
Timothy: Do you recognize her, Piers?
Piers Vitebsky: Why? Should I?
Timothy: Look more closely.
Piers [stepping closer and peering down, brow furrowed]: What? The one-eyed wolfdog who can see the future?
Timothy: Yes, it's the one-eyed wolfdog from your fieldwork among the Eveny reindeer herders.
Piers: She's lost a lot of weight.
Timothy: She's been running.
Piers: No!
Timothy: Yes! All the way here.
Katherine: For the trial?
Timothy: I'm not sure.
Anna: The wolfdog from *The Reindeer People*?
Piers: Exactly.
Séverine: Great book!
Piers: Thanks!
Karen: Twenty years is a long time for fieldwork.
Loïc: Yeah, I'm not really sure how you have so much direct quotation in there or how you remembered it all.
Piers: Conditions were a bit more inclement than a boxing gym a few miles from my university dorm room, but I still managed. Lead pencils work better than pens when it's –80 Fahrenheit.
Loïc: I would never measure distance traversed in miles alone, Piers. As far as the University of Chicago was concerned, The Woodlawn Boy's Club where I did my fieldwork might as well have been in Siberia.
Timothy: The wolfdog ran here from Siberia.

[Scattered exclamations of surprise and disbelief. Everyone interjecting questions at once.]

Alice [nervously]: Everyone!

[General silence.]

Since it's my work that we're ostensibly gathered here to put on trial, and since I'm understandably quite nervous about this, may I get an explanation for what's going on with this wolfdog?

[Voices quiet down.]

Timothy: Last week, I was sitting in this very barn, writing, or at least trying to write, when the creature you see before you appeared at my feet and began speaking to me.

Loïc: Whoa, what are you smoking, Timothy? How long have you been out here alone?

Alice: Let him tell his story.

Timothy: She was very weak and tired, on the verge of collapsing into the coma you now see her in.

Katherine: Why haven't you taken her to the vet?

Piers: A vet would kill this animal. She needs to see a shaman.

Anna: Or a Diamond Queen.

Alice: Can we all please stop interrupting and hear the story?

Timothy: So she staggered into the barn and began speaking to me. She told me that she had run all the way from Siberia, by night, and that each day she slept and dreamed a series of different dreams.

Loïc: Quite a tale, but what's it to do with us?

Timothy: Everything! She arrived here, exhausted, and asked if we were about to stage an ethnographic trial. I said yes.

Alice: She knew about the trial?

Timothy: Yes. Then she began recounting the dreams she had each day as she slept. The first was of a slaughterhouse. The second was of Wall Street investment banks. Then it was matsutake mushrooms, peasants in a Malaysian village, a Mumbai slum, a Chicago boxing gym…you get the picture.

Jim: Sorry to be the dense one here, but I don't get it at all.

Katherine: I don't either.

Alice: She was dreaming the books written by each person gathered here.

Mitch: Unreal.

Loïc: That's one word for it.

Timothy: I tried to write down as much of what she said about each of your books as I could remember. It wasn't your first-year college student kind of summary. It was more pointed. The description itself carried a viewpoint.

Anna [aside]: Don't all descriptions carry a viewpoint?

Timothy [looking at Karen]: She has this strangely poetic description of your book, for example. Instead of it being about stakeholders and shareholders and the Princeton to Wall Street pipeline, she dreamed about people

screaming "Buy! Sell! Trade!" into telephones and about the chains connecting these screams to forests, oceans, animals, and other people all around the world. Every time they screamed, the chains would get yanked, taking down anyone or anything on the other end of them.

KAREN: Wow. That's depressing.

TIMOTHY [looking at Piers]: And her recounting of your book, Piers, was the strangest of all.

[Piers raises eyebrows.]

She said she dreamed of herself, not as she was, but as you saw her and wrote about her. She said that as her dreams went on across several days, she began to feel herself not as the venerated one-eyed wolfdog who could see the future, but simply as the product of superstitious beliefs of a primitive people.

ANNA [under her breath]: I have lots to say about the use of the word "primitive."

TIMOTHY: As she was recounting this, her voice grew fainter and fainter, until, finally, she said something like, "I feel like I have been killed."

JIM: Maudlin.

KATHERINE: Is she breathing? I still think we should take her to the vet.

KAREN [kneeling to get closer to the wolfdog]: Yes, she's still breathing. Just very faintly.

PIERS: So it wasn't the running that wore her down, then.

TIMOTHY: It doesn't seem so.

PIERS: It was experiencing herself through my eyes. How horrid. How absolutely horrid. Although I do write that by the end of my twenty years among the Eveny, I was beginning to question my own application of a Western worldview to their ways of thinking.

ALICE [softly]: Is that all?

TIMOTHY: No, there is more.

PIERS: I was afraid of that.

TIMOTHY: She also delivered these to me.

[Holds up sheaf of equations and vial of serum.]

KATHERINE: What *are* those?

JIM: This story gets stranger and stranger.

TIMOTHY: As the one-eyed dog told it to me, it is this.

[Recounts story of the discovery of the frozen body, the shaman's consultation, and the secret memo.]

MITCH: This is the Fieldwork Invisibility Potion that we have been hearing about in the news?

TIMOTHY: Yes, according to the wolfdog, this is it.

LOÏC: One of the dumbest things I've ever heard of.

Séverine: What is?

Loïc: This idea of an invisibility potion.

Séverine: Well, it's not an idea anymore, it's a reality.

Loïc: An asinine reality that completely negates the purpose of ethnography, which is embodiment.

Anna: Is this the only vial of the serum?

Karen: I think I remember them saying on the news that they had only been able to produce a single vial.

Mitch: What should we do with it?

Anna [joking]: How about we pass it around right now and take some shots!

Loïc [scowling]: I refuse.

Jim [smiling]: I think Anna was playing around, Loïc.

Anna [smiling]: Oh, was I now?

[General nervous silence as everyone stares at the incandescent serum.]

But seriously, what should we do with it?

Karen: Since the wolfdog dreamed of our books, knew about the mock trial we were going to stage, and brought the serum to us, it seems incumbent on us to figure out together what to do with the serum.

Mitch [looking at Loïc]: Seems unlikely we'll all agree.

Karen: True, but at least we'll have a chance to air the issues, and who knows, maybe talking about the serum will also help us with the trial as well.

Alice: How would it do that?

Jim: Since it's Alice's trial that we're all gathered here for, maybe we ought to ask her.

Anna: Spoken like a true anarchist.

Jim: Hey, it's two cheers for anarchism, not three. And anyway, I don't think a true anarchist would support anything like a trial in the first place.

Alice [looking down at the wolfdog]: This is not really a theoretical discussion, folks. This dog is doing extremely poorly, and I think we ought to figure out quickly what to do with the potion and with her.

Séverine: There's far too many of us here for this to be a productive discussion in a single big group. While we're waiting for The Prosecutor, assuming they are even coming, why don't we split into smaller groups to talk about the serum? Maybe we'll be able to reach a consensus on what to do with that by the time The Prosecutor arrives.

Piers: That seems reasonable given what's at stake here.

Alice: I'm okay with that plan too. But how should we split up?

Timothy: Why don't we divide up based on the location of our ethnographies? Mitch, Loïc, Alice, Karen, and I have written ethnographies based here in the United States. Anna, Jim, and Katherine's work is based in Asia. And Piers's work is in Russia, while Séverine's is in Democratic Republic of Congo.

Anna: Well, I'm not sure I agree with that characterization of my work. My book actually travels quite a bit, following the path of the matsutake mushroom from Oregon to Japan and even further afield than that, including forestry efforts in Lapland, Finland, and Yunnan, China. Like the matsutake mushroom itself, my book works with both patches—localities spread out in time and space but interconnected along a common commodity chain—and spores, which can take flight and spread in very open-ended ways. So I wouldn't be so quick to say, Timothy, that my book is based in Asia.

Séverine: Yes, I agree with you, Anna. This location-based way of thinking about ethnography leaves me out as well. *Peaceland* is hardly location-based. While my research for the book did involve extensive time in specific locations, particularly North Kivu, Congo, I also draw on extensive experience in many other places: Kosovo, Cyprus, Nicaragua, Israel and the Palestinian Territories, Afghanistan, Timor-Leste, South Sudan, and Burundi. In fact, the ultimate "field" that I'm studying is non-locatable in geographic terms and is one that I create a metaphorical name for—*Peaceland*—which signifies the common practices and narratives of international interveners wherever they find themselves geographically.[1]

Jim: My first book was definitely based in one location, in a very out-of-the-way peasant village in Malaysia! Even in that book, however, I look at things that travel: rumors, speculation, gossip, and character assassination. I increasingly feel that location is a bit of an old-fashioned way to think about our work. It's an artifact, I think, of ethnography's academic history.

Piers: But if we look at the classics of anthropological ethnography—Malinowski, Radcliffe-Brown, Evans-Pritchard—they are all based on the idea of going into a single, bounded society, a single "place," and trying to capture the totality of that place—its customs, kinship structures, rituals, beliefs, food avoidances, sexual mores, and so on—and then to present the totality of that place to the reader.

Anna [aside]: Usually, the Western white male reader.

Jim: Thus, the term "fieldwork" for what we do when we do research.

Karen: What a misnomer! My so-called "fieldwork" took place in lower Manhattan's concrete and steel jungle, not in any sort of faraway "field"! Much better to call it steelwork than fieldwork.

Piers: Well, I hear what you are all saying about multi-sited ethnography. But whatever we choose to call it—"fieldwork" or "steelwork"—I don't want to completely let go of locality. People, ideas, and things travel, as do the ethnographers who research them, but I would still want to insist on the primacy of the local specificity of ethnography, of its commitment to long-term immersion in a place.[2]

Karen: Long-term immersion and place are two elements of ethnography that I am interested in unsettling a little bit. Certainly, many ethnographies still rightly rely on long-term, immersive, single-site "fieldwork." But we must

also create space for studies of things that move quickly or that present in multiple places, sometimes ephemerally.

Séverine: Like mushroom spores!

Anna: Yes, like mushroom spores! Even if it isn't based on long-term immersion in a single place, like the twenty years Piers spent amongst the Eveny reindeer herders, I still think it's possible to apply an ethnographic approach and sensibility to the study of non-localizable things like mushroom spores and international peacekeepers.

Séverine [laughing]: I never thought I'd hear international peacekeepers and mushroom spores in the same sentence.

[General laughter.]

Alice [laughing then turning serious]: OK, OK, everyone! This is all very merry, but let me remind you that we have an invisibility potion to deal with, an ailing wolfdog to save, and a trial to stage. So, if we can't reach consensus on splitting into location-based groups, would it perhaps be more productive for us to organize ourselves by the kinds of themes and ideas our works engage?

Mitch: Great question, Alice. It perfectly parallels the distinction Clifford Geertz makes between the locus of study and the object of study. He puts it something like, Ethnographers don't study villages, they study in villages.[3]

Katherine: Meaning?

Mitch: Meaning that where we study and what we study are two different things. Two people can have radically different fieldsites but still study the same thing. Alternatively, two people can share the exact same fieldsite but study radically different things.

Katherine: Makes sense.

Karen: Geertz famously goes on to talk about making sheep raids speak to revolutions and winks to epistemology. The primary task of ethnography, for Geertz, lies in making what he calls its "microscopic nature" speak to made-in-the-academy mega-concepts, or as he inimitably puts it, how to make the complex specificity of ethnography "present the sociological mind with bodied stuff on which to feed."[4]

Piers: You have that memorized?

Karen: It's a memorable line.

Jim: Yes, it seems that Piers and others who defend the importance of place to ethnography are speaking, perhaps, to the sheep raid and wink side of that relationship, while Karen, Anna, Séverine, and others who reference the promise of multi-sited ethnography are perhaps identifying more with the revolutions and epistemology side of the relationship.

Séverine: So some of us identify ourselves more with the locus of study and others with object of study?

Jim: Something like that, yes.

Séverine: I hear what you're saying, Jim, but, at least in my case, I don't think that's quite right. I'm just as interested in the sheep raid as the revolution, and I don't think that good ethnography privileges one over the other. But, in my own work, I couldn't even get to the revolution or the epistemology if I didn't attend to sheep raids and winks in not one, but multiple locations, because the kind of revolution and epistemology that I'm after is constituted by and legible only because of its circulation through multiple locations. Or in the language of my own work, there is no way for me to get to the object of my study—the practices, habits, and narratives of international interveners—without attending to multiple localities.

Mitch: My own work is very location-based, but I am open to the possibility of grouping ourselves by theme. Although I must say that I hardly know at the outset of my work what the specific themes and analysis are going to be. I put a lot of value on place, on location, and, most importantly, on what emerges from the people with whom I interact.

Loïc [sarcastically]: Well, Mitch, there you go again, singing the epistemological fairy tale of diagnostic ethnography and grounded theory.[5] That nonsense is exactly what produces an unsophisticated raw empiricism and theoretical absentmindedness that in turn leads to the mistaking of folk sayings for actual categories of analysis, among other errors.

Mitch: First of all, Loïc, I don't even think of myself as a theorist, much less a grounded theorist. Unlike you, I don't think the apex of every ethnography needs to culminate in the reconstruction of theory in order for that ethnography to have value. Theory has never been the pivotal agenda of my own work, and I would even go so far as to say that the best ethnographies are not remembered for their theories but rather for their ability to show the lives and social situations of those they study.

Loïc: If the point of ethnography is not to develop or illuminate theory then what is it there for?

