

The long shadows cast by the field: violence, trauma, and the ethnographic researcher

STEPHEN TAYLOR



Taylor, S. (2019) The long shadows cast by the field: violence, trauma, and the ethnographic researcher. *Fennia* 197(2) 183–199. <https://doi.org/10.11143/fennia.84792>



As more geographers utilise ethnographic methods to explore pressing contemporary issues such as abandonment, precarity, and resilience, they enter into research environments often defined by social marginality and violence. There are emotional and psychological risks associated with embedded research in such contexts, however these challenges have largely been ignored in existing methodological literatures. A frank debate is needed about the emotional and psychological burden that ethnographic research can exact upon lone researchers and how these burdens interface with researcher identity and positionality. Drawing on a reflexive analysis of the author's experience of fieldwork in South Africa, this paper highlights the emotional consequences of conducting ethnographic research with marginal groups in dangerous contexts. It specifically examines the ripple effect of exposure to traumatic events that culminated in the author's diagnosis with post-traumatic stress disorder (PTSD). In so doing, the paper draws attention to the acute emotional and psychological consequences of ethnographic research, while also challenging prevalent professional attitudes within the neoliberal university that promote the downplaying or silencing of such repercussions. The paper concludes with a series of suggestions for how (early career) researchers, our discipline, and institutions might better promote and realise an ethic of collective care for field researchers.

Keywords: ethnography, emotions, mental health, subjectivity, post-traumatic stress disorder, South Africa

Stephen Taylor, School of Geography, Faculty of Humanities and Social Sciences, Queen Mary University of London, Mile End Road, London, E1 4NS. United Kingdom. E-mail: stephen.taylor@qmul.ac.uk

Our fear, doubt, grief, rage, horror, and detachment, our shivers and shakes, and our paralysis and frenzy lay bare our humanity when we are confronted with the cruelty, despair, and suffering that humans can inflict on each other.

(Markowitz 2019, 2)

Introduction

In *Out of Our Minds: Reason and Madness in the Exploration of Central Africa*, Fabian (2000) brings to the fore the many people, objects, and emotions stifled in the “heroic” travelogues and ethnographies of the nineteenth century. Significant in these journeys, Fabian (2000, 9) uncovers, were “the effects of alcohol, drugs, illness, sex, brutality, and terror.” In documenting the often chaotic and panicked nature of early ethnographic knowledge production, Fabian exposes how the silencing of trials and tribulations was a fundamental part of the discursive production of peoples and places as objects of knowledge (Clifford & Marcus 1986). It is testament to Fabian’s contrapuntal acuity that it has now become something of a truism to begin reflexive accounts of fieldwork with an assessment of similar methodological silences within extant accounts of the field. This has become a common refrain for multiple generations of early career geographers frustrated by what they see as a long-standing inattentiveness to the practical and intellectual challenges of fieldwork (Mandel 2003; Billo & Hiemstra 2013). It is encouraging, then, to see how researchers from across the discipline of geography increasingly respond to these provocations with detailed and constructive accounts of (overcoming) the multifarious challenges posed by fieldwork, including negotiating “access” to closed institutions, managing the dynamics of cross-cultural interviews, and defending the study of illicit spatial practices (Belcher & Martin 2013; Turner 2013; Zhao 2017; Dekeyser & Garrett 2018).

Despite this clamour for greater methodological transparency in the discipline, the *emotional* challenges of geographical fieldwork have historically received less attention (Widdowfield 2000; Punch 2012; Warden 2013; Drozdewski & Dominey-Howes 2015). “Geography,” Davidson, Bondi and Smith (2005, 1) contend, “has often had trouble expressing feelings” and has consequently “tended to deny, avoid, suppress or downplay its emotional entanglements.” Despite an “emotional turn” within the discipline, researchers’ emotions and anxieties often remain buried, underacknowledged, or repressed in accounts of fieldwork in particular (Anderson & Smith 2001; Davidson *et al.* 2005; Pile 2010).

In this paper, I reflexively examine my experiences – as a white, cisgender, heterosexual, European, male researcher – of conducting ethnographic doctoral research in urban South Africa. I begin by bringing to the fore my own experiences of the many forms of emotional investment that ethnographic research necessitates. Emotions, after all, draw us to certain topics, flow through our research relationships, and “define the contours of the multiple worlds that are inhabited by different subjects” (Ahmed 2004, 25). I then explore the feelings of vulnerability and insecurity that affected my fieldwork by interrogating how acts of violence directed at both research participants and myself undercut my sense of self and assuredness. Finally, I turn to the lasting impact of this fieldwork on my personal and professional lives. I examine my post-fieldwork experiences of post-traumatic stress disorder (PTSD), the burden of mental ill-health on an ethnographer, and the emotional silences that I perpetuated to stand out from peers and further my career. I conclude with recommendations on how individuals, our discipline, and institutions can better prepare and care for field researchers.

