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Diego Astorga de Ita *Editors*

Navigating the Field

Postgraduate Experiences in Social Research

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Foreword

To the Fractured Field

Once upon a time, back in the bad old days before training courses were largely invented for postgraduates, you could count on the fingers of one hand the advice given about what to publish (and indeed many other things). One piece of advice that I recall though was a slightly incredulous senior staff member looking at a draft I had written on qualitative methods (looking, not reading it) and exclaiming 'Why on earth would you publish about methods? Do you want to end up teaching methods classes for your whole career?' Their astonishment was, of course, very wrongheaded (though their prediction also remarkably wise and true). I was at that time trying to work through how come so much of the literature on methods seemed to be referring to a kind of fieldwork that turned out to be deeply connected to 'the field', noting the definite article, that I was not sure existed. Or, rather, worrying that it did exist only I had not been to such a site, and thus my work was irredeemably flawed. If it was the field experience that gave authority on a topic, then I seemed to risk not being authoritative.

As this volume shows, things have moved on. The sense of 'the field' retains, for sure, a charge and an allure, but the taken-for-granted sense of it as a solid place to which the researcher goes and from which they bring back knowledge has long been dismantled. That spatiality of here and there, field and academy, worlds of things and worlds of analysis has long been undermined by pointing to the worldliness of the academy and the reflexive entanglement of it with the world in which it sits, rather than upon which it comments. As I read the chapters here that entanglement and fracturing of the solidity of both academy and field come through strongly. Where once upon a time, acknowledging the situatedness of the academy, people first began studying up and down, and the exploring positionalities of academics that enabled insider as well as outsider research, this collection further dismantles the confident singularities of such positions as well as of those locations. Let me simply try and draw out what, when I was doing a PhD, I would have been tempted to call the plateaux or molar consistencies among the various lines of flight in the volume.

The first is that the unsettling of the field and the academy clearly unsettle the category of the researcher as fieldworker. In so doing the insecurity produced is not so much epistemological, how we accord truth value to the experience that so worried my generation, but an ontological insecurity. The concerns are with categories of the nature of what exists. Here we have 'local' mothers being judged for class and status, co-nationals confounded on status and ethnic lines, and academia found to be foreign and capricious, far from the safe interior of reason and logic so often implied. It is no longer the case that the complexities of positionality are hard to represent (though they are) and addressed via textual strategies (though they still are) but also that they are complicated to the point various positionalities are hard to enact.

Second, it is not just the assumed identities of 'in here', in the academy, that are pluralised but the category of the field as a stable entity is under scrutiny. No longer other to the academy, nor assuredly known as it seemed when the first ethnographies at 'home' challenged its exoticisation, the notion of what makes a field site is now less clear. The unitary spatiality and temporality, a place and a time apart from the academy, has been challenged not just through multi-sited work, but work that sees the field as never forming a holistic entity — not even topologically stretched or scattered. The patchwork and scavenger modalities raise questions further beyond just a spatially or temporally distributed field. The very registers of the field start to flex, as the reflexive constitution of it binds in with academic knowledge. What is the knowledge produced studying field sites which are the very forum where actors come together to perform, to enact and argue for their identity as say a financial sector? To what extent does this represent a reality that exists anywhere other than in that performance? How does a volunteer community that exists for a trip represent values of practices that exist elsewhere? What if copying onscreen practices means the performance at tourist sites overtakes the thing it emulates?

Third, these two destabilisations open up the affective challenge of fieldwork. As much commentary has noted, the process of fieldwork puts the fieldworker at risk — hopefully not physically, but their identity and emotions are part of the process and thus subject to the pressures of fieldwork. Often this has been traded upon to construct the heroic identity of the fieldworker triumphing over adversity written as a bildungsroman and rite of passage into the academy. Once upon a time, people traded stories of hardship and derring-do, as though the quality of the research produced rose in proportion to the hazards encountered producing it. This sort of narrative formulation couples a personal transformation with the field as liminal site, in the classic sense for people like Turner, where the temporary transitional state leads to a transformed individual in terms of social status. The security of this outcome has never seemed certain to those doing doctoral studies of course, no matter that supervisors may appear infuriatingly confident of it. Instead of the thesis as a tale of self-discovery, challenging the nature of what the field and the academy and the fieldworker mean has exposed the vulnerabilities of the researcher, alongside the researched. There has of course long been a critique of the implicitly assumed subject position of the fieldworker, where a proliferating series of qualifiers have shown how the archetypal fieldworker trades on privileges of, in

something like the order they have been subject to deconstruction, gender, class, nationality, race, sexuality and (dis)ability. Moreover, what has become apparent is the singularity of that assumed fieldworker subjectivity. For sure it is a singular as in particular social positionality, which has been accorded the status of somehow being more objective via a supposed detachment, which serves to write very particular and constricted values into the categories through which valid knowledge is judged (and of course against which we fear both our work and ourselves will be judged to be failing). But it has become clear it is also a rather asocial singular and unitary notion of the subject. The fieldworker singular obscures the various companions that may be involved — spouses, children and other animals. It also displays the methodological individualism of presuming against shared knowledge or team-based research. If the field is pluralised then so might be the researchers, as an answer to the physical exhaustion and carbon footprint suggested by multi-site ethnographies tracking phenomena at global scale. Human geography PhDs may be reframed were we to think more like the physical sciences with laboratories of research students each on elements of larger collective projects.

There is a real pleasure in seeing the accounts of how these different doctoral research projects engaged in fieldwork. It is different from reading the fascinating theses which resulted because theses remain retrospective works, binding together the whole. What if the result was say, less a Socratic dialogue than, an epistolary novel of the process in action? Of course, that form of many of these projects exists in my email archives. I do not for a moment pretend that those capture somehow the real process — being clearly self-presentational acts that perform the identities and relationship of supervisor and student. But they offer a different window on the process before the outcome was known. Reading the accounts here reminds me of that openness, fragility, vulnerability but also the hopes and opportunities. And of both those hoped for opportunities and feared setbacks, only some were realised. And even fewer were written into the theses. What comes through in this volume's revisiting of those moments is the intensity of the experiences, the urgency of the process and the insecurity it produces. Clearly coming through these pages are some of the risks and pressures created — through vicarious stress, emotional transference and all-prevailing fear of just 'getting it wrong.' But what also comes through this volume is one of the strengths — the collective support, the shared intelligence (in the sense of abilities, information and emotional relations) and the camaraderie between students in a graduate school. I had the joy of seeing many of these projects emerge close up. And that is the emotional register with which I want to conclude here. Knowing these researchers, hearing them work through these issues, helping them develop their imaginative solutions and outcomes was indeed a real pleasure. A pleasure where the real job of the supervisor is indeed one of giving permission for the researchers to do what they already knew they had to do, and thus to produce the fine pieces of research they went on to develop.

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Preface

Like many great academic ideas emerging from the Department of Geography at Durham University, the idea behind this book came out of an informal conversation among postgraduate research students at lunch time in the departmental common room — The Manley Room. The discussion was centred on what fellow students considered missing in the academic literature on the PhD research process. It was not a surprise that navigating fieldwork as inexperienced researchers came up as a dominant theme of interest. Every year (at least since 2015), senior PhD students have organised a fieldwork workshop to share their experiences with first-year PhDs who are at the stage of preparing for their fieldwork campaigns and feeling worried about not getting enough data. We thought it was a good idea to document these issues in order to fill the existing gaps in the academic literature, and to provide a text to reference, and also some sort of support for researchers who must construct their own unique research paths. Subsequent meetings and workshops were held to refine the ideas mostly during 'Pizza Fridays', a fortnightly social event in The Manley Room when, as the name suggests, postgraduates and staff gather for pizza and conversations.

Writing and editing have been mutual learning processes. We kicked off the process with a writing workshop, in which we explored the aims, scope and intended audience of this book, agreed on the timeline, and detailed the writing guidelines. In addition to three rounds of extensive reviews by the editors, we also organised rigorous peer reviews where contributors commented on one another's drafts, and a feedback workshop (online due to Covid-19 pandemic) where contributors asked questions and exchanged ideas to help each other to improve the chapters.

The three distinctive features of this book are that (1) it is a postgraduate-led editorship and a volume written from the perspective of postgraduate students, (2) it is a collection of critical conceptual, methodological and practical reflections, and (3) it is useful for researchers of all levels and across disciplines. We hope this will allow the forum to take a step back and think again about access, ethics, identity, positionality and power in research design, which are essential aspects of social research, though often neglected or taken for granted. Given the diverse nature and contexts of the reflections offered in this book, we hope that this book will have an

enduring appeal to researchers at different stages of their career. Whilst we hope that this volume will be useful across disciplines, this also could be the first of a discipline-focused series set out to give a voice to early career researchers to share their unique experiences of doing fieldwork across different forms of field whilst providing a useful resource for inexperienced researchers to both create and navigate their own research paths and ideologies.

Our foremost gratitude goes to all the contributors who took time out from their busy schedules to share their insightful fieldwork ideas and experiences. It was not a short journey — it started off when we all were PhD students; now some are writing up their thesis, some will have become doctors by the time this book is in print, some have started with new positions and others are negotiating life as new parents. We appreciate their willingness to be open and honest about their experiences, including the successes and failures in the hope of encouraging future generations of social science researchers to steer their own research wheel. We are particularly grateful to two of our colleagues, Lucy Smout Szablewska for proofreading the manuscript and Marcin B. Stanek for his contributions during the proposal phase of this project.

We are very thankful to our advisers, Professor Mike Crang from Durham University and Professor Divya Praful Tolia-Kelly from the University of Sussex, who have kindly written the Foreword and Afterword respectively. Mike and Divya are, and have been, the supervisors of many of the contributors in this volume who in no doubt have learned from the wealth of their experiences. We also appreciate Professor Sarah Atkinson, the postgraduate coordinator of the Department of Geography at Durham University at the time this book was written, for her support for this project. We are grateful to the Department of Geography for providing a fertile environment for meaningful academic conversations which led to the birth of this book to take place, in addition to funding the workshops that helped shape this volume.

We are enormously grateful to Springer for their trust and for allowing us to write and edit this book, even though we were all in early stages of our academic careers and had limited publication records at the time we submitted the proposal for this book. Thank you for giving us this invaluable opportunity to share the ‘real stories’ behind our research journeys. We are grateful to Zachary Romano, a publishing editor at Springer, who encouraged us to pursue the ideas put forward in this book during an informal conversation at the 2017 Royal Geographical Society annual conference held at the University of Cardiff, UK. We hope that this postgraduate-led, and discipline-focused fieldwork volume can be carried forward with new experiences and voices.

York, UK — Mildred Oiza Ajebon
Bremen, Germany — Yim Ming Connie Kwong
Durham, UK — Diego Astorga de Ita
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Contents

About the Contributors

Iqbal Ahmed is a final-year doctoral researcher in Human Geography at Durham University. His research focuses on questions of children's perspectives on rights, power relations, postcolonial childhood, and politics of development in Dhaka, Bangladesh. Although he has a horrible sense of direction, he really enjoys outings in the beautiful landscape of the northern UK.

Mildred Oiza Ajebon is currently a Research Fellow in the Department of Health Sciences, University of York, UK. She works on a Born in Bradford (BiB) birth cohort study, a globally recognised landmark study, which investigates inequalities in child health and well-being in the multi-ethnic and deprived population of Bradford. The role is in collaboration with the Bradford Institute for Health Research (BIHR). She completed her undergraduate degree (BSc Hons) in Geography and Regional Planning, University of Benin, Nigeria. She was then trained in Geographical Information Systems (GIS) at both the University of Ibadan, Nigeria, and the University of Leeds, UK. Mildred completed her PhD in Human Geography at Durham University with a focus on integrated approaches to understanding determinants of child health inequalities in Nigeria at different spatial scales. Her MSc (Leeds) and PhD degrees were sponsored by the Commonwealth Scholarship Commission in the UK. She obtained funding from the Schlumberger Faculty for the Future Programme towards her PhD fieldwork. Her PhD supervisors have backgrounds in the fields of anthropology, environmental epidemiology, human geography, medical humanities and public health. Mildred is also a Fellow of the Higher Education Academy (HEA), UK. She started her academic career at the Federal University of Technology, Minna. She later joined the University of Benin as a member of academic staff before starting a PhD at Durham University. Her research interests include GIS applications in health and environment. She has a strong interest in using mixed methods for understanding inequalities in global health, social risk and resilience, female agency and population health.

Diego Astorga de Ita obtained his BSc in Environmental Science and MSc in Biology from the Institute for Ecosystems and Sustainability Research (IIES) at the National Autonomous University of Mexico (UNAM). He has conducted research with Yucatec Maya peasant farmers looking at beekeeping within Maya landscapes and Maya culture. He has also researched traditional production systems in Oaxaca with the Centre of Studies for Change within the Mexican Countryside (CECCAM A.C.), a not-for-profit organisation that works with rural workers, responding to specific problems of these communities and trying to generate knowledge and change alongside farmers and producers. Diego is currently pursuing a PhD in Human Geography at Durham University funded by the Mexican National Council of Science and Technology (CONACYT). Throughout his doctoral studies Diego has also been a recipient of complementary scholarships from the Mexican Department of Public Education (SEP), Durham University's Ustinov College, and Ustinov College's Global Citizenship Programme. His current research looks at the co-production of Culture and Nature in the case of folk music in South-eastern Mexico and the historic landscapes of the region. Diego's fieldwork in this project has entailed living in Mexico for several months interviewing, playing with and learning from traditional musicians and dancers in a politically complex part of the country. Besides this, throughout his academic career Diego has conducted fieldwork in several other places in Mexico, as well as in parts of Peru, France and the USA.

Mike Crang is Professor of Geography at Durham University. He has worked across a range of topics from the role of media and technology in everyday life, to cultures of tourism, to heritage studies through to waste and disposal. Across these, he has conducted a range of studies that came off and proposed and planned for many more that did not, in turn producing a range of writings, which are but a subset of the things he wanted to write.

Christoph Doppelhofer is currently pursuing his interdisciplinary Leverhulme Doctoral Scholarship in Visual Culture at the Centre for Visual Arts and Culture and the Department of Geography, Durham University, researching the role of modern visual media in reshaping the perceptions, identities and meanings of cultural heritage landscapes. Thereby, he uses the impact of the HBO fantasy series Game of Thrones on its filming locations as a case study. He further holds a BA in Classical Archaeology from the University of Vienna and an MA in International Cultural Heritage Management from Durham University. These degrees led him to research and community excavations in England, Austria, Italy, Turkey and Egypt as well as work as a cultural travel guide where he expanded his experience in the heritage, museum and cultural tourism sectors. His research interests include (visual) representations and uses of the past in contemporary social and political discourses, the impact of popular culture on heritage, destruction and reconstruction of heritage as well as public engagement and dissemination of academic research. Christoph is also a visiting lecturer for Durham University's MA in International Cultural Heritage Management, where he teaches on map- and app-based heritage trails and reconstruction of destroyed heritage for post-war regeneration.

Zahra Hussain is a human geographer and an architect who leads the organisation Laajverd in Pakistan at which she developed the Laajverd Visiting School Project which is an interdisciplinary and cross-curricular field school based in sites of conflicts, crisis and disasters bringing together academics, local communities and professionals for a dialogue on shared futures. LVS is a task-based, challenge-led process of actors learning from locals and each other across backgrounds and interests producing varied outputs ranging from publications to community museums.

Yim Ming Connie Kwong is currently a postdoctoral fellow of a BMBF-funded interdisciplinary and transdisciplinary project at Leibniz Centre for Tropical Marine Research in Germany. She is also a Member of the GCRF-funded Silk Route Cultural Heritage Network. She obtained her BSocSc. (Geography and Sociology) and MPhil (Geography) from the University of Hong Kong. Prior to her PhD in Durham University, she was a Research Assistant on a project on walkability which adopted integrated methods (survey, walking, computer and clinical tests), and on another project on the social impacts of day-tripping and movement of commodities and tourists at the Hong Kong-Mainland China border (using business activity mapping and personal interviews). Her PhD studies was funded by various funding sources, including departmental stipend, scholarships in Hong Kong, Global Citizenship Programme (GCP) Scholarship and Ferguson Trust Fund, and fieldwork supported by a scholarship awarded by Ustinov College. She had experiences of coordinating volunteering trips to Cambodia. Whilst at Ustinov College (2015–2018) she coordinated various academic and social events to create spaces for dialogue on issues such as identity, mobility, refugee and global citizenship. Her research interests include cultures, identities and practices; responsibilities and values; tourism and community (co-)development.

Olivia Mason is currently an ESRC Postdoctoral Fellow at Newcastle University, where she also worked as a fixed-term lecturer in Political Geography. She recently completed an ESRC-funded PhD in Geography from the Geography Department at Durham University. Her doctoral research developed an ethnographic study of a long-distance walking trail — the JordanTrail — to explore how contemporary post-colonial configurations of nationhood and statehood are constructed and experienced through everyday sites of cultural production. Olivia's research therefore sits at the intersections of cultural and political geography and explores the politics of everyday cultural practices and lived experiences in the Middle East. During her PhD, she received a Council for British Research in the Levant Language Scholarship and ESRC funding to undertake Arabic language training and also spent time as a volunteer for the Jordan Trail Association. Prior to her PhD, she gained a Master of Research at Glasgow University undertaking a project exploring the feminist geopolitics of Palestinian bloggers. Her BA in Geography at Newcastle University investigated post-conflict tourism in Bosnia for which she received funding from the Sonia Stonehouse Expedition Fund. She has also worked as a research assistant on two Medical Research Council funded projects at Glasgow University.

Ludovico Rella graduated in International Studies in Florence, Italy, and earned a Master in Global Studies in Lund, Sweden, with a background in Political Science. He is currently undertaking his doctoral research in the Department of Geography at Durham University, UK, funded by the Economic and Social Research Council's Northern Ireland and North East Doctoral Training Partnerships. His research is supervised by Prof Paul Langley (Durham), Prof Ben Anderson (Durham) and Dr James Ash (Newcastle). His research lies at the intersection between the economic geography of money and finance, digital geography, social theory of money, cultural and political economy, and science and technology studies of infrastructures. It focuses on money digitalisation, blockchain technologies, and cross-border payment infrastructures, and it develops an infrastructural conceptualisation of money to capture the spatial and political dimension of money's circulation. He authored the Blockchain entry for the second edition of the *International Encyclopedia of HumanGeography*, published in December 2019, and he published in the *Journal of Cultural Economy*, in *Frontiers in Blockchain* and in *Esercizi Filosofici*.

Zuriatunfadzliah Sahdan is a lecturer in human geography at Universiti Pendidikan Sultan Idris, Malaysia. Dr Sahdan received her PhD from Durham University. Her PhD was funded by the Ministry of Education Malaysia and Universiti Pendidikan Sultan Idris. Her research focuses on postcolonialism, space and cultures of domestic violence in Malaysia, using participatory methods and storytelling. She co-authored 'Trauma, Gender and Space: Insights from Bangladesh, Malaysia and the UK' (2020) appeared in *Routledge Handbook of Gender and Feminist Geographies*. Her recent work 'Demonic Possession: Narratives of Domestic Abuse and Trauma in Malaysia' will be published in *Transactions of the Institute of British Geographers*.

Lucy Smout Szablewska is currently a postdoctoral researcher in Durham University. She has a PhD in Human Geography from Durham University which was awarded in 2019 after 7 years of part-time research. Her thesis examined the linked lives of Polish worker-carers in North East England and their families in Poland, and makes the case for understanding the significance of population ageing and valuing the role of unpaid care labour in migration research. Her first degree was in Social Anthropology, followed by a career in English language teaching, women's entrepreneurship and communications. Her work took her to Greece, Zimbabwe and North East England, where she has lived for 25 years. Her PhD was funded through part-time freelance work, a legacy from her late parents-in-law, and grants from postgraduate fieldwork and conference funding initiatives in the Geography Dept and Faculty of Social Sciences and Health at Durham University, and the Polonia Aid Foundation Trust.