Mitch: "Part of my criticism of ethnography as a frame for doing theory for theory's sake is not simply that the people in the studies can't recognize themselves in the work, but they don't even have a sense of how they mattered. I mean how did it matter, why did it matter that this ethnographer spent all this time with me? So he could enter into a dialogue with theory that is utterly trivial, even by academic standards? What is the ethics of that? I want the books I write to contribute to a greater understanding of the world. I want to be able to say to them, the people in my books, that students are going to be able to read this book and they're going to understand homeless people in a better way. To me that's a perfectly legitimate minimal warrant for this work, and in fact the best that I've been able to come up with."[6]

Loïc: That's the best warrant you can come up with for why to do ethnography?

MITCH: I said minimal warrant. And I was referring to my own work. I would never legislate a universal goal for every ethnography. But even your own book *Body & Soul* is a great example of what I mean here, Loïc. Although you are an outstanding theorist, I don't think the enduring value of your book lies in its reconstruction or elaboration of a theory of habitus, but rather in the compelling ways it evokes the lives and social situations of the men in the boxing gym.

LOÏC: I appreciate the attempted compliment, but I disagree. I do think that the theoretical reconstruction of habitus and development of a carnal sociology in *Body & Soul* are its most important and enduring contributions. And the development of theory certainly is the goal of my own ethnographic work!

But, Mitch, even accepting that theory isn't your aim, there must be *some* way that you think about ideas and themes, even at the very outset of your work. You surely don't just walk into a place and say, out of nowhere, I think I'm going to do an ethnography here, without having any idea whatsoever of what might be conceptually or theoretically interesting about the place! To begin with, you can't even separate a studied "here" from a non-studied "there" without exercising some sort of theoretical and conceptual control.

MITCH [evenly]: Well, yes, if reduced to a caricature, of course it is absurd to say that an ethnographer starts doing fieldwork without any notion whatsoever of what interests them. And while I don't utilize a grounded theory approach, even Glaser and Straus, the two people most associated with grounded theory, state clearly and early in their book that no researcher approaches reality as a tabula rasa and always has a point of view that they bring to the field.[7]

What I actually find more worrying is the inverse of your concern: ethnographers so completely controlled by an *a priori* theoretical interest that they are incapable of actually noticing anything in their fieldsites, much less being surprised by it. In this inverted caricature, "the field" exists purely as an inert illustrative putty, shaped at will by ethnographers according to the dictates and needs of their theoretical and scholarly proclivities.

Look, if there's any epistemological fairy tale that needs debunking, it's the notion that there is one single or best way to do ethnography. "As… ethnographers, we walk a line, many lines: romanticizing versus condemning; bringing theoretical questions to the field versus discovering them while working at the site; protecting anonymity versus replication and/or accountability; political agendas versus naive tabula rasa; fully theorized versus open to issues and empirical events; redistributing ethnographic authority versus maintaining the authority of the social scientist; seeing agency/resistance versus all determining structures; accumulating many thinner observations versus a few thick ones; using an in-depth description to enter

into a dialogue with a theory versus telling readers only as much about people and places as they need to know to reconstruct a theory. These are real dilemmas that become embodied as practical trade-offs and enduring tensions in the descriptions and arguments of ethnographies."[8]

Séverine: That's well said. But how do you navigate these dilemmas in your own work, Mitch, especially the one between bringing theoretical questions to the field versus discovering them while working at the site?

Mitch: Some of my earlier work was guided by a personal and theoretical interest in how moral behavior is or is not constructed. That interest provided the broad parameters for what I was alert to, both in initially recognizing certain social fields as potential fieldsites and then again once I conducted research in those defined social fields. Beyond that, however, I was very deliberate in not setting out with particular theories I knew I wanted to reconstruct. And when I did move from data collection to theory reconstruction, it was almost in spite of myself. In fact, "when I went back to Sixth Avenue to work as a magazine vendor, I hadn't yet formulated a precise research question. I had no theories that I wanted to test or reconstruct, and I didn't have any particular scholarly literature to which I knew I wanted to contribute."[9]

Séverine: I have to say, Mitch, as refreshing as I find what you're saying, it seems radically counter to the way we political scientists are trained to represent the research process. I can't imagine anyone in our discipline getting a grant or even passing a dissertation proposal defense if they wrote, "I plan to go to X fieldsite without a specific research question or scholarly literature that I know I want to contribute to."

Loïc [muttering]: I don't think it should be any different in sociology either.

Mitch: Look, I admit that "the fact that I did not know my specific research question at the start may seem counter to the way that sociologists are supposed to operate."[10] But, actually, think about it this way: "In much of social science, especially much of quantitative research using large data sets, a research design often emerges *after* the data has been collected.... Like quantitative researchers who get an idea of what to look for by mulling over existing data, I began to get ideas from the things I was seeing and hearing on the street."[11]

Timothy: Well, that may be the reality of how much positivist research is conducted, but it is certainly not held up as an acceptable approach. One widely influential ideal for both quantitative and qualitative researchers is for the research question to shape data collection, not vice versa. And, in particular, the ideal is that any data that inform the hypotheses in the research design should not be the same data against which those hypotheses are later tested.[12]

Jim: Yes, but now we are dealing with the difference between the fiction of how "rigorous" research is supposed to be carried out as opposed to the reality of how it is actually carried out. Think about the whole notion of "kitchen

sink" regressions, in which every variable in a data set is run against every other possible variable, and whatever emerges as "statistically significant" gets conscripted for post-hoc theorizing and a backdated research design. This backdated design, by the way, is often later presented in an article or book's "theory" section as having informed the theoretical interest in those variables to begin with.

Timothy: So true. But these fictions matter, especially when they are enforced by grants and other professional awards that lead to the perpetuation of a recurring narrative that everyone parrots and strives to conform to regardless of whether it reflects actual practice or not.

Séverine: So would you say, Mitch, that your approach is more inductive while Loïc's is more deductive?

Mitch: Look, "my approach is not strictly inductive or deductive: I engage a variety of theoretical/sociological questions, some of which I brought to the site from the beginning, some of which I discovered through various routes as I worked in the site." What Loïc's comment misses is that "in one sense, every sociologist, when they bring questions to a site, 'projects' the question onto the site. The real question is whether or not they project their answer: Did the research allow for any learning from the site?"[13] That, to me, is the key question, and I approach my fieldsites in order to maximize the possibilities of actually learning something from them, even at the expense of the kinds of *ex ante* theoretical control that Loïc seems to advocate.

Loïc: That's a complete misrepresentation of my approach.

[Uncomfortable silence.]

Alice: Returning to the question of how we should split up into smaller groups, don't you think it would be quite hard to divide our work thematically anyway? I mean, I work on power, broadly speaking, but then again it seems there's hardly a person in the room whose work doesn't centrally engage with power in one way or another.

Piers: But isn't that just another way of saying that power is everywhere, so a good ethnography, by virtue of being a good ethnography, will not be able to help but pick up on power?

Katherine: I come at this question from the perspective of a journalist, not an academic ethnographer.

Timothy: Well, I think your work represents some of the very best contemporary work in the ethnographic tradition, whether classified as academic or not.

Katherine: Thanks, Timothy! Anyway, what I wanted to say is that from my perspective as a journalist, what makes an immersive sensibility compelling is its capacity to depict the particular and the specific. As I emphasize to any and all who will listen, I am keenly interested in power, but not in some abstract, theoretical, or more academic sense. My work is animated by a conviction

that "small stories in so-called hidden places matter. And one of the reasons they matter, I think, is because they implicate and complicate what we generally consider to be the larger story in this country and throughout the world, which is the story of people who do have political and economic power."[14]

KAREN: That's so true, Katherine. In an abstract sense, your work is about class power in a Mumbai slum, but to leave it at that would be a disservice. What makes your writing so compelling is that it never feels didactic. *Behind the Beautiful Forevers* draws us into the intimate details of individual lives, and it's out of that rich and complex storytelling that dominant stories about those in power become complicated and implicated.

KATHERINE: I think that's right, Karen. Although I'm interested in power, I didn't say to myself that I was going to write about this slum in order to teach people this or that abstract lesson about power. I am more interested in understanding who people are and how they live and experience life under those very specific conditions, and then in letting those specifics speak back to the larger stories we tend to hear by and about people who wield more political and economic power.

PIERS [smiling]: And you just happened to win a National Book Award in the process?

KATHERINE: What do you mean by that?

PIERS: It seems a bit of an oversimplification to say that you just wanted to understand who people were and how people lived. The characters you feature in *Behind the Beautiful Forevers*—Sunil, Manju, Abdul, and others—they were chosen by you out of a much wider range of people whom you could have written about, but you chose not to.

KATHERINE: Well, in some ways I didn't choose them, they chose me.

KAREN: What do you mean by that?

KATHERINE: They were the people I happened to get to know best, the ones who allowed me into their lives and who had the most compelling life experiences to share.

PIERS: OK, but still, there was a very deliberate crafting of a narrative, of a story.

KATHERINE: Well, of course! But that story did not come about because I decided from the outset to write about some abstract theory of power.

ANNA: I'm very sympathetic to this. Some of us come to our work serendipitously, by complete accident. Others of us are much more intentional, animated by what some might call conceptual or theoretical questions or by generalized "puzzles" about something we observe in the world.

TIMOTHY: I think the way we write our ethnographies, especially in academia, has something to do with this. There is a bias to the way we present our work—which we often call our arguments, rather than our stories—that causes us to write deductive intentionality backwards into the research process.

Mitch: Because we wish to represent ourselves as in control and knowing at all times?

Timothy [laughing]: Maybe, a bit. But I think it has as much to do with our writing conventions as with anything else. Most books in the social sciences usually lead with what we call a theory chapter, followed by a methods chapter, followed by several chapters of so-called data, and close with a conclusion that reiterates how the data chapters have demonstrated the arguments put forward in the theory chapter.

Jim: A handy little structure.

Timothy: But you can see how the standardization of this structure, through peer reviews for university presses, through dissertation committees, and through sheer repetition can lead to a construction of social science as dominated by theoretical concerns that we then go out into the empirical world to try to address.

Jim: As opposed to a social science dominated by accidents?

Timothy: As opposed to a social science that recognizes its debts to serendipity.

Mitch: Right. Regardless of how ethnographers *actually* came to their subject matter, it typically gets represented as being theoretically driven.

Timothy: I just think there is so much value in laying bare the serendipitous, even accidental, nature of a lot of ethnographic research projects.

Karen: What is the value you see in doing that?

Timothy: For one, I think it keeps our ethnographies truer to their origins. Rather than constructing some cosmic Origin Myth rooted in abstract theoretical concerns, rooted in logic and rationality, it preserves a space for the sensual actuality of lived experience.

Karen: But why should we do that?

Timothy: Because it keeps our feet on the ground.

Loïc: But in the end, don't we all want to reach for something larger and more constant than the serendipitous and the accidental? We are not just short-story writers or novelists, moving through the world weaving threads of our own fancy. We do social science because we care about ideas that are larger than the particular, about arguments and theories that reach beyond a specific locale. I think I am unique in this gathering in that I don't practice ethnography as an exclusive craft; I argue it should be always combined with comparative institutional analysis. *Body & Soul* is complemented by *Urban Outcasts*.[15]

Jim: I wouldn't say you are completely unique in that regard, Loïc. *Weapons of the Weak* is the only work I've done that is strongly ethnographic, if by that we mean based on firsthand participant observation. My book *Seeing Like a State*, for example, utilizes what you might call comparative institutional analysis. And *The Art of Not Being Governed* relies heavily on historical analysis and interpretation.[16]

MITCH: And my book on the ghetto from the early modern Jews to the present is based on historical research.[17] I would go so far as to say that many ethnographers must, of necessity, do other kinds of work because it can become more difficult to do deeply immersive fieldwork once you get entangled in university or family life. Many of these other kinds of studies are quite complementary to the earlier work, completing some aspect of that work which could not have been fulfilled "on the ground."

ANNA: Here we are again, right back in the tension between the sheep raid and the revolution, between winks and epistemology.

TIMOTHY: Exactly! That entire passage is worth reading.

[Walks to bookshelf and thumbs through Geertz's *The Interpretation of Cultures.* Finds the right page and reads aloud.]

> *The methodological problem which the microscopic nature of ethnography presents is both real and critical. But it is not to be resolved by regarding a remote locality as the world in a teacup or as the sociological equivalent of a cloud chamber. It is to be resolved—or, anyway, decently kept at bay—by realizing that social actions are comments on more than themselves; that where an interpretation comes from does not determine where it can be impelled to go. Small facts speak to large issues, winks to epistemology, or sheep raids to revolution, because they are made to.*[18]

I love this passage because it brings to the forefront the creative tension inhabited by the ethnographer, a creative tension generated by this space between the locus and the object. How to make small facts speak to large issues, winks to epistemology, sheep raids to revolution? To live only among the small facts, the winks, and the sheep raids leads to irrelevance. We learn about small facts, winks, and sheep raids, but we have no ability to signal why they matter. Yet to talk only about large issues like epistemology and revolution is to resign ourselves to an irrelevance of a different kind, an irrelevance of made-in-the-academy concepts.

KAREN: Nice, but how to relate the two?

MITCH: Always imperfectly, I think. This is what it means for them to be in creative tension. They are not antagonists, but neither do they fit neatly into some sort of formulaic ratio: take two-parts locus, three-parts object, stir vigorously, and presto: out comes ethnography!

JIM: Exactly. It's interesting to look more closely at Geertz's language in this passage. First, he intimates that there may not be *resolution* to this methodological problem of how to relate small facts to large issues, but only better and worse ways of keeping the problem at bay.

KATHERINE: At bay! Inspired by present company [nodding gently at the sleeping wolfdog], it brings to mind the starving wolf pack chasing the two dog-sledders across the frozen Alaskan Tundra in the opening chapters of Jack London's *White Fang.*

[The wolfdog stirs in her sleep, makes a low growl, and the hairs on her neck rise. Nervous laughter among the ethnographers.]

The wolves and the men are locked in a precarious shadow dance. All day long, the wolves follow at a close enough distance for the men to hear them, and sometimes to see them, but far enough away that the men can't shoot them.

