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# Redefining ethnography «in the strict sense of the term»

## Perspectives gained from non-standard, old-new fieldwork

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**ABSTRACT:** This essay reflects on the consequences for ethnography of the reversal of the direction of the anthropological encounter – what exactly happens when anthropologists from the Global South undertake fieldwork among populations in the Global North? I will argue that this does not merely correct historical imbalances of anthropological knowledge production, thereby changing the substance of that knowledge itself, although of course that is a goal worth pursuing, as has cogently been pointed out by the "world anthropologies" critique of hegemonic disciplinary practices. Using insights drawn from my own research site in the US heartland, where I conducted fieldwork initially in 1989-91, and then again during a "focused revisit" in 2015, I suggest that lasting misunderstandings between the anthropologist and the research participants are themselves a rich source of data and human possibility. My contention is that non-standard and long-term fieldwork of this kind may help reveal dimensions of the practice of ethnography that would otherwise remain obscured.

**KEYWORDS:** ETHNOGRAPHIC FIELDWORK, MALINOWSKI, US HEARTLAND, ANTHROPOLOGY IN REVERSE, FIELDWORK REVISITS.


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On 9th November 2016, dejected and confused, I phoned the three people I knew most closely from my 1989-91 fieldwork in Southern Indiana to make sense of the outcome of the US election. Two of them were former coal miners, one had been a social worker. They had all voted for Obama twice, and were all sympathetic to Sanders. In the 2016 general election, however, they had each voted for a different candidate. One of the out-of-work coal miners had voted for Clinton in an attempt to prevent Trump from coming to power, as many people did. The former social worker realised that Trump was going to sweep the state of Indiana anyway, so decided to vote for Green Party candidate Jill Stein, whose politics were nearest to his own. The third person, also a former coal miner, had voted for Trump and said that he was embarrassed to tell me so. He felt that Hillary Clinton was «a very flawed candidate» and he did not know what else to do. He repeated several times that the world that lay ahead with a Trump Presidency was very «troubling» to him. But, he said, don't you remember that when we first met more than twenty-five years ago, you had said that there is no essential difference between the Democrats and the Republicans. In the dialectical relationship that obtains between ethnographers and their research participants, where each is trying to make sense of the other, it can take quite a while to see how an ethnographer herself/himself appears to these interlocutors. Quite apart from the issue of the durability of mutual misunderstanding, it is worth noting that, as much as anything else, a considerable quantum of our discipline's curiosity, and indeed its promise, is also directed at the question of what research participants make of the fieldwork encounter.

As anthropological orbital paths have changed and the discipline has stopped revolving around the standard issue white male British anthropologist with a double-barrelled name, it may be becoming more possible to see the dark side of the ethnographic moon, to perhaps learn more about ethnography itself. This article will in the first place reflect on the consequences of reversing the direction of the anthropological encounter by having anthropologists from the Third World work among white Western populations. What is the outcome of such a role reversal, of this non-standard form of ethnographic fieldwork? What is it that happens when anthropology's planetary circuits change course? I will argue that it does more than merely correct historical imbalances of anthropological knowledge production, thereby changing the substance of that knowledge itself, although of course that is a goal worth pursuing, as has cogently been pointed out by the «world anthropologies» critique of hegemonic

disciplinary practices (Ribeiro, Escobar 2006). Thus, I am trying to claim that such scholarship goes beyond gathering the new kinds of perspectives and data that only «studying up» (to use Laura Nader's phrase) can uncover. It may in fact help reveal contours of the practice of ethnography that would otherwise remain obscured.

