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Negotiating Research Ethics in Volatile Contexts, Part I

edited by Abida Bano, Rosa Cordillera A. Castillo, Sarah Holz and Andrea Fleschenberg

Book Review

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Negotiating Research Ethics in Volatile Contexts

Editorial

Andrea Fleschenberg, Rosa Cordillera A. Castillo

Research ethics is integral to the entire process of knowledge production: from conceptualising and designing a research project and gathering, analysing and managing data, to writing and other forms of representation and engagement. Yet, there is often a lack of attention given to research ethics pedagogy and praxis in various academic institutions. This problem is compounded by contestations as to what constitutes research ethics in the qualitative social sciences, particularly since the dominant research ethics paradigm is largely based on the biomedical model. Relatedly, questions are raised with regard to how research ethics can be made compatible with the epistemology and methodology of specific disciplines and of inter- and transdisciplinary approaches (see Castillo and Dilger in this issue, part one). Ethical practice goes beyond the clearance-based scholar-centred approach of most ethics review boards and the placement of research projects, with an ethics checklist, into tidy containers of academic research practice before "entering the field" (cf. Katz 1994). Navigating research ethics in praxis is instead messy and entangled with various layers and shifting loops of contentiousness (see Sökefeld et al. this issue, part one) and relations (see Castillo et al. in this issue, part two). Cultivating ethical behaviour and decision-making is thus an ongoing negotiation and continuous process of thinking, acting and reflecting on our research and professional conduct.

Our guest editorial team, composed of Abida Bano, Rosa Castillo, Sarah Holz and Andrea Fleschenberg, came together through a shared concern for establishing a sustained critical research ethics pedagogy, praxis and responsive review process that decentres dominant frameworks and practices of research

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ethics (see Castillo 2018). Guided by indigenous, decolonial and feminist perspectives, we conceive of research ethics not only as a set of guidelines on research methodology and conduct but also as deeply intertwined with the power and the politics of knowledge production. We thus draw attention to the ways in which the coloniality of knowledge, being and power, as well as heteropatriarchy, shape research and knowledge production, and are thus relevant to research ethics. We are conscious, too, of how research and knowledge production have been and can be exploitative, extractive, racist and unequal, particularly between the Global North and the Global South. Among our efforts in line with this are the formation of the "Negotiating Research Ethics Initiative" at Humboldt University of Berlin; the institutionalisation of research ethics in university curriculums and graduate school training programmes; the insertion of research ethics as a central agenda into research networks/cooperations; and the provision of safe working group-based exchange and mentoring spaces for knowledge producers in Europe and Asia who are at various stages of their academic careers and with diverse positionalities and epistemological, methodological and (inter-/trans-)disciplinary approaches.

When preparing for this special issue, we discussed in various rounds, platforms and configurations a number of critical acts and incidents that reinforced and further guided our concerns on research ethics in volatile contexts in Asia through a decentred, decolonial praxis. One such incident took place within the field of academia itself, at an international Area Studies conference, as discussed in detail by Rosa Castillo in her debating input with additional reflections by Anthony Pattathu and June Rubis, in terms of lived realities of decolonial research ethical praxis (this issue, part two). Another surfaced in 2021, when a controversy emerged among South Asian Studies academics on the challenges of academic knowledge production, particularly in the case of US-based Indian medical anthropologist Saiba Varma's work on militarism and care in Indian-administered Kashmir. This case highlights issues on positionalities and fieldwork practices as well as navigations of disclosure, representation and consent with marginalised and vulnerable communities in contexts marked by conflict, occupation and/or repressive governmentalities.[1]

Framing research ethics as a decolonial, decentred and feminist praxis furthermore entails a more critical take on issues of engaging with refusal in academic praxis and knowledge production (Chatterjee 2020, Siam 2022, Tuck / Yang 2014), which leads us to the third critical juncture: the COVID-19 pandemic. Writing at the end of 2022, many of us are experiencing a long-term sense of exhaustion and a kind of pandemic fatigue as well as adjustment vis-à-vis anxieties and uncertainties linked with our academic research practices across various

1 See The Wire 2021; and Siam 2022 for the case of Canada-based Pakistani Anam Zakaria's oral history work on Pakistan-administered Kashmir.

fields and arenas. While contributions to this special issue are not predominantly focused on research ethical practices in (post-)pandemic times or centring pandemic-related challenges of navigating research ethics in volatile contexts, for example due to specific pandemic governmentalities, we would like to take this editorial space to reflect on the nexus of pandemic-related challenges and research ethics for a number of reasons.

The COVID-19 pandemic served as a kind of magnifying glass for research ethical challenges and concerns, leading to increasingly prominent calls for an ethics of care amid a pandemic "kaleidoscope in terms of change and patterns" (Hussain 2020) within academic mainstream discussions on research practices and the ethics thereof. As we have argued elsewhere (Fleschenberg / Holz 2021), pandemic-related academic debates early on centred on concerns with inequalities, injustices and divides. Scholars from a variety of (inter-)disciplinary approaches called for a different praxis of research ethics and knowledge production, including with regard to Global North and Global South interactions and asymmetries in knowledge production, research collaboration and academic publishing.

The widespread lockdown left many Global North-based researchers involuntarily immobile, disconnected from the physical field and with "new" ethical and methodological challenges caused by pandemic disruptions and remote research (see also Suarez in this issue, part two). But we need to draw a carefully calibrated picture here. Dunia et al. (2020) critique a certain "Northern naval gazing". The praxis of remote research and contracting researchers is not novel, given that in pre-pandemic times security concerns already meant that many Global North-based researchers limited themselves to safer (often urban) settings and commissioned local researchers for more risky data collection, thus exposing the latter to "exploitative and unequal research relationships and partnerships instead of nurturing the coproduction of knowledge" (Dunia et al. 2020).

One key article that was repeatedly debated, contested and referred to in our working group "Researching Asia in Pandemic Times", as well as in our university classes and training-and-exchange workshops, was written by Aymar Neyenyezi Bisoka (2020), who critiques the "colonial relationship that has plagued social sciences for the last four centuries, which has often made invisible the work of local researchers from the Global South". Challenging the notion of pandemic-induced transformations and opportunities to rethink power relations in research designs and practices, which were prevalent in academic writings and blog entries from 2020 to 2022 surrounding notions of "ethics of care" and "justice" and "solidarity", Bisoka (2020) instead points to the need for the "decolonisation of knowledge", given the compounded precarities and vulnerabilities of Global South researchers who partake in Global North-centred research projects, a relic of the colonial momentum and its continued racialised legacies in academic research (see the contributions to the current debate by

Castillo et al. and Kamal et al. in this special issue). The COVID-19 pandemic thus presents us with a certain momentum and raises severe epistemological, political and (research) ethical questions that tackle the coloniality of the academic research practices of researchers based both in the Global North and Global South.[2]

Furthermore, the body of pandemic-related (re-)thinking regarding a transformed research ethical praxis highlights a number of challenges and concerns: 1) newly emerging or shifting ethical challenges due to pandemic settings;[3] 2) the relationship between research assistants (or "facilitating researchers") and "contracting researchers", or – as Dunia et al. (2020) have called them – "Northern 'research capitalists' and Southern 'research proletariat'";[4] 3) the need to navigate research via digital means, new technologies and spaces while remaining mindful of communication, connectivity, resources and agency divides;[5] 4) the need to revisit notions of care, reciprocity and relatedness in research ethics to counter extractive research practices and gazing;[6] and 5) questions of integrity and the need for (novel) research in pandemic times.[7]

Calls for more inclusive, diversity-oriented and caring practices – be it for conventional research methods and contexts or for re-devised remote, digital methods and (post-)pandemic contexts – have become increasingly audible, even if this might mean ending a research project in order not to overburden research participants or exacerbate pandemic-related emergencies among already marginalised groups (see also contributions by Thajib as well as by Sakti / Taek in this special issue). Far greater attention has been focused on decentring research design practices and revisiting the weighing of perspectives to determine what kind of knowledge is important and relevant as well as how phenomena are conceptualised (Pacheco / Zaimağaoğlu 2020). As Pakistan-based Rahat Batool (2021) painfully asks, what knowledge is needed and for what purpose when confronted with risky and precarious research settings in already impoverished, marginalised communities within a context of compounded volatilities and vulnerabilities?

The push for digitisation within academia and the wider society has exacerbated the digital divide, and with it come complex ethical challenges, for instance in relation to marginalised groups or for particularly exposed groups like frontline workers, as well as in terms of data security, digital access, digital literacy or substitute data sets and sample populations.[8] Helen Kara and Su-ming Khoo

2 Mwambari et al. 2021, Bisoka 2020; see also Ndlovu-Gatsheni 2020.
3 Batool et al. 2021, Zuberi 2021, Garthwaite 2020.
4 See also Bisoka 2020, Mwambari et al. 2021.
5 Hensen et al. 2021, Howlett 2021, Kalia 2021, Kara / Khoo 2020, Khan 2021, Tiidenberg 2021, Zuberi 2021.
6 Corbera et al. 2020, Shankar 2020.
7 Carayannis / Bolin 2020, Garthwaite 2020, Pacheco / Zaimağaoğlu 2020.
8 See Suarez this issue as well as Batool et al. 2021 and Khan 2021.

(2020) point towards shifting power relations and a reconsideration of who is vulnerable and how vulnerable they are in pandemic research settings, thus calling into question entire research enquiries and their necessity.

The notion of care, however, applies not only to research participants and collaborators. It extends to researchers themselves, whose privilege and power in research settings were more often than not challenged and reversed in pandemic settings along gendered and racialised cleavages (see Bano / Holz in this issue).[9] Having said that, the need to negotiate one's positionality and ethical responsibility – as well as enacting self-care and doing no harm to oneself or those near and dear in the face of traumatic or stressful encounters – has been a daunting journey and a burden for many critical and engaged scholars. Experiences of powerlessness, of not being able to do enough beyond (or despite) metric-oriented, competitive academic work, of not being able to "give back" sufficiently, or even tokenism or researcher-centred face-saving have been known to lead to feelings such as fatigue, numbing, cynicism, hyper-vigilance, guilt and disassociation, among others.[10]

Questions of researchers' mental health and coping strategies for emotional stress, pain and trauma have been discussed by many, particularly when working in the Global South in volatile contexts or when working from a critical approach, where contexts of multi-layered, compounded crises, volatilities, inequalities and uncertainties are the everyday normal matrix within which they must operate and not just an exceptional, temporary crisis – as, for instance, many in the Global North regarded the COVID-19 pandemic.[11]

Linked to the multi-dimensional and multi-directional notion of care are renewed calls for slow research, questioning the timing, pace and rigid sequencing of research steps in times of a pandemic crisis and its long aftermath, but not only.[12] Emma Louise Backe's (2021) notion of an "ethics of crisis" renews the urgent call for slow, decentred research and a "practice of pragmatic solidarity",

> through a locally situated and grounded ethics of concern that is attentive to the particular temporalities and extractive logics of academic research. In these cases, research is oriented not by the "tyranny of the urgent" or the neoliberal demands of the academy, but rather by the priorities and needs of the community participating in the research.[13]

9 See also Kamal in this issue, part two; Batool et al. 2021, Kalia 2021, Khan 2021, Zubeiri 2021.

10 See Ansoms 2020, Lunn 2014, Selim 2021.

11 See Günel et al. 2020 and Selim 2021.

12 See in particular the contribution of Thajib in this special issue, part one, as well as Ansoms 2020, Das 2020.

13 Zahra Hussain (2020) argues that slow science "calls for unsettling the stable typologies drawn from structures of theory and knowledge we are trained in […], in order to enter the unknown territories" in this "project of academic self-regulation" of pandemic research. Similarly, Corbera et al. (2020: 192) opine that "academic praxis should value forms of performance and productivity that enhance wellbeing and care together with solidarity and pluralism". See also Ackerley / True 2010, Chilisa 2012, Günel et al. 2020, Smith 2021.

Referring to "ethical responsibilities toward those on whose lives and through whose labor we build our careers and enjoy professional success", Dunia et al. (2020) furthermore call for a rethinking of authorship along with remaking compensation, remuneration and insurance practices for local research counterparts. Revisiting notions of reciprocity, trust, power, vulnerability and inequality in research relationships in light of the pandemic-instigated "ethics of disruption" for social sciences worldwide, Gina Crivello and Marta Favara (2020: 1) argue that:

> It feels as though we have entered a new ethical landscape, one that is compelling social researchers to re-examine previously held assumptions about what is appropriate, possible, valuable and relevant for their research, and the nature of ethical responsibilities to all those enmeshed in the research relationship during this time [...].

But how strong has the impact of this pandemic-related (re-)thinking of a transformed research ethical praxis been within mainstream academia in the long term, be it in terms of institutional architectures and institutions of research ethics or in terms of individual practices and decisions taken? Or, in other words, how do we deal with a longing for "back to normal" or a post-pandemic "new normal" in the social sciences? How far do the concerns sketched out above lead to a rethinking and transformation of epistemological and methodological approaches and entangled research ethical practices, which we understand as a fundamentally decolonial-feminist, thus decentred, praxis? What new architectures, spaces, teaching pedagogies and materials need to be set up? Or is there potential for reform?

Within this special issue, we have opted for a number of writing formats and a wide range of contributing authors in terms of their (inter-)disciplinary approaches and research fields, their positionalities, academic biographies and career stages. These diverse contributors and formats provide material for further debate and reflective discussions on experiences and practices of navigating and negotiating research ethics in volatile contexts across Asia and beyond.

Volatile contexts, such as the pandemic, are those contexts marked by unpredictability and uncertainty, by ongoing processes of transformation and thus (potentially) rapidly changing dynamics, as well as disruptions with regard to key parameters within the field. Volatile contexts are further characterised by ambiguity and variegated constraints in addition to a particular set of ethical challenges. The vulnerabilities of all research partners involved, the potential risks and the social, economic and political stakes are heightened, necessitating particularly complex and fraught considerations on how to conduct research ethically. Subsequently, such contexts demand from researchers a high degree of preparedness, reflexivity, flexibility, alertness and openness in light of a need for constant (re-)negotiation, (re-)adaptation and creative coping strategies. These impact, in specific and myriad ways, a researcher's toolbox in terms of episte-

mology, theorising, research design and methods, ethics, data gathering, dissemination and engagements.

With a focus on research ethics in volatile contexts, this special issue thus aims to provide various perspectives on research ethics from scholars positioned within a particular discipline, such as anthropology, political science, history, sociology and area studies, among others, as well as those with an inter- or transdisciplinary perspective. Drawing from concrete research experiences and how they have dealt with ethical dilemmas as well as critical reflection and framing of research ethics, the contributors offer ways to think through the relationships between research ethics, power, violence, inequalities, institutions and pedagogy in various volatile research contexts and institutional frameworks.

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Ethics as Embodied Practice: Reflexivity, Dialogue and Collaboration

Rosa Cordillera A. Castillo in conversation with Hansjörg Dilger

Hansjörg Dilger is Professor of Social and Cultural Anthropology at Freie Universität Berlin with a specialisation in critical medical anthropology and the anthropology of religious diversity. His research has a regional focus on Tanzania and South Africa and migratory contexts in Germany. He was a visiting professor at the University of Vienna and visiting fellow at the University of Witwatersrand (both in 2014) and Assistant Professor of Anthropology and African Studies at the University of Florida (2005–2007). Between 2015 and 2019, he was President of the German Anthropological Association (GAA): in this capacity, he and the GAA board pushed toward the firm integration of ethical reflexivity in Social and Cultural Anthropology in Germany.

Keywords: Anthropology, methodology, research ethics, institutional review boards, interview

Rosa Castillo: *Our conversation will focus on practical ways in which researchers can conduct ethical research that remains compatible with anthropology's epistemology and methodology. We will delve into how these ways of doing anthropological research relate to inter- and transdisciplinary approaches, to broader social science discussions on research ethics and to the role of professional organisations and academic institutions. Our point of engagement is your article "Ethics, Epistemology and Ethnography: The Need for an An-*

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The interview was conducted on 26 May 2020 in Berlin and transcribed by Marie Wuscher, Bonn International Centre for Conflict Studies (BICC), Germany. We would like to thank Marie Wuscher for the meticulous transcription efforts and support. The interview transcript has been edited for readability purposes. The interview is part of a podcast series organised by the Research Ethics Initiative at the Institute for Asian and African Studies, Humboldt-Universität zu Berlin, coordinated by Rosa Castillo, Andrea Fleschenberg and Sarah Holz since early 2020 (https://www.iaaw.hu-berlin.de/en/region/southeastasia/research/groups/negotiating-research-ethics).

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thropological Debate on Ethical Review Processes in Germany" [Dilger 2017], your research experiences and your work with the German Anthropological Association (GAA). To begin, please tell us about yourself and how you define and position yourself as a researcher. What are your research interests and what are you currently working on?

Hansjörg Dilger: My career in anthropology started out in the field of medical anthropology from the mid-1990s onwards when I first did my master's at Freie Universität Berlin and then also my PhD. For my PhD I worked on the dynamics of social relations in rural and urban areas of Tanzania and how they changed and became transformed in relation to HIV/AIDS-related illness and death. Thus I focused on systems of solidarity and support, both within families and religious, especially neo-Pentecostal, communities, but also in the context of HIV/AIDS-related activism, for instance in NGOs in urban centres. This focus on the micro-politics of care, death, burials and healing in the context of HIV/AIDS increasingly led me to the field of religion. Over the last fifteen years, I have been doing research on processes of institutionalisation – and their individual and collective embodiment – in religiously diverse settings. I have recently completed a book manuscript on the learning of morality, inequalities and faith in Christian and Muslim schools in Dar es Salaam, where I look at the teaching and internalisation of values in postcolonial and global settings [Dilger 2022]. So all of these topics, both in medical anthropology and the anthropology of religion, were related to different aspects of value formation, both with regard to my interlocutors' pursuits of a good life and to how their struggles for moral orientation are embedded in, and configured by, larger political-economic circumstances. But this interest in the processes and politics of value formation also became important for me, myself as a researcher, where I also had to question my own ways of doing "good research".

Rosa Castillo: *We will discuss your previous research in Tanzania in more detail later when we delve into the ethical issues that you dealt with. You have written on research ethics in anthropology, and more specifically in German anthropology. What for you is research ethics?*

Hansjörg Dilger: Research ethics is obviously a huge topic, but for me, it is mostly a professional orientation of doing research in a good and appropriate way. There are obviously very different understandings of what constitutes "good research" in relation to many different topics and contexts. So, this can challenge us in relation to establishing trustful and reciprocal relations in our field sites, but also in how we deal with issues of confidentiality. The responses we give to these challenges and the questions they imply can vary among indi-

vidual researchers, as well as also within our field sites among our interlocutors, or even in the institution within which we work. Ethics has to do with finding suitable answers to all these questions, but then also applying them – and, if necessary, challenging them again – in our practice as researchers and scholars. And what is especially relevant in relation to anthropology and the qualitative sciences is that all these questions and challenges become relevant in *all* phases of research. Research ethics in anthropology is often narrowly considered with regard to fieldwork, for instance. However, it is important that we think about ethical orientations and how to act ethically even *before* we enter our field sites, as well as *afterwards* when it comes to the interpretation of research materials or the writing up. So, all these questions will play a role in all these different phases and contexts of our work. And what we also need to do, I think, as anthropologists, is always to discuss these questions and challenges in close interaction with our various interlocutors in our research sites. Thus there should be a priority for us to establish conversations on all these ethical challenges together with our interlocutors, and not just to talk with them about our research questions and methods in the narrow sense.

Rosa Castillo: *The discussion and debates on research ethics and anthropology are extensive and span several decades. The American Anthropological Association website, for instance, has a vast resource on research ethics and statements dating back to 1948. But, as you wrote in your article, German anthropology was a rather late comer to this discussion. It was only in 2009 that the German Anthropological Association adopted a declaration of ethics, notwithstanding the efforts in 2001 and 2005 from subcommittees to draft ethics principles. Despite these ongoing discussions on research ethics in German anthropology, you referred to the response and reflections to ethical dilemmas in research and teaching as "muddling through" (2017: 192). That is, these responses and reflections on the ethical implications of our work are based on individual rather than institutional or disciplinary criteria. Professional organisations play a major role in advancing ethical research. Within German anthropology, how prominent are these ideas on research ethics that you shared with us? Why do you think it took so long for the German Anthropological Association to draw up an ethics declaration? And what are the advantages and disadvantages of "muddling through"?*

Hansjörg Dilger: The discussion on ethics in the German Anthropological Association goes back quite a long time, actually. A working group on ethics was established already in 1989, but it received very little support from the members of the association in its efforts to establish a commitment to ethical standards within the discipline. I think there was a concern among colleagues

that they would "lose" their freedom in doing ethnographic fieldwork, and that the research process might become over-regulated and overly bureaucratic due to any ethics code or statement. And there were good reasons for such concerns: there were these negative examples from Anglophone countries – the bureaucratic machineries of the institutional review boards in the US and the rise of an audit culture in the context of neoliberal academia in the UK in the 1990s – and so on. Anthropologists in Germany saw that institutional (in the sense of standardized) responses to ethical challenges could have a negative impact on research practice: that these institutional frameworks might restrict the openness of ethnographic fieldwork and limit the flexibility necessary for conducting qualitative research successfully. However, the proponents for establishing a debate on ethics and for coming up with a declaration on ethical principles also had good reasons for their initiatives. Some working groups in the German Anthropological Association formulated their own collective statements on ethics earlier. For instance, the working group on Anthropology and Development adopted its own guidelines on ethics in 1999, the working group on Medical Anthropology did the same in 2005, and then the German Anthropological Association actually followed in 2009. These two working groups saw the problematic consequences that the absence of clear ethical commitments might have for their research – both for individual anthropologists and for the communities or organisations they worked with.

So, in a way, all this created the context of having to "muddle through", as I called it in the article you mentioned. There is a clear disadvantage to having to establish your own ethical standards if you have no clear, professional or institutional guidance in relation to the ethicality of your research; and I think not having such an institutional response is especially problematic for early career researchers who are looking for such an orientation. I, personally, missed this as a PhD student in the late 1990s and early 2000s [cf. Dilger 2011]. There is a lot of uncertainty, and even anxiety, that you have in such a situation. It can become very challenging when you are facing certain ethical dilemmas and are not really sure how to respond to them – even if at the end, of course, you have to take the individual decisions yourself. Thus, the advantage of "muddling through" – in terms of having more flexibility for your research – is really an advantage for more experienced scholars. The critics of institutionalised ethics are right that the idea of a "quick fix" for ethical dilemmas can be very harmful: that you need only tick your ethics boxes according to an ethics code or an ethics review board and can then proceed with your research; that you do not have to think about ethical dilemmas afterwards because you have in a way already "fulfilled your obligation" in an ethical sense. Thus, it is important to think about how to maintain both this freedom and this reflexivity while at the same time having an institutional and more formal response to it.

Rosa Castillo: *In your article you point out that, in the absence of an ethical review process in German anthropology for your research on HIV/AIDS in Tanzania, the ethical aspects of your doctoral research were first addressed by the ethics review process of the Tanzania National Institute for Medical Research (NIMR), which you had to go through to conduct your research. However, their process followed the biomedical model, whereas your research was social and cultural. You therefore encountered problems with their informed consent process, which they asked you to implement using a written form. That is, your interlocutors were supposed to sign an informed consent form, in which you had to mention that you were conducting research on HIV/AIDS. This was particularly problematic for your interlocutors in a neo-Pentecostal church in Dar es Salaam and in the rural Musoma region, where people understood their illness predominantly in moral and social categories, for instance through notions of evil spirits and witchcraft, and where HIV/AIDS was strongly stigmatised. You thus could not speak directly about HIV/AIDS to your interlocutors, rendering the NIMR's approved informed consent form problematic.You provided a practical and epistemological critique of the NIMR informed consent process. However, were there any insights that you gained from the NIMR ethics review process that you would not have been made aware of if you hadn't gone through it? That is, were there blind spots that their review process alerted you to?*

Hansjörg Dilger: This is an important question, and I think the problematic aspects of an ethics review process based on the standards of the health sciences become very obvious the way you have summarised them – at least from an anthropological perspective. Talking or speaking about a certain illness can be highly problematic in contexts where this illness is stigmatised, but that is exactly what you are expected to do according to biomedical or public health standards. Addressing suffering by its name is what these settings take, in a way, for granted – that you discuss the phenomenon in question in the "correct" biomedical terms. But how do you do this in a context of strong stigma and discrimination? To give an example: I was introduced to potential interviewees in a rural area of Tanzania by an AIDS counsellor in one of the local hospitals. In one instance, he introduced me to a couple by letter and told them that I was interested in doing research on "the issue" they had. Thus, he did not even mention HIV/AIDS, because he was aware that this might put them off, but said instead that he wanted to talk about "this issue". However, because the letter came from the AIDS counsellor, the couple knew immediately that I was interested in knowing more about the wife's infection with HIV. His letter would have had the same effect if the word "HIV" had been mentioned explicitly.

The couple were very reluctant to talk to me and also refused to do an interview with me. I felt really bad about this situation, because I knew it had caused pain for the couple. I therefore started to follow the advice of my local interlocutors outside of the hospital, and of my research assistant, not to address HIV/AIDS directly but to frame it in more metaphorical terms. I had to "sense" my way into and through the field with the help of my interlocutors; they gave me orientation on how to engage in meaningful conversations by employing morally acceptable and non-offensive language.

However, the ethics review process by the NIMR as such was also helpful. At the time it alerted me to the fact that there were important ethical issues at stake in my research. Neither my home institute in Berlin nor my funding organisation, the German Research Foundation, nor my supervisor – none of them had asked me any substantial questions about research ethics at that time. In the context of drafting my ethics proposal for the NIMR, I was also alerted to the fact that the American Anthropological Association had its own code of ethics; I had not been aware of this because we did not discuss this in our doctoral training or in teaching. During my master studies this was never in any way an issue that was mentioned. It was therefore a fortunate coincidence that I could access this code of ethics by the American Anthropological Association through an internet café in Dar es Salaam, and their code of ethics showed me actually what the informed consent was that the NIMR was asking for. It also stated that I could also ask for this consent orally and not only through a written statement, which was the kind of documentation that the NIMR was looking for. For me, this was a very reassuring moment: to know that there was an ethics code by an anthropological association that established this possibility. It provided me with an authoritative source that I could rely on and refer to in my application for ethical approval. It gave my approach legitimacy and was then also accepted by the NIMR.

Rosa Castillo: *The importance of closely listening to our interlocutors, of consulting them regarding the ethical implications of our research and the validity of our analyses, as well as involving them in the research design and process and adjusting our approach accordingly – these are crucial to our praxis and ethics. Can you give us another example of an ethical dilemma that you encountered in your past or ongoing research and how you dealt with it in concrete terms?*

Hansjörg Dilger: Another example I can think of was from my ethnographic fieldwork on faith-oriented schools in Dar es Salaam. I had access to two of the Christian schools of my study through the pastor of one of the largest neo-Pentecostal churches in the city. She owned a network of schools, and in these

schools there were certain problematic and partly also semi-legal practices. For instance, they brought teachers from Kenya and from Uganda to Tanzania with a missionary visa – although the schools themselves were not explicitly religious. The pastor owned the schools as a private person, but she operated them more like a business than, for instance, a charitable organisation. So, the teachers did not actually work there as missionaries. Furthermore, the teachers were not allowed to join unions; they did not get contracts and could easily be fired. All of these things were problematic (though legal), but I wondered whether it was ethical to write about them, to reveal these aspects, as I had this ethical obligation not to do harm to the schools or to their owner. The pastor had very kindly granted me permission to enter her schools, so I did not want to abuse this trust. Moreover, what made it an ethically particularly challenging situation was that it was impossible for me to hide the identity of the schools because there was only one faith-oriented school network of this kind in Tanzania. At the same time, there were also the teachers who complained about these practices and suffered from their negative consequences.

Ultimately, I decided to write about these issues. I found it important to talk about these practices in order to explain how religious entrepreneurs from the neo-Pentecostal field established themselves within the neoliberal educational market. These details were not just interesting ethnographic anecdotes, but were crucial for making my conceptual argument. What helped me to make this decision of addressing these issues in my writing was that most of these problems were also discussed in local social media and in Kiswahili print media. I often advise students and early career researchers who are concerned about revealing ethically sensitive information, especially on institutions or organisations that can easily be recognised later by the reader, to check social media or print media for such information and refer to these sources – not necessarily to your informants, who are worried about exposure, and rightly so. Ethnography rarely detects something completely new when it comes to certain problematic details. If you search carefully, you will find that often these things are addressed in some kind of local outlets. So it is important to look for these other sources so that you do not expose your interlocutors with this information.

Rosa Castillo: *In the article "Ethics, Epistemology and Engagement: Encountering Values in Medical Anthropology," [Dilger et al. 2015] you and colleagues pushed for "the active formation of an academic environment that supports young scholars throughout the research process and encourages them to find (and potentially redefine) their own ethical and moral positions, and to provide platforms for critical and constructive engagements with our own and our colleagues' work" (p. 5). Is there anything else you would like to add as to how this can be achieved in pragmatic terms?*

Hansjörg Dilger: It is particularly important, actually, to promote this discussion on research ethics in groups of students and among early career scholars. We did this with a summer school in 2013 where we focused on the issue of research ethics with the outcome of the special journal issue on *Ethics, Epistemology, and Engagement: Encountering Values in Medical Anthropology* [2015, *Medical Anthropology* 34(1)]. But you can also initiate this discussion in other settings, for instance in colloquia of doctoral students or in research seminars for BA and MA students. And I found it always very helpful to read the texts of other scholars, or to discuss students' own research proposals, to make the potential for ethical dilemmas very concrete. It is important to have concrete examples as starting points for such conversations.

From my experience, it is crucial that early career scholars get the opportunity to ask questions about all parts of the research process, and not to take research relations or the way you apply your methods for granted, as something you can just learn and apply directly from the textbook. "The field" is a highly dynamic situation. It is important that students learn to be flexible when it comes to research ethics; there is no "blueprint" for them out there that they can directly apply in their own research settings. Furthermore, while it is important to have these conversations between students and supervisors, ethics should also become a topic among peers themselves as well as in the field with interlocutors. All these conversations should be conducted in various constellations in order to get different types of responses to your questions. Talking about your positionality, the ethical challenges that you potentially face in a field situation, all this will help students to make ethical choice an embodied research practice. This is not just an individual matter but a collective and professional responsibility of being and becoming an anthropologist – thinking about all these questions and ethical challenges *together*.

Rosa Castillo: *You have raised the importance of engaging in sustained research ethics discussions, whether peer to peer, student-teacher or between collaborators and research interlocutors in which I strongly concur with [Castillo 2018]. To your knowledge, to what extent is this kind of pedagogy undertaken by various anthropological institutions in Germany?*

Hansjörg Dilger: It is hard to tell on what scale this is already happening. My impression from individual feedback is that the awareness of thinking and teaching about ethics or integrating this as a topic in doctoral training is growing, especially among the younger generation of university professors and lecturers, but also among students themselves. I have also received very positive feedback from colleagues who found it important that we pursue this topic at the level of the German Anthropological Association. Some of them have faced

challenges with regard to the absence of institutional ethics review processes in Germany, for instance when they submit a proposal for European funding, when they are planning fieldwork with students in countries where an ethics vote is required, or when they get requests by publishers and journals to provide evidence of ethical approval. For them it is important to have a formalised framework on which they can draw, which they can use in thinking about including ethics in teaching or doctoral training. At the same time, they still emphasise the importance of adjusting ethical review processes to the specific conditions of ethnographic fieldwork.

