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**NORTHWESTERN LAW
INTERROGATING ETHNOGRAPHY
CONFERENCE**

ETHNOGRAPHY, ETHICS & LAW

Northwestern Pritzker School of Law
375 East Chicago Avenue
Chicago, Illinois
October 21, 2017, 1:00 p.m.

PRESENT:

PROFESSOR JULIET SORENSEN, Presider
PROFESSOR GARY ALAN FINE
PROFESSOR COLIN JEROLMACK
PROFESSOR PETER MOSKOS
PROFESSOR ROBERT L. NELSON

MS. JULIET SORENSEN: Good afternoon, Everyone. I'm so pleased and really honored to be moderating the afternoon panel related to the book launch, yet another truly important book by my colleague, Steve Lubet.

The title of this afternoon's panel is "Ethnography, Ethics and the Law," and we'll be focusing on comparative issues of confidentiality, privilege, counseling or participation in criminal conduct, as well as reporting misconduct and responsiveness to subpoenas as they arise both in law practice, legal research and scholarship, and social science research.

Before I go further I should probably introduce myself. My name is Juliet Sorenson. I'm a professor here at Northwestern Law School. I'm the director of the Bluhm Legal Clinic and the associate dean for clinical education. And as I said before, I am happy to be here for my colleague, Steve Lubet.

We have two presenters and two respondents. Our presenters are Bob Nelson and Peter Moskos, to my left. And we're going to be starting by hearing from Professor Nelson. Bob Nelson is a professor of sociology at Northwestern University and he's the MacCrate Research Chair and director emeritus at the American Bar Foundation, conveniently for us located in this building. He's a scholar of law and inequality, with a particular focus on the legal profession, as well as organizations and discrimination law. He is the author or editor of no less than nine books. And his latest book, *Rights on Trial*, which he has co-authored, builds on his earlier work on gender inequality in organizations, which includes *Legalizing Gender Inequality: Courts, Markets, and Unequal Pay for Women in America*, which won the best book prize from the American Sociological Association. So, Bob, over to you.

MR. ROBERT NELSON: Thank you very much, Juliet. And thanks, Steve and Gary, for organizing this. I think Steve made reference yesterday to the fact that the summer after my first year of law school I worked in the clinic and Steve was one of the faculty members there. And we were both considerably younger then. I was in the JD-Ph.D. program at that time, but the way it was structured back then was you did a year of law school, a year of

grad school, a year of law school, a year of grad school, and then finished things up. So the following year I had my education in ethnography from Howard Becker. So it is interesting how these circles come back around. And what I want to talk about—

MR. STEVEN LUBET: I took a course from him as an undergrad.

MR. ROBERT NELSON: Oh, you did? Very good. So I'm a social scientist who is interested in advancing social theory through empirical search. And I am interested in how we can improve social research, and that is the perspective I bring to looking at Lubet's book and asking to what extent it contributes to that enterprise. And as a sociologist of law and someone who studies law and inequality in various ways, very often looking at powerful actors—lawyers—but also less powerful actors—such as women who experience inequality in pay in organizations—or looking at the targets of discrimination and the experiences they have in the litigation process, I am keenly sensitive to the power dynamics that we encounter in our work.

A lot of what we are doing research on is power and inequality. So I'm going to echo a lot of the remarks we've already heard from yesterday and today, but try and bring them together a little more with a kind of focus in the sociology world. So, I think we literally work in the field of power. That's not just like a slogan, you know. We actually are interacting with the law and the power in law in various ways.

So if we just think through the various powers of law and how it affects our work, and in respect to my own work, law can be very helpful in generating data. Documents that are produced in discovery in litigation become a very rich set of materials on organizations. Very often the materials, like testimony, are given under oath and with cross-examination. And so even though the data are not collected for the purposes of social research, they have these interesting and important characteristics. But law can also be a barrier. So confidentiality agreements in employment civil rights, for example, as well as many other areas, then become a barrier for understanding what happens in those cases. And in *Legalizing Gender Equality*, the book that I did with Bill Bridges some years ago, we identified a set of case studies. And several times we went out to talk to lawyers. They were interested in sharing their documents with us on a case. We began to go through the files, and we would come upon a confidentiality agreement, which meant that we are not going to be able to use that material.

I'll talk a little more about one of the four cases that we used in *Legalizing Gender Equality*. It's a case we referred to by a pseudonym because we went through the files and discovered some confidentiality agreements, but they were not complete. So we decided the better part of wisdom was to not identify the organization: to anonymize it. We used a lot of the data or the documents from that case, and did some interviews with the actors around that case, but we decided not to possibly disturb a powerful corporate actor who might act to try to silence that research, that part of the research. Law is also potentially a threat.

And so, Shamus, your subpoena is Exhibit No. 1 of that. And I think, you know, even though Steve in the book says they're very rare—only happen like once a year or whatever—I think it is a shadow that hangs over a lot of research, especially in certain areas of research. So what we usually are doing in this area is working under conditions of regulation by an IRB or controlled by an IRB. And I think as we've sort of surfaced earlier

today, there is a tension between the protection of subjects and various practices that some folks had suggested here, like unmasking, you know, not masking confidential data.

But I think this is also in part an internalized value or norm for researchers that we're not only interested in protecting our subjects, but also getting more candor from our subjects because their identities are kept confidential. And in the kind of work that I have done, which involves also working inside organizations, that kind of anonymity has been very important to getting candor. One of the questions I was tempted to ask this morning of Steve Mills was how can you tell when people are lying to you? That's something we have to keep in mind all the time. You have to do that when you're doing the fieldwork in an organization. Some people are just going to give you a self-interested account and you're not going to know for sure. You will hear other perspectives from other people in your organization and you have to kind of weigh that.

I think when you report the research, you know, you are sort of weaving a tapestry of the results you're going to report, but very often you do have to report those possibilities of bias or self-interested statements or whatever. Usually the informants who you think are just giving you self-serving statements tend not to show up in your reports because you discount them, not according to a simple formula but in terms of your sense of the situation. One thing I just want to mention is certificates of confidentiality. Which is—Shamus, I assume—did you get that for the current study?

MR. SHAMUS KHAN: I have them for the current study. But they've never been tested in court, have they?

MR. ROBERT NELSON: I think they have. I think they're pretty—

MR. SHAMUS KHAN: Our lawyers are, like, you can have them, but there is not a lot of legal precedent for them, but they haven't really been rigorously and fully tested. So we cautioned all of our participants to this fact because we didn't want to promise them that we could give them this certificate and that it had definitive legal standing.

MR. ROBERT NELSON: Well—

MR. SHAMUS KHAN: I could be wrong. There are lawyers here that—

MR. ROBERT NELSON: I think you're probably wrong. I hope you're wrong. I mean, it's not like—this is somewhat being expanded in terms of access to these certificates. Anybody who gets an NIH grant now is automatically granted a certificate of confidentiality. And it's not limited to projects that are funded by NIH, it is others—not just federally funded research but other research. And the scope of that—there is something about how the research has to have something to do with HHS's mission, but it's not just clinical research, it includes behavioral research. So it is a resource that we should probably take a look at it.

But the other way in which we are dealing with power as we do this kind of research and try and study sociology of law is we are dealing with powerful actors, whether it's the police, the courts, legal professionals, or the corporations that have a lot of legal professionals who work for them. And a big challenge for the researcher to understand

these kinds of organizations is how to get access to study them. And, you know, maybe Lubet's book and maybe Colin's and Shamus's new collection is going to urge me to reconsider this, but I always thought it was smart to provide confidentiality to informants to get access and to get more candid interview responses from them.

But then I also think there is the concern about imbalances of power amongst the groups we are dealing with and trying to study. So there are populations who are at risk, and the question is how do you get access to them, how do you protect them. And so in the research on employment discrimination, for example—or even just the other research on organizational pay systems—in talking to less powerful actors who might have suffered negative consequences if their identities were revealed in our research, it was valuable, in my view, to provide them with anonymity. So I think, you know, the topic of this panel sort of brings to focus what are the ethical and the legal obligations for researchers in the face of these kinds of issues. And what I want to argue for is very much what Claudio was suggesting. I think implicit in a lot of Mary's remarks, et cetera, is an argument for basically the use of professional judgment by researchers in a regulated environment. Because we are in a regulated environment.

Now, I think Steve raised a lot of interesting examples. You know, Chapter 8 where he's looking at legality, criminality, you know, about how to deal with different kinds of situations where you might be involved in criminal activity or might learn in advance about criminal activity. And, you know, Lubet is kind of interesting, he doesn't really say very much about what people should actually do. Right? I mean, you sort of say it's kind of an inherent obligation to not violate the law, but it's very—that's sort of about all you say. I guess it's—

MR. STEVEN LUBET: You never were a very close reader, Bob.

MR. ROBERT NELSON: But he does throw out, you know, those contrasts between, for example, the cases of, you know, Contreras and Goffman and Venkatesh who were really close to criminal behavior, and by some accounts were either encouraging it or participating in it. And then—but also contrasting it with cases like Laud Humphreys and The Tea Room Trade, which involves homosexual behavior in public bathrooms, or Howard Becker, *Becoming a Marijuana User*, and do we want to treat them the same way, you know, for those researchers.

You know, the first three obviously involve victims, crimes that involve victims. The second—the last two are, you know, victimless crimes. I think it's a serious question. I think I would sort of fall back on the general position to rely on the professional judgment of researchers to make these judgments. But I'm not exactly sure I'm comfortable with that.

