Northwestern Journal of Law & Social Policy

Volume 13
Issue 3 *Northwestern Law Interrogating Ethnography Conference*

Article 1

Spring 2018

PANEL DISCUSSION: AUTHOR MEETS CRITIC

Recommended Citation
PANEL DISCUSSION: AUTHOR MEETS CRITIC, 13 Nw. J. L. & Soc. Pol'y. 108 (2018).
https://scholarlycommons.law.northwestern.edu/njlsp/vol13/iss3/1

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Northwestern Journal of Law and Social Policy
Vol. 13, Issue 3 (2018)

NORTHWESTERN LAW INTERROGATING ETHNOGRAPHY CONFERENCE

AUTHOR MEETS CRITIC

Northwestern Pritzker School of Law
375 East Chicago Avenue
Chicago, Illinois
October 20, 2017, 4:00 p.m.

PRESENTERS:
PROFESSOR GARY ALAN FINE, Presider;
PROFESSOR PHILIP COHEN;
PROFESSOR COLIN JEROLMACK;
PROFESSOR SHAMUS KHAN;
PROFESSOR STEVEN LUBET
PROFESSOR MARY PATTILLO

MR. GARY ALAN FINE: Hello. Welcome. If we could all gather and have a seat. All right. Well, this is great. Dear friends, I wish to welcome you to our two-day symposium to witness the launching of a very special book, *Interrogating Ethnography, Why Evidence Matters*, by my good colleague here at the Pritzker School of Law of Northwestern University, Steven Lubet, who is the Williams Memorial Professor and director of the Bartlit Center for Trial Advocacy. Steve is an expert on the use, and also on the misuse, of evidence.

I thank the Northwestern Pritzker School of Law for their support of this event. It was about eighteen months ago that Steven Lubet became simultaneously skeptical of and enamored with ethnographic research. Now, there is much to say about Alice Goffman's *On the Run*, none of which I will say here, except to note that among the virtues of her ethnography is that it inspired my colleague to take our methods seriously, leading to a lengthy dialogue from which I have learned much. It was my challenge to persuade my colleague that his analysis should not be about one writer and one project, but to think broadly about what advice a scholar of trial advocacy could give several generations of fieldworkers. He agreed. And, however we assess the outcome, we must be grateful for the attention, which has been both complimentary and candid. He is our Judge Judy. Thank you, Steve, for your counsel, whether we chose to acquit or convict.

MR. STEVEN LUBET: I have to say something about Judge Judy. I was in a conference and somebody made the observation that at the time Judge Judy earned more money than Chief Justice Rehnquist. And the response from the audience was, “She's a better judge.”

MR. GARY ALAN FINE: That is a direction I am not going to go. In planning this conference I told Steve that he was not to be treated as either an icon or as a piñata, but as a colleague whom we should prod, praise, and critique as needed. That is my hope for the next two days, both in sessions and beyond. We have organized three sessions. The first this afternoon is styled as an Author Meets the Critic session, after which Steve will be able to respond. Tomorrow starting at 10:00 a.m. we have a session on Ethnographic Evidence, and in the afternoon a session on Ethnography, Ethics and the Law.

So let me briefly introduce our four speakers this afternoon. Phil Cohen is a Professor of Sociology at the University of Maryland and an expert on families, demography and inequality. He is soon to publish *Enduring Bonds: Inequality, Marriage, Parenting, and Everything Else That Makes Families Great and Terrible*.

Now, Phil has not been invited by mistake. But we felt that it was important that this not be a conference in which ethnographers talk only to one another. Phil and Steve have been in dialogue as Steve has been writing and thinking about his book. And we hope, I hope, that perhaps Phil will at the end of this conference decide to drink our Kool-Aid.

Colin Jerolmack is Associate Professor of Sociology at New York University and Chair of the Environmental Studies Program. His book, *The Global Pigeon*, is a classic of environmental ethnography and is iconic in demonstrating how the mundane can be made crucial. As Colin insists, pigeons—those flying rats of the city—are the carriers of social theory.

Shamus Khan is Professor of Sociology and Chair of the Sociology Department at Columbia University. Shamus represents the best of the current younger generation of ethnographers. And I hope you appreciate that I called you the younger generation.

MR. SHAMUS KHAN: I did appreciate that.

MR. GARY ALAN FINE: His book, *Privilege: The Making of an Adolescent Elite*, is taken as a model of how to study up and not down, as is so often the case. Elites deserve our gaze, both skeptical and generous.

And, finally, my dear colleague at Northwestern, Mary Pattillo, Harold Washington Professor of Sociology and African American studies. Now, I worried that if I praise Mary as much as she deserves, you will think that it is a case of special pleading. It wouldn't be, of course, and I suspect that you know that because you, like me, have been inspired by *Black Picket Fences* and *Black on the Block*. We have many superb urban ethnographers today, but there is only one Mary Pattillo. At this, I will turn the floor over to our colleague, Philip Cohen.

MR. PHILIP COHEN: Okay. Great. Well, thank you, Gary. And thanks, Steve, and whoever else made this happen at the law school. I really appreciate it. I am not a lawyer and I'm not an ethnographer, so I'm just happy to be here. I'm also not going to be super critical of the book. And although I'm going first, I'm not going to summarize the book, just because I don't want to waste my time on it. I don't have much time, even though I have minutes. I know that you haven't read it because it just came out today, so I'm going to try to—I don't know exactly how I'm going to handle that. I found it fascinating, insightful, challenging, and I think it's going to be very important. It's also a great read, so you should read it if you're interested in ethnography at all. I'm also a little bit nervous because I'm first on the panel, and the other people are ethnographers and I don't know what they think about the book yet, so if I say something that's totally—I could be embarrassed later, I guess. So I'm totally open to criticism of or disagreement with my opinions, from anybody, really, but from the ethnographers in particular, of which I'm definitely not one.

The other thing is that I haven't read most of the work he's critiquing. One of the great things about this book is that he read many, many ethnographies and has a lot to say about them kind of systematically. The one ethnography that I've read most carefully is actually Alice Goffman's book *On the Run*, just because of the controversy around it; I got sucked into all of that. And, of course, it is interesting and very important. It's probably the most famous ethnography in recent years, at least before *Evicted*, for better or for worse. And one that has a lot wrong with it that I'll talk a little bit about. So my experience in documenting somewhat small parts of that book to be wrong, incontrovertibly wrong in my opinion, was very frustrating. And that is probably part of why I'm here.

Alice Goffman—I'm also not going to summarize that book—but she did an ethnography of a group of black men in Philadelphia and their struggle with their legal records that followed them, and all the implications of that for their lives. Okay. I did summarize the book. In it she did a survey, and the survey was obviously wrong. There was stuff about it that just could not have been true. And when I pointed that out and I did some of the work to show her that, her response to a journalist was that it was an ethnographic survey and so it shouldn't be held to the same standard as a regular survey. And I didn't agree with that.

Then the journal that published that work, the *American Sociological Review*, which is our No. 1 journal in sociology, chose not to acknowledge that it had published work that was demonstrably inaccurate. And so that also increased my concerns and irritation with our peer review and publishing system, which is something that I work on quite a bit. And, by the way, all the stuff I'm mentioning today, the stuff that I have written and all that, I put on my blog today which is called *Family Inequality*. So if you go there you'll see the links to the papers and the blog posts that I wrote about this. I put them up at the top of that blog today for you.

So my experience with that shapes all of my thoughts on this. I'm also very involved in efforts to improve scholarly communication in academics, and social science, in general. I created SocArXiv—that's why I'm wearing this button—and I'm happy to talk more about it. It's an open access paper server which as of today hosts 1,562 papers that are either pre-publication, post-publication, or that is their publication, and includes links to research materials, data and code and proper archival metadata, and things that are important for publishing. And our next step in that process—I'm not going to dwell on that very long—is to rethink and hopefully reinvent the peer review process for social sciences. I think some of my thoughts as I'm reading this book about ethnography are really informed by the struggles we've been having over our peer review system and to try to get it out from under the inertia of a system that combines sort of ancient practices, like having paper page limits and publishing in print altogether, with unjustified, untested assumptions about what is proper, such as double-blind peer review. And exclusive reviewing by just one journal, as if something is reviewed once by one journal and then it is done.

In light of all that, my question is really not—and I really, really don't intend to offend ethnographers—is whether ethnography is part social science or science. And if it is, having read this book, I think we have a problem. However—and this is going to make many people unhappy—that problem is shared by most of the rest of the social sciences too. So we all have a problem certainly not unique to ethnography. Considering the other work I do, even including census work, which is basically a survey, and survey research. Surveys are 100% hearsay. Are the subjects knowledgeable about what they're reporting? Well, hopefully they are. Maybe they are. But we also ask them to make estimates of things all the time that are never verified.

The American Community Survey, which is our biggest national survey of top quality produced by the Census Bureau, in that survey about percent of -- there are 4% more people in the ages that end with a five or a zero than there should be. And there is a large number of eighteen year olds, many more than there are seventeen and nineteen year olds.

This is called the Whipple problem. It has a name in demography. And there is a standard for it, that percent overestimate of people that are those ages. If it had been 5%, it would be considered bad data by the United Nations. So it's pretty close, and that's our best data. It turns out, however, that because of the norms and standards in survey research, we can investigate this problem, and we have now been able to narrow it down to know that it occurs only in the in-person interviews. That is, they send electronic requests, they send mail, paper, mailbag surveys, and the people who don't respond to the American Community Survey get either a call or a visit from the survey taker, and the entire Whipple problem is in that population. Those are largely people with low education. And many of them are providing proxy reports that are reporting the ages of other people in their households, including people that are only marginally attached to their household, so it's not surprising that they're rounding to zeros and fives. Problem explained because we have procedures for that.

I'll give one other quick example of that, which is the usual hours worked in the Current Population Survey. The Current Population Survey is where we get the unemployment rate and all that. For the past fifteen years, they asked you, “How many hours did you usually work per week last year?” Who can answer that question? For the past fifteen years, the percentage of people saying forty has not changed really at all. It has been within a percent of 50% of all people say, “I usually work 40 hours.”