ALICE [remembering]: And in any case, I think they only have like three or four bullets left.

KATHERINE: Right! And then at night, when the men stop to make camp, the wolves circle around the very edges of the firelight. And soon, the entire existence of these two men is defined by how to keep these wolves at bay for enough time that they can make it to the next fort.

ALICE: But then the men begin losing their sled dogs one by one. And soon it proves too much for one of the men, who goes charging after the wolves with his handful of bullets and is taken down.

JIM: A pretty grim metaphor for ethnography!

KATHERINE: Yes, but look at the language Geertz uses to evoke how to keep this methodological problem at bay. It's the language of realization and willpower. The ethnographer must *realize* that social actions are commentaries on more than themselves. That is, she must realize that the space of her work is in the tension between the locus (the social actions) and this larger thing (the objects) that extends beyond the social actions. Then, Geertz goes on to use two highly interesting phrases: the first is that interpretation must be *impelled* to go somewhere. And the second is that small facts speak to large issues because they are *made* to. In other words, there is no formula or method or step-wise process for getting from small facts to large issues. It is the task of the ethnographer to impel this connection, to, quite literally, make it happen.

ALICE: Yes, and even then the end result is that the methodological problem of ethnography's microscopic nature is merely kept at bay, rather than being resolved in any final way.

JIM: Michael Burawoy's essay on the extended case method is another example of how ethnographers have tried to navigate this tension.[19]

TIMOTHY: Yes, as its title suggests, one of the central questions Burawoy deals with is how the specificity of ethnographic research can be made to speak to larger questions and issues. How a case can be extended to theory.

MITCH: I'd just point out, again, that it need not be the goal of every ethnography to reconstruct or extend to theory. Sometimes we're in danger of fetishizing theory to the point that it becomes the tail that wags the dog, to the point that the holy grail of theory makes the people and their social situations secondary. This, I think, is a danger to avoid. What ethnography excels at—and I would even say that this is one of the moral functions of ethnography in the social sciences—is showing the people and the social situations behind the theories and the statistics that we construct.

Jim: Right. We simply should not do social science behind the backs of the people we claim to study.

Anna: We've been talking about this tension between the microscopic and the macroscopic in the context of ethnography, but I think all research, all knowledge creation, regardless of its specific method, wrestles with these tensions in one way or another. We are, so to speak, always among wolves.

[The wolfdog growls even more loudly in her sleep.]

Piers: Including, quite literally, the wolfdog on this floor.

Mitch: And so, at least on this dimension, you could imagine methods and methodologies as specific ways that have been devised in different research traditions to deal with the wolf pack.

Karen: I like it. Playing with this a bit, you could imagine that programmatic extermination is one methodological option. These wolves, these enduring tensions between small facts and large issues, are pesky, dangerous, and evil, so let's put a bounty on the wolves and exterminate them, try to make them extinct. Let's play it completely safe and try to deal with the wolf problem by getting rid of the wolves altogether. The method itself acts as a kind of poisoned bait that you can, by virtue of employing it, just scatter throughout the forest and along the trail with relative certainty that it will get rid of the wolves altogether.

Jim: Well, which method or methodologies would that be?

Timothy: Anything that creates a step-wise, formulaic series of actions for the researcher to take in order to programmatically eliminate, or at least domesticate, the ravenous wolves.

Mitch [getting in on the metaphor]: And other researchers run out into the pack of wolves like the dogsledder did, with just a few bullets in his rifle, hoping for the best. Once in a while, someone comes out alive, having vanquished the wolves, but for the most part, those approaches tend to swallow up the researcher completely, leaving them ravaged either by the fangs of too-much-about-the-small-facts or the death grip of too-much-about-the-large-issues.

Jim: And which method or methodology would that be?

Mitch: I don't know—something completely idiosyncratic?

Anna: But must it always be about the extermination and killing of the wolves? What about a heightened understanding of and sensitivity to their movements and to who they are, understandings and sensitivities that make something amazing more likely without eliminating all danger?

Timothy: Great questions, Anna. Many readings of *The Call of the Wild* and *White Fang* emphasize the Darwinian "survival of the fittest" themes in London's work. In that sense, there are clear affinities between the notion of willpower in London's work and Geertz's emphasis on small facts being *made* to speak

to large issues. But more recently, other, more subversive readings of London have emerged, readings that create space for genuine interspecies relationships between humans and wolves.[20]

Anna: Yes, *The Call of the Wild* and *White Fang* are mirrored tales about the movement from civilized to barbarian and back. In *The Call of the Wild*, Buck is a dog who eventually becomes a wolf. In the other, White Fang is a half-dog, half-wolf who gradually becomes more and more domesticated and doglike.

[Wolfdog growls even more loudly, her hair bristling.]

Piers [stepping back with some alarm and speaking under his breath]: Well, I wonder which direction this wolfdog is moving in.

Anna [continuing]: But in both cases, London opens up spaces of possibility for new kinds of interspecies relationships to be formed with these wolfdogs. It's not always, or necessarily, reduced to humans shooting wolves or wolves killing humans.

Katherine: Oh, I see, Anna. So instead of thinking about an antagonistic relationship, pitting humans against wolves, we might think of the ethnographic sensibility as offering the possibility of living among wolves.

Anna: Living among wolves! Yes, I think that's more promising.

Timothy: This is where the magic of ethnography happens, in this uncomfortable but generative tension between locus and object. Unlike other methods, which bypass this discomfort with step-wise formulas—"just follow these steps in linear order and all will be well"—ethnography asks its practitioners to live in the creative tension between locus and object, to—as Katherine said—live among wolves.

Anna: Yes, and to do so with keen attention to one's own reflexivity and positionality.

[Wolfdog growls again. Again, Piers steps back.]

Piers [still under his breath]: I'm not so sure this wolfdog is very interested in living with me.

Anna [continuing]: All this wolf talk closely parallels the way I think about ethnography's capacity to tell multiple stories. "To listen to and to tell a rush of stories is a *method*. And why not make a strong claim and call it a science, an addition to knowledge? Its research object is contaminated diversity; its unit of analysis is the indeterminate encounter. To learn anything we must revitalize the arts of noticing and include ethnography and natural history."[21]

Séverine: I'm sympathetic! But, again, hearing this through the lens of my disciplinary training in political science, I can't help but hear the refrain that stories are only anecdotes and that the task of the social scientist is to somehow extract data from those stories, data that is reliable and valid

and reproducible. As some of my political science colleagues claim, all social science involves extracting information from the social world and analyzing it as data.[22]

Timothy: All this talk of extraction makes us sound like mountaintop coal miners, deep-water oil drillers, Great Plains frackers, or, for that matter, dentists!

[General laughter in the group.]

Anna: I advocate for an approach wholly contrary to what's suggested by an extraction metaphor. In insisting on the importance of stories, "we [run into] a problem with scale. A rush of stories cannot be neatly summed up. Its scales do not nest neatly; they draw attention to interrupting geographies and tempos. These interruptions elicit more stories. This is the rush of stories' power as a science. Yet it is just these interruptions that step out of the bounds of most modern science, which demands the possibility for infinite expansion without changing the research framework."[23]

Séverine: Yes, exactly.

Anna: "Arts of noticing are considered archaic because they are unable to 'scale up' in this way."[24]

Séverine: By 'scale up' you mean generalize? That it's hard to generalize from ethnography because ethnography is so specific?

Anna: Yes, "the ability to make one's research framework apply to greater scales, without changing the research questions, has become a hallmark of modern knowledge."[25]

Loïc: Are you saying that ethnography cuts against the grain of modern knowledge?

Anna: Ethnography that privileges the arts of noticing and that views a rush of stories as a method does, yes. I myself have always tried to privilege curiosity over coherence in my work.[26]

Jim: Maybe it's not so much against the grain of modern knowledge, per se, as it is against certain conceptions of what science is or should be. When people in political science say ethnography is not scientific, "the snotty reply is, Too bad for political science if it ends up excluding a lot of insight about politics that does not come in a certain package or format. But the real question is, does the book say something about power and the state? If it does and it's presented in an easy-to-swallow way, then so much the better."[27]

Loïc: It seems like it's a very slick slope from there to producing mere journalism.

Katherine: Hey now, don't be so quick to denigrate journalism!

Jim: I agree with Katherine. And I'm reminded "of something my colleague Charles Lindblom once said about a student's thesis, 'It's a failure, but it addresses big questions that are formulated in a brilliant way. Even though the student failed to answer these questions, the thesis still advances political insight further than lots of things that are rigorous but address trivial and banal questions.'"[28]

Séverine: Listen, as sympathetic as I am, we still run into this question of how to convince our colleagues, particularly in political science, that ethnographic work meets their standards of what constitutes science.

Jim: You know, "I read David Laitin's review of *Seeing Like a State*, and it's a rather interesting review. He says, 'The book is good, it will last forever, and it will become a classic.' But he also says, 'It ain't social science, because methodologically it's a mess; Scott selected cases on the dependent variable and so on.' A colleague of mine actually ran into Robert Bates and David Laitin at a political science meeting shortly after *Seeing Like a State* came out, and he asked them what they thought of the book. I think it was Laitin who said, 'What an artist, he's a real artist.' At one level it was a compliment, but at another level it was meant as a put down, because he was saying my work was not scientific. Well, I am happy to be called an artist because I don't believe political science is a natural science in the first place. I like Laitin's work, and I think he's an interesting intellectual. But I also think he is less of a social scientist than he believes himself to be, and I think the interesting ideas that Laitin has had don't really add up to anything particularly 'scientific.'"[29]

Anna: From the perspective of dominant knowledge systems which prize scalability, criticisms like the one leveled by David Laitin make sense. But from another perspective, a perspective more skeptical or even critical of scalability, returning to a rush of stories as method may be a way forward. And, as Jim points out, it's worth asking whether our most provocative and generative insights about power come from highly technical work that addresses relatively small and banal questions, or whether they come from more risk-taking work, from a sense of artistry.

Mitch: What you're saying resonates with me, Anna, but can you say more about what you mean by scalability and why it is problematic?

Anna: By scalability, I don't just mean certain conceptions of science or of research methods, although the idea certainly applies there as well. We should understand that "progress itself has often been defined by its ability to make projects expand without changing their framing assumptions. This quality is 'scalability.'. . . . A scalable business, for example, does not change its organization as it expands. This is possible only if business relations are not transformative, changing the business as new relations are added. Similarly, a scalable research project admits only data that already fit the research frame. Scalability requires that project elements be oblivious to the indeterminacies of encounter; that's how they allow smooth expansion. Thus, too, scalability banishes meaningful diversity, that is, diversity that might change things."[30]

Loïc: Could you give a concrete example of what you mean by progress being defined by scalability?

Anna: Think about "the European colonial plantation. In the sixteenth- and seventeenth-century sugarcane plantations in Brazil, for example, Portuguese planters stumbled on a formula for smooth expansion. They crafted

self-contained, interchangeable project elements as follows: exterminate local people and plants; prepare now-empty, unclaimed land; and bring in exotic and isolated labor and crops for production. This landscape model of scalability became an inspiration for later industrialization and modernization…. It was a success: Great profits were made in Europe, and most Europeans were too far away to see the effects. The project was, for the first time, scalable—or, more accurately, seemingly scalable. Sugarcane plantations expanded and spread across the warm regions of the world. Their contingent components—cloned planting stock, coerced labor, conquered and thus open land—showed how alienation, interchangeability, and expansion could lead to unprecedented profits. This formula shaped the dreams we have come to call progress and modernity…sugarcane plantations were the model for factories during industrialization; factories built plantation-style alienation into their plans…. By envisioning more and more of the world through the lens of the plantation, investors devised all kinds of new commodities. Eventually, they posited that everything on earth—and beyond—might be scalable, and thus exchangeable at market values. This was utilitarianism, which eventually congealed as modern economics and contributed to forging more scalability—at least in appearance."[31]

Loïc: Strong claims.

Alice: So research methods that prize scalability are like European colonial plantations? They are in league with colonialism and capitalist modernization?

Anna [laughing]: Perhaps it is not just that they are "like" European colonial plantations. Perhaps the relationship is closer than that. Perhaps modern research methods and the expansion of global capitalism have co-constituted each other, each drawing strength from the other and seeing in the other its outward sign of success.

Karen: There's a lot of talk about scalability here, but what about the non-scalable?

Anna: Yes, exactly, Karen. "It is time to turn our attention to the non-scalable, not only as objects for description but also as incitements to theory."[32] That's why I am so interested in matsutake mushrooms! Their existence relies on a highly non-scalable and relational ecology and political economy that, so far anyway, have resisted all efforts at imposing a plantation model. Matsutake mushrooms are my own incitement to theory.

Jim: So, then, Anna, if the dominant methods of modern science are plantation-like in their emphasis on scalability, do you think that ethnography as a method is "anti-plantation" in the same way that matsutake mushrooms are anti-plantation?

Anna: Matsutake mushrooms are anti-plantation in the sense that they cannot be farmed. But, fascinatingly, they often thrive in the ruins of abandoned scalable projects, in the ruins of capitalism. So, for example, in the US Pacific Northwest, matsutake mushrooms often thrive amongst the firs and lodgepole pines that replaced the great ponderosas once a plantation

model was applied to forestry. So it is not that matsutake mushrooms are absolutely "anti-plantation," but rather that they exist in spite of, or perhaps because of, scalable projects. So, I would say that ethnography, while not inherently anti-plantation, exists in spite of, or perhaps because of, scalable approaches to research. It is this quality that makes it more likely to be anti-plantation.