James D. Todd has submitted a PhD in Human Geography at Durham University. James' PhD research explores the everyday life experiences of young trans people in the UK. Funded through an ESRC Doctoral Scholarship, his thesis draws on participatory methodologies to explore the lived and everyday realities of having a

young trans body in differing spaces and times. James collaborates with Gendered Intelligence, a community interest group supporting trans young people in the UK and hopes to encourage an increase in the presence and voice of trans youth in geographical and social science research. James holds an MArts degree in Human Geography, also from Durham University.

Divya Praful Tolia-Kelly is a Professor in Geography and Heritage Studies at the Global Studies in Sussex University. Divya was born in Kenya and arrived in the UK in 1973. Her research is focused on postcolonial and anti-racist approaches to cultural geographies, migration, landscape, memory, heritage, visual culture and material culture using participatory methodologies. Divya has published several articles on the theory and politics of 'race' in relation to these themes, including decolonising the academy, ethnocentrism in cultural politics and more recently on decolonising museums and race, affect and the Anthropocene. Her books published include a monograph *Landscape, Race and Memory* (2010); *Visuality/Materiality: Objects, Images and Practices* (edited with Gillian Rose) and the co-edited volume (with Steve Watson and Emma Waterton) entitled *Heritage, Affect and Emotion: Politics, Practices and Infrastructures* (2016). Divya is currently Series Editor (with Emma Waterton) of the Routledge Book Series *Critical Studies in Heritage, Emotion and Affect* (https://www.routledge.com/Critical-Studies-in-Heritage-Emotion-and-Affect/book-series/CSHEA). Currently, Divya is writing a research monograph entitled *An Archaeology of Race at the Museum* (contracted with Routledge). Divya has a keen commitment to praxis and thus is committed to extending her research on 'race' to the everyday environment of HE: working towards a truly universally inclusive place to work, research and write. Divya is thus committed to supporting the *decolonising the curriculum* and *why isn't my professor black?* campaigns through her equality and diversity work in situ.

Yu-Shan Tseng is a postdoctoral researcher at the Centre for Consumer Society Research/Urban Institute, University of Helsinki. She is currently exploring the impacts of two different types of digital platforms (food delivery platforms/platforms for participatory budgeting) on urban (in)justice and democracy in Helsinki City. She completed her PhD in Human Geography at Durham University, supervised by Prof Colin McFarlane and Dr Andrés Ayala-Luque. Funded by the Taiwanese Government, her PhD research investigated and compared the politics and implications of Open Source Software (OSS) enabled public participation platforms — Decide Madrid and vTaiwan-Pol.is. She worked with Madrid City Council and the Taiwanese government (the Cabinet Office) during the fieldwork. Before her PhD research, she completed an MSc at Urban Regeneration at Bartlett School of Planning at University College of London (UCL) (2013–2014) and interned at the Department of Science, Technology, Engineering and Public Policy (STEaPP) at UCL for the two UN-funded projects on urban resilience and leaderships (2014–2015).

Abbreviations

DHS	Demographic and Health Surveys
DPPPs	Digital Platforms for Political Participation
ECR	Early Career Researcher
EU	European Union
FinTech	Financial Technologies
GOT	Game of Thrones
JTA	Jordan Trail Association
NGO	Non-governmental Organisation
TBW	The Bucket Wish
UK	United Kingdom
USAID	United States Agency for International Development
WAO	Women's Aid Organization

Chapter 1
Introduction: Postgraduate Experiences of Navigating the Field

Mildred Oiza Ajebon, Diego Astorga de Ita, and Yim Ming Connie Kwong

Abstract Fieldwork is a central part of the research process and navigating 'the field' can be challenging for researchers, especially for postgraduate students with limited experience. This volume is not a recipe book for conducting fieldwork or social research, rather it presents critical reflections on a range of conceptual, methodological and practical issues permeating the social research process. Whilst there are general perspectives from institutions and academic disciplines on how the field may be conceptualised and what doing fieldwork may involve, this chapter emphasises the need to acknowledge the fluid, ever evolving and interlocking nature of the postgraduate fieldwork research process. It also stresses a need to project the voices of early career researchers which remain under-represented in the academic literature on the subject matter of fieldwork. These voices are both celebrating endeavours in the field and admitting difficulties and messiness. It challenges the traditional conception that these open and honest reflections are 'feminine' or 'weak'.

1.1 Learning from Postgraduate Fieldwork Experiences

Fieldwork is a central part of research, especially in social sciences. Practically, everybody in human geography, sociology, anthropology, and other kindred disciplines undertakes diverse activities in the field to try and understand any given aspect of reality. The field comes in many different shapes and sizes, from emplaced

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experiences happening over long periods of time to research done in archives, ephemeral events or online spaces. Whatever 'the field' may look like, navigating it can be a challenging process for researchers, particularly for postgraduate students with limited experience. Very often, postgraduate students reference research methods and methodology books written by more established researchers, or are told what to do from the perspective of their supervisors. However, every project is different and what constitutes 'the field' differs accordingly. Each experience of fieldwork sends us off into new and strange circumstances, and so, navigating the field often feels less like navigating a well-made ship or a hot air balloon, and more like navigating a house in a tornado, and one soon discovers that we're not in Kansas anymore.

Whilst there are general guidelines on how to conduct fieldwork and what constitutes an ethical research practice, the practicalities of doing fieldwork change and are unique to each individual researcher. Hence, different researchers come across different ethical and practical challenges for which there are no simple solutions or clear-cut procedures. Even researchers with previous experiences of fieldwork must deal with new challenges when doing new research, and it seldom is as simple as following a yellow brick road. Existing reflective volumes tend to focus on doing fieldwork in developing contexts (e.g. Lunn 2014; Robson and Willis 1997). This might easily result in a relatively restricted framework of methodology, procedurалised and institutionalised ethical review processes, and reduced preparedness of the postgraduate researchers. Supervisors might not always have empirical experiences relevant to the particulars of their supervisees' projects, and so, postgraduate students must often navigate the politics of research and their own paths in relation to their own specific contexts. This initial fieldwork experience is likely the time during which Early Career Researchers (ECRs) such as PhD students and postdoctoral fellows start to find and establish their style of conducting research. Our edited volume specifically speaks to these gaps by providing a platform for ECRs to reflect on their fieldwork experiences in various geographical contexts and forms of field.

The intention of this book is to provide a safe space for ECRs not only to demonstrate their intellectual abilities as typical of academic writings, but also to be open about the hidden labour of doing postgraduate fieldwork. This openness encourages the authors to disclose moments of self-doubt, rejection and anxiety as well as the thrills and perils of fieldwork, leading to reflections on the learning processes of the PhD, admissions of mistakes, and celebrations of small victories. Through these reflections, this book makes an important point that there is a need for researchers to document the 'real story' behind the fieldwork process. We ought to be able to see the man behind the curtain, not only the projection of the Wonderful Wizard of Oz; for the field is never just wonderful, it also is strange and changing, and we move through it accordingly. This volume expresses the ways in which each contributor navigates specific ethical and practical conundrums encountered in the field in order to produce quality research data. In so doing, it demonstrates that admitting and documenting the difficulties and messiness of the research process is a necessary endeavour. There is a tendency in academia to shy away from the messy, complicated, and emotional aspects of research; these characteristics — traditionally

thought of as 'feminine' — are seen as weaknesses. We do not think that admitting doubt and reflecting upon our feelings when in the field translates into intellectual weakness; rather, there is a long neglected academic value in sharing experiences, complex problems, dilemmas, frustrations, little but incremental victories and joys with colleagues across all career stages. We emphasise repeatedly that reflections and retrospective self-critique is not a weakness, but a strength. We hope this approach will encourage and help other researchers to create and navigate their own unique research pathways in manners that are more realistic. It is also important to point out that this book is not meant to serve as a manual for doing fieldwork; but rather, this is meant to be a place of reflection and mutual learning for ECRs.

Writing about the experience of being a postgraduate researcher, and mostly from the perspective of postgraduate researchers, this book aims to be a documentation of fieldwork experiences, gathering critical reflections on 'the field' from a wide range of ECRs. The issues in this book go from the process of identifying and understanding the field (what *is* the field?) and navigating life in the field, to practical difficulties facing new researchers in the field and concerns in using fieldwork data. This then shows a different set of methodological considerations in relation to ethics and practices, highlighting that the traditionally adopted framework of research design could be broadened with the postgraduate fieldwork experience as a reference. We hope readers across all levels of experience will find the ideas shared here useful for thinking through and planning their fieldwork. Looking at how postgraduate researchers make sense of these issues and what kind of decisions they make in specific circumstances helps to reveal broader questions and matters of concern such as institutional practices and constraints, especially in supporting fieldwork and writing, supervisor-supervisee dynamics, and hidden agendas behind funding.

This book also aims to specifically discuss issues of conceptualisation of 'the field', ethics, positionality and logistics arising from recent fieldwork experiences. Its scope is distinctive in the sense that it is neither individual journal articles on the methodology of one or a few studies, nor a step-by-step guide for conducting research. The book is thus not structured based on research approaches such as qualitative, quantitative, and mixed methods. Neither is it an edited volume with a geographical focus (e.g. Global South). Rather, this book offers a broad collection of writing with a focus on conceptual, practical, and contextual reflection on 'the field'. Thus, across the chapters, the contributors write about diverse themes such as the ontologies of being-in-the-field, the empirical issues that arise around ethics, positionality and practical challenges that they have faced, and questions of whether we can actually leave the field behind and how this might happen.

In the existing academic literature, the in-ness, out-ness and in-between-ness of doing fieldwork have been widely explored, but this book is distinctive in three ways. First, it is explicitly written through the perspectives of postgraduate researchers who have recently experienced fieldwork. Secondly, throughout the volume we recognise and reflect upon the emerging nature of 'the field' and of the many permutations this entails, including some 'field-less' spaces such as digital platforms, which are becoming more and more common as our world and social interactions

become increasingly digital. Lastly, contributors of this volume hope that sharing their experiences, difficulties and concerns with other researchers at different career stages and across different disciplines will help start a conversation and widen the fundamental discussion on ethics, power and privilege of a researcher through these 'beginner-level' considerations. While the list of contributors in this volume might seem homogeneous at first sight — we are all human geography postgraduate researchers from Durham University, present or past — this collection of contributors and writing is a diverse and interdisciplinary one. Furthermore, human geography is a wide and welcoming discipline, which allows for questions to be raised regarding the social research process. It deals with the study of people and their communities, cultures, conflicts, economies, health, politics, and relationships and interactions with the environment across time and space. This leads to various focuses on fields of study such as cultural, social, political, economic, development, health, urban and population geography, as well as their numerous sub-fields. This matrix of fields of study means different ways of defining and approaching 'the field', as well as a broad set of questions on ethics, positionality, power, cultural practices and thus methods used when navigating the field. More specifically, human geography at Durham University has a diverse community of postgraduate researchers with a wide range of research topics in extensive geographical locations and cultural contexts. Having said that, the experiences of the contributors are far beyond the scope of human geography, as anybody reading their biographies will quickly realise. They have a significantly diverse background in terms of their previous studies, prior academic and working experiences, supervisory teams (cross-departmental or cross-institutional), sources of funding, fieldwork sites, and fieldwork approaches. This edited volume epitomises this diversity and richness as well as the expansive challenges that arise from it. Because of the above, we hope that the issues and ideas raised in this book will find room in disciplines beyond geography, and that the writings presented here will be useful for our peers in wider social sciences and the humanities.

This edited volume was fueled by and builds upon existing literatures in human geography and, more broadly speaking, social sciences and humanities that reflect upon the role of the researcher in the field, and the practices that take place in it. In this sense, the works on experience and positionality of feminist geographers like England (1994), Rose (1997), and Pink (2008, 2009) come to mind as relevant signposts, particularly given the emphasis that this book makes on ECRs' experiences of and in 'the field'. Most contributors reflect upon their positions and provide up-close-and-personal accounts of 'the field' and of the anxieties that ECRs face when working on their own. The works of Smith (1999), Krotz (1997), and Puri and Castillo (2016) are also a relevant part of the framework of this collection of texts, given that it includes reflections of ethnographic work carried out in places beyond 'the West', by scholars hailing from diverse regions. There are chapters that talk of going into 'the field' in Bangladesh, Hong Kong, Mexico, Nigeria, Pakistan and Taiwan when these places are 'home', and that engage with the questions that arise from doing fieldwork in places that are supposedly familiar. Additionally, texts that

reflect upon arriving, being in, and leaving the field — both practically, reflexively, and in more theoretical or philosophical terms — inform the ideas put forward in this volume. Hence, works like Robson and Willis (1997), Mullings (1999), Skelton (2001), Crang and Cook (2007), Iversen (2009), Lunn (2014) and Scheyvens (2014) are part of the literature with which the volume has engaged.

The ethos of this book has been influenced by texts that reflect upon writing fieldwork (Stea 1969; Clifford and Marcus 1986; Taussig 2010). Following this line of thought, this volume provides a space for creative expressions and for crafting different styles of texts. The 13 chapters collected here are not all standardised or conventional academic papers laden with a scholarly tone, as one can see different styles across the book; Socratic dialogues and pieces of poetry are interspersed with critical reflections, narratives and academic references. In so doing, we hope this book is more welcoming and accessible for a wider audience, while maintaining robust conceptual reflections, thus moving away from what Stea called 'the era of the academic ostrich' (1969: 1).

As we have previously mentioned, the volume at hand adds the voices of ECRs to current conversations within social sciences. It is precisely because this volume is written and edited by postgraduate researchers currently enrolled in or recently graduated from university, that it helps to fill this gap in the literature. The reflections of postgraduates regarding going into 'the field' and the experiences that develop in it are central to this book. It further adds to the literature by documenting research experiences on emerging topics and revisiting long-debated issues and methods of conducting fieldwork. Novel forms of fieldwork that look at new spaces and methods are explored, such as digital platforms, temporary communities, multi-sited fieldwork, and other innovative methods that build upon conventional approaches in social sciences. Reflexivity is essential on long-standing issues including ethics, access, positionality and power relations, and on conventional and creative research methods; the 13 chapters in this book offer fresh perspectives from recent and current postgraduates in conducting social research.

1.2 Plan of the Present Work

This book is structured into three sections in addition to this chapter and a conclusion. The first part of this volume includes three empirically rich chapters that interrogate the notion of 'the field' by engaging with fluid conceptualisations that include boundless spaces, temporary communities, and moving fields. The second section focuses on reflections and practicalities of doing fieldwork; it considers a wide range of ethical dilemmas that emerge when conducting research in various kinds of spaces. Through the five chapters that compose it, this second section presents the experiences of core 'data gathering' activities in different fields. Lastly, the third section explores the question of writing through three chapters that present issues researchers face after leaving the field.

1.2.1 Part I: The Field

In Chapter 2, a moving field emerges amidst questions of access, mobility and immobility, as Mason shares experiences of denied access, deportation, and their emotional impacts. It explores the entanglements of moving in the field and of moving as a method, looking at walking methodologically, considering the practicalities and challenges of this approach, and thinking through questions of researcher privilege.

Chapter 3 discusses the methodological approach employed for identifying and actively engaging with time-specific, post-place communities. In it, Kwong offers some critical reflections on the intensive and extensive efforts that are required for identifying, gaining access to, and developing rapport within temporarily formed communities of volunteer tourists for ethnographic research. It demonstrates that the spatial and temporal dimensions of researching temporary communities on the move may further blur the scope of the field and the kinds of relationship entailed. The temporary researcher-researched relationship can be transformed into long-lasting social relationship, or even friendship as a form of reciprocity when different temporal scales are considered.

Expanding upon the idea of the field as a fluid network of people, in Chapter 4, Rella critically examines and challenges the notion of 'neat divisions' between 'virtual' and 'real' fieldwork spaces. It demonstrates the potentialities for research that emerge from an interplay between 'online' and 'offline' spaces of data collection while looking at networks of geographically dispersed and fast-changing digital economies. It argues for the need to take seriously the claims made by digital and multi-sited ethnographers that the field site is as much produced as it is discovered, and its production is as much a theoretical as it is a logistical effort. In assembling a multi-sited hybrid field, between online and offline places and diverse locations, one must be attentive to the challenges of this methodology, regarding mobility, timing, and limitations. Rella writes of these things reflexively and through the idea of a 'scavenging ethnographer', attending to questions of access or lack of access as a texture rather than as an already given clear-cut border.

1.2.2 Part II: In the Field: Ethics, Practice and Positionality

The second section of this volume challenges the simplistic assumptions about ethics, researcher positionality, power and responsibility in research encounters. It begins with Chapter 5 where Doppelhofer and Todd talk about an often taken-for-granted, undervalued and underrepresented empirical aspect of the fieldwork process: participant recruitment. They stage a conversation about their experiences of participant recruitment as two postgraduates and talk through anxieties, discussing participant recruitment and engagement in two different contexts: one recruiting on an ad-hoc basis in touristic spaces, the other following a more conventional and

long-term collaboration where trust and provision of 'safe spaces' are necessary components of recruitment. The chapter stages a *Socratic Dialogue* as an innovative presentation of theoretical, practical, and ethical challenges on approaching participatory research methods. It offers interdisciplinary perspectives and examples of participant recruitment across different research contexts.

Chapter 6 furthers the discussion of the potential of deploying participatory methods for researching the often unexplained, undiscovered, and contested lives of working children in Bangladesh. Through this chapter, Ahmed projects the voices of subaltern children that are often ignored in research. He reflects on the ethical guidelines in children's research, which provide a framework for protecting children from harm and exploitation. At the same time, he argues that, whilst upholding ethical guidelines in children's research is paramount for opening debates for reorienting, localising and addressing children's issues, ethical research with marginalised children needs to be examined and understood as a dynamic discourse of socio-economic, cultural and political practices and peculiarities.

Chapter 7 examines the 'otherness' of research and the central nature of alterity to ethnography. Through this text, Astorga de Ita posits two poles of ethnographic research, the totalising and the relational, and proposes that the latter can be achieved when ethnography is done poetically. The author looks at poetry and draws upon methods that include music and photography to explore the idea of relation. Through this he argues that ethnographic alterity places researchers in an 'in-between', muddling the categories of home and foreign, self and other, even of real and oneiric. The chapter explores the self-consciousness and tension involved in navigating alterity and proposes that, like life, ethnographic research is an angsty mess, but also kind of fun.

In Chapter 8, Tseng reflects on accessing, immersing into and working in-between two governmental institutions made up of a network of people with a shared identity and the associated ethical issues of gaining access and trust. It highlights how the dual positionality of the researcher as an intern and a researcher facilitates ethnographic practices in order to compare two digitally mediated policy-making platforms in Madrid City Council and the Taiwanese government. The chapter discusses the challenges of being an intern and a researcher, navigating language barriers, and dealing with tensions between the researcher and political elites during participatory observation and interviews.

Chapter 9 demonstrates that in researching child health issues with mothers of young children, the researcher's identity, particularly vis-à-vis motherhood, may have implications beyond data gathering. Through the chapter, Ajebon considers how identity and positionality shape, not only the object of research enquiry, data generation and interpretation, but also representation. The chapter demonstrates that the field is a site where the professional and personal identities of researchers converge. It is a leaky space in which relationships with participants shape and are shaped by the relationships with those (children and research assistants) who accompany the researcher. It highlights the need to make explicit the everyday struggles, and dilemmas of motherhood/parenthood for the PhD research process. In doing so, it challenges the notion of neat boundaries between being in and out of

the field, home and foreign, insider and outsider, personal life and academic practice. Ajebon makes an important point that researchers often must negotiate subtle power dynamics, as fluid and shifting insider-outsider statuses emerge even when doing fieldwork at 'home'. But home may not be really home, especially in multicultural and diverse settings like Nigeria.