In the instructions to the interviewers, the Bureau of Labor Statistics tells them to steer people away from "it varies" as a response or giving ranges, like thirty to forty. They don't want that. They want you to pick one number, a whole number, and that should be the number that you usually, 50% of the time or more, work per week. This is likely to be literally impossible.

That is, it's not a question of your perception. There may very well not be one number that is the number of hours you work more than all the other weeks.

Anyway, that is a very key indicator that we use in our research. So there is a lot of storytelling and hearsay and errors in all kinds of research. One more example is the American

Community Survey also asks you in the past twelve months, did this person get divorced. They ask that about each person in the household. And they find the divorce rate is 13% higher for women than for men every year. It's not because lesbians are getting divorced, although we don't know that. We can't quantify that exactly, but there's no way that that's accounting for all of this.

So women are getting divorced more than men, but nobody is checking this. It's not an administrative record, there is no verification. It's a simple question, "Did you get divorced?" Women are—we don't know why. But we do know that we can study this problem. We all have access to that data and the instrument that was used and the procedures that were used and the company that does the interviewing. And all that stuff is things we can all access and figure out. Okay. We can benchmark it against other sources, we can share our data and code, we can use our third parties to verify and so on.

So survey research is not inherently better or more accurate than ethnography, but we have standards and norms for reliability, verification, and replication. And reproducibility does not mean interviewing exactly the same people at the same moment in their lives. People think ethnography cannot be reproduced because it's this totally unique experience. Every phone interview is a totally unique experience, and, yes, in lots of social sciences we have standards where we try to approach reproducibility. If ethnography is social science, we have to think more about these things. And I'm not saying people don't, and I know that they do. So that's just what the book makes you really think about, and it is very successful at that.

Professor Lubet talks about a lot of cases and pursues a lot of different potential problems that occur in ethnographies, and also some particular practices—the masking of identities of people and places, the construction of composite characters, the hearsay problem, urban legend, and so on. This is all very good.

There is another problem that he doesn't talk that much about, but in this fake news milieu, I think we have to consider it more, which is what about dishonest researchers—researchers who simply lie or fabricate things. We have to have norms and standards that everyone adheres to so that we can catch that hopefully rare case. If we have a standard that says we're not going to publish unverifiable information, then we're going to have a chance at least to stop that rare bad actor.

I know that's sort of in the background of this book. He doesn't impugn anybody's motives. But we have to consider that. We've had plenty of experience with that in recent years in the quantitative social sciences altogether. Our system runs on trust, and yet people can make stuff up and they have incentives to do that. So we either are going to trust everybody, or we're going to not trust everybody and devise ways to handle that.

One odd thing about ethnography that Professor Lubet doesn't really talk about is the reliance on the book as the medium for ethnography. Much more so than other subfields in social sciences. And that's interesting. It's especially problematic for the peer review process, and I've been thinking about that, like I said, quite a bit. In the case of Alice Goffman, we know that her dissertation—her article in the American Sociological Review—and her supposedly peer-reviewed book published by the University of Chicago Press were all basically identical. They did not differ at all. It is not how we usually think peer review goes. Normally it's a painstaking terrible process with multiple revisions. It's unusual in that case. So without knowing or talking about things I don't have any firsthand knowledge, it appears from the outside that the prestigious university she was at, the prestigious book contract she had, and the prestigious journal article she published all somehow reinforced each other and led to her series of highly prejudiced publication decisions, and so raised a question of what is going on with peer review, which I think we need to seriously rethink in general in social sciences.

One thing that I think the book is great at illustrating is how we need different kinds of people to review different kinds of work. A demographer, a legal scholar. These are people who could have reviewed Alice Goffman's book and come up with some pretty interesting impressions. Okay.

The thing about the book that's interesting is that we don't know—we have the standard where something is published and it's done. It doesn't really make sense to do that in this day and age. Those of us interested in peer review are focusing a lot on the question of post-publication peer review. That is, why is there such a thing as a work and the work is finished and it's reviewed and it's out and then it's done?

I'll give you an example of what I mean. When a journalist confronted Alice Goffman with my criticism about her survey, her response was, and he quoted her, Jesse Singal, "I should have included a second appendix on the survey in the book. If I could do it over again, I would."

Well, what is stopping her? Right? I mean, we're all still alive. We still have the Internet. We can distribute any information we want to. The idea that the book is done—well, I really wish I had taken the time to explain that, but now unfortunately the book is done. It doesn't make sense. But that's the way our publishing works. It is reasonable that there are sort of milestones and things are stamped at certain points, but the idea that the research stops, that the reviewing stops, and that the scholarly record ends at the moment of publication is something that I really think we need to rethink. This is not Professor Lubet's problem.

MR. STEVEN LUBET: Yeah.

MR. PHILIP COHEN: So what about the notes and records from ethnographic research? And from this? I know there are all kinds of different practices. But from my perception, especially having just read this book, they really just have to be available for inspection by other people. Maybe not everybody, but some other people. They should be anonymized as minimally as possible and made available in their entirety. The idea that an ethnographer has the last word forever on what details from the fieldwork anyone else will ever be able to see, and that decision can never be revisited after the work is done, simply can't stand. If ethnography is science—and, again, I'm not saying that in an antagonistic way. I'm just saying that's a factor in the way science works. It would be like if somebody did survey research and the only thing the public could ever see was the regression results and then the data is destroyed. You cannot see the data, you cannot see the descriptive statistics, you can't see the survey instrument, or anything else about it. It just really doesn't make sense.

Now, of course, people have to edit their work. They're going to write a book and they want to write a book that people will read. You can't put every single fact in it. But Steven has a very nice description of the selectivity problem, which is sort of like the file drawer problem in the quantitative work. You do 100 experiments, you don't get a good result, none of those are published. But the 101st experiment has a great result and you publish it. Right? But the 100 failed experiments are not part of the scholarly record. So somebody comes along later and looks at the meta-analysis or analyzes all the work on this topic and all they see is all the successful studies. This is a well-known problem in psychology, for example.

And the answer to it is preregistration of hypotheses. So now people are realizing that the way to handle this is to publish your study design and your hypothesis before you do the experiment. And there are journals that are now committing to publish those papers regardless of what the findings are. I mean, we sort of teach our students to do that. That is, your research should be important regardless of what you find. That's how you know you have a good topic. It's a question we don't know the answer to and you have the ability to answer it; and whatever you find, that's good research if it's good research. Right?

But that's not the way academic publishing works and it's because of all incentives and so on. So we really have to think about that. Right. Selectivity and the file drawer problem. It just doesn't make sense that people—ethnographers love to tell you, which is great and I'm jealous, that they had—how many millions of words their field notes are and their interview transcripts are. It's a stack of paper from here to there. And the fact that they distilled it down to one book is a sign of how awesome they are. And they've turned it into a three-by-two categorization and they've got—you know, it's unbelievable the things that they do. And they present us with the six kinds of people in the situation of what they do, or whatever it is.

That's great, and I'm not saying that's wrong at all. But five years from now where is all that? Somebody might think of something else that we want to get at with those notes. If it turns out to be super important, like Alice Goffman's book, we might even want to question that.

And I realize that, no offense, the vast majority of ethnographies are just not that important and no one is ever going to really want to do that. The same with all my work. But once in a while, something really is, and what we need to have is a standard and a way to handle it so that we're prepared for that case. Right?

So like with the person who lies, we need procedures that just can handle that. It's the same with what if we want to revisit the data? What if there are other things to learn from that? So we just need to work out how to preserve all that stuff in a way that other people can study it. And there's just no reason not to. The question really is only how. So I'm willing to be wrong about all this. I'm willing to hear ways that Steven is wrong. I haven't given any because I think he's basically right. But I'm happy to hear from the ethnographers who disagree with that, or whatever, on that.

I think this is a really important conversation. I thank Steve for provoking it and inviting me to participate in it. And I'd love to hear what everybody else has to say. Thank you.

MR. GARY ALAN FINE: Our second speaker is Colin Jerolmack from New York University.

MR. COLIN JEROLMACK: I want to start with something that might be unpopular in this setting, which is a defense of Alice Goffman. This won't be the bulk of my talk, but I want to say that I reject the implication that *On the Run* is on the opposite end of the continuum, as Steven says, from *Evicted*, serving as little more than a cautionary tale of how not to do ethnography. Despite the controversy, which has unearthed some real issues but has also produced a lot of hot air, Alice does a lot of things very well. And I dare say that *On the Run* does a number of very important things better than most ethnographies.

Alice did six years of serious, sustained, embedded fieldwork. I don't think it can be argued that she does not convincingly demonstrate her core thesis through her fieldwork, which is that, for her poor, black, male subjects, staying out of prison and maintaining family, work, and friend relationships become contradictory goals. If they try to engage in working and holding an apartment with a lease, they will increase their chance of getting arrested. That's the main thesis. She demonstrates it with her fieldwork. Also, Alice shows how the police's expanding web of entrapment holds entire families hostage, not just the offenders, and she shows many examples of that.

Regarding ethnographic methods, there are a number of things I think Alice does very well. She grounds many (though perhaps not all) of her claims in systematic observations over time and across situations. In doing so, I think she demonstrates quite well the ways in which thickly describing *how* social processes unfold enables an understanding of *why* social life is the way it is. One thing that really bothers me about many of the ethnographies that I read is that, although they're carried out over a long period of time, most of their claims are not temporal. Once ethnographers decide the analytic categories that they're interested in, they say something like, “There are four types of people, there's this type, there’s that type; here is a vignette from 2013, an interview from 2011.” And they actually do nothing with the temporal aspect, with how people may have changed the way that they act over time, or with how you can leverage time as an explanation for how social life varies across different situations.

This is to say that many ethnographers, while they do longitudinal work, treat their data as cross-sectional. But Alice shows over time how, when wanted men attempt to act respectable, they actually do get locked up. And then she describes instances of how, after getting locked up, they subsequently learn to cultivate a more shady and distrustful character, as she says. In doing so, I think Alice provides unique explanations for criminality and poor residents' disinclination to call the police.

