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Failing in the Reflexive and Collaborative Turns: Empire, Colonialism, Gender and the Impossibilities of North-South Collaborations

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DESIRÉE POETS, APR 9 2020

**This is an excerpt from *Fieldwork as Failure: Living and Knowing in the Field of International Relations*.
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I am a half-German, white, cis settler who was born and raised in Brazil and was working with urban indigenous and black communities there. At the same time, I was institutionally located as a Ph.D. candidate in Wales, in the UK. In such a project, questions of ethics and accountability were imperative. They quickly led me to self-reflexivity – the practice of being transparent and reflexive about one’s positionality in the field and how it affects the research design and process, and to collaborative methods – which, in the words of Himika Bhattacharya (2008, 305), are about ‘doing ethnography “with” people rather than “on” or “about” people, with a purpose of bringing about positive change in the lives of the researched’. Despite the important contributions that such methodologies make to ethical and accountable research, I incurred certain failures in this process that were both my own and part of academia more broadly. I will focus here on two such failures. The first was my falling (or failing) into what D’Arcangelis (2017) called ‘the White settler fantasy of transcending colonialism’. The second was my failure to openly reflect on sexualised encounters and sexual harassment in the field.

The story I tell here, although it is built somewhat chronologically, should not be read as a linear progression towards ‘better’ understanding and ‘better’ research practices. The fieldwork and my Ph.D. as a whole were an uneven process of theoretical and empirical trial and error with no guarantees. They included experiences that I was only able to articulate and name after the fact, and difficulties or impossibilities of which I may have been intellectually aware, but only comprehended on a more affective level after experiencing them myself.

Two occasions made my failures evident. The first was during one of my returns to Brazil to share my analysis with the groups with whom I had worked. During one such meeting, a female member of a *quilombo*, a history teacher and postgraduate student, rolled her eyes at me and challenged the overly academicist focus of my presentation. What she wanted instead were political strategies that would support the community. In response, I put my notes down and quickly rethought my experiences in a different light to answer her question. But, I thought, why were *those* findings not already central in my Ph.D. dissertation? To what extent was the Ph.D. itself ultimately useful to the groups? The second occasion was when my then Head of Department contacted me about a prize for ‘research impact’ and asked whether I could demonstrate any direct impact of my work and apply for the prize. Leaving aside the problems with the ‘research impact’ agenda in the United Kingdom, the truth was that I could not demonstrate any. The only tangible outcome of my collaborations had been my Ph.D. title and, more recently, an early academic career in the US. The struggles and realities of the communities in Brazil remained more or less unchanged by my project. This is a very real material outcome of my collaborations, which repeatedly put me face to face with the extractivism of my work – an extractivism which collaborative and reflexive research methods between North and South run the risk of renewing and re-legitimising.

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The first limit to my collaborations was my research design. For example, I had applied to the Ph.D. programme with a project I had designed on my own. Working between three cities and with four groups, I had limited time to collaborate with each. And my central research question, the anchor of any International Relations dissertation, namely 'Who counts as indigenous?', was a white, settler question. As to my second failure, besides my research design, gendered and sexualised encounters also shaped and limited my collaborations. Sexual harassment, for example, stopped me from engaging more fully with one of the groups. At the time, I tried to ignore these encounters, not mentioning them to my supervisors or colleagues, and left them out of my dissertation. This is a common response of female researchers who experience harassment in the field. Returning to these experiences for this chapter, I therefore understand these failures as my own, but also as part of the racialised, heteropatriarchal, and colonial foundations of the academic industrial complex (Stein 2018; Grosfoguel 2013).

Despite decades of critical and feminist critiques, these foundations continue to invisibilise sex, gender, and the body in knowledge production (Hanson and Richards 2019), normalising and/or obfuscating sexual harassment both in the academy and in the field. They can also co-opt reflexive and collaborative research methods to reproduce racial/colonial hierarchies in seemingly more benign forms, replacing structural transformations in the academy and the society in which it is located (here) with collaboration, solidarity, reflexivity, and activism in the field (out there) only. Yet, the field and the academy are continuations of one another, so that our failures here are our failures there. I am not alone in this discomfort and the dilemma that such North-South engagements bring in the era of the decolonial, post-colonial, reflexive and collaborative turns. Sam Halvorsen (2018, 12) describes this discomfort as an 'ethicopolitical moment' that has led many, myself included, 'to revisit the otherwise implicit value of learning from [and, in my case, collaborating with] the South'.