1.2.3 Part III: Writing the Field

The last section of this volume starts with a reflexive account of secondary trauma resulting from encountering abused women. In Chapter 10, Sahdan writes about the emotional and psychological impacts that some postgraduate students face as they continue to deal with traumatic research contents beyond the period of actual data collection in the field. Secondary trauma is an unintended health consequence of prolonged engagement with violent content and occurs because of empathy and prolonged engagement with participants' stories of violence. Sahdan points out that although flashbacks are important indications of secondary trauma, it may take a long time for ECRs to detect it due to a lack of training for researchers working with traumatic contents, and the inability of female researchers in particular to identify, express and seek timely help from secondary trauma.

Chapter 11 highlights another often taken-for-granted issue in the social science postgraduate research process: academic writing. Although there is a growing body of work on ways of supporting doctoral researchers to acquire scholarly writing skills, including useful templates on how to structure the all-important argument, this work could be enriched by research into how doctoral researchers make sense of their writing journeys, and the strategies and resources they use to demystify unspoken rules. In this chapter, Smout Szablewska discusses her personal experience of acquiring the academic skills required for writing up data collected in 'the field'. She makes a case for the need to render explicit the tacit underlying principles of social scientific thinking in order to equip researchers with a thinking 'toolkit'. This skill is particularly relevant to international students and mature doctoral researchers who may not have undertaken training in social research.

Chapter 12 revisits the questions raised earlier in Part I of this book, as Hussain thinks about how the field may be defined through an open inquiry framework. An open inquiry creates the space for understanding 'the field' in multiple ways and lets the field itself decide how research is conducted. Hussain shows how the researcher too does not have a fixed position but must assume more than one position in order to meet the demands of the field — that ties in with the idea of ethics. The chapter highlights the need to conceptualise the field as a dynamic entity both in space and in time rather than a static location, and thus it bears the power to affect the researcher's engagement in and with the field. It argues that the 'field' should not only be understood as the material locations visited during fieldwork, but also the overall experience of imagining, remembering, being in and reflecting upon the field. This chapter closes the section as well as the overall reflection, which leaves room and

hope for further discussions to emerge on the multiple themes raised throughout this book.

Lastly, Chapter 13 presents a summary of how various authors have conceptualised the field and navigated complex ethical challenges which emerged. It clearly highlights the anxiety and emotional uneasiness associated with the social research process especially for ECRs. It argues that fostering a research tradition that reflects on tensions, self-doubts, failures, changed directions and successes with openness is important to encourage both existing and future researchers to navigate their social research paths more realistically.

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Part I
The Field

Chapter 2
Moving Across the Field: Researcher Mobilities and Immobilities During International Fieldwork

Olivia Mason

Abstract In this chapter I explore the entangled relationship between researcher mobility and immobility while undertaking international fieldwork by making two arguments. First, I contend that the movements of researchers are increasingly being challenged in many locations as researchers are denied access and have their mobilities curtailed. Second, I argue that researcher mobilities must also be considered in heterogenous and nuanced ways and brought into conversation with research in critical mobility studies. As a result, researchers need to consider more carefully the means by which certain movements and certain moving human bodies have been privileged. Researchers must think more critically about their own entangled mobilities in the process of doing research and how this in turn shapes the field site.

This chapter draws upon my own fieldwork experiences in Jordan and Israel-Palestine. I reflect on my experience of being denied access to Israel-Palestine. I explore how this had intimate and emotional impacts on myself and how this impacted me as a postgraduate researcher embarking on my first sustained period of overseas fieldwork. I reflect on the move of my field site to Jordan and explore the tactics researchers are using to research sites they cannot access, illustrating the fluidity of the field in the context of geopolitical conflict. I then explore how I developed a new project, centered around mobility and walking as method in Jordan, and how this related to the immobility I experienced. In summation this chapter discusses access to the field site, embodied emotions while researching, and navigating my own and others' mobility and immobility during international fieldwork.

Keywords Jordan · Mobility · Access · Emotion · Walking methods

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2.1 Denied Entry: The Non-moving Researcher

> Your entry to Israel has today been denied. Could you please follow my colleague? What followed was a lengthy search of my belongings, confiscation of my electronic items, a ride in an armoured vehicle to a detention centre in the airport ground of Ben Gurion Airport, an overnight stay in a cell, an escort in another armoured vehicle to a plane, a document signed by the pilot promising to transport me off Israeli airspace, and an escort back off the plane at Istanbul airport. It was only once I was safely in the terminal building in Istanbul that my passport was handed back to me (Field notes: 10/01/16).

The experience I detailed above occurred while I was conducting fieldwork for my PhD. I begin here with a period of immobility I faced in my research to then explore how this impacted my future research plans and methods. At the moment in which I was denied entry I had been in Palestine for 3 months studying Arabic at Bethlehem University and preparing for a year of fieldwork by making contacts and acquainting myself with my research sites. I was denied entry at the Jordan-Israeli border after returning from a trip to Jordan to renew my 3-month tourist visa,[1] and subsequently at Ben Gurion Airport when I attempted to re-enter a second time. The experience above details the moment of denied entry in which all my preparation work was undone in an hour-long interrogation. I subsequently returned to Durham University, where I was undertaking my PhD, with a sense of deep failure and unease. I felt failure that I had not managed to enter the field successfully and had not been better prepared to deal with the interrogation. I felt unease about what would happen next with my PhD.

I took a 3-month unpaid break from my PhD in order to emotionally recover but also work out what to do next. For most postgraduates embarking on a PhD, their project is one they feel is important. My own research, an investigation into tourism practices in Israel-Palestine was one that I felt had methodological importance in its prolonged ethnographic approach. Indeed, Leuenberger (2015) argues that conducting grounded research in politically and socially sensitive unstable contexts offers more in-depth insights into frequently mis-represented social groups. Harker (2011) argues too that a failure to capture everyday accounts arising from Palestine, frequently represents it as a place only associated with violence. To continue with a project in Palestine would have involved a remote project. At times this felt possible. Although I was physically separated from Palestine, in some ways I could still go there: I could WhatsApp friends, skype families I had lived with and thus make contacts remotely. However, ultimately it felt painful to continue a project that did not engage with the ethnographic approach I felt was important and a project that would be second best.

While considering what direction my new project would take, I heard numerous accounts of researchers being denied entry to Israel and other states, or state institutions in the UK and U.S.A. My experience of denied entry was therefore part of

[1] At the time of this fieldwork, late summer 2015, it was common and legal to undertake fieldwork in Palestine with a 3-month Israeli tourist visa — leaving and re-entering to renew it.

wider discussions about research and researcher access.[2] There are discussions around risk assessments, health and safety, and restrictions put on researchers by their own institutions or else by states or institutions they intend to research. These issues are aspects of the research process many PhD researchers, including myself, are unprepared for. Indeed, the process of navigating risk assessments, risk management guidance and institutional restrictions is often unchartered territory for new researchers (see Sultana 2007). The experience of being denied entry is one many researchers are unprepared for, especially its emotional impacts. The emotional impacts of fieldwork, feminist scholars argue, are often left out of geography (Caretta and Jokinen 2017). Caretta and Jokinen (2017) argue that female researchers especially are often placed in vulnerable positions during fieldwork and are often given no support institutionally or otherwise. They suggest more support be given to postgraduate students who are at the 'lowest rung of the academic ladder' to support them emotionally but also open up discussions about reflexivity and privilege in the field (Ibid.: 276). Such arguments follow a long history of feminist work arguing for more transparency about the role of emotions in fieldwork and how drawing attention to emotions can open up better conversations around positionality, ethics and power (Bondi 2005; Woon 2013). To return to my denied entry, my experience illustrates the need to understand the emotional impacts of research and how these link to institutional guidance, researcher mobility, and the politics of place. The Palestinian academic Ghazi-Walid Falah (2007) has written of his experience of interrogation at the border. He writes:

> Based on clear evidence I encountered in interrogation, I am certain my arrest was political indeed part of the politics of knowledge production and its repression and has no relevance as a 'matter of state security', as the Israeli authorities sought in the summer of 2006 to portray and propagandize it (Falah 2007: 588).

The experiences of Falah, who now resides in Canada, resulted in his imprisonment in an Israeli jail and subjection to severe interrogation, and therefore is not an experience I equate to my own. However, in both our cases it was what Falah (2007: 587) calls 'the politics of doing geography' that resulted in interrogation, intimidation, and, for me, deportation. Denial of entry at a border thus becomes a violent encounter as the researcher is identified as a threat and treated as such. It also raises questions about the state's ability to use violence to deny entry and the positions researchers must place themselves on in order to gain access. These are questions that have deep emotional impacts. For instance, the reason for my own denied entry was stated as 'threat to public order' — an arbitrary and unsubstantiated reason. It recast me as a threat that was at odds with the research I set out to do and my identity as a researcher. Belcher and Martin (2013), through their experiences of gaining access to their respective research projects, argue that access or a lack of access to state agencies is not always evidence of state liberal logics nor it is always

[2] Two years after I was denied entry to Israel, a Durham PhD student was detained for several months in the U.A.E. An incident that received a lot of media interest, intervention from the UK Government and promoted several discussion groups at Durham.

intentional. My own experience of being denied entry also illustrates what Sultana (2007: 374) writes as the disjuncture between 'aspects of everyday behaviour in the field and the University's institutional frameworks'. Institutional frameworks around fieldwork, including the risk assessment system, are not often able to deal with very different and context specific fieldwork experiences.

Part of the shock of my denied entry was my own privileged sense of mobility that arises from my subject positions and made me intimately and personally aware of entangled mobilities and mobile privileges. I am white-British and being the holder of a British passport gains me access to 156 states world-wide visa-free. This is in stark contrast to other nations: for instance, Afghanistan's passport, which has the lowest rank on the passport index, affords its holder access to just five states visa-free.[3] My passport gives me the privilege of a very mobile existence. As researchers, negotiating our own privilege in accessing our field sites is crucial but also acknowledging researcher privilege is far from homogenous. For instance, Griffiths (2017: 2) reflects on his own class history to argue for a 'more heterogeneous conceptualisation of Western postcoloniality that accounts for the varied experiences of the British working classes.' In other words, class and gender can reconfigure power imbalances between researcher and those researched.

I argue that understanding and thinking more critically about research mobility is important because it is a crucial part of how we conduct our research (or indeed are able to conduct relative to certain embodied privileges in differing spatial locations). I decided to choose a new research site and moved my project to Jordan, an option enabled by my relative priviledged mobility. After being denied entry to Israel at the end of 2015, I spent time in Jordan and developed contacts there. I returned then to the UK and spent six anxious months deciding what to do. This was a period of time in which I was meant to be in the middle of my fieldwork in Palestine but instead I had been abruptly sent back to the UK to plan a completely new project. After much deliberation, I travelled to Jordan in the summer of 2016 to undertake an Arabic language scholarship and also to build this new project. In Jordan I met other researchers who had also been denied access to Israel or else told by their universities that it was not safe to research (in) the place they originally intended. I was surrounded by researchers and Arabic language students who had come to Jordan because they could not travel to Egypt, Iraq, Iran, Syria or Libya. Researchers who had lived in Jordan for several decades, told me that until a few years ago, Jordan was a place few researchers went, an unexciting location — especially for social scientists. Yet as numerous other locations in the Middle East became increasingly inaccessible, researchers in recent years were turning their attention to Jordan to both research in and use as a base to travel from and across other locations.

Jordan, as a base for researchers to access and explore other locations in the Middle East, further illustrated the need to understand how research mobilities and immobilities are transforming how we do research and where we do research.

[3] Information retrieved from the Passport Index: https://www.passportindex.org/byRank.php.

Research sites are not static but transient and open or close depending on different political conditions at the time. This is also true of researcher positionality and factors such as race, nationality, class and gender that can at times help to enable access and at other times make access impossible. My new research focus and site, as I detail in the following section, was focused entirely around the idea of mobility, as a result of my own denied entry. I moved in sum from immobility to mobility through a new project about walking in Jordan. I argue, therefore, that we must consider questions of mobility and access in relationship to our own experiences while undertaking research but also in relation to the research we do, the places we research, and those we research.

2.2 Researching Walking: The Moving Researcher

When I returned to Jordan in the summer of 2016 to undertake the Arabic language course, I looked for ways to walk, because walking is an activity that improves my mental health and enables me to think clearly. I was fortunate to grow up in a house with a garden and I spent much of my childhood walking in fields behind my garden and at both high school and university was part of walking and climbing clubs. My relationship with walking is therefore linked to a British history of walking and climbing clubs and my own privileged access to outdoor space. It was important for me therefore to explore walking in Jordan by paying attention to how my emotional experiences of denied entry and my personal history of walking shaped my understanding. I chose to research walking as something that would help me to recover emotionally and I soon realised it was an important way to understand the embodied and emotional experiences of mobility of those I researched. The 3 months I spent in Palestine had illustrated to me the connection between walking, mobility, and politics. Shehadeh's (2008) 'Walks in Palestine' beautifully illustrates how walking in Palestine connects him to his much-loved landscape, but also highlights the loss of this landscape to Israeli occupation.

Mobility and immobility have been explored by researchers as central to understanding everyday life in Palestine. Ramos (2015: 108) uses Palestinian theatre to explore the 'dynamics of immobility that locate Palestine in a marginal position, marked by the exclusion from the patterns of global mobility, the impact of the Israeli occupation and the disruption of Palestinian socio-political life.' Mobility in Palestine is often a key marker of difference and identity, and a relational understanding of 'relative im/mobilities' can emphasize that the mobility of some is always in relation to the immobility of another (Harker 2009). Similarly, Griffiths and Repo's (2018) work on checkpoints in Palestine argues that these sites demonstrate how moving bodies between the West Bank and Israel are constantly regulated, differentiated and governed by the Israeli state.

My interest in walking as a methodological framework sprang from a wish to explore the relationship between mobility and politics. I was interested in exploring how this everyday mobility of walking linked to broader politics in Jordan and to

uneven and entangled mobilities. Walking, therefore, became both the method and conceptual and empirical focus of my research, deeply entangling my theoretical ideas with my methodological practices 'in the field'. I focused primarily on the Jordan Trail — which I found out about through an internet search in the dead of night, during the anxious period I spent developing a new research project. The Jordan Trail is a long-distance walking trail running 650 km from the north to the south of Jordan. While the Jordan Trail has existed in various forms since the 1980s, in 2014 the project was given a large grant from USAID and an NGO, the Jordan Trail Association (JTA), was founded. The role of the JTA was to project, maintain and manage the trail. The goal of the Trail was both a tourism and a nation-building project, a Trail that could connect Jordanians to their state but would also offer opportunities to boost a tourism economy damaged by the outbreak of the Syrian War. In other words, the Trail itself was also connected to geopolitics of immobility in the region (see Figs. 2.1 and 2.2). It would be a tourism project that would reach communities along its 650 km and thus benefit a wide range of groups. My aim here, however, is not to enter into a discussion of the politics involved in the creation of the Jordan Trail. This has been the subject of other published work (see Mason 2020). Instead, I want to reflect on how walking as a methodology and mode of enquiry can be used to explore questions of entangled mobility.

Little research has used a 'thru-hike' — a continuous walk of a long-distance trail — as a method or considered walking as a practice that is linked to uneven

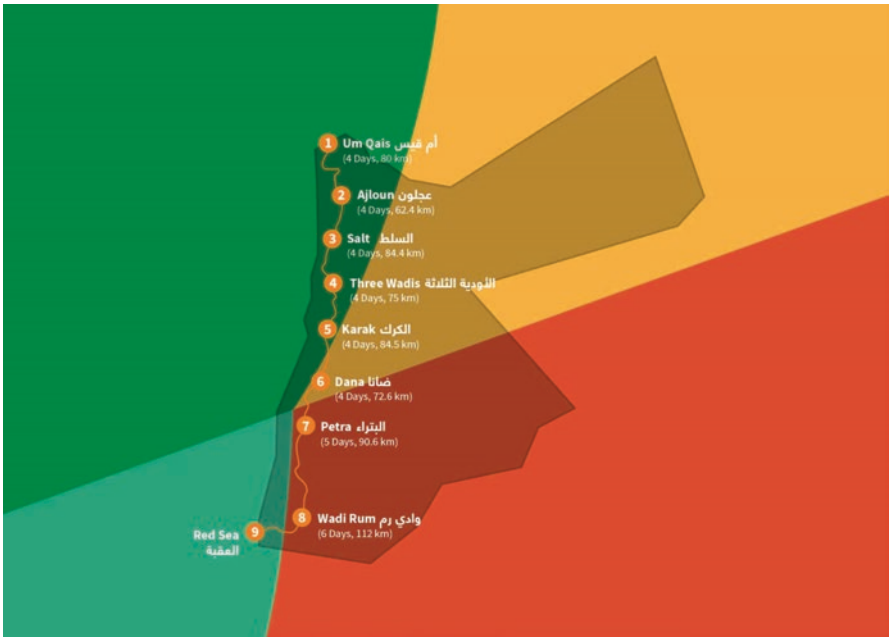

Fig. 2.1 Schematic map of the Jordan Trail. (Source: Jordan Trail Association)

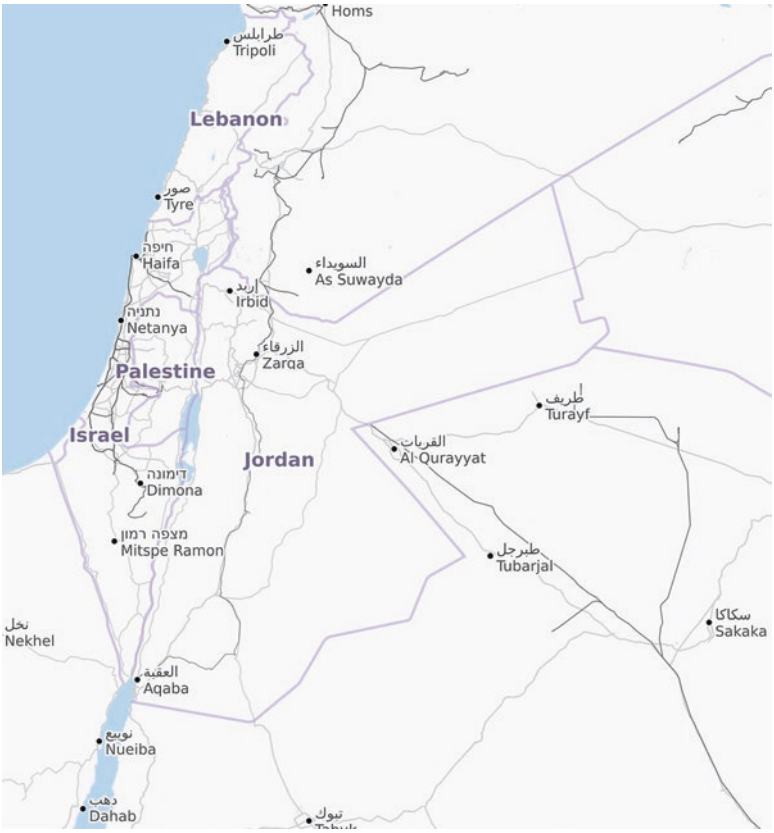

Fig. 2.2 Political map of Jordan. (Source: OpenStreetMap)

mobilities.[4] My methodological approach therefore moved beyond simply walking as a method to an in-depth ethnographic exploration of a long-distance walking trail. I did not just use walking as a method but explored walking as a practice itself. To do this, as well as spending time walking on the trail, and conducting walking interviews, I spent 6 months volunteering for the JTA. I took on numerous roles within the JTA including translation, writing content for the website, answering emails, and scouting of the Trail. I attended board meetings, meetings between USAID and JTA, and marketing meetings, and I was able to be part of everyday conversations and negotiations. Volunteering for the JTA came from an ethical imperative to ensure I was offering my time in return for their help in progressing my research. Being both a researcher and a volunteer provided unique insights into how an NGO was run and how a walking trail was developed. However, it also blurred the lines at times between my role as a researcher and a volunteer. Therefore,

[4] See den Breejen (2007) and Collins-Kreiner and Kliot (2017) for two studies of long-distance walking trails.