For instance, Alice frames criminality not as a result of Mertonian anomie, or as the men adopting what Elijah Anderson called “the code of the street,” but rather as an unintended consequence of tough-on-crime policing. And she understands the refusal to call the police not as resulting from a pervasive culture of “legal cynicism,” in which the police are seen as illegitimate or unresponsive, but rather as a consequence of the simple fact that when someone—usually a woman—has somebody with an arrest warrant living in their house, then calling the police to report a crime may result in her house being searched and her loved one being arrested. This is actually a novel sociological explanation of what is normally called legal cynicism, which in her account has little to do with cynicism toward the police.

There are other ways that Alice leverages time. For instance, the episode of how Ronny “snitched” on Mike, but then Ronny did clever things, like show up for Mike’s bail hearing and pay for his bail. Over time, Alice shows how Ronny’s actions actually caused Mike to misremember what happened such that he no longer believed that Ronny snitched on him.

Alice does cross-situational and cross-temporal moves like this a lot, and they're actually really smart moves that build explanations based on how changing definitions of the situation unfold over time that I don't see in a lot of ethnographies. The exceptions I would think of are really long-term ethnographies, like Rob Smith’s *Mexican New York* and Tim Black’s *When a Heart Turns Rock Solid*, but it's not something that most ethnographers do.

The other thing that I particularly like that I know that Steven would appreciate, but that he doesn't acknowledge in Alice’s work, is the way she hunts for contrasts between saying versus doing. For instance, the men talk about what they would do if they were “clean” and they didn't have a warrant: they would get an apartment, get a job, etc. But because Alice follows them over six years, she can say, “You know, there were months when they did not have a warrant out for their arrest, but they still didn't do those things." These episodes become the grounds upon which we can start to make theoretical sense of discrepancies between saying and doing.

Now that I've said all that, I'm done talking about Alice. Let's talk about Steven. I actually expected, and I almost wanted, to hate *Interrogating Ethnography*. I wanted it, and I expected it, to be so polemical that I could just dismiss it and say it didn't really have a lot of things for me to think about. But that's not the case. It's compelling. Ethnographers need to reckon with it. And I find it very generative to depart from Mitchell Duneier's notion of the "ethnographic trial," described in his article "How Not to Lie with Ethnography" (which should be more widely read, cited, and taught). Steven's book is in many ways an expansion and a more literal interpretation of Mitch's idea.

One thing Steven takes on is the problem of relying on accounts. My buddy Shamus Khan and I have written about this. But in our articles on this topic, we kind of proudly puffed out our chest and said, "Interview researchers and survey researchers rely on accounts, and there's a lot of problems with using accounts to explain behavior." We knew there were ethnographers that do this too, but our paper was a defense of ethnography. Well, Lubet reveals that, all too often, ethnographers do the same damn thing. If they do talk about both observations and accounts, they often treat them as equivalent sources of data. They don't think about how they might be different sources of data. So many ethnographers, Lubet shows us, wind up producing what Mitchell Duneier calls quotation-driven rather than context-driven ethnographies. Steven's notion of firsthand observation being the gold standard for ethnography seems self-evident, but he shows us that ethnographers routinely don't even tell you whether they saw something firsthand or whether it winds up being something that was reported by somebody else.

The rule of distinguishing between what one saw and what they were told is a good one. It's easy enough when you're describing an event to say "reportedly," and then maybe you put a footnote that says "I didn't see this but I talked to three different people and blah, blah, blah." Right? Yet people don't really do it. Steven shows us that many people don't do it at all. And it's actually quite painful for me as an ethnographer to watch him reveal the frequency with which ethnographers report an account as factual without checking, even at times when the facticity really matters for the argument that they're making.

Another part of the book that it's not surprising I appreciate, and that I think ethnography has not recognized very much as a problem, is masking people and places. Lubet talks about how almost every problem that he identifies—standards of evidence, being able to check stuff out, and so on—is compounded by anonymity. Alexandra Murphy and I have written about this, and it's something I've been obsessed with lately. In particular, the issue is that it actually can impede ethnography's contribution to social science. It's impossible, unless the original researcher does it, to do a revisit of a field site, whether you want to check stuff out because you think people might be lying or you just want to look at how things have changed over time, which Michael Burawoy did. But Burawoy still gave the factory he studied a pseudonym, and he only discovered this factory accidentally because Donald Roy also gave it pseudonym. So how can we really go about making a program out of doing revisits, which Burawoy champions, if we're masking the places of our research?

Masking also can impede generalizability. One of the things that I really appreciated when Robert Vargas's recent book came out on violence in Chicago was this: given the topic, I sympathize with him giving anonymity to his participants, since some of them are gang members or whatever; but it was a really important move to not anonymize the neighborhood. He's engaging with theories of violence and "neighborhood effects." People who study neighborhood effects actually have quantitative data on the neighborhood that Vargas is studying. This allows him to engage in a more productive, fruitful conversation with other scholars about his particular neighborhood and relate it to the quantitative data and other sources of data that we have about the area.

I would say—I actually had said this in an earlier version of the article that Alexandra and I wrote, but I was encouraged to take it out, so I'll say it here: Masking institutionalizes lying. Changing names and details and making composites is making fiction. I think what's really important about this issue is that, in theory, I'm sympathetic with sharing field notes and making them more transparent, but I think sharing field notes, if you don't deal with masking, is not going to solve a lot of problems. You have to redact so much if we also have to still hide the place and the people. You also still have the problem, which everyone knows, that the ethnographer influences what they see. If you have access to my field notes but my subjects and site are masked, you still only have access to what *I* saw and chose to write down in the first place. So you can't have an independent way of talking to those people or going to those places. Therefore, even if you have more of my field notes than what winds up in my publications, you still have only my interpretation, what I decided was important enough to put down on the page. You still can't do a revisit or bring other sources of data to bear on my case. And so I think that this issue of masking and transparency is a first order problem that we have to solve before we can get to sharing field notes and making data available in other ways.

I would also note, too, that the growing expectation of making field notes public is an interesting result of ethnography being attached to the social sciences. Journalists don't usually make their notes public. So it's an interesting kind of tension to think about ethnography as journalism and ethnography as social science, and how we're kind of caught in the middle with what we should do with these field notes.

I really appreciate from Steven's book the mapping out of techniques of how to do adversarial testing of our field notes, which is something I want to teach in my methods class. As Lubet points out, opposing counsel in an ethnographic trial could sometimes readily tell a completely different story based on identical facts. Now, the thing is—and I guess I don't blame him because he's a law scholar —Steven could have acknowledged and learned from ethnographers and sociologists who are thinking along these lines. In particular, in the *AJS* article "A Pragmatist Approach to Causality in Ethnography," Iddo Tavory and Stefan Timmermans talked precisely about how to consider alternative causal narratives. And, of course, their book *Abductive Analysis* goes into a longer meditation on how to think this way. If you read Steven's book, you might think that ethnographers are not thinking about or doing any of these things. But there are people that are.

While I do sometimes worry about the lying ethnographer that may be out there, I also worry in Steven's book about the presumption of guilt. He talks about how there is nobody keeping the ethnographer honest, which implies that we're not being honest. Well, I have a problem with that because sometimes it seems to me that in Lubet's interest to build a case of reasonable doubt against the ethnographer's argument, he winds up making counterarguments that to me seem implausible.

Let’s talk about the one very basic example of whether or not those impoverished Mississippi students that are talked about in Kathy Eden and Luke Shaefer’s book *$2 a Day* really knew about elevators or not. They probably did. I buy that the kids probably knew about elevators, even though they acted like they didn’t. But Lubet won’t let it go. He wants to give you as many different angles to show you that these kids probably knew about elevators. At one point, he finds himself saying that, because the person who invented the elevator safety door was African American and we know that some state textbooks mention this fact, these students probably read that textbook. Moreover, because they're black, it should have stood out that a black man invented the elevator safety door. They must know that. I find this just as implausible as the notion that they don't know what an elevator is. I mean, it seems kind of funny. But I worry about this runaway train of reasonable doubt: you say that the ethnographer might be lying, and to try to prove it you wind up building alternative scenarios that could be just as implausible.

Also, and Lubet has been criticized for this before so this isn't new, but he's all too happy to use statements from police, lawyers, et cetera, to cast doubt on ethnographers. This can sometimes make it feel like his goal is to make ethnographers look bad. For instance, in building the case against Goffman's claim that Tim got a three-year probation sentence, he points out that a public defender told him that Pennsylvania doesn't have fixed probation terms for juveniles. Now, I pointed out to him, as he knows, in an e-mail exchange there must be some exceptions because I myself got a fixed-term probation sentence in Pennsylvania as a juvenile. I thought maybe it changed over time, but he looked into it, and it hasn't changed over time. Steven shared with me an email from a lawyer that noted that judges “generally” followed the guideline of not assigning a fixed probation term, implying that sometimes they may have given a fixed term. But in his book, Lubet writes, the Juvenile Act “always called for indeterminate terms of probation." By dropping the caveat in the book of "generally" and instead emphasizing “always,” Lubet makes it sound impossible that somebody could receive a fixed term, which is a much heavier indictment than "unlikely." He suggests that Alice’s claim is absolutely impossible when it’s not, branding her as a liar when the fixed-term probation statement could conceivably be true.

There is an important difference between what *generally* happens, what is *unlikely*, and what is *impossible*. Even if Lubet is right regarding his overall deconstruction of Alice’s claims about Tim's case, I'm trying to make a broader point. (I will concede that Lubet is quite convincing that Tim probably didn't get that probation term, but for reasons other than the one just discussed). It’s a point about his willingness to privilege the accounts of certain actors (e.g., cops, lawyers) that tell him how things generally go or how things are supposed to go, to impugn the integrity of ethnographers. And we all know that the written law differs from the law as practiced by situated actors with organizational prerogatives.

