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Collaborating with the Radical Right

Scholar-Informant Solidarity and the Case for an Immoral Anthropology

by Benjamin R. Teitelbaum

This article investigates the moral content and epistemological utility of scholar-informant solidarity in ethnography. It supports efforts to highlight the potential for immoral outcomes when ethnographers prioritize the interests of those they study during the conception, execution, and dissemination of their work. However, this article advocates reinforcing the imperative of scholar-informant solidarity, recognizing the practice as morally compromised but epistemologically indispensable. I illustrate these claims by referencing my experiences as an ethnographer of white nationalist groups in the Nordic countries. In three case studies, I show how solidarity—and with it collaboration, reciprocity, and advocacy—led to troubling contributions to political causes while offering ethnographic knowledge unlikely to be gained through other forms of research. The article argues further that, while the moral and epistemic consequences of solidarity are exposed in the study of oppressive and violent groups, the potential for power asymmetries and political conflict among scholars and informants is ubiquitous. Therefore, the article addresses the need to embrace solidarity and the immorality that comes with it to ethnographers at large.

Magnus Söderman is the host of a white identity podcast called *Svegot* and a former spokesperson for the militant Nordic Resistance Movement. "Saga" is the stage name of a woman often regarded as the foremost white power singer in the world. Daniel Friberg is—together with Richard Spencer—CEO of AltRight.com and a founder of the online ultraconservative reference page Metapedia. John Morgan cofounded the antiliberal, antimodernist publishing house Arktos and is a writer for the white nationalist blog *Counter Currents*. Erik Almqvist is a former member of Parliament for the anti-immigrant Sweden Democrats party, forced to resign after video surfaced of his shouting racial slurs in public and threatening a man with an iron bar.

No two of these individuals think alike, and some identify each other as their foremost adversaries. But all fight liberalism, multiculturalism, and immigration to protect the ostensible purity of majority white populations throughout the West. They go by many names: outsiders describe them as right-wing extremists, organized racists, or neofascists, and they tend to call themselves nationalists. I call them friends. I have been conducting ethnographic fieldwork among radical nationalists in the Nordic countries and beyond since 2010. I have observed their public demonstrations and private meet-

ings; attended their concerts; laughed, drank, dined, and lived with them. My aim has been to cultivate close long-term relationships with nationalists fed by honesty, personal exchange, and trust. Friendships were both preconditions and by-products of such contact, as were instances of collaboration, reciprocity, even advocacy.

All of that may seem unremarkable. Anthropologists have often treated scholar-informant solidarity and its elaboration as an ideal—as our bedrock "political stance" (Heyman 2010: 289), the "ethical and moral commitment [that] transcends all else" (Lassiter 2005:91), and the content of our social capital (Kulick 2006). In doing so, they extend a charge to prioritize the interests of research participants that is all but mandated throughout the human sciences by institutional review (IRB) regimes. But the moral virtue of collaboration, reciprocity, and advocacy fades in the study of oppressive or privileged populations. In such cases, showing solidarity with those we study may make us accomplices to acts of symbolic or real violence. This is the primary reason anthropologists have been weakening their once-uncompromising imperative to prioritize the interests of research participants. As seen in recent changes to the AAA (American Anthropological Association) Statement on Ethics,[1] the field is striving to make space for researchers who want to work in open opposition to those they study, suggesting in the process that scholar-informant solidarity is an inessential element of our practice. And such changes

Benjamin Teitelbaum is Assistant Professor in the College of Music at the University of Colorado, Boulder (Imig Music Building, 301 UCB, Boulder, Colorado 80309, USA [benjamin.teitelbaum@colorado .edu]).

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1. I discuss these changes at greater length below.

would make sense, were study of the unjust genuinely exceptional and were embrace of collaboration, reciprocity, or advocacy only a matter of promoting social good.

In this article, I defend scholar-informant solidarity in ethnography as morally volatile and epistemologically indispensable. Seldom can we campaign against the people we study while collaborating and engaging with them personally, yet it is through exchange and partnership that we gain our signature claims to knowledge. Research eschewing these practices cannot be equated with ethnography as it has come to be understood and valued; it is instead a ghost of ethnography's past, one subject to aged criticisms of scholars' capacity to represent and interpret the lives of others and one that perilously treats itself as a foundation for informed activism. I offer a defense of scholar-informant solidarity while sharing in emerging assessments of its moral content. It should be championed, not because of its promise to promote good but despite its potential to do bad and spur conflicts for all involved. This affirmation of our need to prioritize commitment to the people we study also amounts to a critique of moralism in our field. However, I do not seek to refetishize objectivity or dispassion for the implications of our work or necessarily to once again highlight the ways relationships formed in ethnography fail to embody egalitarian ideals. Instead, I claim that ethnography's tortured human drama is both its inescapable liability and its foundational resource. So long as we prefer dialogic and intersubjective models of understanding to those of observation and monologue, we are led to embrace a research practice laced with political and moral compromise.

I will make this case referring to my experiences as an ethnographer. The partnerships and interpersonal sympathies I formed with Nordic radical nationalists during fieldwork resulted in predictably unsettling outcomes, most notably scholarship and commentary that could have aided my informants' cause. Yet the affectionate reciprocal relationships that underpin my actions also exposed me to key information. To reject them would have been to reject urgent insight into movements that many discuss but few understand.[2] My approach is hardly standard. The handful of scholars who have conducted ethnographies of the far right—such as Blee (2002), Fangen (1999), Holmes (2010), Shoshan (2016), or Simi and Futrell (2010)—seldom attained (or at least disclosed) extensive sympathetic and collaborative relationships between themselves and those they study. Instead, most of those scholars strive toward a position as neutral observers rarely regarded as an ideal among other ethnographers today. And these studies, too, responsible for some of the richest information about the far right available, are a minority. Scholars of far-right populations typically identify topics, conduct analyses, and write with the explicit goal of subverting the people they study. Predictably, contact of any kind between researchers and insiders is rare (Blee 2007; Goodwin 2006), while some engage in dialogue or follow in-

sider discourse through covert infiltration and deception (e.g., Wåg 2010)—a solution that may offer a scholar data but that undermines possibilities for long-term collaboration and exchange.

Say what one will about virtue and social good, but why would we trust the knowledge produced under these circumstances, especially when decades of criticism and reflection on anthropology's methods compel us to think otherwise? Questions like that prompted my interest. But I refer to my case study and subfield because it exposes what I believe are issues of wider relevance, for while the moral and epistemological conflicts in my experience are raw, they are not uncommon. The staid impression of anthropologists as scholars of the disenfranchised and oppressed—those with whom solidarity appears morally and politically unproblematic (Asad 1973; Owton and Allen-Collinson 2014; Wolf and Jorgensen 1970)—is a mischaracterization. Studying up is no longer the novelty that once motivated Nader's (1972) appeal. And even research that on the surface conforms to the relationships of the Malinowskian archetype often masks dynamisms of agency and sympathy at the interpersonal level, something suggested by the preponderance of women in cultural anthropology doctoral programs (Philips 2010), as well as by the growing presence and mobilization of non-Western ethnographers (Ribeiro 2014; Ribeiro and Escobar 2006). Anthropologists are thus likely to find themselves studying individuals of relative structural privilege whose personal or political profiles clash with their own. Examples and advice as to how one might navigate these situations abound. Politically compromising research can simply be avoided (e.g., Stoczkowski 2008), the powerful can be denied our transparency and honesty (Pillay 1994), we can forge solidarity with informants during fieldwork only to break it while writing (Castañeda 2006:139), or we can limit our inquiries so as to pursue an anthropology of, rather than for, morality (Fassin 2008; Zigon 2010). Such approaches not only confine appreciably the range of peoples we can study and the questions we can investigate but also imply further that political and moral disagreements in the field are exceptional mishaps. In contrast, they can be ideal for our epistemological model, for they establish conditions for intellectual and social collaboration—for "work across difference" as Anna Tsing (2015:29) writes.

All of that leads me to generalize conclusions from my own situation. If scholar-informant solidarity is vital to our preferred research method, if its practice in the study of oppressive and privileged people stands to perpetuate injustice, and if study of such people constitutes a sizable and crucial portion of anthropological inquiry, then immorality imbues our enterprise. The field therefore ought not pretend to advance good when that reputation is irreconcilable with its methods and topics. Ethnography today entails being a collaborator of both the good sort and the bad sort.

The core of the following article features three short case studies from my fieldwork experiences involving self-identifying National Socialist Magnus Söderman, a white na-

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2. For a recent example of a call for more ethnographic research into far-right groups, see Bangstad (2017).

tionalist journalist I call Mr. X, and white power singer Saga. I choose these examples because they reveal morally fraught forms of reciprocity, collaboration, and advocacy. At the same time, I argue that my actions in each instance either advanced or emanated from essential features of ethnography. Before analyzing those exchanges, however, I will first outline the debates and discussions contextualizing my broader argument—those surrounding the shifting moral, ethical, and epistemological investment in showing solidarity with those we study.

Dyadic Imperatives

Anthropologists must weigh competing ethical obligations to research participants, students, professional colleagues, employers and funders, among others, while recognizing that obligations to research participants are usually primary.[3]

"Usually." By introducing that qualifier to their statement on ethics via a 2012 revision, the AAA broke with more than 40 years of precedent. The original statement, the 1971 Principles of Professional Engagement, proclaimed unequivocally, "In research, anthropologists' paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first."[4] This encompassing call to prioritize the interests of participants further aligned anthropologists with emerging standards for human research at Western universities. It also endured throughout multiple rewrites, the only notable changes occurring when the term "paramount" was altered to read "first" in 1990 and then "primary" in 1998 (Fluehr-Lobban 2003:227)—the same year the statement began declaring explicitly that commitments to research participants also superseded pursuit of knowledge.

The steadfast messaging on this principle in AAA statements on ethics and beyond concealed a long-standing unrest among anthropologists, however. Whether targeting the ethical codes of academic societies or the totalizing imperatives of an IRB regime designed more for medical research than for cultural anthropology (Bell 2014; Blee and Currier 2011; Hoeyer, Dahlager, and Lynöe 2005; Marshall 2003), critics argued that deference to privileged or oppressive research participants could lead to morally wanton outcomes (Daly and Mills 1993; Lederman 2013; Tedlock 2000). Arguments like these inspired changes to the AAA statement in 2012: both its new affirmation of conditional rather than obligatory prioritization of those studied and corresponding revisions that subordinated pursuit of knowledge to a nonspecific "do no harm" charge now gutted of its previous focus on harm to research

participants.[5] As members of the 2012 revision committee would later explain, they came to regard the primacy of responsibility to informants "a cherished anthropological value rather than an actual principle of ethical practice," adding that "anthropologists 'studying up,' studying those in power, do not owe a greater ethical obligation to powerful individuals than to those vulnerable to that power" (quoted in Plemmons and Barker 2016:61).

Changes to the AAA statement revealed that anthropologists had been attempting to embrace two potentially conflicting commitments: one, codified in past AAA statements and prevailing institutional review procedures, to show solidarity with the people they study, and the other, energized by critical anthropology, to counteract exploitation and injustice at large. We can better understand their structural differences if we think of them as an ethic and a morality, respectively. While those terms are often used synonymously (e.g., Edel and Edel 1959; Howell 1997), one tradition of modern English casts ethics as an explicit, formalized system for assessing relativistic and situational virtue and morality as an informal system based on what are perceived to be transcendent values (Castañeda 2006; Heintz 2009). Context dependency allows ethical systems to be more detailed and thereby better suited to legalistic implementation and scrutiny, hence the existence of ethical "codes," "violations," "transgressions," and so on. Moralities are comparatively general in their content, expression, and application and tend not to acquire official status. The AAA's consideration of commitments to research participants appears, according to this typology, appropriately labeled as one of ethics.[6] Not only have its conclusions been expressed through official guidelines for conduct but also its imperatives address the bounded sphere of the fieldwork encounter. In contrast, the call to advance social good is moralistic, given that it is often conveyed in general, universal terms and in less formal discursive venues rather than official codes or guidelines. Distinctions between ethics and morality can dissolve when we look past contexts and styles of expression. Likewise when the two are seen as working together, when—as is the goal for philosophers such as Charles Taylor (1989:3–4)—an ethics becomes the practical implementation of a morality. Alternately, the differences sharpen when they are seen as working at cross purposes.