But then there is the issue is of masking and confidentiality. So I've already made the pitch, basically, that it aids the quality of research. And I think that is a very important argument to make. So let me just do some research biography here. So my first book, my first study, was a case study of four law firms in Chicago way back in the late 1970s, right? The book wasn't published until like '88, but those four law firms were identified by pseudonyms. None of the informants were identified individually. It entailed a combination of survey work with a fairly large sample in each of the four organizations, and then interviews with elites in those firms. There is no way I could have gotten access to those firms without that kind of bargain. The data collection was so intense, in a way,

that, you know, there may have been some parts of the analysis that you could question à la Labet. But part of the bargain with these firms was that they had a chance to read my interpretation or analysis of their firm, not for the purpose of editing, but for the purpose of seeing whether or not their identity had been disclosed.

They all thought that they could identify—that their firm would be identified. Essentially they didn't care. They let me go ahead and publish the work. Now, that's a very tricky kind of negotiation, but I would not encourage other people to try and copy it because it is sort of dependent on the decisions of powerful people. But without that kind of confidentiality I would not have gotten that kind of access, and we would know less about corporate law firms. And, you know, the book didn't pull punches.

You know, there is this—another sort of argument that we've been having at this conference is about whether or not you're sort of taking pre-positions when you're doing research and therefore you're going to select certain findings to confirm a thesis. Very often in this kind of work we are discovering what's going on and then we mount an argument. And if you don't mount an argument, in some respects it's not going to have any impact or make any kind of meaningful intervention. So it doesn't mean that you're necessarily rejecting all the contrary evidence, but you need to come down in some respect to have some kind of parsimonious argument about what all this material is. So in the *Legalizing Gender Inequality* book, three of the case studies involved organizations that were revealed in litigation and so there is no mystery about who the organizations were; there were a lot of public documents that were available for people to look at.

But then when we did do sort of qualitative interviews around those cases to talk to the different actors involved in it, we did provide confidentiality. And, you know, there were some things that I'm quite sure people would not have said to us, which I think are very credible, but they wouldn't have said them to us if we had not provided anonymity. So I remember in the *State of Washington* case, the *AFSCME v. State of Washington* case, there was a union official who said that although they went along with filing the lawsuit, it was primarily to defend against women organizing in unions and that they were really more invested in collective bargaining because they thought backdoor negotiations were going to be better for basically male dominated occupations. And his phrase was, "We're not going to rob Peter to pay Pauline." Now, if that were public or that was attributed to that individual, that would have been tough, I think.

Then in *Rights On Trial*—I've got a new book, *Rights On Trial*, which is also, all my work is, mixed methods. This is mixed methods too. It involves a large quantitative data set of employment discrimination lawsuits which we analyze quantitatively. But then we also drew a random sub-sample of individuals, parties, both plaintiffs and defendants and their lawyers. And so we have conflicting perspectives on what happened in these cases. For this book we actually got permission in the consent form to tape record and use the tape recordings. So you can go online to the website and listen to the voices of these respondents. But they're not identified by name. And the voices I think are very helpful. It brings what we call verisimilitude to what we're doing. It's very informative, you know, in terms of the race and class and professional positions of the people we're talking to. But the other thing that is interesting, I think, for—you know, in this book, it sort of demonstrates in an empirical way what we talked about a few times already. There are contested views of these cases. You know, these parties do not share the same view.

The plaintiffs don't have the same view as the lawyers. They certainly have different perspectives than the defendants and their lawyers. So there is no one reality in these cases. And I think that's very common, I think, in social research. But maybe looking at a controversy like this it's more pronounced. So, I think that's just, you know, kind of an important part of what we were trying to capture as we reported these different perspectives.

Now, one thing that one of our cases or one of our chapters reports is a story from a plaintiff in which he said that the judge sitting on the bench announced in the courtroom what happened in the OJ Simpson trial. The OJ Simpson verdict had come in, and he announced that from the bench. It had nothing to do with the case he was hearing. And these plaintiffs had the view that—and then right at the end of the trial the judge wrote a ruling and ruled against them. And the plaintiffs were convinced if it had not been for the OJ Simpson verdict they would have won the case.

Now, actually, what Steve has prompted me to do—we do actually cite the transcript for part of what the judge says in the ruling of the case. But I haven't gone back to look to see whether or not the trial transcript has OJ Simpson in it. It probably is not on the record, actually, but I'm prompted now by what you have suggested and I'm going to go back and take a look. So this suggests—you know, in a way I think it's important for us to understand the politics of research. What I've described to you is a lot of sort of political negotiation in the process of doing research. But I also think that we need to keep in mind the big politics of research.

And there is a danger, you know, that in Lubet's critique that it's going to be attacking or undermining this kind of research. And I know that is not what Steve has in mind, and I know all of us as scholars when we publish things know we don't have control over how it's used or interpreted. But it is a danger I think. So to assess Lubet, you know, and the overall importance of the book, I would say that, despite many disagreements with Steve, that this is a very helpful provocation for ethnographers. And, you know, even though it may not really be a corrective to what we do in this field, I think I'm still going to encourage my students to provide anonymity. But it's very important to have the conversation, so for that I appreciate it.

MS. JULIET SORENSEN: Thank you very much, Bob. Before I introduce Peter Moskos, I just wanted to pick up on one point that you made, and that is there is no one reality. I think that is actually true in any of the truth-seeking professions, be that law, sociology or otherwise. I recall a public corruption trial that I participated in when I was an assistant U.S. Attorney, and it was, at least we had thought, a very strong case in terms of the strength of the evidence as presented to a jury. In brief, there were three codefendants. There were extensive tape recordings admitted as exceptions to the hearsay rule in our case in chief. There was a cooperating witness who was, of course, vigorously cross-examined on the terms of—actually, there were two cooperating witnesses—of his and her respective cooperation deals with the government.

When the verdict came in, it was a truly mixed verdict. One defendant was convicted, found guilty by the jury. One defendant was the result of a hung jury; the jury could not agree as to a verdict to that particular defendant. And the third defendant, who was the wife of one of the other codefendants, was acquitted, she was found not guilty. So we proceeded to retry the person whom the jury had hung on. And, of course, the second

jury doesn't know anything about the previous trial, right? They're not allowed to hear references to the fact that the previous jury couldn't agree on the verdict the first time around as to this one remaining defendant.

Now, I will tell you that the verdict came in on the retrial, and this person was found guilty of having participated in a City Hall bribery scheme. We were given permission, along with defense counsel, to interview the jury after the verdict was returned. And the number one question was, why didn't you charge the wife? She's all over the tapes. She's complicit, she's clearly guilty. Why wasn't she on trial with her husband? And, of course, we couldn't answer the question, which is to say we had tried her and she had been found not guilty by a jury of her peers.

To your point that there is no one reality; truth is elusive, no matter what the methods are that are used to try to determine it. With that in mind, our next presenter has an official biography that states that he studies people the old-fashioned way, he talks to them. So I'm pleased to introduce Peter Moskos, an associate professor at John Jay College of Criminal Justice at CUNY. Which, by the way, is near where I grew up. And the author of *Cop in the Hood*, as well as two other books. A former police officer and a sociologist trained at Harvard and Princeton. We're happy to have you.

MR. PETER MOSKOS: Thank you. Thanks for having me here. It is good to be back in the great city of Chicago. But New York is better. So before I start with what I prepared, I want to mention *Rashomon*, the movie, because it was one of my father's favorite movies. And some of you knew my father, who was a sociology professor at Northwestern. And he always referred to the idea that one happening could have different viewpoints and perceptions, and I use that illusion many times. And then I finally watched the movie many years later, which I found kind of boring, by the way, but I was surprised because what he left out of the story and the concept of the *Rashomon*; there's not just different perceptions of one reality, it actually presents a different reality.

The stories are not possible all at the same time, which sort of brings to light what—I forget who mentioned the four books of the Gospels. There are different stories; it is not four interpretations. Yeah. It's always good to bring in religion in sociology. Academics love that. But what I really wanted to start with was a confessional of when I committed my first felony. When I was fifteen or sixteen working at the Evanston movie theatre on Central Street I got drunk after work and with a friend we, I'd say broke into Dyche Stadium. It wasn't even breaking in. We would go in there a lot to play because you could just crawl under the fences, ride your bikes up and down ramps, jump on big mats. But I think because of the alcohol and the youth, I decided it would be fun to tear down the goalpost, because, you know, that's what you do.

We went to the goalpost and I put a belt around it and tried to tear it down. Those things are tough to tear down, I found out. And I did not succeed, but I did succeed in damaging it. And it stayed damaged for weeks because it costs a lot of money to replace those damn things. I felt kind of guilty. But given the amount of dollar damage I probably committed to Northwestern's athletic department, it probably would have qualified as a felony. A few years later when, funny, that same friend turned twenty-one. He was a student at Northwestern, he was a good friend of my brother's. And when he turned twenty-one, we went to the DMV to get me a fake ID. And it's not a fake ID, it was a real ID, it was an Illinois license with his name and my picture. And there were huge signs saying if

you are doing exactly what I was doing, it is a felony. And we looked at the sign, and they called his name, and I stood up and I got my picture taken. And I was a young looking 18-year-old too.

I mention that not out of pride or trying to get it off my chest, but simply I found something slightly precious in Steve's description of crimes committed by the ethnographer, which I want to separate from witnessing other people committing the crime. But the idea that someone legally did by most standards take part in and was an accomplice to an attempted murder. I know your argument runs deeper than it was just summarized, as an ethnographer we can all agree we should all follow the law, but I actually disagree with that. I don't think we should all follow the law. I won't go through many examples, but, I mean, I ride a bike through red, I don't commit many—I try not to commit felonies. But the argument in the book, what is it, three or how many felonies a day, that we all violate the law all the time.