To his credit, Lubet does admit times when his skepticism was off base. For instance, he notes that he doubted Randol Contreras's claim that his subject demanded and received a transfer upstate from Rikers, but it turned out there was a time when this was allowed in the early 1990s, which is the time that Contreras was doing his research.

I have one question for Steven. He makes a lot about the fact that Matt Desmond employs a fact checker, and he did too. And I think it's admirable. But neither one of them tells us what was wrong that the fact checker picked up. I just really want to know the value added before I decide that we ought to all be employing fact checkers. If they uncover like six or seven really minor things, then I'm not going to be bothered to get a fact checker. But if they totally changed the course of his book, you know, I would like to know that.

Okay. Now to get a little more critical. At times I think there is a naïve understanding of ethnography. For instance, Lubet puts a lot on the idea that ethnography is grounded in trust. I don't need to say much about this. I think Mitchell Duneier has written very thoughtfully about how much ethnographers can actually do in the absence of trust. Journalism often reveals the same lesson, as do taxi cab confessionals.

This whole issue of trust is something ethnographers are talking a lot about and challenging. But more deeply, Lubet's idea that ethnographers must determine which subjects are trustworthy, which is very important for an eyewitness in a trial, may not matter so much to ethnographers—especially if you're focusing on the actor's point of view, the way they make sense of themselves, rather than the facticity of accounts. Lubet does acknowledge the difference here. But I think he under-acknowledges how often the actor's point of view and their definition of what's going on is the ethnographer's central object of inquiry—not the fact that our subjects accounts may not be accurate.

Also, and more importantly, ethnographers' claims are context-driven and grounded in observation. Subjects are not equivalent to eyewitness. In a trial, all you have is eyewitnesses and documents—all after the fact. Ethnographers are, or at least they should be, primarily relying on direct observations. Therefore, the credibility of what the subject tells you is not the central axis around which everything revolves in the way that it would be for a courtroom trial. Because courtroom testimony relies entirely on post hoc remembrances or documents, trustworthiness is more essential to that enterprise than it is to ethnography.

Now, another thing: This problematic notion, which Lubet puts a lot on, that records created contemporaneously with an event "by someone with no likely incentive to fabricate" can generally be treated as objective. For example, if a hospital worker is recording medical records for a routine procedure and doesn't know that, down the road, the patient will suffer a heart attack in the operating room and die and that those records will become evidence in trial, Lubet would say that we can trust the record as objective.

I disagree with this. This dichotomy of lying vs. truthtelling is wrong. Think of Garfinkel's classic essay on "indexicality" in coding medical records, in which he showed that administrators used their knowledge of what the records would be used for in the future. That is, they took account of the organizational goals and culture that they were a part of in deciding how to interpret and code the data. There is no neutrality. Rather than viewing an organization's records as representing what "really" happened, Rhonda Levine notes that Garfinkel, "saw in those records the practice of the people who make records: the hedges they play, the shortcuts they take, the theories in their heads, the purposes at hand." As Garfinkel notes, then, there are often very good organizational reasons for very bad clinical records.

Also, Lubet says that quantitative sociology engages in adversarial testing, i.e. replication, implying that the issues raised are a uniquely qualitative problem, or more of a qualitative problem. Quantitative social science *is* more readily replicable, but that doesn't mean that it's actually being replicated. Who gets a dissertation, a top publication, or an elite tenure-track job for doing a replication? Few pursue it. Replication tends to only happen when a finding is striking enough, usually meaning that it goes against what scholars expect or has an unusually large effect that warrants greater scrutiny. One example: McPherson, et al.'s article, "Social Isolation in America." It just seemed so out of tune with all the other measures of social isolation that it screamed out for replication. More recently, we can think of Saperstein and Penner's contentious research on racial fluidity. But these are actually rare instances, the exceptions that prove the rule.

I have one more small point to make. I don't really understand why Steven is asking us to cite the dates and locations of interviews and observations that we include in our write ups. It's not something that journalists generally do in their own work. It seems incredibly cumbersome in the narrative, and I feel like I can make up the dates that an interview happened just as easily as I can make up the interview data.

So I'm going to conclude here. I was going to say a little bit about ethics, but apparently I'm now on a panel about ethics tomorrow, so I'll leave that one alone. There is a subtext to Lubet's book. It isn't the point of his book, but underlying it is a tension between ethnographic conventions and the realities of the information age. Amidst calls—and Phil alluded to this in his talk a little bit—for data transparency and replicability, ethnographers continue to conceal their subjects' identities and the places where they carried out their research. Despite the ubiquity of social media, few ethnographers are following their subjects online. Although every smart phone is equipped with a camera and a digital recorder, many ethnographers still rely entirely on field notes; they reconstruct scenes and quotes after the fact and present them as if that's the best we can do to faithfully transcribe the social scenes we witness. It's actually really easy to do a lot better than that. So to me, perhaps one of the greatest contributions of this book is that it helps drag ethnography, maybe even kicking and screaming against its will, into the 21st Century. For that I thank you, Steven.

MR. GARY ALAN FINE: Our third speaker is Shamus Khan.

MR. SHAMUS KHAN: Thank you. I'm also not going to use the mic. Colin and I share authorship and loudness, I guess.

MR. COLIN JEROLMACK: What? I didn't hear you.

MR. SHAMUS KHAN: I have a long history with this book, I think, as you know. Which is that I was the reviewer of the proposal when it first went to the publisher, and I read it twice before it was published. And now I've read it a third time, different iterations of it. And so in preparing my notes, I made the poor decision of reading it and then waiting two weeks to start to write things up, so I'm somewhat unclear on what was said in different iterations of the very text, which suggests something about the difficulty of recollection and the importance of taking notes in an immediate moment. I'm going to highlight less of the argument of the book, although I'm going to read from the book a couple of times. And instead, draw on some current work that I'm doing in order to reflect on the core idea of the book.

And my purpose here was to try and use the book as a sort of interrogation of my current ethnography. I have been doing a two-year study of a community, and it has been a two-year study of rape and sexual violence at Colombia University—so I was a part of a very large research team. We have I think twenty-five people on the team, and I'm one of the five primary research scientists. And we did things like design a survey, a random population survey within the campus, designed an app and had people check in every day for sixty days answering the same basic set of questions about their sexual behavior, drinking, mood, socialization, sleep, things like that. And then interviewing over 150 people, many of them multiple times, about experiences that they have with sex and sexual violence. And in addition, had a series of people embedded in different parts of campus. So embedded in the kind of classic ethnographic sense, visiting fraternities and sororities, joining intramural sports teams, going to religious student organizations, trying to do a kind of community portrait in order to understand sexual violence.

I thought to myself, would law be an ideal space to use epistemically in order to deal with and better understand sexual violence? So I thought to myself, let's take trials, for example, as a model for the understanding of sexual violence, and ask ourselves do they get us closer to truth?

Or, instead, what are the series of problems or challenges that emerge if we use basic legal principles in order to evaluate these kind of data.

And I really like this book. Like Colin, I initially wanted to not like it when I first read the proposal. And like Gary, I pushed in my reviews to constantly say do not make this book about Alice Goffman. Because if it's about Alice Goffman, it's very easily dismissed. By that I mean, one could simply say, well, this was a problem for Goffman, but if we ignore Goffman we can ignore Professor Lubet and we need not pay any attention to him. And I think quite happily he engaged in a really systematic reading of fifty-plus ethnographies, I believe --

MR. STEVEN LUBET: 100.

MR. SHAMUS KHAN: 100. Well, in the text you say something –

MR. STEVEN LUBET: You said how many did I read.

MR. SHAMUS KHAN: Did you read. Okay.

MR. STEVEN LUBET: Not how many did I write about.

MR. SHAMUS KHAN: Okay.

MR. STEVEN LUBET: Some of them were garbage.

MR. SHAMUS KHAN: Well, actually, you know, if fifty were good out of 100, we're better than most scientists. Because if you were to say fifty out of 100 published papers are worth writing about, we're doing pretty well.

I suspect you had guidance, though, as to which ones to potentially read. And I must say it's a really, really nice systematic treatment of ethnography and using the law as a way to think through ethnography. But what is disappointing about the book is that the same critical analytic approaches that he takes in dissecting ethnography, he does not use on law and legal principles. I suspect I am here going to be speaking to someone who knows far more about this than I do, someone that knows much more about legal trial and procedures and the ways in which those are deployed in order to generate sets of outcomes for people. But I might just pose a few things for us to think about.

So in the context of sexual violence, if we were to look at sexual violence, rape has one of the lowest conviction rates of all violent crimes. And the question might be why? If we were to compare it to other kinds of assaults—say, assault and battery—the rates of a conviction are more than five times higher for assault and battery than they are for rape. Now, part of this is a reporting problem; that is, rapes are reported at a far lower rate. They have about half the rate of reporting. But even that doesn't explain this as a phenomenon. Now, we could say, well, maybe in rape there is a lot more false reporting. But we would need to have false reporting rates about four times higher than the highest current estimates of false reporting rates in order to generate this outcome. So we might ask why? Why is it the case that trials consistently generate such low rates of conviction for rape and sexual violence? We might also ask other things, like why is it that trials have much higher conviction rates for black defendants than for white ones, and look and see what is it about the ways in which trials work that they generate particular kinds of knowledge or particular forms of knowing.

And what I push Professor Lubet to perhaps think about and talk to us about today—so Colin asked a question, it's a small question. My question is maybe a bigger one that you may not have time to fully engage with, but nonetheless I would like you to think about the problems of trial. The problems of law. The ways in which these systems don't necessarily produce justice. Which may not be in our interest, but certainly don't get us to truth. Instead they're a set of procedures that produce outcomes. A set of procedures that produce outcomes which somewhat naively in the book divorce truth, knowledge, and power from one another.

I would suggest, just as Colin said earlier, that there is a kind of impugning of ethnographers compared to other kinds of accounts. And I might push this further and ask what is the relationship between power and knowledge? Now, I'm not asking you to reflect on Foucault here, but it might be a little bit helpful to think about the ways in which forms of knowledge and reliable knowledge are intimately tied up with systems of power and inequality. And this is something that if we simply take, say, a blind justice view, we may not be able to actually get closer to the truth of our particular subjects.