In this regard, I want to emphasise that we pushed these discussions after my colleagues and I were elected into the board of the German Anthropological Association in 2015. Advancing the agenda of formalising ethics reviews in a way that was still congruent with the discipline's epistemological and methodological standards was only possible because we tried to involve scholars and colleagues *broadly*, i.e. not only the "usual suspects", so to speak, those who were already concerned with or interested in research ethics in any case, but by establishing a broad discussion on these themes. Nevertheless, some colleagues remained highly reluctant in this regard and were explicitly critical towards this push towards a more formalised framework on research ethics and the adoption of a collective disciplinary stance on research ethics reviews.

Rosa Castillo: *Given your criticisms of the dominant ethics governance regime, you advocate ethical assessments that are based on individual academic disciplines and are optional. Can you elaborate on how this can be done? Are there German anthropological institutions that are using such a process and how is their experience with it? What are the challenges and/or pitfalls of making this ethical assessment optional for the researcher?*

Hansjörg Dilger: Let me give the example of my own home institution, Freie Universität Berlin, because I know it best. A Central Ethics Committee was established here in 2019. It is important to emphasise that such ethics committees differ across universities. In the FUB committee there are representatives from each faculty. When a request for ethical approval is submitted, it is always reviewed by two members of the committee. One reviewer is the representative from the faculty from which the request was submitted, for instance the social sciences, and one comes from a different faculty. What I have learned from my tenure on this board between 2019 and 2021, and what I think is quite positive, is, first, requests for approval are only submitted when needed; so there is usually a certain reason for submitting such a request: it is voluntary, it is not mandatory. Second, the standards of the academic discipline remain intact, which was also very important for us in the German Anthropological Association

because we, as scholars in these fields, know best about these standards. The committee members are referred to the relevant ethics documents which have been formulated by a group of colleagues of our discipline [DGSKA 2022]. In addition, however, there is always someone from a different discipline looking at the proposal – which is a good combination, I think. Involving an outsider's critical gaze is important.

The disadvantage of this process is of course that not *every* research project is checked for its ethicality at the level of the Central Ethics Committee. But as I said before, in my view it is not that important to have an institutionally mandatory framework that everyone has to go through, as in the review process of Anglophone countries. The institutionalised ethics review at FUB is reserved for those cases where a funding organisation, publisher or host country of the anthropological research requires ethical approval. Beyond such situations, however, I find it equally important to cultivate an ethical attitude among students and researchers that they incorporate into their research practice, without giving it too much explicit thought. The discussion on ethics should not be a separate aspect of teaching, nor should there be the idea that all ethical dilemmas can be fixed by obtaining the ethical approval of review boards or ethics committee. The emphasis should be rather on strengthening reflexivity and self-responsibility in all aspects of our professional practice.

Rosa Castillo: *Bringing our conversation beyond anthropology, can you elaborate on inequalities that affect research, particularly in relation to the power dynamics between Global North academics conducting research on the Global South and on Black, Indigenous, and People of Colour communities in the Global North? That is, how is research ethics entangled with structural issues of racism and coloniality?*

Hansjörg Dilger: This question is not just an ethical issue, but I think it requires a broader discussion on the state and conditions of anthropological research as such. This discussion should address all aspects of our research: the way we define and practice our methodology, the epistemologies we build on in our analysis and our writing, and the collaborative formats that we want to establish. How can we collaborate in more symmetrical ways with our interlocutors from different communities? How can we involve students in our research? An example from my own experience at Freie Universität Berlin concerns a student-initiated research project on and with refugee women in Berlin. Students came up with the idea for such a project in 2015 when many refugees arrived in the city. Together with an activist organisation, the International Women Space, they wanted to do research on, and potentially improve, the living conditions in collective accommodation centres, especially with regard to the situation of

women. Together with a colleague from our institute, Kristina Mashimi, we guided this project. What we especially tried to do in this collaboration with students and activists was to involve the women from the accommodation centres in the formulation of the research questions, methodologies, and so on – to do this in a participatory way. However, what we realised in the collaboration was that the women often had very different priorities: the living conditions at the accommodation centres mattered to them, but they had to secure their and their families' and children's legal status; they wanted to establish a longer-term perspective of being able to stay in Germany; they wanted to learn German. Others had to move quickly to new places and it was difficult for them to stay in touch with the students or with us as researchers. It was very difficult and challenging under these circumstances to enter into a reciprocal or symmetrical relationship. To enter into such a reciprocal relationship is a very important condition, obviously, for collaboration.

In the end, we were able to complete the project and published the research findings in a book [Dilger / Dohrn 2016], but we were not satisfied with its collaborative character. In a way, the collaboration remained one-sided and so we decided to engage in a second project in which refugee women were to adopt a more active role. The women in this second collaboration were different women, with whom we established contact through a neighbourhood organisation, again in Berlin. The idea that was developed together with the women was that they told their own stories of coming to Germany, how they established themselves in the city (if they were able to do so) and their perspectives of being able to stay. We published a multilingual book out of this project, in which the women told their stories to each other [Kollektiv Polylog 2019].

This collaboration worked better than the first one because we were able to involve women with refugee backgrounds actively in the formulation of the goals of the project and in the way we published their conversations as a book. It taught us a lot about how we can engage in more symmetrical collaboration in the context of vulnerability and inequality. At the same time, it was a challenging collaboration because it required a great deal of resources and commitment that went way beyond the usual context of a seminar. We still have to learn how to do these collaborations more systematically, and I think the multi-linguality is just one aspect of such a challenge – how to do translations in joint seminar settings where people have different linguistic backgrounds, speak Arabic, Farsi, Turkish, German or English. This concerns also the editing and translation of texts when it comes to a joint book project, and so on. It was a very interesting process and we actually need to have a broader discussion on collaboration in postcolonial settings. And this is definitively about ethical issues, I fully agree, but at the same time there are many more things at stake here.

Rosa Castillo: *In relation to collaboration, which features in much of our research work, you have drawn attention to a research and teaching ethics where the needs and expectations of our interlocutors and collaborators are foregrounded. The history of anthropology is rampant with unethical research against many communities in the Global South. And, unfortunately, this continues to be the case. Another issue is the asymmetrical relationship between Global South and Global North knowledge makers, seen, for instance, in how knowledge by Global South scholars is devalued, ignored, appropriated, extracted or erased, and also in terms of how research partnerships are unequally conducted [see also Castillo / Rubis / Pattathu in this issue, part two]. How do we conduct ethical collaborative research given the North-South asymmetries in knowledge production?*

Hansjörg Dilger: The whole issue of knowledge production in postcolonial contexts needs to involve consideration about how multiple knowledge traditions can become part of collaborative research endeavours. The ethicality of collaborations in the global North-South context goes beyond changing our citation practices: this is important, but ultimately what matters is comprehensive epistemological and methodological reorientation of our research practices. It is also about challenging structural hierarchies and the distribution of resources in postcolonial research settings, starting with the very mundane bureaucratic process of funding allocation. To give an example, I was involved in a collaboration with colleagues from the University of Cape Town, the University of Dar es Salaam, Freie Universität Berlin and also from SOAS University of London, in which we worked on Christian and Muslim faith-oriented organisations in the urban public spheres in Dar es Salaam, Cape Town and Lagos. The collaboration also included PhD positions in Cape Town and Dar es Salaam, which were funded and supervised there. Ultimately, however, the money came from the German Research Foundation and was channelled through Berlin. So, in a way, I remained responsible for reporting to the German Research Foundation and for accounting for the spending of funds. Of course, we can do this accounting in collaboration with our partners abroad, and identify and define the priorities of how to spend these funds. But there is still a hierarchy involved, because I am ultimately the one who is responsible to, and thus officially recognised as "funding-worthy" by, the German Research Foundation.

Such issues – as well as the epistemological and organisational agenda we wanted to pursue – were central themes at one of our first project workshops: What texts and concepts did we identify as relevant for our joint research? Where would we hold the workshops and conferences of our collaborative project? The answers to such questions could never be taken for granted. It was very important to address them openly and to see where we ended up. Obviously, there was a lot of potential and room for failure. While we could all do our

best to resolve these issues *together*, there was always the lurking challenge of postcolonial dependencies, which required a particular sensitivity in coming to terms with these challenges. This is not an easy process, and I think that this is something that anthropologists need to address much more systematically than they have done so far: finding adequate modes of collaboration in postcolonial research settings, which challenge existing power relations openly, and finding ways to transform them into more equitable ways of working together.

We do not need to establish an illusion here: the structural context of postcolonial inequalities does not go away just by being identified or discussed. There are always very concrete material challenges: Who can travel where and under what circumstances? For instance, in our research collaboration I was able to travel easily to Cape Town or Dar es Salaam, but my colleagues needed visas – and they could be denied visas for particular reasons or for no reason at all. This is a challenge that constantly reminds us that we need to push for broader discussions on these issues beyond specific ethical aspects, and beyond concrete practices of who to quote or how to resolve a certain money issue. This is about an all-pervasive structural configuration that shapes how we do research in the postcolonial context. It is very important that we address these conditions much more systematically, on various disciplinary levels, and also on the level of professional societies. There is a start being made, but much more needs to be done here.

Rosa Castillo: *These are systemic and structural issues that shape so many aspects of our work, issues that go beyond academia and that would necessitate structural and systemic changes and responses. I turn now to the last question. What valuable insights can we derive from research ethics discussions within the discipline of anthropology for other disciplines or inter- and transdisciplinary approaches such as Area Studies, Global Studies or Gender Studies, to name a few?*

Hansjörg Dilger: I think this question touches on all aspects that we have addressed in our conversation until now: how to do ethically appropriate research in postcolonial settings; how to establish more symmetrical research relations in a Global North – Global South context; how to do fieldwork as such. All of the disciplines you mentioned are doing fieldwork in one way or another, and they face very similar ethical challenges. It is therefore important to connect across disciplines and to share what we have in common with regard to the challenges we face. Furthermore, it is crucial to keep in mind that this is not a conversation that we can have only in a national context, but that we are moving and doing research in an interconnected world. It is very important to engage in conversations on these issues in the settings and countries where

we do research, with the partners and universities with whom we collaborate. It is crucial to give a much more permanent presence to these discussions in all our conversations

Rosa Castillo: *Thank you very much for this conversation. Is there anything else you would like to add?*

Hansjörg Dilger: Maybe just one thing: all of these processes need to start with teaching, because long-term change happens only with the training of the next generation of scholars. So, it will be important to think about how to integrate the topics of research ethics and the postcolonial conditions of anthropological research in our teaching. It is important that students and early career researchers start thinking systematically about these issues, that this critical engagement with the problematic foundations of our research becomes part of their habitus and practice in the years and decades to come.

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Keep Research Ethics Dirty!

Current Debates

Martin Sökefeld

In social and cultural anthropology, the institutionalised discourse on research ethics began in the late 1960s after it transpired that plans had been made by the US army to enlist anthropologists, in order to provide data for anti-insurgency operations in Latin America. The harsh criticism of this "Project Camelot" was a major departure from earlier positions. In 1919, Franz Boas was heavily attacked by the American Anthropological Association (AAA) when he condemned the collaboration of anthropologists with intelligence agencies during World War I, and he was only "uncensored" by the AAA in 2005 (AAA 2005). However, the debate ensuing from the critique of Project Camelot resulted in the association's adoption of a *Statement on Problems of Anthropological Research and Ethics* in 1967, followed by the more comprehensive *Principles of Professional Responsibility*, adopted in 1971 (AAA 1971). The Principles endorsed fundamental ethical axioms such as the rejection of covert research and the principle of doing no harm. No one should dismiss such axioms; they are simple and well-intentioned – but deceptively so, as I shall argue. Since then, the debate on ethics has broadened, and, significantly, it has become supplemented by institutionalised practice.

In this short contribution, I focus on anthropological practices, experiences and reflections concerning research ethics, because anthropologists work more often with real people than in the archives – with real people from all ranks and backgrounds, in "natural" contexts and not in some controlled, sterile lab. The methodology of anthropological fieldwork is first of all based on interaction with our research partners, and like all social interaction, it is potentially replete with ethical issues and dilemmas, even more so because anthropological research is fundamentally open. More often than not, we do not have hypotheses

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to test, we do not know our "samples" in advance, and we do not have a fixed mechanism with which to draw a sample. Even our research questions develop and often change considerably in the course of fieldwork. While hypotheses-based disciplines may regard such changes as invalidating research, we regard this as a positive outcome, as a consequence of knowledge gained.

We are increasingly required to obtain an ethics clearance before starting research. In the US and the UK, this is often standard, even for graduate students' research. In Germany, we lag slightly behind, and such clearance is largely required for EU-funded projects only. The procedure for ethics clearance was originally designed for research in the medical sciences and related disciplines, and it was meant to prevent human research subjects from harm. That is, "Institutional Review Boards" or "Ethics Committees" initially worked for hypothesis-testing disciplines with fixed methodologies, but they have now been extended to encompass many other disciplines involving human research subjects. The impetus for this extension has not come from the disciplines themselves but from funding institutions and universities.

I do not deny the utmost importance of weighing potential ethical issues before embarking on research, but I doubt that standardised procedures are always helpful in this respect. To put it very succinctly: First, such routine procedures may be designed more to protect institutions from harm than to actually protect the subjects of research. Second, having obtained ethical clearance, this milestone may suggest that any ethical issue has been successfully resolved. I will not dwell here on the first point, but the second is decisive, since any such assumption might turn out to be an illusion; in fact, ethical issues begin only once the review process is over. While fieldwork is fundamentally open and largely unpredictable, the ethics clearance procedure requires us to pretend to know in advance what is at stake in the field.

The fallacy of ethical clearance may be aided by the structure of professional ethics codes, to which such procedures respond. Usually, the ethics codes of professional associations have the form of a collection of norms that should be followed. Norms are abstractions meant to provide orientation for how to deal with situations in real life, and they abstract from the inconsistencies and dilemmas with which real life has to deal. However, while norms should be clear and convincing, their application to life is often not so; for example, the norms of not doing covert research and of informed consent are deeply anchored in anthropological fieldwork ethics. They are clear and convincing: of course, the people we interact with during fieldwork should know and understand what we are doing, and they must also have the right to withdraw from the research. Yet, problems start in the field when we have to consider who exactly needs to be informed about our research, and to what extent. Everyone? There are limits of practicality, but this is not the most pressing issue. Doing fieldwork in a highly surveilled field, for instance, might require deceiving some

actors in order to protect others (Sökefeld / Strasser 2016), and other considerations have to be taken into account in dangerous fields (Kovats-Bernat 2002). Is it permitted, for instance, to deceive in order to protect oneself and others? And, if so, to what extent? Where are the limits? Perhaps we come to the conclusion that our main research partners must not be deceived. But then, what about an ethnography like Nitzan Shoshan's about young neo-Nazis in Berlin? Shoshan, an Israeli Jew, assumed the fake identity of an US-American anthropologist in order to work among his radical research participants. The social workers whom he accompanied, and who introduced him to the field, had required him to do so for obvious reasons (Shoshan 2016: xi). Judged by the standards of anthropological fieldwork ethics, however, it was a serious infringement, even if it could be justified and yielded extraordinary insights.

Of course, ethical codes are not fixed once and for all but are sometimes changed and amended. They tend to become more complex and to grow in length over time, in order to do justice to the complexities of their practical implementation. The current *Statement of Ethics* of the AAA, adopted in 2012, is the fifth version after the original statement of 1967. The 1971 version emphatically stated:

> In research, anthropologists' paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first. Anthropologists must do everything in their power to protect the physical, social, and psychological welfare and to honor the dignity and privacy of those studied. (AAA 1971)

The current statement of 2012 is a bit more cautious and restrained, asserting that "obligations to research participants are *usually* primary" (AAA 2012, emphasis added). For Benjamin Teitelbaum, for instance, the qualification introduced by the word "usually" is unacceptable. In his view, obligations to research participants remain paramount even if, as in his case, they are what he calls "radical nationalists" whom others might call "neofascists" (Teitelbaum 2019: 414). Teitelbaum relates that while he intended to do fieldwork among these people in Sweden as a "neutral observer", research drew him into close relationships of reciprocity and solidarity with persons whom many others would strictly reject because of their political ideas and actions. For Teitelbaum, the strict endorsement of primary obligations to research participants whom perhaps most others would see as unlikeable and as "repugnant others" (Harding 1991b) was not the consequence of an abstract norm but an outcome of field experience, which made him challenge the dilution of the original rule.

These examples show the difficulties of applying codified norms to practice. While I do not know whether Shoshan's or Teitelbaum's fieldwork projects required ethics approval, it is safe to assume that the clearance of their research practices would have been difficult. And while I would not necessarily endorse their approaches, both have significantly advanced our knowledge in a research field that is unfortunately growing significantly in importance.

After an extended debate about ethical guidelines, the German Anthropological Association (GAA) chose another approach (cf. the interview with Hans-Jörg Dilger in this issue, pp. 505–518). With the goal of contributing "to the formation and improvement of the ethical judgement" of fieldworkers, the association compiled not a fixed set of rules and norms but a list of six questions intended to enable the reflection of and "differentiated engagement with the ethical dilemmas of ethnographic work" (DGSKA 2016). As a list of open-ended questions, this *Frankfurt Declaration of Ethics in Social and Cultural Anthropology*, adopted in 2009, mirrors the open research methodology of the discipline. It also reflects the insight that often the relevance of questions lasts longer than the particular answers given to them.

Recently, the GAA unfortunately lost the courage to take an unconventional approach to fieldwork ethics. In a kind of anticipatory obedience to potential future requirements of German funding agencies, it adopted a seven-page questionnaire listing all sorts of potential ethical issues and pitfalls to be used for ethical reflection on future fieldwork. It looks a bit like a manual of confession to be filled in for the soul-searching of not yet committed fieldwork sins. Seemingly following the Christian doctrine that no one is without sin, it supplements the question "What are the major ethical issues connected with your research, and what steps will you take to address them?" with the directive: "Please do not write 'none'" (DGSKA 2021). This small instruction perhaps shows that the authors themselves anticipate a potentially rather strategical and routine use of this questionnaire. I regard this as highly problematical, because such routinisation carries the danger of being less attentive – or of being attentive to formality only – and not taking things seriously. And we know that once a routine has been established, it is very difficult to abandon it again.

Anthropology is not concerned with fieldwork ethics only. In recent years, ethics, or moralities, have also become a subject matter of fast-growing significance for the discipline. I suggest taking inspiration from Didier Fassin's anthropology of ethics for the debate on fieldwork ethics. Fassin points to the "purification" through which ethical norms are "extracted from the course of human activities" and by which "social scientists, in particular anthropologists, have tended to reproduce what philosophers generally do when they isolate moral principles or ethical dilemmas" (Fassin 2015: 177f). He also points out that in contrast, the "moral and ethical dimensions of human action are empirically and normatively impure" (ibid.). Fassin emphasises that what contaminates ethics is ultimately politics.

This also holds true for research ethics. Remember that the debate on research ethics was sparked by the political question of whether anthropologists should collaborate with intelligence agencies and the military, or not. The ethical dilemmas of Shoshan's and Teitelbaum's ethnographies also resulted from their intersection with politics. For instance, there would have been no

need for covert research, in Shoshan's case, had he not worked with a politically highly problematic and dangerous group of neo-Nazi youngsters. And nobody would have objected to Teitelbaum's emphasis on the primacy of researcher-informant solidarity had he not insisted on solidarity with a group totally at odds with anthropology's "liberal settlement" (Mazarella 2019). Solidarity is an important concept here, as it intimately links ethics with politics. In our research fields, we cannot show solidarity with everyone. The question of who deserves our solidarity, and who does not, is not only an ethical question but also a political one. It is coupled with our political ideas and goals. This is most obvious in cases of engaged anthropology that select a particular group for one's solidary engagement, albeit at the expense of others.

I venture to assert that in most cases, ethics cannot be uncoupled from politics. Ethics of research is at the same time politics of research, and so we have to turn as much to the political reflection of any research as we have to attend to its ethical consideration. The formalisation and routinisation of research ethics in guidelines and review processes attempts to purify the field by separating ethics from what actually creates ethical issues. But "the moral and ethical realms are not pure – and can only be purified artificially", emphasises Didier Fassin (2015: 205). We should resist the urge toward purification as much as possible, I think, in order to truly attend to the real and messy mix of ethical and political issues rising in the field – and this is of course also a political stance. Let's keep research ethics dirty!

A Peek into Institutional Review Board (IRB) through an Ethical Lens

Tabassum Fahim Ruby

In "Keep Research Ethics Dirty!" Martin Sökefeld persuasively argues that the ethics of research cannot be uncoupled from the politics of research. He makes this argument in the context of the presumption to protect human research participants from any perceivable harm by requiring a research ethics clearance from Institutional Review Board (IRB). Sökefeld does not object to "weighing potential ethical issues before embarking on research" (p. 520). However, he argues that "the ethics clearance procedure requires us to pretend to know in advance what is at stake in the field" (ibid.) and may falsely suggest that "any ethical issue has been successfully solved" even though they "begin only once the review process is over" (ibid.), that is, during and after fieldwork.

Sökefeld raises these concerns against the backdrop of Germany recently institutionalising a research clearance procedure that "carries the danger of being less attentive – or of being attentive to the formality only – and not taking things seriously" (p. 522).

As I concur with Sökefeld's arguments, I contribute to this conversation by further discussing shortcomings of ethical clearance typical for social sciences research. I argue that the United States IRB standards exhibit discipline hierarchies by commanding a clearance model for social sciences research that is more suited for medical sciences research. The IRB protocols also raise some ethical concerns when researching communities who may follow different ethical norms, and they seem to protect the institution against any perceivable bureaucratic allegations and lawsuits more than the research participants, provoking further ethical questions.

The most recent version (21 January 2019) of the IRB application in the United States requires ethical clearance for research that involves collection of "blood samples", "biological specimens", "data through noninvasive procedures" and "research involving materials" (WCU 2019). These descriptions demonstrably show that the form is intended for medical research even when one would argue that social sciences research involves collecting data by recruiting human participants. The question thus is why a more appropriate IRB application has not been developed for social sciences research. Is it because academia and the public regard medical sciences more highly and consider them as "objective" knowledge, in contrast to social sciences research, which they often render as "subjective"? If this is the archetype, then are not we reinforcing discipline hierarchies that feminists have long been critiqued, such as when they show the subjective nature of medical research (Code 1991, Harding 1991a, Haraway 1991).

The COVID-19 global pandemic has further powerfully exposed the subjectivity of the medical field as healthcare professionals learn through trial and error how to treat the virus and develop vaccines for it. They do not magically know when the pandemic will be over, what long-term effects of the virus are on the survivors, how long the vaccines will be effective, or if they will be effective against the new variants. All these questions have yet to be answered. However, if the IRB application is imposing a medical research model not because it regards this model as superior and objective, but because of the wildness and unpredictability of social sciences research that makes it difficult to draft a more fitting ethical clearance, then it remains unclear whether such ethical clearance addresses ethical issues when conducting social sciences research, as Sökefeld argues.

Based on my research experience, I also wish to ask how ethical it is to apply the IRB standards to fieldwork in communities that may hold different ethical standards. For instance, the United States IRB application emphasises

that, prior to conducting research, a consent form must be obtained that informs participants about the scope and use of the data, their rights, and assurance of data confidentiality. While on paper these measures seem important, acquiring a consent form according to the IRB protocol may be problematic in different cultural settings where participants hold informal consent to be trustworthy. On the day of the interview, asking them to consent yet again may leave participants disenchanted because, in their understanding, they already had agreed to participate. They may see yet another need for confirmation as violation of the trust that the researcher and the interviewee have developed. This can be a particular issue where communities regard verbal dealings as an ethical commitment, even if these do not fulfil the IRB protocol. Rather than building trust, such a process seems to weaken it and appears counterproductive. Further, hypothetically, even if the researcher let the participant know in advance that a consent form would be required according to the IRB standards, are we not imposing foreign ethical models onto others?

Since I have encountered precisely such problematic and awkward situations during my fieldwork in the global South, the need to obtain a consent form according to the standards of the global North seems to serve only to reinforce colonial relationships. There is a body of literature (cf. Tomaselli 2016, DiPersio 2014, McCracken 2020) that underscores the limitations of consent forms: they take the outlook of paternalism, intimidate participants, make them feel ignorant and do not take into account any distrust of written documents. To this can be added the fact that such forms fail to acknowledge the participant's initial consent to participate in the study.

Further, the IRB data confidentiality protocol is built to protect individual rights, but in closely knit communities it may not be possible to conduct research privately where other people cannot hear/know the participant's views. Or the participant may deem the holding of a private meeting to be unethical due to sociocultural and religious norms. What should the researcher do in such situations? Should the research be quit, which may mean not acquiring important information? Or would adopting a culturally suited ethics be more ethical, which may mean abandoning some aspects of IRB protocol when engaged in fieldwork? These scenarios underscore the fact that the IRB standards raise more ethical concerns than they aim to resolve, especially when conducting research across different communities that may hold different ethical standards.

As I have obtained IRB approval at several different institutions, I have come to realise the ways IRB standards seek to protect institutions from potential lawsuits in the name of protecting participants. For instance, a couple of years ago my home institution insisted that I obtain a research clearance from the country I was travelling to for my international fieldwork. Since I already had conducted phase I of this research at a different institution that did not ask for such paperwork, I tried to convince the authorities that an external ethical

clearance was not needed for my research. They told me that their funding could be revoked if they approved my application without an overseas clearance, and that my previous institution had illegally approved my phase I research. However, the national board of the IRB where I was planning to conduct my research would not grant me research clearance because it did not fall within their jurisdiction. After months of delay, a senior faculty member suggested that I seek help from the federal office for Human Research Protections. They told me that if my research was not federally funded, I did not need to obtain an ethical clearance from overseas. While their email satisfied my home institution, in the meantime I had wasted the whole summer. I had to delay my research until the next summer and ask for an extension on my internal research funding.

Frankly, with some hurdles, I could have obtained an external clearance through personal contacts, as the IRB board asked me to obtain a document from any university or ministry, even one that had nothing to do with my research. I knew some colleagues who did obtain such a document because they did not want to delay their research and fight the ugly battle. I did not want to opt for that route until I had exhausted all other options because to me it seemed unethical, and antithetical to the whole purpose of IRBs. In the end, it was worth the effort. I wanted to set the record straight and to pave a smoother path for my colleagues and myself for conducting international research next time. One might assume that it was only my university IRB board that did not know the federal requirements, but studies show that IRB boards are often concerned about protecting their institutions (Hessler et al. 2011). Thus, it should not be too much of an ask: let's be more ethical and clearly state in the IRB application whose interests the IRB protocols really aim to protect.

To conclude, it seems to me that the IRB standards are entrenched in the bureaucratic structure of the institution. Therefore, despite their best intentions and genuine desire to safeguard the rights of participants according to an imagined landscape that may even take the form of ethnocentrism, IRB boards must protect their institutions first and foremost against potential lawsuits. To that end, I contend that the IRB protocols are less about attempting to "purify" ethical issues, as Sökefeld argues, and more intended to shield institutions legally, at least in the United States. However, I agree with Sökefeld that "ethics of research is at the same time politics of research" (p. 523). Not only our choice of research topic, but also whose stories we wish to tell, and how we want to communicate them and represent our research participants, are ethical decisions as well as political ones because researchers choose and privilege certain issues, narratives and representations over others. One way to be clear about both our ethics and politics is to engage in the reflexivity that feminists have long advocated (Avishai et al. 2012, Nagar 2003, Nencel 2013, Rajan 2018).

Surveillance, Guidelines or Reflections? Research Ethics Re-considered

Chien-Juh Gu

In his comment "Keep Research Ethics Dirty!" Martin Sökefeld questions the growing trend of routine procedures of ethics reviews in Germany. His discontent centres around two reasonings. First, he contends that the standardised ethics review is designed to protect institutions but is less effective at protecting subjects of research. Second, the "ethical clearance" granted by institutional reviews rarely solves the real ethical issues and dilemmas during fieldwork. Martin Sökefeld ends his essay with resistance against the "purification" of institutional reviews and politics of research. In this response, I reiterate and illustrate his first point by providing examples from my Human Subjects Institution Review Board (HSIRB) submissions. However, I contest his second point that uses dichotomous notions of *pure* versus *dirty* in perceiving research ethics.

I work at a large research university in the U.S. Midwest region, at which HSIRB reviews are a standard practice to ensure the ethical conduct of research. While acknowledging that an HSIRB review can vary by discipline and institution, I base my discussion on my personal experiences. As a sociologist who frequently conducts in-person interviews and ethnographic observations, I have submitted numerous HSIRB documents in the past decade. In this common practice for institutional reviews, researchers must detail the research purpose and procedure; subject recruitment process; anticipated risks, costs, and benefits; and preparations for reducing risks or handling unexpected situations. In U.S. society, in which lawsuits are common, the language used in consent documents and research protocols reveals much about an institution's intent to prevent potential lawsuits. Once in my protocol, I explained that a possible scenario during my interview was that my interviewees might show signs of distress when recalling negative life events. If this should happen, I would stop the interview and provide a list of local counselling services in case my subjects needed the information. My protocol was returned by the HSIRB, which demanded that I added the following language to both my protocol and consent form: "If you [subject] decide to use these counselling services, you will be responsible for the cost." This requirement exemplifies the institution's intent to protect itself and avoid potential lawsuits and financial impacts.

Another major problem of bureaucratic screening lies in reviewers' lack of research expertise. Most of those who review research protocols are not researchers. The few researchers who serve on the review board are often not in

the same field as the scholars who submit proposals for review. As a result, the review comments and revision requests are rarely helpful. Once, my protocol was returned and I was asked to explain my qualifications for conducting the research, although I was already a tenured faculty member and experienced researcher. Another time, I explained in my protocol that I would save my interview recordings on my computer. The review board commented that this phrasing was not specific enough. My application was returned, and I had to add: "After each interview, I save the recording on a USB drive and on my personal computer. I will store the USB drive and subjects' information in a locked drawer in my office to which only I have access." For another protocol, I was asked to describe the step-by-step process after an interview is completed: "I will bring my files and recording to my car right after each interview and drive back to my office without stopping by other places to prevent losing the data." Often, the requests for additional detailed descriptions took 2–3 months of back-and-forth communication when I was revising a protocol for approval, which significantly delayed my research. The truth is, after receiving an approval, I never remembered those step-by-step procedures I wrote in my protocol, nor did anyone from the review board ever check on me to see if I followed those steps. Nevertheless, without those tedious descriptions, I would not have been able to proceed with research. The bureaucratic review process is not only exhausting but also useless for addressing real ethical issues. Power play is also on full display when HSIRB reviewers assume that researchers are incompetent to secure their own data or to conduct research in their own fields of study.

