

Telling Tales?

Subjective Ethnography and Situated Narratives in Longitudinal Research on Violence in Nicaragua

Dennis Rodgers

■ **ABSTRACT:** The ethnographic representation of violence is a controversial issue, involving debates about (avoiding) sensationalism or (acknowledging) emotionality, for example. Less considered is how the subjective nature of ethnography and the fact that ethnographic narratives are always situated can have ramifications for both interpreting and representing violence, particularly in the context of longitudinal ethnographic research. Drawing on my investigations into Nicaraguan gang dynamics begun in 1996, this article explores the subjectivity of the longitudinal ethnographic experience of violence both in and out of “the field” through three specific examples. These highlight in different ways how ethnographic understanding is highly situational and time-bound, meaning that longitudinal research is particularly prone to episodes of discomfiting conceptual disjuncture. At the same time, it is precisely this that arguably imbues it with exceptional power and insight.

■ **KEYWORDS:** disjuncture, ethnography, longitudinal, representation, subjectivity, violence

In March 1974, the anthropologist Fredrik Barth (in)famously published a review article in *Current Anthropology* dramatically titled “On Responsibility and Humanity: Calling a Colleague to Account.” The article, about Colin Turnbull’s recently published *The Mountain People* (1972), sensationally argued that the latter “exhibits a number of anthropological difficulties and failings in such a crass form that it deserves both to be sanctioned and to be held up as a warning to us all” (Barth 1974: 99–100). Turnbull’s study of the mountain-dwelling Ik of Northern Uganda was carried out at a time of acute drought and famine and describes how these had a devastating effect on Ik social structure. In particular, Turnbull ([1972] 1994: 155) maintained that “[t]here seemed to be increasingly little among the Ik that could by any stretch of the imagination be called social life, let alone social organization.” Ik culture had been reduced to little more than an individualistic “survival machine,” geared toward the single concern of “the filling of [one’s own] stomach” (Turnbull [1972] 1994: 285, 238), and *The Mountain People* contains harrowing descriptions of food being snatched out of the mouths of the elderly, young children being left to fend for themselves, sons and daughters letting their parents starve to death, and friends, families, and neighbors systematically stealing from each other.

Barth (1974: 100–102) ostensibly focused his criticism on Turnbull’s “grossly irresponsible” ethics, but it is clear that in fact it is Turnbull’s representation of the Ik that is at the heart of his



virulent reaction. Barth repeatedly expresses doubts as to the veracity of Turnbull's account of the violence of Ik society, which he qualifies successively as being "dishonest," "superficial," "misleading," "bizarre," "pathetic," "flimsy," and a "systematically false record of . . . events," despite not having any firsthand—nor, other than through *The Mountain People*, any secondhand—knowledge of the Ik himself. Although not quite as vitriolic, this is also the general tenor of all the various contributions to the subsequent debate Barth's article sparked in the columns of the September 1975 issue of *Current Anthropology*, which all tended to seek to uncover "hidden" or "misinterpreted" expressions of continued sociability and peacefulness among the Ik in Turnbull's ethnography (see Wilson et al. 1975: 343–352).¹ Turnbull's ethnographic representation of the Ik and their violence against each other was in other words deemed "almost 'too bad' to be believed," despite the fact that it "has correlates in other descriptions of people and societies in calamity," as Nancy Scheper-Hughes (1992: 132–133) has pointed out.

To a certain extent this rejection of *The Mountain People* can be attributed to differences in opinion about "strategies of authorial voice and narrative form" (Jeffcutt 1994: 242), with some of Turnbull's critics particularly disapproving of his writing due to its personal style and the way he explicitly passed (critical) moral judgement about the Ik (see Grinker 2001). Ultimately, though, the basic problem is that both Turnbull's ethnographic research and writing have been perceived as extremely subjective, and his book is now more often than not taught in anthropology classes as a textbook example of "biased" ethnography. The fact that ethnographic research and writing can be subjective is of course by no means surprising; indeed, it is widely recognized that the interpretative nature of ethnography means that it will inevitably always be biased, so this does not really explain why Turnbull's study raised such a storm. Rather, what the furor around *The Mountain People* highlights instead is how this subjectivity is potentially considered especially problematic in relation to certain topics, and more specifically, in relation to the ethnography of violence (see Jones and Rodgers 2019; Koonings et al. 2019).

Although violence has long been a controversial issue within anthropology, whether from a methodological perspective (see Rodgers 2007), a representational perspective (see Bourgois 1995:14–18), or even a theoretical perspective (see Poole 1994), all too often, ethnographic writing about violence tends to be quite categorical, assuming that "the only . . . reality of violence is that wounds bleed and people die" (Nordstrom and Martin 1992: 14). Part of the reason for this is clearly the extremely visceral and emotional nature of the violence, both for those suffering it and those studying them, but at the same time, such uncompromising statements are obviously in contradiction with the fundamentally subjective nature of ethnographic data, and this whether viewed intrinsically, in terms of its narrative representation, or its interpretation. While issues relating to the latter two perspectives have been extensively discussed within the discipline (for an overview, see Jones and Rodgers 2019), the former has not, at least partly because there exists a widespread presupposition that even subjective "data" is immutable, and that it remains constant once collected. This is perhaps especially clear in relation to longitudinal ethnographic research, insofar as this is generally considered to be linearly cumulative in nature.

Certainly, coming back to controversy surrounding *The Mountain People*, it is striking that the two most respected criticisms of Turnbull's work are probably those written by Bernd Heine (1985) and Curtis Abraham (1998). To the best of my knowledge, they are the only critics who measured Turnbull's research against their own fieldwork among the Ik, which they carried out respectively 18 and 29 years after Turnbull's original investigations. They both find that Turnbull's work is fundamentally flawed for different reasons, but their critiques are widely considered to be particularly powerful at least partly because of their longitudinal perspectives. Yet a number of important changes had taken place during the intervening period between their and Turnbull's fieldwork, with most of latter's informants having died by the time Heine and

Abraham traveled to the Ik homeland, and Pirre, the village where Turnbull spent most of his time, no longer in existence following an outbreak of cholera at the end of the 1970s. Moreover, Uganda as a whole had suffered a devastating civil war and Idi Amin's tyrannical regime during that decade, both of which wrought significant changes even in as remote a region as the Kidepo Valley, the heart of the Ik homeland. In addition, Heine is a linguist and Abraham a journalist, and their foci of interest consequently differed substantially from Turnbull's. When seen from this perspective, the extent to which their longitudinal viewpoints really offer a greater measure of validity to their critiques is clearly open to doubt.