I ensured that I checked and showed the JTA all conversations I was including in my research by sending them copies of articles for publication and a copy of my thesis. I was also aware that many of the conversations and meetings I engaged with were not for the purposes of my research but in my role as a volunteer. Many of the conversations I was part of, while empirically important for my research, I did not include in my thesis with mutual agreement with the JTA as they disclosed information about the JTA they did not want to be made public.

My time walking on the Jordan Trail included day walks; however, a large part was spent joining thru-hikes — a term used, particularly in America and adopted by the Jordan Trail to describe, in English, a continuous walk on a long-distance trail. The first thru-hike I joined for only half of the Trail's 650 km, following a 'technical thru-hike' in which a walking 'trail expert' was hired to check the Trail to ensure the path was safe and that it was easy to follow. The second thru-hike I joined from start to finish (walking for 40 days — with 4 rest days — and 650 km) and was the first ever official thru-hike of the JTA. This thru-hike is now an annual event (three subsequent thru-hikes have been organised since I finished my fieldwork) to market and to raise awareness and money for the JTA. These thru-hikes and day trips amounted to over 80 days or 1920 h on the Trail. While walking, I took fieldnotes on my phone as I walked, and wrote these up in longer form in a notebook in the evenings. I noted what I saw and heard and how I was feeling emotionally and physically at various points and in relation to others and the surfaces of the Trail, alongside how I walked in relationship to others and conversations we had as we walked. Finally, I wrote about the Trail itself.

Conducting interviews while walking was also an important aspect of my research. Lee and Ingold (2006) suggest that walking with interviewees encourages a sense of connection with the environment. The advantages of the physiological movement of the body through place enables walking interviewees to be part of 'an unstructured dialogue where all actors participate in a conversational, geographic, and informative pathway creation' (Anderson 2004: 258). Walking interviews helped to capture deeper accounts of the relationship between people and place but also resulted in a deeper relationship between myself and those I interviewed. As a result, walking interviews became another element of my ethnographic approach. These interviews often took place while I was out walking on the Jordan Trail with other groups of walkers, including on the 'thru-hike' and were often unstructured and arose naturally as I fell into rhythm with another walker. Across the research process I conducted over 60 walking interviews. All of those I interviewed were recruited during a walk. I recorded the interviews on my phone, but I simultaneously took notes on my phone. This is because the sound quality of the interviews was often compromised by the sounds of our footsteps, the environment, and other walkers talking. To hold the phone right up to the person's face seemed unnatural, therefore making notes on my phone too and then writing up as much of the interview soon after helped with this disturbance. These interviews were mostly conducted in English, as many of those involved in the Jordan Trail or walking on it

spoke a good level of English. Some interviews were undertaken in Arabic with the assistance of a translator.[5]

Walking enabled me to understand mobility and immobility in a grounded and everyday way in which emotion, power and privilege were central. The process of walking brought me together with others through our shared enjoyment of walking. We would discuss why we enjoyed walking and such conversations felt placeless at times. They felt placeless as the process of walking, the way it made us feel, the rhythm of movement, and the connection between our mind and body created emotional connections between us. At other times, walking brought me into contact with other bodies and places that highlighted privilege and power. For instance, those walking on the Jordan Trail were composed of expats, international tourists, Bedouin communities and Jordanians from Amman.[6] For each group, different histories of walking became entangled. My history of walking is one linked to British legacies of colonialism, the 'right to roam' and access to outdoor spaces and national parks (see Edensor 2017). In these histories, walking as a leisure activity was traditionally for the upper classes while walking for work was linked to the working classes (Ingold 2004). Connecting this to race, Tolia-Kelly (2007) argues that the English Lake District has been culturally embodied as a memorial to a sense of Englishness which alienates and excludes multicultural history. Walking and the spaces in which we walk are therefore often linked to geographies of exclusion and inclusion through associating certain mobilities with certain ways of enacting those mobilities.

In Jordan, a history of walking is linked to very different histories of trade, work, travel, and Bedouin nomadic histories (see Mason 2020). Discussions would therefore arise in interviews and conversations about what walking means in different political settings and in different cultural settings. This links to a growing body of literature within critical mobilities studies that argues for the need to capture non-Western and indigenous accounts of movement to speak back to dominant narratives of mobility that privilege the white, Western body (see Sheller 2018; Edensor and Kothari 2018).

A further aspect of the heterogenous and situated mobilities of walking is through gendered experiences of walking that arose in my fieldwork. To illustrate I use an example of the experience of three Jordanian women — Lara, Yasmine and Maha — in a homestay on the Jordan Trail, run by a local Arab Women's Group just outside of Amman. Lara, Yasmine and Maha were walking with a male and female group, and were questioned by women in this Women's Group as to what their

[5] The issue of language is not one I am able to go into detail here, but I speak conversational Arabic as a result of two intensive Arabic courses in Jordan and Palestine and a year of Arabic language training prior to my fieldwork. I found my basic Arabic enabled connections with people I met in Jordan whom I would not have been able to speak with using just English.

[6] Many of the Jordanians who do walk on the Jordan Trail are from more privileged backgrounds in Amman, which links to questions of class important to British histories of walking. However, this is slowly changing, with the Ministry of Tourism offering reduced rates for Jordanians joining organised walks on the Jordan Trail and many new walking groups in Amman running with very low joining costs.

family thought about them walking, particularly in a mixed group, as they did not think it was appropriate. In an interview with them, they told me it highlighted to them assumptions about movement and gender that are prevalent in Jordan, but they did not expect to encounter them from other women. Throughout my time on the Jordan Trail, I was often asked what my family thought about me walking on the Jordan Trail as a single woman. These gendered questions were often further complicated by my role as a foreigner. For instance, in one homestay I stayed in I was able to enter both male and female areas of the house as I was a woman but as a foreign woman I could also transcend this identity. As a result, walking became a way to consider questions of which bodies could move beyond certain spaces and which could not but also to consider the cultural and historical contexts in which walking takes place.

A final point about movement and non-movement on the Jordan Trail returns to my denied entry and the geopolitics of mobility in the Middle East it relates to. Walking on the Jordan Trail frequently brings walkers into close and intimate proximity with the borders of Israel-Palestine, Syria and Saudi Arabia — at times only 5 km from the border with Israel-Palestine. While that border can be crossed by some of those walkers, for many, particularly Palestinian-Jordanians, it cannot. The Jordan Trail also attracts many domestic tourists. This is because many Jordanians are looking for options to travel within their state as geopolitical conflict has made travel to neighbouring states such as Lebanon, Syria and Iraq difficult or impossible. Similar to the Afghanistan passport, a Jordanian passport offers access to limited states visa-free. The other group who are walking on the Jordan Trail are the international community including researchers, NGO workers and Arabic language students living in Amman. The Jordan Trail brings these groups of walkers together on the Trail with different experiences of mobility and different cultural histories of walking. In sum, to understand researcher mobility it is crucial to explore it in relation to emotion, power, privilege and specific political contexts.

2.3 Conclusion: Entangled Mobilities During Fieldwork

This chapter has explored the entangled mobilities of fieldwork. I have explored these entanglements through my own fieldwork experiences in Israel-Palestine and Jordan to make two key contributions to methodological thinking and writings around postgraduate research. First, that we must think more critically about access to field sites and the increasing restrictions put on researchers. This requires greater awareness of potential access issues while doing a PhD, but also that the politics of access be brought into conversation with questions around emotion, power and privilege. This all requires thinking about researcher access to field sites as heterogenous and entangled with differing degrees of privilege. By reflecting on these questions, I call for future work to query how power and privilege impact access.

Second, I reflected on how my own experiences of mobility and immobility led to a new research project. I discussed the practicalities of using walking as a method

before reflecting on questions of positionality and power that are entangled in this method. Shared experiences of mobilities can bring myself as the researcher and those researched together in ways that are placeless, as walking creates shared emotional connections regardless of where we walk. However, walking also highlights that mobility is dependent on the cultural politics of a place and highlights that not all bodies are free to move equally. This everyday approach to mobilities gained through using walking as a method offers more intimate and engaged ways in which to explore questions of access, mobility and privilege in the field site. It also illustrates the connections between the methods we use and our own mobilities and immobilities as researchers.

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Chapter 3
Travelling with the Field: Post-Place Communities of Volunteer Tourists on the Move

Yim Ming Connie Kwong

Abstract Rather than entering and getting immersed into a local community for a prolonged period, this chapter focuses on the ethnographic process in working with post-place communities which are tied together with a shared identity and set of norms. Using an example of two temporarily formed communities of volunteer tourists from Hong Kong and Taiwan travelling to Cambodia, I highlight the complexities and messiness of various identities of the fieldworker during active engagement with time-specific mobile communities, and the challenges of defining and leaving the field. The chapter starts with how I identified the 'field' — time-specific space of temporary communities of volunteer tourists — through collaborating with NGOs. It then explores and reflects on my identities and roles, and on the embedded relational ethics that framed my practices as the fieldworker and shaped the field, relationships and research process. Working with these post-place communities invites discussion about what is temporary and what is temporal throughout the process, and of friendship as a form of reciprocity.

Keywords Post-place field · Volunteer tourists · Temporality · Relational ethics · Friendship

3.1 Field on the Move

Ethnographic research is usually conducted in a fixated physical location or community, which is mostly defined or bounded based on place. However, studies with a focus on post-place communities, which are 'networks of people tied together by solidarity, a shared identity and set of norms that does not necessarily reside in a

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place' (Bradshaw 2008: 5), have not been explicitly discussed methodologically. It requires timely identification of and registration into such communities that entail a group of people temporarily formed for a travelling purpose. Working with them also implies a particular set of relational ethics that frames the practices of fieldworker and shapes the field, relationship and research process.

In this chapter, I am going to discuss these methodological and ethical concerns using my PhD fieldwork experience with volunteer tourists — 'who, for various reasons, volunteer in an organized way to undertake holidays that might involve aiding or alleviating the material poverty of some groups in society, the restoration of certain environments or research into aspects of society or environment' (Wearing 2001: 1). My PhD research aimed to explore the social trend of participating in volunteer tourism in non-Western societies, delimited to Hong Kong and Taiwan. Thus, the fieldwork was crucial for gathering data to construct a systematic and theoretical understanding of the rationalities and practices of taking part in volunteer tourism. In the following, I first describe how the post-place 'field' was identified and accessed, then reflect on how and why the 'work' was carried out in a particular way through which relational ethics was played out.

3.2 Registering into the Temporary Communities

In a study on tourists, the field was less about a physical location or static community; it was the time-specific space of active engagement with temporary communities of travellers. Therefore, it was vital to identify the point of contact through which I could approach the volunteer tourists who appeared at specific temporal and spatial coordinates. During the planning stage, I could only first decide on which organisations to contact for recruiting research participants. Collaborating with sending organisations was highly dependent on personal knowledge and contacts because there was no organised platform compiling a list of sending organisations and volunteer tourism programmes. I was a project coordinator of The Bucket Wish (TBW), a student-led organisation in Hong Kong, and went to Cambodia as a volunteer through their programme in 2015. I thus contacted TBW's Chairperson and agreed on the collaboration very smoothly. As part of the collaboration and a form of reciprocity, I helped planning and organising the project by making use of my previous experience with TBW. My work included editing publicity materials and project proposals for funding, booking flight tickets and accommodation, and giving suggestions for local arrangements (further reflection on this position in the next section). Having no previous volunteering experience through Taiwanese organisations, I navigated through the web of social relationships. I reached out to a Taiwanese contact who has been active in international volunteering and who helped me identify a few reliable and popular organisations in Taiwan. Eventually, I settled with ELIV International Service (ELIV) in Taiwan and TBW in Hong Kong as partners and their respective Programme Director and Chairperson as gatekeepers.

I signed up as a volunteer in one project of each organisation so as to register into these two temporary communities at the same time as my potential informants. I first joined TBW's 12-day Cambodia programme in January 2017, which consisted of volunteering for a community-led mangrove forest restoration project for 10 days in a fishing village and visiting Killing Fields and Tuol Sleng Genocide Museum in Phnom Penh. The second project was ELIV's 8-day programme in Cambodia in February 2017, during which we built a house for a family and a toilet to be shared by five families (part of a 2-year relay of building toilets for the whole village), and conducted visits to families in the village and social enterprises in Siem Reap. Both programmes consisted of discussion and debriefing sessions throughout the trip.

The process of contacting TBW's teammates was rather straightforward; with the consent of the Chairperson, I sent private messages to the 21 volunteers and arranged pre-trip interviews with all of them before the first pre-trip workshop (four in total). In ELIV's case, after a long period of waiting until a project team was formed with enough signups, the recruitment of informants was delayed due to technical issues. Not until mid-December did I receive the notification about the only pre-trip briefing which was held in late December. There were then more steps and obstacles in recruitment. Each ELIV team was managed by a primary and a deputy team leader. I had to ask for my primary team leader's permission to recruit informants after the team was formed. With her consent, I sent private messages to all the 18 team members (including both team leaders) before attending pre-trip briefing but got confirmation from only five of them. Therefore, I had to approach the rest at the briefing; however, three of them were absent. The recruitment had to be done partly on *Facebook* or *Line* (a social networking tool commonly used in Taiwan). At the end, 13 out of 18 participants were recruited from the team. I had expected this process to be challenging, and starting the researcher-researched relationship with the two groups differently created a distinct relationship with varied intensities of rapport between the respective groups and me (further discussed in the following section).

3.3 Doing Belonging

After registering into the temporary communities, actively engaging with them in order to develop trust and rapport involved a lot of methodological and ethical concerns. Thus, the ethnographic tools employed both shaped and were shaped by the practice of 'doing belonging' (Bennett 2012). In this field, I had several roles and identities — researcher, volunteer and project coordinator — that enquired the insider-outsider status and influenced belongingness. This posed questions on how to manage intersubjectivity — 'meanings and interpretations of the world created, confirmed, or disconfirmed as a result of interactions (language and action) with other people within specific contexts' (Dowling 2016: 39). In these social interactions, how the informants perceived me, how I perceived them, and how we interacted were determined and framed by social norms. This required critical reflexivity

to become aware of the nature of involvement and influence of social relations (Dowling 2016; Bakas 2017). This was a process to reflect on how the research was conducted and to acknowledge that I — the researcher — was part of the research (Phillips and Johns 2012).

Being a researcher entering the lives of informants and spending most of the travelling time together blurred the distinction of researcher versus friend. Due to the short timespan of these service projects, I was anxious at the planning stage that I would not have sufficient time to be immersed into the field. At the same time, I was wary that I might seem too deliberate in gaining rapport while in the field. A combination of two ethnographic methods — participant observation and personal interviews — enabled me to have more episodes of personal and intimate interactions with my informants, increasing the intensity of the travelling experience and relationship. Being a 'participant as observer' (Gold 1958) — an ethnographer with a high level of participation in the researched community — I engaged in my informants' rhythms and routines and had a shared identity with all volunteers as part of the group. It was not just about 'watching'; I did the same as my team members for the projects and had genuine interactions with them while jotting down observation notes as a researcher. Spending time with the team members during pre-trip preparation, volunteering, discussion and reflection, and informal interactions such as eating together and commuting, all helped develop understanding through the spontaneity of everyday interactions.

This 'participant as observer' stance also means a mutual awareness of a field relationship. As Paul (1953) has pointed out, 'participation implies emotional involvement; observation requires detachment. It is a strain to try to sympathise with others and at the same time strive for scientific objectivity' (cited in Musante and DeWalt 2010: 28). I attempted to strike a balance between maintaining this relationship and establishing rapport and trust. I decided on a 'selective disclosure' during fieldwork. When introducing myself at the briefing in Taiwan, I disclosed to the team that apart from my interest in volunteering and sustainable development in general, my main purpose of joining the programme was for my PhD research. Since I had contacted some of them prior to the briefing, it would seem odd if I avoided mentioning it. However, when we were asked again by the local Project Manager in the first meeting in Cambodia, I withheld this information and introduced myself as a member of this project team. It was because I did not want to give my research participants the message — 'be alert, she is watching'. Reminding them of their participation in my research might lead to intentional modification of practices (Barbieri et al. 2012) or to some kind of uneasiness, although I did emphasise at informed consent that this research was not aimed to judge their behaviour. Also, I tried to avoid creating awkwardness or estrangement since not everyone in the team participated in my research. This selective closure sustained the researcher-researched relationship while engaging me in the communities more easily and naturally.

Interviewing was particularly useful for me to access social interactions that were restricted or invisible through participant observation as the service trips were very short. Interviews are alternative ways of 'knowing', or 'conversations with a purpose' (Burgess 2002: 102), through which researchers could understand how

people 'experience and make sense of their own lives' (Valentine 2005). Instead of one-off meetings, interviews were undertaken both before and after the service trip, enabling me to understand complicated issues and my own observations in the compressed time of the trips more in depth than in breadth (Crang and Cook 2007). In addition to methodological concerns, the interviewing time allowed me to talk to them beyond my 'professional' role. Pre-trip interviews were short, but the informants and I were spending more time talking about our previous volunteering experiences and other things like politics in Hong Kong and Taiwan and travel plans. That was the time we got to know each other as team members, away from my identity as a researcher. However, as explained earlier, I did not conduct pre-trip interviews with all Taiwanese informants in person due to time constraints. Upon informed consent, I sent the questions to some of them on *Line* and they replied with text or audio messages while I was travelling with the Hong Kong team. This, to a certain extent, undermined the depth of data to be collected, but still served the purpose of eliciting prompts for observation and post-trip interviews. This lack of opportunity to get to know and talk to everyone in the Taiwanese group in person, which I did with the Hong Kong group, made me feel uneasy; this left me feeling like 'something hasn't been done yet' before we set off to Cambodia. It was a bit awkward when I met part of the Taiwanese group the first time at the airport, because I had not offloaded the researcher identity temporarily to meet them as my team members instead of research participants; also because I felt strongly that I had a closer relationship with the Hong Kong volunteers. Post-trip interviews were then more like reunions, reminiscing the time in Cambodia together while reflecting on what they did with my aide-memoire in hand. We were laughing at the same jokes over and over again, like friends do, but at the same time I had to conduct the interview, wondering if I had talked too much.