The "cherished" ideal of anthropology was indeed that our morality and ethics would align, that by prioritizing the interests of research participants, we would be advancing justice in the same instance and vice versa. The 2012 changes to the AAA's ethics statement thus grapple with the potential for an ethical/moral mismatch, for instances when showing solidarity with informants leads to immoral outcomes or when

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3. http://ethics.aaanet.org/ethics-statement-4-weigh-competing-ethical-obligations-due-collaborators-and-affected-parties/ (accessed September 13, 2016).

4. https://www.americananthro.org/ParticipateAndAdvocate/Content.aspx?ItemNumber=1656.

5. http://s3.amazonaws.com/rdcms-aaa/files/production/public/FileDownloads/pdfs/issues/policy-advocacy/upload/ethicscode.pdf (accessed January 9, 2018).

6. Heyman (2010) calls solidarity with participants the "ethics of the ethnographer" (289).

moral actions violate ethical commitments to informants. Considering such mismatches, the authors of the revisions to the AAA statement express a clear preference. They encourage scholars to cast aside situational commitments in order to pursue the greater good. Our morality, apparently, trumps our ethics.

We ought not regard this solution as predestined or obvious, if only because the association between practices of scholar-informant solidarity and morality (in addition to ethics) has been both widespread and decades in the making. Post-WWII reforms to human research in North American and Europe not only established the basis for institutional review processes centered on protecting the welfare of research participants but also responded to perceived immoralities of the past—specifically, research that misled and harmed its subjects. The influential 1978 *Belmont Report* in the United States, for example, stressed informed consent, the reduction of risk, beneficence, and justice in research—all principles directed toward research participants (Plattner 2003; Shamoo and Resnick 2009). But moralistic investment in the prioritization of participants also emerged from concerns more localized to anthropology. Peter Pels (1999) traces this association to our field's criticism of its past role as accessory to colonialist regimes, criticism linking immorality in research to allegiances with nonacademic sponsors. The notion that these unseen third hands in the ethnographic encounter were sources of moral corruption formed a conceptual framework for regarding an exclusive commitment to the studied as the pathway toward a more socially conscious practice. As Pels (1999) put it, "This radical ethics of representation not only excluded administrators and missionaries from the epistemological relationship between anthropologist and people studied but also excluded them morally by defining them as a threat to the latter" (110). In part via the drafting of a code of ethics in 1971, the AAA sought to help anthropology gain a moral compass and to do that by championing a dyadic model focused on solidarity and trust between ethnographer and those studied. Immorality seems to flank that relationship on both ends. It emerges when third parties (missionaries, private interests, and governmental agencies) enter the relationship or when two actors reduce to one: when ethnographers act as self-interested individuals pursuing research for their own exclusive gain, such as the free-enterprise-based "Reaganethics" Gerald Berreman (2003) claimed to witness in the late-twentieth century.

The ideal of dyadic commitment and solidarity in ethnography is most often contemplated in the many methodologies in which it is manifest and extended. Among these, collaborative ethnography and the practice of reciprocity are the most theorized. But the category came to include more radical approaches such as engaged anthropology or activist research, the latter term defined by Charles Hale (2006) as "a method through which we affirm a political alignment with an organized group of people in struggle and allow dialogue with them to shape each phase of the process, from conception of the

research topic to data collection to verification and dissemination of the results" (97; see also Low 2009:390). A lesser-known strategy codifies scholar-informant solidarity in interpersonal relations through the "friendship model" of ethnographic research. Advocates of this approach conceive of friendship in terms of both forming sympathetic and affectionate relationships with research participants and using friendship as a metaphor for a more harmonious type of interaction in the field—an ethnography whose primary procedures are "conversation, everyday involvement, compassion, giving, and vulnerability" (Tillmann-Healy 2003:734; see also Code 1991; Cooley 2003; Hellier-Tinoco 2003; Pelto and Pelto 1973; Titon 1992, 2008).[7]

Like the principle of dyadic solidarity at large, these methods have also been defined and defended based on their moral impact. Luke Lassiter (2005), for example, declines to label instances of scholar-informant cooperation in early Bureau of American Ethnology texts as genuine collaborative ethnography on the grounds that these partnerships were not put to the task of "critiquing Western society and culture." As he would later specify, "collaborative ethnography is first and foremost an ethical and moral enterprise, and subsequently a political one" (Lassiter 2005:37, 79).[8] Charles Hale (2008) claims that there is an "elective affinity" between the method of activist research and a political profile rooted in "a shared commitment to basic principles of social justice that is attentive to inequalities of race, gender, class and sexuality and aligned with struggles to confront and eliminate them" (7–8). This makes an activist research of the political right, in his mind, "unlikely."[9] Lisa Tillmann-Healy (2003) similarly prioritizes ethical and moral defenses of friendship-as-metaphor for field relationships, writing, "Perhaps the most important aspect of this methodology is that we research with an ethic of friendship, a stance of hope, caring, justice, even love" (735).

Because practices of scholar-informant solidarity have so often been characterized and justified as tools for advancing good, they are implicitly targeted in critiques against moralism in anthropology—critiques alleging that the pursuit of moral virtues can conflict with the pursuit of knowledge (e.g., D'Andrade 1995; Nyamnjoh 2015; Stoczkowski 2008). Surprising, then, that rather than Reaganethics or visions of a renewed scientific objectivity, moralism itself has weakened the dyadic model of ethical obligations in anthropological re-

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7. The friendship model of ethnographic research is often invoked with reference to the feminist-inspired ethics of care (Gilligan 1982; Ryan 1995; Sichel 1991).

8. Ethics is often described as the primary drive behind scholar-informant collaboration, whereas epistemological agendas are often foregrounded in defenses of collaborative research among scholars (White 2012:88).

9. Other definitions of activist research describe the method as starting with the will to address a social problem rather than partnering with research participants per se (see Calhoun 2008:xvii)—it is applied research, with the added feature that the problems it addresses are injustice and oppression.

search. After all, it was moralist criticisms of scholar-informant solidarity that motivated revisions to the AAA statement on ethics in 2012.[10] But those dismissing the imperative of scholar-informant solidarity—whether they speak from scientific objectivism or critical anthropology—fail to realize the methodological vacuum in which ostensibly liberated scholars find themselves.

Loving and Knowing

The drive toward increased solidarity with participants in fieldwork stems not only from critical anthropology but also from the crisis of representation. As David Mills (2003), George Stocking (1983), and Bob White (2012) note, efforts during the 1970s and 1980s to break down hierarchical relationships between scholars and informants relied on discussions about the epistemological limitations of the scientific method. Weems (2006), following Magolda (2000), points out that even reciprocity as a theorized concept in ethnography was initially "linked to the potential of ethnographic research to generate 'more accurate' or 'truthful' understanding by closing the distance of interpretation and identification between the researcher and the researched" (997). Latter-day scholars continue to value such practices for their epistemological import. Douglas Holmes and George Marcus (2008) regard collaboration's potential to enhance the accuracy of research as its central justification.[11] Nancy Scheper-Hughes's (1995) passionate call for moralism contained an often overlooked claim that the more she engaged politically on behalf of the Brazilian women she studied, the more her "understandings of the community were enriched" and her "theoretical horizons were expanded" (411). Charles Hale (2006, 2008) likewise asserts that activist research leads to "privileged insights"; acts of political solidarity increase rapport and access while encouraging those we study to invest in our research. And as George Lipsitz (2008) argues, activist research often entails a move to "expose and challenge the epistemological and ideological underpinnings of contemporary science, law, medicine, urban planning, and business," which can lead to "new ways of knowing" (91). Similar claims underpin epistemologically based defenses of friendship in the field. Nicole Beaudry (2008:244), for example, argues that following the

friendship model increases chances that scholars will receive invaluable insider evaluation of their writing, while others assert that basing field relationships on friendship encourages multifaceted exposure to the people we study and the lives they live. Lisa Tillmann-Healy writes,

> Friendship as method can bring us to a level of understanding and depth of experience we may be unable to reach using only traditional methods. In my project, by studying . . . literatures, I learn about my participants historically and politically; by observing their interactions, I get to know them interpersonally and culturally; by giving them my compassion and devotion, I experience them emotionally and spiritually. (Tillmann-Healy 2003:737; see also Coffey 1999; Owton and Allen-Collinson 2014)[12]

Advocates for friendship in a literal rather than metaphorical sense also justify their approach as a tool for increasing knowledge. Frank Salamone (1991) channels this thinking when he writes, "Each field experience is successful only if an anthropologist can find a friend or friends in the field who help make sense of the kaleidoscope of stimuli that assault the senses of any field anthropologist" (67).

While it is a means to establish partnership and closeness, scholar-informant solidarity also functions as a measure of preexisting rapport and mutual investment in a research project. Luke Lassiter (2005) and Douglas Holmes and George Marcus (2008) are justified in asserting that collaborative ethnography, as they define it, requires conscious implementation. But to the extent that the impulse to collaboration and other forms of partnership speaks to a yearning for closeness and contact with those we study, it may also emerge as a by-product of the fieldwork experience. As we spend time studying and coming to know a set of people, opportunities to feel empathy and affection toward them grow. Indeed, those instincts are likely to intensify as research becomes more extensive and penetrating. Lassiter (2005), like Barbara Tedlock (2000) before him, recognized that the dialogue born of collaborative ethnography can inspire further alignment—that while it "may generate the exchange of knowledge and meaning, it may also deepen commitment, friendship, and mutual moral responsibility" (12). Robert Kemper and Anya Royce (2002) theorize this process most explicitly when they propose a scale tracing common relationships in long-term fieldwork that progresses from detached observation, to partnership, to advocacy or friendship (see also Kirsch 2002:178; Taylor 2011). And given that it is during these latter stages when scholars are best positioned to access rich detail about their informants' lives, not to mention earnest cooperation and dialogue, we might be tempted to think of Kemper's and Royce's scale as describing a progression from a protoethnography to ethnography proper.

10. Put in other terms, the decision was motivated by concerns about an inability to advance a particular morality, in contrast with charges that ethnography cannot fulfill the terms of its own ethics of emancipatory egalitarian ideals between scholars and informants (Stacey 1988; Visweswaran 1994).

11. Ethnomusicologists likewise defend collaboration in mutual music making on account of its ability to reveal information. Writing in the landmark text *Shadows in the Field*, Rice and Titon outline a "new fieldwork" focused not on "observing and collecting" but "experiencing and understanding" (Titon 2008). Here, mutual music making creates a field of "experience"—a necessarily shared one where distinctions between researcher and subject become more ambiguous (see also Rice 2008).

12. Note also Newton's (1993) defense of acknowledging (though not seeking out) sexual relationships with informants and describing the ways that those relationships can be productive from a research perspective.

While few champions of collaboration, reciprocity, activism, or friendship claim that their strategy represents the only pathway toward deepened understanding—Hale (2006:109) dismisses that notion explicitly—it is hard to conceive of other approaches that so effectively nourish avenues toward intersubjective insights. The challenge facing researchers of the oppressive or privileged is therefore more daunting than critics of the dyadic instinct in anthropology imply. Were scholars to conduct research in opposition to those they study, they would likely compromise their ability to adopt some of the most effective research techniques in the same instance. Even heathy skepticism of a for-or-against dualism and a goal to remain neutral might not escape a certain inflexibility: our access to dialogic knowledge could well exist in equal measure with our ability to form and sustain exchange and partnership—behaviors that forge and derive from solidarity. Put another way, while striving to maintain an oppositional or even dispassionate posture may seem attractive, good ethnographic practice will likely impede it.