This article explores the subjectivity of longitudinal ethnography and more specifically the situated nature of ethnographic narratives both in and out of "the field," with particular reference to the ethnography of violence. Drawing on the ongoing longitudinal ethnographic research on gangs and violence that I have been carrying out since 1996 in *barrio* Luis Fanor Hernández,² a poor neighborhood in Managua, the capital of Nicaragua, I explore the ramifications of this subjectivity for both interpreting and representing violence through three specific examples. The first considers the perspective of ethnography as empirical data, the second reflects on ethnography as representation, and the final one, ethnography from an interpretative perspective. Taken together, these three "vignettes" highlight in different ways how ethnographic understanding is highly situational and time-bound, meaning that longitudinal research is particularly prone to episodes of discomfiting conceptual disjuncture that are magnified in relation to research on violence. At the same time, it is precisely this that arguably imbues the longitudinal ethnography of violence with exceptional power and insight.

Malleable Memories and Respectable Lives

The idea that the passage of time affects our memories and understanding of past events is a common one. Yet despite the fact that this would seem an obvious issue to take into account, particularly in relation to longitudinal research (see Rodgers 2019), it is not something that is widely considered by ethnographers. To a certain extent, this is likely because longitudinal research is seen to be *research*, that is to say, the careful and systematic documentation of a given social reality, and so it is—at least implicitly—often assumed that even if researchers' memories will change, their data will not. Hence the importance of well-curated field notes, interview transcripts, etc. This may well however apply to one-off interviews, but not, arguably, when an ethnographer engages in repeatedly interviewing the same individuals over a prolonged period of time, as I have done with Bismarck, a former *barrio* Luis Fanor Hernández gang member whom I have interviewed every single time I have gone to Nicaragua since the beginning of my research in 1996. He has had a particularly colorful life trajectory, moving from being a gang member to a mid-level drug dealer, to a (legal) entrepreneur running a couple of shops in a nearby market, to becoming a major property owner in the neighborhood, before recently losing almost everything due to a series of rather unfortunate events (see Rodgers 2016). My interviews with Bismarck at different points in time reveal the twists and turns of this trajectory, many of which I observed in person. We are also in regular email, Skype, and now Whatsapp contact, and I talk with him every few months.

Our formal interviews have, since my second visit to Nicaragua in 2002, always followed a set pattern, including most notably recapping his trajectory from birth to the present. I distinctly remember thinking somewhat smugly in 2007, when I interviewed him for the fourth time in as many visits to Nicaragua, that I was beginning to know his life history very well, even if the focus and emphasis of his story had always changed a little bit from one interview to the next.

For example, he had been a proud gang member in the 1990s, but during our interviews in 2002 and 2003, he presented his having been a gang member as a moment “when I had lost my way.” In 2007 and 2009, however, he wore his former gang member status as a badge of pride again. This was clearly due to the evolving nostalgia about the gang in *barrio* Luis Fanor Hernández. The gang in the 1990s had been a vigilante gang, protecting the local neighborhood population and providing them with a modicum of security in a broader context of chronic insecurity, but in the early 2000s had transformed into a predatory organization that terrorized the local community, tainting the memories of the past. By the late 2000s, however, the gang had been replaced by an even more violent local actor, the drug-trafficking *cartelito*, who were responsible for a significant rise in violence in insecurity in *barrio* Luis Fanor Hernández, meaning that the gang was remembered more positively than in the early 2000s (see Rodgers 2015).

In 2012, however, Bismarck completely changed his life story. Our interview had begun as usual, with Bismarck talking about his unhappy childhood, then how he had fallen in with the first postwar gang when it emerged in the neighborhood around 1989, how he was then sent away to the countryside by his mother for a few years, before then coming back and becoming one of the major figures of the mid-1990s iteration of the local gang, and then how spending some time in jail had persuaded him to go on the straight and narrow. At this point he would normally talk about the *pulperia* (cornerstore) he set up, how hard a business it was, and how as a result he turned to drug dealing, and how this had provided him with the financial capital to invest in property and that this had enabled him to get out of drug dealing. During our interview in 2012, however, he moved directly from setting up his *pulperia* to explaining how this had been the beginning of his systematically investing in property. Somewhat nonplussed, I interrupted him, and said: “*Espera, espera* (wait up, wait up), Bismarck, you’ve forgotten about your career as a drug dealer! Don’t you want to go through that before we get on to how you became a property owner and entrepreneur? After all, it’s what really allowed you to do so, no?”

“What do mean, Dennis? I was never a drug dealer. . . ¡*Sós loco!* It sounds like you’ve been smoking something!”

“But, but. . .” I spluttered. “*Que la verga? No jodas, maje, sé que fuiste púsher, no lo podés negar, ¡Te conozco desde casi toda tu vida!* (What the fuck, mate, I know you were a drug dealer, you can’t deny it, I’ve known you practically all your life!). I’ve seen you selling crack, cooking it—hell, I’ve even stirred the pot for you!—and you’ve been one of my major sources of information about drugs and how dealing works . . .”

“I don’t know what you’re going on about, Dennis,” Bismarck answered.

“Well, fuck this for a laugh, you know what, I don’t have to try to persuade you, I can prove it to you, I have a copy of the recording of our last interview with me where you tell me all about your having been a drug dealer.”

I was in the habit of always going through our last interview before doing a new one and had not been able to do so before leaving for Nicaragua, so had brought the actual recording with me (on an encrypted USB stick) to listen in situ. I therefore played the recording, and we sat and listened to Bismarck explaining to me in 2009 how he had become a drug dealer and providing me with details about his activities, sales techniques, evolving relationship with the neighbourhood *narco* (wholeseller), and so on, and after about 10 minutes or so, Bismarck said, “OK, OK, so perhaps I was a drug dealer.”

“¡No me *digas, maje!* (You don’t say, mate!)” I answered. “Now, do you want to tell what this was all about?”

The discussion that followed revealed that Bismarck felt a need to dissociate himself from his past now that he had become a “businessman” in order to reinforce his new-found “respectability.” As he put it: “*Pues, Dennis, when was the last time you came? 2009, right? You see, then I*

was still mainly known as a drug dealer, even though I gave it up in 2007, but now, in 2012, I'm a successful businessman, I have lots of properties, there are lots of people who owe me, who have a roof over their head because of me, I'm *respectable*. I can't change what I did in the past, but I don't have to be associated with it, I'm a new me, you know."