I don't think anyone here hasn't violated a law, whether it is drug use, traffic laws, or more serious crimes where people actually get hurt and injured. I have done a few other things that I'm not telling you about, by the way. Those are just the ones I'm admitting. So all that is in the—so I do sort of believe John Van Maanen, which is where I stole the line about I talk to people the old-fashioned way. That's stolen straight from him. He was a great influence and advisor when he was at MIT and when I was at Harvard. He's still at MIT. He did a study on police in the late 60s in Seattle where he went to the police academy, so I started my research basically following in his footsteps. And he said that often our crime was one of confession.

Now, I would say maybe that's not our only crime from an ethical academic standpoint. But was she an accomplice to an attempted murder? Well, I did want to point out, nobody did get hurt. And the accomplice laws, along with conspiracy laws, I think are, you know, too broadly defined from an ethical standpoint. But that's sort of neither here nor there from saying that is the law, and I think she would have been guilty as charged. It would have been shocking if someone would be charged for a felony in which nothing did happen. You know, if there was intent, as she stated, you know, I wasn't there, but I kind of think there was intent. I think she was guilty of that. I don't think it was a morning ritual. I think she went in to kill the son of bitch. But they didn't. So she could have left that out, I don't—but she chose to include it. You can hold that against her personally, just like you can hold my crimes and sins against me personally. But I don't think it distracts at all from her ethnography. I want to make that distinction, because I think there are other more valid criticisms.

Specifically, I mean, to put it bluntly, one's a fraud. And I find some of your points more convincing than others. I do think that the standard that a critic has, though, in a way should be even higher than the standard of the original researcher. I thought, as came up yesterday about people's first perceptions. I won't mention the E word. People's first perceptions to an object. I found it believable; you didn't. I would be willing to give the researcher the benefit of the doubt, because she was there and we weren't.

MR. STEVEN LUBERT: Well, she wasn't there. If she had been there, it would be different.

MR. PETER MOSKOS: Oh, you're right.

MR. STEVEN LUBET: If she'd been there, I would not have argued.

MR. PETER MOSKOS: I would also mention the power dynamic that, absent a teacher, if the kids had seen—if the kids knew what an elevator was—there, I said it, what an elevator was. I think it's almost irrelevant. I don't think a teacher would know. The same way you could ask somebody in Baltimore, do you know that your neighbor right now has four kids living on a roach-encrusted mattress with no food and no functional adult in the house, and they may not know that either.

You can be very close to something and not know it. This gets a little more to my sort of personal fieldwork. I did write—when I received your manuscript, of course I immediately went to the index and I wasn't in there. It was a corrected proof. Where am I? Flip, flip, flip. And I thought, hey, he hardly mentions me. And then I read this thing and I was, like, thank God. So I will take that as a sign that my research passed your judgment that I know I'm only alluded to tangentially. But I did worry going so—because there is no reason you should know.

My story in brief is I went into grad school in 1995 in sociology. The crime drop was happening. All the experts in the field were saying the crime drop wasn't happening and couldn't happen because we weren't addressing the root causes. Meanwhile it continued to happen, and I said, my God, if everybody is wrong, this is a good field to get into. So I found John Van Maanen, who is, I think—is he writing *Qualitative Methods, Tales of the Field*? Is that still—

MS. ANYA DEGENSHEIN: There is a new version.

MR. CLAUDIO BENZECRY: There is a new version.

MR. PETER MOSKOS: He works a mile from my house. He was wonderfully supportive, as I found everything in ethnography to be, unlike a lot of academia. I received nothing but support. I didn't actually know I wrote an ethnography, people don't believe this, until way after it was out until it was either Mitch or Eli Anderson told me I had. I was sort of unfamiliar with the term because my department was very quantitative. I wrote—I don't know. I wrote a dissertation. I knew it was qualitative, but I didn't know the concept of an ethnography. It might have helped me; I don't know. So I went to be a participant observer in Baltimore in the police academy, and they approved it, surprisingly, through a personal Greek connection.

It's important to mention. I had two departments approve my research and both had personal connections through Greek Americans. Other departments wouldn't return my phone call. Anyway, I go into Baltimore and there was a new commissioner who did not share those connections and he said, here, on the first day of the academy, no, you get out of here, kid. I don't think he actually knew I was there. He vetoed my research. He came down to me and I met him and the acting police commissioner in his office, and he said, "Well, why don't you want to become a cop for real?" I said, "Well, who would hire me knowing I'm going to quit after a year," though I stayed longer, "and write a book on it?" And he said, "Well, if you get hired, I would." And he was true—I didn't consider him my ally, but he was true to his word and he honored his commitment.

So I was in Baltimore for two years. I know the book says one year. That's a test of careful reading. I didn't want to put two years, because it was just under two years. But I didn't want to be criticized by someone saying you weren't there for two years. I was there for about twenty, twenty-two months total and then went back and finished my dissertation. So I was worried going into Baltimore about what would I see. I do not come from a police background, I didn't have friends who were cops. I didn't know Baltimore at all. And then I'm going into basically a dysfunctional city and into a dysfunctional police department. And then after the academy they assign me to a dysfunctional district, the Eastern District, which is where *The Wire* was filmed. And then I go to midnight shift and it's sort of—I mean, you know, this is the proverbial “Heart of Darkness.” I didn't know what to expect. So what will I do if—when—I see bad things?

This does get into the issue of the IRB, which I want to describe a bit more because I think most of us feel we understand it. And some of us, especially the journalists, may have no idea what we're talking about. So it came out of abusive research, specifically the Tuskegee syphilis experiment is a big one, but there are other examples of bad research. And somehow it was applied to social science research, when I think it was meant more for medical research. So perhaps that oversight is good. But it has become a tough barrier to ethnographic work. And I find that interesting and disappointing because I don't understand why professors with tenure who talk about academic freedom have such unwillingness to push back on systems. I don't understand why IRB boards in general—and it sounds like, Gary, that you did—are so willing to not push back or test the limits of the regulation or regulatory requirements, and instead seem very quick to say, “Oh, let's just apply it to everything.” That's the easiest way out.

I find it's a lack of spine in the academic community to say no; we will follow the regulations and the law, but we're not going to—we're not going to be excessive about this. And our interests are not to please the bureaucrat, or even our university's legal department, which of course the university might disagree, but the professors don't have to agree with that. Our goal is to do what and only what is required. One way you could say is ethical research. I don't think anyone denies that. Of course, the devil is in the details in what that means. Generally, it is believed, whether it's proper or not and it's true, that IRB boards demand signed informed consents—signed consent forms and unequivocal conditions of anonymity.

I tested those limits, and I have always done my research explicitly stating I'm not going to get signed consent forms and I'm not going to guarantee anonymity. And I don't just say “nah nah.” I mean, I defend it in a longer form. But the basic argument is I cannot get signed consent forms. I mean, you walk in—if this is a police station and I walk in there it's physically impossible for me to get signed consent forms from everyone. You consent by being there. To benefit me—on the other hand, you really don't have the right of refusal, though, honestly. I've given out surveys and gotten 100% response rates. And cops carry their own pens; it's a researcher's dream. I tell them—I mean, because it is an IRB requirement—I say you don't have to do this. But because someone approved me to be there and they're doing it, they all basically do. Occasionally you get somebody who kind of fills it out not for real. But it's—yeah, I get fabulous response rates.

But the bulk of my research is qualitative, informal interviews, and it would be impossible to get signed consent forms. People know who I am. But the other problem I have before oral history was exempted from IRB—which our first memos said, oh, we'll

still keep the requirement. What? Which I thought crazy. But the government said oral history is not covered. I don't know what their stand is now, but the university said, oh, we still (inaudible). It's more protection. No. No, it's not. I did interviews in an oral history style, and like was just mentioned, it felt very legalistic to give this consent form. I realized, look, I'm talking—and, of course, this is hard, I'm not talking to a vulnerable community, I'm talking to people with at least a high school diploma, police officers who are retired, but positions of power. If anything, they're the ones doing the abusing and not the abused and vulnerable.

So I'm giving you a form, let's say, and I'm not giving this form for your sake. You know exactly what you're doing. This form is actually to protect me. And I find that an absurd use of the IRB where the only job, the purpose of the IRB broadly defined is ethical research, but specifically to protect research subjects. And this form that you just signed does not protect you at all. In fact, quite the opposite. You're better if you never signed it. If I did something unethical, you can come back and say I'm going to sue you because it gives you more power. You feel, and to some extent you have signed away your rights through informed consent. And that I find is an absurd concept. So that's something else I need to get out of this. The way I get out of promising anonymous research is I say that there are things—if I were to watch a cop commit a horrible crime, damn right I would rat him out. It would have to be pretty bad, honestly. But I can certainly imagine a situation where my obligations are not to my research subjects, they're to the person who was just tortured.

Am I supposed to guarantee? That struck me as absurd. At least with my IRB board, and maybe a little better at a college of criminal justice, I haven't had a great problem with it. But it is time-consuming, it is a pain, and there is the assumption that it won't—that it's not going to happen. So you get this idea of masking identity from an ethical standpoint, and there is a long tradition. I think the urge should be—I did not mask my location at all because it's public record. I find some masking that sociologists and ethnographers do kind of funny. One of my projects whenever I get an article or a book to review is unmasking. You know, doing a little detective work on Google, and I can usually find the location in ten, fifteen minutes. Now, maybe that's a certain barrier that does protect subjects, that I had to want to do it. But I mention this to the author, and some of them are, like, oh, and they do a better job of defining it. But they always give these characteristics that give it away. And then I have a pet peeve that the pseudonyms are always these boring names. I don't want to read about small town. At least give them a name that sounds interesting. All the characters end up being called Bob and John and so on. Like, good God, as a reader my eyes glaze over.