Solidarity with Abandon

I embraced practices of solidarity as epistemologically useful, socially intuitive, and morally volatile, separating the techniques from the values and mission of critical anthropology. If our past ethics and morality worked at cross purposes, I—in contrast with the guidelines given in the AAA statement—sided with the former. The approach adopts certain claims of past critics of moralism, observing the potential for irreconcilable oppositions between moral agendas and a search for truth. However, it also embraces reflexivity and intersubjectivity as virtues worth prioritizing. Rejecting both calls to do good in research and those for a disinterested science, my approach was a method, ethic, and immorality born of a radical devotion to the people studied and the social, moral, and intellectual entanglements that come with it.

This was not my original plan, however. When I began studying Nordic radical nationalists in fall 2010, I intended to conduct fieldwork as a neutral, dispassionate observer. My intention was to interact with insiders via formal interviews and monitor from a distance their public gatherings and demonstrations. Scholar-informant partnerships, reciprocity, and friendship were to be no part of my research. It even crossed my mind that my work might someday serve as a small protest against ethical dogmas that appeared to me premised on a loathsome obligation to love. Considering the potential for solidarity to be feigned for the sake of wooing research participants or impressing academic colleagues—as Van der Geest (2015) writes, "use of the term 'friend' in anthropological writings is often an indirect claim of successful research" (4; see also Foster 1979:180)—aspiring toward dispassion and neutrality during the opening stages of research still seems sound to me today. However, my efforts to remain dispassionate proved unsustainable as fieldwork advanced. Personal solidarity and empathy grew as frequent interviewees and I

became interested in each other as people. The appeal of distance and holding a monopoly on interpretation faded as I was exposed to the dynamic and complicated nature of their activisms. And their participation and engagement increased as they sensed a new willingness on my part to understand their lives. Within a year of beginning fieldwork, I started to study this remarkable population through what, in the broader context of ethnographic research, were unremarkable techniques. I was aligning with them as a scholar and a person, and my work grew more penetrating, informed, and sinister in the process.

While a drive toward solidarity figured into many aspects of my work, here I present a sampling of instances that reveal its epistemic and moral consequences. Reciprocity, for example, was in one instance necessary for an informant to participate in my study. I first contacted this individual in 2011 when he was a journalist for the far-right, ethnonationalist, and lightly anti-Semitic newspaper *National Today* (*Nationell Idag*).[13] He was skeptical of my interest in him and his activism from the start. He agreed to be interviewed on one condition: that he be allowed to interview and photograph me for an article in his paper. I was initially hesitant to grant his request. Knowing well how *Nationell Idag* had profiled other nonnationalists—especially those with Jewish backgrounds—I assumed that an article about me would be defamatory, unpleasant, and an impediment in my efforts to expand a network of informants. I nonetheless found myself struggling to rationalize declining his request. After all, he held similar fears about participating in my research. As is standard, he had kept his politics and activism hidden from friends, family, and colleagues, and he was worried that something I published might expose him, causing considerable personal and professional harm. Assurances from my side could do only so much. Confidence in mainstream scholars and journalists among nationalists is low.[14] It seemed only reasonable for me to share in the benefits and risks of our exchange, and so I agreed to his requests. However, I asked him to wait to publish any piece until I had completed my fieldwork, just as publication of my data would also be delayed.

By seeking these concessions from me, this nationalist managed to weaken the power imbalance he would normally face as a subject of an ethnography. As we began recording our interviews, we each possessed the ability to harm each other. While the cold nature of our negotiations initially made our interviews stiff and uncomfortable, they grew to be some of the richest I experienced during fieldwork. The mutual threat we posed to each other provided him assurance of fair treatment that I could not have marshaled through

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13. This paper is the forbearer of today's *New Times* (*Nya Tider*).

14. Given that scholars and journalists have openly embraced practices of deception when studying nationalists, I think complaints against that approach are valid, even though nationalists often exaggerate and instrumentalize these complaints for political purposes.

words alone, and this allowed him to converse with me in ways he might otherwise have not. As the tensions between us eased, I came to feel less concerned by the prospect of appearing in his paper—at least as regards my own treatment. I also found him to be a skilled photographer, and I bought one photo he took of me to use professionally. However, I was bothered by the notion that being interviewed as an American doctoral student might lend the paper additional legitimacy, and I had no solution to this concern.[15]

In this case, a willingness to reciprocate allowed me to expand my network of informants and improve the quality of my interviews. In other instances, the research benefits of reciprocity were less obvious and emerged as an outcome of preexisting relationships with insiders. My interactions with a man named Magnus Söderman offer one example. A former member of the militant Nordic Resistance Movement, a self-identifying National Socialist, and one of 89 Europeans blacklisted from entering Russia in 2015 likely on account of his support for far-right Ukrainian separatists, Magnus appears in many ways a typical fringe nationalist. His head is shaved, his body is tattooed with Third Reich imagery, and he has a long history of violent confrontation with nonwhites and leftist activists in Sweden. However, and contrary to the hardened image he projects, I found him throughout interviews and informal exchanges to be an exceptionally curious person, witty and articulate, with a sense of irony and humility rare in nationalist circles. Despite the predictable differences between us, and despite his fixation on my Jewish heritage, we came to enjoy each other's company. The more time I spent with Magnus, the more dynamic, complicated, and likable I found him.

It was out of the friendly nature of our interactions that Magnus once asked a favor of me while I was on a fieldwork trip in 2014—namely, he wanted me to help him become a better writer. He was, at the time, in the midst of a move away from his skinhead past and toward a new, more upstanding activist persona. As part of that move, he had shed his ties to organized militancy, founded a think tank, initiated a weekly online radio program, and began writing novels. Knowing that I was a university instructor, he asked me to read and evaluate his first novel, *The Defiant One* (*Den trotsiga*, 2013) published by the largest active nationalist press in the Nordic countries, Logik. An allegorical treatment of "white genocide" narratives, the novel describes the life of a white Swedish teenage woman attending a secondary school dominated by Muslims and Africans and chronicles her struggles to gain racial pride and her

15. As chance (or luck) would have it, I would not appear in *Nationell Idag*. Shortly after we ended our interviews, a political conflict removed this individual and the entire sitting editorial staff at the paper. Further interpersonal struggles a year later led him to withdraw from activism almost entirely. Infighting of this kind has been the curse of Swedish far-right activism since World War II. As I see it, he still has permission to write and publish an article based on his interview with me if he so chooses.

tragic inability to avoid harassment and subjugation as whites become a new minority in Sweden. Magnus had not asked for feedback on the ideological and political messages in his book. Rather, he wanted me to critique his writing style. Declining to help him on the grounds that I assumed he would use my insights only for bad ends felt like a paranoid and insulting response. I therefore agreed and responded with criticism that focused exclusively on style and plot development. Reading and commenting was an easier task than one might imagine; with nearly four years of fieldwork behind me, I hardly ever found myself surprised or shocked by nationalist rhetoric. Magnus later thanked me for the feedback, adding, however, that he was a poor student and would likely disappoint me with his next piece of writing.

All presumptions about the powers of my criticism aside, I could be accused of helping to enhance nationalist messaging by assisting Magnus in this task. Though I commented on a text that was already completed and in print, I occasionally felt as though I was serving as a volunteer editor for forthcoming publications. Magnus's subsequent books were indeed stronger pieces of writing than his first novel and appear to have incorporated some of the suggestions I made to him earlier. Accordingly, the episode stands out in my mind as the closest I have ever come to participating in the production of nationalist political expression.

Practicing reciprocity with Magnus hardly left me with a sense of smug self-satisfaction. But did the episode at least improve my research? It might have strengthened our collaboration, sending him a signal that I was unafraid to engage with him closely and that he in turn could approach me with the same openness. But though to this day he has been a reliable source for information and scrutiny about race revolutionary activities and culture in Europe, I cannot link that with my having done him a favor. Rather than being an impetus to future interaction, the significance of my reciprocation with Magnus lies more in what it responded to. He felt comfortable making his request and I felt uncomfortable declining it because of the rapport we shared with one another. Instances like these—troubling as they were—served for me as a barometer of my general success in fieldwork.

In addition to collaboration and reciprocity, advocacy also figured into my research. Though I never felt driven to serve as an apologist for nationalists' political ideology, I have taken to popular press outlets with the aim of defending their dignity as people and criticizing aspects of their reception in the mainstream. One instance came in fall 2011 when I wrote articles in defense of one of my closest informants, Swedish white power singer Saga. She had recently gained international notoriety when it was discovered that Norwegian terrorist Anders Behring Breivik, who had murdered 77 people, mostly children, in the Oslo area that summer, regarded her as his chief musical inspiration. Saga responded on her website and in radio interviews by denouncing the killings, expressing sympathy with those harmed, and distancing herself from Breivik. However, Swedish media questioned whether her expressions

were genuine. One magazine even asserted that social pressure "forced" her to make some kind of public condemnation (Bergroth 2011). Criticisms of Saga's music and political activism aside,[16] I found the claim that she would feel indifference toward the mass murder of children offensive and unfounded. The allegations struck me as one of many ways in which nationalists' status as the ultimate political pariahs exposes them to boundless allegations from critics who are seldom held to account. I therefore wrote letters, some of which were published, criticizing the coverage of Saga and encouraging readers not to dismiss offhand the capacity for empathy even among their ideological opposites. I continued that line of argument years later in *New York Times* op-eds (Teitelbaum 2014, 2015), where I highlighted the growth of political violence directed toward nationalists in Sweden and its unfortunate and shocking acceptance by mainstream political leaders.

Defending nationalists—though not nationalism—has been my main undertaking as a public commentator. Throughout dozens of print media and radio statements in addition to those just mentioned, I focused on challenging prevailing reactions to the people I study. Never have I felt the urge to make a public statement condemning nationalists or their politics. That instinct stems from an assessment of my ability to contribute to public conversation. I consider myself qualified to comment on the dynamism and complexity of nationalists' lives, and I find such insight to be lacking in current media debates. In contrast, evaluations of nationalists' political claims and agendas not only are ubiquitous in public commentary but also often flow from talented pens. Yet my inclination to act on behalf of rather than in opposition to nationalists also relies on less tangible factors. I feel obliged to my informants because of our ongoing reciprocal exchanges of service as well as friendship. Inaccurate or misleading characterizations of them now anger me. I have formed emotional attachments to these people because of our extensive contact, and those relationships shape the ways and occasions in which I feel compelled to express myself.

That counts as the most troubling aspect of my story. If I once avoided taking a critical stance in the name of scholarly disinterest, I later repurposed and altered the approach as experiences in the field brought me closer to my informants. Regardless of what the AAA statement on ethics may now say, I regard nationalists' interests as my "paramount" concern. I grew chiefly interested with what they thought of my work and wanted to see them benefit in some way from participating in it. That aim shaped both my language and lines of inquiry. I notice as I read my own texts that I relish telling nationalists' life stories, am excited by the unexpected twists and turns of their expression and activism, and strive to find and communicate what is good about them. Even my decision to label

them nationalists and not fascists or extremists—though I conceived it as part of an effort to blunt stereotypes and stir curiosity—became an expression of solidarity. And when I do criticize them, it feels like a defeat, as though I failed an opportunity to defy expectations, to uncover deeper complexities, and to prompt new learning. In no way is it amusing or gratifying: it is to highlight and publicize the flaws of friends.