The nature of voluntary work and the duties of a volunteer made it challenging for me to also play the roles of researcher, team member and/or friend simultaneously. Instant note-taking was impossible in the first four days in the fishing village because we were working in the muddy mangrove forest and seedling nursery site from 9 am until late afternoon. Each sub-team then took shift for cooking, washing up and cleaning bathrooms. With the ELIV group, everything was about team work, including construction, home visits, serving lunch, and even going to the toilet (another ELIV toilet). On the one hand, as a researcher, I wanted to take out my notebook and log my observations as much as I could. On the other, if I took out anything to write, it would seem too obvious that I had shifted to the researcher role and such disclosure might make them become alert and behave differently. At the same time, as a participating volunteer I could not suddenly leave my positions of responsibility to hide somewhere to scribble my notes, not to mention that there was indeed nowhere to hide when we were doing construction work in the village. I became more cautious after one time a volunteer asked what I was doing when I was writing on my notebook in our free time. It probably was a general question, simply asking what I was doing at that moment because we were relaxing. However, because of my researcher role, the question sounded like asking what I had written about them. Despite the selective disclosure and mutual awareness of the field

relationship, the act of note-taking showed a sudden shift of roles from a travel companion to a researcher, and being caught doing it felt like I brushed off the blocs of trust and rapport I had built. Hence, I often attenuated my presence as a researcher in order not to disrupt the rhythm of their experience, but to create a contact zone of engagement with trust because I felt like a friend of theirs and did not want to react like I had to hide something from them. All these restricted and suspended note-taking at times of happening, which have been reported in previous studies using similar methods (e.g. Barbieri et al. 2012). Sometimes I could barely have a few keywords down on my notebook or mobile phone, so I relied on writing my field-notes from memorisation at the end of the day. Despite this, the advantage of observing as a participant was engaging myself in the intense experience simultaneously with the informants. This allowed me to rebuild the scenes from the scribbled keywords and photos afterwards, while the reminiscence at post-trip interviews gave more details to my notes.

I was also very aware of my role in planning the TBW project as it might create a sense of authority or conflict of interests. This position allowed me to attend their staff meetings during which I started building rapport with the five student project coordinators who were also volunteers and my informants. I was very conscious of this position because of the feeling that they had to listen to me who was a veteran project coordinator of TBW. Despite the expectations to get in-depth data, I did not want to participate too much in organising the project so as to avoid imposing any 'should-do' which might in turn frame the project in the way I wanted for data collection. It turned out to be a useful role because my previous experiences both as a project coordinator and a volunteer in Cambodia acted as 'insider voices' that helped them resolve some practical problems and doubts; it showed my ability to help as a member of the team rather than my privilege as a researcher to just tag along. More importantly, I developed friendships with them more naturally and quickly. But I have to admit that it was hard to discern myself as a team member or a veteran project coordinator, or both, when I gave suggestions.

Having previous volunteering experience in Cambodia was also helpful in developing rapport in general. The Taiwanese team leaders allocated a fresh volunteer and me to the same sub-team and room because they believed I could help ease her anxiety of first overseas experience. Later during home visit, I was asked to start the conversation only because I could greet in Khmer more fluently but not because I was a researcher. These seemingly trivial acts however made me feel that I was treated as a part of the team rather than someone who pretended to be participating.

3.4 Spatialising the Field

This post-place field can be extensively stretched geographically due to its nature. Although we joined these temporary communities at the same time, the question that remained was how much I should be involved in my research participants' lives

and how I should leave the field. This is the status that Bakas (2017: 127) refers to as 'a friend but not a friend' or O'Reilly (2005: 36) calls 'one of us, but not one of us'. This status was profound and complex particularly during the time when I travelled to different cities in Taiwan to conduct interviews. I did several short trips across Taiwan and some Taiwanese volunteers offered me the opportunity to stay overnight at their place. This was their hospitality; they treated me as a friend, so I accepted their offers; I also treated them as friends, so it was like making trips for purpose of visiting them rather than doing interviews. During those excursions, I let them decide when they wanted to start being interviewed as I did not want to dominate the time which was partially leisurely. On the other hand, their participation in the research would not end until my post-trip interview with them was finished, which means I could not entirely enjoy the time with them as a friend. It was a complicated feeling. But I believed this 'hanging out' was part of the process of developing understanding, and also my commitment to maintaining friendship rather than a transient membership of the temporary communities. As De Munck (1998: 41) explains, this gives the informants 'an opportunity to watch, meet, and get to know you outside your "professional" role'. However, it confused me in what relationship we were at the moment of hanging out — researcher and the researched, friends, or both. When one Taiwanese informant and I had a 2-day road trip, we exchanged a lot and shared personal stories during those hours of driving. That personal and intimate time-space was not arranged for observation, not even expected. Undeniably, it helped to elongate the field time and I had a deeper understanding of her in terms of her family and values after that deep talk (and an interview with her afterwards). But that was far more than a methodological interpretation; those were chats between friends and travel companions at that time rather than 'conversations with a purpose' (Burgess 2002: 102). I understood that I had become part of that community throughout the times of volunteering and gathering; but a mix of socialising and doing fieldwork made my identities as a researcher and a friend more ambiguous and messier. This made me worry it would intensify the perceived tension, at least by myself, between extracting data and developing a genuine friendship. Having said that, our friendships are still lasting.

Returning home from the service trip and then from fieldwork, it again came down to the questions of when and how to define the point of leaving the field, and what the ethical implications were from the kind of relationships entailed. After all post-trip interviews and TBW's post-trip workshops, my informants and I have still maintained our friendship through social networking tools and reunions whenever I went back to Hong Kong (also in Taiwan and Cambodia). For a few months after I had finished the fieldwork, I was not sure whether they thought I talked to them only for 'watching' or getting more data from them. Those online interactions, which were part of their everyday life, could be the sort of things to be observed and interpreted, but I did make it clear at informed consent that their participation in my research ended once post-trip interview was conducted. Still, I was quite cautious of how I commented on their Facebook posts or interacted in the group chats so as to avoid the feeling that I was 'creating a scenario' to gauge responses and collect data. This led me to wonder whether I had hinted — both to my informants (I did not

explicitly say 'this is the end of your participation' after each post-trip interview) and myself — that I had left the field, or whether it is possible to leave completely. One year after fieldwork, I travelled to Cambodia and hung out with a few Taiwanese team members who were volunteering there again; one Hong Kong team member came to visit me in the UK. These relations and interactions took place outside my professional role, which reaffirmed the possibility that I did not leave *the field* completely, or the option that I did not need to clearly signal the end of fieldwork, while recognising that it is normal and reasonable to maintain the friendships. Maybe it is the messiness and awkwardness of 'ethnographic intimacy' (Frohlick and Harrison 2008), from engaging in nuanced understanding of embodied complexities, that has kept me in the close and emotional proximity with the field, or it is simply our 'habitus of collectivity' (Kwong 2019) constructed from the intense travelling experience.

Reciprocity has been a key matter in addressing the issues of power in the researcher-researched relationships and possibly guiding how to conduct the research in a more ethical manner (Harrison et al. 2001). As a researcher, we always want to get enough data, quality data, and a better understanding of the subject of research in order to answer our research questions. Due to the short timeframe of the trips, I shared the same anxiety about not spending enough time with the informants as Frohlick and Harrison (2008). Thus, I attempted to engage in conversations and social activities as much as I could throughout the projects. In spite of some episodes of intense emotional involvement, 'relationships between interviewer and interviewee often end abruptly once the researchers have finished collecting the information that interests them' (Kirsch 1999: 30). When short, one-off interviews are conducted, interviewees are usually left uninformed or uninvolved in the later stage of knowledge production. Besides thinking what I owed the field, I was more committed to maintaining the social relationship as a form of reciprocity. Although trust does not necessarily entail reciprocity, I had a *felt* need to give back, especially because our process of knowing as a researcher relies on the researched community. Since we enter the researched community with a research agenda, we hope to leave something in return to soften or compensate for the unequal researcher-researched relationship. Sometimes, we may take the easier ways and 'tick the box' of 'giving back'. It is posited that the unequal nature of researcher-informant relationship could not be altered with reciprocity (Huisman 2008), thus leaving without promises or gestures of reciprocity could possibly be better than giving something irrelevant in return. For these temporary communities of volunteer tourists, admittedly, this research project was not going to produce any outputs that would directly and significantly impact them. Despite that, I hoped to return with something co-produced with my research participants, allowing them to own part of the knowledge produced throughout the process, mainly because they empowered me as a researcher to present the data in the form of knowledge (Weiler 2009). However, I did not promise anything upon informed consent or end of their participation due to my lack of capacity, in terms of both time and resources. What I wanted to do requires careful planning to engage them in the dissemination of further co-production of knowledge, and I think reciprocity could be considered further beyond the timeframe of the PhD. Upon completion of my PhD, I produced a written report

summarising the findings as requested by ELIV's Programme Director, which could be perceived as a reasonable form of reciprocity to the field by bringing the researched voices to a non-academic audience. More relatedly, friendship has proved from my experience to be an appropriate form of reciprocity, as it is intangible and immeasurable; mutually beneficial and long-lasting; and, more importantly, it does not involve the calculation of power relations.

3.5 Temporary or Temporal?

From the data, I analysed how different forms of social relations emerged from the *communitas* produced during the trip (see Kwong 2019). *Communitas*, a modality of social relationship, is produced in the liminal space in which individuals engage in collective tasks and are treated as equals (Turner 1969). In this research, *communitas* was not vanishing right after the trip, as it was not on our timescale; it still lingers in its own way as time passes, in and out at different spatiotemporal coordinates. On the one hand, it is recognised that increasing distance may reduce the intensity of sentiments (Ginzburg 1994); on the other, Davies and Herbert (1993) argue that relationships can be maintained over greater distances with the support of a global sense of place. From this experience, the *communitas* produced has helped to hold the dots in the web of relationships together, strengthening the bonding. The relations here are social relations beyond the researcher-researched ones. Although the trips were very short, our bonding is far stronger than expected due to intensity of the experience and team members' active moves of doing belonging. This again has blurred the distinction between the researcher and friend statuses, and between relationships during and after fieldwork, as the emotional string is still attached.

In the framework of working with temporarily formed communities, addressing some of the concerns of positionality and reciprocity accentuates the intricacies of knowledge production through ethnographic research. Viewing positionality is a critical factor in framing the social and professional relationships in the field and beyond. It is not feasible to fully resolve the tensions, dilemmas and unequal relationships; rather, it is necessary to grapple with and reflect on the ethical dimensions of research and continue to involve ourselves in a 'spiral of self-reflexive cycles' (Kemmis and McTaggart 2005: 563, cited in Huisman 2008). While I attempted to reflect on my methods and positionality, and to negotiate some challenges throughout the process, I had to acknowledge some other ingrained limitations and let them live on.

Having said that, the process does not end when we, as researchers, have finished the fieldwork and written up the methodology chapter. As a researcher, our role is to unpack the thickness of the data by constructing a reading of the entangled, multiple layers of meaning of different actions or speech. The informants are active participants during the data collection, so they decide what to tell us; we are then the ones with the power to decide what to write. Since the researcher is the person who chooses what to research, where/how to conduct the fieldwork, and what/how to

write about it, we bear the responsibility to convey our research findings in a clear, coherent, compelling manner (Hay 2016). The course of going through the data and thinking about how to tell the story in the best, most lucid way helps to probe into more questions and further analysis. It is also an important step to translate our immersion or even intrusion into others' lives into something meaningful. This process of interpretation, analysis and writing, to a certain extent, also maintains the social relationship in another temporal scale. When I transcribed the interview with the Taiwanese informant with whom I did a short road trip, for example, I realised I was not listening to a stranger talking but an informant who has become a friend of mine, as I had heard some other stories of hers. Re-engaging ourselves in the stories might somehow make it difficult to leave *the field* entirely as the emotions fold over and back. But what resulted from this conundrum is that 'something meaningful' produced at this stage of the PhD could possibly be a transformation of a researcher-researched relationship into a long-lasting social relationship — even friendship — supported by the collective joy at a new temporal coordinate, despite the fact that this post-place community was once temporary. Although Paul (1953) has posited that in ethnographic research, 'participation implies emotional involvement; observation requires detachment' (cited in Musante and DeWalt 2010: 28), what is more likely to remain is *emotional attachment* to the researched community; as it is hard, or sometimes unnecessary, to define clearly when we have left the field methodologically and emotionally.

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Chapter 4
Assembling the Fieldless Field Site

Ludovico Rella

Abstract This chapter develops a radically networked approach to the study of digital infrastructures. Drawing from digital and multi-sited ethnographic literature, this chapter illustrates the challenges and opportunities inherent to a research project that does not entail intensive fieldwork in a specific location. This chapter argues that we need to take seriously the claims made by digital and multi-sited ethnographers that the field site is as much produced as it is discovered, and its production is as much a theoretical as it is a logistical effort. In assembling a field site that is hybrid between online and offline, and multi-sited between different locations, one has to be attentive to the challenges of this methodology with respect to the mobility, timing, and their limits when it comes to access, data collection and analysis. When conducting a fieldless fieldwork, the researcher's positionality is that of a scavenging ethnographer that attends to access and limitations to access as a texture rather than as a clear-cut border. Reflexivity is paramount to understand when theoretical saturation has been reached and data gathering can stop. This chapter contributes to literature in multi-sited ethnography, anthropological research in trade fairs, conferences and expos, and in digital social research debates about the necessary methodological specifications required by digitally mediated settings.

Keywords Multi-sited ethnography · Digital methods · Networked field site · Trade fair ethnography · Scavenging ethnographer

4.1 Introduction

> *A process cannot be understood by stopping it.*
> *Understanding must move with the flow of the process, must join it and flow with it.*
> (Herbert 1982: 30)

This chapter is based on my 18-month fieldwork, and it aims to illustrate how digital infrastructures may require a radically fieldless approach to fieldwork. My

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fieldwork entailed the use of online archival research, ethnography of online meetings, participant and nonparticipant observation of online forums, participant observation of industry trade fairs, expos and conferences; and both digitally mediated and traditional in-person, in-depth expert interviews. The chapter combines methodological, epistemological and practical reflections to illustrate this fieldless approach to fieldwork, and addresses specific concerns regarding time and temporality of field research, power and access, and space.

Ethnography as 'an immersive research strategy that seeks to understand how people create and experience their everyday worlds' (Kavanagh and Till 2020: 321) has been changing as a consequence of increased mobility of people and things, heightened connectivity and circulation of information (Marcus 1995). Multi-sited ethnography emerged as a type of social research that is 'self-consciously embedded in the world-system […] to examine the circulation of cultural meanings, objects, and identities in diffuse time-space' (Ibid.: 96). Recently, this 'multi-sited imaginary' (Pierides 2010) has been further developed into a conceptualisation of the 'field site as a heterogeneous network' by Burrell (2009: 182). Such definition goes beyond the idea of 'embedding' the localised, idiosyncratic characteristics of the field site in larger global processes. Rather, one should 'imagine the whole' (Marcus 1989) and follow the instantiation of such whole by following the people, the things, the metaphors, the plots, the stories, the allegories, the biographies, and the conflicts that traverse and connect field locations (Marcus 1995). Technological developments and the emergence of new sociocultural spaces represented by digital media further illustrate how social processes are hybridised between online and offline spaces (Burrell 2009; Kitchin and Dodge 2011; Ash et al. 2018).

The core concern of my project has been the making and remaking of monetary spaces through the deployment of digital payment infrastructures including, but not limited to, blockchain technologies and cryptocurrencies. Money, especially in its digital form, is arguably the social relation that is the most embedded in the world system (Hart and Ortiz 2014). The fields of Financial Technologies (FinTech), cryptocurrencies, and blockchain technologies are particularly 'hybridised' between online and offline spaces. FinTech companies operate in geographically dispersed and online-offline hybrid spaces. They are socially interconnected, yet geographically dispersed ecosystems of applications, devices, material and virtual commodities, and communities. These spaces are fraught with power and knowledge asymmetries in terms of technical expertise, and the opacity deriving from non-disclosure agreements surrounding technological solutions applied to financial services. Accessing and exploring these spaces, then, requires a 'polymorphous engagement' (Gusterson 1997: 116) with different types of research data.

A fieldless fieldwork requires a specific type of fieldworker subjectivity that Seaver (2017: 6) calls 'scavenging ethnographer': 'the scavenger replicates the partiality of ordinary conditions of knowing'. The remainder of the chapter will focus on how issues connected with spatialities, temporalities, and positionalities shaped my fieldwork in an increasingly 'fieldless' fashion. The next section expands on issues of the spatial construction and assembly of a radically multi-sited and fieldless fieldwork, and how boundaries are and should be drawn and redrawn

throughout the research process. The subsequent section expands on time, temporality, and temporal boundaries in such fluid research projects. The conclusion highlights explicitly the contribution that the chapter makes, which is especially timely as place-based fieldwork is going to be transformed by the proliferation of digital technologies and by the practical challenges posed by Covid-19.

4.2 The Spatialities of the Fieldless Field

My initial research project focused on the making and remaking of subjectivities through and by networked technologies and their interfaces (Greenfield 2017). During the first year, the project progressively drifted towards the making and remaking of monetary spaces through digital payment infrastructures, driven by an uncanny similarity between the flow of money and logistics (Rea et al. 2017). I decided to focus on Ripple, a software company that applies blockchain and interoperability technologies to cross-border payments. It was first conceived in 2004 and deployed in 2013, with the ambition to provide an integration infrastructure between cryptocurrencies, alternative currencies, and traditional interbank payment systems. It presently has more than 300 clients throughout the world, mainly banks and payment providers. Ripple was interesting to me because it allowed me to investigate the materialities and spatialities of money in different settings such as cryptocurrencies, banks, public regulators, FinTech companies and alternative currency schemes. It was then the perfect setting to see the different imaginaries, materialities and political economies played out at once (Rella 2019, 2020).

Since I wanted to test the extent to which a project on digital money could employ similar methods and concepts as research projects in critical logistics, I asked a professor in the department for feedback. I was told that yes, this analogy could hold critical purchase. However, to capture it, I should 'get to the control room' where money is actually moved. Hence, I started attending industry conferences and expos primarily as networking sites, where I could recruit my informants such as bankers, software developers and marketing specialists. Alongside this in-person recruitment, I started sending messages via email and social media to practitioners in the field. I sent 120 recruitment emails and 53 LinkedIn messages to current or former Ripple employees, and to individuals and press and PR offices of financial and software institutions that were clients and providers connected with Ripple.

Three realisations, however, brought me to redesign the approach. First, there were issues of access and gatekeeping that are well known for anyone researching elites. In studying the very rich, very powerful and the technical experts, traditional forms of ethnographic engagements might prove themselves unfeasible or inadequate both practically and ethically (Nader 1972; Gusterson 1997). Practically, institutions like banks and tech companies are fraught with institutional, economic and knowledge-based barriers for access that can take years to overcome, often only thanks to fortuitous personal connection or based on one's idiosyncratic background

(Thomas 1995; Seaver 2014). Ethically, the ethnographic ethos of giving voice to one's informant might work to reinforce, rather than question, the power that informants already have (Pierce 1995). Ripple's software developers were open to discuss regardless of seniority, and with around five of them I managed to build rapport and trust through follow-ups and informal email exchanges. FinTech companies such as payment providers, cryptocurrency exchanges and software companies were also available to discuss and lead to insightful conversations. A wholly different tune, however, was played by banks: of the 120 emails that I mentioned above, only around ten bankers replied. Non-disclosure agreements and patents, furthermore, significantly restricted the scope of the answers. The process of recruitment and attendance of conferences made apparent what Dos Santos (2018: 103) calls 'trial of access'. In Seaver's words (2017: 7), access 'is a protracted, textured practice that never really ends, and no social scene becomes simply available to an ethnographer because she has shown up'.

Second, I came to realise that industry conferences and expos were not an entry point *to* the field, but an integral part *of* the field itself. Temporary exhibitions and trade fairs are one of the paramount venues where FinTech markets are constructed, produced, maintained and reproduced, and where 'tournaments of value' take place (Anand and Jones 2008; Moeran and Pedersen 2011; Aspers and Darr 2011). Figure 4.1 depicts one of the eight industry conferences and expos that I attended between 2017 and 2019, where each company had a dedicated space, the size of which depended on the seniority of the company and on the amount of money they paid for it. Different spaces were used for different purposes, such as exhibiting, discussing, and arranging formal and informal networking events.