While I agree with Martin Sökefeld that research ethics is rarely a clear-cut, black-and-white matter, I caution against his call for "keeping research ethics dirty" as an opposition to institutional reviews and the use of the dichotomous concepts of *dirty* versus *pure* in perceiving ethics. Naming matters. The terms we choose to convey ideas can sometimes give an unintended impression. The word "dirty" could be misunderstood as "playing dirty," which could be misleading without careful consideration. In my view, research ethics involves professional principles that serve as "honour codes" in conducting research. Our professional principles not only serve as research guidelines, but they also provide the foundation upon which judgement calls are based, especially when encountering unforeseen circumstances in the field. As Martin Sökefeld accurately explains, ethical considerations are often messy and complex. I argue that making good judgement calls in such grey areas requires knowledge of professional principles, deep reflections on the issues, and research experience.

Several years ago, I witnessed a researcher from another university "trick" Burmese refugees into filling out her survey. At the community town hall earlier that day, she spoke as a representative of a government council whose mission was to advocate for Asian-Pacific Americans in the region. At the town hall, many refugees raised concerns about various difficulties they had encountered,

with the hope of receiving government assistance. At the end of the event, the representative announced that she would gladly continue the conversation in another smaller room. As many refugees moved to the other room, the representative's assistants began to hand out an English survey concerning Asian Americans' health behaviours. As most Burmese did not understand English, a bilingual assistant helped translate the questions and filled out the questionnaires for them. Meanwhile, many refugee women asked questions about child-raising issues. The researcher/representative, who did not have a background in education or refugee studies, provided all kinds of advice. My assistant and I were stunned by what we saw because the content in the consent document distributed to participants differed from what was conveyed at the town hall event.

In my opinion, this case exemplifies how researchers could "play (dirty?) tricks" in the field to accomplish their research objectives. Some might argue that the "harm" this researcher caused to her subjects is insignificant, but several questions warrant careful considerations. Was the deception necessary? Was the deception used only to make data collection easier for the researcher? When I initiated a conversation with the researcher afterwards about ethics, I was told that I had no right to judge or intervene in her research. Apparently, we held different values about what constitutes ethical conduct. Later, I used this incident in my research methods class to discuss research ethics.

While institutional reviews have increased in many Western societies, ethics regulations vary across the globe. During my college years in Taiwan in the mid-1990s, the only discussion I heard about research ethics lasted less than one minute in a sociology methods course, when the professor told the class to check the Code of Ethics on the American Sociological Association's website (see ASA 2018). During my master's study in Taiwan, research ethics was never taught or discussed, and institutional reviews were nonexistent (as in some other Asian countries). With the absence of institutional regulations, conducting research seems "easier," but students and researchers miss an opportunity to learn and reflect on how to conduct responsible research and how to protect subjects' well-being. As a contrast to the intensifying "institutional surveillance" in Western academia, this "research freedom" might be desirable for some, but researchers can also exploit their subjects without realising it if they omit ethics considerations. Neither of the systems is of much benefit in training researchers or conducting actual fieldwork.

In contrast to Martin Sökefeld's viewpoint that researchers cannot show solidarity with others, I consider researcher-informant solidarity appropriate and necessary in some research contexts. For example, studying undocumented immigrants requires researchers' commitment to conceal subjects' identity and not report them to the authorities. Such a commitment is not only an ethical requirement to maintaining confidentiality but also a demonstration of soli-

darity. Other marginalised groups in society, such as LGBTQ individuals, social welfare recipients, and people of colour, all deserve researchers' solidarity and compassion. Sociologists' core concern for social inequality places us in solidarity with the powerless, although some reveal their political stances more than others. Sociologists' liberal ideology often prompts many to advocate for socially disadvantaged groups and criticise those in power. During fieldwork, most researchers remain neutral or express their political views implicitly, but sociologists who use a participatory action research (PAR) approach usually embrace their insider's position in collaboration with activists or non-profit organisations. To me, showing solidarity with those who are vulnerable to inequality and injustice reflects sociologists' core mission to advocate for the powerless; understand, uncover and challenge social inequality; and, eventually, help create a better world – a practice that Iain Wilkinson and Arthur Kleinman (2016) call the "critical humanism" rooted in the history of sociology.

In summary, I argue that research ethics is essential training in our profession. Codes of ethical conduct are guidelines for conducting conscientious research, which provide the foundation for making good judgement calls in the field. Although institutional reviews are an unavoidable trend, researchers can make recommendations to their institutions to improve the review process and policies. In fact, my university's HSIRB review process has improved in recent years as a result of incorporating researchers' feedback. In my opinion, ideally, professional associations should form peer committees that offer consultation, which could provide precious support when researchers encounter ethical dilemmas. It would also be beneficial to create more resources for discussing ethical issues for both teaching and research. So far, teaching materials for ethics are fairly limited, and discussions of ethics are not always valued in academia (Gu 2020). As explained above, I strongly believe that researchers must take reflexivity and accountability seriously, and ethics deliberations offer important opportunities to practice both. It is my sincere hope that research ethics will gain more scholarly attention, not in a backlash against institutionalised reviews but as a way to pursue excellence in research.

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Following the Heart
Ethics of Doing Affective Ethnography in Vulnerable Research Settings

Ferdiansyah Thajib

Abstract

This paper chronicles my fieldwork among Muslim queer people in Indonesia. The ethical thrust of "following the heart" lies in the continuous reinvention of research devices in order to keep up with what we feel during, before and after fieldwork, how we are affected by encounters with others, and how others are affected by us. This idea of "following what the heart tells one to do" can be traced back to the old opposition between body and mind, where the head is thought to be rational and cold, and the heart is considered to be emotional and warm. Here, I truncate the metaphor's dichotomous meaning and discuss the potential values of applying it as an ethics of doing affective ethnography in vulnerable settings. Anthropological knowledge production in vulnerable contexts is not only about providing careful interpretation and representation of the affective experiences of our research participants, but also about making ourselves affectively vulnerable as researchers. This ethics is both a method and a source, remaining existentially inscribed into the researchers' embodied realities and continuing to shape our academic practices and everyday livings.

Keywords: Anthropology, fieldwork, methodology, affective ethnography, research ethics

> I feel quite content with how today went, but at the same time nervous. Nervous about what comes after listening to all these powerful stories. What is one supposed to feel when people revealed to oneself their inner fears, hopes, and dreams; personal tragedies and drama, intimate feelings, and aspirations? What could be done with this abundance of feelings? If my role is to retell these stories, how then to attend to all the details, without reducing them to mere illustrations nor ending up with exaggerations? [...] I doubt whether my memory could retain all the details that made these stories so vividly felt in the first place. There were just so many impressions impressing upon me at this moment, almost too many. Affects inundate me. (Ferdiansyah Thajib, emotion diary entry, 30 July 2014)

This was one of the passages in one of my research tools: an emotion diary, written in the middle of my field research, which focused on the multi-directionality of affective dynamics infusing the lifeworld of Muslim sexual and gender

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minorities in Indonesia. My fieldwork took place for around 15 months from mid 2013 to 2015 in three locations in Indonesia, namely Jakarta, Yogyakarta and – where this particular note was taken and where I focus on in this article – in Indonesia's northwesternmost province of Aceh. In a later part of this article, I will further elaborate on the diary's function as a methodological device. Before I do so, I attend to the implications of focusing on affective dynamics not only as a research topic and epistemological premise, but also as an ethical concern.

The entanglement between affects and ethics alluded to in the above citation remains a crucial challenge for anthropological practices of fieldwork and writing. Medical anthropologists Lindsay Smith and Arthur Kleinman address this entanglement through their understanding of ethnographic engagement as an enactment of responsibility to the Other, which:

> emerges less from an intellectual or ethical decision and more from these fundamental emotional processes. And yet like life, fieldwork exposes individuals to the complex interweave of values and emotions in the setting of real-world inexpediency and resistance, so that emotion is almost always multiple, complex, and divided. It is this uncertain, multisided, and often dangerous human reality that we seek to privilege. (Smith / Kleinman 2010: 174)

The thorny dimension of anthropological knowledge production that emerges from affective engagements also reverberates through the questions in this article concerning our moral and ethical responsibilities as researchers when research interlocutors share their innermost feelings, intimate life stories and emotional responses: How are we as ethnographers affecting and being affected by such encounters? What responsibilities do we bear when listening and bearing witness to these often emotionally taxing narratives? Other difficult questions may arise later on, when we return from the field and try to transfer these affect-laden moments into writing: What to make of these experiences? How to navigate our own cognitive and interpretive limits in retelling the research participants' stories? How to take the entanglements of affect and emotion in the field into account, and to translate these embodied experiences and situated knowledges into a language that speaks to those who have not "been there" (Stodulka et al. 2019)? In short, how do we ethically engage with the thick messiness that "the world of affect brought into view" (Stewart 2017: 192)?

All of these concerns are equally pertinent in a research context pervaded by vulnerability (Liamputtong 2007). As I embarked on my fieldwork, assaults upon people of non-heteronormative genders and sexualities intensified across Indonesia in various ways; from the raucous debates in mainstream and social media to widespread stigma and discrimination in various aspects of public life (Thajib 2021, Kantjasungkana / Wieringa 2016). The situation was even worse in Aceh, for a number of reasons that I shall detail in later sections. It was in my encounters with Muslim queer and transgender women there that the ethical dimensions of doing affective ethnography were continuously tested.

This article offers a modest contribution to the discussion around research ethics in volatile contexts, by elaborating an approach which I call "Following the Heart". This approach entails ways of leveraging the affectivity of conducting research with vulnerable groups of people to enable an anthropological knowledge construction that is methodologically, epistemologically and ethically sound. The idea of following what the heart tells one to do, or in the Indonesian popular saying "*mengikuti kata hati*"[1] can be traced back to the traditional opposition between mind and body, where the head is thought to be rational and cold, and the heart is considered to be emotional and warm. In this article, I want to truncate the dichotomous meaning of this metaphor. I engage with the ethical relevance of following the heart as a mode of homing in on the researcher's "capacity to affect and be affected" (Massumi 2002: 5) in relation to all the elements of fieldwork encounters, including environments, places, situations, materialities and people, as well as the writing of an ethnographic account.

In the next section I discuss some of the conceptual and methodological bases of affective scholarship and outline Following the Heart as a more specific ethical research orientation. Afterwards, by focusing on two sets of field encounters, I elaborate how ethical tensions in vulnerable research settings can be reconfigured through this approach. It is worth noting here that the focus of these latter sections is not solely about rehashing the content of my emotion diary, as the beginning of this article might suggest. Rather, they provide an account of my attempts to bring documented affects and emotions into dialogue with my observations of the unfolding affective dynamics in the field. The first set of ethnographic examples consists of situations that illustrate the fragility of research relationships in a context where most of the research participants are struggling with structural and interpersonal violence. It draws on the different challenges in initiating contact and forming rapport with the research participants whom I encountered in Aceh. The second set of examples illustrates emergent situations during the later stage of my fieldwork, where both the research participants and I were confronted with issues of safety and protection. This section particularly frames how, by paying attention to affects and emotion, ethical action in research engagement is not only about nurturing a sense of responsibility of the researcher towards the Other in the field, but also about carving out a shared responsibility *with* each other.

Naturally, due to the vulnerability of the individuals involved in this research, all names are pseudonyms, and details of persons, places and situations discussed have been altered. In the final section I provide some reflections on how

1 This is a transcultural translation, as in Indonesian and in the broader Malay-speaking world, *hati* literally means the "liver", while the English word "heart" translates as *jantung*. But in Indonesian popular culture, hati is understood as the heart, which is metaphorically expressed in various world languages as the seat of emotions.

the already established understanding of research ethics involving vulnerable populations can be further enhanced by embracing the open-ended, unsettling and incomplete facets of Following the Heart as an ethical orientation of affective research.

Doing affective fieldwork

Scholars in various disciplines have recently introduced the term "affective ethnography" to describe emerging research practices that acknowledge the centrality of affect and emotions in knowledge production (Gherardi 2019, Rai 2019). My approach falls in line with this contemporary scholarship as I embrace the affective dimensions of ethnographic fieldwork as a guiding principle in conducting research. This framing may prompt debate, especially since some would argue that all ethnography involves affective engagement; thus, adding the label "affective" is somewhat tautological. But to me this framework is particularly useful for contemplating the ethical potential of paying attention to the affective dynamics that infuse research engagement in vulnerable settings and with vulnerable subjects.

In the field of organisation studies, Silvia Gherardi (2019: 742) defines affective ethnography as "as a style of performative ethnographic process that relies on the researcher's capacity to affect and be affected in order to produce interpretations that may transform the things they interpret". This research practice, she continues "acknowledges that all elements – texts, actors, materialities, language, agencies – are already entangled in complex ways and that they should be read in their intra-actions, through one another, as data in motion/data that move". While I draw some conceptual affinities with Gherardi's definition, especially in her theorising of affective entanglements as a resource for ethnographic practice, my research practice departs from her framework of "style". She defines "style" as a set of aesthetics that can be recognised "when you see it (or read it), and the characteristic features are performances that could have been otherwise" (Gherardi 2019: 745). Gherardi's conceptualisation of affective ethnography is situated in the debate of "post-qualitative methodologies", which relies on the researcher's idiosyncratic disposition and personal aesthetics. For me as an anthropologist, however, my long-term engagement with affective ethnography, although similarly based on embodied knowing, has been primed through the quest to foster a transparent and systematic way of understanding the researcher's positionality in methodological terms.

Two main strands of intellectual projects prefigure this methodological emphasis. The first one is the anthropological debate on self-reflexivity, positionality and research ethics from the late 1970s to the early 1990s (Rabinow 1977,

Clifford / Marcus 1986). The second affinity can be traced back to the endeavours of feminist anthropologists to reclaim emotions as ways of knowing the self and the world (Lutz 1988, Visweswaran 1994, Wolf 1996). The call for a heightened sensitivity to affective dynamics in ethnographic studies found its critical momentum some two decades later, in the works of psychological anthropologists such as James Davies (2010), Dimitrina Spencer (2010) and Maruška Svašek (2010). Their varying lines of argument coalesce around an emphasis that the ethnographers' affective practices and emotional experiences not only provide important insights into the lifeworlds, people, spaces and places they study, but also carry valuable methodological and epistemological import when comprehensively and systematically attended to.

The challenge to advance the methodologically and epistemologically rewarding aspects of field affectivity has more recently transformed into a research paradigm that numerous scholars have dubbed "affective scholarship".[2] This line of study focuses on the practical implications of taking affects and emotions as sources of research insight. This suggests the elaboration and diversification of heuristics to help ethnographers capture the affective dimensions of research encounters, construct them into knowledge, preserve them as "data", interpret them and convey them through writing.

My own research has benefited from this paradigm, mainly through my involvement in the project "The Researchers' Affects", a collaboration between social and cultural anthropology, literature, science and primatology based at Freie Universität Berlin, Germany, and the University of Bern, Switzerland.[3] Through this project, I was able to study the relevance of affectivity for fieldwork and ethnography from various angles, including from an ethical perspective. For a research practice that takes seriously the affective valence of fieldwork, to exclusively give accounts of how research interlocutors experience suffering, despair, joy or mourning without making ourselves vulnerable as ethnographers is tantamount to exploitative research conduct. What is at stake here is "the risk of reproducing simplifying dichotomies by putting them into emotional 'hot seats,' and presenting the anthropological persona as 'cool', and more 'reasonable' in abstracting 'thoughts' from 'feelings,' or 'culture' from 'nature'" (Thajib et al. 2019: 15).

Furthermore, through the Researchers' Affects project I had various opportunities to co-develop methods that can support ways of putting affective scholarship into practice. One of the research devices resulting from the collaborative project is the above-mentioned emotion diary. Designed as a semi-structured device that can be used by fieldworkers to chronicle their affective states and experiences, the accounts preserved in the emotion diary can also be employed

2 Cf. Stodulka et al. 2018, 2019 ; Davies / Stodulka 2019; Thajib et al. 2019.

3 For more information on the project The Researchers' Affects, see http://www.loe.fu-berlin.de/en/affekte-der-forscher/index.html.

to complement more conventional accounts of the phenomena studied, such as field notes, recorded conversation transcripts, audio recordings and visual images. The diary serves various other purposes, such as providing psychological and epistemological support. I have detailed most of these purposes elsewhere (Stodulka et al. 2019). Here I want to link the diary's strategic purpose of fostering "affectively attuned ways of navigating field encounters" (Stodulka et al. 2019: 285) to the notion of Following the Heart.

As I revisited the entries of my emotion diaries for writing this article, I was struck by the detailed descriptions of fluctuating "field emotions" (Stodulka et al. 2019) or "field affects" (Stodulka et al. 2018) in the records of my research sojourn in Aceh. These comprise fear and anxiety regarding my research interlocutors' safety, elation when a community embraced my presence in the field, bewilderment when people failed to reply to my invitation to meet up, the boredom of waiting until they become available, or disappointments over unkept appointments. Some entries also recount feelings of isolation and loneliness in new environments, the thrill of going to new places and meeting new faces, and feelings of apathy due to physical exhaustion and the constant mental meandering between all these different emotions. In my emotion diaries, I also kept track of implicit, otherwise elusive, moments of shared vulnerability with the research participants. These include episodes when I was swept away by inexplicable sensations while engaging with a certain interlocutor, when I felt changes of intonation in the research participant's voice or my questions were met by silent pauses and other non-verbal responses, or when I sensed the subtle shifts of atmosphere in the interaction between the participants and myself or in our immediate surroundings.

In his theorising of "multi-sited ethnography" George Marcus (1995) has suggested that in a world where spatial and cultural boundaries coalesce, what constitutes the "field site" is increasingly constructed by ethnographers, as they decide which of the various scenes of interaction are relevant for their research. The modes of constructing an ethnographic object in this sense include literally following people, things, metaphors, plots, stories, allegories, lives, biographies and conflicts (Marcus 1995). I invoke Marcus's constructivist strategies here to highlight how the constructed nature of the "anthropological site" often involves a degree of pragmatism and serendipity (Clifford / Marcus 1986, Marcus / Faubion 2009). But rather than framing influential yet unplanned moments of gaining insight as the materialisation of a free-flowing external force, Following the Heart, as I intend it, involves continuous reinvention and modification of our research practice as our bodies not only become physically and emotionally affected by vulnerable situations in the field, but also ethically co-shape these situations.

As I shall further describe in later sections of this article, many of the steps that I took in the course of fieldwork mainly depended on what to me *felt right*.

This approach has coloured my considerations, among others, of how to engage with social relations and material-spatial environments; with whom I established close bonds in the field sites and from whom I distanced myself; and which methods I used to elicit stories from the participants. At the same time, this "feeling right" was never entirely based on my own experience but was generated through moments of sharing feelings *together with* others in the field.

In this sense, Following the Heart is not about discarding research techniques and procedures that have been "rationally" planned and thus replacing them with gut impulses. Rather, it is about embodying and attending to affective relationality as a key resource for ethical research practice in vulnerable settings. I now turn to some snapshots from my fieldwork to illustrate how ethical orientation is crafted by feeling my way through the messy sides of fieldwork experience and relationships.

Grasping through fragile connections

What led me to conduct field research in Aceh was, first and foremost, a longing to learn about my ancestral origins. I was raised in the capital city of Jakarta in an aspiring middle-class family, then I spent a large part of my early adult life in Yogyakarta. Both of my father's parents migrated from Aceh to the more densely populated island of Java in the late 1960s to seek a better livelihood. I had never visited the north-westernmost part of Indonesia before I started my fieldwork there in 2013. My budding imagination about this place was mainly guided by the stories told by my elders.

As I grew up, it was common for people in my surroundings to make assumptions about my cultural heritage as an Acehnese, determined through my given first name, Teuku, an ethnic title usually given to a male born into a noble family in that area. I remember I often blushed after shaking my head whenever people asked me follow-up questions, such as: Have you been to Aceh? Do you speak the language? During my teenage years, I spent much time following the news of the bloody armed conflict between the military and armed combatants who demanded a fully independent Aceh province. This conflict, which had been ongoing since 1976, had prompted my rather young mind with a longing to better understand the Acehnese people and their culture.

The urge to connect with "Aceh" turned even stronger when the tsunami disaster on 24 December 2004 annihilated large parts of its provincial capital of Banda Aceh and most of its north-western coast (Samuels 2019). As people in the region were still recovering from the tsunami, the approximately 30 years of civil war came to an end in 2005. In parallel to that, as of 2001, Aceh had been granted special autonomy, which allowed the provincial government to

implement Shari'a (Islamic law). Afterwards, reports of corporal punishments for unreligious conduct and violent abuses of human rights began increasingly making headlines in international media whenever the name "Aceh" was brought up, and they continue to infuse the (global) public imagination of Aceh as "radical, dangerous, backward" (Kloos 2017).

Initially my plan was do research only in Aceh and Yogyakarta, but due to unfolding situations that I will explain shortly, Jakarta was added to the list of research locations. As I was about to embark on my fieldwork to Indonesia in early 2013, my plan to investigate the lived experiences of sexual and gender minorities in Aceh generated various cautionary warnings. My supervisors had advised me to skip Aceh entirely if it meant risking not only my safety, but also the security of potential research participants. Colleagues compassionately reminded me to stay alert during my stay in the region, admonishing me to fly out the moment things appeared too hazardous. These concerns were shaped by the escalating violence that sexual and gender minorities had been experiencing in post-conflict, post-tsunami Aceh, and this was compounded with news of the ongoing local government's deliberation of a by-law, called Qanun Jinayat. This by-law was introduced to criminalise same-sex behaviour and, by extension, gender non-conforming expression; violations carry a maximum punishment of either a public flogging of 100 lashes, a fine of 1,000 grams of gold, or 100 months in prison (Human Rights Watch 2016).

The whirlwind of personal expectations and built-up anticipations within me began to unravel as soon as I arrived in the provincial capital of Banda Aceh. My attempts to establish "first contacts" with potential research interlocutors in the city were arduous. At the beginning I was hoping that I could connect with Northwestern Light (NL), the only local NGO which focuses on advocating for the rights of sexual and gender minorities in the region. I followed the recommendations given by a number of queer activists who told me during my transit in Jakarta that NL would be the perfect intermediary for reaching out to potential research participants in Aceh.

While I did manage to have a few preliminary meetings with three NL activists to talk about my research plan, I soon learned that they did not have the capacity to provide the formal support that I needed, except for sharing a few contacts from their personal network who they thought could be asked to join my research. The NL activists stayed reserved with regard to my request for support because the organisation itself was forced to go into hiding at that time, after facing increasing surveillance from the state apparatus and neighbourhood vigilantes. During the few times that I visited the NL office, its door and windows were always completely sealed from inside. The activists even went so far as to burn all their official documents to "remove evidence" after hearing a tip-off that their office was about to be raided by the Shari'a police force, the Wilayatul Hisbah (WH). Not wanting to burden them further with my research

agenda, I decided to independently follow up the contacts that they had shared with me.

Having NL as a reference point indeed paved the way for me to arrange initial meetings with several prospective research participants in Banda Aceh. When introducing my research to potential participants, I usually started by disclosing my personal and professional details. This approach, known as the researcher's self-disclosure in research methods, has been deemed as essentially important in conducting research with vulnerable and hard-to-access groups, in order to "level the playing field" (Dickson-Swift in Liamputtong 2007: 72). But many times, my efforts to initiate a basis for reciprocal sharing with people I met during the early stages of fieldwork in Aceh were futile. Their responses included either hinted, unspoken refusals (such as not responding to my follow-up invitations via online chat or phone messages for another meeting) or, at worst, direct antagonism. One person adamantly refused to be part of the research, for example, because they were suspicious that the research was following a scandalizing agenda. Although I am an Indonesian national, the fact that my research was hosted by a German university made me suspected of reproducing a "Western-biased" approach to representing Muslim cultures in Aceh.

Perhaps even by paraphrasing a part of that conversation here, I am already crossing some ethical lines, since I never did get the person's consent. Yet I do so to illustrate that this strong reaction is only a fraction of the wider social practices of silence and secrecy (see Samuels 2016, Lovell 2007) that are normalised by Aceh's geopolitical conditions. For example, the indifferent responses I received can be understood in relation to the post-tsunami and post-conflict situations, in which many people had become exhausted by the presence of researchers asking various kinds of questions, thus instigating silence and secrecy as strategies of what Sherry B. Ortner (1995) describes as "ethnographic refusal".

Another reason for the climate of discretion can be found in a widespread sense of vulnerability shared among sexual and gender minorities in Aceh in the face of increasing stigma and public persecution. On the one hand, I have accepted the fact that my research topic entails the risk of what scholars have described as "stigma contagion" (Kirby / Corzine 1981, Liamputtong 2007). This means that the researcher shares the stigma of the population that they study. But on the other hand, this "guilt of association" may extend to the research participants. Being seen around someone whose topic of research is considered a social taboo may consequentially expose vulnerable individuals to further risks of unwanted disclosure.

After spending almost two months looking for research participants, I finally met Denny, a 23-year self-identified gay man who welcomed me to join in his everyday activities. I often tagged along when he met different groups of friends to hang out (*nongkrong*) in the *warung kupi* (coffee shop). In Acehnese urban

settings, the *warung kupi* is the main gathering place for young people. One of the most salient features of these coffee shops is the limited access for women into these spaces (Siapno 2002). This gendered public space often helped to conceal the (homo-)sexualised aspects of the interactions within the particular group of men I spent time with in the region.

To accommodate the contingent nature of social space while protecting the physical and psychological well-being of the research participants, I employed a certain degree of adaptability in order to befit and respect the participants' mobility, their sense of temporality and strategies of discretion in circumventing risks of violence. Even in conversations that took place in private settings, I always tried to remain attentive with regard to how the questions or topics that I raised were perceived.

During my interactions with Denny and his friends, for instance, I worked with a common practice of "everyone is in the know" (*tahu sama tahu*) (Juliastuti 2008, Anderson 1966) when referring to same-sex practices and desires. This involved a tacit agreement that we both knew what we were referring to without explicitly addressing it. We employed indirect ways of addressing homosexual identifications, such as by alluding to terms like, *aku kayak gini* ("I'm like this"), *dia kayak gitu juga* ("He is also like us") or *tertarik sama lelaki lain* ("attracted to other men"). This code-switching allowed both the research participants and me to effectively engage in the topics being discussed without having to take recourse to debates on terminology and concept.

Amidst this process of building rapport with Denny and trying to connect with more people in Banda Aceh, I became very ill and had to return to Jakarta to recover at my mother's home. Two months later, still recovering from a nerve-related illness, I decided to return to Banda Aceh to continue my fieldwork. But then I discovered that the few contacts that I had previously built had dwindled, since I had not been able to maintain communication with them while I was bed-ridden. My efforts to restore our relationships during my second visit to Banda Aceh were unsuccessful.

Feeling exasperated, I began to have second thoughts about continuing my fieldwork in Aceh. Not knowing what to do, I reached out to a new colleague from the Netherlands, Annemarie Samuels, for advice. Annemarie was a post-doctoral researcher who at the time was also conducting ethnographic fieldwork in Banda Aceh. After listening to my dilemma, Annemarie encouraged me to endure, while not overstressing myself for not finding enough research participants. Instead, she suggested that I review all the stories that I had listened to thus far, to see whether they carried some important ethnographic insights that I might have overlooked and that still needed deeper examination. Her response made me recollect the stories shared to me not only by the interlocutors I had met in Aceh, but also by a few old friends in Yogyakarta and Jakarta long before the fieldwork had begun. Aside from rekindling my motivation to

keep going in Aceh, the conversation with Annemarie prompted me to involve those who I considered as old friends in my research. I eventually added Jakarta as a "third field site", which during the initial stage of my fieldwork was more a place of sojourn between my research trips to Yogyakarta and Aceh.

With regards to the ongoing research in Aceh, I decided to widen my search for research participants and visit different peri-urban areas outside of the provincial capital on the northern coast of Aceh. It was during my visit to a district town called Namu that I met Maya and her community. Maya is a 27-year-old *transpuan* (or transgender woman), an acronym combining the first syllable of the English word "transgender" (trans-) and the last syllable (*-puan*) of one Indonesian word for "woman" (*perempuan*).[4] She had not only responded very enthusiastically when she heard about the nature of my research but also offered to host my stay in her hair salon, Salon Primadona, during my fieldwork in Namu, which in the end lasted eight months in total.

I was able to establish trust and rapport with her and her group of friends. But even as intimate bonds formed between myself and these research participants, they were often subjected to products of structural violence. The vulnerable nature of the research relationships was mainly manifested through the ways in which the research participants and I affectively negotiated safety and protection in our day-to-day interactions.

Negotiating safety and protection

It was mid-Ramadan, June 2014. By that time, I had been staying and doing participant observation at the Salon Primadona for a couple of months. One afternoon, Nanda, one of the *transpuan* employed at the hair salon, asked me to join her in buying snacks and foodstuffs at the town's market for the breaking of the fast. Achiel, a *transpuan* who worked at another hair salon located adjacent to Salon Primadona, also joined us. We took to the streets of Namu on two scooters. I rode my own motorbike while Achiel rode the other scooter with Nanda perched on the backseat. That day, Nanda wore a pair of tight jeans, a full face of make-up and a T-shirt adorned with a colourful scarf that draped down her shoulder. Achiel, on the other hand, once told me that she did not like to wear women's clothes. On that particular afternoon, she was

4 During my fieldwork seven years ago, the term *waria*, which is an acronym of two Indonesian words: *wanita* ("woman") and *pria* ("man"), was widely used by both ingroups and outgroups in local and national daily parlance. However, today, particularly among Indonesian transgender activists, the term is increasingly being replaced by *transpuan*, as a bid towards self-determination and broader social justice (Hegarty 2022). Following a recent debate on social media regarding the use of *waria* or *transpuan* as a descriptive term, I contacted Maya to ask about her preferred term of description. While she explained that the term *waria* is still used today within her community, she advised me to use the term *transpuan* to educate the public towards social inclusion.

bare-faced, but wore stylish accessories such as fashionable blue-framed sunglasses and high-heeled wedges. She did not wear a helmet while riding the scooter.

When Nanda and Achiel asked me to accompany them to the market to buy food, the string of cautionary comments that I had received from my peers and supervisors before embarking for Aceh flashed through my mind. But there I was, tagging along behind Achiel and Nanda on the road that connected Namu to a neighbouring town further to the south. We had just missed the turn that would take us directly to the market. Apparently, Achiel and Nanda had decided to take a longer route to pass the time. I did not protest and continued following them. From inside my helmet, I felt my gaze shifting. The landscape that unfolded before me looked like a film scene shot in slow motion. Nanda let the wind play with her glaring red hair, dishing out her smiles to passers-by on the sidewalks, while occasionally turning down her head bashfully. Meanwhile, Achiel drove the motorbike single-handedly, waving her free hand sideways, and giggling.

On the sidewalks, people busily bought and sold food. This traditional pre-breaking-of-the-fast activity, colloquially called *ngabuburit*, is popular in many Indonesian towns and cities. I saw many women and men, the old and young, staring at my friends riding the motorbike in front of me. Some looked surprised or laughed, others with indifference, and a few frowned with contempt. People riding in the opposite lane turned their heads, a few even stopped, took a detour, and followed us. I could hear whistles and shouts addressed to the women. And from behind me I could sense a couple of young men on their motorbikes trying to come after Achiel's scooter. I was stunned when some of the chasers not only tried to chat with Nanda and Achiel as they passed by, others even moved their vehicles very closely in order to touch or tap them. I feared that these hands were aiming not only to touch but also to harm them. After a few more kilometres, our small entourage took a detour, only to experience a similar curiosity from the public's gaze. I could easily blend in with other motorbike riders because, aside from my gender-conforming appearance as a cis male, I was wearing ordinary clothes, and the visor of my black helmet was closed.