All of this prevents me from calling myself a neutral, apolitical, or amoral scholar. My writing is emotional and partial. I try to hide it; editors and reviewers see it nonetheless. I have few indications, for better or for worse, that my tone prevents readers from categorizing those I write about in familiar ways (confirmed every time I hear, "Wait—you're calling those Nazis 'nationalists'?!"). But the approach holds potential to normalize dangerous ideas, and that troubles me. I fear, too, that friends, family, and colleagues will mistake my academic interest and personal engagement for political sympathy. These concerns are balanced by others, however. I recoil from attempts by colleagues to defend my work based on its potential benefit to future antiracist activism. Those claims may be justified; as Roy D'Andrade (1995) argued, moral authority flows from knowledge, rather than vice versa. But they live in a universe where pursuit of social good is the core justification for conducting research and leave me wondering how these readers would regard knowledge that does not support their moral agendas. However, it is not the professional scholarly but the insider nationalist reception of my writing that causes me the most discomfort. Both when *Radio Aryan* called my book "an amazing breakthrough" for white nationalism and, alternately, when other insiders label me "politically correct" or claim with malice that my "Jewishness" resonates in what I say and do.

The social and political cleavages between myself and nationalist sow both insight and emotional strain; my research remains, to borrow Anna Tsing's words again, work across difference. However, much of the promise and torment of the approach rest in the fact that fieldwork dilutes those differences. By entering into reciprocal, collaborative, and affectionate relationships with radical nationalists, I moved toward a position that was neither insider nor outsider, neither cheerleader nor opponent, and neither an accomplice nor an innocent. It was a position liminal in its morality as well as in its perspectives, accessing something like what Susan Harding (2001:xii) calls the "narrative space" between belief and unbelief in her study of Born-Again Christians and what Peter Pels (2000) describes as the ambiguous "trickster" persona of the anthropologist. Occupying this position may be ideal for generating ethnographic knowledge, but it is no guarantee of moral virtue. Like the Golden Rule, its outcomes depend on the people involved.

Conclusion

I do not expect all to endorse such radical, reckless use of collaboration, reciprocity, and friendship. The ethnographic

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16. Contrary to Kaminsky's (2017) allegation, never have I commented on—or claimed to have commented on—the exceptionally complicated issue of whether and in what ways Saga should be held responsible for the reception of her music.

practice I embraced featured, unremarkably, intense social and intellectual entanglement with research participants and surrender of interpretive and communicative authority, both by empowering others and by subjecting oneself to transformative social influence. It also offers unparalleled capacity to learn from and with those studied, and that is my core justification for using it despite negative consequences. It is a suspect defense, I will admit, principally because it prioritizes research efficacy over moral integrity, counter to the foundational precepts of institutional review standards and the guidelines of academic societies alike. If acting in solidarity with informants promises epistemological rewards but moral volatility, should use of the practice not be conditional rather than obligatory, precisely so that research outcomes like mine can be prevented?

Perhaps, but the likely impacts of such a solution strike me as dystopic. If we decide we cannot tolerate moral compromise in research, we would be drawn to reserve ethnography in its most effective forms for studying people whose values and actions we find agreeable. Such cases offer predetermined moral agendas the best chance of surviving a research experience that confines and mutates a scholar's voice. In contrast, when studying people we find less agreeable, maintaining the moral and political profile of our work would require us to turn away from ethnography and toward, by varying degrees, its antithesis: a research model—let them call it "cultural critique"—with no insider contact and with the scholar holding expansive control over the conception, interpretation, and communication of research. The latter is also a model providing limited capacity to learn from and with those studied. Conditional use of scholar-informant solidarity in this fashion foretells a situation in which we know more about those we like and less about those we do not. That sounds intellectually, politically, and socially dangerous to me. And I might be too charitable in my characterization, for how many individuals and communities are there who the morally devout scholar could deem righteous enough for ethnography?

We should label the moralist attenuation or elimination of scholar-informant solidarity for what it is: a strategy of reversing long-standing efforts to collapse physical, social, and power differentials in anthropological research, a strategy to secure distance and assert control. If those we study cannot be trusted to wield agency in ways that advance our moral agenda, the reasoning appears, then we best keep agency to ourselves, even if that means denying others the opportunity for transparency, dialogue, or being studied at all. The foremost consequences of such moves are, again, epistemological. They illustrate what author Rebecca Solnit (2018:5) describes as the neglected inverse of the knowledge-is-power truism, namely, that "power is often ignorance" because of the social and intellectual isolation it affords. Indeed, the scholar who decides to bypass or weaken exchange, partnership, and learning from others has leveraged power to ensure ignorance, to ensure that pieces of the human experience about which we seek expertise remain to us a mystery.

Comments

Sindre Bangstad
KIFO, Institutt For Kirke-, Religions- og Livssynsforskning, Besøks-og, Øvre Slottsgate 2 B, 0157 Oslo, Norway (sindre.bangstad@kifo.no). 26 IX 18

Benjamin Teitelbaum has written a thought-provoking and challenging article that advances the case for what he characterizes as an "immoral anthropology" resulting from what he regards as the "imperative of scholar-informant solidarity." With reference to his own extensive ethnographic fieldwork among radical nationalists in the Nordic countries and beyond since 2010, Teitelbaum is critical of what he sees as "moralism in our field" of anthropology and the weakening of scholar-informant solidarity in the 2012 revisions of the 1971 AAA Principles of Professional Engagement. The 2012 revisions that are the targets of Teitelbaum's critique state that for anthropologists "obligations to research participants are usually primary," whereas the 1971 original stated that "in research, anthropologists' paramount responsibility is to those they study."

There has, of course, been an enormous development within anthropology and the various anthropological subfields in the more than 40 years between those two AAA statements. And I think Teitelbaum is right to pinpoint that whereas we could once imagine the wider field of anthropology as being centrally concerned with "studying down," anthropologists nowadays very often "study up." And right to note that the shifting terrains of what anthropologists generally do in the field has certain consequences on both ethical and methodological levels. "Ethnography today entails being a collaborator of both the good sort and the bad sort," writes Teitelbaum, and we can no longer pretend that the "staid impression of anthropologists as scholars of the disenfranchised and oppressed—those with whom solidarity appears morally and politically unproblematic" still holds.

And yet in reading Teitelbaum's article, I often found myself in disagreement with the conclusions he draws from his own ethnographic engagement with radical nationalists in the Nordic countries. Teitelbaum has some terminological slippages in first describing the people he has studied as "radical nationalists" and then simply "nationalists." The latter term veers way too much in the direction of sanitizing and whitewashing what they represent through a wholesale adoption of their own preferred emic terminology. Looking back at the record of the many anthropologists who, from the early days of the development of ethnographic fieldwork methodologies, have worked with the proverbial "people they do not necessarily like," it strikes me that the moral and political conundrums Teitelbaum describes are really not all that new in the discipline. My own point here in a short commissioned essay back in 2017 (Bangstad 2017) was that in any given ethnographic field, ethnographers are likely to have encoun-

tered individuals whose politics, behavior, or morals they disapprove of on a personal level. I myself certainly did, in my erstwhile ethnographic fieldworks among Muslims in the townships and informal settlements of postapartheid Cape Town in South Africa from 2000 to 2005. To admit to the very fact that we as anthropologists have variegated personal relations and variegated senses of personal obligations to the people we study is simply to admit to having some of the same standards in our relations to informants or "friends" (Teitelbaum's preferred term) that we have to any friends in ordinary life. And in contradistinction to Teitelbaum, I think the caveat to the principle of scholar-informant solidarity that the AAA introduced in its revisions in 2012 makes relatively good sense. Let me try to be more precise in describing why I happen to think this is the case. Quite a number of radical nationalists in Sweden have a violent and criminal record. By way of an example, the Nordic Resistance Movement—which I, unlike Teitelbaum, would not hesitate to describe as "neo-Nazi" rather than "radical nationalist," and for patently obvious reasons having to do with their ideological roots—has, according to any number of Swedish media reports, a significant number of active members with violent criminal records. Those criminal records also include violent attacks on individuals of immigrant background and antiracists. A basic moral question for any ethnographer working with such activists and groups would be what one would actually do if one, through ethnographic fieldwork, learned about one's informants or "friends" planning or having executed such attacks. To call for an unflinching commitment to scholar-informant solidarity and an "immoral anthropology" in such an event seems to me unsustainable, also in light of anthropology's long-standing commitment to the defense of human rights for all humans regardless of their background. When I raise this, it is of course not an entirely hypothetical question: the Norwegian sociologist Katrine Fangen in a study cited by Teitelbaum (Fangen 1999) conducted an ethnographic study of Norwegian neo-Nazi groups in and around Oslo in Norway, whose members would go on to knife to death Benjamin Hermansen (15, and of Norwegian-African descent), at Holmlia in Oslo East. Fangen of course knew nothing of what would happen with the neo-Nazis in question long after she herself had left the field, but she was by no means naive when it comes to the violent potential of some of those she worked with.

It also seems to me that Teitelbaum is too dismissive of ethnographers who have opted for a more disinterested and neutral approach to the study of far-right activists in recent years. Nitzan Shoshan's (2016) *The Management of Hate*, based on undercover ethnographic fieldwork among neo-Nazis in former East Berlin, does at the very least problematize Teitelbaum's contention that the generation of richly textured and deep ethnographic knowledge necessarily presupposes "extensive sympathetic . . . relationships" between the ethnographer and those that he or she studies. In reference to his role as a public commentator on radical nationalists in Sweden, Teitelbaum admits that "defending nationalists . . . has been my

main undertaking as a public commentator" and argues that "nationalists' interests [are] my 'paramount' concern." Here Teitelbaum seems to be making a vice out of professional virtue: as an "anthropologist of Islam" I have never seen it as any part of my responsibility to any Muslim informants now and then to "defend Muslims" regardless of what they do. My own alternative to Teitelbaum's would be to realize that there are no shortcuts to easy and uncontested answers when it comes to our professional ethics.

Kirsten Bell
Centre for Research in Evolutionary, Social and Inter-Disciplinary Anthropology (CRESIDA), Department of Life Sciences, University of Roehampton, London SW15 4JD, United Kingdom (kirsten .bell@roehampton.ac.uk). 25 IX 18

Solidarity and Immorality or Empathy and Ambivalence?

More than 20 years ago, in the very pages of this journal, Roy D'Andrade (1995) excoriated the rise of a moral model of anthropology. In his provocative essay, Benjamin Teitelbaum is also concerned with the consequences of the moral model, although he distances himself from D'Andrade's critique. If a moral hierarchy of legitimate objects of research increasingly dictates what anthropologists study and how (Fassin 2008, 2012), for Teitelbaum, a consequence of this shift is that our moral commitments have become incompatible with our epistemological ones. Contra Laura Nader (1972), Teitelbaum's position is that a different ethics should not necessarily attend the process of "studying up." Instead, based on his fieldwork with white radical nationalist groups in Nordic countries, he defends "scholar-informant solidarity in ethnography as morally volatile and epistemologically indispensable."

Teitelbaum's main argument is that anthropologists who bypass exchange, partnership, and learning from those they study have effectively ensured that pieces of the human experience will remain a mystery. I agree. I also agree that bypassing trust, intimacy, and reciprocity is inimical to ethnographic fieldwork—at least as anthropologists conceive it. But the caveat is important, because any attempt to characterize ethnography must confront the fact that the term has different meanings across the disciplines that have come to claim a stake in it. Therefore, sliding between anthropological and other disciplinary accounts of ethnographic fieldwork as if they are speaking about the same thing causes some confusion, not so much in Teitelbaum's core arguments (which, again, I broadly agree with) but in the terminology he uses to describe them.