Coming to the Reflexive and the Collaborative Turn

My undergraduate degree in International Relations did not include methods training. When I came to research methods and design courses during my Master's, these were either taught from an overtly theoretical, critical realist perspective, or were University-wide courses that did not go in depth into specific methods. I came from a theory-heavy department in a discipline that already did not prioritise ethnographic fieldwork with marginalised groups, and in which indigeneity and race were – and still are – invisibilised. Although I had engaged with indigenous and feminist methodologies through, for example, Linda T. Smith (1999) and Gayatri C. Spivak (1988), I only came to feminist ethnography after the end of my first year as a Ph.D. student, when I had already designed my project and fieldwork.

Feminist ethnography is still one of the most productive sights of fieldwork training and theorising for those of us engaged in research with minoritised groups. Emerging in the 1970s and 80s, it has been concerned, alongside matters of gender and sexuality, with questions of voice, representation, objectivity, and power in the research process. A diverse field, it unpacks how research is always marked by the researcher's positionality (the intersections of race, gender, class, religion, sexuality, institutional location, and so on) in relationship with the researched (Haraway 1988; Visweswaran 1997). Feminist ethnography centres 'the basic political issue at the heart of most anthropology – the issue of Western knowers and representers, and non-Western knowns and represented' (Abu-Lughod 1990, 11). Part of the West/non-West dichotomies, as Abu-Lughod (1990) argues, are issues of self/other that contain hierarchies of power that force us to pay attention to the political implications of knowledge production.

One of the most helpful lessons of feminist ethnography is its attention to self-reflexivity, the practice of being transparent and reflexive about one's positionality in the field and how it affects the research design and process. While self-reflexivity was already part of my research, feminist ethnography helped me deepen it in practice. Throughout my fieldwork and writing process, I engaged with the task of reflecting on how the intersecting differences of class, colour, institutional location, gender, and others were shaping my experiences in the field – although, as I discuss later, I excluded any reference to sexual harassment in my final drafts. I was, however, also mindful of how Western academia had been historically instrumental to colonialism and Empire, fixing the non-European world as its object of study while extracting knowledge from it. Besides being reflexive about this reality, I wanted to avoid reproducing these dynamics in my own 'home' country.

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To this end, I contacted all groups with the explicit offer 'to make myself useful'. This offer was gradually taken up: I organised a community's archives, helped with a community's online presence, and fulfilled other small tasks I was asked to do. I also began to write for an NGO-run activist journalism platform, often in defence of the communities with whom I was working. It was through this activist research practice during fieldwork that I came to the literature on the collaborative turn (Bhattacharya 2008; Lassiter 2005) at the end of my second year, when trying to make sense of the solidarities I was building on the ground. That was the summer of my final longer fieldwork trip, after which I was supposed to start the write-up phase and final year of my Ph.D. That literature pushed me a bit further and, during that summer, I agreed with all the groups that I would return before and after my Ph.D. defence to present my findings, allow for their interventions, and to report on the defence.

Feminist ethnography, such as the work of Richa Nagar (2014), also reminds us that the inequalities implied in the intersecting differences that make up our positionalities cannot be done away with in the field, including in collaborative and solidarity work. Such work is therefore marked by inevitable impossibilities and difficulties, and the ever-present risk of epistemic violence. I was aware of these conditions of possibility, and yet – as will become clear in the next section – I was not prepared for their effect on my scholarship. During some of the 'return' meetings it became clear that the limits of any collaboration in the field were already set by the very design of my Ph.D., including the central research question, and that the actual contents of my dissertation were only marginally interesting and not immediately useful to the communities.

On Not Dismantling the White, Settler House

Sometimes when I tell this story, my interlocutors try to rescue me by pointing out how much I have done to address the issues I raise here and/or read it as an expression of individualising and even navel-gazing white guilt. Yet what I found myself embedded in were, quite to the contrary, power structures that mould academic knowledge production and in which self-reflexivity and collaborative methods are entangled. These structural entanglements persist despite my individual efforts to address them, as I discuss below. There was a moment in my fieldwork when my embeddedness in these structures was made clear to me, and which could have, or should have, given me pause. When I was contacting groups in Rio during my first year, I spoke to a maroon leader who started our conversation by asking numerous questions about my race, religion, foreign language skills (beyond Portuguese), and my ability and availability to help him with projects for the community. After my answers, he quickly said 'no'. I respected that 'interview process' and his decision, but, in my anxiety to complete the Ph.D. requirements, I also tried to 'move on' quickly, focusing on not thinking about his refusal too much. Yet, this failure to gain access was important in ways that I only later began to comprehend.