When we got back from the market, I bombarded Nanda with questions. What was she doing? Was she not troubled by the men who tried to touch her on the motorbike? Was she not afraid that people would hit her? She just giggled and blamed Achiel: "It's all because of Achiel's blue sunglasses, the colour is so striking (*norak*) that people keep on looking at us." She continued:

> I'm not worried, because I am from here. People here already know me since I was a child or at least had seen me before once or twice. They are quite used to me; they can accept me. If anyone did try to bother me then most probably that person is not from here, a foreigner, who still sees me as weird or a perfect target for jokes. But I'm sure the local people here will protect me if this happened. (fieldnote, 15 June 2014)

Unsatisfied with Nanda's answer, I spoke to Maya, as I felt closer to her. I asked her why she thought Nanda acted as if she was without concerns for her own safety. To my surprise, Maya also averted my question, by responding: "You have to understand, Nanda is still young and she has never lived outside of Namu before. She still has a strong desire to be seen, who else can appreciate all that effort in making herself beautiful, all that make-up worn and dresses bought if there is no one that could see her?" Maya continued, "I told her so many times to *jaga diri* (protect oneself), but she is still young, so what can I say?" (fieldnote, 15 June 2014)

The above vignette illustrates the push and pull between the rush of concern that I felt regarding the research participants' safety and their ways of downplaying it. Similar events occurred throughout my fieldwork in Namu. However, this does not mean that matters of safety are taken lightly by the community members. Many of them have not only been subjected to injurious speech in public spaces, but also to various forms of physical harassment in their private premises. This is because, on the one hand, *transpuan*-owned hair salons have become the only space outside of private homes where the community members can socialise in relative safety, away from public admonition. On the other hand, the fact that the hair salon is the only place that accommodates *transpuan* sociality has made it a routine target for harassment and control by different power actors, including official state police, local moral police and neighbourhood vigilantes. The fragility of the *transpuan* hair salons cannot be overstated.

In response to the contingent nature of much of the violence that has plagued *transpuan* social existence in Namu, Maya and her friends employ various tactics of self-protection. This is mainly captured by the expression *jaga diri* ("protecting oneself") earlier stated by Maya. This phrase, used interchangeably with other words such as *buat-buat diri* ("behaving oneself") or *jaga-jaga* ("be cautious"), often came up in casual conversation, such as when the speaker and the person being addressed were about to part ways, or the addressee was being reprimanded for trivialising security concerns. *Jaga* means "to protect", whereas *jaga diri* means either "self-protection" or "self-care". In a relational context, the idiom is used to refer to the virtue of knowing one's place in the social world as well as ensuring the maintenance of one's social position before others. This latter meaning is also expressed by the term *buat-buat diri*, which suggests mindfulness in performing / presenting oneself to the world. The phrase *hanya jaga-jaga* carries the double meaning of "taking precautionary steps" while at the same time describing a kind of "just in case" situation, an active anticipation of potential harm.

The practice of constantly attuning oneself to risks of violence is most apparent in situations where *transpuan* collectively navigate the public spaces in

Namu. Throughout my entire stay in the district town, I counted only a few times when Maya, her friends and I actually went out of the salon together during the daytime. The outdoor activities that they did together as a group, and that I had the chance to join, always happened close to midnight, right after the closing-time of their hair salons. On these occasions, we always rode different motorbikes to go to one particular food-stall located on one corner of the streets near the town centre. Once we arrived, the women would banter with each other while enjoying a light meal and unwinding after finishing their 12-hour work shift.

This practice also framed my research interaction with the *transpuan* in Namu. When I made appointments with Maya to meet outside of Salon Primadona during the daytime, for example at a coffee-shop, it was not uncommon for her to change the rendezvous-point at the last minute. She did this to *jaga-jaga*, which usually entailed closely surveying the coffee-shop from afar. If the crowd of men sitting at the designated coffee-shop seemed unfriendly, she would suggest a new location to meet.

My presence as a researcher coming from "outside" was also subjected to such precautionary steps. This is exemplified by an instance during a focus group discussion joined by 13 *transpuan* in Namu that was held shortly after my arrival. When the discussion was about to start, one of the focus group participants rejected my request for permission to audio-record the unfolding conversation. She was worried about her own safety if her voice was recorded and became publicly available, echoing the broader climate of discretion that I described earlier as rooted in the region's historical context and its increasingly draconian legal landscape.

In a way, I was also brought into the fold of these protective gestures. At the beginning of my stay in the Salon Primadona, I was often startled by the slightest intrusions: the sound of car tires screeching on the intercity road in the wee hours, the curious gaze of the salon customers upon seeing me hanging around the salon day in, day out, the numerous stalkers in cars and on motorbikes that followed our entourage every time we had our routine midnight snacks. I was constantly haunted by stories of local young men or the moral police raiding houses whenever unmarried men and women stayed inside for too long after dark. In the beginning, I tried to repress this anxiety, but then I decided to ask Maya what she told people or neighbours if they wanted to know who I was and what I was doing there. She casually responded, "Don't worry, I told them you are one of us, of course". While I have never fully understood what she really meant when she said I was one of them, I would silently repeat this remark like a mantra, whenever feelings of unease began to well up inside me.

Even without having a stranger such as myself spending extended periods in her hair salon, Maya had been pre-empting rumours and gossips from arising

in the close neighbourhood of Salon Primadona. She regularly frequented the neighbours' houses for a small chit-chat. This she did as a part of her tactics of *bawa diri* ("carrying oneself"), which involved maintaining connectivity with social surroundings. But these efforts were not always painless, since Maya told me that often these conversations ended with a bitter aftertaste, especially when her attempts to be cordial with the neighbours were met with grudging responses.

The everyday practices of Maya and her friends in navigating risks of violence during my fieldwork in Namu have compelled me to think about how vulnerability is not fixed universally across time and space, but rather formed relationally. Similar accounts have also been shared by scholars who address the importance of recognising how vulnerability operates beyond categorical labels, as it is constituted through social and spatial processes (Mitchelson 2017, Taylor 2013). The relational emergence of vulnerability is indicated by the friction between my initial assumption that *all* non-heteronormative subjectivities in Aceh were highly vulnerable to violence, and thus in need of protecting, and the ways that the research participants experience vulnerability as a resource for enacting collective agency through self-protection. They did so by employing tactics of negotiating social acceptance with their immediate neighbourhood and by habitually inhabiting the public's gaze. Hence, self-protection for them is part and parcel of communal care work, rather than an enactment of individualised capacity. Maya had even passed on this form of care work to me, the researcher who had the privilege to leave when things got dangerous, despite her own vulnerable social position.

Again, all of this is not to suggest that the research participants are not vulnerable human beings. Nor is it the case that as the researcher, I am ultimately as vulnerable as the research participants. The spaces of difference between us remain noticeable. The point is that for the *transpuan* in Namu, vulnerability constitutes a world of socio-spatial boundaries that demand constant negotiation. The ethical thrust of Following the Heart lies in attuning oneself to this immanent sense of vulnerability. The initial reactions of fear, concern and self-doubt documented in the emotion diary helped me in reconciling with the limits of my actions and positions when it came to the safety of the research participants. At the same time, they enabled me to sidestep paternalistic forms of protection and establish in its place an understanding of how safety in the field is relationally produced and is constantly (re-)made and negotiated between the research participants and the researcher.

Embracing open-endedness

The two ethnographic snapshots above show how my insistence on Following the Heart enabled me to map alternative pathways for engaging with vulnerable subjects and provided the opportunity to investigate how vulnerability and political agency form not a simple opposition, but rather an entanglement. In a field site that is rife with structural and interpersonal violence such as in Aceh, Following the Heart means accepting the possibility of being received with suspicion and doubt by prospective research participants. It remains crucial to attune ourselves to the often-implicit boundaries set by those whom we encounter. This involves not only listening to what is expressed and left unsaid by the interlocutors, but also being aware of how our bodies respond to them. No less important is the affective capacity to hold back, being fully aware that our invitation to participate in the research could still be received as an imposition, or even a safety risk, despite the research's good intentions.

Whereas traditional research ethics guidelines always stress the need for the researcher to protect vulnerable research participants, my evolving relationships with the research participants in Aceh diffused the sole power and responsibilities of the researcher to protect others into a common but differentiated quest for protection that necessarily takes place in relation *with* others. Affectively tapping into this relational vulnerability also shaped other aspects of the research project. In the field interaction, this approach allowed me to sensitise myself to the embodied knowledge of the research participants and to the ways we were mutually affected by each other and by our immediate surroundings as pathways for navigating "hairy" circumstances. In the process of analysis and writing, Following the Heart translates into the challenge to do justice in representing the research participants' multiple engagements with vulnerability. My greatest challenge in the writing process was about finding ways to ethically give an account of how the research participants' everyday experiences of vulnerability are neither about testimonies of victimhood nor tokens of heroism, but that they take place as affective processes where efforts to shun risks of violence, and attempts to endure them together, intermesh.

As a coda to this concluding remark, it is worth pointing out that a few months after completing my fieldwork in Aceh, the regional government put into force the criminalisation of homosexuality through the passing of Qanun Jinayat. Since then, the number of arrests of those suspected of being lesbian and gay in Aceh has escalated. At least four men have been subjected to public caning for engaging in sodomy thus far. Concurrently, especially since 2016, sexual and gender minorities in Indonesia, subsumed under one acronym as LGBT, have become the subject of public controversy on a national scale (Thajib 2021, Kantjasungkana / Wieringa 2016).

This harrowing string of events, in both the regional and national contexts, also exacerbates the living situations of the *transpuan* community in Namu. As I was busy writing my dissertation in the safety of my home in Berlin in early 2018, Maya and her friends had to flee Aceh after five hair salons owned and operated by *transpuan* were raided in a joint operation of the national police force and Shari'a police. They detained 12 *transpuan*, forced them to strip off their clothes and then cut their hair in public. While Maya and her group of friends managed to escape by seeking refuge outside of Aceh and remaining there for a couple of months, upon their return to Namu they learned that gender nonconforming expression in hair salons had been officially banned in Namu as well as in other towns across Aceh. Maya told me that since the ban, she and her friends have avoided group meetings or spending time in the hair salon outside normal working hours. The moral police regularly visit the salon and intimidate her employees into no longer wearing women's clothing. The life inside Salon Primadona, as I knew it, has undergone drastic change.

The vulnerability of sexual and gender minorities in Aceh has reached such a staggering degree that I wondered whether, if I had started my fieldwork only a few months later, it would still have been ethically acceptable to conduct the research in the way that I did. In fact, would it be ethically possible at all to continue doing research when the lives of those being studied are beset by such heavy surveillance and persecution?

The dilemma suggests more than just a speculative musing, as it instils further ethical consideration of the research's "afterlife", especially in disseminating the research outcomes within the academic community and to a broader audience. How to share these insights without causing greater ramifications for queer and trans lives in Aceh? To whom can this knowledge be passed on and to what extent can this be done? In light of these emergent conundrums, I contend that questions on what feels right will continue to shadow every one of us who conducts research in volatile contexts. As many of our research participants still have to endure layers of vulnerability long after we have finished our fieldwork, for us as affective researchers, the ethics of Following the Heart will time and time again be put to the test throughout our academic journeys and everyday lives.

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Navigating Respectability in Patriarchal Contexts: Ethical Dilemmas of Women Researchers in Pakistan

Sarah Holz, Abida Bano

Abstract

The article explores the field research experiences of two women researchers who navigated patriarchal social dynamics in Pakistan. We use respectability as a lens to explain how we established ourselves in our research contexts and how we negotiated our positions. Drawing on extensive fieldwork for our PhD studies, we show the moral conundrums of constantly moving on a spectrum of being both a "respectable" woman and a "woman researcher". We had to navigate both identities carefully to access research respondents and build rapport. This meant at times adhering to patriarchal gender norms, while in other situations, willingly or unwillingly, transgressing them. We show that we had to constantly mediate between professional goals, personal and social norms and values, and our own wellbeing. We thereby seek to contribute to discussions on the vulnerability of researchers and the ethics of care.

Keywords: Research ethics, fieldwork, gender, patriarchy, feminism, ethics of care, Pakistan

Introduction

When conducting social research, we, as researchers, often encounter unexpected and unplanned situations that cause some form of unease. We have to react to these situations instantaneously, and such responses might significantly impact rapport with respondents and our interactions in research settings. By "unexpected and unplanned situations", we are referring to occasions when we have to mediate between professional standards and goals, our own emotions and personal norms and levels of comfort, and prevailing social norms and conventions. These occur despite careful planning and the associated identity management and modulation of conduct this entails. We consider such situations critical events that create research ethical dilemmas. Often, we immediately feel that something is not right; in the moment, however, there is little room for pondering. The significance of such critical events might only become apparent

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in hindsight as we continue to think about them, and as we start discussing them with colleagues, questioning our choices, wondering whether we should have acted differently, whether we should have been more critical, spoken up or toned down our reactions. Due to the uncertainty such events create, with potentially direct bearing on researchers' safety and security, and because they are very difficult to prepare for, we consider them to be factors that create volatility in field work.

Here, we focus specifically on instances where we had to reconcile seemingly irreconcilable situations in patriarchal settings in Pakistan. These unplanned and highly complex encounters placed us in situations that pushed the boundaries of our own and our respondents' cultural appropriateness and ideas about ourselves and themselves in society. None of these interactions were outright violent or hostile; nevertheless, they not only caused us discomfort in the moment but have remained on our minds. Agata Lisiak rightly observes that "incidents and encounters in the field can leave a researcher deeply moved, confused, angry, even shattered" (Lisiak 2015: 30). A number of other scholars have also encountered situations that left them feeling uneasy during fieldwork (cf. Gallaher 2009, Kloß 2017, Radsch 2019, Johansson 2015, Trigger et al. 2012). Because of their subtlety, uncomfortable situations are sometimes overlooked in debates about research ethics, even though their reverberations for our data collection and analysis processes and for us personally are significant (Brown 2009, Davies 2010, Johnstone 2019, Tomiak 2019).

These encounters can be subtle and confusing, which makes it difficult to articulate clearly why they make us feel uncomfortable; hence, this is an important topic to consider in debates about research ethics and researchers' vulnerability. In the current neo-liberal and male-dominated academic environment there is little room to reflect on these interactions, especially from a gendered perspective. In conversations and public debates such situations are often trivialised as the personal issue of an individual researcher who "failed" to "endure" what is required for fieldwork (Kloß 2017: 397f.). In this way any further discussion is silenced. This male-centric and neo-liberal research environment has been rightly criticised (cf. Günel et al. 2020, Corbera et al. 2020, Hussain 2020, Nagar 2014, Talwar Oldenburg 1990, Ross, 2015).

This article presents our shared reflections on encounters that created moral and ethical conundrums for us, as women with particular positionalities who conduct research in Pakistan.[1] We explain how we navigated uncomfortable situations in the field and "bargained" within patriarchal constraints, and we offer systematic reflections on these incidents. We foreground patriarchy as a significant and transversal factor in our research and use the notion of respecta-

1 Several markers of identity (e.g. ethnicity, class, skin colour, religion, age) intersect with gender and thus determine how we experience the world; therefore we can only speak from our respective positionalities (see Banerjee / Ghosh 2018, Crenshaw 2017).

bility to explain why we reacted in certain ways. We thus highlight patriarchy, and consequently gender, as significant factors to take into consideration in research ethics, because in patriarchal contexts the establishment of non-hierarchical relationships between researchers and respondents, often seen as a gold standard for ethical research, is impossible (Knowles 2006, Pereira 2019, Sohl 2018).

While our reactions to these particular situations were spontaneous and did not follow a specific strategy, by consciously reflecting on them and comparing notes with others, we were able to learn from each situation, and when we encountered similar situations at later points in time, we were, to some extent, able to modulate our behaviour and, consequently, felt better prepared (Berik 1996). By systematically scrutinising some of these instances, we seek to normalise discussions about incidents that leave us conflicted (Kloß 2017, Vithal 2012) and to move away from male-centric criteria of "good" research in the "field". We consider the discomfort that stems from these unplanned encounters and the subsequent reflections as indicators of ethical challenges and as productive instances of learning that require us to reflect on our privilege, helping us to better understand the context in which we live and work (Fujii 2014, Hoffman 2021, Klutz et al. 2020).

The identification, documentation and reporting of context-specific dilemmas and "what can be done with them" (Vithal 2012: 20) helps to re-code such disruptions and uncomfortable situations as ethical challenges that are part of most research experiences, since patriarchy is a structure that permeates almost all societies. In this way, such situations are no longer anomalies in "exotic" contexts that lower the quality of the collected data. Such locally grounded accounts create room for discussion and contribute to de-centring research methodologies and knowledge production.[2] We have found reflexive and collaborative accounts to be very helpful in bringing out complexity (Cerwonka / Malki 2007, Bröckerhoff / Kipnis 2014). This is why we have adopted a similar approach to compare our experiences.

A number of articles focus on the specific experiences and ethical challenges women researchers might encounter in the Global South[3] and in volatile settings.[4] Only a few authors systematically connect their research experiences to patriarchy.[5] As two political scientists who utilise qualitative research methods and immersion, we are interested in institutions, albeit quite different ones, and we both mainly interacted with people who are "literate, articulate, self-conscious and with the power, resources, and expertise to control information and protect

2 Cf. Denzin et al. 2008, Chilisa 2012, Mignolo 2018, Smith 2012.

3 Cf. Asif 2010, Berik 1996, Godbole 2014, Halai / William 2012, Johnstone 2019, Kloß 2017, Lunn 2014, Pardhan 2012, Radsch 2009, Schwedler 2006, Shamim / Qureshi 2010, Srivastava 2006, Vithal 2012.

4 Cf. contributions in Nordstrom / Robben 1995, Sriram et al. 2009.

5 Cf. Charania 2021, Dossa 2021, Jabeen 2013, Khalid 2014.

their reputation" (Jabeen 2013: 220) and could thus be part of an elite. While our positionalities were quite different in some respects, we both occupied privileged positions – we are both highly educated/academically trained, Abida Bano being raised in a respected/influential family and working at a renowned regional university in Pakistan, and Sarah Holz being a Western, white woman holding a position at a German university. When we discussed our experiences, we found that we shared many remarkably similar concerns due to our gender identity.

Women are not a homogeneous group; class, cultural capital, socio-economic status, ethnicity, religion, age and skin colour create multiple intersectional concerns. Our observations and discussions with colleagues showed that no matter to what extent markers of identity differed, many of our experiences were structured by patriarchy and thereby remarkably similar. Moreover, many of the research ethical dilemmas we came across were quite similar due to prevailing patriarchal norms. By this we mean asymmetrical power relations between women and men where men dominate norm-setting and decision-making which creates gender- and age-based inequalities. These patriarchal norms and structures determine individual and collective conduct significantly. While patriarchy exists in countries around the world, in Pakistan, we find its disciplinary power very intense. In hindsight, while planning our research trips, neither of us reflected much on how our identity as women would impact our practice and what kinds of emotions it would create because we were focused on our professional interests. Patriarchy only emerged gradually as a significant factor in our research through the accumulation of experiences and exchanges with colleagues and friends.

Hence, for us, patriarchal structures cut across class, religion and skin colour and affect every single person's professional and personal lives (Banerjee / Ghosh 2018, Toor 2007). While male scholars also have to adhere to the patriarchal transcript, their experiences and ethical challenges look quite different.[6] We noticed that many male colleagues hardly mention gender in their reflections beyond the pro forma nod to the difficulties that gender segregation creates. In contrast, many female colleagues are very conscious of their gender identity, such as Wajeeha Tahir (forthcoming 2023), who enquired how Pakistani students negotiate their identity, and produced similar findings. While many male students hardly mentioned gender as an important part of their identity because they saw it as a given, among women, gender identity was a topic that emerged much more strongly than initially expected.

We link the need to be constantly mindful of our gendered bodies, and the attached expectations, to volatility. In Pakistan, not only do we consider volatility in terms of political instability and the risk of extremist violence, but we

6 We believe that patriarchy affects women and men alike. However, there are only a few male researchers who engage with this topic (e.g. Khan 2021, Galam 2015, Rahat Shah in Batool et al. 2021).

also see patriarchy as a structural factor that renders a context volatile and thus requires attention in terms of research ethical considerations. First, the power asymmetries that characterise patriarchal settings control our behaviour. Our room for action is constantly shifting, depending on the patronage and favours that are extended to us by dominant groups or individuals. Second, patriarchal norms and expectations are often contradictory because the boundaries between "good" and "respectable" v. "bad" and "disreputable" are ambiguous and fuzzy. This creates uncertainty and volatility and places particularly women and those considered as non-male in vulnerable positions, because it is not always clear when a red line is crossed.

Especially in the past few decades, women's rights have been a fiercely contested issue in Pakistan, as shown by recent debates about the Aurat March (Women's Day Marches) or contestations regarding drafts of domestic violence bills (Dossa 2021, Charania 2021, Tanwir et al. 2019). Women's movements have become increasingly articulate and present in the public sphere, a situation that some sections of society view as a form of Westernisation and moral breakdown. Due to globalisation (including migration, shifts in the labour market and exchanges of ideas), rising levels of education and mobility, the dominant social norms and conventions are contested in Pakistan. This means that boundaries of social conduct are porous and constantly shifting, making it possible to overstep boundaries quickly. The crossing of social boundaries can go either way and, given the heated climate around the status of women in general, could potentially have far-reaching implications, personal as well as professional, for a researcher. It was this uncertainty that surrounds the patriarchy, and the constant re-negotiation of gender norms, that framed our work, rather than political instability and large-scale conflict.

Whether we wanted to or not, we were unable to remove ourselves from the broader circumstances. We wanted to continue to immerse ourselves in this context, which is why it was important to us to appear "respectable" within the patriarchal framework. Performing respectability was not only important for our professional goals but also for our personal wellbeing and ultimately for our safety and security.

The following section provides a brief overview of our research settings and our positionalities. We then explain how we use respectability as a lens to reflect on our experiences. Thereafter, we discuss how we sought to establish respectability, and in the last section we explain how we navigated uncomfortable situations that arose from our desire to "be respectable".

Situating ourselves in the research context

Abida Bano

I used in-depth individual interviews and participant observation as the main tools of data collection to research women's representation in local democracy in a peripheral province of Pakistan, Khyber Pakhtunkhwa (KP). I gathered data from three districts: the urbanised parts of Peshawar district, the suburban Mardan area and the rural Swat district.[7] The three districts are predominantly ethnic Pashtun areas where most people adhere to patriarchal notions of gender seclusion and segregation, and they practise *Pashtunwali*, a codified aspect of Pashtun culture that may fit the definition of informal institutions established by Helmke and Levitsky (2006). Between 2015 and 2016, over a period of around seven months, I often travelled to more than 12 union councils (rural and semi-urban) in the chosen areas to perform in-depth interviews with elected men and women local councillors. I did 63 in-depth interviews and took copious field notes. Each interview lasted 60–90 minutes; however, they sometimes went longer. The interviews took place at the respondents' preferred locations.

Pashtun society is primarily rural in character. Peshawar still exhibits more rural characteristics than urban ones, even though other provincial capitals, such as Karachi and Lahore, are heavily industrialised and modernised. The cultural expectations and norms of rural Pashtun culture dominated the contexts in which I performed my research.

At the field site, I positioned myself as a native, educated and respectable working woman, familiar with the rural and urban milieu of KP. I could be considered respectable because I come from a "good" family. My clan was elevated above others in the Pashtun social structure in the rural setting due to their reputable ancestry and landholding. In urban environments, my family's respectability stemmed from highly educated family members employed in the government and our stable socio-economic standing. My position at the university also enhances my respectability, since teaching is seen as a respectable and desired occupation for women in Pakistani society. Even though I was reared outside my native village, my family and I frequently visited our family members who lived in rural KP, which kept me informed about rural social contexts. In agrarian patrilineal Pashtun society, family lineage and ancestry are still relevant and vital categories for identifying an individual. As opposed to individual identities, people are known and addressed according to their families. Furthermore, those who maintain links with their village and culture

7 The rural-urban divide comes from the government's demarcation of urban and rural areas for the purpose of Local Government elections in KP.

are considered "solid and respectable" in popular rural discourses. My family kept in touch with our ancestral roots, to maintain privilege in the rural social structure. Furthermore, I had a thorough understanding of the language, cultural norms, subtleties of intercultural communication, respect/honour gestures and humour. I was confident of the expectations and norms I had to adhere to while visiting rural areas, since I had strong connections with the society and shared its culture. Hence, I considered myself an "insider".[8]

However, some people thought I was an "outsider" and they were very vocal about it while I was conducting my research. Depending on who was looking, my status as an educated, comparatively independent woman, living in a city, and having a job as a university professor may have made me seem like an "outsider" (Crean 2018, Wolf 1996). Managing the emotional impact of the ambiguity in identification (both "insider" and "outsider"), as well as its implications for my research, was a continual struggle during my fieldwork.

I pursued my PhD studies at a US university. One of the prerequisites for a PhD research project's acceptance is to submit the study plan to the Institutional Review Board (IRB) – HSIRB[9] of the university before formally commencing fieldwork. Obtaining IRB permission is a laborious, protracted procedure that might take months. Researchers have to complete extensive documentation and take an accredited training course (CITI). The board's clearance procedure mandates that the study take measures to guarantee the rights and wellbeing of the respondents, focusing on the safety and protection of participants during the research. The IRB permission letter serves as a guide, advising researchers to rigorously adhere to study guidelines in order to keep themselves and others safe while doing fieldwork. While the procedures in the document are helpful, their applicability in all contexts is not always a given.

I carefully planned my fieldwork approach after getting clearance from the HSIRB. Some of these precautions included dressing appropriately (*shalwar kameez*, *chaddar* – a loose cloth to cover the body from head to knee – and face veil), abstaining from speaking in English or even Urdu and having a male escort the entire time – preferably a relative (Sultana 2007). I always took these safety steps to ensure a successful fieldwork experience and I tried to follow established cultural norms to navigate the complex cultural waters present in patriarchal and conservative settings (Asif 2010).

8 See Crean 2018, Dam / Lunn 2014, Godbole 2014, Owais 2021, Sultana 2007.

9 Human Subjects Institutional Review Boards (HSIRB) are a regular feature in US graduate programmes. Every student wanting to do fieldwork has to go through the arduous and lengthy process to secure approval for her/his project beforehand. The process is about ensuring that studies adhere to procedural standards of research ethics and safeguarding the rights of the research respondents. Apart from the formal approval, attending certified training courses that are part of the Collaborative Institutional Training Initiative (CITI Program) is another requirement.

Sarah Holz

I conducted interviews and observations for my PhD project in exclusively urban settings, mainly in Islamabad, over a period of nine months from March 2013 to September 2016. I wanted to understand the institutional history and sphere of influence of the Council of Islamic Ideology, an advisory body made up of lawyers, judges, *ulama* (religious scholars), scholars and technical experts who advise Pakistan's executive and legislative bodies on the conformity of laws with Islamic principles (Holz 2023).

Federal government agencies, embassies and head offices of international development organisations are located within a small radius in Islamabad, the capital of Pakistan.[10] I moved primarily in this area and in these spaces, where government officials, shopkeepers and other service providers are used to seeing and interacting with foreigners, at least on a superficial level. Hence, the offices and institutions I primarily spent my time in were more heterogeneous than the rural or semi-rural contexts Abida Bano moved in. A woman moving around alone in this part of Islamabad, even though still not that common, was not unheard of.

Almost all of my respondents were men, and the government and educational institutions I visited most often were male dominated. I also interacted with persons in the development and civil society sector, private enterprises and public universities, where more women were present in the workplace. Interviews and interactions mainly took place in offices and official settings; only a few interviews occurred in cafes or private homes.

I am German, and before my PhD studies I had worked in Pakistan and was, to a certain extent, familiar with the political situation and (urban) social customs and had a working knowledge of Urdu. My status as an educated, white, female, non-Muslim foreigner meant that I enjoyed certain privileges that allowed me to sidestep constraints that Pakistani women researchers might face. As a result of colonialism and the donor economy, white foreigners are treated with much respect and greeted with open doors because they are associated with "success, modernity and wealth" (Bonnett / Nayak 2003: 309) and hence higher social class positions. Falcón (2016) calls the benefits and entitlements that come with a Global North nationality "imperial privilege", which is even further enhanced by white skin colour. Like other white women scholars, I definitely enjoyed the advantages associated with this imperial privilege. At the same time, I had to contend with prevailing prejudices against "the West" in general and white Western women in particular (Schwedler 2006, Radsch 2009, Cilliers et al. 2015, Faria / Mollet 2016). For instance, the assumption that I was arrogant, ignorant about the culture and "easy". I believe that foreign

10 This includes the so-called Red Zone, which includes the Diplomatic Enclave where many embassies and donor organisations are located, as well as sectors G-6, F-6, F-7, F-8 and E-7.

researchers of colour and those not read as Christian would have had quite different experiences.[11]

Due to my "young" age and status as an unmarried woman, I was often put in the position of the unknowing student. At times, this was beneficial because it allowed me to ask questions that might have otherwise been dismissed as too trivial. On the other hand, respondents often did not take my questions seriously, or I was quickly interrupted and expected to listen without questioning. My experiences were quite similar to those of Abida Bano and others.[12] Hence, apart from gender, also my skin colour/race and country of origin, which are intricately connected to social class, were important markers of identity that structured my interactions. My ability to speak Urdu and my choice of clothing were central factors in the negotiations of my positionality, a point that I will elaborate later.[13]

For both of us the intersection of gender, class, education, age, language abilities and marital status affected our positionality and power (im)balances during interactions with respondents. We both occupied privileged positions primarily based on our university education and our perceived class identity as "ladies" (*khatun*/*bibi*); in Abida Bano's case due to her family background and education abroad, in Sarah Holz's case due to her education and imperial privilege. Our gender identity and our relatively young age, coupled with being unmarried and PhD students, affected this privilege. Before we elaborate on concrete examples of our experiences, we discuss respectability as our conceptual lens to show that it is central to interactions in patriarchal contexts.

Patriarchy and respectability

Pakistan's society is deeply marked by patriarchy and social stratification. We see patriarchal contexts as characterised by structural asymmetrical power gradients between men and women and between young and old. Deniz Kandiyoti (1988) identifies male domination, the preference for male children and restrictive codes of behaviour as characteristic features of patriarchal societies that intersect with capitalism. Suad Joseph (2000) has observed that patriarchal styles of conduct and values are closely interlaced with cultural and kinship bonds (see also Berik 1996, Joseph 1996).

11 See Bouka 2015, Fujii 2014, Lin 2022. I would also include women who are part of Pakistan's diaspora (Dam / Lunn 2014, Pardhan 2012).

12 Cf. Guerney 1985, Jabeen 2013, Johnstone 2019, Khalid 2014.

13 I limit my discussion to interactions with Pakistani citizens and will not discuss interactions with (white) expats and members of the Pakistani diaspora in Pakistan because the discussion of these experiences would go beyond the scope of this article.