Emotions silenced, traumas downplayed

The current and ongoing silencing of emotion within geographical accounts of field research, identified as a concern by Davidson, Bondi and Smith (2005), can be traced back to two key influences. First, the pressurised and antagonistic neoliberal university is considered to be hostile to emotion. Attempts by geographers to “write vulnerably” (Behar 1997, 16), for example, have been decried or dismissed by colleagues for demonstrating an “excessive emotionality” (Mehta & Bondi 1999, 75). The prevailing view of emotionality is as a symptom of powerlessness: “to be emotional is to have one’s judgment affected ... it is to be reactive rather than active, dependent rather than autonomous” (Ahmed 2004, 3). The dismissal of emotion is typified for many in the use of managerialist research assessments that deride reflexive ethnographic writing as little more than “annotated fieldnotes” (Jeffrey 2012, 505). Consequently, the emotional dimensions of field experiences are downplayed by those reluctant to have their peers, managers, or assessors “penalising emotion” (Ratnam 2019, 24; Dominey-Howes 2015; Caretta *et al.* 2018). Beckett (2019) suggests that this marginalisation and rejection of researcher personhood is a form of “cruelty that masquerades as intellectual rigour.” As such, there remains much

to be done to raise awareness of the emotional impacts of ethnographic research within an increasingly neoliberal university system that does not reward – and, often, actively penalises – such emotionality.

A second, and undoubtedly linked, reason for the silencing of emotion in accounts of the field is the prevalent masculinist epistemology of the discipline. Geographers are frequently encouraged to overcome personal anxieties around fieldwork, sometimes in potentially violent settings, by heroically and intrepidly embracing the “throwtogetherness” of research (Fraser 2012). Such a hubristic stance promotes the silencing of researchers’ concerns about safety, avoids questions of disability/impairment, and does not challenge the presumption that stoic determination in the face of adversity is a hallmark of good research. In comparison, non-academic professionals working in such contexts receive well-defined training on how to manage the high emotional cost of their work with traumatised individuals in times of “crisis ordinariness” (Berlant 2011, 10).¹ The preparedness of ethnographers entering similarly unpredictable settings ought to be a significant professional concern, particularly as data points to an increasing incidence of mental health issues in this group, but disciplinary preparation for the emotional impacts of fieldwork remains uneven (Woon 2013; Calgaro 2015; Loyle & Simoni 2017; McGarrol 2017; Markowitz 2019). It is also important that we foreground attempts to challenge such limited preparation by acknowledging the continued prevalence of a masculinist epistemology within the discipline (as well as the university more broadly) that frames discussion of emotion as a “feminine” form of weakness. The field researcher is too often assumed to be an emotion-less witness, akin to the output-maximising researcher lauded by the neoliberal university. This masculinist conception of the stoic geographer normalises certain experiences of the field, mockingly dismisses those who could not “cut it,” and leaves researchers inadequately prepared for differentiated field experiences (Pollard 2009; Billo & Hiemstra 2013; Coddington 2015).

Against such a disciplinary and institutional backdrop, one could question how far the discipline has come from the deadened accounts of exploration unpacked by Fabian. There are, thankfully, exceptions to the tendencies outlined above, particularly from a growing number of women geographers breaking the silence around the emotional challenges of the field (Drozdowski 2015; Klocker 2015; Coddington 2017; Eriksen 2017). Calgaro (2015), for instance, reflects upon the feelings of disorientation and horror that accompanied her longitudinal research on Thailand’s unfolding traumascapes in the wake of the 2004 Indian Ocean tsunami. Jokinen and Caretta (2016) reflexively examine their experiences as young women postgraduate researchers with (invisible) disabilities and discuss how isolation, fear, ableism, and gender-based violence encountered in the field undermine disciplinary assumptions about researcher mobility and well-being. Sundberg (2005) explores how the intersections of race, gender, and sexuality shape researcher vulnerabilities and responses to tensions arising in the field. These accounts, rooted in a critical feminist epistemology, acknowledge *the differentiated emotional effects of fieldwork*. However, with notable exceptions (e.g. Dominey-Howes 2015) and acknowledging the importance of gay, queer, and trans fieldwork perspectives (e.g. Browne & Nash; Kedley 2019), the intersection of gender, masculinity, and emotion remains relatively underexplored in accounts of fieldwork.

As England (1994, 85) notes, “the researcher cannot conveniently tuck away the personal behind the professional, because fieldwork *is* personal.” I use this statement as a provocation to discuss the personal consequences of my own fieldwork and, importantly, to counter male geographers’ emotional silences to date. My intention in the rest of this paper is to contribute my own autoethnographic reflections on doctoral research within the neoliberal university to the burgeoning interdisciplinary and geographical literature on emotion, violence, and researcher positionality outlined above. Before I continue, however, it is important to emphasise that all researchers encounter the field differently. I am cognisant here of efforts to deconstruct the unproblematised EuroAmerican “we” of critical human geography (Jazeel 2016). While I discuss my experiences in what follows, I do not claim authority to speak definitively for others who – owing to their own positionality – may have had similar or different experiences. Instead, the aim of the following discussion is to promote an appreciation of the often underestimated emotional and psychological toll that research can exact upon all researchers, and to identify lacunae in guidance and practice that could be addressed to better prepare those conducting ethnographic research in the future.

The research project

My doctoral study was the first major research project that I conducted as a lone researcher. The project, funded by a competitive studentship, focused on the globalisation of clinical trials and the challenges posed to trial recruiters of making pharmaceutical knowledge in and between new national jurisdictions (Petryna 2009). In January 2010, I relocated to South Africa from the United Kingdom for 15 months to study efforts to enrol marginal urban populations into first-in-human clinical trials. This was my first time in South Africa. The field location was chosen because of the burgeoning rate of trial recruitment rather than any familiarity with the country on my part. That I received competitive funding to do research in a country that I was entirely unfamiliar with is reflective of the ways in which our discipline disproportionately rewards doctoral studentships to white, male candidates from “prestigious” EuroAmerican institutions with often “edgy” but untested field ideas (Desai 2017).