I did, of course, protect—I did change the names of my research subjects because in the beginning they were paranoid of a researcher coming around. They did not even know what sociology was. Not just ethnography, they didn't know what sociology was. Often I was described as a psychologist. I would say, "No, I'm not a psychologist." "What do you do?" I was like, "Oh, I'm going to write a book about policing." And that was usually good enough. But I would say I'm not writing a book about you personally. But they did not have to write a refusal. Well, they did not have the right to not be included in my book. Two people, only two people said specifically I don't want anything to do with your book. One, interestingly, was fired soon after the academy, sort of an ethical thing. And the other wrote me later, years later, and said, "Oh, man, I loved your book." He's actually the one I

quote saying I don't want to be in your book. But I didn't quote him saying that. I was like, "It's you, Lieutenant." And he's like, "Well, I didn't know what you were up to and I didn't know if you could be trusted."

My angle is to give people plausible deniability. Because I do quote cops saying things that are, you know, if they were on Facebook or YouTube they could get fired for. I just wanted them to say no, I didn't say it, for their sake. That said, I gave the manuscript to everyone I worked with. Most everyone I worked with. I don't think they read it, by the way, which I thought was interesting because it's not very intellectual. I mean, I wrote about a work experience. Anyway. But I never received any complaints. Those who did read it loved it because—and it's a term not normally used on police officers, but simply because I gave them a voice. It's not a term that they would use. But they felt misunderstood, they felt picked on, and they just wanted the truth to be told. As long as I told the truth, they didn't care that they weren't portrayed as angels or heroes, or even always good workers. I have not received a single bad comment, literally a single bad comment from any police officer who has actually read the book. I was worried, actually, but I'm surprised at that. They just wanted the truth out. But the IRB issue is real and I think will prevent more un-anonymous work, and I don't know what we can do about that, except for pushback.

In some ways I think perhaps we should claim a slightly more journalistic standard because I've got First Amendment issues with the IRB. I go down and visit, less often every year, but I still go see the people who are in my book. I went there—down there about two months ago. I talk to them about everything. I am in their living room, literally, with Fox News on TV. And I don't know if I'll use that in a future published work. I might. Technically it's against the rules because I don't have IRB approval. But, damn it, these are my friends and this is their living room and it's simply impossible for me to get IRB approval for my life. I didn't go down there with the intent of doing research, but it certainly becomes that at some point. And there is a gray area for a lot of ethnographers between your life when you're immersed in the field and research, and the IRB concept is horribly suited for the problem for ethnographers.

The last thing I'll mention on that, getting approval without informed consent. I do say, look, if you want, I could lie and say I'm going to do this, and that has certainly happened with other research with signed consent forms in the past. But I'm not going to lie. It cannot be done. So either the research—either ethnography the way I do it can't be done, or you have to approve it the way I want to do. And they did it. So push back more against these IRBs. In terms of—so I wrote in the book that by and large police integrity is hard, and I stand by that, despite everything that has happened in Baltimore in the last few years, including one of the guys I went to the academy with is currently—was recently indicted and in prison. At some point you have to trust me on that. And many people don't, by the way. I was there, you weren't. I may not be able to convince you of this. People say, well, maybe you contaminated the research. Possibly in the sense we all influence our surroundings and I was an active participant, not a participant observer. But I was often working alone. I would say this, that if it only takes one person to make a whole squad, district, department clean, well, that is great news. I wish I had that influence on the people I worked with. I don't think I did. By and large cops are clean just because it's a job. You don't want to get fired because you'll lose your pension. That's the consideration. I would—and I still say this. Certainly not all cops are honest, but the integrity of the police

department I saw is certainly higher than the integrity of any academic department I've ever worked in. Absolutely.

How many of you know of scandals, of rumors of inappropriate relations between the faculty and the students? How many of you have said anything? Do we have an ivy wall of silence? Well, no. Well, maybe. But more likely I think it's you don't know enough. You have an interest in protecting yourself. If you know too much, you're in too deep. If there were cops that gave me—you know, made my “spidey sense” tingle, I was not an ethnographer at that point, I just wanted to stay away like any other cop because when the crap hits the fan I don't want to be there. That's the interest of self-preservation. Now, does that make my ethnography work hard? Well, yes, because it was a job as well, it was a job in my life. But there was an ethical human instinct to preserve my own hide and other hides that comes into play that in one way—in some ways was unique to my research and in some ways I don't think it was unique to my research. I never set out to write—sort of write a kiss and tell or do an exposé of policing. And I consider myself empathetic to police officers and policing. But you can certainly take things from my book and weave a very negative portrayal of policing as it was done Baltimore. I think by definition, taking it out of context, but it's there. Those are the editorial decisions I made. I will say I did not go in with any sense of advocacy or knowledge. I really went in completely ignorant about the world I was going to enter. For better or for worse. Just sort of—I had no—not only didn't have a stake in the game, I didn't even know what the game was in terms of the world I was seeing.

So I want to talk about some of the ethics of data and data criticism a little bit. I mentioned earlier, I think it is important not to conflate downright fraud with misinterpretation or errors, or even using hearsay as fact. I should reread my book. I haven't read it in years. I don't think I ever would report something I didn't see as fact. It never would occur to me. Gosh, that just seems to me an obvious lie that maybe hasn't been stated explicitly enough in ethnography. But that's a good line. Don't relate other people's stories as fact, relate them as stories. I think it could come off as fact. That said, I will say when I read *On the Run*—look, I read it and said this portrayal is (inaudible), I don't believe it. I said I think there are errors. I don't think the errors are always in the places you spotted them. The same with the hospital thing, to talk to people in the hospital and saying that would never happen. I can vouch that it is not common practice. I never heard of it. But I worked at Johns Hopkins Hospital. I would be shocked if it hadn't happened. That's all I am saying.

Now, whether she witnessed it is another issue. But the idea that it doesn't happen I find patently unbelievable. Cops, of course, they will do anything they can. And cops and nurses are tight. I mean, this is their sort of home in a way. The relationship between hospital personnel and police officers is very much of camaraderie, and I can easily imagine a situation where a cop says, “Let me see that clipboard,” and someone gives it to them in violation of HIPPA rules and laws and everything. I never saw it, but I believe it has happened. Which is different than saying downright I saw something that didn't happen. I know Alice personally. I'm not close to her, but I consider her a friend. I did not review the book, despite being asked for it many times. I didn't want to review the book because I had serious problems with it. But I think that ignores in a way that—and I hate terms like greater truth because it is a way of hiding things that are not true. But there is a greater truth. I read the book with a certain polarized filter that said I don't need to sort of—some of this I think

is crazy, is wrong. I don't think—to me that wasn't what her book was about. I think it suffered a bit from advocacy that—I mean, if I have time to get into it a little bit—as opposed to simply having a point and relating data. I think it was weak when it got into advocacy and theory. The strength in her book is she hung out with, befriended and wrote about a hidden population that probably no one in this room has ever dealt with in any in-depth level. I mean, I would say as a police officer in Baltimore I dealt with it at a different kind of level, but I saw it enough to understand that thank God someone wrote this book and is describing this.

And in criticizing the book, which you didn't, Steve, but as other people have, that, you know, white girls shouldn't be doing that, well, you know, then you go and do it. Like how dare you say that she can't transcend class and race boundaries to befriend—these were her friends. To say she shouldn't write about it. I found a lot of the criticism not only to be unjustified, beyond your book, but also missing the point and the fabulous world she revealed that needs to be out there. So is that a greater truth? I don't know. But there are errors in qualitative analysis, and you point those out very well. And perhaps, even if it's not formal fact checking, yes, we should be more careful about that. I don't know if the errors in qualitative analysis are worse than the errors in quantitative analysis. And I find that as the idea—I don't like describing myself as a scientist as my own personal choice of identity. I think of science very much as hard science, and I'll leave it to Claudio to say whether that is even science. But I don't know what I would do.

I'm an academic, I'm a professor, I'm a criminologist. I'm not even in the sociology department anymore, but I don't mind being called a sociologist. I'm a writer. I think Mary made many very good points, one of which is the quality of your writing matters and it is one reason why some things get out there and others don't. I think ethnographers tend to write better than sociologists, and I think journalists write better than ethnographers. Just because something is replicable—yesterday somebody said, you know, just because you've replicated doesn't mean—I think it was Colin—doesn't mean it is replicated.

MR. COLIN JEROLMACK: Yes.

MR. PETER MOSKOS: But there is also the issue of errors come in different factors. You know, my favorite criticism almost of all quantitative analysis is what about your nonrandom missing data. They never answer it. But nonrandom missing data is a serious problem with statistical analysis. And if we're talking say 20%, 30% missing data, depends on the sample, that's usually a fatal flaw, but they just put a little qualifier saying, you know, it's a limitation and then go on. And in the language of the traditional scribe class, write above the comprehension of most of the world, and even most academics, and you're supposed to trust that ultimately their conclusion means what it does. To put it in my world of crime data analysis. Until the *Guardian* and the *Washington Post*, and one guy who does Fatal Attraction—not Fatal Attraction, Fatal Encounters. Two very different things, but they both unfortunately up in death. Started keeping track of police-involved shootings, our only source was Uniform Crime Reports, which it turns out now we know in hindsight were missing 50% of police involved killings.

