

This article is an invited comment on “Collaborating with the Radical Right: Scholar-Informant Solidarity and the Case for an Immoral Anthropology” by Benjamin R. Teitelbaum, published in 2019 in *Current Anthropology*, volume 60, issue 3. The final version is available via the publisher’s website at the following link: <https://www.journals.uchicago.edu/doi/abs/10.1086/703199>

Solidarity and immorality or empathy and ambivalence?

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Over 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 shouldn’t 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’s 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 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: 275-76) 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 towards 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” (p. 25, emphasis 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 in press). 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, for example, 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 doesn’t 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 weren’t 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 isn’t advocating solidarity in the sense of shared goals or interests (the dictionary definition of the term), but empathy, respect and understanding, which isn’t 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’s possible to sustain this arguably classic position (now shorn of any positivist threads) when studying up. Nader (1972) didn’t seem to think so, because she made it clear that this new subject matter would require new methods – including a de-privileging of participant observation as our *modus operandi*. However, I’m with Teitelbaum that ethnographic practice offers “unparalleled capacity to learn from and with those studied”. What I’m 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’s 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 doesn’t 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).

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