KAREN: I like the idea of using these mushrooms as an "incitement to theory," a way to think about the anti-plantation qualities of ethnography in relation to other methods that prize scalability. Might we say that ethnography is anti-scalable insofar as it privileges a rush of stories that sit uneasily with one another and that cannot be formulaically generalized across time and space? And that this ethnographic method can thrive within—indeed, is in some ways dependent on—the very ruins of scalability, both in terms of our notions of progress and modernity and in terms of our dominant notions of what constitutes good research?

SÉVERINE: Ethnography as a way of thinking about the possibility of life amongst the ruins of dominant research approaches: that's a provocative metaphor.

ANNA: Yes, and more specifically, an ethnography attuned to a rush of stories as a method. An ethnography that valorizes the arts of noticing.

TIMOTHY: We are now talking about ethnography as a kind of counter-discourse, a kind of counter-conduct within the dominant array of research methods. We are talking about ethnography as anti-plantation, and, by extension, as anti-colonial. But positing ethnography as anti-plantation, and, by extension, anti-colonial, curiously inverts the history of anthropology, the academic discipline most closely associated with ethnography as a method.

KAREN: Yes, and it's not a history we anthropologists are particularly proud of.

KATHERINE: What do you mean?

KAREN: Well, the history of ethnography within anthropology is bound up with projects of colonialism and empire. The European and American "discovery" of "new" lands and peoples developed in conjunction with the establishment of the study of those lands and peoples as a field of academic expertise. And that field in turn informed—some would even say served as a handmaiden to—colonial projects of power.

JIM: Ethnography became a way of making the unknown legible so that it could be conquered and ruled.

KAREN: Exactly. If you look at the history of ethnic highland "tribes" in Southeast Asia, for example, you can see very clearly how Western ethnographers and their colonially trained counterparts calcified identities that were previously incredibly fluid.

JIM: I would even argue that those identities were fluid by design, as escape hatches from oppressive structures.

KAREN: Right. But the work of ethnographers who entered those areas under the auspices of colonial and state power created fixed identities. So "author-

itative" field guides were created, stating that those who wear these kinds of garments and practice these kinds of rituals shall be known as the Lahu, and those who wear those kinds of garments and practice those kinds of rituals shall be known as the Akha, and so on and so forth.

Jim: Like the creation of a *Peterson's Field Guide for Birds*, except for highland peoples instead.[33]

Karen: And the effect of this was twofold. First, it offered colonial and state powers a ready-made grid onto which to map people. And because of the power behind that mapping, the mapping—which ethnographers thought corresponded to reality, but which was in fact only an artifact of the specific moment of entry and interaction of the ethnographer—actually inscribed and created new realities for the peoples being mapped.

Jim: In some ways, the combination of ethnographers and state power called those people into being and fixed them in relation to one another and in relation to colonial and state power.

Piers: It's a story you see replayed again and again in the history of anthropology and ethnography. Such that you could actually rewrite the history of ethnography as a history of spying, intelligence gathering, and incursion on behalf of empire.

Katherine: Wow, that's a strong statement!

Piers: Not just intelligence gathering, but the creation of new facts on the ground that those ostensibly being studied would then have to conform to in order to navigate the colonial or state realities that were being imposed on them through the gridlines of the "knowledge" that the ethnographers were producing.

Jim: The irony, of course, is that the only way we know this is with the help of other ethnographies. So, it's Edmund Leach's *Political Systems of Highland Burma* that gives us a sense for the fluidity of political structure and ethnic identity in highland Burma.[34] We wouldn't know about the artificiality of the fixing of identities independent of other ethnographic work.

Karen: True! And so we're caught in a kind of conundrum. The fallacies and follies of ethnography in the service of colonialism could only be undone by other ethnographic work.

Anna: A rush of stories.

Karen: But in both cases, it is still the production of knowledge by outsiders.

Jim: Yes, and the story is even more complicated still. In many cases, the ethnographic work done by Western anthropologists was explicitly meant to be protective of, rather than destructive of, the peoples it was studying.

Katherine: Protective from whose perspective?

Jim: An astute question! Take, for example, in the early 1950s, the Tribal Research Institute that was created in Chiangmai, Thailand. The whole stated purpose of this institute was to protect highland peoples from the pressures of state power. At the time, Thailand viewed highland peoples living in the

mountainous border region between Thailand, Burma, and Laos as threats to national security. These highland people were seen as poppy growers and opium producers and as potential communist sympathizers. The approach at that point was conversion or killing; they either were to become homogenized as Thai or else eliminated or driven across the border entirely.

Seeing this, several Western anthropologists and the sympathetic Thai anthropologists they worked with embarked on a project to catalogue and document the "tribes" of northern Thailand. They created a museum of tribal peoples that showed them in their "native" costumes and persuaded the Thai government to officially recognize nine tribal groups and hill minority peoples in Thailand.[35]

KATHERINE: So was that a good thing or a bad thing?

JIM: It was a complicated thing. On the one hand, it slowed or blunted some of the most oppressive policies of the Thai state towards these peoples. But on the other hand, much like all colonial ethnography preceding it, it froze, both in time and as established knowledge, a particular moment in a broader and more fluid series of power relations and identity constructions.

KATHERINE: It's almost as if you're saying that the anthropologists invented these ethnicities?

JIM: Yes, I think that's correct. They invented them in order to save them. But, in doing so, they harnessed the power of the Thai state and the legitimacy of Western academia to their cause, and these inventions in fact became the new realities on the ground. They set the parameters for identity within which these peoples either had to work with or push against.[36]

KATHERINE: So the moral of the story is?

KAREN: Well, I think one moral of the story is that even at its best-intentioned, ethnographic work is always implicated in larger projects of power.

KATHERINE: Well, OK, but isn't that true of everything: that it's implicated in larger projects of power?

KAREN: Sure, so as a general claim, maybe that's not so interesting or important. What matters is to always look at the what and the how of the implication and the nature of the projects of power themselves. And when we begin to look specifically at the ways ethnography has aided and abetted colonial regimes or state projects of control and domination, or when we look specifically at how anthropologists with progressive aims end up constructing dynamics that further control and domination, then I think we can begin to talk about the ways in which ethnography is always already a deeply problematic enterprise.

MITCH: Differently problematic from any other enterprise of knowledge creation?

JIM: Well, yes, I think so. Or at least more visibly so. Because ethnography takes as its stated aim the understanding of the other from the perspective of the other, it creates a much more tangible and palpable tension than other kinds of projects of knowledge creation that do not make those kinds of claims. Other projects of knowledge creation, you might say, are already much more

explicit about their aims and ends being separate from the aims and ends of those they purport to study.

Séverine: I'm not so sure that ethnography is the only method that claims to understand and represent the worldview or lived experience of those being studied. Think, for example, about survey research, which is also trying to get at how those being studied understand the world.

Mitch: You might even say that large-N regression analysis and formal modeling, in their own registers, are trying to represent some aspect of the lived experiences of those they study. Of course, ethnography is typically the method that does this in its most concentrated form, but I think we are talking about differences of degree, not of kind. After all, if there's no point of connection at all, no place where the method meets up with lived experience, then what is the point of the method?

Jim: Good points. But with ethnography, I think there is a singularly unique emphasis on the accessing of participant meanings. And this emphasis often leads ethnographers to question whether surveys, and even some types of interviews, are really able to capture those meanings.[37] And certainly to question whether large-N regression analysis and formal modeling are capable of accessing the lived experiences of the researched from their own internal perspectives.

Piers: Yes, and the intensity of ethnography's interest in the lived experience of its research subjects is true, I would argue, even for ethnographers who ultimately and self-consciously impose interpretations that their subjects would disagree with.

Karen: What do you mean?

Piers: Well, some ethnography—we might call this ethnography that employs a hermeneutic of trust—explicitly takes as its criterion an attempt by the ethnographer to enter into and represent the understandings that the ethnographic subjects have of themselves and their worlds. The ethnography, in this hermeneutic, is a success or a failure based on the degree to which it accurately conveys how the subjects themselves construct and understand their worlds. Other ethnography—and, following Paul Ricoeur, we might call this an ethnography that employs a hermeneutic of suspicion—does not make itself exclusively beholden to its accuracy in representing the subjects' self-understandings for its legitimacy. In this kind of ethnography, the researcher is free to, and indeed often expected to, impose interpretations that might run quite counter to the subjects' self-understandings.[38]

Mitch: Can you give an example?

Piers: Well, in my own work, I think there are examples of both kinds of hermeneutics being applied. When I studied the Eveny reindeer herders in Siberia, I quickly became alert to the power of omens and dreams in their social lives. Initially, as a Western ethnographer, I approached the omens and dreams rationally and with skepticism. Rather than being expressions of what the

future held, I interpreted these dreams and omens psychoanalytically in accord with my own training as a Western academic. I saw the dreams and omens as expressions of repressed anxieties, desires, and tensions.

MITCH: OK. And what about the hermeneutics of trust?

PIERS: Well, as I spent more and more time in the field, I found myself becoming inducted into an entirely different way of understanding. Inducted not just as a researcher or an ethnographer, but as a person. It was as if my years of graduate training were slowly being eroded and replaced by the ways of seeing presented by the Eveny. So, one of the final chapters of my book is entitled "How to Summon a Helicopter," and it reflects my own adoption of the Eveny ways of seeing the world, in quite clear opposition to the Western modes of thinking that I had started out with. And because the book reflects over twenty years of fieldwork amongst the Eveny, there are parts of it that are written with a hermeneutic of trust and parts of it that are written with a hermeneutic of suspicion.

MITCH: You know, "sometimes whether or not a story is true or false is really not important. What matters a lot more is that the story somebody tells can be an index to the kind of person that they are or the kind of life that they live."[39]

ANNA: I really agree with that point, Mitch. I think sometimes we fixate on the facticity of the things we hear in the field to the neglect of broader and ultimately more generative questions about what kinds of work the stories we hear are doing. Political work, social work, identity-building work, and even performative work in the context of a relationship between the teller and the ethnographer. I'm reminded of stories I heard in the fieldwork I did for *In the Realm of the Diamond Queen* about government head hunters in Kalimantan, Indonesia. I spent a bit of time trying to figure out if the stories were factually "true"—where were the graves for the bodies of the people killed for their heads?—before realizing that what was most central was that the stories said something extremely important about how the storytellers saw their relationships with government officials.[40]

KAREN: That's a nice invocation of the hermeneutics of trust and of suspicion, Piers. And I take your points, Mitch and Anna, about the facticity of what ethnographers hear in the field sometimes being less important than the work those stories do. But I would also make the point that regardless of which hermeneutic the ethnographer is employing, sussing out the facticity of stories or taking them as an index to the social situation of the storyteller, ultimately the legitimacy and authority of the ethnographic claims still rely on a bedrock assertion that the ethnographer is able to—has been able to—enter into a shared lived experience with those she studies. And this, I think, is what makes ethnography so visibly troublesome as a method, given its historical and contemporary entanglement with projects of dominating and oppressing power.

Jim: Right, and therefore, when ethnography is so blatantly used to oppress or dominate or even to create out of thin air the groups it studies, there is a more apparent tension than with other methods of knowledge creation.

Alice: OK. So, is the answer to stop making claims about representation, to be more naked and truthful about ethnography's imperialist ambitions, or is the problem with all knowledge creation regardless of what its claims are?

Karen: Deep, important questions. One thing that's interesting is how anthropology has taken a serious turn in recent decades towards critically examining its own history and its own complicities with projects of colonial and state power. A lot of the substance of anthropology in relation to ethnography is no longer about the actual conduct of ethnographic fieldwork but rather about the examination of the power relations between researcher and researched.

Timothy: Some have even said that the whole enterprise of anthropology has become so consumed by these questions that it has become an exercise in navel-gazing.

Karen: I think that's an easy enough criticism for anyone to make. But even recent history shows us that these debates are far from settled and that critical self-reflection is anything but navel-gazing.

Timothy: How so?

Karen: Take, as the latest example, the brouhaha over the United States military's so-called Human Terrain System (HTS), operative from 2006 to 2014.

Jim [sighing]: Ah, yes.

Karen: The $725 million-dollar program recruited anthropologists and other social scientists to aid the United States military in places like Iraq and Afghanistan. In small units that included armed soldiers, these anthropologists entered villages and towns and "mapped" the local "human terrain" in service of US aims. Montgomery McFate, a Yale Ph.D. in anthropology and one of the early champions of the program, published several essays arguing that cultural knowledge is an integral part of warfare and celebrating anthropology's return to its roots as a "warfighting discipline."[41]

Timothy: Yes, and many of these anthropologists also used the information gleaned while working for the US military in warzones as the basis for academic articles and dissertations.[42] I don't think you could find a more blatant example of how ethnography is, even today, tied up with projects of imperial power.

Karen: The debates that HTS has ignited within the American Anthropological Association (AAA) have been fascinating. In 2010, several hundred anthropologists sent a signed statement to the Speaker of the US House of Representatives expressing strong opposition to the HTS. They noted that the executive board of the AAA had determined in 2007 that the use of anthropologists and ethnographers in the direct service of war was unacceptable.[43]

Jim: To be provocative, isn't HTS simply a formalization of a longstanding power dynamic that has characterized ethnography's relationships to its subjects from its very founding as a method? That is, one in which the knowledges produced by the ethnographer are enlisted, whether explicitly or not, whether with the express consent of the researcher or not, in the service of projects of control and domination?

Karen [upset]: Are you saying we should not oppose projects like the Human Terrain System?

Jim: No, of course we should. But we should also stop to ask ourselves about how other kinds of ethnography, even seemingly innocent ethnography, might contain undercurrents similar to those made painfully explicit by HTS. There is a way in which the overt and highly vocal opposition to this one program might serve to deflect attention from the ways other kinds of ethnographic work also serve the agendas of those in power, agendas that the researchers who produce that ethnographic work might themselves be opposed to.

Karen: Like what?