As Rena Lederman (2004, 2009, 2013, 2017) has illustrated, there are subtle but significant differences between ethnographic fieldwork as anthropologists and sociologists conceive it—differences that stem from its contrasting epistemological

status in the two disciplines. In the former, ethnographic fieldwork is the default research style, needing no special methodological justification, and its validity is conventionally understood to be based in "field relationships characterized by intimacy, reciprocity and trust" (Lederman 2009:11). In the latter, it is a minority approach that is in persistent need of methodological and ethical justification, precisely because it upsets sociological norms regarding the relationship between the researcher and the researched, which is typically "construed as definable and delimitable by the researcher" (Lederman 2009:11).

Another core difference between ethnographic research as practiced between the two disciplines relates to the ultimate goals of the ethnographer. As Katz and Csordas (2003:275–276) note, anthropologists "characteristically have illuminated native groundings for subjects' perspectives," whereas ethnographic sociologists are more likely to break with members' perspectives, "deconstructing what subjects treat as naturally significant." Importantly, the idea of the anthropologist as learning from those studied has always been fundamental to the discipline's stance toward fieldwork—recall Malinowski's (1922) dictum that the goal of fieldwork is to "grasp the native's point of view, his relation to life, to realise *his* vision of *his* world" (25; italics in original).

In light of these differences, it seems to me that some of Teitelbaum's characterizations of ethnography are based more on a sociological than an anthropological version of the practice—or, at the very least, these disciplinary variations require further recognition (see Bell 2019). For instance, while the scientific paradigm meant that anthropologists' relations in the field were historically backgrounded, trust, reciprocity, and exchange have always been integral to the research enterprise as anthropologists conceived it (see, e.g., Evans-Pritchard 1973). This, in conjunction with the desire to illuminate rather than deconstruct the local point of view, partly explains why anthropologists have generally rejected deception in a way that ethnographic sociologists have not (see Lederman 2017).

As Teitelbaum notes, the shift that occurred in anthropology was the new moral emphasis on solidarity that resulted from a variety of quarters—from internal critiques of the discipline starting in the 1970s to later calls for "collaborative," "militant," and "moral" anthropologies. But, again, this does not mean that the "ghost of ethnography's past" was one in which trust, intimacy, and exchange were absent; the difference is that these earlier anthropologists were not wedded to solidarity as a moral value. Thus, the mistake I think Teitelbaum makes is in treating solidarity as synonymous with intimacy as a scholarly knowledge practice, because it seems to me that he is not advocating solidarity in the sense of shared goals or interests (the dictionary definition of the term) but empathy, respect, and understanding, which is not quite the same thing. To quote Teitelbaum himself, "I moved toward a position that was neither insider nor outsider, neither cheerleader nor opponent, and neither an accomplice nor an innocent."

The million-dollar question is whether it is possible to sustain this arguably classic position (now shorn of any positivist threads) when studying up. Nader (1972) did not seem to think so, because she made it clear that this new subject matter would require new methods—including a deprivileging of participant observation as our modus operandi. However, I am with Teitelbaum that ethnographic practice offers "unparalleled capacity to learn from and with those studied." What I am less sure of is that maintaining our epistemological commitments regardless of whether we are studying up or down results in an "immoral" anthropology—an ambivalent one, maybe, but that is not quite the same thing (see Kierans and Bell 2017). Instead, it seems to me that what Teitelbaum is advocating is "reversing the relationship between analysis and ethnography so as to give the latter logical priority over the former" (Holbraad 2018:44). This does not make his fieldwork immoral but instead serves to unsettle the analytical framework of morality/immorality itself, "articulating the many ways in which it may come up short when exposed to the contingencies of different ethnographic situations" (Holbraad 2018:45).

Carolyn Fluehr-Lobban
Professor Emerita of Anthropology, Rhode Island College (cfluehr@ric.edu). 27 VIII 18

The paper merits comment for the multiple ethical issues it raises. As ethics and morality are often confused, the paper helps to sharpen the difference between the two. Studying ultra-right, neofascist social groups may be considered morally compromising for the vast majority of anthropologists, but the ethical issues raised by the author get to the heart of the researcher-informant relationship and thus to ethically responsible research.

I was close to the debates within anthropology, specifically the American Anthropological Association (AAA), including those referenced by the author regarding the change in the AAA from language that referenced the "paramount [ethical] responsibility to the people studied" (1971) to that responsibility being described as the "first responsibility to those whose lives and culture anthropologists study" (1990). The addition of professional responsibility to other stakeholders in 1990, including "clients and sponsors," expanded ethical responsibility beyond "the people studied" and likely reflected the growing number of practitioner anthropologists working outside of academia. That language evolved to a 1998 revision of the AAA code and the provision that "researchers must be open with funders, colleagues, persons studied, and all parties affected by research" (Fluehr-Lobban 2013:12).

The years of research the author spent conducting research, since 2010, of radical right organizations in several Nordic countries demonstrate and reaffirm the power of participant observation, anthropology's primary method. However, counter

to most anthropological research, as the researcher-informant relationship deepened, so did the moral and ethical complexity. This is especially sharp when the author deals with the moral righteousness of scholar-informant solidarity where collaboration might make researchers accomplices to acts of violence. As the AAA code of ethics' most recent revision (2012) reads, "striving to make space for researchers who want to work in opposition to those they study" indicates that scholar-informant solidarity is a dispensable element of ethnography. This may extend to some contemporary researchers making explicit a rationale of subverting of the goals and tactics of the people studied. However, for the author, the moral questions and outcomes of this type of research are, indeed, morally compromising and politically dubious. This is, perhaps, the most provocative aspect of the paper.

Further, the author argues that the contradiction raised by researching morally dubious, far-right organizations and their followers is both morally volatile yet it is epistemologically indispensable. The author's critique is to moralism arguing that such research is not for the purpose of doing good but in spite of the potential to do harm.

The study of the far right, such as sections focused on Swedish National Socialist Magnus Soderman, resolve to an assessment of relative harm and good or perhaps a "greater good." Ethics, as assessing relativistic and situational virtue and morality as transcendent values, the author argues that moral imperatives trump ethics while at the same time warning of conducting politically correct research. The primarily solitary nature of field research makes matters of ethics and morality a process of personal self-consciousness and accountability. This underscores the value of external review, without which the role of research "friendships" can be stressed over the complexities of engagement. The 2009 AAA Code of Ethics is clear that professional responsibility "is superior to seeking new knowledge," suggesting to the careful reader that some research topics might not be undertaken if they possess the potential for doing or causing harm. In this way the principle to "do no harm," or to avoid harm in the initial conception or actual conduct of research, might have cautioned the researcher to alter the course of research or consider more deeply its potential consequences. The author's personal struggles with the research, including ambivalence about assistance in moving a skinhead to a larger public platform and being a self-identified person of Jewish background, nonetheless for the author resolve to one of a commitment to defend nationalists and not nationalism and to regard his informants' interests as paramount. This section of the paper is likely to provoke the greatest passion and criticism. One might reflect upon the potential effects had there been critical anthropological researchers in Berlin in 1935–1937 or in Madrid or in prefacist Spain, not to mention the many contemporary examples of protofascist and fascist states.

As a conclusion of sorts, ultimately, the author-researcher is neither insider nor outsider, neither an accomplice nor a bystander. The author justifies the research as ultimately serving the people we study, a radical reaffirmation of the "paramount" rather than "primary" responsibility of anthropological research. This article serves mainly as a vehicle for discussing the broader professional ethical and moral issues that it raises.

Tobias Hübinette
Department of Language, Literature and Intercultural Studies, Karlstad University, SE-651 88 Karlstad, Sweden (tobias .hubinette@kau.se). 30 VIII 18

Is It Possible to Dance with Wolves? A Swedish Antifascist's Reflections on Benjamin R. Teitelbaum's Article

I have been asked to reflect upon Teitelbaums's article because I have studied the Swedish far-right movement and radical nationalist scene since the 1990s, including both the historical and the contemporary Swedish radical right. I have followed and written about this political movement both inside and outside of academia. I also have a long history as an antifascist, meaning that I am not just very familiar with all the myriad names, events, parties, acronyms, songs, and facts that turn up in Teitelbaum's (2017) unique study of Sweden's radical right, including its music scene and countercultural practices: I have also actively opposed, combatted, and exposed many of the very same individuals and organizations that Teitelbaum writes about in his book and that he also names and brings up in his article.

To get straight to the point, Teitelbaum has chosen a research topic that is considered to be highly contested by almost everybody in Sweden and elsewhere, as well as a research path not only that is considered to be controversial but also that practically everyone condemns within the Swedish academic world. By studying Sweden's radical right not just through texts and statistics but also possibly through interviews and focus groups, which many Swedish scholars actually do today—and not the least as the right-wing populist party, the Sweden Democrats, is currently Sweden's second biggest party—Teitelbaum has chosen to practice a long-term and emotionally committed, collaborative, and reciprocal ethnographic work among some of the leading ideologists, artists, and activists of the Swedish far-right. This to the extent that he has become friends with several of them, according to what he calls the spirit of "scholar-informant solidarity."

Teitelbaum's story behind and argumentation for his choice of practicing an "immoral anthropology" together with Sweden's radical nationalists are honest, compelling, and, at times, also convincing—especially when he implicitly or explicitly criticizes what in the old days would have been called armchair research and is perhaps still the dominating way of studying this particular political scene within Swedish scholarship. In that sense, Teitelbaum's methodological approach

is without doubt superior to the majority of studies on the far right, which never include the radical nationalists themselves as informants. However, some researchers who have actually talked to Swedish far-right activists are not mentioned in his article, such as Diana Mulinari and Anders Neergaard (2014), Heléne Lööw (2016a, 2016b, 2017), Mattias Gardell (2003, 2015), and Vidhya Ramalingam (2012)—even if none has engaged with radical nationalists to the extent that he has.

So, to put it a bit bluntly: Is it OK, then, to become a friend of far-right activists as a researcher, to help them with texts, and to even defend them in the public sphere and in opposition to how they are portrayed in the media, as well as in academia, in order to be able to develop even better knowledge production on the movement and to nurture what might be called the ethics of scholar-informant solidarity? For many years, I was myself against this. I remember that I, for example, opposed the Norwegian anthropologist Katrine Fangen, who is mentioned in Teitelbaum's article, and who was strongly stigmatized among antifascists within the academia, such as myself, as she became (in)famous through her ethnographic work among violent Nazi skinheads in Norway in the 1990s. However, since then I have myself interviewed Sweden Democrats, and I have also encountered, communicated with, and been in continuous contact with some radical nationalists now and then, although not to the same degree as Teitelbaum (Hübinette and Hyltén-Cavallius 2014).

At the same time, I also understand all of the Swedish researchers whom I know and who have reacted strongly toward Teitelbaum's research methods. I remember, for example, the negative reactions when during a presentation in Sweden Teitelbaum disclosed that he occasionally sleeps over at the homes of certain Swedish radical nationalists—in addition to telling about his meetings, dinners, and drinking experiences together with them. Such strong reactions are simply outright condemnations, as the vast majority of Swedish researchers would never do what Teitelbaum is doing, even if some of them do "study up" nowadays, such as within the fields of Swedish whiteness and masculinity studies. This condemnation is not caused by some "do-good" moralism toward a political scene that is considered to be "untouchable," nor is it about an unwillingness to conduct uncomfortable "work across difference," to paraphrase Anna Lowenhaupt Tsing (2015), whom Teitelbaum refers to.

Instead, most Swedish researchers—and here I agree with them—would claim that even a sincere pursuit of more knowledge about the far right (regardless of whether this sincere search for better knowledge is fueled by "pure" and even "naive" academic curiosity) cannot be compromised by the development of friendship ties with some of the most notorious and influential Swedish radical nationalists who directly or indirectly have caused so many problems for others, including pain, suffering, fear, and stress among the minority population of Sweden, which is constantly and massively stigmatized by the Swedish far right. Where the line goes is, on the other hand, not easy to demarcate because collaborative

anthropology and the friendship model of ethnographic work naturally result in personal relationships between the researcher and the informants. However, at least to me, one step over the line was when Teitelbaum was confronted with the request to help with far-right text production and eventually did so. Other examples that Teitelbaum brings up are not that easy to either comment on or condemn, as personal relationships are for sure always messy.