Now, until two years ago a lot of people were using that Uniform Crime Report. And, of course, you can say yes, science corrected itself. But that is not enough because basically for decades we were using data that was invalid and presenting it as truth. And,

yes, it's nice we can use better data, but that doesn't screw up the flawed studies in the first place. There is a new study, which I haven't read yet, about body cams in Washington, D.C., that says they have no effect. I haven't read it yet. I really have nothing to say about if it's valid or not. But if it's—I find it hard to believe cameras don't have some effect. But this is now being reported out there, and I hope it doesn't become the truth. Well, science has proven that body cameras don't matter. We don't know that yet. So let us not hold qualitative data to a higher standard than quantitative data.

Understand that errors come in different ways, that no data is perfect, and strive to make it the best. Let me just mention a way that could perhaps help the future gathering, ethical gathering of data. I don't think an adversarial system is a productive one. As Mary also said yesterday, anyone who spends time in a court is shocked at how dysfunctional it is, and it's not a search for truth. It is a bad template from which to base the system.

Now, that said, there might be good aspects that come from it. But the idea that an adversarial system is somehow a model for anything I find, from my own knowledge of courts and the law, kind of disappointing. Nor do I think a regulatory approach works because then you get sort these of abusive expansion of the IRB. Perhaps ethnography needs a sort of honor code to—and I mention this because I went to a college that had one. I saw a lot more teaching in high school—a lot more cheating in high school. And then I went to college—I also cheated in high school occasionally on German exams. I didn't in college because I had to sign an honor code, because it was no longer a game. Because I said I wasn't going to cheat, there was no proctor, and then I had to do it. Perhaps as a reminder to researchers and grad students you should sign a little code saying I promise that all my—everything I say is going to be true. I think people go into the field thinking that, and then whether it is because of advocacy or deadlines, perhaps then me make stuff up. You can certainly make up quantitative data as well, by the way, as has been done. But I'm afraid that the simple fact of fact checking field notes would be like pushing a security barrier at the airport forward or back.

If you want to cheat, you can. You can lie on your field notes just as well as you can lie in your analysis of the field notes. At some point, given the fact that we're professionals and, you know, honorable people, I think we have to—it does rely on trust. Though it is a tough standard to enforce legally, I think it is a better way to sort of enforce the idea of trust of your data, loyalty to your research subjects, up to a point, is a better way to achieve ethical behavior than some of the adversarial or legalistic approaches. Thanks.

MS. JULIET SORENSEN: Thank you, Peter. In my Public Corruption and the Law class as a complement to more traditional assignments I always assign the book *Behind the Beautiful Forevers* by Katherine Boo. You may have read it. I've never before thought of it as an ethnography; I've actually thought of it as a form of long-form journalism, which I tell my students is an important complement to the legal system in terms of addressing and in some instances uncovering or exposing corruption. This past spring was the first time that I had ever received feedback from a student criticizing the fact that the author was a white American. I thought that was interesting that I received this criticism of the book, and implicitly of my assigning the book in this class. I don't know if it was part of a series of conversations that roiled the law school community to some extent really since the presidential elections in November. But actually I thought that the more interesting question is one that Peter mentioned with regard to Alice Goffman, which is to say why

couldn't the book have been written by somebody else? Who should those others be? At least in terms of a book like this reaching an audience like us. So it's definitely worth considering that. And the fact that the book has been written at all may be in and of itself worthwhile. So I want to turn this over to our three illustrious responders. I encourage us to make this all an interactive conversation. We have Gary Fine, Colin Jerolmack and Shamus Khan. So, Shamus, why don't you start with some responses. And, again, this will be interactive, but I'll go more or less down the line.

MR. SHAMUS KHAN: Okay. I'm going to make six points and I'll make them concisely, so hopefully in three to five minutes. The first is just a correction. I write a lot with Colin. I did not write the anonymous piece with Colin, the anonymity piece. It's written by Alex Murphy, a really wonderful sociologist at the University of Michigan. She does great ethnographic work, not urban ethnographic work, but ethnographies of suburban poverty. And she deserves credit for that piece, not me. So I just wanted to air that since I feel like I got credit for writing it multiple times in this discussion and I don't deserve that.

MR. COLIN JEROLMACK: I actually don't even know what you think about anonymity.

MR. SHAMUS KHAN: The second is, with all due respect to the person who is working the hardest in the room this entire time, the stenographer, we are not stenographers. So I might ask one provocation, which is to say what does it mean if our subjects disagree with the accounts that we provided. And would we always want to give the supreme primacy to, say, their verification of our accounts. Or do we believe that we do something more than provide, say, a transcription of what they say that may in fact differ from their understanding of what they're doing. The third is to point to the importance of changing details, something that we would disagree on with the text, and the ethics of that.

I'm going to give you a story from the sexual assault project that I've been working on. It's about a young man whose parents are undocumented workers from Pakistan, who is gay and therefore cannot go home during the winter breaks. So he has found different office spaces that have been abandoned in New York to sleep in during the day and then to ride the subway at night. And often reported to us engaging in sex work in order to help subsidize his summers—I mean, winters and other expenses. And this is something that he told us. And it's important to write about because there are certain ways in which we find within our work that precariousness increases the likelihood of being assaulted. So people who experience precariousness, particularly economic precariousness, have much higher rates of assault than other people.

Now, I did not tell you the right details of this young man's life. They're close to what I just said, but it's not accurate. The problem is if I told you the real details of his life, as you might imagine, there aren't that many gay Pakistani children of undocumented immigrants who attended Columbia University between the years 2015 and 2017. And to out him as being someone who did that would perhaps be beyond what is responsible in this context. But I think that the story is important enough, and it's not important just in terms of a pornography of poverty, but it helps us understand the ways in overlapping forms of precariousness actually expose him to a lot of risk and drive him to a set of behaviors, reported behaviors—so we didn't observe these behaviors, he talked to us about them—which I wouldn't feel comfortable reporting if I told you the real story, the quote/unquote

real story. So changing details in that context is actually really important, and I would say it's not that rare. I want to reiterate the idea that it is sometimes not great to get consent forms.

We didn't get consent forms in a lot of the sexual assault research we did because we didn't want names associated with the research in any way. So that if people filed requests to get our information they could not connect, say, the population of people we talked to, the 150 to 175 of them in interviews, to the actual transcripts. And that struck me as—

MR. PETER MOSKOS: There is a basic dilemma here, which is signed consent forms and anonymity. That is not addressed enough.

MR. SHAMUS KHAN: And then finally I want to ask one provocative question, and then I'll shut up. The question is if the existence of an example of a deviation from, say, a norm within ethnography, or at least things we can critique as bad ethnography, is a good thing or a bad thing. So we might suggest that where you find no violations of ethical rules, norms and other kinds of things within an academic space we should be happy. But I would suggest the reverse. That, in fact, the presence of this conference may actually suggest a robustness to this. And we may be much more worried about fields where there is no contention about these sets of issues because it means that ideas that push or don't exist, or that the norms are actually not normative.

So I would remind us of like my favorite discussion in all of sociology is Durkheim's discussion of crime and the ways in which people—crime is a normal phenomenon and that there are these normative things that require us to sort of recommit ourselves to it. I may worry more about an ethnographic context where a book like Lubet's could not be written because everyone so strictly conforms to some set of rules. Now, we could have pathological versions of this where we had too much violation of this, but I might suggest that this is a sign of something actually kind of healthy within a space. And also suggest that the debate has not been squashed.

The discussions have been actually quite public and they have had impacts on the field, and this is a sign of actually a really healthy area. I now insist on seeing all of my students' field sites, I visit their field sites. I do things as an advisor that I probably wouldn't have done in light of this. It suggests a kind of robustness to the area that I think, you know, we might want to acknowledge and think through and ask, maybe it's worse if cases like this didn't exist and what would that mean. And some of the demands it seems that are being made upon ethnography towards always revealing sources, only doing research under a very set of circumscribed conditions could actually lead to a much more impoverished research space than the one that we have right now.

MS. JULIET SORENSEN: Thanks, Shamus. I agree that as a general matter an injunction to be open to reevaluation and reexamining our methodology, our supervision of our students is more—is worthwhile and is what keeps the field dynamic and on the cutting edge. I'm going to ask our presenters to hold off until all the responders are done. So, Colin, over to you.

MR. COLIN JEROLMACK: You know, one important thing related to ethics with our research subjects that hasn't come up, I don't think, a single time in this conference is the ethics of representation. So, Steven, I think it's useful, but some of your concerns around criminality I would categorize as harm that we don't stop, but that is already happening. But there is also—and I don't want to exaggerate the extent to which it may be true; I actually think we don't know the extent to which it is true—a harm that we create through the way that we portray people. And I don't know the answer to this because I don't think that IRB's should be able to expand their powers to how we represent our subjects. But we harp a lot on how we have to protect them in research, and how protecting them means pseudonyms and masking, and that may or may not be true.