Today, I understand his refusal as telling me that my 'good intentions' and 'perseverance' could not undo my structural position and the asymmetry of our potential collaboration, one which he was not interested in reinforcing or validating. Although this thought did cross my mind when 'access' was denied to me, I did not linger with the effects of my inevitable complicity. Part of me still, perhaps subconsciously, believed that through self-reflexivity, collaborative methods, and solidarity-building I could make my research more benign, less problematic, or somehow not as implicated in racial/colonial structures – if only I were given the chance (see D'Arcangelis 2017, 350). I fell into the trap of what Carol Lynne D'Arcangelis (2017), in her own critique of self-reflexivity, called 'the White settler fantasy of transcending colonialism' (340). This fantasy is tied to a desire for innocence which, as she puts it, is anchored in 'modernist/liberal imaginings of a subject capable of transcending structural power inequalities' (342). Even though I was intellectually aware of the arguments otherwise, I had not lingered with the *affect* of that complicity until I reached the write-up phase, when I had to retreat from more activist initiatives and systematised my work in a way that felt entirely out of context.

My project, too, was extracting value from the Global South and my own 'home' country. This came in the form of both symbolic capital (a doctor title) and material capital through my Ph.D. funding and, later, an academic position in the US. I would even argue that it was precisely my collaborative and feminist methods in the Global South that set me apart in the competitive academic job market – an uncomfortable illustration of the structural contradictions of my work. I found myself removing the communities' local knowledges from their immediate political contexts – which Sam Halvorsen (2018) identified as their use-value for grassroots movements – to introduce them into Anglophone

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academic circuits, where they accumulate exchange value in a global academic labour division that privileges scholars in the Anglophone world (Halvorsen 2018). It was unclear what exactly the groups or even Brazilian academia were gaining from this, despite my 'local' activism and solidarity. In the incisive critique of Judith Stacey (1988, 23), 'the lives, loves, and tragedies that fieldwork informants share with a researcher are ultimately data, grit for the ethnographic mill, a mill that has a truly grinding power'.

Moreover, and beyond my individual career and material gains, how were my good intentions in the field actually re-legitimising the Western academy more generally, making it only seemingly more benign? To paraphrase Tuck and Yang (2014), how were my collaborative methods and self-reflexivity masking a political economy that reproduces racial/colonial power relations to allow the Western academy to accumulate more and more territory in the Global South? There was no self-reflecting and collaborating out of these dynamics. Having said this, my admission here does not undo my complicity either and deeper self-reflexivity will not resolve it. As Sara Ahmed (2004, point 4; emphasis in original) put it, 'the work of critique does not mean the transcendence of the object of our critique; indeed, *critique might even be dependent on non-transcendence*'. Keeping with this theme, Andrea Smith (2013, 266–267) also reminds us that such moments of confession can constitute rather than challenge the settler/white subject through the 'raw material of the Native':

A typical instance of this will involve non-Native peoples who make presentations based on what they 'learned' while doing solidarity work with Native peoples in their field research/solidarity work. Complete with videos and slide shows, the presenters will express the privilege with which they struggled. We will learn how they tried to address the power imbalances between them and the peoples with which they studied or worked. We will learn how they struggled to gain their trust. Invariably, the narrative begins with the presenters initially facing the distrust of the Natives because of their settler/white privilege. But through perseverance and good intentions, the researchers overcome this distrust and earn the friendship of their ethnographic objects.

In this act of self-reflexivity, the white/settler subject comes into being through how they affect the Other – the white settler's self-reflexivity is made possible through the Other (D'Arcangelis 2017; Wasserfall 1993). This is an ever-present risk in my work, and it is not to dismiss self-reflexivity or individual acts to address asymmetries. It is to recognise that, as *individual* acts within wider structures, they remain complicit. It is not to replace individualised with structural actions either, in a move that dismisses the role of the individual scholar in reproducing systemic conditions. It is to see the individual researcher and the structure as entangled, and therefore to see transformations on one level only as limited without transformations on the other.