To shift into a different metaphor, going against the grain of the colonial encounter can rob the anthropologist of the cloak of expertise and prestige that comes from being a (typically reluctant) emissary of a dominant collectivity in the global order. It is important to recognise that there are specific things that this nakedness enables one to discover. Anthropologists have reflected previously on fieldwork by postcolonial anthropologists in other postcolonial spaces. Writing in *Granta* in 1986, Amitav Ghosh mused about an argument that had erupted between himself, an Indian anthropologist in the making, and an Imam in rural Egypt, where Ghosh was conducting his fieldwork. Provoked by the Imam, Ghosh was astonished to find himself making boastful assertions about India's bomb-making capacities, insisting that they were superior to those of Egypt. «So there we were, the Imam and I, delegates from two superseded civilisations vying to lay claim to the violence of the West» (Ghosh 1986). A beautiful re-interpretation of this incident by David Scott makes the argument that Ghosh, as a postcolonial anthropologist investigating another postcolonial context, was seen to be making a bid for the «empty space of power» of the West, and it is this move that was being resisted by the Imam (Scott 1989).

Moving away from postcolonial places, what are the implications for the practice of ethnography when the anthropologist-delegate from one of these «superseded civilisations» undertakes fieldwork in the US heartland? Does completing the ethnographic circuit in this manner reveal something about ethnography itself? This formulation of naked ethnography – ethnography in the strict sense of the term – is an allusion, of course, to Malinowskian exposure (Malinowski 1989), and to the assertions made by several commentators that Malinowski's diary must be read together with the ethnographic accounts he published in order to understand the full import of his work (Clifford 1986; Rapport 1990). It also looks back towards Donna Haraway's formulation of «situated knowledgs», and attempts to heed her words of caution. «"Subjugated" standpoints are preferred because they seem to promise more adequate, sustained, objective, transforming accounts of the world. But how to see from below is a problem requiring at least as much skill with bodies and language, with the mediations of vision, as the "highest" technoscientific visualizations» (Haraway 1988: 584).

Stemming from this, there is a related note of caution regarding language. For all that this essay makes its arguments in a first person voice, it is important to underline that these reflections strictly pertain to anthropological fieldwork, do not interrogate realms of personal experience separate from fieldwork, and are concerned with what we can learn about ethnography itself from non-standard fieldwork over the long term. This is not auto-ethnography, I would argue. Denielle Eliott laments, «like Malinowski's *Diary* left abandoned on our office bookshelves, layered with dust from neglect, we are still absent in many forms in our own ethnographic writing, there but not present. We continue to construct ourselves as the invisible, neutral observer» (Eliott 2014: 155). Conversely, in making ourselves manifest in the context of our fieldwork encounters, we are only following the longstanding, if inadvertent, Malinowskian tradition. It is patently ethnography «in the strict sense of the term».

I first arrived in the town of Newburgh in Indiana to begin my fieldwork in the world-changing year of 1989. This was the year that marked the momentous end of the era of the Cold War, celebrated ever since in media images of the crumbling of the Berlin Wall. I stayed there until the summer of 1991, after the local Fourth of July parade had celebrated the US victory at the end of the Gulf War – the first large-scale conflict of the new world order. The initial framing of my project drew heavily on anthropological political economy perspectives, an approach which contrasts profoundly, of course, with Malinowski's own conspicuous silence regarding the imperial subjection of the people of Melanesia. As Joan Vincent notes (and Gupta and Ferguson subsequently pick up on this), Malinowski presented the Trobriand Islanders as completely isolated, even though they were frequently subjected to «blackbirding», being among the estimated one hundred thousand Pacific Islanders who were abducted and forced into indentured labour (Vincent 1990; Gupta and Ferguson 1997). That this is not a presentist allegation to level against Malinowski is evident from the contemporaneous publication of W.H.R. Rivers' *Essays on the Depopulation of Melanesia* (Rivers 1922).

I certainly had no wish to overlook the shaping role of world historical forces in the context of my fieldwork. Starting from the premise that cultural processes in the United States cannot be adequately understood without grappling with the symbols, structures and practices of contemporary capitalism, I had arrived with a particular interest in the defining material conditions of the area. In the immediate vicinity, these were determined principally by the flagship Alcoa (Aluminum Company of America) plant that

had stood outside of Newburgh since the early 1960s and the surrounding coal mines that had lured it there in the first place. As a woman scholar from India, I had initially been drawn to the project by the paucity of studies of Western societies by anthropologists from my part of the world. I realised only later that spending nearly two years in the American heartland at the end of the twentieth century has taught me invaluable and unlovely truths that may have been less accessible elsewhere or at another time. I went back to Newburgh again for further fieldwork in the summer of 2015, and am again back in touch with the majority of the research participants I had known decades ago.