Jim: The feminist anthropologist Kathleen Gough wrote a terrific piece in the late 1960s in which she argued that anthropology is basically a child of Western imperialism.[44]

Loïc: That seems like a rather sweeping claim. I've also heard of anthropology referred to as the handmaiden of colonialism and the tool of colonialism.

Jim: Yes, but I like the way Gough puts it. She lays out the position of the anthropologist very nicely, likening her to the white liberal reformer. Anthropologists, especially the first ones, were located higher in terms of social status than the people they studied, and were usually white. This put them in the curious position of benefiting from imperialist and racist orders while at the same time often trying to soften or protect their research subjects from the worst effects of those same structures.[45] It's an inherent contradiction: the anthropologist is there because, and only because, of the very colonial power whose worst effects she is often trying to blunt.

Karen: Yes, as Talal Asad writes, from the beginning the very enterprise of anthropology, and of anthropological ethnography in particular, has been tied up with the context of power in which it was born.[46]

Piers: All these terms—handmaiden, child, tool—make it sound like anthropological ethnography was somehow central to projects of power. And they risk making it sound like projects of domination and exploitation somehow continue to rely centrally on ethnographic work to advance their aims. Yet I think it's in that same essay of Asad's, Karen, where he makes a point of cautioning against attributing too much centrality to anthropology's contributions to colonial power.[47] After all, colonial power also relied heavily on the knowledges being constructed by missionaries, traders, travelers, and bureaucrats.

Anna: All of whom were traveling on the tailwinds of colonial power and dependent, in one way or another, on the success of the colonial project.

Karen: According to Oscar Salemink's history of ethnographies in the Central Highlands of Vietnam, the earliest anthropologists were preoccupied with establishing their legitimacy as scientists and professionals, unlike "mere" missionaries, administrators, and travelers.[48]

Piers: That's so true! Bronislaw Malinowski, considered by many to be one of the most influential founders of modern fieldwork methods, goes to great lengths to establish the scientific credibility of professional anthropology. You could read the entire first section of his famous *Argonauts of the Western Pacific* as an attempt to show how academic anthropology is scientific and rigorous where the accounts of missionaries and travelers are not.[49]

Anna: That is accurate but incomplete. Malinowski argues against an additional antagonist in that classic essay: those who would advance academic knowledge from afar, without grasping the lived experiences of those they claim to study. In contrast, Malinowski argued for what he called the "imponderabilia of actual life," and he insisted that the ultimate goal of the ethnographer is "to grasp the native's point of view, his relation to life, to realise *his* vision of *his* world."[50]

[Laughing.]

And that's a direct quote that I still have memorized from my days in graduate school!

Jim: Yes. So there's this double-sided charge against early ethnographers: on the one hand, they were like spies or agents of the state, agents of imperialism. Then, on the other hand, they were not different from travel writers, traders, and missionaries. Against those two charges they had to create a space of their own that insisted on their autonomy, their immersion, and their scientific credentials.

Karen: I don't know how much autonomy from the colonial state they were seeking. Malinowski may have championed the importance of the "imponderabilia of actual life," but his essay on "Practical Anthropology" argues for the importance of pressing such anthropological knowledge into the service of colonial administrators.[51]

Piers: That's not the whole story, though. Across the Atlantic, Franz Boas, the other founder of modern anthropology, was waging his own battle against military and colonial applications of anthropology. In 1916, Boas published an essay in *The Nation* decrying four unnamed anthropologists for using their status as academics as a cover for intelligence gathering and spying activities in South America.[52] Boas was furious about the use of academic cover as a pretext for espionage, and wrote a fiery missive exposing, but not directly naming, the four anthropologists. He even said that the academics had "prostituted science!"[53]

Karen: Quite telling to see the nearly opposite stances taken on the right relationship between anthropology and state power by two of the most important founders of the discipline. While Malinowski was penning essays encouraging the use of anthropological knowledge in the service of the colonial project, Boas was exposing espionage activities that used academic credentials as a cover. This tension suggests a schism at the earliest moments of the method about the ethical relationship between ethnography and power and continues to play out in the recent debates over HTS.

Loïc: Quite telling also to see how furiously Anglo-centric this whole discussion is, with no mention whatsoever of Francophone contributions to early anthropology, and in particular the signal importance of Marcel Mauss to the development of both anthropology and sociology![54]

Jim: That's a great point, Loïc, but again I'd just caution us to return to the insight that the entire discipline—whether we are talking about in the UK, the US, or France—had as a condition of its existence the colonial project.

Karen: Indeed. The involvement of star anthropologists with war projects exploded during the Second World War. Anthropologist Gregory Bateson, known for his ethnographic research in New Guinea that was published as *Naven* in 1936, served with the Office of Strategic Services (OSS) in the Arakan Mountains of Burma and aided with the development of propaganda. Margaret Mead also worked with OSS to establish a Far East psychological warfare unit. And a Harvard professor of anthropology named Carleton Coon trained and smuggled weapons to resistance groups in German-occupied Morocco and wrote about it in *A North Africa Story: The Anthropologist as OSS Agent, 1941–1943*.[55]

Piers: To this list we can add Ruth Benedict, who in 1943 headed the Basic Analysis Section of the Bureau of Overseas Intelligence of the US Office of War Information. While there, she authored a book that combatted the racial superiority theories of the Nazi regime. Titled *Races of Mankind*, it sold more than 750,000 copies and was even turned into a Broadway musical.[56] Later, from 1947 to 1952, Mead and Benedict would form the "culture-at-a-distance" program at Columbia University with direct sponsorship and oversight from the US Office of Naval Research. The goal of the program was to reconstruct whole cultures from afar using artifacts and textual sources when wartime conditions would not permit close, immersive ethnographic encounters.

Karen: All throughout the 1950s, anthropologists continued to contribute actively to military efforts. Major General Edward Landsale used anthropological knowledge about folklore amongst the Huk in the Philippines to devise psychological warfare operations that included, for example, snatching the last person in a Huk guerrilla patrol, killing him, puncturing his neck with two holes, then placing his body back on the path in order to exploit Huk fears of *asaung*, or vampires. Also in the 1950s, Charles Bohannan, who had

advanced anthropology degrees, co-authored *Counter-Guerrilla Operations: The Philippine Experience*, which is still widely respected in the military as a classic in counterinsurgency literature. In 1959, Bohannan led a secret team in Colombia for the US military, traveling more than 14,000 miles and using anthropological and ethnographic techniques to interview more than 2,000 officials, civilians, and guerilla leaders. He published the results of this work in a three-volume report.[57]

Séverine: Probably worth keeping in mind here that not all military endeavors are necessarily colonial or imperialist. It sounds like at least some of these anthropologists were involved in aiding resistance efforts. And certainly many contemporary peacekeepers, including military peacekeepers whom I did research among, were not there for colonial or imperialist purposes.

Anna: True, but one person's resistance is another person's occupation. And the larger point is that anthropologists were explicitly using their craft to help state projects, whatever your normative judgment of those state projects might be.

Piers: Continuing with this history, the mid-1960s and early 1970s is when things really reached a boiling point. In 1964, the Special Operations Research Office (SORO), founded by the US Department of Defense in response to the particular challenges of counterinsurgency wars, partnered with American University in Washington, D.C. to start Project Camelot, a research program whose aim was to identify what made societies vulnerable to internal warfare and conflict, and to manipulate those factors so as to either encourage or discourage internal wars. The first target of Project Camelot was Chile; its lead researcher was Hugo Nuttini, a professor of anthropology at the University of Pittsburgh. Although Nuttini tried to be covert about the project's military origins, the Chilean government found him out and filed a formal diplomatic protest with the US Ambassador. After the Congressional hearings that followed, Project Camelot was cancelled in 1965.

Karen: Yes, the blowup over Project Camelot and the politics of what is known in the United States as the Vietnam War in the subsequent decade led many in anthropology to renounce any direct service to the military and the interests of empire. And this renunciation set the stage for increased and ongoing scrutiny of the historical, ethical, and epistemological origins of anthropology as a discipline, as well as to the sources of ethnographic authority.

Jim: It's interesting, though, that even though she writes from that same spirit of scrutiny, Kathleen Gough calls for nearly the same kinds of research as Project Camelot was after, but does so with an anti-imperialist sensibility.[58]

Anna: Like I just said, one person's resistance is another person's occupation.

Jim: Exactly. The problem, in my view, is not primarily one of motivations. McFate wants ethnographers to create knowledge for the US military. Gough wants ethnographers to create knowledge that resists imperialism. But both are creating knowledge all the same. Gough's argument—that

progressive, critical anthropologists go after the same kinds of questions as the ones that are of keen interest to the Department of Defense and the economic interests it represents, but that they do so in their own way—seems to ignore the possible uses of that information regardless of our motives in creating it. Let's say we are pro-insurgent researchers operating in Guatemala. We would like to see an overthrow of the current US-supported regime and its replacement by a socialist government with a redistributive agenda. So we begin to conduct research on the questions that Gough lays out: who the guerrilla leaders are, who the labor organizers are, what the likely conditions are for their successes, and what they have learned from other resistance movements elsewhere. We conduct all of this research as truly independent scholars, taking not a penny of money from the US military or even from the US government. What then? Will our findings be of any less interest to—or, more to the point—of any less usefulness to the US military and the US government?

Alice: This question could certainly be raised about my work.

Timothy: Mine too. And yours too, Jim, for that matter.

Jim: Agreed. But it gets us back to an issue I have been trying to raise all along. What, in the end, distinguishes any ethnographer from a spy? Or a merchant? Or an administrator? Or a missionary? Or a journalist? Or from anyone else in the business of gathering information and disseminating it? Information on people who are below them in social status, information on people who, despite their best intentions of providing aid, are only being made more legible, and therefore more controllable and more vulnerable to the designs of those who wish to oppress them? If the history of ethnography's relationship to colonial power has anything to teach us, it surely must be that it is not primarily a question of motivation or intention, but rather one of consequences, of effects, that most matters.

Karen: So, I wonder where all this leaves us in terms of understanding ethnography as anti-plantation? From this history, it seems ethnography is quite capable of serving the needs of capitalist and colonial power.

Anna: Yes, of course I agree. It's not so much that ethnography is necessarily anti-plantation, but that of all methods in the social sciences, it contains the greatest potential to be anti-plantation.

Jim: Why's that?

Anna: Because of its insistence on the increasingly archaic arts of noticing, and because of the space it creates to tell a rush of stories, stories that interrupt scalability and linear notions of temporality and progress.

Karen: So it's possible to think an anti-plantation ethnographic method, one that's needed for our times? Isn't this just a new version of Gough's call for an anti-imperialist anthropology? And what keeps the information generated by these ethnographies—these rushes of stories—from being co-opted by projects of domination?

ANNA: Nothing. There is no guarantee. But I do think it's possible to think and write and listen in ways that contest rather than contribute to domination, and that—its troubled history notwithstanding—ethnography provides us with some of the best tools for doing so.

MITCH: As I listen to this genealogy of ethnography in anthropology, I can't help but think about the equally troubled and troubling history of ethnography within the discipline of sociology.

LOÏC: Me too, Mitch. It's lamentable how often ethnography is automatically associated with anthropology when, in fact, it has roots in sociology that are just as deep. Some of the very first ethnographies were produced not by anthropologists studying faraway places and cultures, but rather by sociologists and activists conducting empirical studies of their own cities and countries. Ethnography was first honed to study the "savages of the interior" of the Western world, workers, immigrants, and peasants. Read Friedrich Engels's *The Condition of the Working Class in England*, first published in 1845![59]

MITCH: Yes, exactly. We already talked about anthropologist Bronislaw Malinowski's *Argonauts of the Western Pacific*, which is seen by many as one of the founding examples of ethnography. But Malinowski didn't publish his book until 1922, well after some of the first ethnographic studies of urban communities had been published.

PIERS: Which studies?

MITCH: As Aldon D. Morris points out in *The Scholar Denied: W.E.B. Du Bois and the Birth of Modern Sociology*,[60] one of the first recognizable ethnographies produced by a self-identified sociologist is Du Bois's *The Philadelphia Negro*, published in 1899.[61]

JIM: I've always associated Du Bois with *The Souls of Black Folk*![62]

MITCH: I think a lot of people don't realize that in addition to being a theorist of race in the United States, Du Bois was also a pioneering ethnographer.

TIMOTHY: I've always heard that ethnography in sociology began with *The Polish Peasant in Europe and America* by William I. Thomas and Florian Znaniecki published in five volumes from 1918 to 1920.[63] In the story I usually hear from my sociology colleagues, *The Polish Peasant* was a founding text of their discipline and a landmark transition away from armchair theorizing to deep empirical engagement with the world.[64] And that Thomas was deeply shaped by—and in turn shaped—the Chicago School of Sociology of the early 20th century and the symbolic interactionists who are mostly closely associated with ethnography in sociology.[65]

LOÏC: I would dispute that conventional history. I think the real movement from armchair theorizing to deep empirical engagement actually occurred in the work of the Durkheimians around the same time. But no matter whether we attribute the rise of ethnography in sociology to Du Bois or Thomas and Znaniecki or the Durkheimians, the larger point still stands: sociology has as

much or more of a claim as anthropology to the creation of academic ethnography as a mode of research and writing.

KAREN: Wait, am I hearing this right? W.E.B. Du Bois published his ethnographic work decades before *The Polish Peasant?*

ALICE: That's exactly right, Karen! Long before *The Polish Peasant*, Du Bois was conducting studies of race and race relations in the United States, drawing in particular on the earlier work of social reformers Jane Addams and Charles Booth. They argued that a close-to-ground empirical understanding of poverty in urban areas—one that used maps, census data, descriptive statistics, in-depth interviews, and ethnography—was essential to creating positive change.[66]

ANNA: So why don't we typically hear much about Du Bois in relation to the origins of ethnography?