In Pakistan, status and class within the patriarchal system draw on family, marriage, caste (or *biraderi*), religious affiliation and economic situation. The combination of these characteristics determines an individual's freedom to move and act within patriarchal social structures. Additional factors that frame patriarchal social conditions are volatility and uncertainty due to ongoing conflicts, everyday violence, social inequality and widespread debates on Westernisation.

Expectations of appropriate behaviour for men and women and interactions between the genders regulate social interactions. The social pressure to conform is constantly present, even if it varies in scale and degree depending on the context. Women are seen as custodians of honour and respectability (Kandiyoti 1991). Even if men commit transgressions, women are generally held accountable for them or bear the consequences of their male relatives' transgressions (Chaudhary 2014, Naseer 2019, Shah 2016). This is why the respectability of women is under perpetual scrutiny, and women face pressure to be "good" and "respectable" (Ahmad 2010: 5ff). A person's adherence to dominant norms and conventions is often seen as "the external manifestation of his [and her] moral character" where "good character is identified rather closely with conformity to the rules of social propriety" (Nardin 1973: 1).[14] In short, everyone in the society has to embody and perform respectability.

As women political scientists who conduct qualitative research in patriarchal contexts, we had to balance social norms and customs related to patriarchy and respectability with professional goals and the demands we place on our work (Aboulhassan / Brumley 2019). Tension arose because we were crossing established norms of conduct and respectability through our work in the public sphere and our mobility in male-dominated spaces (see also Amirali 2017: 147f). This caused discomfort not only for us but also for the people we interacted with. We were constantly being positioned, judged and discussed . As Stephen Brown (2009) notes, this friendly gossip often arose out of curiosity but was beyond our control and had both positive and negative ramifications. Therefore, we often wondered: Where are we, women researchers, located in this structure? Are we supposed to completely conform and acquiesce to earn respect and ensure our safety? This could entail that we would not be able to do our work as we wish. Or could we simply ignore social boundaries? Was it possible to escape labelling, judging and being watched as women researchers in such contexts? How should we cope with and navigate the clash between our work, our perception of self and social expectations in our research settings? What kind of professional persona did we want to project? Which norms and behaviours would we be ready to push to their limits? Where would we compromise, and

14 While Jane Nardin puts forth this argument to explain the concept of propriety in Jane Austen's novels, the description appears rather fitting in this context.

where would we draw a hard line? In short, how would we bargain with culturally established norms of patriarchy?

Salam Aboulhassan and Krista M. Brumley (2019: 5), referring to Deniz Kandiyoti (1988), described the tension that results from patriarchal bargaining in the following manner: "women submit to specific gender rules that disadvantage them, strategizing to gain social or economic benefits while unknowingly re-creating the system of patriarchy." The difference in our case was that we were acutely aware of our potential participation in the perpetuation of patriarchal structures, which is where much of our discomfort stemmed from. Out of respect for the people we worked with, to enable data collection and maintain our status and respect, we had to acquiesce to social and patriarchal norms and conventions. Nevertheless, the bargaining impacted how we collected and interpreted data (Chong 2008, Davies 2010, Tomiak 2019). Even though we were also creating instances of subversion through our presence and our actions in male-dominated spaces, the negotiation of divergent and often contradictory expectations, and the making of choices accordingly, did not come easy, and many of the ethical conundrums we faced during our work resulted from this tension.

The paradoxical situations in which we often found ourselves are best illustrated through a story that we were both told numerous times by different respondents. We call it "the diamond story", and it appears to exist in different versions and with slight variations across South Asia and the Middle East. The story is used as an analogy to state the importance of women in society and the respect that is due to them while simultaneously justifying their seclusion and, by extension, male control. In the story women are compared to diamonds; they are beautiful and highly valued. Because diamonds are precious (*qimati*), they are kept in velvet boxes to protect them from harm.[15] Similarly, women are the treasure (*khazana*) of society and the family; therefore, they need to be protected. Like the velvet box, the best protection is seclusion in the four walls of the house (*char diwaree*), i.e. the practice of *purdah*.

The men who told us this story in different settings seemed to agree with its message, while we wondered how we should respond. Mostly we just smiled, nodded and changed the subject. However, the implication of the story lingered on and triggered a series of questions in our minds. If this was our respondents' opinion about the place of women in society, what were they thinking of us sitting across from them and exposing ourselves to the public sphere and the male gaze? Were we diamonds outside the box or just rocks in the field? Did they consider us outliers or "other", and were they telling the story not as a commentary on our actions but rather to convey their own respectability? Whichever meaning it held for the respondents, there was no way to enquire and confirm. Whether intended or not, the telling of the story had an effect on

15 In other versions, women are compared to pearls that need the protection of the oyster shell.

our emotional selves and the research process. Its unsaid implications made us feel that we were being disciplined, because the conclusion that came to mind upon hearing the story was that we were not "respectable" in the respondents' eyes.

This seemingly innocent story and other similar offhand remarks cast shadows on the prospects of collecting robust data, our research objective and our commitment to decolonise methodologies, as well as on our safety and security. Perhaps respondents would not talk to us about certain subjects because they did not consider such issues fit for the ears of women. We also ask ourselves if respondents did not trust us because we did not match their classic ideal of a diamond. The most profound effect was that the story provoked us to re-think our position and re-strategise our relationship with respondents to achieve our research objectives and to maintain our wellbeing.

The story is just one example that illustrates how we contended with patriarchal constraints in the form of dominant gendered expectations and norms that collided with our own professional zeal and personal convictions throughout our everyday lives and our research process. The moral of the story is that gender relations and segregation, in particular, are strongly regulated in the private and the public sphere and this had a significant impact on how we worked. Unlike others, we found that even though we could push boundaries at times, as researchers we were neither "asexual" nor "ungendered", nor did we exist outside these patriarchal structures (Kloß 2017: 408).[16] We continued to ask ourselves: "What do our respondents think of us?" More than just perception, this question boiled down to where respondents would position us on the scale of being a "good" and "respectable" woman. These criteria are relational and context dependent. We cared about this question because we were immersed in the context and because it had direct implications for our freedom to move and act and our safety and security, which we discuss in further detail below.

On the one hand, we wanted to be treated with the respect and courtesy that patriarchal norms allocate to women; on the other hand, we wanted to transcend these restrictions and push boundaries. As a result, we were constantly involved in the negotiation of respectability, which is why we find it a useful lens to scrutinise our own actions and reactions. Respectability works differently for men and women and depends on class and context. While all women are expected to act within the limits of female respectability, especially in the public sphere, it is also a class signifier primarily reserved for those women who are considered of privileged background, and hence it applied to us. Due to our positionalities, Sarah Holz as a white foreigner and Abida Bano as a highly educated woman from a "good" family, performing and embodying female respectability became central to our everyday life and work in Pakistan.

16 For instance, Jilian Schwedler (2006) suggests that she was treated like a "third" gender in the Middle East. Many male scholars hardly engage with gendered experiences.

We connect respectability to the notion of *sharafat* (loosely translated as "respectability") and the related concept of *sharam* (loosely translated as "modesty" or "reserve").[17] We understand respectability as a marker of social status and moral distinction based on "honourable descent" but one that can also be achieved through performing and embodying social expectations associated with *sharafat* in order to display a "defined character" (Lelyveld 1996: 30), i.e. being *sharif*. In the context of colonial India at the turn of the 20th century, Shenila Khoja-Moolji observes that discussions about *sharafat* were "connected to concerns about women's mobility […], knowledges deemed appropriate for women […], and engagement in paid work […], which in turn had implications for their status as respectable (*sharif*) subjects" (2018: 23–24). What is considered suitable work and an appropriate career depends on socioeconomic status and class. While low-income and informal work such as domestic labour, tailoring or agricultural work is seen as suitable for women from lower-income households, women from relatively affluent settings pursue teaching and medicine to contribute to the family income. Only in the past decade have white-collar women professionals emerged as a visible group on the labour market. And we were and are part of this group whose position is still under negotiation.[18]

To display respectability, performing and embodying *sharam* is central. *Sharam* can be defined as the ability to control one's emotions and place one's body in socially appropriate settings. Being *besharam*, i.e. without modesty, is what people, especially women, want to avoid. Anna Maria Walter, in her work on (pre)marital relationships in Gilgit-Baltistan, a territory in the north of Pakistan, points out that women embody and perform *sharam* in the form of modesty, self-control and reserve in order to claim agency. This means that by adhering to patriarchal social norms, women can carve out room for manoeuvre (Walter 2022: 39–45, 103). For women, performing not only modesty but also respectability is therefore a tool for patriarchal bargaining (Kandiyoti 1988). Being *sharif* and embodying *sharam* are traits that are conferred by others. This means that to be recognised as *sharif*, a person must conduct oneself according to dominant, yet fuzzy and often ambiguous, social and moral standards that are framed by dominant patriarchal, customary and religious discourses.

To substantiate our experiences and what we identified as important features of respectability, we conducted a very small online survey in August 2021 among 23 men and 14 women who were students at Quaid-i-Azam University or the University of Peshawar, i.e., among highly educated individuals. Most respondents said they were from and lived in Khyber Pakhtunkhwa, 26 stated

17 So far, there is little systematic conceptualisation and theorising with regard to respectability (Hussein 2017, Khoja-Moolji 2018), though there is much on the related concept of honour (Chaudhary 2014, Naseer 2019, Shah 2016). It is beyond the scope of this paper to theorise respectability in detail.

18 Some scholars have already noted the emergence of a (new) middle class in South Asia, which is also relevant to the position of women professionals; cf. Donner 2008, Fernandes / Heller 2006, Maqsood 2017.

that they had spent most of their lives in a town or city, and 11 stated that they had spent most of their lives in villages and rural contexts. We asked, in Urdu, which traits a woman (*khatun*)[19] or man should display to be considered respectable or worthy of respect (*sharmati / sharmata*). The majority of respondents stated that a respectable woman/lady (*sharmati khatun*) should conform to the conventional cultural norms and should know where the boundaries are. The desirable attributes cited most frequently were honesty, respect, loyalty and support for her family (parents, husband or other close male relatives). About two thirds of the respondents seemed to imply that respect meant accepting the decisions of male relatives because respondents also mentioned that a *sharif khatun* should be polite, not quarrel and speak back or "interfere". Five female respondents and two men added that the term *sharif* is vague and its meaning depends on the cultural context.

Seven women noted that being *sharif* is a flawed category and an unattainable ideal and they seemed to imply that *sharafat* is used to discipline women. Most women appeared to be much more aware of the negotiations that are involved in establishing respectability, while many male respondents seemed to view the boundaries of *sharafat* as given. Despite the small sample size, the responses confirm our own experiences and what much of the literature suggests. While the content and expectations of respectability are ambiguous and vague and depend on the individual performing *sharafat* and the person judging, everyone is familiar with the broad contours of respectability, especially its boundaries. The boundaries appear as part of common sense and are therefore hardly questioned, especially by men. The lines between *sharamati/sharmata* and *besharam* are thus porous and depend on the intersection of the social context, class, education and gender of all parties involved in social interactions.

We find that the notions, desires and expectations attached to female respectability shaped how we conducted ourselves and how we made decisions during our fieldwork. For us, as women who already occupied a paradoxical position in Pakistan's social structure, establishing and performing respectability was important not only because it made us feel recognised as humans but also as a safeguard from harm, because respectability also means privilege, which is equal to (access to) power.[20] If someone violates the respectability of

19 We chose the word *khatun*, which translates in English as "lady" and is an indicator of social status, rather than the direct translation of the term woman, *aurat*, which refers more to the biological body. In the Quran the term *aurat* is used to refer to intimate parts of the body. This is why professional women are referred to as "lady judges" or "lady journalists", rather than "woman judges" or "woman journalists". Since we wanted to ask about respectability, *khatun* seemed more fitting. Salam Aboulhassan and Krista M. Brumley (2019: 10), studying how Arab American women understand and construct their gendered position and behaviour among the Arab diaspora in the United States, note: "Women in the Arab world were not typically referred to as a *mara* (woman), but a *sit* (lady), emphasizing her social standing, not her biological makeup. To refer to a woman as mara stripped her of her social standing, calling attention on to her female organs and suggesting someone worthy of little or no respect." These findings are also relevant in the Pakistani context.

20 We concur with studies that have shown that violence is not only gendered but also classed; see Phipps 2009.

a privileged person, the likelihood that there will be repercussions for the perpetrator is high.[21] Hence, embodying respectability can act as a deterrent from harm, especially from those in less or equally privileged positions. In the next section we discuss ethical challenges that arose from establishing and performing respectability. In the section thereafter we discuss the ethical dilemmas that emerged from negotiating respectability in instances where established gender relations were transgressed, by accident or intention.

Establishing respectability in our research contexts

When we enter a new setting, we make conscious and unconscious decisions on how to present ourselves. Based on what we know, we might modulate our behaviour and self-presentation, e.g. appearance, language, choice of words or content of discussions. However, we have only partial control over our self-representation because respondents and those we interact with also position us within their social matrix. How we present ourselves and how we are perceived by others impacts the quality of data collection and analysis.[22] In this section, we describe strategies we adopted to establish ourselves as respectable in order to build rapport with respondents. We took some of these decisions before we started data collection; others we made due to immediate experiences. Being respectable was not only important to us but also a collective duty. Our respectability also reflected on the respondents' respectability and standing in society. Being respectable is thus intricately connected to not doing harm. Hence, whether we wanted to or not, we inadvertently had to weigh our appearance and our conduct against the dominant criteria of women's respectability.

It is important to note that notions of respectability vary, because Pakistan is a multi-cultural and heterogeneous society. Thus, the particularities of performing respectability can look different, depending on the setting and a person's markers of identity. Nonetheless, some overarching traits remain the same across the country.

Abida Bano

In the Pashtun social structure, male and female spaces are distinct, and both genders are expected to act within their domains. *Purdah* – the segregation of women – is practised widely (Chiovenda 2015). The relatively urban society in Peshawar is slightly more used to women's presence on the streets, yet women

21 This is why in many discussions related to (honour) killings and sexualised and domestic violence, perpetrators make an effort to portray the women who are victims of such crimes as dishonourable (e.g. licentious, wearing revealing clothing, being out alone in public, etc.). Because the women, allegedly, violated dominant patriarchal norms, perpetrators seek to show that violence was justified.

22 See for example Asif 2010, Guerney 1985, Lisiak 2015, Talwar Oldenburg 1990, Zubair et al. 2012.

still have to abide by the approved cultural protocols. Social expectations for respectable women are explicit, such as wearing an appropriate dress, covering their heads in public and wearing long *chaddars*, among others. A respectable woman is not supposed to be out in public in the late evenings or late at night without family. I followed these expectations. Hence, my research visits took place between 9 am and 4 pm and I made sure to be home before dusk. Even though I was always accompanied by a male family member, it wasn't proper for me to converse with strangers in public, even in the relatively urban parts of Peshawar. These circumstances were the same in Mardan and Swat, but in those areas, *purdah* and the presence of a male companion were more important. In terms of how women researchers are treated, I discovered a few differences between the suburban areas of Peshawar and rural Mardan and Swat.

The importance of the family to a person's social identity in rural Pashtun society cannot be overstated, especially for women. They are expected to be "obedient" and "loyal" to their families (Jamal 2016). Due to the patrilineal nature of Pashtun families, everyone in the household is identified by the male head. Women don't have identities of their own; instead, they are recognised by their relation to their closest male relatives. My belonging to a respectable family was evident to many because of how I carried myself. Still, some people insisted on asking me personal questions regarding my family or workplace. The questions served as a means of establishing my origin and confirming my social identity and respectability. To satisfy my research participants, I mostly divulged details about my family, clan, place of residence and occupation. I was not always comfortable sharing that information, but it was essential for building rapport, mutual trust and transparency.

In rural areas, most Pashtuns adhere to *Pashtunwali*, a normative code to control interpersonal and group relations. The foundation of *Pashtunwali* is *nang/namoos* (honour). Women are seen as the "honour of the family"; thus, they must be careful not to compromise it (Naseer 2019). This was one of the reasons that respondents inquired about my immediate male kin. Many female respondents said to my face that I look like I come from a respectable family. They would make it clear that, despite my high level of education and residence in the city, I was expected to adhere to the criteria of being an honourable lady by dressing modestly, covering myself and refraining from mixing with strangers. One female respondent stated: "You are someone's honour, and it is good that you are carrying yourself to protect it."[23] These implicit and explicit statements influenced my behaviour and research, posing ethical dilemmas.

I meticulously chose my dress code for all visitations regardless of the rural-urban divide. Women are judged by their appearance,[24] including dress, so as

23 District Mardan, Pakistan, 2016

24 People would express their opinion on women's dress and manners very openly. Many expressed to me that I looked to be from some "good" family. "Good" referred to many things to my understanding, including economic status, mannerisms and in some instances "authentic" Pashtun lineage.

expected, I wore a *chaddar* and covered my face in all public spaces and interactions, also when interviewing male research participants. This was a specific measure to distinguish me as a "respectable" woman. I was aware that rural residents would find it disrespectful and unsettling if I didn't wear a veil, which would impact my research collaborations.

Despite my education, career, age and position in society, I had to be accompanied by a male family member, even if he was a teenager or child because it is seen as the responsibility of men to protect women (Khalid 2014). When walking in public with my male chaperone, I lowered my gaze and timidly and quietly walked behind, not beside my male guardian, on the margins of the streets and alleys to conform and to demonstrate respectability. As expected in rural areas, I kept a certain distance from him and did not converse with him or anyone else while walking. I had known and practised these conventions all my life and did so throughout my field trips. But this required negotiating with my male guardians what I would do during the day.

Another strategy for maintaining a respectable image was to speak only at a minimum with male respondents and stay away from the male family members of female respondents. This meant it was impossible to generate a rich discussion when speaking to male respondents. Since I was conducting in-depth interviews with a semi-structured interview guide, I would pose a general query but would be unable to go deeper, establish connections or ask further questions because doing so would be considered a "forthcoming" gesture, which is discouraged for women in particular. Additionally, there were occasions when men, particularly male family members of female respondents, spoke over me, intervened and asked me questions that weren't necessary. I did not respond, however, and I kept my word count minimal because exhibiting a greater willingness to speak with unrelated men would immediately have placed me in the *besharam* (having no respect) category.

In some instances, when I was interviewing female councillors, male relatives occasionally chimed in from outside the room to provide their opinions, casting doubt on the views of the female respondents by "demonstrating" their superior knowledge. I would listen intently to what the men had to say, just like the female respondents did, even if it had no bearing on my research. As long as I was in their home as a visitor, the family hierarchy applied to me as well (Kloß 2017). Remaining silent was a show of respect for the patriarchal heads of the houses, which I had to do because I was already doing something out of the ordinary by daring to interview women (Moghdam 1993). As a counter strategy, I waited for the male family members of my female respondents to exit the room or leave the house before I would continue my interview, since I had to remain silent due to their intrusion. This had profound logistic and emotional effects on me and my research. I lost time and energy and it increased the expense of my visits since I could not meet the day's goal.

My willingness to comply with cultural standards also had emotional and affective repercussions. I bargained my autonomy and agency for data quality. Similar practices have been recounted by other women researchers (cf. Godbole 2014). In patriarchal societies, social expectations do not allow women to make independent choices; instead, they act according to the script in order to fit in (Kandiyoti 1988). I tried to establish respectability meticulously in order to minimise disruptions and ensure quality data collection. Moreover, I had to live and work in the same context after my field work, so being a member of that society added to the pressure to maintain my appearance and be seen as a respectable woman; I could not afford to be taken otherwise. Hence, my fieldwork was difficult logistically, but it was also distressing emotionally and affectively, and at times downright unpleasant.

Sarah Holz

Most of my fieldwork took place in the urban context of Islamabad. As I explained earlier, gaining access to offices and institutions, securing meetings and conducting interviews was not exceedingly difficult for me because as a white foreigner I enjoyed imperial privilege and as a PhD student I did not seem too threatening. Another reason for the welcoming behaviour in offices appeared to be a certain type of curiosity: "Why would anyone, especially a foreigner, be interested in this topic?", "Why is she here and what are we going to do with her?" were often the unspoken questions in the room (see also Amirali 2017: 147f.). When I arrived, I often overheard whispers that the German *khatun* or *bibi* (lady) had arrived. My every move was observed, hence none of my visits were confidential. I was not able to resolve this ethical dilemma. At least I was not working with vulnerable groups and I did not discuss any topics that might put respondents into danger. I did anonymise and treat my data with confidentiality when presenting and publishing my work, but for people working in these institutions during the time I was there, it was probably quite obvious whose views I was referring to.

It was clear that my presence interrupted the everyday workings in these male-dominated environments. Despite their curiosity, many respondents and their supporting staff were unsure how to treat me, at least at first. I want to note here that the support staff and colleagues were as much important actors in these situations as the respondents themselves, which should not be forgotten. The prevailing social norms determined our relations, hence they and I had to find ways to bridge discomfort and awkwardness. Our interactions often contained an element of "patriarchal performance" that was enacted primarily for the observing eyes of the other persons in the office – from the person bringing the tea to the personal secretaries who sat in the anterooms, to the colleagues and guests who often dropped in for business or a chat. As much as I was con-

cerned about my respectability, they were concerned about theirs. Association with a foreigner might enhance the status of many respondents in their work setting (Schwedler 2006), but my gender identity could also raise awkward questions. Therefore, doors were mostly kept open to signal that social distancing was observed. This meant that conversations were frequently interrupted because other people dropped in. Even though these disruptions were not conducive for conversation flows and impacted data quality, I did not attempt to ask for more privacy because I was concerned that closing doors or suggesting that we meet in another place was not compatible with respectability criteria. Like Abida Bano, I made sure to only meet during office hours, never in the evening.

When women walk in public in Pakistan, they mostly do so in pairs or groups and they do not stroll aimlessly but seek to convey a sense of purpose. I used the same strategy to put respondents and their support staff at ease. All meetings had a specific purpose, and I had concrete questions to ask. Informal interaction, a method that many anthropologists use to gain familiarity with the context, was impossible because it might have suggested a level of contact beyond a professional relationship. The formality took away from rapport building and shaped the variety and depth of information that respondents shared with me.

Respondents also did their part to normalise my presence in these male-dominated spaces. One strategy was to place me in their kinship network. I was often addressed as a daughter or sister; for this placement, my age, as well as my status as an unmarried woman, was an essential facilitating factor (Berik 1996, Joseph 1996). Assigning me a place in their kindship network signalled that they felt responsible for me, not as a friend but as a relative. It conveyed a sense of support and protection. At the same time, I also became entangled in the patriarchal norms that structure these kinship networks, which could also mean that some male respondents felt that they were in a superior position, somewhat eroding my imperial privilege (Kloß 2017, Sharp / Kremer 2006). Consequently, a certain type of patriarchal benevolence was detectable in some of these reassurances. This allowed some respondents to treat me like they might treat young female family members: I was talked over, or my questions were not taken seriously and used instead as a means to "educate me" (Radsch 2009, Khalid 2014). This prevented more detailed discussions and was only resolved if I decided to ask detailed or difficult questions that startled my counterparts because they had not expected me to have a deeper level of understanding. However, such questions could be perceived as "talking back", which is not seen as a good trait for women. I thus had to weigh carefully whether to use this tool or not.

While my presence at the offices was accepted for a little while, the body language of some office members made it clear that they were glad to see me leave. My prolonged presence disrupted their routines; for instance, they had to explain to any person who entered the office who I was and why I was there.

When I had questions or requests, for instance to copy a file, they were performed immediately and prioritised, out of respect and deference, but I suspect also to shorten my stay. Navigating these social pressures hindered engagement and prevented me from attaining deeper levels of rapport. The established patriarchal social norms were therefore successfully enforced. My experiences stand in contrast to the fieldwork accounts of other foreign female researchers who work in patriarchal – often framed as Muslim-majority – contexts and who argue that research in such a setting is not much of a problem for women (Schwedler 2006, Radsch 2009). I find that these accounts do not focus on how gender relations and our gendered bodies affect the knowledge production process. While I concur that female researchers are treated with respect and that foreign women can conduct research in such contexts, these interactions still occur in tightly regimented patriarchal settings, a fact that impacts every aspect of research and also affects our personal lives.

My ability to understand Urdu and my choice of traditional Pakistani dress emerged as additional factors to establish respectability, because many persons saw both as signs of respect for Pakistani culture. At some point during most encounters, I would receive comments on the way I dressed. Perhaps somewhat naively, I had not expected that my choice of clothing would be a point of discussion, especially for men, or that it would impact my research. When I had worked in Pakistan before I started my PhD, I had lived with a Pakistani family. All the women of the family wore *shalwar kameez* (a long shirt and matching long pants) and *dupatta* wrapped around their shoulders (a large matching shawl); hence, I had adopted this style too.[25] While many foreign women wear *shalwar kameez*, few opt to wear the matching *dupatta* and even fewer wear it wrapped around their shoulders, unless in winter. To me, wearing *shalwar kameez* and *dupatta* provided a way to blend in, at least to a certain degree. It gave me a feeling of safety because it felt like a shield against the constant male gaze. Many respondents appreciated my willingness to adopt the "Pakistani style". Meant as praise, such comments, especially when uttered by men, nonetheless sometimes made me feel uncomfortable, because they drew attention to the constant male gaze on my body. This made me very conscious of my every move, my gender identity and the fact that I was often the only woman in an office.

In more international settings or when interacting with Pakistani who might consider themselves more "liberal",[26] the fact that I was wearing a *dupatta* around my shoulders caused a range of reactions, from jokes to furrowed brows and rather

25 Respectability and class status are also expressed by the colour, pattern, quality of cloth and cut of the *shalwar kameez*. Like most well-situated urban women, I bought my shalwar kameez in the high street shops and thus also conveyed a certain social status.

26 For lack of a better word, I am using the term liberal in quotation marks and cautiously because such labels can be easily misunderstood.

condescending remarks.[27] Some people quipped that it was funny to see a foreigner wear "traditional" clothes while Pakistani women had started to wear "Western" clothes. Others gave me reproachful looks and indirectly conveyed that I was feeding into patriarchal structures and providing fodder for conservatives, who could use my example to tell Pakistani women that they should continue veiling. It was impossible to withdraw from this contested global debate related to (Muslim) women, veiling, "empowerment" and agency.[28] Thus, my choice of clothing became a research ethics dilemma: Was I going a step too far, supporting patriarchal power structures and "misrepresenting" "Western" values, or was I merely respecting local conditions? For me, the choice boiled down to my level of comfort and the need to feel safe and respected. Other scholars have reported similar decision-making procedures. My choice of clothing was also a "covert subversion of the male-dominated world", both in Pakistan and in Euro-America (Talwar Oldenburg 1990: 261), because I consciously chose to block the male gaze (ibid.: 273) rather than feel uneasy in an attempt to "represent" so-called "Western" values via my body and my choice of clothing. While many situations were beyond my control, the way I dressed was the one thing I could determine (Lisiak 2015). Moreover, all choice of clothing in all circumstances is conditioned by external circumstances. I could use my choice to blend in and to complicate black-and-white thinking because I did not fit in with prevalent expectations on any side.

As the accounts show, both of us tried to perform respectability through identity or impression management: we modulated our conduct, mobility and choice of clothing to fit general patriarchal expectations. We were aware that these choices are not only personal but embedded in larger debates around Westernisation and "clashes of civilisations" and it was impossible for us to withdraw from these discourses. We made choices that were framed by dominant conditions but that were also informed by our comfort and our wish to appear respectable. Simply by being in public places, moving around and asking questions, we were already subverting patriarchal structures to a certain degree.

Navigating respectability in the case of transgressions

While we took some measures to establish respectability, we were often confronted with situations where respondents, intentionally or unintentionally, transgressed patriarchal codes of conduct and social norms. We had to react

27 It is beyond the scope of this article to discuss the reactions of foreigners in Pakistan (the so-called expat community) or Pakistani diaspora in Pakistan. These reactions are framed by Western-centric conceptions of empowerment, freedom and agency.

28 See Fluri 2011, Lisiak 2015, Mahmood 2005, Mohanty 1987, Schwedler 2006, Zubair et al. 2012.

immediately but felt uneasy. This feeling stayed with us and these incidents prompted us to reconsider our actions and reactions and what we were willing to "endure" to collect data (Johnstone 2019, Kloß 2017). Hence, we had to navigate respectability. This is why we consider these instances critical events that constitute ethical challenges. They were productive because they forced us to reflect on our research practices, and they allowed us deep "accidental" insights into the context in which we were working. Such "'accidental' moments in the field", as Lee Ann Fujii suggests, are generally considered "non-data" but are significant to understanding the broader social and research context (Fujii 2014: 2).

Abida Bano

Since I shared the culture, language and context with my research participants, I first believed I would not need assistance navigating challenging situations. I had pre-planned some of my fieldwork, including safety precautions such as adhering to gender and cultural conventions. To my amazement, however, I encountered many unforeseen circumstances that emotionally taxed me and I did not have the tools to deal with them. Deflection and acquiescence were my go-to strategy to get around tricky circumstances in the field. This was an instantaneous and reflexive reaction rather than a calculated action.

For instance, several of my female respondents or their family members would enquire about my marital status and, if I were, who my husband was. If not, why was I not married? They asked about my monthly pay, where my family is and what they do. Sometimes, I gave honest responses to these inquiries. Other times, I tried to change the topic or to give the bare minimum of details. I didn't want to reveal to respondents that I was unmarried since in rural regions it was unusual for my age and might characterise me as "autonomous". Such attributes would place me in an unreliable and suspicious group and question my respectability. Although it was vital to protect myself and my family, I felt burdened when I failed to address respondents' questions fully. I believed I owed them for voluntarily agreeing to participate in my study. Marcel Mauss (1954) stated long ago that a researcher is indebted to anybody who has ever assisted them in their research, whether by granting access to the field or by providing data or a life narrative (see also Johansson 2015). This debt-based connection assumes a notion of reciprocity that obligates a researcher to make some form of repayment, and it presents ethical dilemmas to inexperienced fieldworkers. I frequently struggled with this issue and persuaded myself that speaking less and sharing fewer details was necessary in order to preserve my respectability.

Another situation when I felt helpless was when male respondents asked to conduct their interviews in spaces that are generally reserved for men. As men-

tioned earlier, I chose the local council offices as the preferred interview location since all my respondents were locally elected council members. Still, I let respondents choose the interview location. Many male councillors chose their *hujrah* as the location for the interview. The *hujrah* is an annexe to a private home where unrelated male guests and visitors are lodged and entertained. In the Pashtun social system, *hujrah* refers to the complete male social sphere where men interact with other men in the community, visitors and bystanders. Women, whether related or unrelated, do not enter the *hujrah*. As explained earlier, I carefully conducted my research without excessively transgressing the bounds of dominant norms and culture. Even though I would have never chosen *hujrah* as an interview location, I went along with it.

Due to my upbringing and socialisation, I was well aware of the status of women in society and knew where they should go and stay. Even with my face covered, I felt uncomfortable entering a *hujrah*. It was the first time in my adult life that I had sat in a *hujrah* and spent time there. It did not feel right. I felt disrespected and uncomfortable when I had to conduct interviews in the *hujrah* since it is not how respectable women are treated in Pashtun culture. By being there, I was overstepping boundaries, which would raise eyebrows. Given their awareness that I came from a similar culture and background, I wondered why male interviewees picked the *hujrah* for the interview. Did they see me as an outsider, unfit to enter their homes?