Now, here, part of my own sentiments may be quite different than Professor Lubet, so I want to read for a moment a quote, not from your book, but from someone you quoted. This is in the preface. It is a quote that says, "Few critical ethnographers think in the language of evidence. They instead think about experience, emotion, events, processes, performances, narratives, poetics, and the politics of possibility." To this, Professor Lubet says, "Those are worthwhile inquiries, but they are well outside my empirical wheelhouse." And I want to highlight that perhaps this is a central tension of the book. A central tension of the book wherein there is a certain assumption about the manifestness of truth, or rather, its objective quality, which exists in the world and that methods reveal and that these other things, like narratives, experiences, emotions, processes, performances, and narratives simply exist outside of that. We kind of end up with this sort of dichotomous relationship between objective truth, which procedures can discover, as opposed to other stuff that's kind of ambiguous. This may relate a little bit to Philip's overall point about, say, is ethnography a science? That if it's the revealing of those sets of things, perhaps it's not a science.

I'm going to raise another aspect of this. I'm going to get back to the sexual assault, but this is actually particularly pertinent to me this week, and it speaks directly to what we might do with our notes and the consequences of that. So this Monday, I received a subpoena for all of my field notes for everything I'd done on the St. Paul's book, a book where I did the research years ago and I went and I lived within a community. And some two or three years ago there was a major case of a sexual assault on that campus. And I suspect, though I don't know, that the lawyers are attempting to construct an account of a history of neglect at the institution. And they expect that within my notes, there will be something that can help them in this enterprise. I don't know because I don't remember. I certainly wrote in that text about a history of sexual relations on campus, which I identified as particularly problematic. But what the lawyers will find is uncertain to me. There are, however, things in those notes that I do know about which were never revealed in the book and I've never written about before. Things like the death of a student that happened on campus the year that I was there, which I chose not to write about because it was particularly sensational and struck me as something that was important for that year itself, but not really necessary for the argument that I was making. Things about people who were having affairs that I believe have never come to light. Things about students who had first sexual experiences with men who are fellow men who I don't believe are currently gay.

Now, the question to me becomes if we are to follow the suggestion of yours, which is that notes need to be available and anonymized as minimally as possible, I have to start thinking through what that would mean. Now, what's often proposed is the idea of all that we would gain from this process. What is less considered is what we would lose. What I might chose not to write down because I know that should I write that down, it's actually going to be subject to inspection from all kinds of people. One of the great challenges of doing an ethnography is that in the moment one cannot know what relevant information is. You simply need to write down as much as what you can remember, as much as what you saw that day, and try and present it quite literally and then, often years later, impose an interpretative frame upon that information.

Now, one of my concerns with this sense of suggestion that we receive in this current climate is how would ethnographers respond if they knew that everything was to be subject to inspection from others? I certainly think that the notes I would have taken at St. Paul's would be quite different if I was aware of the future me subject to some kind of subpoena. And to be honest, it's nice to have to think through this in a law school because I'm not sure what I'm going to do about this. I may in fact choose to act as a journalist and say, "No, these notes were constructed on the condition of anonymity and the condition of confidentiality, and I realize that doesn't have legal standing, but it has ethical standing, and so that's how I need to act in this context." I'm not actually quite sure about this. But I do think, and this relates to my overall criticism, that often the arguments are made so strongly with the assumption of the correctness of their position that the same analytic focus isn't returned back onto the thing that's sort of being proposed as a solution.

So we know that open data is the solution, except that quantitative scholars do open data all the time and it's really hard to reproduce results. I mean, really hard to reproduce results. So if it's the solution to a problem, it doesn't seem to be working very well. And we might say, "Well, perhaps there just needs to be a better kind of discussion as to what this solution would be." And I say this as someone who has constructed a large-scale historical data archive and made it public. I mean, I'm part of the tribe. But I'm going to suggest that we use part of the same analytic precise or piercing critiques to analyze the solutions that were generated. Let me return for a moment to sexual violence and help elucidate why it's really difficult to write on this and why it is that law, or getting to the truth of what happened, can be a real challenge.

So over the course of this project we interviewed students. These were two-hour interviews. I actually have the dates and times and places where they were done. And in the course of those interviews generated seventy-six accounts of some kind of sexual violence. This is higher than the normal rate, but some people were recruited through fieldwork and other people were recruited because they had a story to tell. Often when people told us these stories, one of things that we wanted to do is to follow-up to gather more details. So this meant getting a transcript of the account, reviewing it—and as you might imagine, I had a very large research team—and then asking subsequent follow-up questions, such as, “We would like more context.” And one of the real problems that I discovered very quickly was that people's accounts changed of the basic thing that had happened. So in a second-round interview, one would know with really precise detail what they had said in the previous round, because it was your job to do so, and in subsequent rounds of interviews would be able to generate slightly different narratives. There were additional details, previous details that were provided and were no longer present, or some things actually changed.

Now this made me initially think, “Well, maybe there is a lot of fabrication of this phenomenon.” Something politically I didn't believe, but as a researcher, I thought I should be open to this as a possibility. And I ended up talking to the clinical psychologist on our team who said, “Of course the details change.” When people experience highly traumatic events, one of the things they do is they're not really able to remember all of the details. And more importantly, as people have social relationships and ways in which they start to tell a phenomenon, they settle into some parts of the story and not others. I began to think, perhaps somewhat curiously to you all, of the Gospels. So if one has any interest in the Gospels of Matthew, Mark, Luke and John, you'll realize that in some, there is no virgin birth, and in others, there is. Right? The Gospels are not consistent overall in their telling of the life of Christ. You might use this as a sort of leverage point to say, “Well, we have these multiple perspectives on a phenomenon which don't actually converge upon a core truth.” Or if they do, there are some differences within that truth.

One of the basic problems with what we were doing is that we were actually acting a lot like lawyers. I understand why there is such low conviction rates within assault. If you have a standard of truth which is reasonable doubt, it's going to be nearly impossible to convict someone for an assault because if one person says it didn't happen, you have reasonable doubt. This is very, very clear to me. And the standards of evidentiary treatment of, say, getting the stories right and getting them consistent are often inconsistent with how people experience their own truths. So if you were to ask me over time how it was that I ended up in the job that I ended up in, I would probably tell you very different stories at different parts and times in my life. And which one of these are true? Well, we simply don't know.

The problem is that the adjudication of those multiple truths requires something that goes beyond simply a mechanical procedure. But also that certain truths, like the ones that we were trying to reveal, were not subject to a standard of objective truth. And if they were, there was no way to actually reach that thing.

I'm going to end, because I've been going for too long—I know I said I wasn't going to even need minutes—with three points. The first is this one that I've just made. I would like to hear Professor Lubet think for a moment about the problems with trials and truth. Because if he's asking ethnographers to use this as a basis for pushing forward in their own use of evidence, then I would like to know the context where he truly is expert and he can give us some guidelines that push beyond. And I won't read them, but, actually, they're very nicely and succinctly summarized on pages six and seven in the text relative to accuracy, candor, and documentation. So these three basic core principles that he builds beyond.

The second is a greater acknowledgement of truth, knowledge, and power, and the ways in which certain claims are more likely to be accepted as true, certain positions are more likely to be recognized as objective positions. And there has been so much written, say, in critical race theory of law, for example. So, looking at Delgado and Stafancic and all these people who have written in this area, it is sort of absent here. There is what I would suggest is a slightly naïve presentation of the idea of truth.

The third point, the final point, which is one I didn't really get to elaborate on, but I potentially will because I'm also on another panel tomorrow, is this is a very individual driven account of ethnography where one understands ethnography by reading testimonies of individual people, which is a law-based approach to ethnography. But a lot of ethnographers don't take individuals as their units of analysis. Many of us, in fact, are really interested in space. So in the context of sexual assault, what I was going to talk about was that some of the ways in which we think about minimizing sexual violence on college campuses is thinking about the fact that when students leave a bar, one of the places they have to go back to is a dorm room, which has a desk, a chair and a bed. That slightly over-determines a set of future outcomes, then, say, if there were other contexts or spaces that they might exist within. Or we might draw more extensively on, say, Rob Sampson's work, not within ethnography, but on this great city of Chicago, or even Colin's work on the study of pigeons, to recognize—which is not really a study of pigeons—that often our characters and major explanations are not people. We're not interested in that. We can elicit no testimony from them. And how is it that if we think of a less-populated ethnography, or an ethnography that is based in spaces and relations, that we can do this kind of work? I'll end there. Thank you for such a provocative work. I'm excited to have this conversation over the next few days.

MR. GARY ALAN FINE: Our fourth speaker is Mary Pattillo.

MS. MARY PATTILLO: I am usually very loud, but this group makes me sound soft. So, just coincidentally, as we are discussing a book by a lawyer about a field that is somewhat new to him, mainly ethnography, I happen to be an ethnographer who has recently begun a qualitative research project on a field that is somewhat new to me, namely lawyers and the legal system.

For roughly the past year I've been doing ethnography in courtrooms. I started off slowly just to get my bearings, learn the lingo, find my way around, figure out who was who, what was what, what were people talking about. My research was obviously not designed to prepare me to talk about this book for today. It has other purposes. But the fact that I've been spending a lot of time in courtrooms at the same moment that I'm called to think about a book that puts ethnography on trial in a courtroom brings me to my opening point, which is that I don't want ethnography to look anything like a courtroom. And definitely not like the courtrooms I've been observing. And I don't want ethnographers to seem anything like lawyers, and not the lawyers I've been observing.

So let me read for you a snippet of my field notes to give an example of what I mean. So the notes are from July. I'm really in this research right now. I'm not going to name the county or the courthouse, and I'm going to give a pseudonym to the judge, which we can talk about anonymity later. But it's part of a much bigger project. There are multiple PIs, we haven't yet decided what we're going to do about these questions, so I'm not going to talk about that here. But these are from July.