I'm really drawn, and actually sometimes heartbroken, by the ethnographers who take the time and have the honesty to write about what happened when they showed the book to their subjects. It seems that sometimes the extent to which their subjects feel hurt and betrayed has nothing to do with whether they were identifiable to anybody else. In her new edition of *Unequal Childhoods*, if you have not read this, Annette Lareau returned to the family she wrote about. There is no evidence that anybody's confidentiality was compromised, yet she said that half the families were deeply troubled by the book. Some felt that it made them look bad. Others felt that her portraits failed to grasp the core elements of their own subjective experience. For example, one family complained that Lareau seemed to not take seriously the fact that the kid might actually enjoying playing violin and that's why they go to all these violin lessons, that it's not just about cultural capital. Nancy Scheper-Hughes writes something similar about the Irish village she studied. Her subjects said that they saw her book as a “science of scandals”. She only wrote about their troubles and not their strengths. I think what these responses indicate is that some of our subjects may have folk definitions of research ethics, what they consider to be ethical research, that are totally orthogonal to the way that IRBs, or even, as Bob says, professional ethnographers think about ethically responsible research. It may have nothing to do with the confidentiality principle in some instances. And what worries me, actually, is that they may be largely incompatible with the norms of scholarly writing. Because what we say is yes, Annette's job as a scholar, which makes this sociology and not journalism, is that the Tallinger family, whose kid played the violin so many hours per day, is a social type. And that's what we are doing, we are creating generalizable stories.

It's not about the particular people, it's about the types that they represent. That's our prerogative. But that may not be our subjects' prerogative, and I wonder about the amount of times that we don't know that's the case because people don't do what Annette or Nancy did, which is go back, or they don't want to tell us about it because makes us look like terrible human beings. So I would just say this, relating back to confidentiality and masking, if it's out of a sense of ethics that we mask, I think it's worth recognizing that masking might do little to reduce the unanticipated costs that our knowledge production produces for our subjects. And these costs are often borne quietly and anonymously unless scholars go back, as Annette did, and revisit these subjects and write about it at a later date. That's it.

MR. SHAMUS KHAN: Can I add one thing?

MS. JULIET SORENSEN: Sure.

MR. SHAMUS KHAN: I just wanted to mention I looked up about whether certificates of confidentiality have been legally challenged. There are very, very few cases. The one case that is sort of widely reported is from 1973 when its authority was upheld by the New York Court of Appeals. The Supreme Court declined to take up the case. This could either be evidence that no lawyers have decided to challenge it, or that it's seen as codified in law. I'll let the lawyers interpret that for us. But apparently it has basically been since '73 that there have been cases that have challenged it. That's why we were given the advice that we shouldn't think of it as settled law because there is so little case law around it.

MS. JULIET SORENSEN: Sort of like the emoluments clause today. Gary, you presided yesterday, but Steve said you didn't say your piece yesterday. Now is your chance.

MR. GARY ALAN FINE: Well, Julia, what I would like to do is throw the floor open for discussion, and then if I could get maybe the last ten minutes. And then we'll let Steve say what he wants to say, which I predict will be to thank us all and to say that the elevator story is still wrong. So let's have a discussion and—

MR. STEVEN LUBET: There will be a word or two about ethnographers in cars with guns.

MR. TIMOTHY HALLETT: Yes. Alright. So since we're talking about ethnographers in cars with guns, this goes back to the point that I raised earlier about the very real crisis and détente that happened in ethnography as realist assumptions were confronted with postmodernist subjectivity, feminist critical understanding and radical subjectivity. Since then ethnographers have tried to do a lot to temper their sort of vainglorious assumptions about reality and ultimate source of truths. And one of the things that happened in Goffman's book is that the methodological appendix was very much an effort to address the ethics of representation. Because she is a white woman studying African American men and to address the issue of positionality, and very much to address how close she came—you know, something that ethnographers talk about all the time, the dilemmas of going native and how close that can come to be.

So this was something that she had to do in contemporary ethnography to deal with the postmodernist, impressionist, confessionalist critique of realist ethnography, it's something that the discipline demands. It's also something that very much got her in hot water in her account. So, you know, one—Gary and I have talked about this, and one thing glibly that we could say is she shouldn't have burned her field notes but she should have burned her methodological appendix. But, the other way to say this is she didn't do a very good job as a realist ethnographer and she didn't do a very good job as a postmodernist ethnographer either. Or we could say she's damned if she does and she's damned if she doesn't. And that's the complexity and difficulty of reviewing ethnography from a particularist perspective without the context of the whole.

MS. JULIET SORENSEN: Any response from the presenters?

MR. PETER MOSKOS: No.

MS. JULIET SORENSEN: You left them speechless.

MR. COLIN JEROLMACK: I'll say one small thing about this. I was somebody who, when my book was peer-reviewed the second time, was asked to write an ethnographic appendix, which I refused to write. I felt like I inserted into the book the moments where I needed to be to tell you how I came to know the things I know. I think that what you sometimes see in these ethnographic appendices is a literal checklist. What's interesting is that, as ethnographers, we often want to unpack and make variables messy, but then we'll say, like, "I was white. Here is how being white affected what I did in the field." "I was a man. Here is how being a man affected what I did in the field." You know, "I was young when I did this. Here is how being young" These are statements that sometimes are obligatory.

But there is this way in which we sometimes create this laundry list of our various attributes and then attribute causality to them with more confidence than we make our causal statements about what's going on in the field. So I do think this is something we have to reconcile and deal with. On the other hand, I concede that I was uncomfortable with some of the moves that Matthew Desmond did to write himself entirely out of the book, to the point where you have to read an endnote to find out that the friend who lent the U-Haul truck to someone being evicted was him. I felt like I needed to know that in the book. So I'm not saying that we go back to the fly on the wall, but I have gotten a bit uncomfortable sometimes with the way that methods appendices get written to try to deal with this postmodernist, feminist critique.

MS. MARY PATTILLO: So I have a question I think for Shamus and Bob, or maybe Shamus and Gary, about IRB. So my understanding is actually along the lines of what a couple people—oh, I think what Peter said. IRB is in many ways to protect the university and sometimes the research, not often the subjects. And then we hear the story, Shamus, that when you need some protection, when you get a subpoena, they're like, go hire your own personal lawyer. So my question to maybe Gary and Shamus is what is the university's response or responsibility once they vouch for your research when it is needed?

MR. SHAMUS KHAN: So I just want to—for those of who were not here, the notes from my first book have been subpoenaed just last week, so I'm dealing now with the question. In terms of the university's quote/unquote refusing to protect me, I think that's probably good legal advice. The lawyers here may have a better interpretation of this. But their job is to protect the university, and there are probably instances where the interests of the university and my interests are not going to align and under those conditions I may want independent counsel.

Now, this means—I mean, I didn't just contact Colombia's general counsel. I also contacted Princeton's, because Princeton published the book, and Wisconsin because Wisconsin approved my IRB as a graduate student when I did the research. The other two general counsels have not gotten back to me. I think they're just hoping it goes away and that they'll be made aware if there is some relevant thing that happens. I'm kind of curious to see what happens there.

MR. ROBERT NELSON: You signed that contract.

MR. SHAMUS KHAN: What?

MR. ROBERT NELSON: You signed that contract with Princeton. No doubt there is a disclaimer.

MR. SHAMUS KHAN: I'm sure there is.

MR. STEVEN LUBET: I crossed those out, by the way.

MS. JULIET SORENSEN: I'm sorry. I did miss this earlier reference. Is this a grand jury subpoena in a criminal investigation?

MR. SHAMUS KHAN: No. It's a civil case against a school that I have—I spent a year documenting things. So they want all of that documentation. I think in this instance it's—you know, the way that I've been thinking about this for the last week is the difference between my legal obligations, which is certainly to turn over the material, and then my obligations to my subjects, which is not to turn over the material. And the challenge in thinking through this is that even—you know, the obvious solution is to just redact the material sufficiently and hopefully have that be allowed and then give it to them. But even that it strikes me violates some of the ethical commitments that I made to the research subjects. So I suspect that there will be like procedural solutions around this, but I'm not very equipped to think through it.

The IRB, it's interesting to think, could intervene actually in this instance in order to protect the research subjects, right? To say that there were a series of commitments that were made and it's our responsibility as an IRB because we certified this research in this way to actually protect the information from these people. I've never heard of an IRB doing that. It would be a very interesting move for them to undertake. I doubt in this context that the University of Wisconsin, a public university in this current political climate would be the institution that would first do that.

MS. JULIET SORENSEN: I've never heard that as a basis for a motion to quash a subpoena, which is the legal way in which it would be presented.

MR. GARY ALAN FINE: I remember back in 1975, when I was first doing my research on Little League Baseball, and this was before the Belmont Report before the IRBs were before human subject committees where we still called them ethics committees, I think. And the reason that I was very happy to have my research with Little League Baseball go before them is because I was driving kids around in my little, what was it, Plymouth, and I was told by Harvard that if I got in an accident Harvard University would cover me up to \$3 million per person if we had some crash. So I felt really good about that.

I think the important thing, one of the things I've emphasized as a social psychologist, is to remember that institutions are local. And so there is not an IRB, there is an IRB at Northwestern. Actually, there are several IRBs at Northwestern, medical school versus the social sciences. And that would be true—you know, I hear stories about what goes on at the IRB at other schools, and I know it's really quite different from what was

done at Northwestern, at least when I was chair, which was in the early 2000s. So there is a lot of individual differences. Well, should I—

MS. JULIET SORENSEN: Well, let's see. As I recall, you asked for the second-to-last word, with Steve having the last word. I do want to circle back to our two presenters first to see if you guys would like to respond.

UNIDENTIFIED MALE SPEAKER: And there is another hand in the back.

MS. JULIET SORENSON: Thank you.