ALICE: Aldon Morris's *The Scholar Denied*, which Mitch just referenced, demonstrates how anti-Black racism prevented Du Bois from being hired in a top US department after his return from Germany, where he had studied alongside and deeply influenced the intellectual development of one of his contemporaries, Max Weber. And Morris shows how Du Bois, despite a lack of resources, pioneered a new school of empirical sociology at Atlanta University, long before the Chicago School became prominent.

MITCH: That's right. Du Bois strongly believed in the power of empirical sociology to combat the anti-Black racism prevalent in society and the academy. His research for *The Philadelphia Negro*, for example, was funded by Susan P. Wharton and facilitated through the University of Pennsylvania Sociology Department, which wanted a black scholar to carry out research on blacks in Philadelphia because they thought it would therefore be more credible.[67] As Du Bois would later reflect in his book *Dusk of Dawn: An Essay toward an Autobiography of a Race Concept*, he saw the research as an opportunity to use ethnography and other close-to-the-ground empirical methods to fight back against the racist theories of his time, including those held by his funders.[68]

TIMOTHY: Wow, sounds like a pretty anti-plantation way to deploy ethnography!

MITCH: If we take seriously Morris's claim that Du Bois was a pioneer of the ethnographic method, it's not just that Du Bois deployed ethnography in an anti-plantation way, but that he actually *developed it* for its anti-plantation capacities.

ALICE: And as is often the case in academia, the story gets even more personal! Robert E. Park, the man most often credited as being the genius behind the Chicago School of sociology, worked as an assistant to Booker T. Washington before he ended up at the University of Chicago. Washington, of course, was one of Du Bois's main rivals, primarily because he espoused a conservative and racist vision of blacks in America, arguing that blacks were primitive and that their best hope lay in vocational training for manual and service labor. Because Washington's views were safe for the system of white supremacy,

white funders were content to let Washington serve as a gatekeeper to funding and support.

Park worked as Washington's ghostwriter and assistant for seven years, and he agreed with Washington's views of race relations in the US and held similar ideas about the status of blacks. According to Aldon Morris, Park and others at the Chicago School wanted to create an academic approach to understanding white supremacy as a positive good. They argued that both African colonization and African slavery were important steps in lifting blacks into the superior civilization of whites.[69] What's more, while working for Washington, Park perpetrated a smear against Du Bois and then, once at Chicago, proceeded to appropriate Du Bois's work without acknowledgment.

MITCH: And here's perhaps the greatest irony of all: the racist perspectives of Robert E. Park and other members of the Chicago School were presented under the guise of neutral, universal science, while Du Bois was portrayed as a partisan and denied funding because of his lack of "objectivity." Park took for granted white biological and political supremacy, all the while simultaneously telling his students at Chicago that their role was to be calm and detached scientists who investigated race relations with objectivity.[70]

LOÏC: So, the supporter of the racist status quo represents himself and his method of study as objective and detached while portraying competing scholars who use close-to-the-ground methods to challenge that status quo as crusading partisans! And so it is that the contributions of Du Bois, and the seminal role he played in pioneering ethnography and other close-to-the-ground research methods as a way of combatting scientific and societal racism, remain repressed to this day.[71]

MITCH: That's a damning counter-history. Damning not only on account of the racism that Du Bois faced personally, but also on account of the founding racisms of sociological theory itself.

ANNA: Yes, but it's also a hopeful history.

MITCH: How so?

ANNA: It suggests, as Du Bois himself believed, that ethnographic work, work that is close-to-the-ground, work that interrogates made-in-the-academy racist theories with the complexity and specificity of lived experience, contains the power to counteract these theories, to locate them and place them as the handmaidens of power that they are.

LOÏC: Yes, or more pessimistically, and perhaps more realistically, it also shows how ethnography, on its own, is not capable of countering anything. After all, if Aldon Morris's counter-history is correct, then Park and the other founders of the Chicago School were more than able to advance theories about natural racial hierarchies and biological determinism using ethnographic methods.

ALICE: True, but that itself is an ethnographic insight, insofar as it shows that power shapes what counts as legitimate knowledge, that there is no sphere of

knowledge independent of power.[72] Du Bois himself practiced a science that engaged with, rather than detached from, the power relations shaping both its practitioners and the "subjects" of its study. For Du Bois, "One could not be a calm, cool, detached scientist while Negroes were lynched, murdered, and starved";[73] he saw the purpose of his life's work as "leading to the emancipation of the American Negro." In his words, "history and the other social sciences were to be my weapons, to be sharpened and applied by research and writing."[74]

Anna [admiringly]: Sounds like you have that memorized, Alice.

Alice [laughing]: I do.

Timothy: I'm also interested in the ways power operates internally, within the conduct of ethnography itself. I'm familiar with the passage that Alice referenced earlier from Du Bois's *Dusk of Dawn*. One of the things I find most intriguing about it is that Du Bois writes about not only the racism of his white financial backers, but also the resistance to his research from blacks in Philadelphia. "Are we animals to be dissected and by an unknown Negro at that?" he imagines the blacks of Philadelphia asking in response to encountering him as a researcher.[75] It's a provocative line, because it highlights the way in which this sociological and ethnographic movement inward to the inaccessible urban center was—even for Du Bois—still marked by a crossing of boundaries and an establishment of power relations which mimic the move by anthropology's later ethnographers to cross the boundaries of nation-state and language. Du Bois was alert to this tension and to the resistance on the part of his "subjects" to being treated like animals to be dissected.

[Wolfdog growls and shows her fangs. Silence for a long moment as the ethnographers look at her with concern before Timothy continues.]

So despite its focus on the "local" and the "urban," there is still an exoticization of the other that is possible even within sociological work that is ethnographic.

Piers: There's that fascinating fragment, "and by an unknown Negro at that?" So, Du Bois, chosen by his white racist funders because of supposed "insider" status as a fellow black person, recognizes his own outsider status in relation to the communities he is studying.

Alice: Du Bois was known, actually, for being quite reserved and aloof, for dressing conservatively, carrying a walking stick, and for speaking in a New England accent.[76] So it's intriguing to think of him in the seventh ward of Philadelphia, administering surveys and conducting interviews. The animosity that he anticipated towards his research and even himself as a researcher shows that there can be gaps to bridge even when others perceive the researcher to be "native" to a situation.

Mitch: Yes, absolutely. And when the researcher is clearly an outsider, the tensions are even more obvious. It's a charge that's sometimes been leveled against my

work in both *Slim's Table* and *Sidewalk*.[77] I'm a white Jewish male, so what gives me the right to study poor, black men? And in particular, what makes me think I am able to understand them and make claims about them that they themselves might not agree with?

PIERS: Right. So are you a kind of modern-day colonialist ethnographer?

MITCH: That's the implication.

PIERS: Well, what's your response to those charges?

MITCH: This is a complicated question that I've done a lot of thinking about.[78] The first thing to note is that I always understood there to be a metaphorical chain-linked fence that existed between me and those I was studying.

KATHERINE: A chain-linked fence?

MITCH: Yes, the metaphor comes from Elliot Leibow's *Tally's Corner*, which, alongside Carol Stack's *All Our Kin*, still offers, to my thinking, one of the best exemplars of how to think about the privilege of white ethnographers in the United States who study nonwhite others.[79]

TIMOTHY: These themes seem to recur over and over throughout the history of ethnography.

KATHERINE: So what about the chain-linked fence?

MITCH: Well, the metaphor suggests that there is a very real racial separation, one that is informed by "the historical relations between the kinds of people being studied and the kind of person doing the study, in this case a privileged white man and a group of poor US black descendants of slaves."[80] But it also suggests that this separation does not need to be absolute, that there is an opening for, as Leibow demonstrates in his own work, "the possibility of a white man entering into a serious dialogue with the lives of poor blacks and producing a book that was not nothing, that gave you something significant, even if was not a full understanding, which we can never get."[81]

PIERS: I like the metaphor. But it still leaves the question of how you negotiate the specific relationships in the field.

MITCH: One way I deal with them is by making sure I include as much of the voices of my participants as possible. In *Sidewalk*, I even invited one of my participants, Hakim Hasan, to write an afterward to the book and co-teach a seminar with me at the University of California, Santa Barbara.[82] I see this not as an attempt at resolution but as a way of recognizing the tensions and of trying to move through them by redistributing ethnographic authority.

ANNA: I wonder if that really gets around the problem? Selecting one or two model subjects and granting them symbolic co-authorship or inviting them to teach with you might only serve to obscure the larger power dynamics at play.

MITCH: My aim is not to obscure the larger power dynamics but rather to surface them so that we can acknowledge them and think about them. As I said,

inviting Hakim to write an afterword to the book or co-teach a seminar does not resolve the tensions. If anything, it makes them more available to the reader. This is what I mean by redistributing ethnographic authority.

Loïc: What qualifies a member of a given universe to co-teach a seminar on his own universe? Since when is the average person a sociological expert on their life as opposed to just another informant? That's the real question that's avoided here in the name of seeming "inclusive" and "respectful" of the subject of research, and it is epistemological nonsense and political farce. Would you do it if you were studying 'up'? Would you invite the prison warden and the prosecutor to co-teach a course on the ethnography of incarceration? You wouldn't, for fear of being seen as a shill.[83] But perhaps I can also speak a bit to this from a different perspective, as several of my critics charged me with a kind of imperialist ethnography of the boxing gym.

Piers: How is that?

Loïc: Well, I am a white Frenchman who claims in my book to have somewhat sidestepped or superseded the problem of white–black racial relations in the US more broadly and on the South Side of Chicago more specifically by virtue of my nationality. I further make the claim that in the boxing gym, and especially in the squared circle of the ring, race is conditionally suspended as it gets pushed to the background and overtaken by properties more relevant in the urgency of practice: do you have a good left hook, can you exchange sparring sessions, do you live by the ethic of "sacrifice"?[84]

Mitch: And isn't it also the case that in your book you go to some lengths to persuade readers that you've been accepted by the black men in the gym as one of their own?

Loïc: Well, in fact, I think I was accepted that way. My whiteness was interpreted by the members of the boxing gym through the lens of my Frenchness, which I came to view as a kind of propitious prop. As a non-US citizen, and as a cultural outsider, my racial naïveté, in US racial terms, really played to my advantage. As my ringmate Ashante once put it, "'French people ain't crackers, they always had good rapport with blacks, goin' all d'way back,' by which he meant that a Frenchman is not directly implicated in the bitter black-white dualism that organizes American society and may even have cultural affinities with his community."[85]

Mitch: I'm not sure that's entirely persuasive, though. I think it's a bit naïve to accept at face value comments like these, believing that we have become one of them. "I am reminded of my own experience of being told by poor blacks that they and I have a special rapport, owing to the relationship between Jews and blacks during the civil rights movement. I would later learn that the very same people were saying anti-Jewish things behind my back. I am skeptical that any white researcher could ever really know what his poor black subjects think of him on the basis of the things they say to him."[86] This is why I keep returning to the metaphor of the chain-linked fence first articulated by Leibow.

In ethnography, as in life, the possibility certainly exists for extraordinary moments of solidarity and even friendship across race and class lines, but we should not be overly romantic in ways that lead us to imagine a complete dissolution of the chain-linked fence erected by long and still ongoing histories of racial domination.

ALICE: I think this raises an important question about the role that ethnography has historically played in the discipline of sociology. This tradition of relatively privileged, outside academics studying various aspects of the social lives of classes ranked lower on society's hierarchy continues largely to this day. We can think, for example, of Sudhir Venkatesh's *Gang Leader for a Day*, an ethnography of his research on gang life, also on the South Side of Chicago.[87] Both Loïc's research for *Body & Soul* and Mitch's work in *Sidewalk* could be similarly understood. And as I'm sure we'll hear if The Prosecutor ever shows up, my book, *On the Run*, has also been explicitly criticized along these lines.

ANNA: Well, not to bring it back to anthropology, but I think Karen's book is also unique in this way.

TIMOTHY: How so?

ANNA: *Liquidated* is very much a book that "studies up," to invoke anthropologist Laura Nader's felicitous phrasing. Nader was writing in the 1970s, but her argument that anthropology, and by extension, ethnography, needs to do much more to study those in positions of power in society, to study the colonizer rather than the colonized, the affluent rather than the culture of poverty, continues to be relevant today.

KAREN: I agree, Anna. And Nader's point isn't just about what knowledges are produced by whether ethnographers study up or down, but also about how the power relations we have with our so-called "subjects" exert an influence over how we ask questions and define problems in the first place. Studying up, Nader argues, might lead us to turn a lot of our conventional questions on their heads. Instead of asking why people are poor, we might explore why some are rich. Instead of studying the ghetto, we might study the institutions—banks, real estate agencies, and school districting—that create those areas.[88]

PIERS: But think of all the problems with access when it comes to studying up.

JIM: Yes.

KAREN: I think that's exactly Nader's point, though. She's raising a critically important question: does ethnography inherently favor objects of study that place the researcher in a dominant–subordinate relationship with her "subjects"? And could part of the reason for that be that access is typically easier when one is studying down than when one is studying up?

TIMOTHY: You could take it a step further, if you wanted, and critically ask whether urban sociology specifically, and ethnography more generally, tends towards

a voyeurism of the poor, or the perverted, or the deviant. There is a long and distinguished list of reputations and careers that have been built on ethnographies based on the infiltration and elucidation of subcultures otherwise closed off to a middle-class readership base. This critique could be made of my book too, based on undercover work in the slaughterhouse.

Jim: It's not just academically driven work, either. Think of *Nickled and Dimed* by Barbara Ehrenreich.[89] Work of that kind deserves a mention inasmuch as it involves full immersion in another life-world, trying to "live that life." It's interesting that our own society's poorest people are so remote from our experience that we have to send anthropologists to them as if they were a tribe in the New Guinea Highlands.