So in Judge Jones's court in the afternoon there was a long trial for a domestic battery charge, which was about a stepfather hitting his stepson. I had met the family in the waiting room before the trial and had given them fliers about my research, and the son and daughter seemed interested in talking. Then I watched the trial. It didn't have anything to do with what I was researching—fines and fees. The stepson was seventeen at the time, but now eighteen. He had a long list of juvenile infractions. And in this case he basically attacked his mother, which was a totally separate charge now pending against him, and then his father slapped him because of that attack. So that was the charge that this case was about and that was the charge the stepfather was now facing.

The two lawyers were the young woman prosecutor, who I name in my field notes but I won't name here, and the young male defense attorney, a public defender. The stepson was the victim, and, while I was observing, the witness for this domestic battery case. At one point he said in response to what was a pointed short-answer question from the prosecutor, "My testimony today is that there is nothing wrong with somebody disciplining their child who you've known your whole life. I'm sure if you have kids you would discipline them too." To that, the judge told him only to answer the questions he's asked. The judge said, "This is not a living room. Not a conversation."

That's the part I want to highlight. Ethnography happens in the living room. And ethnography aspires to be a conversation. In the criminal and misdemeanor courts that I observe, the defendant almost never speaks and is rarely spoken to. Not by the judge, not by the prosecutor, not by his or her defense attorney. I completely understand that a lot more conversation and discussion and information gathering happens before the courtroom interaction, especially between the defense attorney and his or her client. But it was often clear in the courtroom that there hadn't been even that much conversation beforehand either, once they got to the courtroom.

In contrast, we ethnographers let the people we interact with talk for hours. The more they talk, the better. We want to make every interaction feel like it's in their living rooms. Don't get me wrong, I'm no expert on the lawyerly arts of data gathering and fact presentation, but my introductory forays into the court make me leery of comparisons with or words of wisdom from that world given the kind of stinted communication that I see there.

In the preface to *Interrogating Ethnography*, Lubet sets out his goal as, "To assess the use of evidence in ethnography by comparing it to the standards that have been developed to determine the reliability of evidence in law practice." But I think if the aims of the two endeavors are so very different, then why should the standards be the same? So I look forward to our discussion over the next two days about what really are the two endeavors, and I think we've already gotten on the table a lot of questions around those two endeavors.

What I would like to do is focus first mostly on the book, and then come back and conclude with that question about what the two endeavors are. So first focusing on the book. It seems pretty clear to me that Lubet really wants to make ethnography better. So I'm going to take him at his word on that, and I'm going to think about many of the suggestions. And I group them into three buckets and three practical reactions to the arguments that he puts forth; they are, simply, ideas that I like, ideas that I don't like, and things that Lubet suggests that I think we already do. I'll give two examples from each of those buckets, and I'm going to shorten my first two examples because I do want the author to be able to say something after all of this critique, and hopefully have some time for us to discuss. So the first thing: things that I like. This is where I'm going to shorten.

Perhaps my biggest takeaway from the book is that I'm going to work hard in my own practice to represent subject's recall as exactly that—recall—rather than as a supposed contemporaneous and witnessed account of events. So using words like, "so and so reported that" and "so and so remembered that," as opposed to sometimes we paint whole pictures as if all of that actually happened. So a lesson to us ethnographers, don't tell a story that someone recounted to us as if it actually happened. Point very well taken. Second thing that I like. No composites. Lubet goes through a reading of Laurence Ralph's *Renegade Dreams* where he—I think you use that great phrase that this composite that Laurence Ralph created was the "worst of both ethnographic worlds." And I generally came to agree with that. I don't see any need for a composite figure, basically melding multiple people together, beyond anonymity, which I'll talk about in a minute. So, again, point well taken, and I definitely agree.

Okay. Now things I don't agree with. So the first is this issue of pseudonyms and anonymity. In my own work, I've done it both ways. In my first book, *Black Picket Fences*, I used all pseudonyms. In my second book, *Black on the Block*, I had promised the people anonymity when I started the research, and then when I started writing it, it became clear that I couldn't write about the politics of the neighborhood without naming the neighborhood, so I had to name the neighborhood. And then that would mean that so many of the people would become pretty obvious. So I went back to everybody who was quoted in the book, I sent them the passages where they appeared, and I asked them if I could use their real name, and if not they could choose a pseudonym. Most, in fact, chose their real names.

So why the difference in the two books? I think the difference lies in the kinds of claims I wanted to make and the kinds of research I was doing. *Black on the Block*, the second book where I had mostly real names, was about the working of a place. A place whose location near the University of Chicago and along the lake was crucial for its development. A place where there were power brokers and controversies and deals and rivalries and developers and people whose names had shown up already in newspapers and public documents over and over again. Not everyone's identity and biography was salient, but many people were. *Black Picket Fences*, the first book, on the other hand, was much more about the private sphere. Families, relationships, classrooms, funerals, summer sports leagues, church choirs and block clubs. Whereas the players in North Kenwood, Oakland, which is the neighborhood that I name and it's the neighborhood in the second book, were acting out on the public stages of campaign trails and courtrooms and organizations subject to Open Meetings Act, the people in *Black Picket Fences*, in a neighborhood that I give a pseudonym to, were living in the relative anonymity of their private lives.

This was especially true before the internet and social media. So we actually might have a conversation about how the internet has changed maybe the preferences and the effectiveness of anonymity. But my larger point is that these two were very different types of ethnographic projects. The stakeholders in *Black on the Block* were already living their lives out loud, whereas the families in *Black Picket Fences* definitely were not.

The second thing I don't agree with is fact checking. So I hate to eschew fact checking in this surreal era of "the largest audience to ever witness the inauguration," so fact checking sometimes is obviously important. And I'm not completely rejecting fact checking. But I'm rather just challenging Lubet's sanguine characterization of how it might get done. Lubet commends Matt Desmond for having hired an independent fact checker for *Eviction*, but rightly notes, "Many ethnographers will be working with smaller budgets than Desmond." And I would say that's probably a gross understatement. So Lubet goes on to quote Desmond about a possible solution, which is the following. This is Desmond speaking now. "I think there are ways we can do this that don't require massive amounts of resources. So let's say you and I were in grad school together and I was doing ethnography. I could give you my field notes and you could do the same for me and we could fact check each other's claims, and we could write that in our publication so that we hold each other accountable for that." So that sounds good. But that would slow both ethnographers down in terms of their progress on their own dissertations, and their funding is for a fixed number of years. Will universities extend their funding for this additional work? No matter how you slice it, time is always money. Moreover, ethnographers are trained as ethnographers, not as fact checkers, which I imagine is a skill of its own.

So I'm not completely running away from this charge of some kind of fact checking because I very strongly agree with Lubet's point that there has to be some checks and balances. But there is a lot of infrastructural, logistical, and institutional work to be done to get to what Lubet suggests, and I'm not convinced that the payoff is actually large enough for that.

Okay. So those are the things I don't like. And now for the things that I think we already do that Lubet suggests. So Lubet makes a call for replication, revisits and re-interviews. While we don't do this in the way that Lubet envisions—going back to the exact same place, talking to the exact same people—there definitely is ethnographic replication. For example, when I was working on *Black on the Block*, which is a study of black gentrification in North Kenwood, Oakland, I struggled to figure out really what was new that I could contribute beyond what many ethnographies had already written about black gentrification in particular, and gentrification more broadly. So in the book I wrote, "Recently, Monique Taylor, Sabiyha Prince, and John L. Jackson have done similar research on middle- and upper-income blacks moving back to Harlem, and Michelle Boyd has written about these processes in Chicago, and Derek Hyra has studied both black Harlem and black Chicago. In a rare and encouraging kind of qualitative verification, there are remarkable similarities in the scenarios portrayed and the sentiments expressed by gentrifiers and established residents across these settings."

So that's what I wrote in *Black on the Block*. I was basically admitting that my book was but a replication of many previous studies. Luckily it was my second book, I already had tenure, and I didn't have to make that big splash. But we do replication all the time. I didn't have to be first. And when we repeat data and replicate findings, the ethnographer is basically doing exactly what Lubet says already. But I also think that even when we have similar findings, the ethnographer him or herself is a research tool, such that the precise tilt of our inquisitive heads yields different insights and different discoveries. So I belong to the Zora Neale Hurston school of research, who writes, "Research is formalized curiosity. It is poking and prying with a purpose. It is a seeking that he who wishes may know the cosmic secrets of the world and they that dwell therein."

So while I might have highlighted some new cosmic secrets about black gentrification and black gentrifiers, many of the quotes and the descriptions are remarkably similar across the books that I just named on this topic. We didn't go to the same place, but studying the same thing, even in different places, created a knowledge base from which to be more sure that we really actually saw real things that were real social phenomena.

Another thing that I think that we already do that Lubet suggests that we do is have formal skepticism. In fact, Lubet frequently cites one of the more famous examples of such formal skepticism, which is the debates between Mitchell Duneier and Eric Klinenberg that played out in our flagship journal, the *American Sociological Review*, about Eric Klinenberg's book on the heat wave here in Chicago. And, of course, Alice Goffman's book was subject to that formal skepticism as well. It did not receive uniform praise in our discipline, with Victor Rios's review in *American Journal of Sociology* and critiques at our conference in an Author Meets Critic session as just two examples. And then this kind of formal skepticism happens all the time in graduate classes where we tear books apart. We look at them from every angle and talk about their data and their method and their believability and their consistencies and their assumptions and argumentation, so on and so forth.

So let me conclude by thinking about what is the purpose of ethnography and does Lubet's exercise of the ethnographic trial align with the goals of ethnography. I think Lubet actually understands the crux of the matter quite well. He writes in Chapter Seven, "The masking of subjects thus underpins the view that distinct facts are less important than epistemic truths." I think he characterizes this right, but I'd have one editorial change which falls in line with what Shamus was saying. I would say that distinct facts are less important than epistemic *arguments*.