UNIDENTIFIED MALE SPEAKER: This may be of no interest to people. But there was a slight discussion yesterday about the organization research practices. I gather Shamus is now part of a team that involves multiple methods and multiple disciplines. Mary said that she still prefers to work as an individual ethnographer and writer. So I wonder has there been any discussion as to how a number of these problems regarding validity within a hermeneutical science could be solved or addressed more effectively by working more in teams? And I ask this in part because of one reason, the physical sciences have become so powerful in the last 40 years because they've become ever more collaborative and ever more grounded in laboratory research. I'm wondering if the social sciences should be doing more of that.

MR. PETER MOSKOS: Interesting. I share Mary's belief. I like working alone and working on the cheap and avoiding the whole mess of hassles that comes from collaboration. I wonder if collaboration in the other fields is more simply out of publication issues and getting your name on publications and less out of how it helps the research, though still may help the research independently. One of the things I didn't say because of time reasons, but I think if anything ethnography should resist the urge to sort of set up a single set of standards and practices. Now, that may sort of be separate from a collaborative question. But I do find that insecure academics that always look up in the field to the more quantitative, whether they admit it or not—and I think we're all in this room probably qualitative.

But sociologists seem to be insecure when it comes to economists and anthropologists seem to be slightly insecure when it comes to sociologists. In academia we all look down on journalists, with good reason, and I don't know what the journalist perspective of that is. But I think even given the constraints of ethnography that there is still such variety that we would—in a way I think it's better to be a band of misfits. I think we would suffer if we decided that the same ethics that apply to, say, doing research up in the power structure as opposed to doing research down, those I think would be entirely different matters.

I don't think one size fits all in this thing. In terms of collaboration, I very much wish that *On The Run* was written in Baltimore when I did my research. It would be a fabulous sort of collaborative—even in that case a serendipitous and collaborative effort. I presented one side of a very small part of a really small community. I would prefer to see collaboration in a way being at a broader level than sort of three people trying to do the same thing, both in terms of efficiency but also in terms of knowledge based.

MS. ANNA MUELLER: I just want to—so I actually personally do think that collaboration could be an interesting sort of methodological approach to solving some of the issues within ethnography. I work collaboratively on a qualitative in-depth case study with one colleague who is—we're both assistant professors, and so it is not quite what Shamus or I think what Mary described where there are sort of—it's like a true—we go into the field together, we've done interviews in like side-by-side rooms, we talk, sometimes he goes to the field, sometime I go to the field, and so on and so forth.

But we do notice difference things in the field and by, you know, sort of rigorously debating and challenging each other—we have different specialties within the field of sociology, and it just means that we pick up on different things that aren't necessarily incompatible, right? So I think in the end our work has been much more robust because we've had two—well, also, we're both white, but he's a man and I'm a woman and that actually allowed us some flexibility with some respondents who felt more comfortable with a woman or more comfortable with a man. It was just—I don't know. So I actually see a lot of value in going into the field—

MR. PETER MOSKOS: Do you think it was twice as robust? I'm just wondering.

MS. ANNA MUELLER: I'm not going to say that it is twice. I think that is a mistake, that's—

MR. PETER MOSKOS: Okay. But just better.

MS. ANNA MUELLER: I don't even necessarily would say that it was better, right? But I think that it is something that I never—I don't really hear that talked about very often because we do have this sort of lone wolf model. Or we have the model that I think was more like Shamus and many of my friends have engaged in where it's more sort of formal organizational ethnography with multiple people following kind of more strict protocols so that there is some matching. I'm just offering a third. I'm really actually quite—I'm more interested in these conversations and much, much less interested in say, my way is the best way. But I do notice that there is no—that this sort of third option in my opinion I rarely see it, like, coming up. Perhaps it is special when you have somebody that you can work that closely with and that easily with. Some of my collaborations are not that fluid and haven't been that much fun, but this in particular works. But I just would stress that different things were seen because there were two people with different perspectives actually in the field.

MR. PETER MOSKOS: If you don't want to say it's better, I'll say it's better. It certainly doesn't sound worse.

MS. ANNA MUELLER: I'm not ready to say it's better.

MR. PETER MOSKOS: Perhaps different – I mean, that sounds great.

MR. SHAMUS KHAN: I want to throw some cynicism on this, though.

MR. PETER MOSKOS: Yeah, you do that, Shamus.

MR. SHAMUS KHAN: The Open Science Collaboration, which was a project to try and replicate major findings within psychology, was only able to replicate of sort of the hundred experiments that they tested in the top journals in psychology only able to replicate one-third to one-half of the results. There are a host of reasons offered up as to why. At least half of the results were non-reproducible. Some was that the methods weren't clearly articulated enough. But one of the opined reasons was actually having large scale labs with abdication of responsibility where it was much easier to actually imagine that other people were doing things in the correct way. So you had these kind of like big organizations where it was pretty simple to assume that everyone was doing their job correctly and therefore as they sort of pushed forward they were—what they were doing was basically like—I mean, kind of p-hacking, but like looking to find significant results. And I would say that like, you know—at Columbia, I'm the Arts and Sciences representative for the research compliance group, and we see very few people from arts and sciences; we see a lot of people from these huge labs.

Often the explanations—this is very anecdotal—is well, these are big organizational labs, it's hard to keep track of everything that is going on. I would say just a little bit of caution because if we look to those fields where there are these big labs, they may be increasing in status but I don't know that they're creating systematically more consistent and ethically produced and solid knowledge. Psychology certainly isn't. The replication crisis in psychology right now is fairly acute. Econ has gotten better, but econ has gotten better in part because almost every training program in econ right now demands that first- and second-year students reproduce the findings of major papers from the previous years. So as you're publishing those papers you know that there is going to be a little army of future econ doctoral students reproducing your results. So I think that I would be—I would be cautious of all of these sort of mechanical solutions.

MR. PETER MOSKOS: That doesn't mean we should be cautious of—and this is what I mean by we are a wonderful band of misfits when they make the movie. To have—to say that we shouldn't have a single bit of norms and standards is not to say we shouldn't have any standards. I'm saying sometimes maybe we do need an adversarial standard. Sometimes we do need a journalistic standard. It depends on the kind of research you're doing. Each of these fields, you know, has its strengths and negatives. And to have—I mean, I do think as ethnographers we're just happy to be noticed. Thank you for inviting us to the dance, Steve. I'm sorry I didn't dress for it.

MR. STEVEN LUBET: I like what Shamus is wearing.

MR. PETER MOSKOS: But there's certainly—

MR. STEVEN LUBET: And Gary's okay too.

MR. PETER MOSKOS: Are we going to go through everybody?

MR. STEVEN LUBET: No, we don't have to, Peter.

MR. PETER MOSKOS: But so replication is fine in some places, in other places it's either irrelevant or counterproductive. And so, I don't know, maybe the last thing I would say is I want to emphasize the idea that it's important to be good writers, and a lot of that I think you could learn from journalists. It is important to understand that we're not necessarily telling the story about the person that we're writing about. Often we are trying to use that person as an example in a greater truth. But it's not about you necessarily, the research subject. I hope that my own research goes beyond the individual ten people I was dealing with all the time. I'll stop there.

UNIDENTIFIED MALE SPEAKER: I wonder what's the difference between studying up and studying down in terms of this ethics and law and the presentation of data. My personal case is I study Chinese art students back in China who were high school students. And then I'm a researcher. I consider it as studying down because I'm from leading university in the States. Chinese students, teenagers, see me as a scholar, as, you know, kind of a Ph.D. student. So in mind it provided whatever they can to form my research. And in my writings I can say whatever I want because I am the author and I can portray them—in terms of the style of writing, I can do whatever.

Then the second case I'm about to study maybe one of the Hollywood film studios and the Chinese (inaudible) owned by China's richest man, and in terms of this research I have to obey all these rules and when I present the data it has to be very sensitive. I would definitely call—you know, this is a quote from where and it is from which document, but in the Chinese art student case I would not do that. So I wonder, you know, like with the elite sort of professors who study in urban sociology usually would either be homeless people and inmates, a lot of more studying down, so I wonder when you present data and in terms of ethics and the law how would you deal with that until we obey the same standard or like the same criteria in terms of research.

MR. PETER MOSKOS: So we have to sort of individually as professionals judge it. One of the things I did—and, again, the most I'm saying we shouldn't, if we don't have to make people anonymous. I had an extra problem, which is in one instance when I changed names because I said I would is I couldn't use characters in a narrative sense because then they would be identifiable. That was a writing challenge. I'm trying to write a book that's readable and yet I can't have characters or any plot line because you can go through public records and get the list of names of people in my book if you wanted to, and I didn't want quotes to be attributable to them. So I used a form of masking and sort of had to mix up descriptions sometimes for people to appear in different contexts. I never say a lie, I just may identify this person as a female sergeant and this one as someone with, you know, 13 years on because I don't want those two things to be linked. But that's sort of a unique situation I think dealing specifically with my research, and I don't know about researching up or down. But I don't think there is a single answer.

MR. COLIN JEROLMACK: I'll be very quick. I just think this is an important point. For instance, I'm going to generalize, but friends that I know, and I'm not one of them, who study organizations, particularly powerful ones, the organizations have the legal power to

affect your representation. So they can say, “Even if you're anonymizing, we have to sign off on it.” So this gets back to whether there is an ethics of representation that we may owe our subjects beyond what IRB or professional ethics require. Because in the absence of those powerful actors, when you are dealing with your students, you're not required as a term of getting access to show them what you're going to present about them before it comes out in the open. So then for many of us, not all of us, that means that we're not compelled to do it so we don't do it. But then those who are more powerful compel us to do it, so for them they get to see how they are presented and have some kind of discussion about that.