It became increasingly clear to me that the nitty-gritty of the logistics of money was just as important as the speculation, myths, enchantments and promises held by technological innovations, as noted by several ethnographers of infrastructure (Winner 1984; Thrift 2001; Harvey and Knox 2012; Larkin 2013; Anand et al. 2018). This led me to let go of the sense of frustration or expectation connected with how many informants I would have been able to recruit during the few days each expo lasted. This approach allowed me to expand my focus and juxtapose the expo floor to the control room, rather than striving to get to the latter through the former.

The third realisation was that, maybe, there was no one control room to begin with. The digital infrastructure that I was studying emerged as much more heterogeneous, fragmentary and contingent than I originally imagined. Instead of one or more control rooms, held at one or more banks, there were multiple 'legacy' payment infrastructures using different standards for different types of payments, and each of these instances had a tailor-made synchronisation system. There was not a unified network with a master switch that one could flip to enable or disable a specific connection, but many switches, the shape of which depended on the pair of organisations that were being connected each time. Offices around the world were not like ports in logistics but more like *pied-à-terre* for the organisation: as one informant put it, 'you cannot be sitting in San Francisco and sell to the world, not in banking' (Interview 29th May 2019).

Fig. 4.1 FinTech Trade Fair. June, 2018. (Source: Author's own)

Again, this realisation came with a mix of relief and renewed anxiety. I was relieved that now I did not have to pry open the doors of a control room, but then I was caught in the anxiety of a potentially endless list of locations. Each local office was potentially connected and relevant to my topic and case study, but they were all so far apart from each other that it was not thinkable for me to cover all or even most of them. A conceptualisation of my fieldwork as networked and fieldless entailed embracing the research subjectivity of the 'scavenging ethnographer' (Seaver 2017). This type of fieldworker collects and analyses data through 'chains, paths, threads, conjunctions, or juxtapositions of locations in which the ethnographer establishes some form of literal, physical presence, with an explicit, posited logic of association or connection among sites that in fact defines the argument of the ethnography' (Marcus 1995: 105). I started to think that what mattered were the material or discursive connections made either in-person or online, between different locales by the active production of a multi-sited field.

Furthermore, technological decisions and design and standard choices were made as frequently through online conference calls and forums as in in-person meetings. The software developers I studied made most of their decisions through fortnightly calls via Zoom. Ethnographers of the Internet have long argued against a strict dichotomy between online and offline worlds. An ethnography *in* and/or *of* digital infrastructures (see also Marcus 1995), then, must show how online and offline settings are mutually constituted (Hine 2000; Kozinets 2011). The problem

of how to bridge and inhabit different spaces was not only my epistemological and methodological problem, but also my informants' practical and material problems of establishing a cross-border payment infrastructure. The multi-sitedness of the fieldwork, and the peculiar relationship between online and offline spaces it is based upon, also create a peculiar relationship between fieldwork and time.

4.3 Time in the Fieldless Field: When to Start, When to Stop, When to Go

Blockchain? Blockchain is so 2016! (An informant, June, 2017).

In digitally mediated environments, time plays an important role not only in the data collection strategies, but also in the nature of the data itself. Hine (2000: 23) argues that one of the resources afforded by online ethnography is that 'ethnographer and participants no longer need to share the same time frame.' However, this lack of synchronicity creates challenges of its own. I identify four ways in which time played both a positive and a negative role in shaping my fieldwork: time as a medium, time as past and memory, time as hype and attention cycle, and time as a limit.

First, time influenced the rhythms of recruitment, data collection, and analysis. Scheduling online interviews with informants who were several time zones away made it visible that digitally mediated interviews are far from frictionless. The mismatch in time and location often means that the informant did not know what I knew and the other way around. In a 'traditional' ethnography, interviews can be both built and expanded on previous interviews, because both the researcher and the informant keep mental and written records of past observations and interactions. This is far less likely to happen when interviews are carried out remotely or without the previous building of rapport with the informant through physical proximity (Hannerz 2003). On the one hand, digitally mediated interviews with informants in far-away locations made room for more paced data analysis: the ostensibly empty time between message and reply, between scheduling and interviewing, and between interviewing and follow-up can be used to start reflecting on the data already collected. On the other hand, since interviews were not built on one another, I could only make sense of data in earlier interviews through the answers and notes collected during much later chats. For example, my fieldwork started in summer 2017, but I managed to recruit one of my key informants in May 2018. Only thanks to his insights, I was able to retroactively make sense of previously collected material, as well as to structure subsequent interviews in ways that were beneficial to my project.

Hence, the lack of synchronicity and physical proximity allows for projects with a much broader geographical spread, but it also poses challenges in terms of building rapport and access. Key informants and gatekeepers acquire an even greater importance in this respect yet relying too much on individual informants can incorporate bias in the overarching narrative. The key informant I mentioned above

laid this risk bare in front of me when I recruited him. In his affirmative reply, he asked me what my preconceived ideas on cryptocurrencies were, because, he said, everyone has one or more preconceived ideas. He then said, 'nearly everybody in this space is in self-promotion mode, and it might be hard to discern what their real agenda is behind self-promotion' (Interview 15th of May 2018). As said above, then, the power asymmetry deriving from access to knowledge and resources must be managed carefully: taking informants at their word might mean becoming a promotional echo chamber rather than a social researcher.

Second, time plays a role as past and memory. The Internet is a living archive, both of present and of past interactions (Chun 2013). Digital ethnographies are very often used in asynchronous ways that strongly resemble archival research, rather than direct participant observation (Tunçalp and Lê 2014: 70). Through the so-called Wayback Machine it is possible to access versions of websites that are no longer online (Rogers 2013; Arora et al. 2016). Through it, I traced a genealogy of Ripple through the content that was published on the page but no longer visible. Figure 4.2 illustrates the Wayback Machine's graphic interface. The search bar gives the address of the archived page. The timeline provides the number of times that page was changed or updated each year.

Archival sources also provide an important resource to 'route around' (Seaver 2017: 10) constraints to access and gatekeeping. However, digital archives are by no means universal or frictionless to access. Even a cursory research on the Wayback Machine, in fact, reveals multiple dead ends and points where data was lost without repair, especially in the case of online forums. Hence, it is important to be constantly wary of the risk of digital ethnography becoming a new form of 'armchair anthropology' (Tsuda et al. 2014; Hine 2017). Whenever I could, I would contact members of the online community whose archive I was perusing, even though I did not always receive replies to my messages.

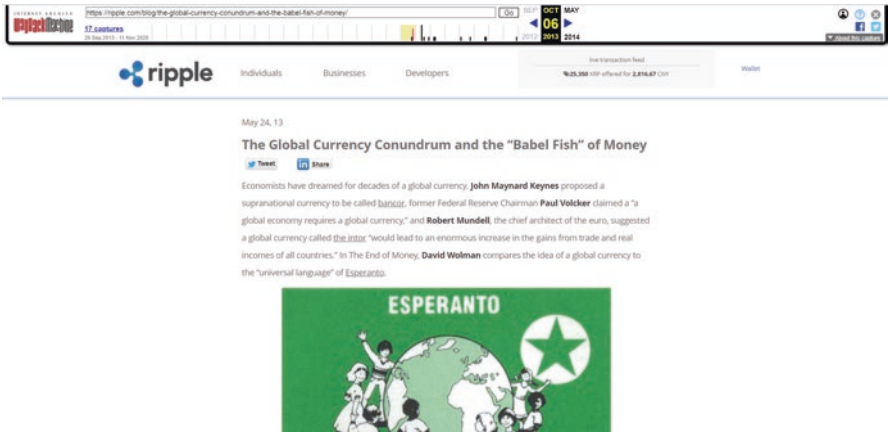

Fig. 4.2 The Wayback Machine's visualisation of a page on the Ripple website. June 2017. (Source: Internet Archive n.d., Archive.org)

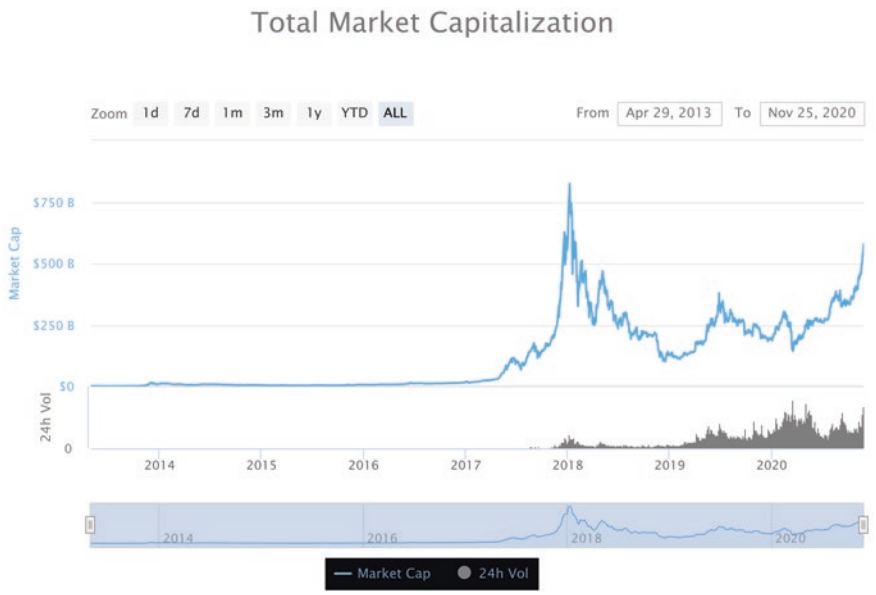

Fig. 4.3 Total market capitalisation of cryptocurrency markets, 2013–2020. June, 2019. (Source: Coinmarketcap 2019)

Third, time figures as a cycle of attention and hype. Cryptocurrencies and blockchain technologies have gone through wild oscillations in value, popularity, and public awareness between when I started this project in 2016 and the time of writing. Figure 4.3 shows the total market capitalisation of the cryptocurrency markets from 2013 until 2019. Until 2016, one can see that the size of the market remained extremely contained, even though it was already the object of public attention and scrutiny. In 2016, cryptocurrencies were on the rise, but still a quirky niche conversation topic for most, and a research topic mostly for computer scientists, some monetary and financial economists, and very few social scientists. In 2017, attention picked up momentum. Bitcoin almost hit the $20,000 price threshold in December, and the cryptocurrency market almost reached the trillion dollars in collective market capitalisation. However, already in late January 2018, prices started to drop and the so-called 'crypto winter' set in. The amount of floor space in expos and trade fairs also shrank quite visibly, and news started covering companies that went bust more than those who were launching their operations. Every peak was seen by enthusiasts as ushering in a new world of digital money, and every drop was seen by the sceptics as the bursting of a speculative bubble. There is even a website that lists all the times Bitcoin has been declared dead by technological and financial commentators, that has now surpassed 350 obituaries (99Bitcoins 2019). If I had had an intensive fieldwork for a 6-month period at any point between mid-2017 and now, I

would have been more prone to seconding the hype of the moment and I would have missed important trends that would have made sense only if put in context.

Fourth, time acts as a constraint and limit. The boundlessness of hybrid online-offline, and digitally mediated research also has implications when it comes to putting a halt to the data-gathering effort (Reich 2015). The fieldless fieldwork can just as easily become the endless fieldwork. Single-sited intensive ethnographies, however long in their duration, have an endpoint, the crossing of which helps the researcher to take the necessary distance from the field itself before analysis and writing up. A fieldless fieldwork, conversely, lacks not only the topographical, but also the chronological and temporal boundaries typical to conventional fieldwork. I could be 'in the field' during a conference call, remain 'in the field' immersing myself in archival documents, then be 'out of the field' while I was teaching, and then go back into the field several days later for an interview or a trade fair. While this allows data collection and analysis to go hand in hand, a lack of a true boundary between beginning and end of fieldwork also means that the process of data collection could, potentially, go on endlessly. Theoretical saturation, hence, plays a pivotal role in determining the endpoint of data collection, defined as 'the point […] at which theorising the events under investigation is considered to have come to a sufficiently comprehensive end' (Sandelowski 2008: 875).

In my case, given the dynamic nature of my research topic, I had to work on two binaries. On the one hand, I had to isolate theoretical themes that I judged to be relatively stable in the whirlpool of information surrounding blockchain technologies and cryptocurrencies. The decision over theoretical saturation was, then, operated on these lines. On the other hand, I had — and, at the time of writing, still have — to keep my eyes and ears open to the latest developments in the industry, the most recent regulatory measures introduced, and landmark court cases, as well as to the daily oscillations in the price of crypto assets. This prevents a thesis written on this topic from becoming old before it is even sent to print; yet, keeping the door to the field constantly ajar can prove itself stressful.

4.4 Conclusion: The Relevance and Contribution of a Fieldless Approach to Fieldwork

Drawing from a research project on digital payment infrastructures, this chapter argues for a radical expansion of the methodological tools employed in multi-sited ethnography. The chapter sketches the spatialities of FinTech and Blockchain technologies as hybridised between online settings like online forums and conference calls, and offline settings like trade fairs and expos. Rather than drawing a demarcating line between what is or is not a legitimate field site, this chapter embraces a multi-sited approach of following 'the people, the things, the metaphors' (Marcus 1995) and to be more attentive to co-presence than to co-location (Beaulieu 2010). Rather than seeing one specific location (the fair) as an instrumental tool to

gain access to another location (the control room), this chapter shows that both the fair and the control room are part of one and the same fieldless field site, a site that is 'constructed rather than discovered' (Tunçalp and Lê 2014: 60). The field site becomes a rhizome: 'A rhizome has no beginning or end; it is always in the middle, between things, interbeing, intermezzo' (Deleuze and Guattari 1987: 25). In so doing, my fieldwork drew upon and, hopefully, contributed to current research in organisational and institutional anthropology of meetings, conferences, trade fairs, and other temporary gatherings (Høyer Leivestad and Nyqvist 2017).

Through this investigation of the hybrid spatialities of digital money, my research illustrates the complexity and nuances associated with gaining access to a field populated by economic and technical elites like bankers, financiers and technologists. The type of ethnographer subjectivity that is required in this setting is less similar to a detective and more like what Seaver (2017) calls 'scavenging ethnographer'. The radically multi-sited approach to fieldwork that this chapter describes, then, is also a strategy to 'route around' powerful gatekeeping groups and access knowledge about a specific subject, hence contributing to literature on elite research and 'studying up' (Nader 1972; Gusterson 1997; Seaver 2014).

Lastly, the mix of online and offline methods that this chapter outlines points to some specific challenges connected with the temporalities of recruitment, data collection, and analysis in contexts without synchronicity and co-presence between researcher and informants. In so doing, this chapter hopefully contributed to ongoing debates on digital methodologies in social sciences (Horst and Miller 2012; Marres 2017; Ash et al. 2018). As technology evolves and redefines social encounters, and a post-Covid-19 world poses new practical challenges to the in-person encounters that underpin social research, fieldless fieldwork and digitally mediated ethnography will acquire new salience and become more and more frequent, making the contribution of this chapter especially timely.

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Part II
In the Field: Ethics, Practice and Positionality

Chapter 5
Recruiting Participants: A Socratic Dialogue on the Ethics and Challenges of Encountering Research Participants

Christoph Doppelhofer and James D. Todd

Abstract In this chapter, we stage a Socratic Dialogue about a taken-for-granted and undervalued empirical research process: participant recruitment. Being enveloped in different spaces, communities, and research contexts, we reflect upon ethical dilemmas which emerge out of recruitment processes. We offer examples from our research practice and consider our experience of overcoming our own anxieties around researching in distinct communities. In the chapter, Todd discusses his experience of 'recruitment' through collaboration *with* participants and organisations which support them. By reflecting on his research with(in) young trans communities and the feminist and participatory ethos which guides this process, Todd explores participant engagement in a context wherein trust, long-term collaboration, trans allyship and social justice and the provision of 'safe spaces' are necessary recruitment and research components. In contrast, Doppelhofer explores his experience of conducting research in open, public spaces wherein *ad hoc* participant recruitment takes place in an intrinsically international tourism and heritage context. He examines the difficulties of approaching potential tourist participants, gaining access to heritage stakeholders and policy makers, and overcoming cultural barriers.

By reflecting on our experiences of recruitment, we consider our positionalities in the research site and beyond — Todd as a queer, cisgender scholar in trans spaces, and Doppelhofer as an enthusiast and follower of the same cultural phenomenon he researches. We elucidate what participant recruitment means in different contexts and what ethical, practical, and theoretical issues one might encounter, considerations that must be made when implementing particular recruitment strategies. In doing so, we generate knowledges out of our respective relative failures and successes recruiting.

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5.1 Introducing Participant Recruitment and the Socratic Dialogue

Participant recruitment, which we understand as a set of strategies designed to reach, engage, and ensure the consent of research subjects, constitutes the basis of every ethnographic and qualitative research project engaging those other than the researcher. Participants — or informants, co-producers of knowledge, research subjects, or other myriad forms of 'the engaged in research' (as opposed to researchers as 'engager(s)') — are recruited through strategies and means which vary according to the researchers' methods and methodologies, epistemological approaches, and desired level of participant reciprocity and co-design. The *diversity* and *impact* of such strategies, however, is rarely acknowledged or reflexively interrogated within geographical or qualitative research literature outside of feminist interrogations of research processes and methods (see Campbell et al. 2014). Indeed, as most handbooks, guides, and research articles only superficially engage with recruitment processes, there is little guidance available in the human geography or social science canon (Hawkins 2016). Existing literatures that touch on participant recruitment might, for example, elaborate solely on their (often surface-layer) identification of potential participants and stakeholders and their characteristics, and the researchers' choice of method(s) for recruitment (Dunn 2016; Hennink et al. 2020). Such literatures have glossed over, or even ignored, crucial moments where participants are approached and encountered and have not recognised recruitment as constant processes that must be continually and reflexively examined and re-formed by researchers. As such, academic work that focuses on recruitment mainly explores participant representativeness and research quality (see Alto et al. 2018; Czepkiewicz et al. 2017), or advocates for a plurality or innovation of recruitment strategies (see McCormack 2014). As a result, the impacts of the researchers' situatedness, positionalities, intersectional identities over their interaction with the *recruitment* of participants, and indeed the selection of research spaces and the maximising of participant empowerment are largely under-examined in academic texts. This failing is often to the discontent of doctoral researchers and others who are left to muddle through with their knowledge of the ethics, potential procedures, and challenges of participant recruitment assumed as full. Doctoral researchers, we argue, are rarely required to undertake sustained critical reflection on their recruitment approaches and methodologies.

Taking note of these absences and failings, we present a reflective and reflexive Socratic Dialogue (SD) which builds connections between our distinct research approaches, knowledges, and strategies for recruiting and working with potential

participants. Named after the Greek philosopher Socrates and his method of *Maieutic* ('midwifery'), his eponymous dialogues, rather than teaching one particular truth, aim to stimulate the ability to bring forth knowledge through one's own reason and thought and to advance one's own latent ideas into consciousness through dialogic exchange (Plato, *Theaetetus*, 150b–d). The dialogue we present here deviates from classical SDs, in which Socrates heckles his interlocutors and plays devil's advocate. Instead, we follow the neo-Socratic tradition of German philosophers Nelson (1922/1949) and Heckmann (1981) who conceptualised SDs as offering 'attempt[s] to come to a common answer through systematic deliberation about a fundamental question' and a 'systematic reflection upon experience' (Kessels et al. 2009: 36; Turnball and Mullins 2007). Through a 'conversational' approach, also adopted in more recent texts exploring research methods in human geography (e.g. Gorman-Murray et al. 2010), we build common consensus and knowledge through joint reflection.