"OK, I get that, it makes sense, but you know, this is me, *maje*, your *broder*, and we're also doing these interviews so that you can help me understand how things work here in the *barrio* and see what happens to former gang members over time. I can't ignore the fact that you were a drug dealer, it's one of the things that makes your trajectory interesting for me and my work. And as you know, I've also already written about you as a drug dealer, and I can't change what I've published . . ."

"OK, but perhaps you could stop writing about me having been a drug dealer from now on?"

"Well, that's kind of difficult, Bismarck, I mean, as you know, you're one of those whose lives I'm following here in the *barrio* (neighborhood), so I'm always going to have to mention it, even if I'm looking at your life now—the whole point of my research is to have a long-term perspective, to see your whole trajectory. . ."

Writing longitudinally will obviously raise many different types of dilemmas, but one of the major ones is how to accommodate writing about individuals whom we engage with repeatedly and whose opinion of what we write might change over time. This is something that admittedly affects all forms of ethnographic research—indeed, also non-ethnographic research—but it can arguably be particularly problematic for longitudinal research, as Annette Lareau has described in the second edition of her book *Unequal Childhoods: Class, Race, and Family Life* (2011). This offers a very honest discussion about the negative reaction of the youths and families she had studied previously when she provided them with copies of the first edition during the course of a follow-up study ten years later. Many of them were unhappy with the way that they were portrayed, and Lareau (2011: 327) remarks that "this likely negative, even 'traumatic,' aspect of research has not been fully integrated into methodological writings, despite striking examples from well-known—even classic—studies. Arthur Vidich was hung in effigy after *Small Town in Mass Society* was published; communities featured in the early sociological studies such as *Yankee City* also expressed ire. William Foot Whyte reported the tense reaction, particularly on the part of Doc, to *Street Corner Society*."

The latter example highlights well how this is an issue that is especially difficult to deal with in relation to a phenomenon such as violence and its associated activities (such as gang membership or drug dealing). Whyte ([1943] 1993: 346–349) details how part of the tension with Doc was certainly related to the "embarrassment" of having been a gang member, and how this might impact negatively on his future career, and the same is clearly true of the motivation behind Bismarck changing his story. As he subsequently indicated, he felt that his violent drug dealing past had the potential to impact on his present and future "respectability."³ But how was I to write about him without taking it into account? His violent past was fundamental to my research and to my narrative about him. We discussed this, and Bismarck and I ended up compromising. He asked me to cease writing about his drug dealing experiences in Spanish, but allowed me to continue doing so in English and French, the two other languages I publish in. This is obviously an imperfect compromise, but it responded to his main concern to avoid anybody in his social circles being reminded about his past, since few people in *barrio* Luis Fanor Hernández speak English and even fewer speak French (although it must be said very few people in the neighborhood actually read my work at all, whatever language it is published in, partly due to a lack of interest, but also due to the fact that a large proportion of the population are functionally illiterate). I have also been careful to actively bring up his past drug dealing experiences only very selectively—e.g., with other drug dealers of his generation—subsequently.⁴

During our interviews in 2014 and 2016, while Bismarck did not avoid talking about having been a drug dealer, he downplayed the whole experience and its consequences.⁵ This, of course, complicates my writing process beyond simply taking his representational desires into account. If I try to write about Bismarck's life chronologically, this obviously inevitably means losing what might be termed the "situational uncertainty" that characterizes its lived experience. At the same time, however, this also forces me to starkly confront the extent to which my writing, even if chronological, arguably forces something of an intemporal and static framework onto the temporally subjective and malleable nature of the ethnographic reality of Bismarck's violent life, including in particular his own interpretation of this violence. In many ways, though, this is perhaps something inevitable, as the next vignette highlights, albeit in a very different way.

Making Choices

On 27 January 2003, I gave a presentation to the Anthropology Graduate Program Colloquium at York University in Toronto, Canada. The talk was one that I had delivered several times previously, entitled "Making Danger a Calling: The Violence of Ethnography in Contemporary Nicaragua."⁶ It dealt with my research methods and explained how I had ended up joining a gang during the course of my PhD research, including a detailed explanation of how I had suffered multiple beatings by gangs during my first few months of fieldwork, and how this culminated in my undergoing ritual initiation into a gang, on the basis of the logic that "if you can't beat them, join them." The first round of questions I received after I finished talking was relatively anodyne, and I distinctly remember answering some of the queries in a rather smug and perhaps even somewhat arrogant manner. When the floor was opened to a second round of questions, however, a graduate student put her hand up, introduced herself as Sandra Morris,⁷ and made a rather unique comment: "You probably don't remember me, Dennis," she said, "I attended a presentation that you gave on your work when you were at McGill University in Montreal, five years ago. It's funny; I'm having difficulty reconciling some of what you said today about your research with what you said then. It's not so much that anything you've said today is different, more that you're so much more certain about what you're saying now, less tentative, if you will. . . It's like there's a much clearer story to your tale."

Being challenged as a scholar is nothing new, of course; indeed, it is one of the foundations of academic endeavor. Normally, however, challenges are made to theoretical interpretations, eventually factual mistakes, and I have to admit that I found it rather disconcerting to have my narrative contested, all the more so as the veracity of ethnographic representation is ultimately always based on an "I was there and you weren't" justification (see Geertz 1988), and so any such challenge can be interpreted as being tantamount to accusing an ethnographer of lying or making things up. I remember stumbling through a likely not terribly illuminating answer about ethnographic narratives being inevitably iterative, but I was both highly intrigued and troubled by Sandra's comment, and subsequently searched through my computer archives to see if I could find my notes for my presentation at McGill University in 1997. I did, and comparing them to the notes for my presentation at York University, it was clear that these were indeed substantially different. Admittedly, they focused on different things—the presentation at McGill had been a more general presentation of my Nicaraguan research—but there was significant thematic overlap between them on the issue of gangs. In this regard, my presentation in 1997 had definitely been much more "tentative" in explaining how I had gone about doing my research, with the narrative of how I had suffered violence and joined the gang, in particular, much more haphazard and "messy," insofar as I did not necessarily connect all the different elements of what I had

presented at York as a single coherent, sequential whole. This prompted me to engage in further dialogue with Sandra, which gave rise to the following thought-provoking exchange via email.⁸

From: Dennis Rodgers
 Sent: 02 December 2003 17:36
 To: Sandra Morris
 Subject: hello & a request

Dear Sandra,

I don't know if you remember me, I gave a presentation at York last January on my fieldwork on gang violence in Nicaragua which you attended, and at which you raised a rather unique question about the different "storyline" my presentation had compared to a presentation you had heard me give at McGill five years earlier.