Again, I don't know what the answer is because I'm very uncomfortable with the idea that I show it to my subjects, and they say “I don't like this, take that out” and so I say, “Okay, I'll take it out because you don't like that.” So I'm not saying I have an answer to this, but I do think there is a difference where the more powerful get to have more control over how they're represented and for the rest of us it's entirely up to the author how they're represented.

MS. JULIET SORENSEN: Bob.

MR. ROBERT NELSON: Yes. I guess I would say that you don't want to change your standards. You're still trying to make a valid argument. But you should—you know, if you are an ethnographer as tool or instrument you have to be aware of what kinds of data you're getting access to from those two different groups. It may be getting entirely different kinds of filtered data from the more powerful as opposed to the less powerful students. So that may affect what kind of argument you can construct from the data you get. So you do want to be careful in terms of how you analyze that and maybe even in terms of—I mean, there is the issue of, you know, protecting subjects and protecting components of the data and that may be different in those two contexts too. But, also, what is the audience that you're trying to present to? What kind of argument are you actually trying to present in your data in those two different contexts?

MS. JULIET SORENSEN: Gary, I think the floor is yours.

MR. GARY ALAN FINE: All right. Thank you very much. Well, first I want to thank everyone at this conference. When Steve and I began to organize it, we said, “Who are really smart people that we would really like to hear?” And we were very successful in getting a whole bunch of those people here yesterday and today. But in addition I want to thank the audience, and particularly the students in the audience. Because what I hope we made clear was that these are not closed debates, they are open. And as the students you become professors and rise in ranks and become emeritus, et al., that there will be different discussions that will occur and that you will be part of the continuation of the debates and the discussions that we had.

And I thought that I would take a few minutes to think about this conference as it inspired me, as what it meant to me. I was particularly struck in light of an article that Christopher Wellin and I wrote about fifteen years ago on ethnography as work. And what Christopher and I decided to do was that we would treat ethnography, we would treat participant observation as if it was any form of work. That we would apply the sociology

of work and occupations to ethnography. And I was struck by the way that that approach might help us, might inform us today. We have the responsibility of what Howard Becker said of telling about society. Well, how will we do that?

A lot of people tell about society, and we have heard a number of different—about a number of different occupations which tell about society, which use stories, which use qualitative data, fieldwork, real experiences, whatever we want to call them. And I wanted to talk a little bit about these, because I think that what we're doing is talking about what we might call comparative persuasion. That is, how do different occupations construct persuasion in ways that are meaningful within their field. Or, we might think of this as skepticism embedded in structures.

So the first group are, of course, the lawyers. And we've talked about that yesterday and some today, that the particular role of the law is very different than the role of the academy social scientists. That law is fundamentally adversarial and that has implications for how work, how presentations, how stories are considered justifiable or unjustifiable. That they are judged by judges, they are judged by jurors. There are people in the courtroom who have as their moral responsibility not to be persuaded, that is, the attorneys on the other side. And if there was too much persuasion, if the defense attorneys said, well, I guess the prosecutor has a good point. That would be a case in effect of malpractice. And, of course, you also have your clients who are judging the stories that the attorneys are telling. Are these our stories? Is this a justified version of what I as your client believe to be true? So that's a view of the way that lawyers treat the kind of fieldwork stories that we are engaged in.

And then, thanks to Steve Mills and other discussions that we've had, we have journalists. And journalists are engaged for the most part, with a few exceptions, of creating quick knowledge about the work. They're in there, they have deadlines, they don't have six years, except for a very few cases, but they have to get that story quickly and in a way that their readers consider truthful. And even more than their readers, the way that their editors consider truthful. So that an editor of a journalist's story will ask about particular pieces of information. Where did you get that story? And I have to tell you, no one has ever asked me that question. And I don't ask my students that question. I push them hard on theory. I push them hard on analysis. And I take in almost every case their data for granted. And there is very little I can do otherwise, I think. It is a problem that has been raised, of course, about *On the Run*. And a number of people have said, well, what about Alice's advisors. She was an undergraduate, she was a graduate student. And I think the important point is how rare that would be. How unusual it would be for an advisor to say, well, prove to me that this story was right. We just assume as colleagues that the stories, that the field of work is accurate as given.

Then we have the world which was mentioned briefly of the artist. And the artist—and I'm thinking here of visual artists. I've done that research recently. The visual artist is telling stories, is telling about the world, telling about society, but is doing it in the context of a market. In other words, if they tell the story well their work will be collectible, curators will look upon it with favor and they can be part of these major shows or purchased by important collectors. That's very different than our work which has very little marketability for the most part. And when it does, as in the case of *On the Run* or *Evicted*, there are people who are looking over our shoulders.

Then we have storytellers. We love these storytellers. We love listening to the great narrators. Here the issue is the immediacy. It is the story. Is there an immediate response. Is there an emotional response. And this involves an audience in situ. Not thinking later, not thinking, not reviewing the text, but responding at the time.

And then finally there is us. There are social scientists. And our domain, the domain on which we are judged, is peer review. And peer review means that I will read an essay, an article, a book, and I will sit there and say knowing what I know, do I consider this plausible. And knowing what I know about the author, do I consider her credible. And if the answers to those questions are yes, it gets my endorsement. And, of course, in many cases of peer review, most book reviews in sociology are positive reviews. I did a study of reviews in *Contemporary Sociology*, and those of you who are about to publish a book—and, Bob, I don't know if your book has been reviewed. But about 85% of the reviews in *Contemporary Sociology* are positive reviews, and close to 15% are mixed reviews fundamentally. So we have this system in which we are primed to like each other. Okay. So this represents five of the different ways in which occupations can be organized as a means of persuasion. The question we have is how do we examine the truth claims of others, that is, our informants, and how do we present and examine our own truth. So the issue is persuasion. What is the story, what is the moral. And perhaps, I would like to think, that this question is the moral of this conference. Thank you very much.

MR. STEVEN LUBET: Well, I have to say I'm greatly relieved that 85% of the sociological book reviews are positive, but I guess we'll fix that. I think I'm pretty sure we'll take care of that problem by and by. The truth is I'm overwhelmed by the response. This whole project for me started as a book review which I wrote over the weekend, and it didn't occur to me for a moment that I'd still be talking about it two years later. And it was actually Gary who said you need to broaden this out into a book. And it was truly probably the best professional advice I've ever gotten. And that's what I tried to do. Of course, I think I ruffled some feathers along the way. But, you know, I am a fan of ethnography. I said that in the preface of the book. It would have been intolerable to read all those ethnographies if I hadn't liked them. Just impossible to do. I want to make a few more comments about what has happened on this panel and then we'll be done.

Shamus, I understand completely why you would change the details about a gay person whose life might be in jeopardy, but I don't think that's a warrant for the level of detail changing that we see in ethnography. You know, that should be the exception under extreme circumstances, and it's not a reason to do it across the board, which is what I think many people—in fact, what I'm sure many, excuse me, what a number of ethnographers do. But let me go further and say if you are going to do that, I think it requires more flagging than a parenthetical in the preface. I'm not saying you did. But sometimes—seriously, often I would read the prefaces to the book and they would say details have been changed but they don't matter. I think that's an insufficient disclaimer. I think in this particular case it would need to be flagged.

I'm writing about a person. I have changed the details for the person's protection, but here is the point I want to make. Almost like a parable. So to me that is quite acceptable. But the across-the-board detail changing, I think, is not acceptable. I would love to see ethnographers challenge IRBs. I think you're mistreated by the process. I think you're unduly constricted by the process. I don't know how many of you have read Zach Schrag's

book *Ethical Imperialism*. It's terrific. I commend it to you. He also has a blog, the Institutional Review Blog. I think it's something that would serve—

MR. PETER MOSKOS: He's kind of moved on, but he spent some great years of work on that.

MR. STEVEN LUBET: A lot more attention. Bob Nelson said that I don't say what crimes are wrong or acceptable. So let me say. I think we should draw the line at risking human life. I think we should draw the line at risking human life. And I think that line is drawn *ex ante*, not *ex post*. So the fact that nobody was ultimately hurt to me is irrelevant. If lives were put in danger, as they were, I think that's wrong. And I think no ethnographer should drive people around with the thought that somebody might get shot.

And if you don't accept that in this example, let me give you a thought experiment. Imagine that it's an ethnographer driving around some neo-nazis looking for a synagogue to burn. Right? And they didn't find one so nothing happened. You know, do we say, well, then, it's okay? You know, it's fine driving around looking for a black person to lynch, but we didn't find one so that's just good. I don't think we would say that. And I don't think we should say that. So that is—you know, I think it's a fine line. I do want to give you a list of ethnographers who have written about crimes but have not been subpoenaed. Alice Goffman, Laurence Ralph, Philippe Bourgois, Randall Contreras, Victor Rios, Matthew Desmond, Laud Humphreys, William White and Howard Becker. Those are just the ones I could think of in the moment. So, Shamus, I'm terribly sorry this happened to you. I think there is a solution. It may not be cheap. But it is unusual. It is unusual. And I don't think fear of subpoena is really going to dramatically alter the practice of ethnography. I'm delighted to have provided a helpful provocation, whoever said that.

MR. SHAMUS KHAN: It wasn't all of us.

MR. STEVEN LUBET: I'm a big fan of ethnography. I plan to continue reading ethnography. I'm looking forward to your work in the future. This has been a true education for me. And I want to thank all of you for delivering that to us.

MS. JULIET SORENSEN: This concludes the panel, the conference. Thank you all for coming to the Bluhm Legal Clinic, and enjoy your sunny Saturday.