The findings that I had stumbled upon during the 1989-91 period of fieldwork – arguably at a key moment in the process of systemic transformation – and had revisited and discussed with research participants in 2015, are concerned with the nature of work, collective memory, masculinity and nationalism. They seem enormously significant today for this is, of course, classic Trump country as per the standard depiction – with out-of-work coal miners and factory workers inhabiting a landscape of vanishing opportunities and a labour movement seemingly in its death throes.

To return to the question of anthropology in reverse, does it truly describe my 1989-91 research in Indiana? I was a relatively penniless graduate student from India subsisting on a modest fieldwork grant but I came from a middle class family in India, and it was not unlikely that my long-winded educational journey would eventually bring me to middle class shores. The men who were the focus of my research – and it was men primarily – were white men engaged in classic Fordist occupations, who described themselves as middle class, of course. For these were the days when middle class status was claimed and ascribed as readily in the US as the unexamined term "white working class" is used today (for a critical exploration of this new usage, see Mathur, Kasmir 2018). However, the division between manual and mental labour was plainly visible and acknowledged, and the resulting unconscious adherence to class scripts and cues is quite astonishing. Transcribing a taped conversation with an electrician at the Alcoa plant, I squirmed to hear myself complete a sentence for him. The tape captures my immediate apology as I noticed what I had done, and then this response from him: «No, don't apologise. You said it better than I would have». This is not to say that class deference inevitably defined my interactions in Indiana, nor that I was a completely tone deaf fieldworker. I am merely trying to complicate a blanket claim of anthropological role reversal.

Yet, at the same time, I was a distinct oddity – a stranger from India studying in a little known university in New York, both places distant, unknown, and considered somewhat threatening. Moreover, I was a woman, which fact drew harassment so dire that I was forced to change my address three times in my first six months in the field. I have a clear recollection of anger as well as fear during one of the first of these incidents, as I cowered indoors waiting yet again for the police to arrive in response to my call about an inebriated male neighbour hammering on my front door late at night. Anger that nobody (including myself) had anticipated that sexual harassment would emerge as a major issue in the wholesome Midwest of the United States. As we were setting out for fieldwork, I did not receive the same warnings and sound practical advice as a fellow graduate student who was told to be mindful of the difficulties she would encounter as a blonde North American researcher in the Middle East. So powerfully ingrained are the assumptions that mark out certain world regions as misogynistic and others as relatively enlightened that I had arrived quite unprepared for these distressing incidents. The gender-specific risks of fieldwork – one of the usually unspoken «intimacies» of our discipline – is finally receiving attention, for example, in the form of the American Anthropological Association's newly adopted policy on sexual harassment and sexual assault[1].

These difficulties eventually subsided when I was able to rent living space within a family home, and effectively live as a member of a local family. I was able to settle in to the point that I had the luxury of experiencing the sense of claustrophobia that most fieldworkers recognise from Malinowki's diary, the suffocating fear that life and fieldwork will eventually merge and that escape to a familiar world will become impossible. This was, of course, much more justified in Malinowski's case – his fieldwork being in many ways a brave face put upon the crushing fact of exile – than it is for most of us subsequent followers of the methodology he pioneered.

In general, during my years of fieldwork in Indiana, my potential middle class status was more than offset by the fact that I was a woman of colour. The cloak of prestige and expertise still largely eluded me during my summer fieldwork of 2015, by which time I had in fact become a member of the global middle class, as a tenured academic in Ireland, a European country towards which middle Americans are generally well disposed. To the people who did not already know me, yet another implausible location had been added to

1. See http://s3.amazonaws.com/rdcms-aaa/files/production /public/AAA- _SH_Policy_2018.pdf.

the string of far-off places I was associated with – Ireland now, as well as New York and India. Since I could no longer be conveniently slotted as an «exchange student», I encountered new varieties of mistrust and avoidance.