In South Africa, I based myself in Cape Town’s peripheral townships where I examined the tactics used by recruiters – known as “body hunters” (Shah 2007) – to enrol the precarious urban poor into trials. I conducted ethnographic research alongside recruiters and human subjects in a key recruitment hotspot: the Khayelitsha township. Khayelitsha, meaning “our new home” in the Xhosa language, is located 35 kilometres south-east of the centre of Cape Town and is a sprawling mix of formal and informal housing with high levels of unemployment and poverty (see Fig. 1). The township was established in 1985 through the forced relocation by the apartheid regime of black residents from other overcrowded areas of the city. Physical and social mobility was severely restricted through the operation of the segregationist 1950 Group Areas Act and successive Pass Laws requiring “non-white” South Africans to present official documentation when entering “white” areas. The township has grown rapidly since the end of apartheid as a result of migration to urban areas by those in search of work and better opportunities.



Fig. 1. A Khayelitsha street scene with informal corrugated iron shacks densely packed together on the sandy dunes of the Cape Flats. Photo by author.

Violence is an everyday reality for many Khayelitsha residents and shapes a local politics of fear (Meth 2009). Recorded incidences of murder, rape, and aggravated robbery in the Khayelitsha police precinct were ranked within the five highest reporting precincts nationally during my time in the area (South African Police Service 2012, 11). Against this backdrop, local residents have often been excluded and silenced within popular and academic discourse through epistemologically violent subject positionings that have categorised the entire township as an irredeemable space.

Investing in the field

"The field," as Sharp and Dowler (2011, 151) have argued, "is part of a much bigger institution." In particular, universities play a significant role in preparing graduate students for field research through the provision of intellectual support, mentorship, and access to insurance protections. Despite requests to both my academic department and the broader university, however, no specific training to prepare pre-fieldwork students for the physical and emotional demands of time away from the university was made available. Instead, I was encouraged to reflect on and anticipate risks via my health and safety risk assessment and to discuss this with members of the departmental panel. A risk assessment was undertaken at the commencement of the project, in consultation with the generic university safety guidance, and was accepted without amendment within the department. The majority of the thousand words or so of text that I contributed focused on likely risks to my physical health, including natural disasters, exposure to disease vectors, and travel at night. The form did not encourage reflection on risks likely to impact mental health or well-being, and these were not raised as concerns by the scrutiny panel (Eriksen 2017). While the bureaucratic assessment was necessary to gain insurance cover and to have the work signed-off within the department, the risk assessment form was never discussed again.

Following news of the risk assessment approval, a member of the faculty recommended via email that I should "drop everything, get onto the first plane you can, and move into the townships; going *immediately* is the only way to finish the PhD and a couple of publications in three years." Here, at an early point in my graduate studies, career and publication considerations were introduced as factors that senior colleagues felt should shape the field decisions made by a young researcher. These considerations shaped my initial framing of the field arguably more than the anticipation of dynamic, contingent, and uncertain risks (Houghton & Bass 2012; Berg 2015; Webster & Caretta 2019). I moved to South Africa two months after the risk assessment and I set aside several weeks in my initial fieldwork schedule for acclimatisation.

Ethnographic research that moves beyond mere participant observation necessitates a prolonged, personal, and proactive investment in people and places. I had cursorily considered the initial stresses likely to be experienced by any individual plunged into a milieu with which they are unacquainted. I attempted to reduce the impact of this "culture shock" by registering on a Xhosa language course at a local university, and I also used the university as a base from which to contact potential research participants in the clinical trials industry. My initial attempts at accessing researchers and companies involved fraught negotiations, compounded by the secretive nature of much pharmaceutical research, and I appreciated having a familiar classroom environment to retreat to after long mornings of frustrated emails, unanswered calls, and denials of access.

As part of my research into trial recruitment strategies, I sought to examine the everyday lives of those living in areas being targeted by "body hunters." Despite my colleague's suggestion of bravado in approaching Cape Town's townships, I knew that accessing these communities would be demanding. Developing contacts made on my language course, I became affiliated with a community health non-governmental organisation (NGO) in Khayelitsha and this became a valuable space to acclimatise to living and working in the area. The organisation was also a vocal critic of trial recruiters operating in the township, many of whom recruited individuals out of support groups and health clinics organised by the NGO. I participated in many NGO activities, including co-facilitating safe sex workshops and helping to organise several actions against local government underinvestment in community health services. Later, at the request of several local young men who had attended the workshops and heard me speaking about my interests, I hosted a series of informal drop-in

photography classes in the NGO office's meeting room. The men saw photography as a potential employment opportunity, and we spent several afternoons and evenings photographing scenes across Khayelitsha (Fig. 2 and 3). These classes allowed me to scope the local environment and practice my Xhosa click consonants and phonemic tones with a group of unofficial guides who identified places of interest. Investing in dialogue- and rapport-building initiatives was an emotionally draining, time-consuming, and yet ultimately rewarding process, as it allowed me to explore a wider area and access a larger number of research contacts.