Loïc: I object to being lumped into this critique. The object of my study was carnal sociology, not a voyeuristic look at what it was like to be a member of a black boxing gym.

Anna [laughing]: Well, Loïc, surely you can't deny that entering a hidden world was also part of the attraction for your readers? After all, you even wrote part of *Body & Soul* as a sociological novella!

Loïc: Well, the narrative I offer in *Body & Soul* is just a "prelude and stepping-stone to a second, more explicitly theoretical work."[90] It's meant to elaborate the notion of habitus and aims to contribute to a sociology of flesh and blood capable of capturing the carnality of social action.[91]

Anna: Yes, but the theoretical work in *Body & Soul* is inseparable from the visceral, voyeuristic thrill of entering a boxing gym, a space hidden and unknown to the vast majority of us.

Timothy: You can't have the object of study without the locus of study. That's what we were arguing before. In ethnography, no theoretical diamonds can be washed clean of the mud in which they were found.

Piers: Nor would we ever want to wash them clean. That's the beauty and power of ethnography.

Timothy: I completely agree, Piers. In my ethnography of an industrialized slaughterhouse, I even warn my readers against the "impulse to thumb through the pages so as to locate, separate, and segregate the sterile, abstract arguments from the flat, ugly, day-in, day-out minutiae of the work of killing."[92]

Katherine: But wait, can't the thrill also be in the other direction? There is a reason why celebrity-focused magazines and tabloids sell so well.

Anna: Oh, I see. So we could have *Wall Street Investment Banker for a Day* and *Elite Prep School Student for a Day*.

Alice: Yes, or in the case of Ashley Mear's book on the New York City fashion industry, *Top Fashion Model for a Day*.[93]

Jim: You know, whether studying up or down, it's not always outsiders who offer the most compelling accounts. Sometimes it's insiders who have decided to write ethnographically about their own social situations. Think here of Ben

Hamper's *Rivethead: Tales from the Assembly Line* on one end and John Perkins's *Confessions of an Economic Hit Man* on the other.[94]

Karen: Whether or not all ethnography indulges a voyeuristic impulse is one question, but Nader's point is different. She would say, I think, that the political, normative, and theoretical consequences of directing the ethnographic gaze predominantly downward are serious and important.

Timothy: I'm fascinated by this discussion of the history of ethnography within anthropology and sociology, but I think it's important to remember that ethnography has also been taken up in a wide range of other disciplines, including political science, education, organizational studies, nursing, and even journalism, and that the word ethnography takes on different valences within each of those disciplines.

Katherine: Yes, that's a good point. And I think there are particular debates and tensions and "schools" of thought about ethnography within each of those disciplinary histories.

Piers: And yet—returning to the situation at hand [looks at wolfdog on the ground]—I would say that it is not our disciplinary or even academic identities that matter most here, but rather our common practice of deep, immersive research and writing of some kind.

Karen: A kind of research and writing that might loosely be identified as ethnographic.

[General silence.]

Timothy [holding up potion]: At the risk of speaking too broadly, I do think there is a common theme that holds all of our discussion together so far, and that relates directly back to the question of what we should do with this Fieldwork Invisibility Potion.

[The wolfdog growls.]

Alice: And what's that?

Timothy: I would call it attention to positionality, to our locations within structures of power.

Mitch: That is pretty broad.

Timothy: Let me try to be more specific then. Most importantly, I think our conversations so far illustrate how ethnographers need to attend to their positionality within networks of power in the fieldsite itself. As we've already suggested with our discussion of ethnography's tortured relationship to power, there is no such thing as a neutral position from which to conduct ethnography.

Katherine: Can you illustrate?

Timothy: In my own research, for example, I made a conscious decision to enter the fieldsite as an entry-level worker rather than as a guest of management. This had a significant impact, not only on the kinds of information I was

able to access in the field, but also on how I interpreted and understood that information.

KAREN: I imagine it also had a huge impact on how others in the field perceived you.

TIMOTHY: Exactly. Imagine, for example, if in your ethnography of Wall Street you had conducted your research from the vantage point of one of the janitors or cafeteria workers rather than as an investment banker.

KAREN [laughing]: Well, I doubt I would have had very much sustained interaction with the traders at all!

TIMOTHY: True, but if you'd had them, those interactions would have been profoundly shaped by your vantage point.

KAREN: This is such an important point. People criticize my work for not being sufficiently immersive, but what they don't understand is how difficult it is to study Wall Street investment bankers as a participant-observer. Those positions are highly competitive. It is not simply a matter of walking in and getting a position there in order to study them.

ANNA: Right. It's the point about studying up versus studying down.

TIMOTHY: Definitely. My fieldwork is nicely illustrative just because the hierarchies were so explicitly encoded in the site itself. As a workplace, the slaughterhouse had very clear and very consequential divisions of authority.

JIM: Well, and even within those divisions, there were further divisions of labor that really mattered, right?

TIMOTHY: Yes. So, it mattered a great deal that I worked first as a liver hanger, then as a chute worker, then finally as a quality control worker.

JIM: So this level of positionality you are talking about—it's not just a matter of power, is it?

TIMOTHY: No, I guess you're right. It's not just overt or implicit power hierarchies in the fieldsite—manager vs. line worker, guard vs. prisoner, etc.—but also other distinctions that are not as immediately obvious as power distinctions.

ANNA: I think one of the exciting things about ethnography is that you can't always know what those distinctions are until you get into the fieldwork. It's the fieldwork itself, in a way, that illuminates what those distinctions are and why they matter.

JIM: Yes, exactly. Thinking back to Timothy's research again, research that I supervised, I doubt he would have even known the difference between a liver hanger and a liver packer, before starting his fieldwork, much less that it would prove so important to his research.

KAREN [eyebrows raised]: Liver hanger and liver packer? Do tell!

TIMOTHY: Well, I don't know if it's completely relevant here.

PIERS: Oh, come on, do tell us.

TIMOTHY: Well, OK. My first job in the slaughterhouse was working in the cooler as a liver hanger. Basically, I stood in a damp, near-freezing cooler for nine

hours a day taking freshly gutted livers off a line of overhead hooks and hanging them on carts to be chilled.

KAREN: Gross.

TIMOTHY: Yes, at first. But then it became overwhelmingly monotonous.

ANNA: So where do the liver packers come in?

TIMOTHY: Well, after hanging and then sitting on the carts for a period of time, the livers are taken off the carts by the liver packers and put in boxes.

KAREN: Still gross, but why does this matter?

TIMOTHY: See, there was a fierce and recurring conflict between the liver hangers and the liver packers over who was supposed to wash the liver carts after they had been used. The liver packers had a much easier job overall than the liver hangers because they weren't tied to the line in the same way. The liver hangers had to attend to the overhead line of moving hooks, so they couldn't step away from their stations for even a second because if they missed a liver and it went back upstairs to the kill floor without being taken off the hook, they would be fired.

KAREN: And the liver packers?

TIMOTHY: They weren't tied to the line. All they had to do was to take the livers off the carts, which could be manually pushed around in the cooler. So, they could really work at their own pace and often took long breaks between bouts of work.

ANNA: Long breaks?

TIMOTHY: They seemed long to us liver hangers, because we were tied to the line and couldn't step away for even one second.

KAREN: So what happened with this conflict?

TIMOTHY: Again, the liver packers had the flow of work on their side. Because it was the liver hangers who needed to use clean carts, the liver packers could just let the dirty carts pile up without any consequences to themselves. But the liver hangers would ultimately be forced to wash the carts themselves, which caused a lot of problems given that they also had to attend to the overhead line of moving hooks.

JIM: What's interesting about this conflict, reading your book, is how completely it came to dominate your consciousness.

TIMOTHY: Exactly. Entire weeks went by where all my fellow liver hanger Ramón and I could think or talk about was how we were going to get back at the liver packers. Here we are, working in a freezing cooler for just a few bucks an hour, contributing to the mass slaughter of thousands of creatures a day, and all we could think about was this fight we were having with three guys who were also working in a freezing cooler for just a few bucks an hour.

JIM: The point here being that it was a critically important insight for your larger question about how violence is structured and normalized, a moment where you make Geertz's sheep raid speak to Geertz's revolution.

Timothy: Yes.

Karen: So what happened? Did you ever get the liver packers to wash the carts?

Timothy: We did! Ramón and I appealed to our red-hat supervisor. At first, he wasn't sympathetic to our point of view at all, but he relented after we told him the slaughterhouse risked being written up by the USDA for a food safety infraction if we got our gloves dirty cleaning the carts and then touched a liver. He told the liver packers they had to wash the carts. It made Ramón and me ecstatically happy!

Jim: I imagine there are all kinds of small conflicts like that all over the slaughterhouse.

Timothy: Right, and it was pure coincidence that this is where I was positioned when I was hired.

Jim: So it matters both that you were an entry-level worker and that you were positioned as a liver hanger in the cooler.

Timothy: Exactly. Attending to my positionality in the fieldsite really mattered in ways that were both under and not under my control. It was my decision to access the site by seeking employment as an entry-level worker rather than as a guest of management. But once inside the slaughterhouse, I had minimal or no say about my initial placement as a liver hanger in the cooler, my movement to the chutes, where I herded the live animals into the kill box, and my promotion to quality control, which gave me access to nearly the entire slaughterhouse, including its paperwork documentation and radio communication.

Séverine: It just seems so risky, because you couldn't have anticipated in advance where you would be.

Timothy: I guess it's a bit like learning to live with those wolves: risky, for sure, but also exhilarating and potentially enormously generative and creative.

Katherine: So if I understand you right, what you mean by positionality at this level is careful attention to how you will be located in the fieldsite, both in relation to the power relationships that are already in existence there, and also in relation to particular locations that might not seem overtly connected to power but that might exert an enormous influence over what and how you perceive things in the field.

Timothy: I couldn't have said it better myself. It's interesting, for example, to think about each of our research projects in this regard.

Loïc: Yes, how would we categorize ourselves? I suppose, first of all, that each of us strove for and accomplished different degrees of immersion as participants in the sites that we studied. So, Timothy, you and I are very similar in that our research involved a high degree of immersive participation. You were a full-time, paid worker in the slaughterhouse you were studying, and I was a bona fide member of the boxing gym, participating in actual boxing matches. In terms of the ethnographers gathered here, I think we are definitely on the more immersive side of the continuum.

KAREN: My fieldwork was located somewhere in the middle of the immersion continuum. I spent part of it immersively participating as a worker at an investment bank, and part of it as a researcher interviewing people who worked in investment banks.

MITCH: Me too. In *Sidewalk*, I conducted both deeply immersive research in which I set up my own book table on Sixth Avenue while also stepping out of that bookselling role and inhabiting an obvious identity as a researcher. I also examined other locations in order to get a grasp on the forms of power at work in the site where I was more immersed. I interviewed officials working with the Business Improvement District, for example, to understand how political and class power shaped the lives of the booksellers. And, I did research with a white family from Vermont who set up a seasonal Christmas tree stand on Jane Street in order to understand how racial power played into the differential treatment received by the white family and the black booksellers. By definition, I could not have thickly immersed myself in all of these sites.

TIMOTHY: Don't you call that method the extended place method, in a play on Burawoy's extended case method?

MITCH: That's right! Our fetishization of theory so often leads us to want to extend our analyses from our immersive sites to theory when much more can sometimes be gained by extending to the other places that shape our own, sometimes at a distance.

JIM: Karen, I imagine your location in networks of power was not very high when you inhabited your role as a researcher?

KAREN [laughing]: No, not very high at all! These were all high-powered investment bankers, and me telling them I was a researcher did little to impress them or to entice them to give up their time for my project.

JIM: Right. On the other hand, as an American researcher living in a small Malay village and staying with one of the wealthier people there, I commanded a great deal of power in my fieldsite as a "researcher."

SÉVERINE: So all of this can really vary considerably. I suppose the main point here is to be acutely aware, as ethnographers, of the power positions that we do inhabit in the field. These power positions sit orthogonally to the question of how immersively participative our research is.[95]

TIMOTHY: Yes, exactly. And, there's a second level of positionality that has to do with our roles as creators of knowledge. When we think about this level of positionality, we are being attentive to how our position as knowledge creators is already located in particular historical and power contexts. Our knowledge creation is made possible only because of those contexts, and it can also come back and interact with those contexts, sometimes in surprising and unexpected ways. So, for example, earlier we had a rich conversation about the role that ethnography played in the early history of anthropology,

and in particular about the role that ethnography played in assisting colonial projects of power. And we've discussed how ethnography might be playing a continued, similar role in the US military today.

KAREN: Or how it might be playing a similar role in "urban ethnographies" of communities of color in the United States, as is the case with a lot of contemporary sociology, for example.

ANNA: Right.

KATHERINE: What about the positionality of people who do ethnographic writing that isn't located in academic disciplines, work like my own?

TIMOTHY: Your book in particular is interesting, Katherine, because it has had such an influential reach. It won a National Book Award and has been widely read, at least in the United States. So the relevant question there would be about the conditions of possibility that made your research feasible and about what impact it is having on the Mumbai slum you studied, or on other similar slums.

KATHERINE: I write a little bit about this in my afterword, about the hopes I had for what impact the book might have.

TIMOTHY: Yes, you do.

ANNA: And yet what's interesting about your book, Katherine, is that even though you think explicitly about its intended impact, you very self-consciously adopt a non-presence in the narrative itself. So, at the first level of positionality that Timothy was talking about—the level that asks us to think reflectively about how we are located in our particular research sites—it seems that you are almost entirely silent.

KATHERINE: I did think long and hard about that. It's just that I chose not to communicate this with my readers, because I thought it would distract them from the story that I really wanted to get across. I didn't want to become an unwitting protagonist in the story I was telling.