I felt I was viewed as an outsider because of various identification markers, such as being an educated, relatively independent working woman, which others have also observed in postcolonial contexts (Crean 2018, Godbole 2014, Miriyoga 2019). Was I not respectable enough to be seated in someplace other than *hujrah*, which was like a traffic junction all the time? Men would stop by, sit down, drink tea and then depart. I felt subjected to the unavoidable male gaze more than in other public settings. Whenever there would be disruptions due to guests' arrival at the *hujrah*, I would lower my head, stop the interview, and slide to the corner to feel invisible. Even with my long *chaddar* and veil, as well as my bent head to prevent eye contact – choices I made to block the male gaze (see Talwar Oldenburg 1990) – if I had had the choice, I would have preferred not to be there.

The societal rule is that women should not speak much, get too comfortable or make requests of unrelated men. Since I believed that speaking up or asking to change the location would damage my reputation and my rapport with my respondent and the onlookers, I instead remained silent and shrank a little more. Thus, despite feeling uncomfortable with the circumstances and conflicted about my decisions, I carried on with my interview, even if this meant entering a *hujrah*. All female respondents, in contrast, opted to have their interviews conducted at their homes, which provided me with some peace of mind and privacy to do in-depth interviews.

Interactions with respondents and bystanders were impacted by various forms of patriarchal control. These disruptions and constraints had ethical as well as practical ramifications. As in Sarah Holz's case, none of the interviews with male respondents in the *hujrah* were confidential because of the visitors, another ethical dilemma that I could not mitigate because the respondents had chosen the venue. Moreover, interviews with the male respondents were not as rich due to limited contact, the presence of male visitors and my discomfort in the *hujrah*.

My privacy and identity were also in jeopardy. Being asked personal questions and conducting interviews in the *hujrah* put ideas of female respectability into question. Many male respondents seemed to treat me like any other (male) visitor and I wasn't seen as respectable enough to be hosted at their homes. Did they "overlook" my gender? Or perhaps, since I was out in public, did they assume it was okay for me to occupy male social places? Or was the invitation to the *hujrah* a form of disciplining me for pushing boundaries? Whatever the reason, they did not show me the courtesy of asking whether I was comfortable sitting in the *hujrah* or if I would rather sit in a private area (perhaps in their home). Men typically move out of the way and allow women to enter a house when they knock on someone's door, even if the men don't know the woman. However, the typical treatment for a woman researcher like me defied social expectations and was unique. I realised that mostly only those women whose male family members are known to the community's residents receive respectability. Though I was an insider with certain advantages, being a woman and a researcher placed me at the margins of society, having less autonomy and agency (Johnstone 2019).

Reflecting on these experiences, I see my field research experience as a continuous struggle between various positionalities – respectable woman, native researcher, foreign-qualified and university professor. To facilitate my access to study participants, I occasionally forwent certain advantages granted to women, disregarded slights, made fun of them or simply put up with them (Mwangi 2019). I managed the fieldwork dynamics rather well, but the emotional pain from the concessions I had to make has lingered longer than I had anticipated. These unexpected occurrences provide additional data for comprehending the social environment of the field context (Fujii 2014). I became aware of the disparate and contradictory treatment of women in society at various levels, which not only irritated me but also led me to doubt the expectations and concepts of a woman's respectability in my culture. To sum up, respectability is an exclusive practice reserved for women who support dominant patriarchal ideologies and seem to "know their place".

Sarah Holz

Like Abida Bano, there were a few instances during my field research in Pakistan that made me feel uneasy. A seemingly minor situation that I encountered frequently, especially during social gatherings, was that some men greeted me by extending their hand for a handshake. Men and women who are unrelated commonly do not shake hands in Pakistan, which is generally read as a sign of familiarity. In such a situation, I had to evaluate the circumstances instantly: Should I ignore social customs and shake hands even though it made me feel uncomfortable? Should I refuse the handshake and risk dismaying and potentially embarrassing the other person? How was I to interpret the offer to shake hands? It could be a sign of respect for "Western" customs; it could be a way for respondents to demonstrate their "liberalism, tolerance and open-mindedness". Did respondents think that I would not mind, or even not notice this breach of gender relations? Hence, was the offer to shake hands a deliberate transgression and a way to assert their superiority (Johnstone 2019, Kloß 2017)?

In some instances, I shook hands out of reflex and courtesy because I was too surprised and blindsided. On the one hand, I felt disrespected because I was treated differently than Pakistani women. On the other hand, I benefitted from my status as a foreigner because I was afforded much more privilege than Pakistani women and I felt I should therefore not complain about seemingly minor transgressions. On other occasions, I took a step back to increase the physical distance to the person offering to shake my hand and I touched my heart with the right hand in greeting. This refusal also caused discomfort because I felt like I was being rude and had publicly slighted my counterpart.

These instances also raised questions about future interactions: Was the offer to shake hands a gesture of good will or an "innocent" overstepping, or could it be the start of complicated interactions because the respondent might interpret my acquiescence as a silent approval and a possible invitation to transgress gender norms in the future ? Or was I being too sceptical, seeing problems where there were none? These considerations significantly affected how I conducted myself because I was constantly monitoring myself.

Often, I felt that being a white woman, as compared to a white male researcher, was advantageous because I was seen as less threatening (Schwedler 2006, Radsch 2009).[29] Male respondents did not seem to feel the need to compete for power and "manliness" with me. However, my imperial privilege also seemed to provoke some men to use their "maleness to redress the power imbalance" (Johnstone 2019: 87) that my skin colour, education and nationality created, by transgressing gender norms and imposing their will on me. Hence, I concur with Lyn Johnstone, who observed from her research interactions with

29 This impression is based on observations from international conferences and workshops in Pakistan where white, male, foreign researchers were present, as well as from conversations where respondents talked about their experiences with male, foreign researchers.

male politicians in Rwanda and Zimbabwe that "while I might have had more power as a white researcher […], it is reasonable to infer that as a woman I had less" (Johnstone 2019: 87).

In both of our cases, these uncomfortable encounters might seem small and inconsequential but they carried meaning. They affected how we interacted with people, who we spoke to and how we moved around. These incidents also touched us on a personal level. It could be argued that these are occupational hazards that we have to deal with. The advice would therefore be to distance the personal self from the professional self, but this ignores the deeply personal discomfort that these situations elicit. From the existing literature to our experiences and what we heard from other scholars, it is clear that such unexpected and confusing instances occur frequently. It is necessary to open up space for discussion and to examine how patriarchal structures impact research in Pakistan and elsewhere. We do not want to remain silent because this would mean complicity with patriarchy (Eltahawy 2016). While we might not be able to change the structural inequalities as such, by simply having a discussion and practising the ethics of care, we hope to take a first step toward preparing other researchers and alleviating their worries.

The negotiation of respectability was central to our interactions, and writing about these norms might help others devise coping strategies. Respectability is frequently employed as an exclusionary measure by members of dominant groups to discipline those who do not neatly fit in established categories. Through our presence as women researchers in patriarchal contexts we were doing boundary work that can provoke different reactions. The consequences of this boundary work and the potential implications for our safety, security and wellbeing are marked by uncertainty and volatility, which rendered both of us, and any person who does not neatly fit in patriarchal hierarchies, vulnerable.

Conclusion

Our experiences of conducting qualitative political science research as women researchers underscores the anticipated and unanticipated research ethics challenges that researchers face in patriarchal and volatile contexts. In order to help others better prepare for uncomfortable situations, we highlight the significance of telling the stories of women researchers' struggles in the field. The experiences of male researchers in patriarchal settings also require greater attention.

Most of the literature emphasises the researcher's mindfulness of the research participants' ethics and cultural protocols in ideal settings, but fewer studies talk about the vulnerabilities of researchers in fieldwork[30] beyond more extreme

30 See Chong 2008, Jabeen 2013, Mićanović et al. 2019, Sharabi 2020, Sohl 2018.

situations such as experiences of violence, crisis and hostility. The costs of emotional labour have already been acknowledged in other fields of research, for instance in connection to sensitive topics or conflict research (Schulz et al. 2022); discussions on patriarchal contexts can learn from the existing literature and contribute new perspectives.

We want to draw attention to the prevalence of seemingly insignificant events that create unease and ethical dilemmas. These incidents are more subtle and confusing than outright harassment and violence. From the perspective of dominant notions of patriarchal respectability, our mobility (in the form of travel but also the fact of our presence in male-dominated spaces), our inquisitiveness and curiosity during interviews and conversations, as well as our status as professional researchers and women who were working outside of the home, elicited most of the ethical dilemmas we encountered. Hence, we were constantly doing some form of boundary work, which was draining. In this article, we included reflections by many scholars who were in similar ethical dilemmas to demonstrate the pervasiveness of such negotiations. These references can also serve as a reading list for those who might find themselves in similar situations and who are looking for guidance.

As various feminist scholars have pointed out, in a patriarchal society, gender relations are consequential; we and our respondents were hyper-aware of our gendered bodies. As researchers, we were not sure where we fit in the patriarchal hierarchy. The theoretical choice seemed straightforward; the lived reality was much more nuanced and complex. If we wanted to be treated like respectable women this would mean that specific spaces and topics would be closed to us. If we wanted to be treated as professional researchers, where gender was of secondary importance, this would mean that we would encounter more situations that were uncomfortable. Both of us had to decide how far we were ready to compromise or push the boundaries of gender relations to advance our research, and where we had to draw the line.

Then, there was our general desire to support the work of women researchers on a larger scale. Through our work, we might advance change in gender relations, for instance, by normalising the presence of women in spaces dominated by men, such as the *hujrah*. However, boundary work comes at the expense of our comfort and self-respect. Moreover, by acquiescing and transgressing, we might inadvertently contribute to stereotypes of women professionals, Western-educated Pakistani women or white foreign women. For our protection and safety, and to ensure that we could return to our research setting and interact with people again, we tried to negotiate expectations of respectability through dress code, body language and behaviour. We made these decisions consciously and unconsciously. In some circumstances, we had time to deliberate; in others, we had to act quickly.

What we learned is that we do not have to adhere to rigid structures blindly; instead we can identify pressure points to push, more or less safely. Such assessments require immersion and careful observation, being conscious of existing privileges and cleavages and paying attention to existing and emerging public debates. Some of these points might be easily overlooked by others but they are significant to us. It is crucial to uphold the ethics of care, establish limits and place an emphasis on well-being rather than continual endurance, since both our personal and professional welfare are important.[31]

Any issue related to gender is contentious in Pakistan and elicits strong reactions. Whether we want to or not, we are part of these debates and have to situate our choices in these volatile circumstances. For now, we reconcile with our role as slightly suspect and provocative "lady researchers" because we know that our mere presence disrupts highly gendered environments.

31 See Corbera et al. 2020, Günel et al. 2020, Kloß 2017, Lombardi 2022, Mianovi 2019.

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Between a Rock and a Hard Place: Ethics of Fieldwork in Northwest Pakistan

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Abstract

In the wake of the US-led and Pakistan-allied "war on terror", residents in Northwest Pakistan have faced inconceivable structural and physical violence, in ways that pose ethical challenges in ethnographic writing and research. Over the last few decades, militancy, banditry and overall insecurity have hampered relief efforts in the area and significantly weakened basic infrastructure. In this article, the author illustrates how an initial security plan to undertake fieldwork research in this "volatile" region proved somewhat irrelevant because of her positionality, gender and race/ethnicity. The author explores the implications of these dynamics in contexts characterized by unequal gender relations and strict gender segregation. In addition, undertaking empirical work in the context of epistemological frameworks in a region that has been subjected to active conflict, militarised operations and a singular representation in the global and local media, poses other ethical challenges for anthropologists searching for new areas of study and decolonised models of representation. This paper reiterates the importance of a reflexive approach of ethics that acknowledges the interpenetration of race, gender and the thick web of relationships in the production of knowledge and is, at the same time, respectful of cultural specificity.

Keywords: Pakistan, anthropology, fieldwork, research ethics, positionality, gender, race, volatility

Introduction

Ethnographic fieldwork is an empirical form of study that is central to the discipline of anthropology (but not unique to it), one in which long-term cross-cultural encounters are captured and (re)narrativised by scholars to produce knowledge about a place, community or phenomenon. The way in which research findings are understood, written and evaluated in the social sciences is in a constant dialogue with ethical concerns that lie at the intersection of reflexivity, positionality and the history and representation of a research site.

This paper discusses some of the challenges of both conducting and writing about fieldwork in northwest Pakistan by considering two key interrelated themes – positionality and representation – and how they relate to ethical methods.

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First, I begin with a brief overview of anthropological fieldwork and knowledge production steeped in the North-South binary and reflect on the privileges and challenges of my positionality as an East Asian woman researcher in Northwest Pakistan. I discuss the implications of segregated space, what it means to be called a *khor* ("sister"), and how being misidentified as an ethnic minority from a neighbouring country, complicates the already imbalanced process of knowledge production.

Second, I discuss the problems of writing about a place such as Peshawar, which has been severely afflicted, on the one hand, by the burden of its history as a frontier city, and on the other, by various inequalities and the serial violence that the "War on Terror" has wrought. These tend to inform the way the region is represented in media and scholarship as well as how many experience life in Peshawar. What are the ethics of writing about a region that has been, for several decades, an epicentre of extreme and sporadic violence resulting indirectly from Cold War politics, the US-led global "War on Terror", and the rise of various factions of the Taliban and militant groups? The main challenge of conducting ethical research in such a society is to provide a genuine social critique while considering the systematic and often involuntary restructuring of lives under the shadow of contemporary geopolitics.

I conclude the article by positing that, beyond considerations of the structural difficulties of an environment categorised as "volatile" by and for foreign researchers and institutions, we must foreground the challenges of different forms of volatility experienced by the very communities that cannot, unlike the researcher, drop in and out of insecure situations.

Power and ethics in anthropological fieldwork research – An overview

The core methodology and practice of anthropology relies on prolonged fieldwork, often in a country other than one's own, to produce and narrativise experienced realities in the form of ethnography. The personal nature of fieldwork thus requires thoughtful analyses that are based on a constant dialogue with one's own position and perception vis-à-vis the community being "studied". Since its foundation as a discipline in the Euro-American West, or what Michel Trouillot (2003) called the "North Atlantic", anthropology, along with many other disciplines in the social sciences, has been introduced, framed, theorised and dominated by scholars and researchers from the Global North. Such an epistemology and dynamic are the result of the discipline's emergence out of European colonial expansion (Lewis 1973). Fieldwork sites were thus by definition situated "outside of the West" (Hayano 1979, Mughal 2015), in places

that to white Euro-American eyes were inhabited by people from "remote" and "exotic" cultures.

The postcolonial era led to a number of developments within the discipline from which the current moment of reflexivity has emerged.[1] A closer examination of the role of colonialism in the communities of study has also meant interrogating the relationship between colonialism and anthropology, and a serious consideration of the ways "in which the discipline was formed, in service of twentieth century colonial rule and [continues to] play a part in the heritage of modern politics, development and welfare" (Pels 1997: 177). While anthropology is not inherently colonial, it certainly thrived in the colonial period, amid imperial wars, and continued robustly into the Cold War era (Shimizu 1999: 2). Such a fraught trajectory led to the emergence of starkly uneven power relations between researchers from the "West" and the Rest (Wolf 1982). Up until today, these power structures continue to make the object of anthropological study "accessible and safe" for Northern researchers (Asad 1973: 17). Yet from these accounts, one also detects a formation of the notion of safety and security (for the researcher, not the object of study), as the Western scholar has embraced, fetishised and confronted what it means to live in places of instability and conflict, however fleeting.

Many scholars working on various parts of the Global South have provided in-depth overviews of the discipline's evolution.[2] Their academic interventions have contributed to a critique of methodology and ethical conduct, as well as pushing scholars in new conceptual directions. One important intervention is the debunking of the widespread assumption that "objectivity" is only obtainable by anthropologists engaged in research among people whose cultural background is different from their own. Such an idea rests on the premise that fieldworkers can essentially adopt a detached impersonal approach in their research that renders their work real or true – therefore "objective". Postmodernists have long asserted the problems of the claim to a "universal" or "authentic" position (Ashworth 1995), complementing exponents of postcolonial theory. Exemplary (and seminal), of course, is *Orientalism* (1978), in which Edward Said demonstrated how such a "reality" can indeed be constructed using a plethora of supposedly univocal facts about another culture.

Two important developments emerged within the discipline as well as "in the field". First, the anthropological gaze began to turn to "domestic affairs", and second, the field began to witness the rise of non-white, "native" anthropologists (Munthali 2001, Narayan 1993). Anthropology's associations with European colonialism also did not go unnoticed within the very societies that were being studied. Some of the governments of the countries previously studied by Western anthropologists responded by implementing restrictions on foreign researchers

1 Cf. Talal 1973, Lewis 1973, Harrison 1992, Smith 1999, Rappaport 2016.

2 See for example Lewis 1973, Abu-Lughod 1988, Shimizu 1999, Trouillot 2003, Gupta / Stoolman 2022.

for a variety of reasons (ibid.). In the case of post-revolutionary Iran, for example, Shahrashoub Razavi (1992: 152–163) has noted how only Iranian nationals were permitted by the state to do research in villages.[3] Postcolonial states in Africa that became engulfed in civil wars and political instability caused by the expansion of extractive industries were deemed too "volatile" for the presence of Euro-American researchers (Munthali 2001). Here is where the methodological challenge of doing fieldwork in the "Third World" comes in, where difference and volatility both attract and repel researchers from the North.

In what follows, I investigate not so much the evolution of anthropology, but instead how reflexivity as an ethical practice, complemented by postcolonial and postmodern theory, pushes current researchers to think about their engagement and perceptions (both of others and how others perceive them), especially in contexts that are frequently labelled "volatile". Rather than understanding ethics as the rights and wrongs of anthropological researchers, I consider ethics as "essentially a theory of social relations and a fundamental part of human interaction", following Lynn Meskell in *Embedding Ethics* (2005: 126).

Peshawar under the shadow of the War on Terror

In my forthcoming monograph that builds on my dissertation, "Precious Economies: Gems and Value Making in the Pakistan-Afghanistan Borderlands," I examine the coloured stone[4] trade in and from a region flanked by the mineral-rich Himalayan-Hindu Kush mountains. As part of an ethnographic study of how minerals are rendered into precious commodities, I conducted field research primarily among gem dealers, cutters, miners and gemmologists in a market in Peshawar, Pakistan. The project investigated the ways in which precious stones emerge through a sequence of transformations initiated by different actors who each employ processes of value-addition. From 2017 to 2019, I was based in Peshawar, the capital of Pakistan's Khyber Pakhtunkhwa (KPK) province, and conducted fieldwork in multiple mining sites in both northwest Pakistan and the bordering areas of Afghanistan, tracing the supply chain of precious stones to Thailand, an international gem trading hub. At the time of research, the city of Peshawar was home to the most important mineral and gemstone trading and processing centre in the region – a mineral and gem wholesale market known as Namak Mandi (lit. "wholesale market for salt"). The city acquired its position as a centre of the gem trade in the region largely due to its geographic location and the geopolitical context that saw the arrival in the 1980s of Afghan refugees, who brought their connections to the gem producing mines.

3 Other places included measures throughout the twentieth century that made it difficult for Western researchers to conduct fieldwork. For instance, the establishment of the People's Republic of China (PRC) in 1949 led to the subsequent temporary closure of its borders to international researchers (Dos Santos 2006).

4 "Coloured stone" is the market designation for all gemstones or precious stones excluding diamonds.

For various reasons stemming from the domestic politics of Pakistan as well as external factors – most notably its proximity to Afghanistan – Peshawar is, and has been for several decades, a site that is considered too volatile for Western researchers. The official websites of the governments of the United Kingdom and the United States – among others – have both issued an advisory against travel in the provinces of KPK and Balochistan, which includes also the former Federally Administered Tribal Areas (FATA).[5] The US Department of State, for instance, states in their updated travel advisory of 2021 that these two provinces should be avoided, as "terrorists have targeted US diplomats and diplomatic facilities in the past" and that "attacks continue to happen across Pakistan, with most occurring in Balochistan and KPK, including the former FATA".[6] Added to these precautions was the requirement of a "No Objection Certificate" (NOC), a document to be obtained by visitors from the Ministry of Interior of Pakistan for "prohibited areas in the province", a regulation that had been in place since 2012. In March 2016, however, the NOC was waived for certain areas within the KPK province (The News 2016). In 2019, in an attempt to boost tourism, the government decided to end the NOC requirement for foreigners who want to visit different parts of Pakistan, thus enabling foreign tourists to move freely in the country (Dawn 2015).

At the time of my first entry into the field, I was cautioned by both locals and foreigners alike about the insecurity that plagues the region and told to shift my research to cities where foreign researchers have typically done fieldwork, such as Lahore, Karachi and Islamabad. Not only were such cities "more manageable" in terms of security, but they also boasted a larger network of institutions and better infrastructure. Perhaps most importantly, at least for my safety-minded advisors, they were also home to large expat communities and therefore could better cater to the needs of a foreigner. Yet the mineral-rich province of Khyber Pakhtunkhwa and the market in Peshawar constituted the heart of the gem trade of my research and thus a central site of ethnographic data collection. I was caught between a rock and a hard place: Was I to abandon the idea of residing in Peshawar entirely and pursue research in a more familiar place with a pre-existing network, even if less relevant to my research, or was I to venture into the unknown against the advice of my interlocutors and friends?

Before I move on to describing my methodology in the field and how various factors related to my (perceived or self-ascribed) identity compounded my interactions, it is necessary to take a brief historical detour to understand how and

5 The former FATA comprised seven agencies: Mohmand, Khyber, Kurram, North Waziristan, South Waziristan, Bajaur, and Orakzai Agencies. Since the 25th amendment to the Constitution in 2019, both FATA and the Provincially Administered Tribal Areas (PATA) were merged into the province of Khyber-Pakhtunkhwa (KPK).

6 See Pakistan Travel Advisory, US Department of State, https://travel.state.gov/content/travel/en/traveladvisories/traveladvisories/pakistan-travel-advisory.html (accessed 14 June 2021).

why Peshawar became "volatile" in the eyes of not only foreigners but also non-Peshawari Pakistanis. To be concise, I restrict my historical attention to the Soviet invasion of Afghanistan that began in late December 1979, which not only triggered a large exodus of Afghans into the region and beyond, but also planted the seeds of violence in the name of religion, rendering it fertile territory for the emergence of varying factions of the Taliban.

War and conflict shape the social and infrastructural contours of any society. This has certainly been the case for Peshawar, a former frontier city of the British empire with a long and fraught history of conflict. The Soviet invasion of Afghanistan introduced a coalition of powers from within and outside of the region and generated tremendous insecurity for all. Pakistan's border with Afghanistan in the Federally Administered Tribal Areas (FATA) is 1360 kilometres long, artificially dividing communities bound by cultural, kinship and religious ties. From the beginning of the Soviet invasion, the resistance forces in Afghanistan – the Afghan Mujahideen – were supported politically, financially and militarily by the United States. Moral, diplomatic and logistical support also came from neighbouring Pakistan (Abbas 2014: 57–61).

FATA became the site through which the CIA and the Pakistani military establishment collaborated to provide arms and equipment to the Mujahideen. The role of FATA as a conduit indirectly rendered the closest and most populated city of Peshawar a "den of spies and jihadis" (Reuters 2009). These events and triangulations of powers are a major point of departure for understanding how certain circumstances remain crucial and why Peshawar and neighbouring regions continue to be plagued by the violence of geopolitics. The post-Cold War era was marked by the withdrawal of the Soviets from Afghanistan in 1989, but peace did not return. Instead, the country spun into a spiral of anarchy and civil war between multiple factions of the Mujahideen. One faction rose in prominence from the utter chaos in the mid-1990s: the Taliban.

From the twenty-first century onwards, this region has been most glaringly altered by US military action in Afghanistan and Iraq. Although the global "War on Terror" (WoT) has a genealogy that predates 9/11, its beginning as a systematic enterprise can be dated to the attack on the World Trade Center in New York as well as the Pentagon in Washington DC, and to the events leading up to and following the US attack on Afghanistan in late 2001. The bombing of Taliban strategic positions in Afghanistan on 7 October 2001 as part of "Operation Enduring Freedom" unleashed a new wave of insecurity in the region. As the US-led invasion of Afghanistan drove fleeing militants across the border, the Taliban and Al Qaeda regrouped and were aided by Pakistani militants (Nazir 2010: 65). Peshawar became an important "transitional and transnational base" for the Taliban as early as the 1980s, when the American CIA and Pakistani army "armed, trained, and prepared soldiers to fight the occupation of Afghanistan by the Soviet Union" (Massoumi 2011: 210). The

historian Mejgan Massoumi (2011) has shown how the Taliban's return to Peshawar after 2001 fundamentally shaped the urban landscape and public life through performances and practices of dominance and oppression. The emergence and rise of the Pakistani Taliban (Tehreek-e-Taliban Pakistan) and its ideological partners, in combination with the porous border between Afghanistan and Pakistan via FATA, rendered the peripheral region of Pakistan's Khyber Pakhtunkhwa as one of the "frontlines" of the WoT (Qadir / Alasuutari 2013). Within a decade, Peshawar went from being a den of spies to being dubbed the "epicenter of jihad" (Reuters 2009).

For the last two decades, Peshawar, the former FATA and several Afghan cities have constituted the heart of the urban WoT, where residents witness its ideological excesses and experience its violence. One of the worst and most reported attacks was the Peshawar school massacre on 16 December 2014, when seven heavily armed Taliban fighters stormed an army-run primary and secondary school and killed 150 people, of whom at least 134 were pupils. By contrast, acts of extreme violence that occur outside of urban centres, including in the peripheries of former FATA, feature less prominently in the national and international mediascape. Over the last few years, Peshawar has witnessed substantially fewer large-scale acts of extreme violence, even as smaller acts continue (Hayet / Akbar 2019; Sirajuddin 2020a, 2020b; AFP 2019). And while these acts of extreme and sporadic violence often intersect with religious affiliation, gender and class, it is mostly the "terrorist attack" that catches the media's attention. What the media often obscures is the general insecurity felt by religious and ethnic minorities as well as the gendered nature of the violence, complexities that long preceded this current moment of "terror".

Knowledge of the history and social contours of a "volatile" place obliges the researcher to adopt an appropriate field methodology. Upon being awarded a fieldwork grant in 2019, I was asked to provide a "Safety Statement" that showed how I planned to ensure my own security, as well as that of others. In the statement, I wrote that while Peshawar had borne the brunt of much violence in the last two decades, the city had become less insecure. I showed how I was cognisant of the kinds of attacks that happened and received regular safety updates provided by interlocutors in the region. I referred to annual reports by the Pakistan Institute for Peace Studies (PIPS) that have shown that Peshawar is a "more permissible operating environment now" as compared to the past several years (PIPS 2017). According to the Overseas Security Advisory Council,[7] published statistics have also shown that explosions and significant acts of terrorism no longer "occur daily". I emphasised that by avoiding risky areas within the city – this meant avoiding army and police installations, as

7 The Overseas Security Advisory Council is a Federal Advisory Committee with a US Government Charter to promote security cooperation between US-American business and private sector interests and the US Department of State.

well as large political and religious gatherings – I would be less likely to expose myself to unpredictable acts of violence. As my research on the trade of precious stones was neither politically nor religiously sensitive, nor were my sites of investigation located in risky areas, I did not envision encountering any significant acts of violence.

Prior to the commencement of my long-term fieldwork, I relied on a vast body of literature on fieldwork methodology that discusses the challenges of doing research in "volatile" environments in the Global South. While I had access to a body of anthropological research that highlighted the difficulties faced by researchers conducting fieldwork in more volatile places, the anthropological "canon" was often written by white male Euro-American anthropologists conducting research in Africa, Central Asia and South America, and on topics directly related to (structural or physical) violence in these regions.[8] Defined at its outset as "[more of] a problem", these places thus become reservoirs of empirical data by scholars of the Global North, but those that require the concerted efforts of locals (Nhemachena et al. 2016: 19). The result is a plethora of stories told through Northern eyes.[9]

In the last few decades, many more qualitative researchers from Pakistan, many of them women and non-anthropologists, have produced important scholarship using ethnographic methods in both rural and urban areas.[10] In their work, they bring up issues they have faced as local women working in diverse environments and with a range of interlocutors, such as among farmers in Lahore, women in high pastures in the Hushe Valley (Gilgit Baltistan), or middle-class residents in Karachi. Despite being Pakistani or even of the same ethnicity as the communities of their research, Pakistani scholars have noted the challenges of doing research in their own country, which can be chiefly summarised as a symbolic rift between them and their interlocutors that is both social and geographic.

For Asha Amirali, whose field site is based in the *mandi* ("wholesale market" or "marketplace"), the difficulties she experienced came with the visibility of her gender and class as a "Pakistani woman unlike any they [the interlocutors]

8 See for example Gluckman 1954, Goffman 1989, Geertz 1995, Spradley / McCurdy 2012, Stoller 1997, Seale 2004. For scholarly work related to my region of interest, see, among others, Centlivres / Centlivres-Demont 1988, Barth 1956, Lindholm 1982, Tapper / Tapper 1982, Edwards 1998, Grima 2005, Marsden 2005, Sökefeld 2014. I limit my non-exhaustive references to recommended readings within my discipline. For a more grounded critique of North-South relations and "whiteness" in anthropological work, see Ntarangwi 2010.

9 Following the argument advanced by Jean and John Comaroff, places subsumed under "the developing world" for Western researchers became a natural site for "data" as a result of the Western enlightenment, which posited itself as the "wellspring of universal learning, of science and philosophy" (Comaroff / Comaroff 2012: 1). These other worlds, according to them, are perceived less as "sources of refined knowledge than as reservoirs of raw fact: of the historical, natural, and ethnographic minutiae from which Euromodernity might fashion its testable theories and transcendent truths, its axioms and certitudes, its premises, postulates, and principles. Just as it has capitalized on non-Western 'raw materials' – materials at once human and physical, moral and medical, mineral and man-made, cultural and agricultural – by ostensibly adding value and refinement to them" (ibid.).

10 Cf. Ahmad 2009, Ahmad 2019, Ahmed 2006, Ali 2019, Amirali 2017, Asif 2020, Azhar-Hewitt 1999, Faiz 2022, Saif 2014.

knew" (Amirali 2017: 148). In the field, she described being a novel creature that "intrigued, puzzled, and confounded" men in the marketplace (Amirali 2019: 148). Despite possessing similar "skin colour, faith, or citizenship" that can render Pakistani fieldworkers as "insiders", the affiliation to a foreign university (typically based in the US or UK) can spark both interest and suspicion (Sattar 2016: 32), especially in areas where Western intervention has left its indelible mark.