Fig. 2. A discarded tourniquet – eerily similar in shape to the red HIV/AIDS international awareness ribbon – found in a shack frequented by intravenous drug users. Photo by author.



Fig. 3. Assorted pieces of wood and cardboard make up the wall of an informal shack in QQ Section (Tambo Park), Khayelitsha. Photo by author.

The personal emotional investment necessary for meaningful ethnographic access was undoubtedly more challenging because of my position as a white, male, European researcher in a partially informal, black-majority South African township. A sizeable body of work within geography and allied disciplines has explored the intersections of gender, masculinity, and researcher positionality (Meth 2009; Evans 2012). The gender of male researchers, this work suggests, influences the power dynamics of a research situation and this distortion is amplified further by other axes of difference. Despite the petty frustrations and occasional embarrassment stemming from my far-from-perfect command of Xhosa, I was considered to occupy a position of privilege by my research participants owing to the colour of my skin as well as my perceived wealth (exemplified, for instance, by my digital camera). For Rose (1997, 307), this privilege entails “greater access both to material resources and to the power inherent in the production of knowledges about others.” Cognizant of these unsettling aspects of privilege, I embraced my position as a reverent learner who was keen to hear more about everyday Khayelitsha. The dynamics of these relationships evolved as research participants chose to disclose more of their experiences of township life with me.

My time with the NGO was helpful in introducing me to the sites and spaces frequented by trial recruiters. However, I understood that basing myself in the relative safety of their compound risked distorting my view of trial recruitment by focusing solely on those companies seeking to recruit through extant public health programmes (Le Marcis 2004). To gain a broader perspective on the spectrum of sites and methods used by recruiters, I had to access more marginal sections of Khayelitsha. I rented a room in the house of a church pastor within the QQ (Tambo Park) section of Khayelitsha’s Site B neighbourhood. Tambo Park is one of the most underserved areas in the city, built on squatted land underneath high-voltage electricity pylons owned by Eskom (the national electricity utility), with few toilets and limited access to piped drinking water. This move was part of a conscious strategy aimed at understanding the processes of violence and poverty in underserved areas that drove many to consider high-risk trial participation as a rare source of sizeable remuneration. From this location, I was able to explore the vitality of township life that had largely been ignored in extant accounts of Khayelitsha.

The pursuit of such valuable perspectives was frequently hindered by acute localised violence. For example, I made regular trips into sections where released members of Cape Town’s feared Number prison-gangs (“26s,” “27s,” and “28s”) exert control through terror, extortion, and brutality:

I’ve had lots of nightmares after yesterday’s visits. One of the guys that I know from [the NGO] told me that a young girl’s body was found near his house. She would not accept R30 (€2) for sex, so she was slashed with a panga knife and pelted with stones. Apparently, that is the accepted rate for a life. (Excerpt from field diary, 16 August 2010)

As I visited with respondents in these remoter sections of Khayelitsha, black male gang members – suspicious of unknown white people asking questions on their territory – often grinned at me and slowly drew their index fingers across their necks to mimic the cutting of my throat that, they intimated, might be my fate. While this may be explained away as part of a “theatrics of intimidation” (Pratt 2008, 767) that deliberately accentuated colonial configurations of racialised threat, it was now clear that my affiliation with the NGO was no protection against the threat of a knife attack. I considered my (white) body to now be a source of vulnerability, or a marker that I was unwelcome and – in some way – out of place (Parr 2001; Sharp 2005).

Shocks to self

Negotiating the shifting terrains of violence in places like Tambo Park is a significant emotional challenge for those ethnographers struggling to make sense of violent social worlds. While objectivity and emotional distance are important in research, they should not be a pretext for inertia when confronted with distress nor used as an absolution for silent acquiescence in the face of oppression (Hart 2006; Markowitz 2019). Listening to research participants recounting incidences of violence reminded me that life is a fragile thing, and I became more aware of this reality through frequent contact with incidences of lives being cut short around me. Once on a short car journey around the

outskirts of Khayelitsha, I passed the horrific scene of a collision between several overloaded taxi-vans and a fuel tanker. In a bewildered state of fear and anxiety, it took all the emotional strength that I could muster to pull my vehicle to the curb so that I could vomit out of the window.

Nordstrom and Robben (1995, 13) note that the emotional numbness and bewilderment that follow the witnessing or documenting of death and violence by embedded researchers are rooted in "the confrontation of the ethnographer's own sense of being with lives constructed on haphazard grounds." Where I had set out to study clinical trials in Cape Town, I was increasingly researching – and, therefore, *experiencing* – forms of everyday violence that unsettled the stoicism of my own emotional autobiography. In light of the tanker crash and the increased reporting of emotional traumas by my research participants in our discussions of trial participation, I found documenting violence in my field notes to be an increasingly draining and demoralising task (Punch 2012). The leather-bound notebooks that had once been updated every evening with vibrant memories, character-full stories, and sketches, became erratic repositories of conflict, violence, and terror. Writing was a difficult task, and the "transfers of medium" (Hearn 1998, 56) required in the translation from notes or voice memos to written text were emotionally charged moments of recollection. Retreating from these notes for several hours, days, and even weeks, became a way of escaping the realities of day-to-day life in the field – while, of course, neglecting the documentation and retention of potentially significant research encounters over these timescales. It was the only way I could preserve the strength to wake up the following morning and listen to similar tales of violence yet again.