Your comment prompted a whole chain of thought on my part, and I thought that it might be good to actually ask you, if you were willing, to elaborate a bit more on the differences you saw between my two presentations, as this would be extremely valuable.

I managed to rustle up the text of the presentation I gave at McGill that you attended. It's true that it makes interesting reading when compared to the version I presented at York. It's not so much that the content is any different—I didn't think it would be, I do strive for a certain honesty, you know!—but certainly the "storyline" is not as definite as it has become now.

I've since gone back to my original field notebooks, reports sent from the field, and different thesis drafts to try and chronicle the evolution of my tale, and it's been a rather interesting experience seeing where bifurcations occurred, and why. It's mainly down to what I still think is a natural process of finding the framework through which to interpret fieldwork experiences, which in itself is a subjective enterprise, but this is interesting in itself and I think that it has something to say about "anthropology as a kind of writing" (as Jonathan Spencer put it in an influential article published in *Man* in the late 1980s).⁹

At the same time, some bifurcations also clearly happened in response to some comment or specific event, and that's perhaps been the most interesting issue to consider. Clearly one of the most important ones was a challenge I faced in my PhD writing up seminar where the teacher in charge basically queried after a rather confused presentation whether I'd interacted purposefully with the gang at all, which of course completely incensed me and is probably one of the clear moments when I adopted a more "definite" style of writing up my encounter with the gang, downplaying the uncertainty about whom I was interacting with initially that I remember expressing in the McGill presentation . . .

Best wishes,

Dennis

From: Sandra Morris
 Sent: 02 December 2003 23:54
 To: Dennis Rodgers
 Subject: Re: hello & a request

Dear Dennis,

Wow! I felt a little bad after your presentation last term as it seemed that we had grilled you a little more rigorously (as a group) than . . . well, any other speaker in recent memory. I do believe in professional karma, and so, my apologies if we (I) came off as being disrespectful.

Over the weekend, I found the notes I made during the seminar presentation at McGill, although I doubt they will be of much use to you. They're quite sparse, although my memory of the presentation is fairly sharp (which means I really enjoyed it!). I agree with your assessment that the discrepancy between the talks is a matter of tone rather than content; I think that's what I was driving at in January.

I don't doubt your honesty—in fact, I don't think that's the issue, who makes this stuff up??—my point was only that, at McGill, what came through much more clearly (naturally so, given that you'd just returned from the field) was that, at that moment, you were very much trying to figure out how you related to what had just happened to you, what you had witnessed, the decisions and relationships you had made, and what you were then going to write (kudos for having the guts to parade that out in front of a comfortable, inexperienced and hypercritical group of undergrads!).

I'm particularly interested in the process of how all that messiness becomes whittled down to a coherent, saleable narrative (of self?), and I wonder if the messiness would not give a fairer description of the experience you had? For example, a note that I made during your McGill talk reads: "Poverty fucks everything up." I think I was referring/reacting to the idea that people do what they have to do to keep a sense of wholeness and normality in the midst of "social chaos"—can we even say that without the othering overtone?—brought about by civil war, structural adjustment, and so on.

I don't know what an ethnography of that experience should look like, but I was reading something by the Comaroffs [anthropologists Jean and John Comaroff] a few days ago, where at one point they write that the curse of reflexivity in ethnography—the slippage between "fact" and "value"—is as much a major strength as a weakness.

Sandra

From: Dennis Rodgers
Sent: 04 December 2003 19:54
To: Sandra Morris
Subject: RE: hello & a request

Dear Sandra,

That's a nice turn of phrase by the Comaroffs. It really does summarise perfectly what makes anthropology and ethnography so strong and weak at the same time . . . The idea of "fitting" the messiness of reality into particular narratives is something I struggled with in writing my PhD thesis, where inspired by Walter Benjamin, I turned to montage as a means of trying to reflect this messiness in my writing.

Thinking about it, though, it probably didn't really reflect the messiness of the actual process of "gathering data," which is another important issue to take into account. For example, one thing that struck me when looking back on my field notes was that it took me

the better part of a month to get the names of all the members of the Gomez family I was living with straight . . .

Best,

Dennis

To a certain extent what might be termed the “cementing” of a narrative is to be expected. When I did my presentation at McGill in 1997, I had just recently returned from the field and was still processing both my data and the whole fieldwork experience. Part of the whole process of writing up after fieldwork involves the classification, categorization, and the organization of material into a narrative or narratives. Indeed, to a certain extent, academic writing can in many ways be said to boil down to a form of “story-telling” (Agar 1990: 87). Certainly, ethnographic writing has always inevitably involved the selection—whether explicitly or implicitly—of particular “strategies of authorial voice and narrative form” (Jeffcutt 1994: 242). This is however especially true of ethnographic representations of violence, where “the need for action, drama, high-jinx, colorful characters, and purple prose may drive out the calmer, more subtle and sublime features of the [phenomenon]” (Van Maanen 1988: 135). At the same time, there is also a sense in which the ethnographic representation of longitudinal research can be said to involve a process that is closer to editing than writing. Once a narrative has been cemented, it is iteratively revisited over and over again, and as Brian Moeran (2016: 65) points out, “editing . . . is not writing but rewriting.”

Certainly, this is something that is arguably very clear when you sequentially compare my 1997 McGill presentation, my 2000 thesis (Rodgers 2000), the 2001 working paper in which I first wrote up my experiences (Rodgers 2001), the 2003 York presentation, the 2007 formally published version (Rodgers 2007), and finally a new account about my methods published in 2019 (Rodgers 2019). Each different iteration sees uncertainty ironed out, a more definite narrative be established, a more unequivocal short-hand invented. This is particularly the case with regard to my initiation in the gang, which went from being represented from an emic, interpretative perspective that reflected that I did not know what was going on at the time, to a more etic, analytical perspective that by its very nature was more definite. This, of course, raises critical issues with regard to the notion of “intellectual honesty” versus the natural iterative process of interpreting information and experiences longitudinally, although different forms of academic publishing also inevitably lead to the adoption of different forms of representation, and many academics frequently write different articles about the same topic from different theoretical perspectives at different points in time, responding to academic fads, fashions, and processes of forgetting (Law and Lybeck 2015).¹⁰ They are however probably ultimately impossible to “solve” precisely because of the situated nature of ethnography, but they do highlight the need to adopt a fundamentally reflexive and self-critical approach to long-term, repeated research, and this whether from the perspective of ethnography as empirical data, as the previous vignette about Bismarck demonstrated, or in relation to ethnography as representation, as this vignette emphasized. The next and final vignette, on the other hand, highlights how this is also the case from an interpretative perspective.