More specific to the region of my research – the province of Khyber Pakhtunkhwa – Akbar Ahmed (1976, 1980) and Fredrik Barth (1956) pioneered anthropological work on Pashtun men, as did Benedict Grima (1993, 2005) and Amineh Ahmed (2006) for Pashtun women; these studies described the limitations of conducting research in gender-segregated societies. While these works are ethnographically rich, it was nonetheless hard to locate within them my own positionality and fieldwork difficulties as an East Asian woman trained in the Euro-American academy pursuing fieldwork in a region characterised by particular gender and ethnic dynamics. For many, writing and thinking about fieldwork remains a discussion that is kept in the methods section of a dissertation, and far less foregrounded in the conventional work that is published. Fieldwork experience is unique to each researcher and the challenges experienced depend upon diverse factors. How local conditions shape ideas about one's culture and history, and the marked social and moral schisms between the researcher and the "research subject", are important exercises in reflexive ethnography. Anthropological knowledge is, after all, produced through interaction and collaboration between people, rather than through the "detached" and supposedly "neutral" practices of the researcher alone.

While Peshawar and the gem mining areas in Khyber Pakhtunkhwa are not "volatile" in the ways some of the aforementioned scholarship characterises "warzones", they remain places of insecurity and precarity for many. Although the volatility in Khyber Pakhtunkhwa is of course felt by local actors, my own positionality as an East Asian woman confounded many of them. This raised questions about the particularity of my experience, and how my racial and ethnic identity was affecting and shaping my experiences in the field.

"Why aren't you married?": Gendered identity in the field[11]

When I began the project on the gem trade, I had not intended to focus entirely on the social lives of men – but they happened to dominate the trade that I studied and hence my field site. As a woman, I had by default more access to women's lives, yet much of my participant-observation took place in the market, a male-dominated public space. Therefore, even if gender is not at the core of

11 Parts of this section are drawn from the introduction of my dissertation (Lin 2021).

my study of a precious commodity chain, it is critical to discussions of field practices and ethics because of how it filters and shapes my social relations and interactions, and thus the information I collect. It is widely assumed that Pakistan is a "patriarchal" society in which men, as fathers, brothers and husbands, wield or are expected to wield authority over the household.[12] Since men tend to have economic control over their households, including over their wives and children, they also exercise social control over their family. These dynamics run true transnationally. In Khyber Pakhtunkhwa, the province of my research, Pashtuns are the dominant ethnic group, and social norms regulating gender roles and segregation are even stricter.

Writing about fieldwork and one's gendered positionality in the region of Khyber Pakhtunkhwa carries the burden of mitigating certain longstanding Western and local tropes about Islamic or Pashtun society's patriarchal qualities while acknowledging a distinctive set of conventions between religion and Pashtun identity, moral choice and compulsion, and the undeniable presence of patriarchal authority within society at large. Ethnographers of women's worlds in Muslim-majority countries – often in the Middle East – have long sought to complicate understandings of gender beyond the simplistic registers of patriarchy in a corrective effort to restore women's voices and agency.[13] In my ethnographic writing, I struggle to displace an essentialist framing of Pakistan and Pashtun society as uniquely "patriarchal" and women as "voiceless", yet the material I gathered and places I frequented for research were almost exclusively male spaces. However, to view Pakistani or Pashtun society solely through the lens of patriarchy would be reductive – to orchestrate a series of equivalences associated with Islam or Pashtunness in past scholarship and in contemporary foreign media representations. The salience of patriarchy as both social and spatial emerges from my fieldwork practices and shapes the ethnographic focus of my research and my role as a researcher.

As a woman and a foreign researcher, I faced multiple challenges that ranged from finding a residence in the city of field research to navigating social spaces. In terms of finding a place to stay in Peshawar, the biggest challenge proved to be my gender and age. The search ended up taking close to a month and involved every last network I had in the city. In the context of Peshawar, it was simply unheard of – some would go the extent of saying "possibly dangerous" – for an unmarried and foreign female to reside on her own, especially if she had not come as part of a foreign mission. In some cases, I was outright denied apartment rentals, while in others I was asked to provide marital papers that attested to my married status, which I did not possess. These are problems that

12 The systematic domination of men over women exists in different forms in societies across the world. While notions of differentiated patriarchy permeate Northern spaces as well, it is too often defined and captured as only a "Southern problem" (Mohanty 1984).

13 See for example Abu-Lughod 1986, Altorki 1986, Atiya 1982, S. Davis 1983, Dwyer 1978, Fernea 1985, Wikan 1991.

are not uniquely faced by foreign researchers but shared by single Pakistani men and women alike. A few potential landlords, mistaking me as a "Chinese national", appeared enthusiastic to rent their place to me. They told me they welcomed "the Chinese" and gestured to the flourishing bilateral relationships between China and Pakistan. Their attitude did not change when they realised that I held a Taiwanese passport – the privilege remained. It was my single status that proved to be the definitive problem. Eventually, I circumvented the problem of housing when a gracious family, business partners of a close friend in Hong Kong, decided to host me in their own home. Thus began my fieldwork proper in Peshawar. The presence of a family reduced my vulnerability as a single woman, while allowing me to conform to local gender norms.

Drawing on my experiences in different parts of Pakistan, especially in areas frequented by the middle- or upper-middle class, I knew that women can be highly visible in public.[14] Yet with my field site situated in the marketplace – either in the *mandi* or bazaar – where men form the overwhelming majority, I recognise that my fieldwork in Pakistan can be said to have been spent mostly in a "man's world". My Pashto language teacher, research assistants and the gem dealers and cutters in Namak Mandi were all men. In societies characterised by a high degree of gender segregation, factors such as one's gender, class and foreign-ness could also imply relatively privileged access in the marketplace and mobility around the city. In some ways, I was better placed than a male researcher, as I had access not only to the women's domain but also to the men's, especially in residential neighbourhoods where strict *purdah*[15] was practised.

Rather than my gender, the major obstacle to movement around the city in general was urban infrastructure. The lack of proper public transport impacts many lives, particularly those of middle- or lower-class women, who rely on male members of their family for all transportation. I was often told that public transport was not advisable for foreigners in this area – another instance where my "outsider" status and class was highlighted in interactions. Fortunately, the time of my fieldwork coincided with the arrival of the Careem app, a transportation network and web-based private taxi service, which enables its users to travel from one place to another using a simple mobile phone application. Careem is often touted as the most reliable mode of transport in Pakistan because it has vetted its drivers; both the company and the user can track the ride and driver through the application. Careem thus became my preferred mode of transport.

14 The visibility of women and gender segregation in urban centres varies widely from one neighbourhood to the next. In rural areas, gender segregation is more pronounced and female visibility far less.

15 The word *purdah* is polysemous here as it can refer to a curtain or a veil of two distinctive types, literal as well as metaphorical, the latter enshrined within the religious and social practice of gender segregation.

Even so, I travelled primarily during the day and only on very rare occasions ventured out in the evening – and always in company. It was during these long rides to and from my field site, once it was established that I was not a local/Afghan woman, that questions brazenly moved from my religious beliefs to who I lived with and my marital status – questions that other Pakistani women ethnographers have also faced in their fieldwork experiences (Amirali 2017: 148, see also Holz / Bano in this special issue). When I responded that I was single, the question that often followed was: "Why aren't you married?" These moments reminded me that interactions during field research are never one-dimensional and are often filled with complex dynamics, shaped by (at times) uncomfortable and awkward social interactions. Questions of such a nature, depending on the identities and personal histories of the interlocutor, can elicit emotions of discomfort and even offence, as they did for me the first time. In that moment, my feelings of vulnerability arose less from my interlocutors' directness than it did from the uncomfortable realisation of the inconsistencies between, on the one hand, my own geographical background and societal norms, and, on the other, the interpretations and associations generated by my interlocutors.

As an unmarried woman, I was also often assumed to be much younger than my thirty-something age. This often led my interlocutors to treat me like a "girl" (*jinay*) and assume that I needed a companion (chaperone) on any outdoor excursions. My host family cautiously warned me against taking public transport or rickshaws, and advised me on sartorial choices and standards adapted to context-specific environments, all of which I welcomed as a way to ensure not only my respectability but also safety. I considered these acts an ethical commitment on the part of my interlocutors to help me operate within respectable and kin-based networks of mobility and sociality, even if the practices of my ethnographic enquiry in the marketplace (or in the mines) were emphatically unconventional to my middle-class host family.

In the Pashtun-majority city of Peshawar, where I spent the bulk of my time doing field research, gender roles and boundaries frame social interaction. Patriarchal norms shape the everyday, as notions of seclusion and segregation, family systems, *chaddar* ("veiling") and *chardiwari* ("four walls of the house") organise social relations at large. Undergirding these practices is the larger religious concept of *purdah*, which represents a regulatory system for both men and women, but specifically determines and restricts the movement of the latter to the domestic sphere, the "house" (*kor*), from where the word *kor-wala* ("women of the house" / wife) is derived.[16] A plethora of local idioms articulate

16 This practice is not unique to Khyber Pakhtunkhwa and intersects with class. Anthropologists have observed that elite women in the Middle East and South Asia generally keep strictly to *purdah* as a sign of privilege and status among Muslim communities (Mernissi 1985: 142, Hoodfar 1997, Papanek 1973, Abu-Lughod 1988).

such ideas of purdah and rendered my behaviour of regularly leaving the *kor* a "conceptual anomaly" (Kondo 1990: 11). Almost all of the female household members of my interlocutors in Peshawar observe these customs. To speak and interact with women would necessarily require my entering into their homes, after spending the day in the market. These moments were instances when my gender offered a clear advantage and gained me a vista – however small – into the lives of lower-, lower-middle- and rural-class women with roots in the former FATA and Afghanistan, who are less likely to engage in forms of sociality outside of their homes.

To avoid drawing unnecessary attention in the public space, I accordingly donned *chaddars* in white and beige, favoured by local Pashtun women, whenever I stepped outside the house. Whether I was in a taxi or walking in the market, I learnt to balance one part of my *chaddar* over my nose, a skill I had acquired from my friends – albeit with little success at the beginning. The strategy of drawing less overt attention to my foreignness worked to a limited extent. I was less noticeable when seated in the back of a car in moving traffic, yet my attempts at "blending", through my sartorial practice, into the crowd in the market spaces where I conducted fieldwork did little to conceal the gait and the fidgeting of a clumsy outsider from the gazes of the more observant passers-by. As the days and weeks wore on, I grew less bothered by the curious gaze of men and walked with more ease as marketplace regulars also grew more accustomed to my presence.

Still, my gendered identity mattered most in Namak Mandi, the gemstone market where I conducted research. Although my main research contact, Amaan,[17] did not accompany me on my first visit to Namak Mandi, his many brothers and nephews made sure that I was never alone when I was in the market. My initial encounters with the mineral trading and cutting community were shaped by my role as Amaan's "adoptive" kin, for I was always seen in the company of male members of the extended family and stayed in their home in 2017. My first few visits to the market yielded few contacts willing to speak to me. I was somewhat restricted to the shop of Amaan's elder brother and the company of Imtiaz, a jovial man in his early fifties who ran errands for the family firm. When Amaan first related my research project to Imtiaz, the latter made a point of introducing me to all the "big players" of the market. His enthusiastic disposition and chattiness made him the perfect fieldwork associate. Imtiaz knew practically everyone in Namak Mandi, but he chose specific men and families who were involved in the trade of the variety of minerals he specialised in. When introducing me to other men in the market, Imtiaz and Amaan's siblings would address me as their "sister" (*khor*), a boundary-marking label that set a precedent and positioned me in clear relation to them as well as others.

17 In accordance with anthropological convention, I have used pseudonyms in the text.

During the initial weeks of my fieldwork, my sense of the market and of my relations with the gem-dealing community was more or less shaped by this role, and while it gained me access and protection as a guest of a well-known business family, it also made it difficult to establish new relationships and collect interviews with those outside their immediate circle. This was not, of course, because of any explicit restriction imposed by my hosts. Although rarely expressed, it was understood that as kin, as someone who lived within their home, and as a woman, I was to be especially careful and refrain from interacting with unknown men. My interactions were confined to only those who were introduced to me, and whose "honour" could be vouchsafed by my hosts, as any other circumstances would place me, their guest, in potentially dangerous or inappropriate situations in which I might be offended, and thus implicate my host and their reputation. Since the 2000s, anthropologists have increasingly written about sexual(ised)[18] harassment and assault during fieldwork, its implications and the need to raise awareness of the difficulties and dangers for early-career fieldworkers (Green et al. 1993, Sharp / Kremer 2006, Pollard 2009). Questions of conduct, as much as the place itself, can be sources of insecurity. This highlights the complex relationship between personal safety and researchers' ethical obligations towards their interlocutors and kin in the field. I had to constantly negotiate the complexity of these relationships.

While fieldworkers document and analyse a social phenomenon in what might seem like a bounded site and community of research, the reality is that the ethnographic data is in fact more bounded by the fieldworker's own positionality as well as that of her gatekeepers. My fieldwork experience and data collection are entangled with these interpersonal and structural relations, which ultimately shape the construction of anthropological knowledge and reveal it as both a collaborative and sometimes contentious process.

Being identified as someone's "kin", even if in a performative sense, can become an important point of entry into the field as well as a marker of status and safety for a female ethnographer in a male-dominant space. In *Veiled Sentiments*, Lila Abu-Lughod (1988: 11), describes how her father insisted on accompanying her to her field site in Egypt, an offer she accepted rather reluctantly and with some embarrassment – it was simply not a convention that the "fathers of anthropologists accompany them to the field to make their initial contacts". She later understood that his act of coming with her was an indispensable "field strategy" that served a very useful purpose: offering one way to identify her, thus securing her own security as someone's kin and not just a young, unmarried woman "traveling alone on uncertain business".

18 Following Kloß (2017: 400), I use "sexual(ised) harassment" instead of sexual harassment to highlight that the basis of this behaviour lies not in sexual attraction but "modes of reinforcing (patriarchal) power". Harassment may differ across cultural norms but here it refers to both verbal and physical actions that violate prevailing local sexual norms.

Many people with whom I discuss my research (including local and foreign social scientists working on diverse issues in Pakistan) were shocked that I, a lone East Asian woman, was pursuing fieldwork in Khyber Pakhtunkhwa. For two decades, the region and its capital were considered to be largely off limits to foreigners as a result of their widespread reputation for acts of terrorism and the kidnapping of Westerners. Furthermore, the conventional image of the fieldworker in Pashtun / Pukhtun society has been the archetypical white male[19] whose status as an independent and autonomous researcher is widely praised. It is their identity as researchers and outsiders endowed with a certain degree of status – both from past colonial authority and contemporary racial politics – that promises them access to these places. For women to be legible within such a social milieu, they need to be granted the status of "honorary male" or "third gender" – a questionable category of its own. But even this is complicated by dynamics of race, ethnicity and class, subjects I address in the next section.

The marketplace, as I later discovered during fieldwork, was teeming with conspiracy theories.[20] Given the specific geopolitical history of Peshawar and the region more broadly, a foreigner who could not be placed in an easy social category – Western tourists and NGO workers were a rare sight during the time of my field research – would often draw unwitting suspicion from locals. Such was the case for the occasional Western journalist I met who came to the mandi to conduct interviews. However, as an East Asian woman and house guest of one of the gem traders, I was rarely assumed to be working for the "intelligence agency" or the Pakistani ISI (Inter-Services Intelligence). Ethnically Chinese, I was legible in the gem market first as a potential customer or businessperson – mainland Chinese are increasingly emerging as the dominant consumers of luxury goods such as gemstones – and then as a potential language assistant when dealers of gems discovered that I could converse in both Pashto and Chinese. Despite my gendered identity, I was also able to define my own role with a high degree of autonomy and forge friendships with men in the market, relationships that would have been impossible between local men and women.

Misrecognitions: race and ethnicity

There is now ample literature on how a researcher's identity might affect ethnographic methods and their outcomes (cf. Bouka 2015, Clark / Cavatorta 2018, Naz 2012, Yacob-Haliso 2018). While anthropology continues to take as its primary object of study communities and phenomena situated outside Europe

19 See Barth 1956, Lindholm 1982, Edwards 1990, Bartlotti 2000, Watkins 2003.

20 Conspiracy theories about Western intervention are rife in the region, especially since the widely known vaccination ruse orchestrated by the CIA in 2010 in the wake of the killing of Osama bin Laden, which continues to inhibit polio eradication efforts in northwest Pakistan today.

and the United States – although exceptions are increasing – the field is now increasingly also populated by scholars of colour, many of whom choose to study their own communities. This demographic expansion reflects a ubiquitous shift in the relationship between ethnographer and interlocutor. My identity as a Taiwanese raised in multicultural contexts is no longer unique within my field. However, my profile as one of the few East Asians to specialise in a geographic region ironically not too far from my own remains an anomaly in South Asian studies, and more specifically, in Pakistan studies. Many non-native ethnographers of Pakistan tend to be white, Euro-American and male.[21] There are undeniable privileges that come with being identified as a white or Euro-American (male) individual conducting fieldwork in certain parts of the world. In the case of Pakistan, a former British colony, the frequent tendency to accord space and respect to foreigners generally (but more specifically white Euro-Americans) is sometimes coined as the "*gora* complex"[22] and was recently most prominently exhibited in the unprecedented entitlement and attention given to foreign travel bloggers like Eva Zu Beck, Cynthia Richie and Rosie Gabrielle (Shackle 2020).

While the access and connections acquired by way of one's positionality and social standing may indeed enhance academic or entrepreneurial projects, considerations of race and power dynamics are peripheral to much of the literature on Pakistan by foreign researchers. Cultural ideas, structures and processes such as colonialism,[23] neoliberalism and the global order interact to affect a researcher's experiences in the field, and there is certainly more accounting that needs to be done on how one's race and gender, as perceived by the communities being studied, alter the field experience and the collection of "data". Access in the field – and by extension the production of knowledge – continues to be shaped by gendered, racialised and nation-based positionalities.

According to Kai Thaler, variables such as gender, race, politics and ethnicity "can be barriers to access, depending on the setting, research topic, or interlocutors" (Thaler 2021: 23). "Native" fieldworkers, or at least those seen as native, navigate an entirely different field that offers them advantages as well as disadvantages. Their closer connections to the communities they research, language skills and context-specific knowledge help them assuage the concerns of exposing local matters to an "outsider" of a community or sometimes nation/continent. Pakistani anthropologist Muhammad Aurang Zeb Mughal recounts, for instance, how his decision to work in Punjab lies primarily in his proficiency in several regional languages. He further explains how his father's personal con-

21 See for example Verkaaik 2004, Marsden 2005, Ring 2006, Gayer 2014, Hull 2012, Walter 2016. The only other exception to this general pattern is the ethnomusicologist Huang Pei-ling (2020), who, interestingly, is also Taiwanese and has worked extensively in the Sindh province of Pakistan.

22 The Urdu and Hindi word *gora* refers to a light-skinned person or group of people typically originating from "the West".

23 Pakistan is a relatively young country, gaining its political independence only in 1947 after the departure of the British and their colonial rule over "British India".

tacts in the village led to his choice of Lodhan, Punjab, as a field site (Mughal 2015). Likewise, anthropologist Amineh Ahmed, because she was both an ethnic-Pashtun (Pukhtun) woman and could claim *bibi* class identity,[24] gained unprecedented access to the rituals of *gham-khadi* (funerals and weddings) of upper-middle class Pashtun women (Ahmed 2006). Class distinctions aside, Zora Neale Hurston's concept of "skinfolk connection" (cited in Williams 1996: 77), through which she identifies the importance of the perception of "shared race" as "shared culture", is also important here. Yet, the same emotional connections can leave scholars beholden to more personal obligations.

In the case of Mughal's research, his class distinction as a Pakistani educated in the "West" also set him apart from an "insider". As he notes, "studying in the UK grants one a prestigious status in Pakistan" and means that someone who straddles the positionality of both an insider and outsider – a "hybrid" if you want – can be received with a warm(er) welcome in ways that an insider perhaps would not, revealing the complexity of power dynamics present among insiders. As a foreign-educated Pakistani, he might also receive preferential treatment from interlocutors of less advantageous socio-economic backgrounds, given the perceived and structural power imbalances between the two. Researchers deemed as "native" have identified their Western education as both an advantage and obstacle during their fieldwork (Khalid 2014: 82–93, Munthali 2001).

I have benefitted from the works of contemporary African and African American women scholars who are not only mindful of their intersectional identities but of how certain aspects become important at certain times, and how these can also vary across one's interlocutors (cf. Lughod 1991, Bouka 2015, Yacob-Haliso 2019). While their reflexivity and positionality led me to consider more seriously the dynamics inherent in knowledge production, I found it nonetheless difficult to locate myself in these accounts. As neither a "white" nor "native" anthropologist, my appearance solicited two kinds of responses: either complete avoidance of any verbal exchange due to my gender and perceived ethnicity, or a stream of questions that would otherwise not be directed at Pashtun or Euro-American women.

As a Han-Taiwanese (Chinese for my interlocutors) and given my sartorial choice, my appearance often led to my being misrecognised by locals as "Afghan", which is a label that subsumes diverse ethnic groups and appearances. The ethnic group of Hazaras, a predominantly Shia minority in Afghanistan, are among the many Afghan refugees who arrived in Peshawar starting in the late 1970s. In my daily interactions in the market, men hearing me speak in an accented Pashto would ask whether I was "Afghan", by which they meant Hazara. During a long conversation on the complex political situation in Afghanistan, an Afghan interlocutor based in Peshawar commented on the shape of my eyes: "You look

24 *Bibi* is a title of respect for a woman from a wealthy family.

like one of my friends in Quetta, you know? You have eyes like the Hazara."[25] I was further made conscious of this assumption whilst in Kabul, where I was often spoken to in Dari and surprised my interlocutors when I responded in a broken Dari or Pashto. In Pakistan, Hazara minorities are based predominantly in the province of Balochistan and for decades have suffered under the growing sectarian violence that intensified with the militancy in the FATA region.[26] In Quetta, the capital of Balochistan, the Shia Hazara community account for about a quarter of the city's population of approximately 2.2 million. Since 1999, Sunni sectarian groups have killed at least 1000 Hazaras and forced more than 200,000 to relocate to other Pakistani cities such as Peshawar, thus providing a potential reason for my interlocutors to misrecognise me as Hazara, despite the fact that I did not think I looked Hazara.

Given this complex ethnic connection, which caused much confusion among my interlocutors, I inhabited multiple, sometimes fluid, positionalities in the field. Depending on how I was perceived, I was treated differently by my interlocutors. When I was mistaken as Hazara or Afghan, there would be minimal exchange between me and some of the more transient interlocutors, such as taxi drivers or shopkeepers. Being identified as a "Chinese" woman and not an Afghan woman meant that Pashtun – men especially – could engage in open and fluid conversations with me without being perceived as "crossing a line" in local customs. Yet I am also cognisant of the fact that it may have been because I was an Urdu- and Pashto-speaking woman with Hazara-like features, wearing what I wore, that I was able to do this research without stirring up too much suspicion. As a foreigner, I was exempted from the strict patriarchal codes that confined sisters and wives of the men working in Namak Mandi. I was able to interact and mingle with men – young and old – in the market, where I was accorded the kind of respect and distance given to a foreign guest (*mehman*).

Others who recognised me as Chinese were eager to speak with me, hopeful that I was a buyer of gems. Chinese businesspersons, men and women, living in Islamabad did occasionally visit Namak Mandi for their purchases and were thus not an unfamiliar sight. Once it was established that I was Taiwanese, which in their eyes, was the equivalent of "Chinese", gem dealers in Namak Mandi began to utilise my linguistic assistance in negotiations with their Chinese clients. Throughout my year-long fieldwork, I became an ad-hoc interpreter for Pashtun and Chinese interlocutors engaged in the gem trade, as well as for Chinese entrepreneurs based in Islamabad. My role as a translator during the

25 Interview, Peshawar, December 2018.

26 The Persian-speaking Hazara ethnic minority forms a small part of the larger Shia population in Pakistan, a group that is roughly one-fifth of Pakistan's population and both ethnically and linguistically diverse. Relations between Sunnis and Shias in Pakistan declined in the late 1970s and early 1980s under the dictatorship of Army General Zia-ul-Haq, whose policies privileged Sunni jurisprudence at the expense of Pakistan's Shia population (Abou-Zahab 2002: 115–130).

time of my fieldwork was mostly unplanned; the experience brought me haphazardly into the world of China-Pakistan relations on the ground that are too often studied through a geopolitical and economic lens focused on the flagship project of China's Belt and Road Initiative: the China-Pakistan Economic Corridor.

Although I benefited from the independence of mobility and the ease of social relations across genders as a "Chinese" in Peshawar, there were many moments when I would also be subjected to local norms precisely because of the way I looked and the clothes I wore: the *chaddar* along with the *kurta pajama*. Sometimes, during interactions with men in the market, I would often be reminded to "move less" between shops as it didn't "look good" for me, to arrange my *chaddar* in ways that covered my hair properly, or to be seated at the end of the shop when an unknown guest arrived.

On one occasion, when a gem businessman from Lahore sought to shake my hand upon meeting me, knowing I was a foreigner, he was curtly stopped from doing so by my friend and informant in the shop, who admonished him gently for seeking to shake my hand, reminding him to give *izzat* ("respect") to his "sister" (me). My women interlocutors also cautioned me against spending too much time with *ghair mard* (unrelated men) in the market, asking me to spend more time at home.

Rather than interpret such instances as limits to my sociality, and by extension my research, I understood these encounters and interventions of my interlocutors as examples in which they sought to include me within their communities and practices as a pseudo-insider, recognising the kind of vulnerability and exposure I would be subjected to, given my perceived identity in the public space. These aspects of research positionality often go unacknowledged in what scholars eventually publish as "empirical data". Our observations and participant testimonies resulting from ethnographic immersion are too often represented and recognised as tidy, orderly and systematic, unlike the realities of sociality that is teeming with complexity, contradiction and human agency, which all come to bear on our knowledge production (Nilan 2002).

The subject of volatility, or volatility for whom?

Time spent in a place often unsettles the researcher's assumptions of the field and brings to the fore imaginaries of volatility and how they can differ from ground realities. Peshawar, despite all its tragedies, is a place of refuge for internally displaced people from the former-FATA as well as for Afghan refugees. Volatility is both a judgement and an interpretation of the overall sense of in/security of a place. I want to consider here how these ideas and designations of volatility, which affect practices in the field, an otherwise optional undertaking, foreshadow the challenges of those who inhabit it in their every day.

Our understanding of volatility is highly subjective and necessitates a look at one's position, race and gender in the society where one conducts research.

In my interactions at the University of Peshawar, I came to know and befriended several students who originate from former FATA but currently reside in Peshawar, often without their immediate families. Our conversations about their varying backgrounds and stories from their native villages were intermingled with tales of the intense difficulties and trauma they had experienced before arriving in Peshawar. For example, one young man reminded me that the reason why many people like him had come to Peshawar is because they had to flee their hometowns in various parts of Waziristan, which witnessed egregious injustices and violence at the hands of both militants (Taliban and Al Qaeda) and the Pakistani army.[27] Peshawar was for my interlocutors an "upgrade" as far security was concerned, a place where they had access to education and other services. It was not perceived as a "volatile environment" in comparison with their own homes in former FATA. Some confided that they had relatives or friends who suffered from psychological and health problems as a result of the trauma experienced in their conflict-ridden hometowns. Many of the students are categorised as Internally Displaced Persons (IDPs), defined as individuals who have been forced to flee their residences as a result of armed conflict, militancy or natural disaster, but remain within the national borders of their country. According to my interlocutors, the number of IDPs in Waziristan proliferated in the last two decades as Pakistan joined the US-led WoT, and "turned many of their primary schools into army check posts".[28] For the sake of basic education as well as other amenities, the more fortunate were able to move to places within Khyber Pakhtunkhwa, such as Kohat, Bannu, Dera Ismail Khan, Abbotabad and of course, the capital Peshawar.

It is often difficult as an outsider to appreciate sufficiently how volatility is experienced by interlocutors. The different snapshots of intense violence that they themselves have witnessed or experienced are rarely foregrounded in the mainstream media within Pakistan, and thereby figure even more marginally in the perceptions of foreign researchers. As a result of such conversations, I retain a profound ambivalence when encountering our (the anthropologists') classification of a place's "volatility". No longer am I able to separate it from what I have heard and how it has made me conscious of my subject position. Expressed in such stories is the misleading notion of "volatility" in any environment, which permeates the decisions of researchers and the everyday lives of locals. But a place labelled as "volatile" for many can also serve as a refuge for others in the way Peshawar has been for IDPs from Waziristan. Hearing these accounts has had a tremendous bearing on the work of reflexivity that I

27 Interview, Peshawar, December 2018.
28 Interview, Peshawar, December 2018.

emphasise in this article as a form of ethical practice, however difficult it is in the given capacity of an "outsider".

In the decolonising push that is gaining ground within the social sciences today, it has become imperative for qualitative researchers (especially those with connections to the Global North) to reflect upon the implications and ethics of how and why they gain access to cultural and social information. Kate Cronin-Furman and Mili Lake (2018) offer some suggestions towards a more ethical research practice that includes recognising complex layers of identity and social dynamics, which have implications for interactions not only with research subjects but also with local partners. The insights of feminist scholars also emphasise the importance of recognising the asymmetrical effects of the global order, as they intersect with the gender, class, ethnicity, nationality, race and religion of individuals, especially in the production of critical scholarship in volatile areas (Medie / Kang 2018). Reflexivity is not only a resource for good and critical scholarship but also a conceptual tool one can use to achieve ethical practice in research. As Cronin-Furman and Lake argue, to emphasise these identities and power dynamics is not to "deny the agency of actors within the South, but rather to produce explanations and theories that capture the complexity of women, gender and politics" (ibid.: 39). And to this I would also add the importance of providing a perspective for our understanding of volatility outside of our own immediate environments.

I want to end, therefore, with an anecdote that demonstrates the importance of such an approach. It is a story of volatility, but not in what we might label a "traditional" site of volatility. Towards the end of my fieldwork, I spent several weekends in Islamabad, where I moonlighted as a translator for members of the Chinese community, several of whom are entrepreneurs. In July 2019, the residence of a Chinese businessman where I was staying was broken into by two armed men wearing police uniforms. As they entered the house – I was sitting in the guest room on the second floor – they swiftly destroyed the CCTV that monitored the gates and locked the main entrance. I was startled by the rowdy entry into my room by one of the "officers". Gun in hand, he shouted at me to come with him. I duly obeyed. As I descended (or better flew) down the stairs with the menacing man behind me, I saw that the Chinese entrepreneur, whom I'll call Mr Wu, was already seated on the sofa facing the other "officer". As soon as I sat down next to him, it became clear to me that I had been brought in for my interpreting services. The two "officers" began showing us images on their phones containing material that implicated Mr Wu in charges of illegal possession of alcohol, cigarettes and weapons, and of hosting an "illegal interracial marriage" on his premises. As I fumbled to translate these accusations into Chinese for Mr Wu, one of the bottles of alcohol that had been discovered stashed in the kitchen was placed on the table in front of us.

They then began to videorecord Mr Wu's face next to the bottle of alcohol, asking me to explain to him that unless he compromised, he stood to lose all his assets and businesses in Pakistan. At this point I understood they were trying to blackmail Mr Wu, but their police identities remained a mystery. When one began to take a video of me next to my passport, my anger flared. Summoning what little courage I could, I demanded to see their police IDs. In response, the younger of the two said in a false sweet tone: "You are our sister. No harm will come to you. You are a scholar and have travelled to Thailand and Swat recently." Then, as if I did not already feel adequately threatened, he added: "We know everything about you."