Ethnographers claim that we know something after having observed and participated in a social world for an extended period of time. We don't claim that what we know is an unimpeachable truth. The cosmic secrets of the world are always fuzzy and slippery and playful, shifty, ever-changing, and beyond our full grasp. Instead, what we know is really a set of strong arguments about the meaning and importance of what we see. Yes, those arguments are built upon a set of distinct facts, and we have those unadulterated facts in our field notes, our interview transcripts, and our documentary ephemera. But making the argument from those facts is not the same thing as presenting the raw data. As Lubet himself recognizes, like lawyers we are building a theory of the case. But lawyers have much more incentive than ethnographers to leave out inconvenient facts and contrary evidence. As ethnographers we have the luxury of beginning our fact gathering with very few allegiances or preconceptions. We can have an inductive process. Whereas, lawyers are looking for evidence to convict or exonerate the defendant. Our theory of the case, our epistemic argument, our exposure of cosmic secrets can develop from the data.

We shouldn't make things up, and we shouldn't claim that we've seen things we've only heard secondhand, and in these areas, Lubet has made very important interventions and admonishments. But at the same time, people who let us into their living rooms to have a conversation that sometimes lasts for years aren't necessarily signing up to have their lives on blast in our books. There is always room for improvement in ensuring that ethnographers did what they did and went where they say they went. But I hope that we can strengthen our conventions and our practices with the goal of preserving our commitment to understanding the living rooms of social life as opposed to adopting the model of the courtroom. Thank you. And I look forward to your reply.

MR. GARY ALAN FINE: Steven, the floor is yours.

MR. STEVEN LUBET: Thank you. First let me show you the book. These are the only two existing copies. The ship date is today. It was supposed to be last week. I had hoped to be able to distribute them, at least to members of the panel, but, you know, that's show business.

I do want to say I feel quite fortunate that only two members of the panel wanted to hate the book. I think that's probably better than the average for an assigned text in law school, so I'm doing pretty well. I knew that Colin wanted to hate the book. And I want to point out it was my suggestion to invite him. I think it's extremely important always to have a dialectic, and it would make no sense at all to have a session like this without people who approached the project with extreme skepticism. So thank you, Colin. And also Shamus and Mary. And, Phillip, you were far too easy on me.

It's tempting, of course, to begin with Colin's comments, so I will. I am not going to address much of what he had to say about *On the Run*. I did everything I could to minimize my discussion of *On the Run* in *Interrogating Ethnography*. I do think the book had severe problems. I think there were five definably untrue vignettes in it, things that were provably untrue. Nonetheless, I wrote that it was a gripping book, well read, and worth reading. If you read my preface, you'll see that I described it as fascinating and I amply acknowledged its virtues.

I do want to point out, Colin, that the fixed term of probation for Tim was really a very minor point when I was explaining that the vignette about the arrest of Chuck and Tim was impossible to have happened as Professor Goffman described it. The story is about how poor Chuck and Tim couldn't avoid being arrested because they were in a borrowed car, but the police didn't believe them, and they were on their way to school, and Tim was arrested and convicted for riding in a stolen car. That's the story. Now, there is no such crime in Pennsylvania as riding in a stolen car. There is no known incident of any juvenile being convicted for riding in a stolen car. The two examples that Professor Goffman put up on her website were both non-convictions and non-juveniles. But I got the police reports for Chuck and Tim's arrest, which are in a footnote. I got the police reports and they contradict Professor Goffman's version of events.

Now, you can say, "Well, you know, police reports aren't always accurate." But, seriously, nobody was thinking that Chuck and Tim were going to be subjects of a book and figured out they had better gin up some story about the arrests. So here is what actually happened. And I know you know this, Colin, because it's in a footnote. They weren't arrested together; they were arrested separately. They weren't arrested on the way to school, Chuck was arrested at 2:00 in the morning. He wasn't arrested in a car that had been stolen in California, as Professor Goffman says. He was arrested in a car that was stolen in Philadelphia, although it did have Florida license plates because it was stolen from a rental car lot. That's what happened.

Tim was arrested in the act of hot wiring a different car. So whether he got two years or three years of fixed-term probation, I don't really know. I don't think it happened. Maybe it did. But he was not convicted for riding in a stolen car. That didn't happen. Now, does Professor Goffman believe it happened? Sure. I don't question that. I don't think she made up the story. It's in my chapter on credulity. And here, Mary, I really appreciate what you had to say. I think if I made an important contribution it is that ethnographers have to distinguish between things that they heard and things they saw. And many, many of these ethnographies. Fifty. Is fifty okay?

MR. SHAMUS KHAN: It was a little more than fifty.

MR. STEVEN LUBET: Yeah. It was more than fifty. That's just the ones I wrote about. I can't tell you how many passages I read that recounted events saying: "Here is what happened. Here are things that happened." But then you peel back behind it, and it's hearsay. The ethnographer just heard about the events from someone else. Now, is hearsay ever reliable? Sure it is. Is hearsay ever usable to prove attitudes or what people believe, or how people interact in a neighborhood? Sure it is. But it needs to be made explicit. Readers need to know when you're describing something you saw and when you're describing something that you heard. I don't think that is controversial, but I don't think it's always, or even commonly, done in ethnography.

The elevator story, Colin. Well the elevator story devotes two-and-a-half pages of proving that adolescents in Mississippi know about elevators. Now, why on earth would somebody have to spend two-and-a-half pages proving that adolescents in Mississippi know about elevators? I agree, that seems just bizarre. So here is what happened. It is a vignette in the book *$2.00 a Day*, by Edin and Shafer—whom, by the way, I described as exceptionally meticulous. My book is filled with praise for them. But there is one vignette in which they describe a group of Mississippi middle school kids going to Washington on a school trip, sixth graders, who had never heard of elevators. And they were so unbelieving in elevators that when the teacher said if you get in this box you will go to another floor, they thought it was a joke.

Now come on. I relayed this story to half a dozen Mississippians. None of them believed it, and most of them were offended by it. They said, "You know, this is not a third-world country we're living in." So what do you do? How do you prove the negative about adolescents in Mississippi who have seen airplanes, who have television, who have microwaves, who ride in automobiles, who know about computers, who have seen railroad trains, how do I prove that they also know about elevators? It's a challenge. It's a problem. If you were a lawyer, you would spend a week on this. I only spent two-and-a-half pages.

MR. PHILIP COHEN: But six months of working on it.

MR. STEVEN LUBET: Yes. But the point is it's a set piece. Who gives a damn about the elevator story, really? It was one paragraph in *$2 a Day*. I used it as a set piece to show

how circumstantial evidence can build a case. So what else did I use to show that kids know about elevators? Did you know there is a website about situation comedies in which babies are born in elevators. That's how common elevators are on television. And the two most popular African American themed sitcoms ever take place in high-rises, *Good Times* and *The Jeffersons*, and they both have elevators.

So it's not definitive. I didn't claim I had absolutely disproved the elevator story. But I'm trying to show how the phenomenon of circumstantial evidence can be used to meet a challenge. In this case the challenge was elevators.

You're right, Colin, I should have put something in there about the fact checkers. And, Philip, if this wasn't a book I would add that to in the next edition. If only we lived in a world where we weren't confined by paper books I would be able to respond to that.

Do I have a naive understanding of ethnography? Yeah, I guess I do. What can I say? I only spent two years on it, that's not even half a Ph.D., so I'm sort of stuck. Mary, I'm glad that you tear books apart in the graduate seminars. I'm looking forward to reports of what you have to say about me in the graduate student seminar.

Shamus, the reason that I didn't deal with law the way you suggested—two reasons. One is I've written two books about that. And the second, I had to keep this short enough for Mary to assign it in her seminar. The reason it's paperback and the reason it's short is those two magic words, course adoptions. I agree, discussing law, discussing power would have been interesting and worthwhile and maybe that's the next book.

As to the formal skepticism, though, I have to say I have not seen a lot of published skepticism about the factual bases of ethnographies. There is obviously a lot of debate in ethnography, but it's mostly about theory, sometimes about numbers. I have seen very little formal skepticism about facts. If you do it in class, I think that's great. And that's what makes the Duneier-Klinenberg dispute so noteworthy. Because that's the one, right? Some of you know about this, I won't repeat it, but Eric Klinenberg wrote a book, and Mitchell Duneier went back and reinvestigated some of it and then Klinenberg replied. But it stands out as a paradigm because it's so unusual. And there should be more of that. I would be delighted to see more of that.

Shamus, your subpoena. What year is this? Because by my count there is about one subpoena a decade for ethnographers. That's what I'm aware of. There seems to be about one a decade. And I don't know what to tell you about that. I mean, I think what I would say is a protective order, what you need is a protective order. And you should talk to your university counsel about getting a protective order.

I think there is a lot more to say. People made a lot more really excellent comments. Colin and Shamus read the book in draft. Philip read part of it. And I benefitted greatly from your input. If I didn't assimilate everything you suggested, that was my mistake. And, of course, we all make mistakes. But I really appreciate your comments. I suppose we should take some questions now.

MR. GARY ALAN FINE: Questions? Well, I have one because both Mary and Shamus raised issues that I think are very important and that really in my ethnographic career I haven't dealt with very much, and that is the collaborative ethnography where you are not the—you know, it used to be said those of us who were doing this in the 1970s that we were cowboys—a male gender. We were cowboys or gunslingers going into a western town and finding out what's going on. But increasingly organizational ethnography, that is, doing ethnography as part of an organized group, is becoming more common. And, I mean, that raises a lot of questions, but particularly questions in terms of these issues of what constitutes evidence when you're not collecting the material yourself.

MR. SHAMUS KHAN: Do you want me to comment on that? I mean, I can just—I can go through our procedures. So as a nearly forty-year-old faculty member, I can't hang out at a fraternity party. And I did hang out at bars actually for months.

MR. PHILIP COHEN: Until that *Daily News* article.