Encountering Female Gangs in Nicaragua

One of the most unexpected and initially unintelligible findings of my longitudinal research on gang dynamics in Nicaragua was the rise of female gangs in 2015–2016. Historically, Nic-

araguan gangs are very much a male phenomenon; although female gang members are not completely unknown, they are clearly not the norm (see Rodgers 2006: 285–286). In my work, I always associated this particular gender bias with the fact that being a gang member in Nicaragua involves behavior patterns that revolve around activities associated with *machista* “ideals of manhood,” such as “taking risk [or] displaying bravado in the face of danger” (Lancaster 1992: 195), that inherently challenge Nicaraguan ideals of womanhood, which are associated with “subordination” and “domestic roles, especially mothering” (Montoya 2003: 63). To this extent, I effectively analyzed gangs and their violence in Nicaragua as representing the enactment of an exaggerated form of “hyper-masculinity” (even if I was careful not to reduce gangs only to this). Such an interpretation held up for the first 20 years of my research in Nicaragua, as I only encountered all-male iterations of the local gang on my regular visits to *barrio* Luis Fanor Hernández. In 2016, however, I come upon a female gang, which was a complete and disorientating surprise.

Known as a *combo*—a term that seems to have been borrowed from a popular Colombian telenovela shown on Nicaraguan TV—this female gang had a different dynamic to the male gangs it had replaced, in particular vis-à-vis their practices of violence and their relationship to territory. The first *combo* in *barrio* Luis Fanor Hernández came to the fore in late 2015 and was made up of 15 young women aged between 16 and 21 years old, not all of whom were from the neighborhood—although most had been to school together, albeit across different year groups—and who interacted mainly via social media (in particular Facebook) but would regularly meet in person “to party every Saturday” at neighborhood *fiestas* or nightclubs. At the latter, individual *comberas* would get into fights with other young women—who were often members of other *combos*—partly for reasons linked to *macho* pride (“I heard a girl dissing me, so I got angry and challenged her to a fight”) but most frequently over young men (“She was dancing with a guy I liked, so I went up to him and told him that he should be with me instead of her, and she got angry and challenged me to a fight”).

These fights were always one-on-one encounters and could be extremely violent, sometimes resulting in death—a 17-year-old girl from *barrio* Luis Fanor Hernández was killed during my one-month visit in 2016—although most of the time the fights stopped when one of the combatants was seriously injured, ran away, or asked for mercy. These conflicts were often prolonged on social media, however, as fights were often recorded and uploaded on Facebook, and the virtual platform was also used by rival *comberas* to trash talk to each other—both individually and collectively—about their fights, to throw out personal or group insults, or else to upload semi-naked photos of themselves on the Facebook walls of young men that they liked, telling them to dump their current girlfriend because “this is what you’re missing,” “I’m more beautiful than her,” “I’m a better fuck than her,” etc. Due to the semi-public nature of such communications, they would often lead to rival *comberas* challenging each other virtually, and then agreeing to meet and fight in person, mostly near nightclubs or at parties they both planned to attend, where audiences would egg the fighters on.

This new female iteration of the *barrio* Luis Fanor Hernández gang fundamentally challenged some of my long-standing notions about the relationship between gangs and gender in Nicaragua. It rapidly became clear during the course of interviews with youth in the neighborhood—including *comberas*¹¹—that the *combo*’s particular behavior patterns could be related to changing forms of policing, whereby poor neighborhoods, particularly in Managua, had come to be patrolled by a new, purposefully created police unit known as *Los Dantos*. These were heavily armed, motorized units, dressed in anonymous black uniforms, who from early 2015 began to engage in a targeted national campaign of intimidation against male urban youth, indiscriminately stopping and strip-searching, beating, and sometimes imprisoning both gang

members and non-gang members. In *barrio* Luis Fanor Hernández, collective patrols of up to 20 motorcycles each with a driver and a passenger carrying a shotgun or an AK-47 would be a regular presence, often “corralling” male youth in public spaces before strip-searching them, confiscating money and mobile phones, arresting anybody with drugs, and then arbitrarily loading one or two individuals into pick-up trucks that would drive to the other side of Managua where they would be left naked, something profoundly humiliating in Nicaragua’s *machista* social context.

These new tactics clearly had a major impact on the territorial presence of young males in the *barrio*; indeed, their marked absence on the streets was striking in 2016 compared to my visits in 2012 and 2014. Several youths—both male and female—explained how this development had completely upset the local sexual economy. Previously, young men and young women had met each other and flirted in streets, before then pairing off to court semi-privately in the neighborhood parks, squares, and patios. By 2016, this had ceased almost entirely as a result of police repression. The main space for flirtation and courtship between young men and women in *barrio* Luis Fanor Hernández were a couple of local night clubs and the occasional neighborhood *fiesta* where the dynamics of social interaction were completely different: these are eminently collective, public, heated, loud, and hugely performative spaces, characterized by strutting, preening, and aggressive dance-offs, and so on. The sense of competition is extreme, and as one *combera* told me, “girls who have developed reputations for being good fighters have a better chance of getting the boys they want without being challenged.” Indeed, the whole logic of the *combo* seemed almost to revolve around “getting a man.” Certainly, young women tended to “retire” from their *combo* once they had done so—as another *combera* put it, “now that I’ve got my husband, I’ve distanced myself from the *combo* and don’t involve myself like I used to.”¹²

In other words, the new form of policing successfully stymied the spread of male gangs, both in *barrio* Luis Fanor Hernández and more widely, but had the (unintentional) consequence of leading to the unprecedented emergence of female gangs, for reasons that related to the particular spatialized nature of youth’s sexual economy in the neighborhood. This was not something I had expected at all, and it led to my profoundly rethinking how I thought about Nicaraguan gang dynamics, and their relationship to sex, gender, and territoriality. It highlights how it is important not to become too stultified in our interpretations and understandings, but that these need to be in constant evolution in the context of longitudinal ethnographic research. This is all the more critical in relation to violence, partly because it is a fundamentally “slippery” phenomenon that inherently challenges us, and we often react by trying to “tame” it conceptually (Taussig 1987: 241). At the same time, however, as jarring the empirical reality I encountered was in the context of my longitudinal research, partly because the explanation integrates certain elements that come close to being socio-biological in nature, an approach that I have always inherently rejected, I would likely not have arrived at this new interpretation without a further temporal development relating not to my empirical research but rather to my serendipitous reading of the academic literature about gangs.