So to sum up and end my reflections on Teitelbaum's article, I find it to be both bold and honest and an important intervention into the always necessary scholarly discussion on the relations between ethnographic work, ethics, and politics. While today I mostly condone much of Teitelbaum's methodological choices as a researcher, which I would not have done some years ago, I also fully understand his condemning critics in Sweden. I do find that he has stepped over the line sometimes, such as in the case of helping with the editing of a far-right novel. In other words—and given that many radical nationalists identify themselves with wolves—"dancing with wolves" is possible as a researcher doing ethnographic work and even necessary in order to produce scholarship on the far-right that is not just armchair research. Yet those still few researchers who chose to do so must always be cautious not to become complicit in producing and disseminating political propaganda that all too often borders on hate speech, the consequences of which may result in everything from more stigmatization and discrimination to more hate crimes directed toward minorities—and ,in the worst case, even murders and massacres.

P. J. Pels
Faculty of Social Sciences, Leiden University, Pieter de la Court, Wassenaarseweg 52, 2333 AK Leiden, The Netherlands (pels@fsw .leidenuniv.nl). 1 X 18

Benjamin Teitelbaum's essay is useful, timely, and important, and it would be a mistake to restrict its impact to anthropological ethics and morality. If, indeed, scholar-informant solidarity occurs even across large political and moral differences between scholar and research participants, we should take a hard and, above all, empirical look at the cultural practices of social science. His point that "scholar-informant solidarity" is also "epistemologically indispensable" raises the issue of whether anthropologists have sufficiently pursued the question of what values their methodology carries with it. At the very least, Teitelbaum shows that ethnographic research has to be seen—just like social life in general—as a process, impossible to capture by the kind of timeless rules we still often associate with "methods" and "ethics." Teitelbaum's cases (once more) show that the extended case method is paradigmatic for ethnography but take it beyond the colonial race relations that Max Gluckman first experimented with (Evens and Han-

delman 2006; Gluckman 1940). More importantly, Teitelbaum shows that anthropologists do not have to choose between ethical primacy or scientific objectivity. As a purely moral enterprise (encompassing Teitelbaum's broad sense of including both ethics and morality), anthropology has no real raison d'être at an academic level, since it dissolves into situational politics and ignores other notions of the good that anthropological research carries with it. When it exclusively claims objective expertise, it loses the claim to scientific integrity, reflexivity, and connection to real life that more alienated social sciences have lost or defined as irrelevant.

In fact, if anything, I would argue that Teitelbaum's argument remains too modest, especially where he concludes by declaring his own argument to be a "suspect defense" of collaboration, reciprocity, and friendship, "because it prioritizes research efficacy over moral integrity, counter to the foundational precepts of institutional review standards and the guidelines of academic societies alike." Institutional review processes and the guidelines of academic societies deserve no such deference. They have initiated, at least in the Anglo-Saxon world, a process of deprofessionalization (Freidson 1984) in which review of professional performance has increasingly been taken out of the hands of peers and shifted onto the desks of their employers and university managers, driven initially by the fear of malpractice suits. Of course, we should not go back to a time when extra-academically employed anthropologists were regarded as a kind of second-class professionals. However, a better recognition of social hierarchies that are "out there"—in the world of bosses and employees beyond the boundaries of the meritocracies where anthropologists are educated—is required to discuss whether, why, and when anthropologists should prioritize "research efficacy" over "moral integrity." Maybe the phrase can better be changed to "prioritizing scientific integrity over moral consistency," so that we drop the cultural assumptions that scientific epistemology is not a form of moral integrity and that consistency is a moral virtue. A situational ethics shows us that they are not.

More importantly, the recent introduction of "data management" protocols in European anthropology as conditions of "scientific integrity" and transparency shows the dangers of opposing research efficacy to moral integrity (see Pels et al. 2018). This use of "data" shows a lack of reflexivity that imposes a (predominantly quantified) conception of research materials on all sciences. Ethnographic research exposes how social scientists (even when they have outsourced their primary research work to external bureaus, internet programs, or interchangeable interviewers) continue to rely on social relationships for producing "raw" research materials; it also shows that the quality of all knowledge gathered depends on a process of forging transactional and transformational validity within those relationships (Cho and Trent 2006). Teitelbaum shows clearly how much this relies on a constant renegotiation of informed consent—not least information about to what extent the researcher can be trusted. More importantly, Teitelbaum's three cases show how this may differ from situation to situation. This serves to show once more how little anthropologists have done to actually study their own epistemological and methodological practices and sharpen their vocabulary. Teitelbaum's own discussion of the relationship between moral commitments, friendship, and knowledge shows how rarely we discuss when participation (a condition of all ethnography) crosses over into activism (a personal choice), or where Teitelbaum's "solidarity with abandon" (which implies not knowing where this will take you) diverges from the conscious planning of collaboration advocated by Lassiter or Holmes and Marcus. Teitelbaum shows that participation makes us lose (partial) control over the process (but keeps silent over what this does to our observations). Does that mean that we are responsible for such unintended consequences or can take credit for its felicitous, unplanned discoveries? What is the difference between a serious attempt to adopt the cultural competences of the life-world of our research participants, making friends with them, and helping their cause? How do we incorporate the global hierarchies that infuse the life-worlds of both scholar and research participants?

"Scholar-informant solidarity" is, indeed, "morally volatile and epistemologically indispensable." As Teitelbaum notes, it is also an originally colonial construct. Acknowledging it as such means that we recognize that it is never free of the social hierarchies that anthropologists externalize when they reduce research to a dyad between scholar and research participants (see Pels 2014). It also means that this reduction to subject and object is always an abstraction, contrary to what Teitelbaum implies when commenting on collaborative ethnographies. I believe we should first develop a more concrete account of what it means to do ethnography before we succumb to such abstractions.

Nancy Scheper-Hughes
Department of Anthropology, Kroeber Hall, University of California, Berkeley, California 94720-3710, USA (nsh@berkeley.edu). 9 I 19

The Case for a Moral and Politically Engaged Anthropology

I think there is blame on both sides . . . You had some very bad people in that group . . . But you also had people that were very fine people, on both sides. (President Trump, following the Unite the Right Rally, University of Virginia, Charlottesville, 2017)

I know fellow Americans that are evil. I know—are you saying we shouldn't say that a fellow American is "evil"? I've known some fellow Americans that are pretty evil. (President Trump commenting to the International Association of Police on Americans who opposed the confirmation of

now Supreme Court Justice Brett Kavanaugh, October 8, 2018)

I have read Professor Teitelbaum's disturbing paper several times, mostly in the air as I flew from California to NYC, Ireland, and Tangiers for conferencing, research, and lectures. The author's argument is passionate, controversial, even "radical" in a certain sense, but utterly confusing and contradictory. Following a long and unnecessary introduction to our professional anthropological codes of ethics that have wavered between intellectual freedom and protection of vulnerable research subjects, in addition to the requirements of our bureaucratic institutional review boards (IRBs) designed to protect universities from litigation by angry and disgruntled research subjects, I expected to see a new take on what Loic Wacquant and I have called "the ethics of the craft" in our seminars "Ethnography Inside Out."

The ethics of the craft refers to the intimate, local, intersubjective, geopolitical, cultural, historical context within which we must deal with the specificities of the ethical demands of ethnographic fieldwork. There is no universal ethical codebook. Thus, we fall back on a hierarchy of obligations: what we owe to our "informants," to our profession, to our colleagues and our students, to social science, and to the broader public. Teitelbaum's answer is reduced to a single ethical mandate: that fieldworkers, scholars, and writers are bound, above all, to "scholar-informant solidarity." Solidarity, in turn, requires empathy, reciprocity, care, respect, and collusion with our subjects/informants roughly based on the model of friendship and verging on adulation, if not love itself. This applies without restraint, critical thinking, or moral or political judgment. Our role, as the author sees it, is that the ethnographer is not simply neutral but rather a handmaiden to our subjects/informants, to embrace their understanding of the world, and to promote their goals and values whether they are rational or deceptive, conspiratorial, or even "immoral," dangerous, racist, and destructive. The total subordination of the moral/ethical to research is the issue.

This move is not to be confused, I think, with traditional ethnography that requires open-heartedness and open-mindedness toward a "world" that is unfamiliar to us. I have often told my students who are nervously embarking on a field voyage to parts unknown to surrender. That is, to be like a child in the care of strangers. The fieldworker must try to "bracket" his or her personal baggage, including one's sense of time, class and gender, one's sense of humor, and one's sense of justice. We have to be willing to live comfortably with our informants, to support them, to care for their well-being, their dignity, their pride, and, when asked and appropriate, to help them achieve their goals. However, many of us work in extremely complex situations with diverse and dissenting individuals, as, for example, in conflict zones, where our informants may include enemies of the state, spies or counterspies, or collaborators with state police or military. Scholar-informant solidarity is not adequate in such cases. The

work of the ethnographer is to observe and to listen, to interpret and to explain, as well as to honor and respect our informants. The "method," if one can even call it that, is incomplete. Beginning with our ethnographic forbearers, from Malinowski to Hortense Powdermaker, from Levi-Strauss to Clifford Geertz, Marshall Sahlins, Sidney Mintz, Pierre Bourdieu—despite radical theoretical differences—all have understood that ethnography requires layers of interpretation and "translation" in which the fieldworker moves in and out, back and forth, between emic (insider) and etic (outsider) perspectives. Teitelbaum only accepts a naive and total subordination to the values, culture, and goals of his informants. He enters the field and stays there as if he has now completed his ethnography, when, in fact, he has only begun. His approach is a dangerous version of populist, Nationalist, Afrikaner folklore during South African apartheid, which I have written about elsewhere and which is perhaps best captured by Vincent Crapanzano (1986) in his book *Waiting: The Whites of South Africa*.

According to Teitelbaum, anthropologists are to be loyal collaborators even if their informants' values and goals are neofascist, a term that the author, correctly, sees as a foreclosure, a moral judgment. So he refers to his Swedish right-wing white supremacists as extreme nationalists, and he tries to get close enough and intimate enough to understand what makes these young men and one woman tick. This is not whitewashing but an essential part of participant observation. However, the author goes far beyond ethnographic neutrality, beyond the fieldworker's role of "translating" our informants and their cultural, social, and political lives, their dreams and wishes of mind and soul. Teitelbaum embraces their ideologies as if they were his own.

The author insists that our professional role is not to question, interrogate, debate, or interpret our subject's perception of the world they live in. The only lens he uses is local knowledge. He seems to have discarded the obligation of the ethnographer to take the next step: to explain, analyze, and critique. He argues that open-ended, dialogical, and critical approaches toward our informants can only diminish our knowledge and epistemology. To get close enough to our subjects in order to create the basic trust needed requires constant sympathy and solidarity. His paradigm of scholar-informant relations is friendship. Yes, friendships are made in the field, especially after decades of involvement with the people who have taken us in. But even the "closed corporate communities" that Eric Wolf wrote about in the 1950s, suspicious of the world outside their "circle of trust and comfort," are made up of diverse personalities, behaviors, values, and dreams and wishes of the soul.