In the following section, we elaborate on our research contexts and practices to illustrate the distinctions between our participant recruitment approaches and the reasoning behind our recruitment choices. Our dialogue does not follow a rigid structure or flow; instead, we present here an unfolding thinking-through of key issues and nascent points of interest. We hope, in turn, that our dialogue sparks conversations amongst readers around the ethics and challenges of participant recruitment.

5.2 Dialogue Exploring Our Experiences of Participant Recruitment

5.2.1 Introducing Our Research

James (J): Christoph, I would like to hear more about where your research takes place.

Christoph (C): I am researching how heritage landscapes are re-imagined through imaginary worlds created in popular culture. To do so, I explore the filming locations of the HBO fantasy series *Game of Thrones* (*GOT*). Newly emerging on-site performances, tourist offers, destination marketing, and the sharing of these experiences on social media, create new diegetic spaces that reshape those previously existing heritage landscapes for both *GOT* fans and other stakeholders. These individuals include local populations and those uninitiated, who might be unfamiliar with the narratives of the series yet still emulate and reproduce its iconography through tourist performance (see Urry and Larson 2011; Roesch 2009). My fieldwork took me across various public heritage sites in Northern Ireland, Croatia, and Spain where I observed and interviewed different stakeholders to find out how this media-induced phenomenon alters the perception

and identity of heritage landscapes and the role they play as public stages for living out fantasies.

J: I can imagine there would be a diverse set of people.

C: Absolutely. I encountered a wide variety of participants, necessitating a range of recruitment strategies. Participant recruitment was therefore not only an essential process for obtaining research data but also an *ongoing struggle* as I grappled with various participants' cultural backgrounds, stakeholders, and settings. For some participants, such as policy makers and heritage and tourism authorities, recruitment was a multi-step process with weeks and months of planning ahead, whilst for most others, it was conducted in a matter of seconds, depending on my ability to catch them in the 'right moment' while they were visiting filming locations. James, who were your participants and in what contexts did you engage with them?

J: My research primarily took place in spaces designed for young trans people's safety and wellbeing. Consequently, I worked with participants in settings wherein they were mostly already emotionally embedded. In these sites, I engaged in participatory action research (PAR)-informed methodologies — approaches that attempt to deconstruct power relations by both empowering participants as co-creators of knowledge and developing an action-focused agenda. Approaching the research through a participatory focus allowed me to follow an ethos of collaboration at every stage from design to dissemination (Pain and Francis 2003). In my research, this involved undertaking creative and collaborative workshops and in depth, one-to-one oral history, and creative interviews with young trans people aged 14–25. Similarly to your research, then, the spatial contexts in which I worked were *exceptional*, not simply through their unique social and cultural connotations and potential for affirmation and friendship, but in their offering of a space of relative safety and respite from societal hostility. An important dynamic to mention early on in our dialogue is that from a traditional perspective interrogating 'insider-outsider' relation, I might be seen as an 'outsider', as I am not trans and was therefore often the only cisgender (cis) person present in the research space. Additionally, I also played a key role in crafting and maintaining the research sites, which had to be maintained primarily as 'safe spaces' — sites that are cultivated as spaces and times to develop communities for restoration, resilience, and resistance away from hostility and oppression (see The Roestone Collective 2014) — for young trans people. My participant recruitment processes, as a result, had to reflect on and respond to these dynamics. I think it would be fair to say, Christoph, that we both explore how our participants experience their lived worlds, but with differing emotional and political stakes in our research sites.

5.2.2 *Differential Research Agendas and Practices*

C: Both of our research *agendas* present very different ethical dilemmas that need to be confronted throughout our research *practices*. Let us begin by thinking through what each of our participant recruitment strategies and practices looked like.

J: I will start by saying that recruitment was a very long process for me, not least because of myriad ethical dilemmas. I spent a lot of time considering the everyday lives of trans people by engaging with academic literature and exploring young trans people's creative work and writings whilst designing the project. Ultimately, I decided that to ensure that the participatory ethos of working *with* young trans people was maintained at the core of my research praxis, it was necessary to collaborate with, and embed myself with(in), an organisation directly engaging with trans people. Regular dialogue ensured that I was putting my participants' needs and concerns (and those of wider trans communities) at the forefront of my work. I could seek input from trans facilitators and people with a stake in young trans people's lives and tailor my research practices to fit the policies and practices of the organisation.

C: I imagine that establishing contact and researching the everyday experience of young trans people as a cis researcher must have been complex. How did you go about building trust to enter these very sensitive spaces?

J: I established a close working relationship with Gendered Intelligence, a national community interest group supporting young trans people in the UK (see Stewart 2018). I held many conversations with the organisation's leaders and youth workers about the purpose of my research and what had brought me to it, what the research might look like and involve, the spaces it might create, and where the voices, stories, and creative work of my participants might travel. I was also interested in emphasising my hopes that the research and its practices and spaces would benefit both young people attending research sessions and the organisation alongside trans policy in the UK more broadly. Although these were difficult issues to think through well ahead of the empirical research stage, collaboration enabled me to think through my own positionalities and situatedness, ethical concerns related to collaborating with young trans people (particularly those under 18), and the research methodologies along more practical lines. I was able to interrogate my shifting positionalities in trans spaces and the driving forces of my research praxis. In the end, the collaborations I developed enabled me to work in trans and queer spaces in urban centres in London and Scotland. What was the initial idea behind your rationale and approach towards recruitment, and thinking about recruitment for the first time?

C: As my research involved several different locations and recruiting 'on the spot', it was largely impossible for me to engage with a single gatekeeper, unlike your research, James, where a central gatekeeping organisation was an ethical and administrative necessity. With my background in archaeology and cultural heritage management, I had limited experience in participatory methods, so

recruitment was perhaps the most stressful and anxiety-inducing aspect of my research. Given that this was the first time I conducted ethnographic research, I became so focused on the technicalities and formalities of interviewing, observation and proper ethical conduct that, aside from the key stakeholders who I could identify and approach before going into the field, I had very limited strategies in place for recruitment itself. For me, thinking about the on-site recruitment was — quite reminiscent of the literature — underrepresented in my preparation. In fact, I had to come up with it in the field!

5.2.3 Participant Recruitment Anxieties and Challenges

J: Were there any particular strategies you employed to recruit your participants in the field?

C: I would separate my recruitment practices and experiences into two distinct categories. The first involved approaching day visitors to sites on an *ad hoc* basis, whilst the second involved recruiting policy makers and stakeholders within the heritage and tourism sector. Although, I could often pre-arrange the latter, both sets of recruitment brought their own challenges and frustrations.

For the on-site interviews with tourists, I had adopted no plan other than what most of the literature suggested: Go 'in the field' and 'find people who know the answers and can give you the answers', as it was sarcastically summarised by Phillips and Johns (2012: 143), who subsequently glossed over the recruitment process too. Having only limited understanding of what recruitment might look and feel like in situ, this was not as easy a process, as most literature would make it seem. To put it in a way thematically fitting our dialogue: I felt like an ill-prepared Socrates, entering a public forum, harassing people who I (and definitely some of them) thought had better things to do than talking with a PhD student during their well-deserved holiday! My initial recruitment strategies ranged from bluntly — and indeed clumsily — approaching visitors with a generic opener along the lines of 'would you have a few minutes?', to targeting specifically those who appeared to linger at the site rather than to be on the move. I must have looked quite menacing at times, running towards them with my fieldwork gear. In what felt like a desperate attempt to pique potential participants' interests, I even started to offer biscuits and information about the site they were visiting. At least the latter approach felt more natural and comfortable due to my past and present work experience as tour and museum guide.

What was hardest to overcome was a constant, lingering feeling that, depending on the day, would range from feeling social awkwardness to experiencing anxiety when trying to initiate contact with potential participants. Some days, I could not bring myself to approach anybody — even though I had managed to do so just the day before. Just the thought of walking up to a stranger would cause discomfort to me. Often, I think, there was a direct correlation with my previous success rate. For example, once somebody had shown disinterest or had behaved

dismissively when I approached them for an interview, I felt that I was pestering people with my research, rather than seeing it as a knowledge-sharing and knowledge-making opportunity. It took some time to overcome this and, in all honesty, even when I started to have better strategies in place and felt more comfortable in my role as a researcher, my previously described discomfort stayed with me.

J: Likewise, I faced many challenges whilst recruiting participants, whether ethical, emotional, anxious, or otherwise. Recruitment, as it should be, was an *ongoing process* that I had to continually return to and negotiate between myself, the diversity of participants and their needs, field sites, gatekeepers, and other actors implicated in the process. My own anxieties and anxious orientations to research and research spaces, contributed to the messy complexity of this process (see Todd 2020). I know that such mental health concerns all too common amongst early career researchers like us.

One of the most difficult-to-overcome anxieties I faced was related to the necessity of doing justice to the communities and spaces I was researching in. I wanted to ensure that my recruitment not only allowed me to raise the voices and stories of young trans people, but that a diversity of trans youth voices were represented in the research. I overcame this by recognising that research can never be fully representative, particularly when limited by the confines of funding and time constraints. However, I situated myself in a diversity of trans spaces and focused some of my recruitment strategies on working with or recruiting particularly marginalised or underrepresented young trans folks (including trans women, non-binary people, and people of colour), and advertised the research both personally and via my partner organisation through multiple and intersecting platforms including social media, flyers, verbal communication, and email advertisement.

C: It seems that part of this anxiety you are describing comes from dealing with a multitude of (self-imposed) responsibilities involving your research while also being expected not to fail at any of them. Do you think that the expectations of producing 'positive results' and being coerced into presenting yourself as a successful researcher contributed to this pressure and anxiety around recruitment?

J: I think it added to it. One thing we do not hear enough about in academic research is a discussion around failures, mistakes, and changed directions. As Harrowell et al. (2018: 236) tell us, 'there remains a need to acknowledge openly that failure is in fact an everyday, and indeed powerfully productive element of geographic field work'. This is particularly the case, I argue, in the neoliberal, emotionally demanding academic context which asks us to project the image of a linear, always-already successful research process, even when working around emotional labour intensive or distressing research experiences, or whilst still training in social research praxis as early career researchers.

I experienced anxiety and pressure to succeed with particular intensity when hearing my participants' most difficult stories and narratives. I knew that doing justice to a diversity of voices was one of the cornerstones of how I saw my research, but this commitment also constituted an emotionally demanding inter-

nal pressure and embodied tension. Being a cis researcher entering trans spaces, I had to remember that my presence was subject to gatekeepers' and my participants' comfort, and my priority was to ensure the safety and wellbeing of trans folk entering and accessing those spaces. This was less of a concern for one-to-one work or workshops set up outside of already existing spaces and times for the young trans community. In these sites, I knew that everyone was present there because they wished to take part in the research for catharsis, enjoyment, or to contribute to furthering knowledge around trans experiences (although I still maintained spaces and mechanisms for participant rest and withdrawal, also facilitated by collaborators).

I am keen to hear how you coped with and overcame rejection in your participant recruitment, given that your strategies could rarely be planned in advance. How did the strategies you developed for this potential rejection vary according to your interactions with different stakeholders in the field?

C: My anxiety around rejection was less prevalent when contacting identifiable gatekeepers and experts whom I could email in advance, or with local business owners and tour guides who I knew are used to being approached and talked to by strangers. The literature prepares you for some rejections or a lack of responses (Crang and Cook 2007); however, nothing could prepare me for my biggest period of anxiety: the lead-up to my fieldwork in Dubrovnik, one of my main research sites. While I thought I had everything under control after a smooth first 'campaign' in Northern Ireland where I had many positive responses to my interview requests, nothing seemed to work when I employed the same strategy in Croatia. Nobody I had emailed beforehand — often months in advance and multiple times — returned any of my requests, or they claimed they had no information for me. As the days drew closer to my departure, the pressure was suffocating; I feared that my entire PhD was falling apart in front of my eyes! In hindsight, this turned out to be an unwarranted fear, as I developed more contacts and collected more data than I wished for once in the field.

J: What happened in the field site to make this success the case?

C: Once I arrived and familiarised myself with Dubrovnik, many things fell into place. I think this is an important advice for anybody who struggles with participant recruitment in international, unfamiliar contexts: the power of being in the place, talking face-to-face with people, and getting to know the location, its customs and etiquette cannot be underestimated. Fieldwork is — and here I must wholeheartedly agree with the literature — messy (Harrowell et al. 2018; Marshall and Rossman 1989: 21). Often you must find a single person — it may be a tour guide, a shop- or innkeeper, or even a random local site visitor — to start a snowballing process. This learning through 'being in a place' also helped greatly in my recruiting of day visitors, and I became more comfortable to approach strangers. It is important to keep in mind that while one must prepare as much as possible in advance, certain aspects of fieldwork, like the ones described, will only unfold once you are doing them. Also, I think my experience of failure and adaptation illustrates that there is not, and cannot ever be, a one-size-fits-all approach to recruitment. While the same email templates and intro-

ductions led almost always to immediate success in Northern Ireland, this did not apply to the context of Croatia. Research requires a constant updating and reviewing of your recruitment strategies.

5.2.4 Positionalities

C: James, you already indicated the importance of your positionality. I want to hear more about how your identities and 'insider-outsider-ness', as you might term it, interacted with your participant recruitment?

J: Again, I was quite anxious that, in certain research settings, I was entering spaces crafted by and for trans people that should not be controlled by my research agendas as a cis researcher. As a result, I made sure that each time I sought the permission of young trans people to be present there through differing techniques. I volunteered and went along to young trans community spaces and events to introduce myself to their dynamics and practices and to familiarise myself with potential participants. First and foremost, I made sure to introduce myself and the research in easy-to-understand language, leaving plenty of space and time for questions and concerns, whilst in all group settings the young people could also approach trans youth workers or facilitators to voice any thoughts or potential discomfort. I made sure that my voice was never prioritised in any research setting, and a separate quiet space was always available. When working in an already-existing space (such as regularly occurring community events) my workshop took place in a secondary room, to give potential participants and others present the choice to ignore my work entirely or come and go from the research activities as they saw fit. In group settings, I always made sure to stage a conversation or activity around what a cis researcher entering their space meant and felt like for potential participants. These mechanisms formed part of my recruitment process, given that participants *continually consented* to taking part and could withdraw that consent at any time.

However, in terms of my positionalities and multiple identities and subject positions, it is not simply enough to think through my position or 'outsiderness' as a cis researcher, or my relative 'insiderness' as a queer person myself, or someone sharing a similar age to participants. Indeed, there are many ways that researchers can relate to or interact with potential participants, some of which we can never be fully aware of, or even hope to fully interrogate. Indeed, despite the potential for understandable research fatigue, discomfort, or lack of trust around cis researchers in trans spaces or communities (see e.g. Pearce 2018; Vincent 2018), participants frequently told me that they appreciated the solidarity I displayed, my knowledge of trans issues and affirming languages, the care in which I treated them and their stories and spaces, and my attentiveness to their needs and concerns. By being up front about the fact that the research was iterative and partially designed with their stories at the forefront of my research practice and methodology when recruiting, I hope that I allowed participants to feel assured

that they were part of the decision-making process. Again, I also shared my own queer stories which participants occasionally drew on to develop their own storytelling. In many ways, my position should be looked upon and informed by this additional layer of understanding and solidarity I attempted to constantly embody. Without this commitment to my participants and to trans allyship, I could not have felt comfortable engaging in participant recruitment at all. With participants' needs at the core of everything I did during the research, I was able to recruit and work with young trans people in a way that felt authentic and less intrusive or imposing. However, the extent of my awareness of these dynamics and participants' feelings about my presence and the research generally were always incomplete.

C: Being aware of and making use of my positionality was also *helpful* in my recruitment process. One of the strategies I employed to overcome my previously described anxieties and aversions of approaching potential informants, was directly connected to my research subject and my positionality within — I too am a *GOT* fan! I remember a case where some tourists happened to arrive at a filming location at the same time as a costumed *GOT*-tour group which allowed them to join to pose with props and re-enact scenes. The excitement, surprise, and joy they displayed when offered this opportunity was contagious — especially for somebody like me who had participated in one of those tours in the days before. We began casual conversations while participating in this spectacle, naming favourite scenes and characters from *GOT* and speculating about the upcoming last season of the show. This shared experience, which had unconsciously advanced our reciprocity, facilitated the perfect space for an interview. There was a real connection through our shared fandom and a sense of community. Our shared view on the world, much like the queer perspective you described with your participants, became intrinsic to the recruitment process and the research beyond.

In later research and recruitment encounters, I began using this shared knowledge and experiences of the heritage sites as an *entry point* to dialogue with participants. Through this (real or imagined) shared sense of connection, I was able to overcome my apprehension and sense of 'invading' their private space. Identifying myself with, or at least becoming aware of, the motivations of my interviewees helped me to facilitate a better space through which to elicit far more in-depth stories and information. Of course, this also impacted my sampling. I would say that there was a certain 'type' of participant I felt most comfortable around, namely those I could name and identify as *GOT* fans through their clear and obvious performances and comments. Indeed, I would mainly catch the most vocal performers at the site, while those who do not engage in a certain way remained excluded from my comfort zone. I constantly had to remind myself to approach as many different people to catch the necessary nuances and diversity in engaging with the sites. Did you have any similar experiences?

J: I found it helpful to remind myself that my queerness, political commitment to trans allyship, social justice, and queer solidarity figured in my *queering* of

participant recruitment[1] (see Browne and Nash 2010; Gorman-Murray et al. 2010). As Waitt (writing as part of Gorman-Murray et al. 2010: 103) notes, 'recruitment for queer projects often relies upon essentialised identities of lesbian, gay, bisexual, [and] transgender […] [with] implications for what is then concealed and disclosed' by participants in research encounters. It felt both obvious and important for participants to describe themselves on their own terms (whether their identities, names, and pronouns were stable or otherwise) and to recognise the expansiveness of gender diversity. I encourage other researchers to build similar practices of affirmation and recognition into their recruitment and research (see Vincent 2018 on developing ethical and empowering research with trans people). For me, queering research and recruitment meant building relationships of solidarity and developing platforms to empower young trans people to work at their own pace to share their voices and tell their own stories. Queering the research also meant recruiting by informing potential participants that the research would be conducted according to their terms and choices and, again, making a commitment to sharing my own queer stories and histories with participants and knowing when to step back and relinquish control of conversations so that trans voices took priority over my own. When recruiting, participants were encouraged to bring along objects to share their stories through. I often found that the best moments shared in research encounters happened when I told my own stories, or when my participants and I had shared similar experiences as queer folk (e.g. coming out, feeling constrained in certain spaces, crafting queer spaces, and reconciling our queerness in our youth), or indeed as young people, or students, avid readers, fans of music, or through other moments of shared understanding that became crucial in establishing layers of mutual understanding and nuanced conversation. Embodying a queer approach to my research and recruitment, in these ways and more, was a major part of overcoming my anxiety.

C: Too often I feel that participants are seen as a means to an end, as numbers, or as vehicles for 'data', which leads to alienation, and builds a barrier between researcher and participant. In my research, I often felt the relationship was one-sided, in the sense that I both depended on the goodwill of other people and continually questioned why participants would even be interested in contributing to my research. Participant recruitment involves employing and enlisting people for our own personal gains. They give us time, data, and personal details. Do they gain anything out of it? If so, what? If not, why should they bother? As your approach illustrates, it is helpful throughout the recruitment process to think about how to 'give back' to participants and develop reciprocity. When I reflect on my experiences, some of the most successful and insightful interviews I had were those where we discussed our shared fandom or those whom I could provide with more information on filming locations, travel tips, and local recommendations. Some of my interviewees expressed interest in my research and

[1] Following Browne and Nash (2010: 9), I avoid defining queer and queering here, preferring they be left open as terms that 'can and should be redeployed, fucked with and used in resistant and transgressive ways'.

asked for copies of any work coming from their data, showing a sense of personal pride to be part of a research project. This illustrates that sharing our co-created knowledge might in some contexts be both an adequate way of giving back and a means to bolster recruitment strategies.