Unhampered by prestige, I also found that certain kinds of knowledge are shared only with outsiders and pariahs. This was especially noticeable during the first Gulf War – an exceptional moment for the consolidation of my status as an outsider. It unexpectedly drew people towards me who wished to confide their unease with warfare without making a public stand. Thus, I heard from a local high school teacher that he had attended an anti-war march in Washington DC, a fact that he carefully concealed from his colleagues and students. Immediately after a multi-denominational church service that turned into a call for holy war, I received an anguished phone call from a minister who had officiated at the event but disagreed profoundly with what was being said there. Painful as it is to do this kind of fieldwork, it is also exceptionally rewarding in terms of the perspectives it makes available.

There is a further reason for suggesting that an already difficult research methodology should be made more challenging. The second thread that I wish to follow speaks to the peculiarly contemporary relevance of long-term ethnography. Electoral politics around the world has rewarded hate-mongering, nativism and majoritarianism in recent times – as in the election of Narendra Modi as India's Prime Minister in 2014, and the Brexit vote and the Trump victory in 2016. It is also noteworthy that these are comfortably compatible majoritarianisms that publicly express admiration for each other. This frightening new world arrived early for those of us attuned to developments in India, when Modi won the general election in 2014. His party and their thinking has gained ground still further since that time. This has led to reverberations throughout the Indian polity – from the public lynchings of Muslim men for allegedly eating beef by crowds of Hindu right-wing «gaurakshaks» or «cow vigilantes», to the killings, threats of violence and administrative and litigational harassment of writers, journalists, academics and university students deemed to be anti-national.

The xenophobia and anti-minority sentiment that underlies these developments – the «fear of small numbers» in Arjun Appadurai's words, and its accompanying «surplus of rage» – is profoundly at odds with an anthropological sensibility, which exults in difference and enthusiastically seeks out the unfamiliar (Appadurai 2006). In a polarised world rife with violence and majoritarianism, anthropologists will perforce find themselves among group-thinking populations driven by fear and hate. Are our empathetic methods of research well suited to this endeavour? Can

ethnography enable us to understand those with whom we fundamentally disagree? Or could it be the case that even when we do not understand except at a purely descriptive level, the very process of this kind of anthropological research itself becomes part of the movement towards mutuality? This is a plea, of course, for long-term and committed anthropological fieldwork, since temporal depth yields data drenched in relationship, or «intimacy», to use the lingua franca of this symposium.

Hannah Arendt has written scornfully about empathy, «as though I tried to be or to feel like somebody else» (Arendt 1983: 241), but she valued sympathy, or fellow feeling. This is a potentially useful distinction to make in the context of challenging fieldwork among unsavoury others. Writing from the European context, Gingrich and Banks reckon that «it is more likely that anthropologists have avoided fieldwork with neo-nationalists not because they have been unable to gain access but because they do not wish to – for the sake of moral hygiene rather than to avoid bodily endangerment» (Gingrich, Banks 2006: 7). Where fieldwork of this kind has been attempted, there is little agreement about the content of the research process. Drawing on her experience of collecting oral histories from former members of the Ku Klux Klan, Kathleen Blee argues that classic feminist research techniques based in egalitarianism and empowerment are out of place when researching hate groups. On the other hand, Susan Harding argues for the re-categorisation of her Christian fundamentalist research participants: «Why are the margins in studies of culture not occupied equally by politically sympathetic and repugnant cultural "others"?» (Harding 1991: 392). Where others might hear distasteful, even distressing, utterances from research participants, Harding hears the cadences of a separate language. To get within earshot of «born-again language», she recommends «standing in the gap between conscious belief and willful unbelief, in a place I call «narrative belief» (Harding 2001: xii). An anthropologist commencing or returning to fieldwork in the conservative Midwest has to calibrate her/his own research strategy.