However, I do agree with Teitelbaum that we must treat our research informants with respect and that we should never demean their dignity, their habits and habitus, their ways of living and being in the world. We have to create an environment in which the fieldworker can become close enough, intimate enough, so as to enter into deep conversa-

tions by opening oneself to our informants, sharing our secrets, working together with those with whom we have an intuitive understanding and affection. But intimacy and understanding cannot be tethered to a single value—in this case, solidarity—leading to the author's collaborations with his radical Nationalist Nordic white supremacists in the name of "reciprocity,"

Are solidarity, reciprocity, and collaboration the best and only ways to reach the depth of our informants? Teitelbaum shares so little about his two informants, Magnus, an anti-Semitic, anti-immigration journalist, and Saga, the darling radical nationalist siren and popular singer of the genocide of Nordic whites, that I cannot see how his intimate collaborations have brought him to any higher state of ethnographic competency. If anything, these informants are mere shadows and mere ghosts of themselves.

For many years I have conducted research on violence—familial violence, revolutionary violence, state violence, bureaucratic violence, and paramilitary violence (see Scheper-Hughes references). Simultaneously, I have conducted research on international criminal networks on organ trafficking and kidney selling. To do so I had to cross many borders and lines to observe, interview, and sometimes participate in illegal transactions between outlaw surgeons, organ transplant brokers, desperate kidney buyers, and equally desperate kidney sellers. I have found that it is both possible and ethical to engage in discussions and disagreements with my informants as long as it is done with respect and openness to changing one's original point of view. Why else do we travel around the world? Surely, it is not to prove our hypotheses but to learn that our initial understandings are simplistic or quite wrong. Why would anyone bother to travel the world and face the many dangers of fieldwork?

I have made friends with crooks and criminals, especially those who struck me as organic intellectuals, who could help me understand ethics of their craft, human trafficking and smuggling, for a goal they believe cancels the crime: to save a life! Many people have asked me why kidney traffickers, organ and transplant brokers, and outlaw surgeons were willing to talk to me and even to invite me inside their back-door clinics and surgical operating rooms rented by the night. The answer is because they are curious, just as I am curious. They have "let me in" because they enjoy the discussions, the debates, the challenges, and the differences, because they are not afraid of disagreement, and because they recognize real rather than false or simulated emotions.

In one instance I spent the better part of two summers in a federal prison in Pernambuco, Brazil, to get to know Gaddy Tauber, a former Mossad military officer who got caught up in an international organ-trafficking syndicate between Israel, Brazil, and South Africa. I sat through part of his trial; I served as a key witness to a CPI and a congressional investigation in Recife, Brazil, explaining how the Israel-Brazil-South African scheme worked. It took two years during his imprisonment for Tauber to agree to meet me in his federal prison. He told

me face to face: "I hate you!" I replied, "I guess I understand why you do." But we found that we enjoyed talking and loved to argue. Over these months we laughed more often than we shed tears. In the end I spent a day locked inside his two-room cell (with a Brazilian journalist) while Gaddy cooked us a miserable lunch, chicken soup made from a skinny, ugly chicken and droopy carrots and rotting onions. I tape-recorded one of our last interviews.[17]

A few years later, in 2012 I was shocked to see Tauber on the nightly BBC news being arrested in the Rome airport. He looked very old and very frightened. For sure, I thought, he was en route to Boston to visit his daughter. Gaddy told me that he was now on parole and was being allowed to visit his dying mother in Israel. She must have been quite old, I thought, as Gaddy was in his mid-70s. I contacted Interpol to tell them that Mr. Tauber had permission to travel for humanitarian reasons and that he should be freed. When we met up a few years later in Israel, we laughed about my interventions on his part—in fact, Gaddy said he had escaped from Brazil. I shook my head, and I agreed that we enjoyed each other's company but that neither of us really trusted the other. (See fig. 1.)

There is a time and a place for informant solidarity. Like Teitelbaum, I have taken certain risks, in my case supporting and helping two of the young men from the impoverished township of Gugutethu in Cape Town who were convicted as ringleaders of a black youth demonstration that went haywire and ended in a mob scene of stones and a single knife that killed the American Fulbright scholar Amy Biehl, an activist for women's rights in the black and colored townships of Cape Town, in August 1993.[18] But at the same time, I worked closely with the grieving parents of Amy Biehl and have written about their amazing reconciliation with the two young men who they never saw as the murders of their daughter. "Apartheid killed our daughter," Linda Biehl told me. The story would not be complete until I interviewed the Biehls, observed the trial, and interviewed many times the prosecutors, state defenders, the judge, and so on, while absorbing all the contradictions surrounding this case and its amazing aftermath. The solidarity that I sought in this long episodic period of fieldwork over several years was a "solidarity of the shaken," encompassing all of the parties during this traumatic time in the history of South Africa. The Czech philosopher Jan Patočka introduces in his book *Heretical Essays in the Philosophy of History* the idea of "solidarity of the shaken," referring to the particular bond that originates between people who have experienced a strong disturbance of the certainties, big and small, that once held their lives in place. Scholar-

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17. https://www.sss.ias.edu/files/pdfs/Face%20to%20Face%20with%20Gaddy%20Tauber-09%20.pdf.

18. https://www.researchgate.net/publication/320064151_We_are_not_such_things_the_murder_of_a_young_American_a_South_African_township_and_the_search_for_truth_and_reconciliation.

Figure 1. Nancy and Gaddy in prison in Recife, 2006. A color version of this figure is available online.

informant solidarity is most appropriate during traumatic times of civil wars, police violence, natural disasters, political crises, and so on.

However, solidarity is surely not the only intersubjective role and relationship with our informants in the field. While we are trained to hold anthropologist-informant relations as a sacred trust, surely, this does not mean that one has to be a bystander, let alone a collaborator with radical Nationalist/ neofascist genocidal dreams ("We will not be replaced!"), as Teitelbaum suggests. In tense situations such as these, the fieldworker can be easily caught in a double bind, which I think happened to the author in this case. It would be inappropriate and a waste of time for him to try to "convert" his subjects, but to join forces with them in the spirit of "artificial" solidarity is distasteful.

The current revival of fascist tendencies in Europe, the United States, Latin America, the Philippines, and elsewhere is posing a real threat to democratic values. Solidarity with extreme right populist leaders, engaging white dreams, fear-making, anti-immigration demonstrations and cozying up with male white supremacy, provoked by Nationalist nostalgia for a "world on the wane," is a major threat to the idea of the imperfect but democratic "Open Society" that George Soros has spent his life fighting for and is now the victim of anti-Semitic hatred.

As ethnographers during these difficult times, we must try to engage in honest political struggles and to begin deep, rather than dark, conversations and encounters across the cultural, psychological, symbolic, historical, and geographical borderlines. Our minor role as anthropological ethnographers and modest witnesses compels us to contest and resist exactly what Benjamin Teitelbaum is doing—using ethnography as an excuse for political solidarity with extremist white Nordic Nationalists and white male supremacists in the name of empirical science that makes the anthropologist an apologist for Trump's lies and hatreds.

Stacey Vanderhurst
International and Interdisciplinary Studies, Women, Gender and Sexuality Studies, University of Kansas, Blake Hall Room 323, 1541 Lilac Lane, Lawrence, Kansas 66045, USA (vanderhurst@ku.edu). 1 X 18

I work in Nigeria, where villains abound. In popular thinking and in much academic writing, kleptocratic politicians, armed robbers, and international oil executives conspire to seize wealth without regard for injury to others. In the Yoruba proverb made famous by Nigerian author and photographer Teju Cole, "Every day is for the thief." These figures populate novels and Nollywood films, and they reflect how people make moral sense of the ordinary dangers they face.

Good ethnography, however, has no easy villains.

I study Nigeria's human-trafficking policy, and it is common practice for scholars to challenge the villainization of traffickers that is so often taken for granted in media and legal discourses. Anthropologists reclaim traffickers as "travel brokers" and describe their work as mentors, patrons, and protectors. Like Teitelbaum, we "stir curiosity" by shifting the terms and values by which these people may be understood. But unlike Teitelbaum, this work bolsters political projects many anthropologists support, like the decriminalization of clandestine migration.

Informed by these politics, it is rarely migrants and their agents who appear without sympathy in critical anti-trafficking scholarship. Instead, it is those in power, who reproduce the policies and worldviews that anthropologists contest. "Were scholars to conduct research in opposition to those they study, they would likely compromise their ability to adopt some of the most effective research techniques in the same instance," Teitelbaum warns. Worse than sacrificing these tools entirely, ethnographers "in opposition" deploy them poorly. Contempt renders fieldwork both personally unmanageable and empirically untenable. The resulting ethnographies fall flat: state officials appear as drones, elites as unfeeling marauders, and pastors as charlatans.

The method proposed here avoids those tropes by insisting upon forms of mutual exchange that hold the researcher accountable to those they research, no matter their status or politics. It allows researcher and research subject "to become interested in each other as people." In other words, it humanizes them. Teitelbaum notes that this process risks normalizing dangerous ideas, whether in more empathetic scholarly depictions or in purposeful advocacy pieces written in public outlets. Indeed, this has been the major criticism of similar journalistic endeavors.[19] This risk is worth taking seriously. However, there is a difference between humanizing subjects as a means to making better, richer arguments about political movements and social phenomena and humanizing subjects as the only agenda or outcome of research.

19. See especially the reactions to Richard Fausset, "A Voice of Hate in America's Heartland," *New York Times*, November 25, 2017.

As an end in itself, humanizing hated groups is not only morally dangerous. It is also intellectually boring. Teitelbaum defends the necessity of this project—this method—for its potential to contribute new knowledge. But worthwhile projects must exceed the mere documentation of groups and their beliefs, no matter how misunderstood they are. From the days of Edmund Leach, anthropology has dismissed the empirical taxonomies of difference as mere "butterfly collecting." The significance of new knowledge rests not on its novelty or on technical corrections of the public record. Rather, it must have potential to cut through existing assumptions that have real impact on our worlds.

It is up to the researcher to make that case, both in choosing lines of inquiry and in representing those results to the public. That expectation should not require explicit political applications; I suspect many anthropologists might cringe, as Teitelbaum does, at the idea that ethnography must be translated to immediate political projects to matter. Still, we have discretion. To obtain funding, to publish articles, and to earn book contracts, we must demonstrate the value of this knowledge to the public. It must have real stakes. If humanizing traffickers helps us better understand why migrants depend on brokers despite the great risks involved, humanizing other stakeholders helps us understand how shared values and sensibilities sustain border policies that render migrants so vulnerable in the first place. This conclusion matters to the public not only because elites are people deserving of dignity. It demonstrates how legal formulations interact with popular thinking, how internal tensions explain intervention changes over time, and how systems of oppression operate despite the individual or collective virtue of those involved. This insight need not serve a particular political project or set of interests in order better explain the system of criminalization we live with and the consequences it has in our lives.

Here I find Teitelbaum's argument most dubious, as he anticipates. If better understanding is the goal of this kind of work (the "unparalleled capacity to learn from and with those studied"), is direct "advocacy," as he terms it, inherent and necessary to that project? He describes the urge to serve nationalists' interests, as friend and collaborator. It seems unlikely, however, that his informants' expectations oblige him to do so. Unlike collaboration modeled on kinship or cultural stewardship, friendship specifically provides space for a transparency of purpose and honesty of exchange that would recognize a researcher's objections to a given political project. It ought to establish the bounds of solidarity, as well as its mandates. Without them, we confuse friendship with unity, a political form of "passing" that fetishizes informants' stories by removing them from the consequences we understand them to have. It normalizes their ideas rather than explains them.

Ultimately, ethnography is no better suited for heroes than it is for villains. Friendship, empathy, and love are often important parts of fieldwork experiences, but we should resist the accompanying desire to depict our subjects in exclusively flattering terms, however natural it may be. I am often surprised at how readily scholars of human trafficking critique the tropes of ideal victimhood, only to reproduce other impossible ideals in their accounts of vigilant sex worker-activist. Radical in her politics and brazen in her resistance, she is "righteous enough," in Teitelbaum's phrasing. These depictions are both empirically inaccurate and analytically misleading. In its holistic and reflexive narrative traditions, ethnography is uniquely positioned to do better—to reconcile the admirable and the despicable, the extraordinary and the mundane, our friends and their faults.