One of the foundational references within the gang literature is Frederic Milton Thrasher’s *The Gang: A Study of 1,313 Gangs in Chicago* (1927), which as Greg Dimitriadis (2006: 351) has pointed out, remains “without question the starting point for gang research,” partly because it was the first organized empirical study of gangs. I read *The Gang* during my doctoral studies, indeed citing it in my thesis, but I have to admit that I perused it rather partially, and moreover only read an abridged version published in 1963. Since my original reading, I had not really engaged with the work beyond citing it in my writings in a symbolic manner to show that I had some notion of the historiography of gang research. Rather fortuitously, however, concurrent with my 2016 fieldwork, I became involved in a project to (re-)assess the contemporary

relevance of the famous Chicago School of Sociology that Thrasher was associated with (see Jones and Rodgers 2016, n.d.). In relation to this, I (finally) read the unabridged version of *The Gang*, which in particular included Thrasher's observations about the role that sex could play in structuring different subgroups within gangs, how different sexual practices were often spatialized, and how more generally, how the sexual economy of a gang fundamentally responded to broader environmental factors rather than the individual characteristics of gang members. This provided me with the critical elements on the basis of which to develop a coherent analysis of *combo* dynamics. This was however in many ways a very contingent event, insofar as my rereading *The Gang* was coincidental with my 2016 revisit, thereby highlighting how ethnographic (re)interpretation is also extremely subjective and situated, and how neither longitudinality nor the focus on violence are sufficient inducements by themselves, and there is also a serendipitous element to the consequences of temporality.

Conclusion

The particular temporality of longitudinal ethnographic research clearly imbues it with a number of epistemological perils and pitfalls. One of the most obvious concerns the dangers of seeing long-term research as cumulative and "fixed." All three of the vignettes that I have presented in this article highlight how epistemological understandings of ethnographic experiences within the context of longitudinal studies are often highly situational and time-bound, whether viewed from an empirical, representational, or interpretative perspective. This means that long-term research can be particularly prone to episodes of discomfiting disjuncture, something that is clearly magnified in relation to research on violence, due to the visceral and emotional nature of the phenomenon, which promotes a certain categorical imperative, whether empirically, representationally, or interpretatively. At the same time, however, it is precisely its subjectivity that imbues longitudinal ethnography with its power and insight, insofar as it inherently leads the ethnographer toward greater self-reflexivity and a questioning of their assumptions and overarching premises that can only be beneficial, especially in relation to the controversial topic of violence.

For example, as Ben Penglase (2019) has highlighted, the fact that the contexts within which we encounter violence or narratives of violence are generally not the same as those within which we construct our narratives of violence highlights well the potentially powerful impact that a longitudinal perspective can have on the ethnography of violence. He describes how on returning to his original fieldwork notes for a new writing project after finishing his book, *Living with Insecurity in a Brazilian Favela* (Penglase 2014), he realized how the latter had focused almost exclusively on spectacular instances of violence, when empirically, much of his time in the favela had in fact been characterized by boredom and a lack of violence. Although there is no doubt that violence was a major element of life in the favela, his writing had depicted it in a particular way, very much as a result of his having moved from a context where forms of exceptional violence did occur, albeit not on a constant basis, to one where they rarely occurred. In other words, his representational and interpretative epistemological framework had changed as a result of his situational displacement.

One way to potentially overcome such quandaries, and also to get to grips with the emically malleable nature of empirical reality exemplified by Bismarck's changing narrative about his drug dealing, is through more dialogic and collaborative forms of ethnography. As Jonathan Spencer (1989: 159) has suggested, "the moment of writing is a rather late stage for the inter-

preter to reveal his or her interpretations to the interpreted. Instead it would seem more fruitful to try to devise ways in which ethnographic subjects are actively involved in their own self-representation . . . This is not to insist on the subjects' ratification of a particular ethnography . . . but simply to suggest that they be included in the 'interpretative community' to which the text is addressed." Certainly, this is something that Paloma Gay y Blasco has explored in her research on Spanish Gitanos, including in particular in a fascinating article co-written with Liria de la Cruz Hernández, whom she describes as "a long term gitano informant and a close friend," and which insightfully chronicles their evolving relationship over more than 20 years and how it impacted on Paloma's research (Gay Blasco and de la Cruz Hernández 2012; see also Gay Blasco and de la Cruz Hernández 2020).

At the same time, however, the article also highlights how complicated it can potentially be to try to coherently include the contradictions, the blind alleys, or in other words, to describe the haphazard processes of discovery inherent to ethnographic research when trying to write longitudinally across a long-term time arc. Does one write chronologically? Does one focus on themes and look at them diachronically? Which temporal perspective—or perspectives?—does one adopt? Gay Blasco and de la Cruz Hernández's solution has been to structure their article as an informal conversation in the present about the past, organized thematically, which certainly works narratively, and allows for a very perceptive consideration of a range of critical issues pertaining to a longitudinal ethnographic engagement, but also arguably implies a linear, cumulative vision of time, "fitting" the ethnographic experience into a particular temporal epistemology. When seen from this perspective, what this pioneering work can ultimately be said to highlight as the greatest challenge for longitudinal ethnography is arguably the need to generate new forms of representation that allow for "differing forms of [temporal] consciousness [to] constantly commingle and interact" (Harootunian 2000: 105). In doing so, perhaps we will also find ways through which to talk about the phenomenon of violence in a way that is less categorical, less rigid, and reflects better both its "slippery" nature, but also the way it can powerfully impact people's lives in ways that change and evolve over time, thereby representing it more as a process than an event.

■ ACKNOWLEDGEMENTS

I thank four referees, Lidewyde Berckmoes, as well as participants at the workshop on "The Longitudinal Ethnography of Violence" held at the Netherlands Institute for the Study of Crime and Law Enforcement (NSCR), Amsterdam on 22–23 June 2017, for useful and constructive comments.