ANNA: I understand your reasoning, but one might argue that in your complete absence, you actually become much more of a distracting presence than if you'd made yourself present in the text so that the reader could understand how you were located in your fieldsite and how you came to know what you know.

KATHERINE: I do write about this in a short afterword. I tell my readers about the interviews I conducted.

TIMOTHY: This particular discussion shows, I think, how these different levels of positionality are related in various ways; how there can be no simple compartmentalization of each level. Ethnography locates the researcher in the creative tension between locus and object of study, and it asks the ethnographer to think about how her disciplinary location in knowledge creation matters. It also, importantly, asks the ethnographer to be explicit about her location in networks of power in the fieldsite itself. In at least these ways, positionality becomes a central theme running through ethnographic work.

[A contemplative silence fills the room. Soon, attention begins to turn back to the sheaf of papers, the vial of liquid, and the wolfdog lying on the floor.]

PIERS: Well! This has all been very interesting, but it doesn't seem like we're any closer to solving the conundrum of what to do with the vial of liquid.

ANNA: Yes, or of what to do with the one-eyed wolfdog.

ALICE: Or of what to do about my ethnographic trial.

LOÏC: Where is The Prosecutor? I am tiring of this, and I have important field-work to do.

MITCH: Yes, where is that prosecutor?

KAREN: At least the wolfdog seems to be sleeping more peacefully now.

KATHERINE: Do you think we should call a vet?

PIERS: A vet would probably kill her!

KATHERINE: What do you mean?

PIERS: The last thing she needs is treatment at the hands of Western veterinary medicine. What she's facing now is something of an existential and spiritual crisis. I've seen it many times before when shamans reach the end of their trance journeys.

KATHERINE: OK, then, what should we do about the vial and the dog? And what about Alice's trial?

[Wolfdog starts to stir on the floor, gets up weakly, and looks at the group. She opens her mouth, but only a muffled bark comes out.]

ALICE: It looks like the wolfdog has lost her powers of speech!

KAREN: It does. But at least she's moving again.

PIERS: Look, I have an idea. Why don't a few of us take the wolfdog for a walk by the lake? Maybe the fresh air will help her feel better. I know I could use some air myself after this conversation.

ALICE: Well, but what about the trial?

LOÏC: Maybe The Prosecutor will never show up and the trial will never happen.

ALICE: That would be a bit anticlimactic, wouldn't it? The summons from The Prosecutor is the whole reason we came up here in the first place.

KAREN: OK, look, how about this? Why don't I go with Piers to walk with the wolfdog, and the rest of you hold the trial while we're gone?

TIMOTHY: I like that idea, and I'll come on the walk with you two. I've been in this barn for far too long already. And while we walk, why don't the three of us discuss what to do with the potion?

ANNA: That sounds fine. So, Karen, Piers, Timothy, and the wolfdog will go for a walk and continue discussing what to do with the potion, and the rest of us will stay here and wait for The Prosecutor. But please give us a call if anything changes with the wolfdog or if you need help.

TIMOTHY: Cell reception isn't the best up here, but we'll try!

[Lights fade out to darkness as Piers, Karen, Timothy, and the wolfdog exit stage right. As they leave, the sound of a motorcycle engine gradually grows louder and soon a cloaked, hooded figure can be seen in the background approaching the barn on a Harley Davidson, the folds of his cloak flowing dramatically in the air. A long wooden staff is strapped to the back of the motorcycle.]

End of Act Four

Notes

1 For more on multi-sited ethnography, see Marcus 1995, 1998.
2 "Places…are the objects of anthropological study as well as the critical links between description and analysis in anthropological theory. [Arjun] Appadurai's crucial point is that description and analysis are systematically linked (and distinguished) by specific historical *spatializations*" (Clifford 1990: 65–66, original emphasis).
3 "Anthropologists don't study villages, they study in villages" (Geertz 1973a: 22).
4 Geertz 1973a: 23
5 For Wacquant's labeling of diagnostic ethnography and grounded theory as an "epistemological fairy tale" and for his critiques of raw empiricism and theoretical absent-mindedness, see "Scrutinizing the Street: Poverty, Morality, and the Pitfalls of Urban Ethnography" (2002: 1481). Wacquant's language in this section of Act Four draws heavily on, and at times quotes verbatim from, "Scrutinizing the Street." Verbatim language that goes beyond a few key words or phrases is indicated by quotation marks.
6 Duneier and Back 2006: 564–565
7 "Of course, the researcher does not approach reality as a tabula rasa. He must have a perspective that will help him see relevant data and abstract significant categories from his scrutiny of the data" (Glaser and Strauss 1967: 3).
8 Duneier 2002: 1574
9 Duneier 1999: 340–341
10 Duneier 1999: 341
11 Duneier 1999: 341
12 King, Keohane, and Verba 1994: 19–31
13 Duneier 2002: 1566
14 Boo 2012b
15 Wacquant 2010
16 Scott 1985, 1998, 2009
17 Duneier 2017
18 Geertz 1973a: 22
19 Burawoy 1998
20 Lundblad 2015, Jackson 2013
21 Tsing 2015: 37
22 "The methodologies political scientists use to reach evidence-based conclusions *all* involve extracting information from the social world, analyzing the resulting data, and reaching a conclusion based on that combination of the evidence and its analysis" (Elman and Kapiszewski 2014: 44, original emphasis).
23 Tsing 2015: 37
24 Tsing 2015: 37–38
25 Tsing 2015: 38
26 Tsing 1993
27 Quoted in Munck and Snyder 2008: 365.
28 Quoted in Munck and Snyder 2008: 365.
29 Munck and Snyder 2008: 366, Laitin 1999
30 Tsing 2015: 38
31 Tsing 2015: 38–40
32 Tsing 2015: 38
33 Peterson 2001
34 Leach 1954
35 Jonsson 2005
36 For the "romance of the primitive" in anthropology, see Fox 1972, Tsing 1993. For a revalorization of "the primitive," see Graeber 2006.
37 Schaffer 2014, Jerolmack and Khan 2014

38 Ricoeur 1970
39 Duneier et al. 2010
40 Tsing 1993
41 "Cultural knowledge and warfare are inextricably bound" (McFate 2005a: 42; see also McFate 2005b). For more on the Human Terrain System, see McFate and Laurence 2015, Petraeus 2015, González 2007, 2009, 2015, Gezari 2013. For broader relationships between anthropology and intelligence agencies, see Price 2000, 2005.
42 "Recently, a few social scientists have capitalized on their work with HTS, using it as the baseline for publishing academic journal articles.... A few HTS personnel used the research they had conducted in the field as the basis of their dissertations" (McFate and Laurence 2015: "Introduction").
43 American Anthropological Association 2007
44 "Anthropology came into its own in the period in which the Western nations were making their final push to bring practically the whole pre-industrial, non-Western world under their political and economic control. Anthropology is a child of Western Imperialism" (Gough 1968: 12–13, Gough 1967). Decades later, Gough 1993 revisited the themes of her earlier essay.
45 "Anthropologists were of higher social status than their informants; they were usually of the dominant race; and they were protected by imperial law; yet, living closely with native peoples, they tended to take their part and to try to protect them against the worst forms of imperialist exploitation" (Gough 1968: 13).
46 "European power, as discourse and practice, was always part of the reality anthropologists sought to understand, and of the way they sought to understand it" (Asad 1991: 315).
47 "The role of anthropologists in maintaining structures of imperial domination has, despite slogans to the contrary, usually been trivial. The knowledge they produced was often too esoteric to government use, and even where it was usable it was marginal in comparison to the vast body of information routinely accumulated by merchants, missionaries, and administrators. Of course, there were professional anthropologists who were nominated (or who offered their services) as experts on the social life of subjugated peoples. But their expertise was never indispensable to the grand process of imperial power" (Asad 1991: 315).
48 Salemink 2003
49 Malinowski 1922
50 Malinowski 1922: 24–25
51 Malinowski 1929
52 See Harris and Sadler 2003 for one account of these espionage activities.
53 These four, Boas wrote, "have prostituted science by using it as a cover for their activities as spies. A soldier whose business is murder as a fine art...accept[s] the code of morality to which modern society still conforms. Not so the scientist. The very essence of his life is the service of truth" (1919: 797).
54 Chanlat 2014
55 Bateson 1958, Coon 1980
56 Benedict 1980
57 Valeriano and Bohannan 2008
58 "We need to know...whether there is a common set of circumstances under which left-wing and nationalist revolutions have occurred or have been attempted in recent years in Cuba, Algeria, Indo-China, Malaysia, the Philippines, Indonesia, Kenya, and Zanzibar. Are there any recognizable shifts in ideology or organization between these earlier revolts and the guerilla movements now taking shape in Guatemala, Venezuela, Colombia, Angola, Mozambique, Laos, Thailand, Cameroon, or southern Arabia? What are the types of peasantry and urban workers most likely to be involved in these revolutions; are these typologies of leadership and organization? Why have some failed and others succeeded? I may be accused of asking for Project Camelot, but I am not.

I am asking that we do these projects in *our* way, as we would study a cargo-cult or a kula-ring, without the built-in biases of tainted financing, without the assumption that counter-revolution, and not revolution, is the best answer, and with the ultimate spiritual and economic welfare of our informants, and of the international community, before us rather than the short-run military or industrial profits of Western nations" (Gough 1968: 23).

59 Engels 1984 [1845]

60 Morris 2015, Go 2016

61 Du Bois 1899

62 Du Bois 1903

63 Thomas and Znaniecki 1918

64 *The Polish Peasant* is "a neglected classic…[a] landmark because it attempted to integrate theory and data in a way no American study had done before" (Bulmer 1986: 45).

65 Bulmer 1986

66 Hull House 2010, Booth 1892

67 Lewis 2000: 188

68 "My vision was becoming clear…. The world was thinking wrong about race because it did not know. The ultimate evil was stupidity. The cure for it was knowledge based on scientific investigation. At the University of Pennsylvania I ignored the pitiful stipend. It made not difference to me that I was put down as an "assistant instructor" and even at that, that my name never actually got into the catalogue; it goes without saying that I did no instructing save once to pilot a pack of idiots through the Negro slums. The fact was the city of Philadelphia at that time had a theory; and that theory was that this great, rich, and famous municipality was going to the dogs because of the crime and venality of its Negro citizens, who lived largely centered in the slum at the lower end of the seventh ward. Philadelphia wanted to prove this by figures, and I was the man to do it. Of this theory back of the plan, I neither knew nor cared. I saw only here a chance to study an historical group of black folk and to show exactly what their place was in the community" (Du Bois 1940: 59).

69 "Park sought to understand the conditions that would allow the white man to rule the world. He argued that African colonization and American slavery were necessary because they provided the apprenticeships through which backward blacks advanced by being exposed to the white man's superior civilization" (Morris 2015: 222).

70 "While training his students, Park 'told them flatly that the world was full of crusaders. Their role instead was to be that of the calm, detached scientist who investigates race relations with the same objectivity and detachment with which the zoologist dissects the potato bug'" (Morris 2015: 114, quoting Park, Masuoka, and Valien 1975: 17).

71 "[T]he Chicago school – and indeed early mainstream American sociology in general – can be exposed for what it was: a parochial if not provincial body of thought that reflected little else than the worldview and groping aspirations of a handful of middling white men whose interests were tethered to the interests of the American empire" (Go 2016).

72 Even Du Bois, who placed so much faith in science to counter ignorance about race, wrote: "So far as the American world of science and letters were concerned, we never 'belonged'; we remained unrecognized in learned societies and academic groups. We rated merely as Negroes studying Negroes, and after all, what had Negroes to do with America or science?" (Du Bois 1968: 228, quoted in Morris 2015: 112).

73 Du Bois 1968: 222, quoted in Morris 2015: 135.

74 Du Bois 1968: 192, quoted in Morris 2015: 135.

75 "I did it [research for *The Philadelphia Negro*] despite extraordinary difficulties both within and without the group. Whites said, Why study the obvious? Blacks said, Are we animals to be dissected and by an unknown Negro at that? Yet, I made a study of the Philadelphia Negro so thorough that it has withstood the criticism of forty years. It was as complete a scientific study and answer as could have then been given, with defective

facts and statistics, one lone worker and little money. It revealed the Negro group as a symptom, not a cause; as a striving, palpitating group, and not an inert, sick body of crime; as a long historic development and not a transient occurrence" (Du Bois 1940: 59).

76 Anderson 1996
77 Duneier 1994, 1999
78 See, in particular, Duneier and Back 2006.
79 Leibow 1967, Stack 1974
80 Duneier and Back 2006: 549
81 Duneier and Back 2006: 547
82 Hasan 1999
83 Wacquant 2004b: note 20
84 Wacquant 2005a
85 Wacquant 2005a: 447
86 Duneier 2006: 148
87 Venkatesh 2009
88 "If we look at literature based on field work in the United States, we find a relatively abundant literature on the poor, the ethnic groups, the disadvantaged; there is comparatively little field research on the middle class and very little firsthand work on the upper classes. Anthropologists might indeed ask themselves whether the entirety of field work does not depend upon a certain power relationship in favor of the anthropologist, and whether indeed such dominant-subordinate relationships may not be affecting the kinds of theories we are weaving. What if, in reinventing anthropology, anthropologists were to study the colonizers rather than the colonized, the culture of power rather than the culture of the powerless, the culture of affluence rather than the culture of poverty?" (Nader 1972: 289)
89 Ehrenreich 2001
90 Wacquant 2004a: viii
91 Wacquant 2011, 2015
92 Pachirat 2011: 19
93 Mears 2011
94 Hamper 1986, Perkins 2004
95 See Gans 1962: 336–350 for a classic treatment of the continuum of participant-observer roles. See also the discussion of six degrees of researcher participation in Schwartz-Shea and Yanow 2012: 63–66.