The murder of two of my research participants – both former trial subjects – in gang violence related to control of the local drug trade, shattered my increasingly fragile resilience to accounts of violence. In December 2010, Sizakele² – a crystal meth (*tik*) dealer – was bludgeoned to death with an iron bar outside his shack. In January 2011, Qhamani – whose brother was a prominent gang lieutenant – was stabbed 16 times as he opened the door of his house; he bled to death on the dusty doorstep where we had spent many afternoons conversing about his life in the townships and his motivations for doing first-in-human clinical trials. These were not the murders of distant strangers, whose deaths were partly sanitised by the broadcast media, but the bloody and targeted killings of two men who had welcomed me during my first months in Khayelitsha and shared their life stories. It would be paternalistic of me to claim that I, as a privileged white European researcher, could have ever truly shared their struggles with poverty and unemployment, but my research was designed to be mindful of their well-being and also invested in positively documenting their experiences of trial recruitment and participation. Instead, they were now dead and there was nothing in my commitments to engaged research that would bring them back.

When I was able to open my field diary again and document my thoughts about these killings, I began to narrate a disconnection that I saw emerging between my university colleagues, research participants, and myself:

Everyone likes the 'product' when I email updates back to university, but they *do not* want to hear about how much this is costing me personally. I have shared the news about participant deaths and attending their funerals. The response? 'Three more months, you are nearly finished!' 'Do not stop now – you are doing great!' I feel as if I am staying here because of the guilt that will come if or when I run away home. (Excerpt from field diary, 24 January 2011)

In reflections such as this, I began to question my own competency to conduct ethnographic research, mainly as a result of the growing sense of fear and insecurity that I perceived to be constraining my movements and clouding my observations. How reliable, for instance, were my increasingly panicked reflections on Khayelitsha life? Could I claim to be an ethnographer if I was retreating from certain aspects of everyday life? I felt a sense of guilt about the intensity of my personal reactions to the violence that my research participants were seemingly surmounting on a routine basis.

Reflecting on her own experiences of the field, Katz (1994, 67) recognises that "social scientists inhabit a difficult and inherently unstable space of betweenness," partly at home in the field and partly remaining an alien to everyday experiences. Like a tourist, I knew that I would leave my research participants, or "run away" as I labelled it above, to complete my degree at home. Mobility and the ability to extract oneself (and one's data) from violence is a mark of privilege against the researcher, particularly in a South African context in which race continues to dictate everyday mobility opportunities

and restrictions (Crankshaw 2008; Caretta & Jokinen 2017). Indeed, Hammett and Hoogendoorn (2012, 285) note that South Africa has, for too long, been treated as “a site of knowledge production and extraction.” Aware of potentially being labelled as yet another extractive researcher profiting from the suffering of others, I felt unable to discuss my fear and insecurity with South African colleagues, and I rarely discussed these issues with South African friends – both inside and outside Khayelitsha – to protect them from the rawest manifestations of my fear and guilt.

Exposure to situations of emotional duress reveals both the paralysis and productivity of researchers struggling to cope with their surroundings. Interlocutors have since questioned how valid or reliable my ethnographic work was in light of such reactions. An unspoken assumption behind these queries is that data quality must be compromised when researcher objectivity is in doubt. Such assertions, I contend, are symptomatic of the masculinist epistemologies of our discipline. It is presumed that an objective account is one that conforms to disciplinary norms, but the fact that these norms are premised on the covert denial of researcher personhood – an epistemological sleight of hand exposed by Fabian – is rarely discussed. The emotional consequences of extended ethnographic research can, instead, be an important *source* of critical understanding (Lund 2012; Punch 2012). Lee-Treweek (2000, 12), for example, argues that “ignoring or repressing feelings about research is more likely to produce distortion of data, rather than clarity.” As I have reflected on my experiences of the field cursorily outlined above, I have come to recognise – admittedly over a distance of several years – that the guilt I narrated above was often an unexpected way of maintaining a dialogue about my own position within the research (de Nardi 2015; Caretta & Jokinen 2017). Emotions related to loss and fear were, likewise, useful indicators to compare my own emotional responses to those of my research participants (Hume 2007; Dominey-Howes 2015). Finally, empathy facilitated my own far-from-perfect attempts at understanding the participants’ everyday experiences (Ratnam 2019). While researcher safety is paramount, to suggest that emotional responses are not valid or are likely to compromise critical work is to malign certain field experiences and disqualify forms of personhood from the field of knowledge production.