■ **DENNIS RODGERS** is Research Professor in Anthropology and Sociology at the Graduate Institute of International and Development Studies, Geneva, Switzerland. His research focuses principally on issues relating to the dynamics of conflict and violence in cities, with tangents on the historiography of urban theory and popular representations of development. In 2018, he was awarded an ERC Advanced Grant for a project on "Gangs, Gangsters, and Ganglands: Towards a Comparative Global Ethnography" (GANGS), which aims to systematically compare gang dynamics in Nicaragua, South Africa, and France. He previously held appointments at the Universities of Amsterdam, Glasgow, Manchester, and the

London School of Economics. ORCID iD: <https://orcid.org/0000-0001-8957-4796> | Email: dennis.rodgers@graduateinstitute.ch

NOTES

1. See also Thomas Beidelman (1973), Alex de Waal (1993), John Knight (1994), and Paul Spencer (1973).
2. This name is a pseudonym, as are the names of all the individuals mentioned in this article.
3. I did ask Bismarck whether he also feared that discussing his past violence and criminality might lead to his arrest and conviction, despite the multiple precautions I take to pseudonymize him and his life history, but this does not seem to be an issue for him (in fact, he often berates me for using a pseudonym when writing about him, because he “want[s] people to know who I am”).
4. This whole issue also raises critical questions regarding informed consent in the context of longitudinal ethnographic research. During the course of my research in Nicaragua, I have generally taken the approach that I needed to formally re-obtain (verbal) consent from Bismarck for his participation in my research every time I have interviewed him. In many ways, however, it is clear this became for a while something of an empty ritual, devoid of real significance, and the episode in 2012 precipitated my rethinking things. What happens if Bismarck were to withdraw his consent to being part of my study, does this withdrawing of consent apply retroactively? What about in relation to information that I have already published? There are no easy answers here, and the issue is further complicated by the existence of the strong affective relationships, both with Bismarck, as well as with his family—in more “structural, fictive kin terms, he is my “brother-in-law,” married to my “sister” Wanda, who is a member of the Gomez family who have hosted me every time I have gone to Nicaragua since 1996—which complicate these decisions (for both of us). The fact that some of them relate to a powerfully visceral and emotional phenomenon such as violence makes them all the more difficult, although many other longitudinal ethnographers will clearly also face such dilemmas, whether they work on violence or not.
5. In 2020, I carried out joint ethnographic research in *barrio* Luis Fanor Hernández with the anthropologist Steffen Jensen, in the context of a project comparing global gang dynamics (see The Graduate Institute Geneva, “Gangs, Gangsters, and Ganglands: Towards a Global Comparative Ethnography,” <https://graduateinstitute.ch/research-centres/centre-conflict-development-peacebuilding/gangs-gangsters-and-ganglands-towards>). Among our various activities, we interviewed Bismarck together, and he once again decided not to allude to his drug dealing. This was very obviously due to Steffen’s presence—even though I told Bismarck that Steffen had read all of my writings about his drug dealing—so we decided not to push him on the issue. He was subsequently happy to talk about his drug dealing past with me one-on-one, and his wife and daughter referred to Bismarck’s drug dealing several times when Steffen and I interviewed them separately (see Jensen and Rodgers 2020).
6. Subsequently published in modified form as “Joining the Gang and Becoming a *Broder*: The Violence of Ethnography in Contemporary Nicaragua” (Rodgers 2007).
7. At the person’s request, this name is a pseudonym.
8. This email exchange has been slightly edited for ease of reading.
9. Jonathan Spencer (1989).
10. Similarly, writing for different audiences—or across different disciplines—inevitably means tailoring both the substance and form of writings (on this issue, see Wolf 1992).
11. Somewhat surprising, I did not have any difficulty engaging with the new female *comberas*, despite age and gender differences. This was partly due to luck—a member of the Gomez family who host me married a former *combera*, and so she was happy to talk to me and also connect me with other *comberas*, past and present—but also because the longitudinal nature of my research means that I am well-known in *barrio* Luis Fanor Hernández, which combined with my previous local gang membership in the 1990s means that I enjoy a certain legitimacy, with new gang members having heard of me and being willing to talk with me as a result whenever I revisit the neighborhood.

12. Partly as a result of this membership turnover, there have been several different *combos* in *barrio* Luis Fanor Hernández since the initial iteration.

■ REFERENCES

- Abraham, Curtis. 1998. "Land of the Loveless." *New Scientist*, 28 February, 30–33.
- Agar, Michael. 1990. "Text and Fieldwork: Exploring the Excluded Middle." *Journal of Contemporary Ethnography* 19 (1): 73–88.
- Barth, Frederick. 1974. "On Responsibility and Humanity: Calling a Colleague to Account." *Current Anthropology* 15 (1): 99–102.
- Beidelman, Thomas O. 1973. "Review of Colin M. Turnbull, *The Mountain People*." *Africa* 43 (2): 170–171.
- Bourgois, Philippe. 1995. *In Search of Respect: Selling Crack in el Barrio*. Cambridge: Cambridge University Press.
- de Waal, Alex. 1993. "In the Disaster Zone." *Times Literary Supplement*, 16 July, 5.
- Dimitriadis, Greg. 2006. "The Situation Complex: Revisiting Frederic Thrasher's *The Gang: A Study of 1,313 Gangs in Chicago*." *Cultural Studies—Critical Methodologies* 6 (3): 335–353.
- Gay y Blasco, Paloma, and Liria de la Cruz Hernández. 2012. "Friendship, Anthropology." *Anthropology and Humanism* 37 (1): 1–14.
- Gay y Blasco, Paloma, and Liria de la Cruz Hernández. 2020. *Writing Friendship: A Reciprocal Ethnography*. New York: Palgrave.
- Geertz, Clifford. 1988. *Works and Lives: The Anthropologist as Author*. Stanford, CA: Stanford University Press.
- Grinker, Roy R. 2001. *In the Arms of Africa: The Life of Colin M. Turnbull*. Chicago: University of Chicago Press.
- Harootunian, Harry. 2000. *History's Disquiet: Modernity, Cultural Practice, and the Question of Everyday Life*. New York: Columbia University Press.
- Heine, Bernd. 1985. "The Mountain People: Some Notes on the Ik of North-Eastern Uganda." *Africa* 55 (1): 3–16.
- Jeffcutt, Paul. 1994. "From Interpretation to Representation in Organizational Analysis: Postmodernism, Ethnography and Organisational Symbolism." *Organization Studies* 15 (2): 241–274.
- Jensen, Steffen, and Dennis Rodgers. 2020. "Comparison through Collaboration: Dilemmas and Opportunities of Joint Ethnographic Research on Gangs in Nicaragua and South Africa." Unpublished manuscript.
- Jones, Gareth A., and Dennis Rodgers. 2016. "Anthropology and the City: Standing on the Shoulders of Giants?" *Etnofoor* 28 (2): 13–32.
- Jones, Gareth A., and Dennis Rodgers. 2019. "Ethnographies and/of Violence." *Ethnography* 20 (3): 297–319.
- Jones, Gareth A., and Dennis Rodgers, eds. n.d. *Standing on the Shoulders of Giants: The Enduring Relevance of the Chicago School of Sociology*. Manuscript in progress.
- Knight, John. 1994. "The Mountain People as Tribal Mirror." *Anthropology Today* 10 (6): 1–3.
- Koonings, Kees, Dirk Kruijt, and Dennis Rodgers, eds. 2019. *Ethnography as Risky Business: Field Research in Violent and Sensitive Contexts*. Lanham, MD: Lexington Books.
- Lancaster, Roger N. 1992. *Life Is Hard: Machismo, Danger, and the Intimacy of Power in Nicaragua*. Berkeley: University of California Press.
- Lareau, Annette. 2011. *Unequal Childhoods: Class, Race, and Family Life*, 2nd ed. Berkeley: University of California Press.
- Law, Alex, and Eric R. Lybeck, eds. 2015. *Sociological Amnesia: Cross-currents in Disciplinary History*. Farnham: Ashgate.