Gendered and Sexualised Failures

Empire and settler colonialism are also co-constituted through heteropatriarchal gender relations (Simpson 2016; Simpson 2017; McClintock 1995), which equally shape the academy. There (or here), assumptions of the 'standard' researcher or scholar as still white, cis, male, and usually from the West have also informed how ethnographic methods have been developed, despite decades of critical and feminist methods, as Richards and Hanson (2019, 25) have argued in their timely qualitative study with women fieldworkers. They show how these assumptions have given rise to standards for what counts as 'good ethnography' – ethnography that still often invisibilises gender, sex, and the body in the field, especially for researchers of colour and LGBTQIA+ researchers. This is why, they argue, discussions of sexual harassment in the field remain circumscribed to feminist circles, leaving women who experience harassment with an 'awkward surplus', stories 'which can be both difficult and risky to fit into our findings and theories [and that] become superfluous stories, excess that must be cut to get to the 'real' data' (Richards and Hanson 2019, 2–3).

These standards take the form of 'ethnographic fixations' that can place researchers in harm's way. Such fixations include solitude, the assumption that ethnography is an individual endeavour; danger, which includes the ongoing glorification of dangerous ethnographies; and intimacy, the idea that 'good ethnographies' are ones that stem from intimate relationships between researcher and researched (Richards and Hanson 2019, 28–39; see also Introduction, this volume). During my time as a Ph.D. student, I would pendulate between wishing I could replicate the fieldwork experiences of my male colleagues of bonding with (often other male) 'informants' in the field, which

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seemed to regularly take the form of excessive drinking, and those of female researchers of building relationships of trust, solidarity, and intimacy with (often female) individuals or groups. Cases where a researcher may not be in the place to build such intimacies or where her safety is not assumed seemed limited to research with extremist (e.g. right-wing) groups or situations of war and conflict (see Sriram et al. 2009). Discussions of such dynamics within *progressive* movements and in 'everyday life' are still relatively uncommon, although as soon as the topic emerges in a group of female researchers, the stories begin to flow.

As is still regularly the case, leaderships in the social movements with whom I was working were mostly male. In one case, they were all male. Looking back, I suspect that I was granted 'access' to this group, in part at least, as a result of my being a young, female student, and therefore not taken seriously or read as in any way 'threatening'. Gendered, racialised, and sexualised relations can therefore both enable and hinder one's research and have divergent effects on the researcher's 'access' and safety, depending on their positionality. For example, there were also situations in the field where I felt safer for being a white, foreign-looking woman, such as during protests. There was one case, however, in which my research with a social movement was marked by a series of uncomfortable comments, encounters, and interviews that led me to retreat from deeper engagement. One such situation was when I was invited to see a movie in which many of the group members had acted with the explicit confirmation that it would be a group outing, only to find myself on a one-on-one 'date'. Another was when I accepted a lift after an informant had interrupted an interview suggesting we 'go to lunch instead', during which he asked repeated questions about my love life – which is not uncommon for female researchers (see Freitas et al. 2017). I ended up having to jump out of the car as soon as we came to a red light because, without explanation, he simply passed where I was supposed to be dropped off.

After these incidents, I avoided one-on-one interactions and agreed to attend only public and collective meetings, thereby closing off possibilities for solitude, danger, and intimacy. Yet, I did not directly confront the men involved in these incidents, trying not to be 'unpleasant', and allowing several subsequent degrading or sexist comments about me to pass. I was scared that reacting to these may jeopardise my access. My collaboration with them was therefore more limited and ended up taking the form of newspaper articles in support of their movement. I also did not include a discussion of these incidents in the final draft of my Ph.D., failing to reflect on how these interactions shaped my findings. Part of the reason for this was that it seemed inappropriate to disclose them in a dissertation, also for their potential to reinforce racialised stereotypes and to centre me and my whiteness in the project as a victim. While occupying a relatively vulnerable position as a woman, I was also in a position of privilege in terms of my class, race, and institutional location. These shifting power relations could not be easily resolved, so I chose to not discuss these incidents at all.

Another reason for not discussing them was that I felt that, because I 'knew' Brazil and was 'used to' the sexism 'there', these encounters were unremarkable. Why would this 'everyday sexism' be worth writing about? I could almost hear my mother, herself with many accounts of sexual harassment 'at home', saying to me: *Well, what did you think was going to happen?* It was just part of life. Yet, this 'part of life' is missing from usual accounts and training guidelines on collaborative and activist methodologies, which obfuscate more complicated and difficult solidarities. Although gendered and sexualised dynamics in the field are being increasingly written about (Bell, Caplan, and Karim 1993; Freitas et al. 2017), also with attention to race (Berry et al. 2017; Hanson and Richards 2019), the literature on ethnography still assumes either a 'racially privileged male anthropologist' (Berry et al. 2017, 538) or a relationship of trust and safety between researcher and researched that enables 'radical vulnerability' in solidarity-building (Nagar 2014).