It is important, in light of the above, to return to the question of my positionality. There is value in exploring the emotional experiences of male researchers who encounter violence in the field, as these intersections are still relatively limited within geography (Dominey-Howes 2015). As feminist geographers have noted, men occupy positions of relative power inside and outside the field; this leads to the assumed ability to dominate and control the field encounter. Mehta and Bondi (1999, 77), for instance, state that men – here assumed to be heterosexual and cisgender – “take for granted their capacity to move through a variety of urban spaces.” As I spent more time in Khayelitsha following the murders, however, I found that my taken-for-granted ability to traverse urban terrain started to falter. I became increasingly wary of leaving my home, conscious of different individuals and unknown risks in public spaces, and often ended my days fearful of noises outside my house. I tragically believed that as “a man” I ought to be master of my own field experience – a masculine confidence and emotional indifference that had been reinforced through my university career to date – even when I was, like Fabian’s arrogant colonial ethnographers, in a place that I had no previous experience of before commencing the research. My growing fear started to throw this identity into doubt. I spent the evenings wrestling with the growing discordance between what I felt I should be doing and what I physically and mentally could do (Caretta & Jokinen 2017). I repressed my doubts, however, as I was wary of revealing this emotional distress to my predominantly male peer group and colleagues out of a concern that they would question my masculinity and interpret these anxieties as a sign of weakness (see Dominey-Howes 2015). Where some have felt wary of being labelled a “thrill seeker” while in the field, in many respects I was reluctant to be positioned as risk averse.

Leaving the field?

Ethnographers most often return from fieldwork alone. For the researcher, the thought of finally leaving the field can be a motivating factor to endure stressful conditions. However, the transformations that *begin* in the field do not always end at the point of departure. For all the methodological concern

with “getting in” and “getting on” in the field, there remains a noticeable silence on the challenges and repercussions of “getting out” (Irvine & Gaffikin 2006).

When I returned to the United Kingdom at the scheduled end of my fieldwork, I was inundated with requests from friends and colleagues alike, ranging from dinner invitations to projected dissertation drafting schedules. There was a belief that I would slip back seamlessly into life at home and everyone, understandably, wanted to do their best to facilitate this. In reality, I struggled to communicate with those who had seen none of the emotional transformation that I had undergone in another hemisphere (Theidon 2014). I felt that I had so little to say and, like anyone who has spent a significant time away from social groups, my friends’ lives had carried on without me; I had missed these moments – new jobs, new partners, new hangouts – and, honestly, it all seemed so insignificant in contrast to the memories and nightmares that clouded my waking moments. Close friends asked about the bags under my eyes and the noticeable weight loss; I explained it away as a side-effect of the stress brought about from return to thesis deadlines, not wanting to burden them with the full story.

Often, researchers who are suffering from emotional and psychological trauma do so in silence, giving rise to a perception that such issues are absent from research and that there is little need for post-fieldwork institutional support or formal redress (Bloor *et al.* 2010). This lack of recognition further reinforces a broader personal/professional divide in which emotional and psychological issues are still considered to be a purely personal matter, with their place in the professionalised space of research remaining unwelcome. Fieldwork, too, is often seen as a distinct phase in a researcher’s personal or professional life that is disconnected from normal rhythms and routines. The fallacy of such divides is exposed when emotional and psychological distresses arise as a *direct consequence* of professional fieldwork activity.

In those first few weeks back at work, I suffered from panic attacks, bouts of uncontrollable anger, periods of depression, teary outbursts, and recurrent nightmares; these problems did not respect any flimsy boundary intended to parse my personal and professional lives. I remember attending a university reception at which I broke down in the toilets, unable to keep up appearances around colleagues and their partners. I also found it difficult to stay in contact with my South African research participants; I felt that I had abandoned them and worried that the next social media update would contain more news of violence directed towards them. I feared, paraphrasing Sontag’s (2003, 17) work on the visceral power of images, an *assault by email*. On the days when I could force myself to work, I began to dedicate myself to helping raise the profile of the Khayelitsha NGO as this was the only forum where I could confront and raise awareness of the poverty and violence that I had seen. Focusing on these tasks helped me delay the transcription of interviews and having to play and replay audio recordings dealing with the subject of violence.

I shared little of this with my supervisor. Throughout my time in South Africa, I would email an update each month on where progress was being made. I mentioned little of the emotional problems that I was having – aside from the death of interviewees – and I maintained this silence when I returned too. We debriefed my time away mainly in terms of work done and how this would be translated into the thesis and publications. I felt comfortable narrowing our conversation around work, as I could at least present a veneer of being a research productive individual worthy of the recognition and affirmation I craved. I had field stories to tell, all of which skirted over the sleepless nights and the teary moments. I also successfully avoided the issue of my mental health and yet I came to realise that this selfishly undermined my own desire to help my respondents. How, for instance, could I claim to be supporting the work of the NGO and practising a care-full geography when I was neglecting to share the reality of my respondents’ everyday lives and the ways in which this had deeply affected me? These were questions that I was too reluctant to pose to my supervisor, fearing that this would cross a line into the therapeutic and make them uncomfortable, but these are issues that I now feel I would have appreciated their advice and support on most of all.

At the suggestion of a friend who had begun to be concerned about my increasingly erratic behaviour, I reluctantly spoke with a professional counsellor about my experiences three months after returning from South Africa. At first, I am now ashamed to say, I was dismissive of the enterprise and put forth intellectual resistance to the idea of therapy and even discussing emotions. All I wanted was to sleep better and to work efficiently. Slowly, however, I opened up about experiences in Khayelitsha – the

dead girl, the tanker crash, the gang threats, the murders – and, following further consultations over the course of several weeks, I was diagnosed with post-traumatic stress disorder (PTSD).