Nothing came of their threats, since Mr Wu, despite being held at gunpoint, retained his composure, and insisted that he had no more than 20,000 PKR on him and could not offer them the amount they demanded. Exasperated by Mr Wu's intransigence, they turned on me angrily and blamed me for not translating accurately. It is indeed likely that I was *not* translating their threats correctly. They added that they were going to kidnap Mr Wu if he did not pay – did he not understand this? I panicked and relayed these additional threats to Mr Wu, choking back tears of anxiety. During the entire incident, which lasted no more than two hours, from 9 am to 11 am, we were in a large house that Mr Wu had rented in one of the most expensive neighbourhoods in Islamabad, near the Kohsar market. The episode ended with Mr Wu promising to deliver more money on Monday when the banks opened – it was a Saturday. Once the two "officers" left, we were also able to seek help from the Chinese consulate and a local lawyer, before eventually turning to the local police force, whom we suspected to be the perpetrators all along.

This particularly traumatic incident offers us a useful way to reflect upon the notion of volatility inherent in the field. It muddies our usual understanding of *where* and *how* insecure instances take place and *to whom*. While it is Peshawar and its neighbouring regions that are classified as volatile – the marketplace that I study is even described by Western media as "home to terrorist financiers and drug smugglers" (Khan 2014) – it was in fact in the capital city of Islamabad where I experienced my greatest moment of insecurity and danger. In such situations, our notion of volatility – defined mostly by terrorism and other conflict settlings – may not at all align with the kinds of crime that emerge on the ground, specifically in an era of heightened China-Pakistan relations and interactions that are too often subsumed within larger geopolitical discourses. The idea of relative (non-)volatility in larger cities instils among researchers a false sense of overall security, yet it is one that offers vital insight into the kinds of threats a researcher in my position can encounter in any location. As I have shown, the intersection of my gender, race/ethnicity, class in Pakistan led to distinct privileges and also unanticipated moments of vulnerability yet to be rigorously grappled with by Euro-American scholars of the region. The absence

of reference points for an East Asian anthropologist working on Pakistan serves as a useful reminder that we need to continue broadening our discussions of race, gender, ethnicity and nationality as we grapple with the methodological and ethical concerns surrounding fieldwork in supposedly "volatile" contexts.

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Book Reviews

Philip Wadds / Nicolas Apofis / Susanne Schmeidl / Kim Spurway (eds), *Navigating Fieldwork in the Social Sciences: Stories of Danger, Risk, and Reward*. London: Palgrave Macmillan, 2020. 223 pages, €32.99. ISBN 978-3-030-46854-5

All fieldwork has risks. But fieldwork that democratises knowledge production has significantly more risk. *Navigating Fieldwork in the Social Sciences* is one of the most honest and courageous books on fieldwork I have read. Drawing from a wide range of expertise, the authors reveal the risks and rewards of partnering with marginalised people. This partnership allows a better epistemological understanding of and politically contributes to existing subaltern struggles. Indeed, risk is "generative" (p. v). The authors maintain that it is best mitigated by involving participants who overcome it every day – risk mitigation must be a collective act. This embeddedness of research in politics and in risk challenges the ethics review boards that promote objectivity and attempt to eliminate risk naively via formal and rigid protocols.

The book creation was democratic – authors were encouraged to speak freely of the risks they faced within their academic histories and universities and to present the transcripts in a conversational and embodied manner to allow their readers to "witness" these struggles. This method of collecting the authors' narratives following democratically constructed themes is described in Chapter 1. In the succeeding chapters, the authors reflect on how their ethics, politics and social positions – gender, race, age, affiliations – play out and are affected by fieldwork even as they recognise that they "benefit from privilege in myriad ways" (p. 2), notably as scholars affiliated with Australian academic institutions.

Set locally in Australia, Chapter 2 reflects on auto-ethnography among sex workers, Chapter 3 on scholar-migrants' collaboration in trauma research, Chapter 4 on nightlife research on alcohol and drug use and policing, where risk can escalate at any time, and Chapter 5 on work with persons who use drugs and on drug policy reform. Except for Chapter 7, which draws from feminist research experiences using elite interviews in male-dominated spaces, including the International Criminal Court, the remaining chapters all draw on fieldwork experiences worldwide. Chapter 6 shares stories of militant ethnography with anti-fascist anarchist activists. Chapter 8 is on engaged ethnography in rural community-driven development and grassroots women in local politics in India; Chapter 9 investigates development and peacebuilding research in a highly militarised situation. Chapter 10 tells of struggles researching landmine identification and removal in post-conflict situations. The final chapter reveals

the authors' writing difficulties, reflecting on their efforts against the creeping logic of neoliberal publication processes.

The authors' honesty and courage make this publication a provocative and productive read. Offering rich accounts, it enriches debates about the "dark side" and empowering potential of egalitarian knowledge production (Oli Williams et al., Lost in the Shadows: Reflections on the Dark Side of Co-production. *Health Research Policy and Systems* 18(43), 2020, https://doi.org/10.1186/s12961-020-00558-0). The book describes how physical, emotional, moral and career-related risks manifest in different situations. It offers grounded risk management strategies – highlighting the need to "over-prepare" (p. 114), collectively reflect on risks and listen to communities. The contributors also mark how risk and effort permeate the knowledge production process, and advise self-care and care for the research team.

Central in all the stories is building long-term partnerships with participants. Many authors crossed traditional researcher-participant boundaries by becoming friends and offering care to participants in need. As one author admitted, without her local research partners she "would have been lost" (p. 175). This commitment to participants' welfare haunts the authors' publication labours. In addition to leaving out identifying and sensitive information, they agonise about misrepresentation, flawed insights and silencing, given the required brevity in publication. They worry about how the output is publicly used: Will it bear witness to hold people in power accountable (p. 182)? Continuous consultation and reflection post-fieldwork allow participants to challenge findings and enable collective knowledge production and risk-sharing that dislodges the scholar-as-expert.

Given the rich individual contributions, I would have liked to see a collective discussion of the co-constitution of positionalities, engagement and risk-taking, research care work and egalitarian knowledge co-production. Engaged fieldwork ranges from immersed scholars withdrawing from risky fieldwork situations to where the boundaries of research, advocacy and personal lives blur as the researcher is a member of the participants' community. Such discussion needs not be evaluative of the author's contributions, for engaged fieldworkers and their participants better understand the risks, but should be viewed as a distillation of learnings from the rare reflections that this book offers. An important question to be answered collectively is whether other risk-averse approaches could not have produced the knowledges generated in such engaged fieldwork?

A collective reflection could also better examine how the authors' social positions allowed such engagements and sharing of fieldwork narratives within neoliberalising universities. Indeed, we must fight for universities that better recognise embodied research based on extensive experiences and help researchers to overcome the career risks associated with engaged research. This struggle is heightened by hegemonic tendencies of participation that offer illusions of

engagement in order to depoliticise struggles against contemporary exclusion, even in academia (cf. Gulio Moini, How Participation Has Become a Hegemonic Discursive Resource: Towards an Interpretivist Research Agenda. *Critical Policy Studies* 5(2), 2011, pp. 149–168). I re-echo the authors' voices amplifying calls for caring universities that must share care work in knowledge co-production. This responsibility is now disproportionately shouldered by engaged researchers.

The book calls for deeper conversations among activist scholars. Should we talk more about when risk is debilitating? When or *should* we pry open the imagined veil of privacy and better talk about how risks enter our homes? When does politically-engaged research unintentionally results in legitimacy contestation and conflict in communities (as subaltern groups are embedded within differentiated power relations and can contribute to each other's oppression)?

Global South scholar-activists must be included in these conversations. The risks they endure are often more significant, the safe spaces available to them shrinking and the privilege of sharing their struggles absent. Amid the globally rise of neoliberal fascism, engaged researchers in these spaces often continue their work without university protection and funding. Like the field researchers whose participants are their communities, they remain on the frontlines: their own homes and universities.

More than a must-read for field researchers, I hope these contributions beget more honesty and courage from similarly situated scholars, and in this way ease the sufferings and help in the struggle toward egalitarian knowledge production.

Chester Antonino C. Arcilla

Berit Bliesemann de Guevara / Morten Bøås (eds), *Doing Fieldwork in Areas of International Intervention: A Guide to Research in Violent and Closed Contexts*. (Spaces of Peace, Security and Development). Bristol: Bristol University Press, 2020. 308 pages, €94.97. ISBN 978-1-5292-0688-3 (hb)

In contexts where there is a need for development-oriented international engagement, there is an increasing demand for complex, inter- and transdisciplinary research. The conducting of such research is frequently hampered by complicated and unexpected challenges and impediments, which are caused by differences in social, political, religious and language settings, among others (Charles et al. 2021). Fieldwork aimed at researching and diagnosing the actual bottlenecks where interventions are required is often expensive and takes longer to be completed, especially when it is necessary to collect biophysical data or carry out field experiments. There has long been a need for a publication that highlights

the issues and challenges of conducting field research in volatile contexts. However, it is difficult for one or two scholars to compile such information under diverse geographic and socio-political conditions.

Doing Fieldwork in Areas of International Intervention is a comprehensive field guide meticulously designed, written and edited by top experts in conflict, peace and intervention politics. The efforts of the editors deserve recognition, as they have managed to compile the field experiences of various scholars over three decades of monumental fieldwork carried out in different social, political and geographic settings.

This book is divided into four sections, the first focusing on the difficulties of fieldwork-based research in areas of intervention and the associated confusion and failures involving research in violent and closed settings. In the second part of the book, titled "Security and Risk", the various contributors critically highlight the challenges of research in areas that are strictly controlled by authoritarian rulers and violent groups. The contributions highlight the dilemmas of researchers in post-conflict environments that often carry a great deal of suspicion towards researchers from different socio-cultural and political settings. This section provides convincing details about the ethical restrictions faced by academics and researchers in authoritarian states (see John Heathershaw and Parviz Mullojonov: "The Politics and Ethics of Fieldwork in Post-conflict Environments: The Dilemmas of a Vocational Approach", pp. 93–112).

Boukary Sangaré and Jaimie Bleck ("Challenges of Research in an Active Conflict Environment", pp. 113–126) describe the challenges they faced during their research in Mali between 2012 and 2017 amid ongoing sectarian violence, terrorist activities and the state's incompetence in dealing with the instability and widespread public discontent. The authors critically discuss how pragmatic security concerns affect methodological rigor and transparency under such challenging conditions. Meanwhile, George M. Bob-Milliar ("Introduction: Methodologies for Researching Africa," *African Affairs* 121(484), 2022, pp. e55–e65), in a parallel study, highlights the need for genuine research participation while using critical assessment to choose the best strategy and formulate the main research question. A similar study by Acar et al. (*Researching Peace, Conflict, and Power in the Field: Methodological Challenges and Opportunities,* Springer Nature Switzerland, 2020) examines key methodological possibilities with greater detail, especially in conflict-affected, pre-conflict and post-conflict communities.

The articles in *Doing Fieldwork in Areas of International Intervention* also highlight important issues to consider if researchers need to collect data in a conflict environment. This book's third section covers subjects linked to studies in hostile environments from Yemen, Bosnia and Herzegovina and Sudan to Myanmar. It deals with positionality, which has frequently been ignored in political science research to (allegedly) preserve the "objectivity" of the researcher. Furthermore, the authors strongly emphasise the need for a suitable

framework designed to support local researchers in interpretative research. The fourth section emphasises the importance of gender and entangled sensitivities during field research. It also highlights ethical vulnerabilities and subsequent mitigation strategies in the research design process and when presenting fieldwork findings wherever development interventions are intended. The editors, Berit Bliesemann de Guevara and Morten Bøås, highlight the challenges associated with researching the subject of sex and associated sensitivities while ignoring the difficulties field researchers face due to their own sexual orientation.

Although there are fewer examples of religious sensitivities while doing research in those societies where religion forms a vital part of everyday life as well as constitutional discussions, there are nonetheless rich examples of challenges encountered while researching in authoritarian states with gender-related sensitivities, in pre- and post-conflict environments. Unlike the compilations of Charles et al. (*Researching the Middle East: Cultural, Conceptual, Theoretical and Practical Issues*, Edinburgh University Press, 2021) and Mac Ginty et al. (*The Companion to Peace and Conflict Fieldwork*, Palgrave Macmillan, 2021), which focus respectively on the MENA region and Global North only, this book provides a comprehensive review of the intricacies and limitations of doing research in violent and closed environments across a wider geography. It is vital to diagnose the dos and don'ts in an environment where a research activity is being initiated. This publication offers diverse examples and assists early career researchers and graduate students in comprehending the problems of conducting research in heterogeneous violent and closed situations, helping to add value to the quality of the research by mitigating possible risks and issues.

Fazlullah Akhtar

Marjo Buitelaar, Manja Stephan-Emmrich, Viola Thimm (eds), *Muslim Women's Pilgrimage to Mecca and Beyond: Reconfiguring Gender, Religion, and Mobility*. London: Routledge, 2021. 222 pages, £36.99. ISBN 978-0-3676-2810-9

The present book investigates the topic of Muslim sacred journeys to a variety of Islamic holy places in various countries. The most notable of these pilgrimages are to sites in today's Saudi Arabia, especially the Kaaba in Mecca and the grave of the prophet Muhammad in Medina. In addition to these, the articles in this book include religious travels to a number of other shrines found around the Islamic world. The main focus is on Muslim women's mobility from Islamic countries and other parts of the globe to Mecca and other areas of the Islamic world, as well as the link between these mobilities and globalisa-

tion, current technology and infrastructures. The majority of the papers in the book tackle the problem of women's pilgrimage in modern days, and they benefit from productive case studies. By focusing on the connections and interlinkages between the different arenas of Muslim pilgrims (p. 4), the editors have succeeded in reconfiguring their "understanding of Muslim pilgrimage through the lens of women's new mobilities" (p. 5).

Muslim Women's Pilgrimage to Mecca and Beyond is largely written by women scholars and is structured in ten chapters with a helpful introduction. Geographically, the book's essays cover pilgrimages or pilgrimage sites in various parts of the Islamic world, such as Morocco, Indonesia, West Africa, India, Iran, America, the Netherlands and Bosnia and Herzegovina. Muslim women's pilgrimages within various Muslim sects and denominations are also considered, as for example one essay that researches pilgrimage among Shiite Muslims ("Shi'i Muslim Women's Pilgrimage Rituals to Lady Fatemeh-Masoumeh's Shrine in Qom").

The book is especially notable for combining the investigation of women's holy journeys to sites in many countries with that of the *hajj* as a definite religious duty (seven of the ten articles examine the *hajj*, the pilgrimage to the Kaaba in Mecca). Looking at the scientific literature published on these topics, we can see that only a few publications have thus far addressed these two fields together. The articles edited by Marjo Buitelaar, Manja Stephan-Emmrich and Viola Thimm fill this gap and provide an appropriate blend of numerous perspectives and methodologies on the issue of pilgrimage, allowing the reader to delve into a comprehensive and multidimensional examination of female pilgrimage in Islam. Although the articles are primarily written following an anthropological approach, in line with the editors' own expertise in this scientific field, contributions that consider the jurisprudence of female pilgrimage ("Under Male Supervision? Nationality, Age and Islamic Belief as Basis for Muslim Women's Pilgrimage"), the cultural history of female pilgrimage ("Considering the Silences. Understanding Historical Narratives of Women's Indian Ocean Hajj Mobility") and a rereading of some travelogues and pilgrimage memoirs (articles 9 and 10) give this book an interdisciplinary approach and consequently provide rich content.

Despite the efforts of the editors to include studies on non-*hajj* pilgrimage traditions of Muslim women, the lens of pilgrimage research remains primarily under the shadow and impact of the *hajj*, due to the scarcity of academic scholars working on alternate pilgrimage destinations. Even the first article ("Under Male Supervision? Nationality, Age and Islamic Belief as Basis for Muslim Women's Pilgrimage, by Viola Thimm), which explores the question of women's pilgrimage from a jurisprudential standpoint, limits itself to *hajj* jurisprudence and does not examine the legal challenges faced by women on non-*hajj* pilgrimage. Furthermore, some articles pay less emphasis than expected

to one of the book's core concerns, namely mobility and travel. The inclusion of at least a few images of women on pilgrimage would have been highly welcome to deepen the reader's insight into the book's fundamental theme in a visual way. Finally, Muslim communities are facing dramatic social and cultural changes, particularly among young Muslims and the so-called "Muslim Z generation", who make up a substantial portion of the global Muslim community. The book sadly does not pay attention to this huge group of young Muslim pilgrims, which would have rounded off the otherwise multi-perspective elaboration of the issue.

However, in any regard, this timely publication should be of interest for scholars of Islamic Studies, the anthropology of *hajj* in particular and pilgrimage in general, the anthropology of Islam, Mobility Studies and Women's Studies, for some time to come. For example, the essays collected in this book effectively present the connectedness between Muslim women's pilgrimage and other issues, such as the market, shopping activities and business. In this way, they contribute to the flourishing trend to move away from stereotyped, essentialist and exclusionary views of Muslim women. The editors' ambition to concentrate "on how such cultures inform the normative, social and practical dimensions of their pilgrimage practices, and, in turn, how women's practices inform specific discourses on their mobility" (p. 13) is clearly expressed throughout the book. Furthermore, this publication adds valuable perspective to the theories of pilgrimage, which are otherwise mainly formed in a masculine and non-Islamic context.

Peyman Eshaghi

Ismail Fajrie Alatas, *What is Religious Authority? Cultivating Islamic Communities in Indonesia*. (Princeton Studies in Muslim Politics). Princeton, New Jersey: Princeton University Press, 2021. Xvi, 268 pages, 1 map, $95.00. ISBN 978-0-6912-0431-4

The Bā ʿAlawīs, who claim to be descendants of the Prophet Muḥammad, or *sayyids*, have been an increasingly popular topic of study over the last twenty years. Historians and anthropologists, or the confluence of the two, have enormously contributed to the study of this distinct community from the Ḥaḍramawt valley in Yemen, as well as their diaspora in the Indian Ocean, thereby reshaping a new discourse of a "transnational turn" since the 1990s. Among other scholarship, Engseng Ho's macro-diachronic analysis, *The Graves of Tarim: Genealogy and Mobility across the Indian Ocean* (University of California Press, 2006), is a landmark in this transregional/transnational study, as it innovatively interprets the diasporic community from the late medieval period onwards through

the lens of genealogy and mobility. Ho's masterful narrative, creative metaphors and employment of multiple spaces and times invigorate the significance of the genealogical past for the hybrid lives, texts and geographies of this transoceanic community.

What is Religious Authority? by Ismail Fajrie Alatas continues the direction of this interpretive study, combining history and cultural anthropology. Drawing from but departing from Ho, Alatas amalgamates the historical diachrony and contemporary synchrony of the *sayyids* in Java, Indonesia, by reproducing the "practical and ideological centrality of genealogy and mobility in the formation of Islamic authority" (p. 110). In this sense, Alatas's case study is a micro-analysis of Indonesian *sayyids* from the aftermath of the Great War of Java in the early nineteenth century to the early twenty-first century and how they followed two different paths of mobility in the pursuit of religious authority: internal and external. The former is highlighted in the author's account of transregional travel, circulation and the pursuit of knowledge within the same Bā ʿAlawīs, from Yemen to Indonesia back and forth, or within itinerant contexts on the island of Java. The latter is demonstrated in what Alatas calls "divergent mobility", describing the religious authority of Habib Luthfi bin Yahya of Pekalongan (b. 1947), the distinctive Sufi master in present-day Indonesia and the main protagonist of this book, in seeking sacred legitimacy from non-*sayyid* luminaries in Java in addition to his own divine and worldly wanderings in order to "cultivate" Islamic congregations beyond the boundaries of the Bā ʿAlawīs.

By combining these two trajectories, this book does not aim to produce an anthropological biography of Habib Luthfi per se. Rather, it compares various actors, locations, texts and materialities in a bold narrative to examine the transmission of Islamic knowledge and the social enactment of Prophetic teachings, namely the living *sunnah*, culminating in the account of how Habib Luthfi became a saint. The Weberian paradigm of routinisation of charisma, Alatas reveals, is insufficient to understand religious authority. Following Hannah Arendt, the author argues that religious authority rests "on the recognition of their [religious leaders'] connection to the Prophetic past and hinges on a hierarchical relationship that allows them to articulate Prophetic teachings for others without resorting to coercion" (p. 4). A corollary of this formulation leads to the observation that this authority is extremely dynamic and requires the constant labour of (re)production and maintenance. Even authorities that have often been deemed charismatic – in the Weberian point of view – must repeat and reproduce the labour of articulating Prophetic teachings and transmitting to or connecting them with the community.

In this sense, Alatas develops the doctrinal dimension of *ahl al-sunnah wa-l-jamāʿah* (the people of the *sunnah* and the community) into a multifaceted exploration of the social realities, possibilities and contingencies of the foundational past, religious authorities and the community. The author underpins his

Arendtian theorem of "articulatory labor " as "the labor of articulating *Sunna*", which is repeatedly stated as a key concept throughout the book, on the basis of his eclectic, if not synthetic, approach and critical reading of various Western thinkers from Hegel and Marx to contemporary philosophers and different theorists in Islamic studies in Western academia, including inter alia Talal Asad, Shahab Ahmed and Shahzad Bashir. Although the case study is of Central Java, the author's use of the multiple genealogies of Ḥaḍramī scholars and saints produces a theoretical contribution that presents not only a refined understanding of Islamic authority but also the universality of Islam as a "concrete universality". What makes Islam universal is, he argues, the concrete labour of articulating the *sunnah* and the community that has engendered doctrinal and practical multiplicity.

The first part of this book, comprising three chapters, discusses the historical formation of Islamic communities between the Ḥaḍramawt and Java and the making and unmaking of religious authorities in Pekalongan, Central Java, by tracing multiple genealogies and mobilities of saints as well as their connections to politics and infrastructure that materialised their modus operandi in concrete and ever-changing ways. As part of his successful portrayal of the role of the general Sufi orders and saints in Java as links to Islamic teachings, including the Shaṭṭāriyyah and Naqshbandiyyah-Khālidiyyah, the author describes the "Ḥaddādian paradigm", named after the legendary Bā ʿAlawī scholar ʿAbdallāh b. ʿAlawī al-Ḥaddād (d. 1720) – the highly effective praxis of religious articulation between Ḥaḍramawt and Java. Concise texts and litanies that emulated a classical form of textual summary, composed by al-Ḥaddād, were indeed widely disseminated across the Indian Ocean, especially from the nineteenth century onwards. In Java, these textual and performative dimensions became a particular paradigm at that time, as Islamic authorities dispersed following the defeat of Prince Dipanegara by the Dutch colonial power in the nineteenth century. This paradigm, according to the author, was prominent in proselytising practical Sufism and teaching the laity in rural and urban areas of Central Java through its constant reproduction by *shaykhs al-taʿlīm* or religious propagators in various communities.

Based on the historical and cultural settings in the first part of his book, Alatas presents the making of Habib Luthfi's Sufi genealogies, networks, hagiographic imaginaries and religious authority in the second part. The author compares Habib Luthfi and his Bā ʿAlawī counterparts over the last century to those of more recent years to demonstrate the convergence between the Ḥaddādian paradigm and Habib Luthfi's wide-ranging efforts in pursuing different scholarly and Sufi itineraries that reshaped his authority in cultivating diverse segments of Muslim communities and his relations with politics and state actors. Although the labour of Habib Luthfi has not always been smooth, he has become the foremost Sufi master, whose edifying method extensively attracts commoners, elites,

militaries, scholars, businessmen and other social classes locally and nationally. The second part of the book, consisting of four chapters, provide a vivid portrayal of Habib Luthfi as a Sufi master who, unlike his rivals who use only Arabic, excels at local languages, is able to collect diverse social groups, creates cooperation with state apparatuses, and has become the epitome of the "living *sunna* ".

Alatas constantly refers to his informed theorisation of labour and its derivations and finally portrays the historical and contemporary figures with a series of vivid images and re-interpretations. He fills an important gap in the historiography and ethnography of the Indonesian *sayyids* with a fresh scholarly comprehension of cultural semiotics. One of the most important themes is that his redefinition of multi-voiced articulations and practices of the *sunnah* throughout history and contemporary lives encourages scholars of Islam, in Asia and elsewhere, not to use in scholarly production the vague ideas of religious syncretism and heterodoxy as opposed to the Christian-based notion of orthodoxy. Despite the book's merit in bringing together both theories and narratives, readers might expect to learn more about various Arabic terms coined by the Bā ʿAlawīs, such as *shaykhs al-taʿlīm*, which seems to be predominantly used as a category for all Javanese societies. Other technical terms, such as *manṣab* to refer to a "successor", might need to be specifically explained in a glossary, since *manṣab* in medieval Damascus is historically a stipendiary post (see Michael Chamberlain's *Knowledge and Social Practice in Medieval Damascus, 1190–1350*; Cambridge University Press, 1995). In addition, useful appendixes would have been helpful to map the rather tedious and not particularly exploratory genealogies (*silsilahs*) of various mentioned Sufis and the various books they learned as part of their scholarly vitae in addition to their Sufi importance. Such basic research is necessary to ground different notions of intellectual authority vis-à-vis, or even in combination with, religious authority.

Students and specialists of Islamic Studies might also expect this fascinating study to be related to established methods and literatures pertaining to the study of *sayyids*. In this case, Kazuo Morimoto's series of what he calls "sayyido-sharifology" is missing from this book. Other sources that are not cited include, for instance, Abdel Ahad Sebti's 1984 study of the Moroccan sharifs (*Aristocratie citadine, pouvoir et discours savant au Maroc pré-colonial*), which also deals with saints and power. The author's reference to "sociocultural capital" (p. 183) does not mention Bourdieu's famous theory, although Alatas obviously intends to break away from the latter's original theory, as with his intention to destabilise the Weberian notion of authority. Kevin Reinhart's *Lived Islam: Colloquial Religion in a Cosmopolitan Tradition*, published in 2020 (Cambridge University Press), which overlaps with some of the ideas of this book, was probably not yet available to Alatas when this book's manuscript was completed. There is at least one factual error: the death of the Meccan scholar and Sufi master Muḥammad b. ʿAlawī al-Mālikī was in 2004, not 2006. The author additionally

does not adequately address his clarification that "the Ḥaddādian paradigm complemented, but also competed with, other text-centered articulatory paradigms brought by pilgrims returning from Mecca" (p. 63) – a statement that is presumably intended to explain other existing "paradigms", including the Shaṭṭāriyah's role from the seventeenth century onwards, in ordering and maintaining the basic Sufi litany and cosmology throughout maritime Southeast Asia.

Finally, readers of the social sciences will also notice the absence of the concept of "civil society" in this book, and it fails to address the question of whether or not the social formation of Habib Luthfi's religious authority, for example, blurs the identity boundaries of citizens between society and state. Or, reading between the lines, one might view this book as a contemplation of the unnecessary differentiation between state and civil society, describing instead an elastic, yet coherent movement that preserves a unitary vision of political aspiration, a prolonged version of the classical political theology of *ahl al-sunnah wa-l-jamāʿah* (Sunnism): a state-religion-society mutual alliance to maximise social order in pluralistic, if not sectarian, milieus such as Indonesia.

Nonetheless, historians, anthropologists and readers of social sciences in general will definitely consider this highly recommended book as a valuable resource to read and ponder.

Zacky Khairul Umam

Mina Roces, *The Filipino Migration Experience: Global Agents of Change*. Ithaca / London: Cornell University Press, 2021. 254 pages, $49.95. ISBN 978-1-5017-6040-2

The Filipino Migration Experience was refreshing to read, especially because of the way Mina Roces uses "migrant archives" to reveal more detail about how migrants act as "global agents of change" through their consumption, activism and philanthropy. The book is a much-needed intervention in the academic literature that overwhelmingly sees migrants as victims of the global system that exploits their labour and entails high social costs for them and their families. Its historical perspective differs from the dominant narrative that positions the migrant as a "disenfranchised laborer" (p. 7). The migrants the author has chosen to focus on tell a complementary story that is replete with information about how they have transformed social norms, shaped economic activity, acted as philanthropists and curated their own histories.

The book is divided into three parts that examine different ways in which migrants are "global agents of change". First, Mina Roces examines how migrants have impacted social norms that relate to the family as well as gender

roles and sexuality in the Philippines. The reader learns that the concept of family has been expanded so as to be more inclusive of individuals without "nuclear or extended" ties, such as fellow NGO members. Their migration and open discussion of "extramarital affairs and abandonment of wives and children" has also broken a "social taboo" that mythologises the "sanctity of the Filipino family", whose realities should not normally be discussed in the Philippines (p. 31). Mina Roces then argues that migrant women in particular have challenged cultural constructions of the "feminine by taking in lovers … or indulging in adultery" (p. 46). However, one is left wondering: How do these challenges play out in the Philippines more generally?

The second part offers two chapters that make a case for how migrants from the Philippines have changed various economic landscapes through their increased power to consume. Much academic literature has focused on changes in consumption behaviour by migrants and their families as well as casting the migrants themselves as "agents of development". As Mina Roces explains, the remittances they send have transformed the "history of banking and banking services in the Philippines" (p. 85). The opening of financial services overseas that cater to migrant needs, e.g. by remaining open on Sunday for domestic workers in Singapore, are an example of that. Another development that Mina Roces carefully outlines is the impact that overseas earning power has had on investment services and the real-estate market in the Philippines. Such in-depth explorations need to be conducted in other large migrant-sending countries in Southeast Asia, such as Indonesia.

The third and final part discusses how migrants advocate for change in their host country as well as at home. The chapters consistently position migrants at the forefront of efforts to curate their own history, advocate for change in their host countries and provide much-needed expert and material assistance in the Philippines. The main contribution here is that the book shifts the focus away from the well-documented marginal and peripheral roles migrants play in our global economic system to the centrality of their interventions outside the world of work. In fact, the author concludes that altogether these chapters provide a "fresh perspective on migrant influence beyond labor" (p. 173). I could not agree more.

The Filipino Migration Experience achieves its objective as stated at the outset: to tell the story of how migrants have impact. This is an important contribution that challenges the assumption that migrants are always only liminal actors. However, the dual position of so many migrants as "disenfranchised laborers" begs the question: to what extent then are these migrants also "global agents of change"? Mina Roces never suggests that they are not both, yet we do not get an idea of how she measures one against the other in her empowering narrative. Such an appreciation might have justified the absence of any discussion about the vital activism on the part of migrants to resist, for example, depor-

tation. Such omissions seemed at times glaring when the overall objective was to shed light on the dual role of migrants as "global agents of change".

My other concern is that the methodology removed the distinction between different types of migrants (e.g. temporary vs permanent) to then "offer new categories to understand migrant experience" (p. 7). In practice, this meant that it was easier to create the narrative of "global agents of change". But how can the experiences of temporary, employer-tied contract workers in Singapore, for example, be compared to those of ethnic minorities with permanent residence or citizenship in the United States? As a migration expert, I would have liked to know why they are comparable.

Nevertheless, *The Filipino Migration Experience* is important reading for scholars interested in migration research from a historical perspective. At the very least, it provides a much-needed corrective to dominant narratives of migrants' experience that position them as passive victims instead of as simultaneous "global agents of change".

Wayne Palmer

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Notes for Contributors

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