Reply

Although it may not always be apparent, the seven commentators and I agree on much. When they rehearse the dangers of far-right activism and my participation in spreading its propaganda, and when after weighing many factors they find that my actions cross the line of morally defensible behavior, they are not issuing a retort, for I declare my approach immoral from the outset. Fundamental disagreement with my argument instead seems to center on the status of immorality in ethnography, whether lack of moral consistency is inherent to our ideal methodologies as I claim, and, if so, where we go from there.

To begin with, I must clarify what I mean with "solidarity." In my article I describe finding myself motivated to collaborate with, and occasionally advocate on behalf of, nationalists, but not nationalism. When toward its conclusion I affirm my compliance with an older version of the AAA code of ethics, it is the imperative to treat as paramount the interests not of ideologies but of people—to "support them, to care for their well-being, their dignity, their pride," to quote Professor Scheper-Hughes.

The distinction is not airtight, and I did not suggest otherwise. My case studies show how actions I took in the name of interpersonal commitment and affection benefited the broader political cause. The outcomes are at times immoral. But as Professor Bell clarified, to join informants' political causes would be incompatible with my effort to assume a liminal, trickster-like position as an ethnographer. It would instead render the scholar an unthinking conduit, one who indeed fetishizes the thoughts and words of some and whose research would lack even the prerequisites for dialogic qualities that I characterized more than once as essential to ethnographic knowledge. This leads her to assign my approach not to what she and Lederman label an anthropological paradigm of ethnography centered on "grasp [ing] the native's point of view," but rather to a sociological paradigm defined by scholars' willingness to "break with members' perspectives."

Terminological distinctions aside, the point anticipates much of my reply to other respondents. In her thoughtful and nuanced comment, Professor Stacey Vanderhurst observes

that neither ethnography nor friendships benefit from a lack of critical reflection and exchange. Professors Bangstad and Scheper-Hughes make similar remarks. It is often with those whom we trust and care for most that earnest evaluative conversation can take place. Do friends not commit to accepting each other as they are—is friendship not a union that can withstand disagreement?

I described two conceptions of friendship in ethnography: one involving the formation of conventional friendships in and beyond the field, the other a model that mimics friendship, adopting it as a preferred metaphor for relationships in ethnography opposite a metaphor of surveillance. We should not think of the two as equally theorized; my impression is that the latter recognizes something the former does not, namely, that friendships with informants in ethnography are more transparently instrumental than others. They are not ends in themselves; they are also—sometimes instead—a means to an end of gaining information and understanding. When that ceases to be the case, we may still have friends, but not informants.

As I ponder Professor Vanderhurst's words and my own reaction that the relationships she describes seem untenable, it leaves me wondering whether the second form of friendship plays a larger role in my research than I first realized. Granted, I am not entirely in agreement with the terms she put forward. The idea of friendship as a space for unconditional exchange is an idealization: it is in my experience rare for confrontation and criticism to occur between friends without some anxiety. When criticism does come, it takes a different shape than it would between people who do not feel obligations and affection toward each other, let alone lack of contact or desires thereto. We criticize friends while also believing in and knowing their goodness, while valuing the relationship and investing in its survival. It assumes admiration and insight into the dynamism of the person while also entailing social and emotional risk. In the case of friend-informants, that risk extends to include the fate of research, further compounding the strain of confrontation.

Nonetheless, I do criticize my informants. And I wrote about it in the essay. More than one commentary seems unaware of those sentences.

My critical analysis of nationalists is undoubtedly tempered, but as I explained in the essay, this has to do both with interpersonal relationships and with considerations of the context in which my work will be read. Contrary to what Professor Bangstad implies, I do not think (and did not write) that scholars bear a responsibility to defend their informants. I spoke out only after encountering inaccurate claims about individual nationalists in a particular milieu where unwarranted accusations are not held accountable. The fact that in my case the controversy dealt with friends made my words more impassioned and more informed than they otherwise might have been. Similar considerations drive my choices regarding what to include in scholarly texts, including the essay above. Compared with criticisms, my affection and alle-

giances toward informants are more distinguishing of my method and have more to offer a discussion of ethics, morality, and method in ethnography. In other scholarship, I have occasionally chosen to downplay or repackage criticism in order to counteract a specific challenge, namely, that readers often approach research on radical nationalism with extensive stereotypes and an urgent need to confirm their distance from those studied, all of which I consider detrimental to the task of learning. Humanizing for the sake of humanizing can, as Professor Vanderhurst writes, be a boring and sappy exercise. But in certain situations it can also be shocking and transformative.

Concern with reception drives my choice of terminology, and these choices also garnered the attention of commentators. Professor Peter Pels's review of my essay is lush with productive, inspiring criticism. Like Professor Bell he is skeptical of my decision to label my approach immoral, preferring instead to describe it as the prioritization of "scientific integrity over moral consistency." He was led to that characterization by the concluding discussion in the essay where I write that the practices of collaboration, reciprocity, and friendship weaken our ability to mold the profile of our projects. Ethnography unleashes a state of moral uncontrol (epistemic uncontrol too, as he points out), and we can expect outcomes to encompass more than mere immorality. I agree and could not help but occasionally use language to that effect both in the essay and in this reply. However, accepting moral inconsistency requires little effort unless it is inconsistency of a particular kind, hence my language and augmentation.

Professor Pels is also correct to note that formal and informal guidelines may be blind to the many ways in which anthropological research may do good (and bad, I presume). Such considerations are missing from my essay. I discussed the ways friendships in the field impacted me but not how they impacted my informants. There are stories to tell of their experience that parallel mine, stories of their bending their dogmas and risking their social standing in the service of our relationship. I suspect that in several cases dialogue prompted them to reconsider facets of their thinking, just as it did for me. I avoided writing about the topic for fear that it would distract and too easily be used to escape a reckoning with methods and morality for which I was striving. It would be wrong to equate the potential for moral virtue to live in unpredictable and unmeasurable ways with the loud public righteousness of our official statements and professional discourse, but we might find ourselves tempted nonetheless.

Although I direct my argument to ethnographers at large, I am pleased to read the comments of scholars of the European radical right, and I appreciate in particular Professor Hübinette's efforts to place my work in a Swedish context. His criticism of my approach, however, not only returns to the non-question of my moral defensibility but also inserts positivist critiques of intimacy in the field. He writes that my relationships with informants "compromise" my pursuit of knowledge, suggesting that friendship produces a bias unbecoming of serious

scholarship. Perhaps I misread here, for in the next breath he outlines dangers particular to nationalists, implying that the problem may be not friendship at large but friendship with bad people—the compromising not of insight but of morality. Let me state again that my stance on these issues diverges from Professor Roy D'Andrade's objectivism. It is in the rejecting of distance and the embracing of intersubjective epistemologies that I see potential for an immorality worth tolerating.

As Professor Hübinette notes, my approach is far from standard in the field of far-right studies, and that fact underlies another of Professor Bangstad's criticisms. There is richly informative scholarship about this political scene that does not adopt the practices of collaboration, friendship, and reciprocity I champion. Am I not shortchanging these studies, all but asserting the arrogant claim that projects that parallel my approach will be superior to those that do not?

Referencing a similar statement by Charles Hale, I write that "few champions of collaboration, reciprocity, activism, or friendship claim that their strategy represents the only pathway toward deepened understanding" yet continue to describe these practices as ideals for gaining access to an ethnographic knowledge distinguished by its intersubjective insights. I did not always include those qualifiers to my argument—perhaps they should have appeared earlier in the essay—but they are in the text and they do matter. To call a method an ideal—even indispensable to a certain type of knowledge—is not to treat it as the exclusive measure of finished scholarship. As to whether I effectively leverage my method to these ends, I would refer commentators to other works of mine that focus on radical nationalism rather than research methods and morality, recognizing that such a review may be interesting but is ultimately inessential to the argument I make here.

Professor Bangstad also questions my nomenclature. "Radical nationalist"—my umbrella term for the political actors and movements I study—may seem euphemistic. But when I remove the qualifier "radical" for stylistic considerations, am I not pushing the threat of linguistic normalization beyond a critical threshold? Perhaps. As I wrote, though, experience tells me that labels matter less when we study ideas and agendas in depth. Even this article, which does not focus on analyzing ideologies and personalities, seemed to suffice. My language beguiled none of the respondents into thinking that I study uncontroversial people.

I would be disingenuous if I did not acknowledge that I found Professor Scheper-Hughes's commentary troubling. Much of the story can be told through a bookend reading of her text and the ominous coupling it reveals. At the beginning she describes being unable to understand my argument. By the end, newly confident, she makes the startling claim that I am "using ethnography as an excuse for political solidarity with extremist white Nordic Nationalists and white male supremacists."

She certainly has relevant criticisms to offer in between, about my choice of language, about the inevitability of solidarity as I describe it, and about the ethnographer's role as interpreter and critic. Other respondents voiced the same criticisms, though notably they arrived at different conclusions. Meanwhile, many of her other claims are inaccurate or misleading. The careful reader will recognize that her words about the fieldworker's moving in and out of translation are reminiscent of mine, as are her comments about the importance of dialogue; that my activities as I describe them here are not confined to the field but extend into writing; that I discuss criticizing my informants; that my words about the benefits of difference rather than sameness in ethnography are incompatible with assimilation into my informants' worldview; that my "long and unnecessary" overview of ethical codes (seen as clarifying and useful in a comment above by an expert on the topic) does not describe a "wavering" commitment to research participants, a matter of no small importance to my argument; and that I explicitly dismiss the notion that reciprocity, collaboration, and friendship are the only ways to gain deep understanding of our informants. Other claims were confusing. I was surprised by what appeared to be her celebration of "ethnographic neutrality" and find the timing suspicious. I likewise struggled to see the point of her discussion of Gaddy Tauber: if they agreed that they do not trust each other, she may not be well positioned to learn about, interpret, and criticize his life.

An accounting would not be complete without examining her epitaphs, the first of which implies that I, too, am attempting a depraved assertion of moral equivalency among political ideologies—as though my descriptions of my informants' ideas and acts as "immoral" do not nullify the comparison. My speculation that one can find immorality in any case study is hardly an endorsement of such a crude relativism. Still, perhaps I am missing something. Perhaps Professor Scheper-Hughes was drawing attention to my having found something likeable in those I studied, something that led me to call them, yes, "very fine people" even as I labeled their ideology and acts immoral. Perhaps she contemplated my distinction between solidarity with the person versus the ideology and demurred. But then what about the second epitaph: is it supposed to represent my thoughts or hers?

To the extent that the worn expression "politicization" means something to me, it is as a label of politically motivated intolerance of ambiguity. I think of that as I read Professor Scheper-Hughes's response. My argument urges an unmooring of our self-image as global crusaders for justice and accepting a drift in the opposite direction. How far in the opposite direction? The other respondents and I disagree, and many suggest that it would be unwise to offer any single answer. I sense that for Professor Scheper-Hughes, an architect of the so-called moral turn and the systematic intertwining of ethnographic methods and moral agendas, the discussion is irrelevant. It seems she affords no gray space between virtue and vice. Maybe that is why she cannot rest having declared the essay merely "confusing and contradictory."

She ends with a call to action, "to contest and resist" my ostensible efforts to make "the anthropologist an apologist for

Trump's lies and hatreds." If only it were that simple—just a matter of my harboring a contemptible set of values, as Professor Scheper-Hughes here, and Bangstad elsewhere (2019:99), charge. That would reassure those holding to visions of an orderly anthropology, one where our research faithfully reflects our personal convictions, and thus one where messy conversations about methodology can be folded into the safe and predictable territory of political debate.

—Benjamin R. Teitelbaum

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