PTSD, I learned, involves the re-experiencing of actual or threatened traumatic events such as death, serious injury, or sexual violation. The exposure must result from direct experience of a traumatic event, witnessing of an event, recounting of an event by a close associate, or repeated exposure to distressing details of a traumatic event (Friedman 2013). A significant number of ethnographic research projects could expose researchers to such traumas, and ethnographers may be vulnerable to secondary or vicarious trauma as they conduct interviews with individuals detailing traumatic or harrowing events (Calgaro 2015; Markowitz 2019). Lerias and Byrne (2003, 130) define secondary traumatic stress as “the response of those persons who have witnessed, been subject to explicit knowledge of or, had the responsibility to intervene in a seriously distressing or tragic event.” In my case, the death of research participants and repeated secondary exposure to accounts of trauma – rather than a single catastrophic event – were found to be significant contributors to my complex PTSD. At night, I would lay my head on my pillow and my mind would immediately cast itself back to the steps where I had conversed with Qhamani, except this time the concrete steps were smeared in his blood. I would lay there for hours, unable to close my eyes, often leaping out of bed at the slightest of noises coming from the shared house beyond my bedroom door. My flashbacks occurred thousands of miles from Khayelitsha, but they repeatedly refocused my thoughts back to a time and a place that I could not return to, while – at the same time – the trauma colonised new spaces at home (Coddington & Micieli-Voutsinas 2017).

PTSD is a treatable condition and treatment can be relatively short if it is diagnosed promptly. If there is a delay in the diagnosis and treatment of PTSD, the experiences of trauma risk becoming firmly rooted within the sufferer. Diagnostic guidelines for PTSD include four distinct behavioural symptom diagnostic clusters: re-experiencing, avoidance, negative cognitions and arousal (American Psychiatric Association 2013). The number of symptoms that must be identified for diagnosis depends on the cluster, but disturbances must continue for more than one month. In my case, these symptoms were everyday realities over the course of three months after my return before I sought help, and were further amplified when I was exposed to stimuli that triggered violent memories, such as violence in films, in the media, and even as I tentatively returned to my field notes. In one memorable incident while driving to the supermarket, a driver behind me beeped his horn as I switched lanes suddenly. Rather than let this go, I uncharacteristically leaped out of my car at the next intersection and loudly remonstrated at the driver for daring to affront me in this way.

My own treatment only began after I had already lost the ability to moderate my own anxiety levels, as the driving incident demonstrates, to the point that I was worried about the need to pause my doctoral studies and thereby publicly disclose my suffering. While some medications have shown immediate benefit in treating the symptoms of PTSD, the best long-term treatment is talking. Through cognitive behavioural therapy, PTSD sufferers are encouraged to discuss the traumatic events that are responsible for their negative emotions. The aim of this therapy is to normalise speaking about trauma in such a way that feelings of panic and anxiety are no longer triggered. Again, I initially found sharing to be a difficult experience. I avoided certain topics and refused to use certain words that I believed were labels for an emasculating emotionality (crying, anger, fear), instead falling back on intellectualising violence and witnessing. I was encouraged to practice humility – something quite alien to the more prideful, assertive, and defensive posturing of masculinist academic circles – and this took the form of examining and meditating on my own capacities and limits (Eriksen & Ditrich 2015). The veneer of pride and busyness that I sought to project to others, I came to understand, allowed me to avoid considering my own shortcomings. Practicing humility, in other words, gave me the confidence to show colleagues my own flaws in the solidaric spirit of “confessional intimacy” (Harrowell *et al.* 2018, 236).

A key part of practising humility has been acknowledging that my experience of the field, trauma, and PTSD is informed by my position as a white, male researcher from a university and country capable of dedicating significant resources to facilitating researcher support and treatment. I do not want to “grade” my PTSD as an exceptional experience. Trauma talk, Berlant (2001, 46) suggests, is too often captured by interests that seek to “flatten out trauma’s bumpy terrain” in order to deny or

downplay historically sedimented forms of epistemic violence. Researchers identifying at the intersections of axes of difference – notably gender, ethnicity, sexuality, disability, age, and nationality – are vulnerable to traumas arising from targeted identity-based violence, discrimination, silencing, and exclusion in the field and at home (Malam 2004; Hume 2007; Ross 2015; Jokinen & Caretta 2016; Hawthorne & Heitz 2018). It is essential that the discipline makes more space to acknowledge these researcher perspectives and differentiated experiences to make sense of power relations shaping the field. Reflexive humility for me, in this instance, involved recognising the humanising function of fieldwork and seeking solidarity with fellow researchers in the interconnected struggles against forms of power that reproduce violence and exclusion against research subjects and researchers alike.

After many sessions with trained counsellors, I am now in a position to better manage my own emotions through a clearer prioritisation of care. “Care,” Mountz and colleagues (2015, 1238) argue, “is not self-indulgent; it is radical and necessary.” Despite my successful treatment, which I was able to complete alongside my studies rather than through interruption, I still have difficulty controlling my response to certain triggers and this is a necessary task that I will have to contend with indefinitely. Now when I hear a car horn behind me, I still occasionally feel rage, but I control the emotions much better by counting down in my head and considering my breathing: ten, nine, eight, they are probably having a bad day, seven, six, five, ignore the horn, four, three, breathe, two, one. The neurological and hormonal changes brought about by PTSD have necessitated radical changes to my social and professional routines. The physical transformations that accompanied my case of PTSD – significant weight loss, propensity to severe fatigue, episodic irritability around loud noises – mean that I will never be the same person that boarded the plane for South Africa in January 2010. I now take these visible and invisible forms of chronic impairment into the field with me and, as I seek to adjust my body within the field, I encounter the unseen barriers in fieldwork that my prideful self previously chose to ignore (Kitchin 1998; Anderson 2001).