When I went back to Indiana again for further fieldwork in the summer of 2015, having been out of touch with the majority of the research participants for long years, I miraculously rediscovered almost everybody (and this is logistically more difficult now given that people no longer use landline telephones and I do not use social media). It was surprisingly easy to fall back in step again with the people I had once known. To my astonishment, more than one person said something along the lines of «Oh, it's good to see you again – I was talking to someone about you just a few days ago». An encounter with one such individual helps to make the broader points

regarding ethnography that I wish to discuss here. In an essay about the ethnographic technique of the «focused revisit», Michael Burawoy notes that it brings up for researchers «the dilemmas of participating in the world they study – a world that undergoes (real) historical change that can only be grasped using a (constructed) theoretical lens» (Burawoy 2003: 645). This encounter attests to the durability of mutual misunderstanding in the ethnographic context, but simultaneously points to possibilities latent in anthropological fieldwork.

I had been living in the Bible belt at a key moment in the politicisation of right wing Christianity in the US in 1989-91. I spent a lot of time at a conservative local church, since its Minister was, astoundingly enough, a thoughtful, left-leaning individual who helped to assuage the cognitive isolation that had afflicted me during my early days in Indiana. I grew quite close to one of the families at this church – an Alcoa hourly wage worker, his wife and small son. Unlike their Minister, they were traditional religious conservatives, and I remember one occasion when the wife, Helen, and I met for lunch and the talk turned to the fact that I was not a Christian. I explained that I thought that it was perfectly possible to be a good person without being a Christian. Am I not an example of a basically good person, I asked her, without being a Christian and without wishing to be one? Helen grew quite emotional, saying that I was fine as a person, but that she was worried for my afterlife because she was so fond of me. I responded with a logical question. Mahatma Gandhi was a good man. He knew about Christ and Christianity, but he did not convert. Did she think he was suffering in hell in consequence? As may be imagined, this led to a very prolonged lunch. Helen said she did not know enough about doctrinal matters to say for sure what Gandhi's afterlife experiences might be like, that this was a question for more knowledgeable religious people. I was trying to break down for her the automatic identification she made between morality and Christianity, trying to say that good people came from many different religious backgrounds, or none at all. My persistent point throughout this conversation was – isn't it possible to be non-Christian and still be good, non-Christian but not forever doomed?

It was an intense discussion, and when we met again after twenty-five years, we both remembered the occasion vividly. However, we both remembered it in diametrically opposed ways. I remembered it as I have just described it. She interpreted my persistent query quite differently. When I said – isn't it possible to be non-Christian and still good, non-Christian but not forever doomed? – she read it as a desperate plea, as an expression of a

sort of «salvation anxiety» on my part. Although I had been trying to unsettle the certainties of conservative Christianity that she brought to our encounter, as well as its prescribed politics, she was successfully able to fold my remarks into her world-view. It is important to admit that this world-view has remained largely impermeable to me for over twenty-five years. To truly understand it, it is necessary to believe in it wholeheartedly. Hochschild's call for climbing across «empathy walls» notwithstanding (Hochschild 2016), anthropology's intersubjective methods of research only take us so far.

The predicament of mutual unintelligibility is not newsworthy in and of itself. We have all had our statements and sentiments misunderstood at one time or another. The context of anthropological fieldwork is quite specific, however – the anthropologist inserts herself/himself into the field as a means of collecting data. The body and the subjectivity of the anthropologist are themselves the research instruments. The areas of misunderstanding are themselves immensely useful social data. In the encounter that I have described, it does not matter that Helen and I are so constrained by our worldviews that we read radically opposite meanings into the same utterance. It does not really matter that we will probably never understand each other's standpoints. What is significant is that we are in dialogue and will continue to remain so. That is the slender thread of consolation that long-term ethnography offers in a deeply polarised world. These narrow and enduring chinks of dialogue enabled by committed fieldwork, which may be rendered by ethnographic writing into brief sparks of illumination extending in both directions – it is these that will always set anthropology apart from other forms of knowledge-making, especially when the goal is to understand the dynamics of majoritarianism.

This is the kind of anthropology that is under threat in the age of «audit cultures» and neoliberal universities. As anthropologists comply and churn out articles based on hit-and-run fieldwork and dump them on academia.edu to maximise citations, we are turning our backs on what our discipline truly has to offer in these frightening times.

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