The obfuscation of sexual harassment in the field, as Hanson and Richards (2019) observe, is inseparable from academia's own patriarchal and sexist structures, which dismiss sexual harassment allegations in the still male-dominated academy itself – something ongoing denouncements in several academic circles have shown (see Ahmed 2017). Sexual harassment in the field hereby becomes 'a "given," just one more hardship worth navigating to gather good data' (Hanson and Richards 2019, 2). My failure to address harassment in the field (*there*) is therefore also tied to academia's own power relations (*here*). Nonetheless, Hanson and Richards add that the lack of training on gendered fieldwork dynamics is part of the 'colonialist legacy of ethnography', with its roots in colonial and Imperial expansion. This is expressed in 'the assumption that researchers can somehow stand above and beyond the

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community they study' (21), in a renewal of expectations of disembodied, observant neutrality. The writing out of such gendered and sexualised encounters in the field is both a product of and a means of reproducing this colonial illusion of neutrality that persists despite the turns to self-reflexivity and feminist as well as critical methodologies – which, generally, continue to be marginalised in most fields, including (or perhaps especially) in International Relations.

Conclusion

In his critique of recent calls and movements to 'decolonise the academy', Andile Mngxitama (2018) asks, 'is a decolonised University possible in a colonial society?' His answer is 'no'. Efforts to transform the academy, he states, must be tied to efforts to transform society. Similarly, neither are decolonised research methods possible in a colonial society and University. Our collaborative and activist work in the field (*out there*) equally fails if it is not continued in efforts to address the colonial, racial, and heteropatriarchal designs of the academic industrial complex and academic knowledge production (*here*). This includes transformations in and beyond the classroom that challenge the ongoing lack of representation as well as the dominant norms and standards for what counts as 'good ethnography' that obfuscates how ethnography is inevitably embodied and, therefore, racialised and gendered. Within this, they also reproduce the historical invisibilisation and marginalisation of researchers who do not conform to standards of whiteness, heteronormativity, and masculinity.

Relatedly, as I enter my early career and continue to work through what it means to do politically meaningful research, I have come to the following guiding question: As Postcolonial and Decolonial Theory as well as feminist and collaborative methods seem to gradually become accepted in the mainstream, how will we make sure that these movements are not reduced to means through which academia aims to re-invent itself as only seemingly more benign? In responding to such moves to decolonise the academy, the Brazil-based indigenous thinker Ailton Krenak (2019) described them as a haemodialysis, in which colonial academic institutions increasingly run by market logics 'take someone else's blood to keep on working' while we all remain 'immersed in Coloniality' as environmental and political crises continue to unfold. This is the ever-present risk that my collaborative and self-reflexive methodology must navigate. My response to this dilemma has been to turn my efforts to collective action across differences (see hooks 1986) in my current region and institution, alongside my ongoing activism in Brazil, to gradually contribute to the structural change necessary for my methods to be effective. There is a long road ahead, but guiding me, here, are the words of Andrea Smith (2013, 264):

...individual transformation must occur concurrently with social and political transformation. That is, the undoing of privilege occurs not by individuals confessing their privileges or trying to think themselves into a new subject position, but through the creation of collective structures that dismantle the systems that enable these privileges.

I finish here with six acknowledgements to my younger self that may be helpful to fellow fieldworkers:

1. No matter your positionality, your fieldwork is embodied. Pay attention to how your body is shaping your fieldwork: What spaces does it open up for you, and how, and which ones does it close off? Whom are you drawn to, and why? Who is drawn to you, and why?
2. Put your safety first. If you have a gut feeling that something is amiss, you are probably right. Reflect on when the 'ethnographic fixations' of danger, solitude, and intimacy, which Richardson and Hanson (2019) discuss, are potentially putting you in harm's way.
3. The academy, the field, and the societies in which both are nested are continuations of one another, not boundaries.
4. This means that your scholarship is entangled in wider structures of the academic and non-academic world(s). No matter how much you try to address these entanglements, you cannot undo them on your own. We don't come to the field innocently. Think about what this means for your scholarship but also what this means for structural and collective transformation that cannot be reduced to your scholarship or one site only.
5. Let the scholarship and leadership of historically marginalised thinkers and fieldworkers guide you. As the most affected by hegemonic norms and standards, they are the experts and will shine light on where to go.

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This includes but goes beyond a politics of citation.

6. Finally, you are not alone in the challenges, dilemmas, and failures of fieldwork. Find and build those solidarities, for they will carry you forward. It's a path full of contradictions, complicities, and mistakes – but it doesn't have to be a lonely path.

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