Conclusions

My experiences of negotiating the field and transitioning out of fieldwork highlight an acute need for greater recognition of the emotional and psychological challenges of ethnographic research, and also underline the necessity of comprehensive guidance and training for early career ethnographic researchers entering the field (Billo & Hiemstra 2013; Loyle & Simoni 2017). Too often the emotional consequences of professional activity have been marginalised within methodological accounts of the field (Pollard 2009; Caretta & Jokinen 2017; Ratnam 2019). In turning a critical gaze onto my own experiences in the field and with PTSD, I have introduced some of the consequences for early career researchers of disciplinary and institutional cultures that valorise emotionally opaque accounts of the field. I have also sought to counter the silencing masculinist traditions of our discipline by examining my pre- and post-fieldwork experiences through the lens of my own positionality as a male geographer with a mental disorder.

It has not been my intention to abrogate personal responsibility for my own choices in the field, as I hope has been clear through discussion of mistakes and shortfalls, or to apportion blame to anyone involved in the oversight of my research. In a spirit of humility and confessional intimacy, it is difficult to disagree with DeLyser and Starr’s (2001, 6) characterisation that we are *all* “fallible field workers negotiating challenging circumstances, not always with equal success and grace.” A number of professional silences and expectations, however, did have a significant and complicating impact upon that decision. These included a silence surrounding the emotional and psychological challenges of ethnographic research in methodological literatures, and the lack of adequate professional information and training about the potential emotional traumas of research (Evans 2012; Warden 2013; Eriksen 2017). While these expectations may now be taken for granted as the emerging contours upon which contemporary academic research is conducted, change is urgently required in the administration of fieldwork and the professional training of researchers to prepare them to confront some of the emotional and psychological challenges of the field (Mountz *et al.* 2015; Mullings *et al.* 2016).

First, at an individual level, is a call to prioritise self-care both in the field and after. Working constantly risks burnout, as Mountz and colleagues (2015) remind us. *Before I leave*, I now schedule a

period of local furlough part way through extended fieldwork for rest and recuperation. This feels radical, especially as time in the field is often pressurised for many reasons, but allows me to reframe difficult experiences outside of the immediate context of the field and humbly reflect on my own actions (Eriksen & Ditrich 2015). Likewise, I would encourage early career ethnographers to consider engagement with professional counselling services after return from extended periods in the field (Beckett 2019). While many might see such appointments as an unnecessary diversion from the pressures to begin data analysis, talking can identify causes for concern at an early stage and can also be a helpful initial stage in reflexively examining and narrating time in the field.

Second, as a broader discipline with responsibilities for the safety and well-being of early career researchers, we must acknowledge that self-care *alone* cannot solve field traumas or structural dilemmas linked to over-work (Theidon 2014; Eriksen 2017). Our learned societies must prioritise developing accessible guidance relating to mental distress in the field. Funded professional development courses on dealing with emotions and mental health in the field, akin to those offered in other sectors, must be developed and promoted as a disciplinary priority (Pearlman & Mac Ian 1995; Bloor *et al.* 2010). This support could be made available to early-career researchers and their supervisory teams to ensure broad uptake of key messages and principles of support. However, our disciplinary reflection must also extend to differentiated experiences of the field (Anderson 2001; Sundberg 2005; Jokinen & Caretta 2016). If the masculinist and colonialist underpinnings of fieldwork are to be challenged, there must be clearer channels of advice, mentoring/sponsorship, and peer support, for researchers whose gender, sexuality, ethnicity, or disability makes them more vulnerable to traumatic experiences in certain field settings (Malam 2004; Ross 2015; Webster & Caretta 2019). It is also incumbent upon all geographers to cultivate “thick” forms of solidarity that take an anticipatory stance towards trauma and support colleagues before, during, and after fieldwork through radical forms of collective care (Mullings *et al.* 2016; Dombrowski *et al.* 2018).

Third, and finally, universities hosting ethnographic researchers must prioritise practical and financial support for training and (professional) debriefing (Caretta *et al.* 2018). This could include developing care curricula for doctoral researchers, updating and mandating regular review of institutional risk assessments to have researchers identify likely emotional stressors and their consequences for mental health, and the development of clearer, judgement-free procedures around if, how, and when researchers (and even their supervisory teams) can pause time in the field on the grounds of safety or well-being.

Notes

¹ Exposure of clinical staff to the traumatic experiences of patients, for example, is acknowledged and – to some extent – anticipated through professional training (Pearlman & Mac Ian 1995). Likewise, Bloor and colleagues (2010) document the specialist pre- and post-trip intensive security briefings that international humanitarian organisations provide for their staff travelling to high-risk countries.

² All names used in this paper are pseudonyms.

Acknowledgements

I thank Chief Editor Kirsi Pauliina Kallio and the six peer reviewers for their extensive and generous comments that have significantly strengthened this article. I also acknowledge the support that colleagues at Queen Mary University of London have offered as I have come to terms with the experiences discussed in this piece. This research was funded by the Economic and Social Research Council (ES/GO19061/1).

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