- Moeran, Brian. 2016. "The Craft of Editing: Anthropology's Prose and Qualms." In *The Anthropologist as Writer: Genres and Contexts in the Twenty-First Century*, ed. Helena Wulff, 60–72. New York: Berghahn Books.
- Montoya, Rosario. 2003. "House, Street, Collective: Revolutionary Geographies and Gender Transformation in Nicaragua, 1979–99." *Latin American Research Review* 38 (2): 61–93.
- Nordstrom, Carolyn, and JoAnn Martin. 1992. "The Culture of Conflict: Field Reality and Theory." In *The Paths to Domination, Resistance, and Terror*, ed. Carolyn Nordstrom and JoAnn Martin, 3–17. Berkeley: University of California Press.
- Penglase, R. Ben. 2019. "Tubarão and Seu Lázaro's Dog: Spectacular and Banal Violence in a Brazilian Favela." *Ethnography* 20 (3): 397–416.
- Penglase, R. Ben. 2014. *Living with Insecurity in a Brazilian Favela: Urban Violence and Daily Life*. New Brunswick, NJ: Rutgers University Press.
- Poole, Deborah. 1994. "Anthropological Perspectives on Violence and Culture—A View from the Peruvian High Provinces." In *Unruly Order: Violence, Power, and Cultural Identity in the High Provinces of Southern Peru*, ed. Deborah Poole, 1–30. Boulder, CO: Westview Press.
- Rodgers, Dennis. 2000. *Living in the Shadow of Death: Violence, Pandillas, and Social Disintegration in Contemporary Urban Nicaragua*. Ph.D. dissertation. Cambridge: University of Cambridge.
- Rodgers, Dennis. 2001. *Making Danger a Calling: Anthropology, Violence, and the Dilemmas of Participant Observation*. LSE-DESTIN Crisis States Programme working paper no. 6. London: Crisis States Programme.
- Rodgers, Dennis. 2006. "Living in the Shadow of Death: Gangs, Violence, and Social Order in Urban Nicaragua, 1996–2002." *Journal of Latin American Studies* 38 (2): 267–292.
- Rodgers, Dennis. 2007. "Joining the Gang and Becoming a Broder: The Violence of Ethnography in Contemporary Nicaragua." *Bulletin of Latin American Research* 26 (4): 444–461.
- Rodgers, Dennis. 2015. "The Moral Economy of Murder: Violence, Death, and Social Order in Gangland Nicaragua." In *Violence at the Urban Margins*, ed. Javier Auyero, Philippe Bourgois, and Nancy Scheper-Hughes, 21–40. Oxford: Oxford University Press.
- Rodgers, Dennis. 2016. "Critique of Urban Violence: Bismarckian Transformations in Contemporary Nicaragua." *Theory, Culture, and Society* 33 (7–8): 85–109.
- Rodgers, Dennis. 2019. "From 'Broder' to 'Don': Methodological Reflections on Longitudinal Gang Research in Nicaragua." In *Ethnography as Risky Business: Field Research in Violent and Sensitive Contexts*, ed. Kees Koonings, Dirk Kruijt, and Dennis Rodgers, 123–137. Lanham, MD: Lexington Books.
- Scheper-Hughes, Nancy. 1992. *Death without Weeping: The Violence of Everyday Life in Brazil*. Berkeley: University of California Press.
- Spencer, Jonathan. 1989. "Anthropology as a Kind of Writing." *Man (N. S.)* 24 (1): 145–164.
- Spencer, Paul. 1973. "Review of Colin M. Turnbull, *The Mountain People*." *Man (N. S.)* 8 (4): 651–652.
- Taussig, Michael. 1987. *Shamanism, Colonialism, and the Wild Man: A Study in Terror and Healing*. Chicago: University of Chicago Press.
- Thrasher, Frederic M. 1927. *The Gang: A Study of 1,313 Gangs in Chicago*. Chicago: University of Chicago Press.
- Turnbull, Colin M. [1972] 1994. *The Mountain People*. London: Pimlico.
- Van Maanen, John. 1988. *Tales of the Field*. Chicago: University of Chicago Press.
- Whyte, William F. [1943] 1993. *Street Corner Society: The Structure of an Italian Slum*, 4th ed. Chicago: University of Chicago Press.
- Wilson, Peter J., Grant McCall, W. R. Geddes, A. K. Mark, John E. Pfeiffer, and James B. Boskey. 1975. "More Thoughts on the Ik and Anthropology." *Current Anthropology* 16 (3): 343–354.
- Wolf, Margery. 1992. *A Thrice-Told Tale: Feminism, Postmodernism, and Ethnographic Responsibility*. Palo Alto, CA: Stanford University Press.