MR. SHAMUS KHAN: Until the *Daily News* wrote an article about me entitled the "Creepy Professor." So that killed that part of the fieldwork. So I had to rely on other people. And what it meant was constructing instruments. So forms within which people took field notes that had things that they, you know, different sections and instructions on how to fill them out. And then me and the other lead ethnographic investigator, an anthropologist, reading the field notes every week, asking for revisions to the field notes relative to questions that we had, and then directing future observations. So this would mean like, you know, a description that they provided is much too interpretive. So often you make a distinction between—in a field note a description of an event and then later your interpretation of the phenomenon that was going on. So often early on it meant disciplining them to make those things distinct, or as distinct as possible, for future work anticipating we would have to do it. And then, you know, it meant meeting for three hours each week with the ethnographic team to go over their field notes and to discuss them. I would say, you know, I'm very confident that there is not much that is fabricated in this because there were at different times up to six of us producing field notes about a community, and I'm you kind of identify outlying things that seem really strange. I mean, the other advantage is that you can get two different people from two different perspectives to investigate something quite similar.

So I rather enjoyed it. The problem is that I had—over the whole team we had a $2 million budget, so that's not going to happen under most conditions. People aren't going to be able to have like a massive organizational capacity to run something like that. But, you know, there were a lot of benefits to people being forced to produce field notes that were read by an entire team on a weekly basis in terms of the verifiability of information.

MS. MARY PATTILLO: I'll just quickly say I began my career on an ethnographic team. I began my whole foray into sociology. When I was a Ph.D. student, I was on a team that William Julius Wilson was on at the University of Chicago and there were multiple ethnographers in neighborhoods across the city. And one of my first articles was an article called, "Do You See What I See," which was a conversation between myself and the other field worker in the same neighborhood, where we would be in the same places at the same times on the same nights and our field notes would be very different. That brings me to the point that while I am now on another big team that only happened by accident, I don't like big teams, actually. I prefer the lone ethnographer model. I prefer it because I think the ethnographer is a tool. I do think the cosmic secrets are part of an interplay between what exists in the real world and the ethnographer's interpretation and vision and knowledge, and so on. And so I am unlikely to write from any field notes that anybody else writes because I couldn't imagine myself doing so.

MR. ROBERT NELSON: So I think you sort of ducked the issue of looking at the trial as sort of a paradigm in law as a vehicle for finding truth. Because you're making a critique of the ethnographer from the standpoint of, you know, rules of evidence or theories of evidence in law. Philip's discussion primarily—both Shamus and Mary brought up very nice points to sort of challenge you on that, and maybe you've written two books about it. But I think it's not enough to say you've written books on it. We need to hear what your defense is.

MR. STEVEN LUBET: Sure.

MR. ROBERT NELSON: So what Philip is offering is more of a critique from within science and social science, or at least questions about it in terms of social science. And that's a very different approach, I think, then what—you used sort of a very lawyerly approach to your thinking in this critique.

MR. STEVEN LUBET: That's completely fair, Bob. And I don't have a terrific answer. I don't think trials are the paradigm of finding truth. I say this in the book, and of course you and I know, we've known each other a long time. I work with the Center on Wrongful Convictions and I hardly think that trials are the paradigm for finding truth. And I say in the book there are too many false convictions, there are too many false confessions. I will say if we had a better method of finding truth, we would try to use it. So trials, at least in the justice system, are sort of a default. If people could actually arrive at something better, we would use it. We try to improve it all the time. But the book really doesn't posit the American trial as superior to ethnography. What I really say is we have some ways of assessing evidence that are useful. And I don't say—I hope I don't say, I don't mean to say—that trials should replace ethnography. What I think I can defend is saying that law takes a pretty rigorous approach to the way evidence can be used trying to get to truth, and that could be helpful by analogy. And I say that specifically: It could be helpful by analogy to ethnographers. So I'm not making a superior claim about truth finding. I wouldn't. I'd be out of business.

MR. GARY ALAN FINE: Did I see you have your hand?

MR. PETER MOSKOS: Related to this idea of rigorous search for the truth, I want to go back to the elevator vignette. Maybe we can get that out of the way today and not talk about it tomorrow. You did spend two-and-a-half pages on it. You've used the legal approach to break it down, presenting evidence, casting reasonable doubt, even if it were a trial situation. And I think you missed the fundamental point. And I don't actually know how it's phrased in the original book, but what you say is they have never seen an elevator. Which is different than being unaware of the conceptual concept of a box where doors close.

This matters because I can vouch for a couple of things. I've seen people the first time they've been on a flight. They know the concept of a plane, and they still get giddy when they see this plane and imagine it's going to take off. I've seen people get on escalators for the first time in their life. Boy, are they unsteady. I'm sure they've seen them in movies, too. And I'll go one step further, which is I think, I don't know, but I bet you a lot of the people that I know in my ethnography in Baltimore have never been in an elevator after they were born and left the hospital. And I know that because you mention—

MR. STEVEN LUBET: What page do you have there?

MR. PETER MOSKOS: I don't know if my pagination is the same. I mention that because at another point you specifically say that I had no reasonable way to interview people, criminals in Baltimore. I had a great excuse to interview criminals in Baltimore; I was a cop. I could ask them whatever I wanted to. Now, I didn't include it in my book because there are power dynamics and why would they tell me the truth, and the book wasn't about them. But, oh boy, did I interview criminals. I was really curious. Especially juveniles because those are the ones that I had a lot of time with.

So I asked them questions. Have you ever been in church? Nope. Your family situation? Always messed up. Have you ever been out of East Baltimore? I made a list of every kid I ever had custody of. And most of the times the answer was either, "No" or "Yeah, I went once to visit auntie on the west side." This is a place of public transportation where you could walk seven, eight, ten blocks and be in a different neighborhood. Their entire life was this maybe four-block radius, school and back, and a couple of friends. And I mention that because in those four blocks there were no elevators. Now, again, they'd been outside and never done it. Yes, they watch TV, but there are lots of things in TV you're not supposed to believe.

So I find it entirely believable. I also want to say that if you mention this to other people in Baltimore, they would be shocked at the third-world conditions and would defend the city and say, "Oh, no, no, we're not like that. That's a negative stereotype."

MR. STEVEN LUBET: I want to read the actual paragraph from Edin and Shafer because it wasn't just whether they had ever been on an elevator. That's not what they said. If the kids had never been on an elevator, that would be—it wouldn't have been two-and-a-half pages, it wouldn't even have been a footnote.

MR. PETER MOSKOS: It says saw an elevator for the first time.

MR. STEVEN LUBET: Let me read it, Peter:

> Also on this trip many of the children saw an elevator for the first time. Initially some of them didn't believe that the box behind the door could actually transport them from one floor to another. They honestly thought it was some sort of a joke that the teachers were playing on them.

So it wasn't about being giddy and it wasn't about being nervous, and it wasn't about watching their step. It was about not believing there was such a thing as an elevator. And I don't think that is true of multiple sixth graders on a school trip in Mississippi, in which they flew on a plane to Washington, D.C. Now, the events are all masked, Colin. You know, the whole thing was anonymized. We don't know the county, we don't know the school, we don't know the kids, we don't know the teacher. If it hadn't been masked, then there would be an answer and it wouldn't be hearsay. We could ask the participants, and then we would know what happened. But look, if you want to believe the kids in Mississippi—

MR. PETER MOSKOS: Or Baltimore.

MR. STEVEN LUBET: —think that elevators are a joke. Well, go ahead.

MR. PETER MOSKOS: Here's my point about the elevator and the kids. It's about the different perception of how you come across knowledge and truth. And I don't want to get into—I do believe truth matters. I don't want to say, in a way, it's irrelevant. But my point is that in ethnography, the point isn't about the elevator per se, it's about the narrative, it's about their neighborhood, it's about the isolation. And in that sense, it does reflect a greater truth where in a way that the legalistic style, I think, does it a disservice.

MR. STEVEN LUBET: Well, that's the difficulty. Because the theory in $2 a Day is is the isolation of the kids in the Mississippi Delta. And the story that they can't imagine going from one floor to another supports the theory of their isolation. So the accuracy of the story really matters.

MR. PHILIP COHEN: Can I just say one thing on that? If it's about the greater truth and greater interpretation or it's about the group, then why are you using a specific fact? Like, in other words, if the thing about Chuck and Tim is not actually true, but it's reflective of some larger thing, just say the larger thing.

MS. MARY PATTILLO: Well, that's because hopefully this point about isolation came from repeated stories that built the story about isolation. It's not that isolation came first and you go looking for stories that build it. So that the facts matter because you're showing the facts that got you to the theory.

MR. STEVEN LUBET: Well, I talked to the people who live there and they said, “We're not that isolated.” I got as close as I could.

MR. GARY ALAN FINE: Let's have two more questions, I think. Anna.

MS. ANNA MUELLER: The issue with that is that—what I'm hearing about what you're saying is because it's not believable to you, it's not believable. But part of—you know, especially if you think about going into parts of Baltimore that may be very well very foreign to some of us. I'm uncomfortable with that sort of line. I don't know that's an acceptable line. Right?

MR. STEVEN LUBET: I think that's completely fair. It wasn't believable to me, so I investigated it.

MS. ANNA MUELLER: Wait. So you investigated it also by talking to who in Mississippi? Was it the same people?

MR. STEVEN LUBET: No. I couldn't talk to those people, because the story was anonymized, which is one of the problems with ethnography. So I found Mississippians. I spoke to educators, I spoke to social workers.

MS. ANNA MUELLER: I know. But I think this gets back, actually, to the point that Shamus Khan raised about power and knowledge. Right? You know, I lived in Memphis for four years, which is on the edge of Mississippi, and I am completely aware of the fact that teachers and educators are not always aware of what their kids' lives look like. Some good ones are. So some of this substitution is just equally problematic. Although I'd also like to say that I am also interested in the truth and I appreciate the points about solid evidence, so I don't dispute that.

MR. STEVEN LUBET: I think we're done with this.

MS. ANNA MUELLER: I rest my case.

MR. STEVEN LUBET: If you look at it as an exemplar of how one investigates in the face of anonymous sources and at least a questionable story, seeing this as an example of method rather than speaking truth, I think it might make you happy.

MS. ANNA MUELLER: Well, I look forward to the book.

MR. GARY ALAN FINE: Final question? Seeing none, I want to thank all of the panelists and Lubet for a very engaging session. Thank you.