5.2.5 Participant Consent and Forming Relationships with Participants

C: This leads me to something that I thought of numerous times — and I am certain for you it must be an even larger concern: consent. While I gave my 'recruits' an overview of what they are enlisting to, they have very little choice and agency over what happens afterwards and how I use their data. Tourists, who had only a few moments to decide if they wanted to participate in my study, were not able to consider and reflect in what way I might use their words. I always provided them with contact details in case they would reconsider.

Most of my encounters did not necessarily deal with any sensitive data and I took little personal information from participants. However, there were participants — mainly those having economic and professional stakes — for whom publishing certain quotes under their names or organisations would not be in their best interest, even though they had given consent initially. These participants included public relations officials and guides who talked about licensing issues surrounding copyrighted materials used in promoting and conducting tours. While important for understanding the subject of my research, I am aware that obtaining consent is not necessarily a free pass. Where the identity of said organisations and persons might have had negative effects, I had to make decisions on issues that needed to be confronted. Given that you dealt with a perhaps more sensitive topic and the need to be responsive to your participants, I am curious about how you navigated various ethical questions and dilemmas.

J: One of my key concerns was indeed around participant consent. I knew that my research was ethically complex, particularly because some of my participants were under 18, and it was not ethically appropriate to seek their parents' consent regarding their participation in order to maintain their safety and wellbeing, with a number of participants being not 'out' as trans beyond the spaces of my collaborative partner. Although I was able to obtain institutional ethical approval for this and all aspects of my research by following and adopting research council-recognised principles for the ethical recruitment of minor and by building additional procedures of youth verbal and written consent into my practice, I was still careful to ensure that all young people involved fully understood what it meant to participate in the research. The facilitation of the organisations I collaborated with were key in addressing this, as I became a guest (co-)facilitator in spaces exclusively maintained for and by trans people.

My first research encounter was a creative workshop around the theme of clothing, where participants created 'body maps' of their emotional and embodied experiences in relation to items of clothing and (gendered) clothing practices. Because of my status as a guest (co-)facilitator, I was able to articulate to a large group of trans people who I was, why I had entered their space, how I planned to work with them, and my interest in elevating their voices. Again, being present in a space where multiple activities were taking place also allowed me to offer participants a choice of whether to attend my planned creative activity or the other activities that were taking place at the same time. After setting up in the space in this way, I was able to discuss my positionality, and my presence in their space, and the consent procedure with the young people through participatory diagramming and group discussion. This initial experience, part of the recruitment process, strengthened my resolve in maintaining a sensitive, iterative, and responsive participatory research strategy. I was able to take this new ethos and sense of duty to the participants, their wellbeing, and their stories forward into future research encounters and spaces and times of participant recruitment.

C: It seems that you were able to create an intimate relationship with participants, given the time you spent thinking about their experiences and their wellbeing. In contrast to your research, the short nature of my research encounters meant that it was not possible to formulate such a personal relationship. This made it more likely, as mentioned previously, that I saw my participants as disembodied 'numbers'; indeed, at the end of the day, I knew very few of my participants' names!

On the surface level, I assumed that there was neither time nor a necessity of a longer trust-building process because my research focused on holiday experiences, a limited timeframe in an out-of-the-ordinary setting for both my participants and me. I think this absence of an emotional bond was detrimental to the recruiting and interview process, as this perpetuated my feelings of awkwardness and invasion into their leisure time. However, this turned out to be unfounded in some cases, as I would encounter several of my research participants multiple times — sometimes many miles apart — and got to know them better. On several occasions, I would run into the same tourists whom I interviewed previously. One group of visitors were thrilled to see me a day or two after our interview to show me photos of them re-enacting scenes — something they knew was part of my research. Others thanked me for the good recommendations for attractions and bars I gave them. Some even wanted to take a photo together with me on the 'Iron Throne', a prop-replica from the series that was exhibited at one of my research sites. In several instances, after I interviewed them, local guides even became recruiters for the project in a sense. They would introduce me and my research to their tour participants, calling me the '*GOT* expert' and sending them to me to be interviewed. By letting me participate in their tours for free and helping me to establish contact with numerous local authorities, these guides helped me to eliminate some of my previously stated awkwardness and anxiety for recruitment. In return, I was 'recruited' by said guides for their purposes. At one point, a guide spotted me at one of my usual fieldwork sites and deployed me as a 'living prop' for her guests to re-enact a scene from *GOT* for their Instagram

photos. One time, I was asked to help to shoot a promotional video for one guide's website. I became fully embedded into the spaces I researched and felt I was contributing to the perpetuation and reinforcement of the very phenomenon I was aiming to observe.

J: In a way, that is a perfect way to round off our dialogue: being attentive to the ongoing nature of participant recruitment, and the *possibilities* for research encounter that continually examining our participant recruitment practices can generate. Similarly to you, Christoph, as time went on, I also found myself increasingly embedded in the spaces I was researching in by, for example, re-encountering both gatekeepers and participants, playing a role in facilitating research/community spaces, and helping to craft and maintain their 'safe' conditions and uphold their queer and trans dynamics. I also found myself, in some cases, becoming increasingly entangled in my participants' stories and narratives. Though it is difficult for me to articulate what this felt like, I found that building a longer-term relationship with my participants, gatekeepers, and partner organisation over time allowed me to engage in, variously, voice-raising, friendship, solidarity, story-sharing, allyship, activism, and *commitment* — with(in) and to the young trans communities and participants I encountered.

5.3 Conclusions: The Ethics, Challenges, and Opportunities of Participant Recruitment

In this chapter, we have discussed our participant recruitment strategies to think through what participant recruitment means in different contexts. The chapter has raised ethical, practical, and theoretical issues one might encounter when implementing a particular set of recruitment strategies. We have problematised 'recruitment' as always-already constituting more than selecting and engaging potential participants, and have reflected on what it means, looks, and feels like to 'recruit participants'. In doing so, we have positioned participant recruitment as an ongoing, iterative process requiring the researcher to interrogate continually and reflexively their self, potential participants, and the field sites and spaces in which they research. Doing so, we argue, is an emotionally demanding labour requiring social science researchers' commitment to recruiting and working with participants through means which align with participants' experiences and desires for engagement. Recruiting participants ethically to do justice to the diversity of voices and experiences we engage with in social research is a difficult, often emotionally fraught undertaking that can never be fully reciprocal or free of power dynamics.

As we have shown, recruiting participants can be anxiety-provoking and, at times, can feel like a hardship wherein a finished research project seems like an all-too-distant future. However, recruitment can also provide some of the most enriching and exciting experiences in research, particularly as a doctoral researcher.

Indeed, recruiting participants through a careful and considered approach can offer us insight into ourselves, our positionalities, how we work, and the spaces in which we become embedded. Crucially, as we have demonstrated, continually interrogating approaches to recruiting participants can offer more nuanced ways of working with those whose stories we set out to elucidate. We aim to encourage readers to build on our narrative by considering how power relations are emergent and potentially deconstructed through recruitment. We hope that this chapter will offer opportunities for early career researchers to both mitigate their anxieties and doubts and reflect on their planned recruitment practices before entering the field. To that end, we encourage readers to continue our conversation by considering the ethics, challenges, and opportunities of participant recruitment throughout their own research practices.

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Chapter 6
Ethical Research with Children: Reflections from Fieldwork in Dhaka, Bangladesh

Iqbal Ahmed

Abstract During the past two decades, literature has increasingly focused on ethical research with marginalised children. This chapter reflects on the ethical issues I was confronted with during my first encounter with street children in Dhaka. These issues are highlighted through the lens of power relations and compensation to participants against specific research contexts in an informal settlement in Dhaka. In particular, this chapter draws on my experiences of how these issues were materialised and politicised through my encounters with the children and the gatekeeper. I argue that it is necessary for researchers to re-assess institutional ethical requirements because the reality that emerges from the field may not be always similar to what is anticipated prior to their research. Through the narratives of this chapter, I highlight how tensions of power dynamics and compensation have contributed to my understanding of the ethical issues that could potentially open grounds for confronting questions and discussions from other researchers to reorient and localise research practices with children.

Keywords Ethics · Power relations · Compensation · Children's research · Bangladesh

6.1 Introduction

This chapter offers a visceral reflection of how I responded to and struggled with ethical issues of research with children in Dhaka, Bangladesh. I conducted an ethnographic study with street children about their everyday lives during 2018–2019. The children whom I engaged with were between the ages of eight and 17. Most of

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the children were engaged in informal work for various reasons, including supporting their families, meeting their daily needs, and surviving on the street. I worked with three NGOs, which provided me with the access to the children in various sites in Dhaka. These sites included an informal settlement also known as, *bosti*, in Korail, a shelter in Mirpur, and a park in Dhanmondi. The objective of this chapter is to reflect upon the epistemological framework of power dynamics and compensation to the children against the empirical evidence that emerged from my first meeting with the children in Korail.

I met the children through my gatekeeper, Mamtaj, who was a local community member. The collaborating NGO in Dhaka had assigned her to me prior to my visit. From my encounter with the children, I address two issues. One is how power of the gatekeeper can be materialised, understood, and politicised within a specific research setting. The other is compensation to participants as an ethical issue (Hammett and Sporton 2012; Laws and Mann 2004) not necessarily as a disparate topic but one that has perpetuated from the same encounters with the children, their families, and the gatekeeper. Confronting these issues have compelled me to interrogate the thrust for ethical rigours and tensions of these issues (Christensen and James 2000; Christensen 2004; Laws and Mann 2004; Abebe 2009; Abebe and Bessell 2014) in my own research with the marginalised children in Dhaka. Throughout this chapter, I have used several Bengali terms in order to provide readers with insights into some ethnographic conversations with children and community members that shed light on the context of everyday practices in a marginalised community in Bangladesh.

Considering the diverse demographics of street children in Dhaka, ethics can be seen as critical to my research (Young and Barrett 2001). In explaining the relevance of this argument, Young and Barrett (2001: 130) further posit that 'childhood is diverse, with different children … requiring unique approaches which often present the researcher with unexpected moral dilemmas'. Hopkins (2007: 367) explains that the growing importance of ethical research has prompted the 'proliferation of guidance, codes and policies' to guide ethical conducts of research. According to Bushin (2009), however, ethical guidelines need to be understood against the contexts in which researchers engage with their research. Furthermore, Bushin (2009: 22) suggests that researchers need to pursue their ethical judgment based on their 'research project[,] … knowledge of the participants [and] … setting/s for their research'. To extend this view further, my own experience of acquiring ethical clearance in Dhaka resulted in frustrations from having to deal with the lack of established ethical protocols to work with children. Abebe and Bessell (2014) provide some relevance to my experience as they argue that the practices of research with children is somewhat limited in the Global South due to the absence of institutional thrust for knowledge-production about ethical research in children's studies. Yet, the principles of ethical research with children cannot necessarily be ignored. Abebe and Bessell (2014: 129) offer an example of an alternative guideline in Australia that extends 'a degree of discretion' that researchers can use while conforming to the institutional ethical guidelines.

From the discussions above, this chapter engages with the evidence from the field that may inform other researchers to rethink the boundaries of ethics. I argue that it may be necessary for researchers to re-assess the institutional ethical requirements because the reality that emerges from the field may not be as transparent as what is anticipated prior to their research. In doing so, I explain the tensions of research from the field in order to inform other researchers about the cultural and political practices and the peculiarities that may not be always articulated by the institutional guidelines and requirements.

The structure of this chapter is as follows. I first introduce key epistemological discussions on power relations and compensation respectively as key ethical issues relevant to the understanding of research with marginalised children; I then explain my ethical practices and tensions within the research environment and reality in Dhaka. Finally, in the concluding remarks I signpost the nuances of ethical research through the lens of power relations and compensation, aimed at understanding the roles of ethics within the research context in Dhaka.

6.2 Power Relations and Compensation: Politics of Korail *bosti*

Research with children presents a number of concerns about the inherent power relations between children and adults (Cornwall and Jewkes 2010). Dowling (2010) argues that power is central to qualitative research because the information researchers gather from their research can influence people's lives both directly and indirectly. However, the process of gathering information by researchers is also fraught with power dynamics, which may arise from the authority of the locals and the cultural practices in the field. Allen (2003) discusses about the association of geography and power that I find relevant in the context of my own research in Bangladesh, in which power is an effect of the social relation that exemplifies proxies of authoritative practices among men, women, and children. I examine this form of power dynamics in Dhaka that reveals 'messy co-existences and awkward juxtapositions of power that characterize places' (Allen 2003: 159). In this sense, this narrative discusses power dynamics of the gatekeeper of an informal settlement in Dhaka. But scholars have also criticised these power dynamics as being 'conceptualisation of power' (Gallagher 2009: 87), in which the discussion of power relations between adults and children remains within the notion of the 'powerlessness' of children and the dominance of adults over them (Gallagher 2009). With this understanding, I hope to offer some insights into the ethical tensions of research with children that are not always known and understood, but are necessary to gain reasonable understandings about their lives through an 'ethically acceptable research relationship' (Gallagher 2009: 89). To an extent, neutralising power dynamics and children's participation in research requires giving children an option of their participation, which can manifest into opening up the meaning of their lived experiences (Greig and Taylor 1999).

After I met the children in Korail, I explicitly told them that they were not obligated to participate in the meeting with me, and that they could leave at any time if they chose to. But the issue of power dynamics, in my case, did not just exist between the children and myself. It involved the tension of power dynamics between myself and Mamtaj, my gatekeeper, which had led me to learn about the importance of limiting the involvement of the adults during my interviews with the children. During my first meeting with the children (more details in Sect. 6.3), adults had certain control over me. For example, I did not have control over the selection of the children for the interview. Prior to my visit, Mamtaj told me that she would arrange the children for their interview with me. I did not object because I was perhaps too naïve and inexperienced as a researcher. Yet, I was eager to meet with the children. But from the experiences of adult interactions and interventions that emerged, I had begun to think that it was not only necessary to interview the children without having to expose them to any conflict or power struggle with the adults, it was also critical to learn about their own perspectives. My intention was to seek 'insights into the worlds of children and predicaments they face' (Jones 2009: 198) in their everyday lives, which they understood and were able to articulate from their own perspectives, including their expectations from me.

Compensation, financial or otherwise such as gifts, to research participants is fraught with debates. On one hand, it recognises participants' 'time and contribution' and, on the other, it opens up expectations 'of recompense for participation' (Laws and Mann 2004: 39). My intention to compensate the children arose from 'my respect for their participation' (Couch 2010: 155). So, I intended to offer gifts, lunch, trips for the children. But I could never fully know what the 'real' intentions of the children and their parents were to have allowed me to work with them. This uncertainty, however, reflected the context and circumstances in which I made my offers. Hammett and Sporton (2012: 498) argue that compensations or payments to research participants in marginalised communities can create tensions if those communities are 'frequently visited by visiting research parties'. An NGO worker in charge of various projects in Korail informal settlement had mentioned to me that it was common for researchers to offer gifts to the children. He also said that sometimes this can raise tensions within the community if the gifts, donations, or other services were not distributed through 'proper channels'. Sherry (1983: 161) argues that the intended meaning of gift giving can be construed as 'situational conditions of giving' arising from a multiplicity of circumstances in the field. Similarly, by 'proper channels' the NGO worker meant community leaders, gatekeepers, or other stakeholders who possessed some forms of authority within the community. The gifts, he said, were necessary for not only children, women, and/or other groups within the community to participate in research, but also to establish goodwill between the researcher and the participants. 'They [gifts] are not mandatory, but sometimes it's necessary to keep them, *khusi*, happy', he added. In the following section, I highlight issues of power and compensation through my ethnographic observation from the field.

6.3 Experiences and Tensions from the Field

Mamtaj had invited me to her home to meet with the children. Upon my arrival, she told me that my meeting with the children was called, *nam dewa*, name-giving. It is known among the Korail residents as a process for researchers, NGOs and others to engage with the children for the purposes of conducting research and social services. Besides being enlisted for interviews, 'name-giving' also implies that participants are to be 'listed' by the community leaders like Mamtaj before they receive benefits from local and foreign NGOs, government agencies, civic societies, and individuals. The benefits include the opportunities to attend schools in the community and to receive books and school supplies, uniforms, clothing, food, etc. I did not know how the selection process worked. When asked about it, Mamtaj only told me that it was based on the needs of the children, which did not tell me much, but I did not want to risk offending her by appearing to be too pushy. Mamtaj explained a few rules for interviewing the children. That I would have to call her prior to coming to Korail to arrange a schedule for the interview. This would allow her to gather (arrange) the children for interviews. Furthermore, she had cautioned me that there would be no exception to these rules because she would be responsible for, *dekha-shona*, looking after, the children. She had emphasised that this would be her, *daa-ittoo*, duty. Mamtaj's rules in the selection of the children represented a sense of my 'lack of authority' (Skelton 2008: 453) to engage with the children.

Mamtaj also invited two female community members to the meeting. As Mamtaj introduced them to me, she said that they worked with her in the community and that they had wanted to join. I did not object to their presence because I did not want to offend neither Mamtaj nor the women. Seven or eight children, a mix of boys and girls, a couple with their parents, also arrived. Mamtaj brought tea and biscuits for me. After initial introduction and pleasantries over biscuits and tea, I asked Mamtaj if I could talk to the children. 'Yes, go ahead', she said. I asked the children and the parents if I could talk to them. I explained my role as a researcher and that I was there to talk to them about their lives. They agreed. So, I turned to a girl who was sitting next to me. I asked her name. She did not reply. 'Tell your name', her mother, who accompanied with her, said. She did not immediately respond. I waited. 'Fatima', she spoke shyly. 'How old are you?' I asked. 'Ten', her mother said. 'What grade are you in?' I asked Fatima. I did not ask whether she went to school or not. My intention was not to offend Fatima and her mother in front of her children and parents by making any negative assumptions about Fatima. While I was waiting for her answer, I noticed Mamtaj and the two community members were whispering. I did not know what to make of it, but Mamtaj told me that I could not interview Fatima because she had not been 'listed'. Fatima's mother became confused. 'Why not'? She asked. She demanded that her daughter be interviewed. At her insistence, Mamtaj and the two community members became visibly agitated and they started an argument with Fatima's mother. Gradually, Mamtaj and her colleagues started speaking in an abusive way to Fatima's mother.

I sat silently but kept observing what was unfolding around me. Their argument escalated to the point that everyone, including myself, Fatima, and other children in the room, froze, unable to speak or to dare. Fatima's mother started to cry and with an unsteady voice she blamed the three women for taking something for their own benefits from the interview. No sooner had she said that than Mamtaj's fury turned ugly. She screamed at Fatima's mother and said to her that I was not here to give anything. '*Uni ekjon gobeshok*, he is a researcher', she screamed, pointing her fingers to me in order to draw attention to Fatima's mother about my identity. But Fatima's mother, whether she understood my purpose of being there or believed Mamtaj, kept blaming the women for presumably gaining something unfairly for their own benefit. At that point, Mamtaj and her colleagues became visibly violent. They started to shout at Fatima's mother and curse her collectively. 'Get the hell out here you bitch', one of them said. They rose from their chairs as if getting ready to hit Fatima's mother. I turned to Fatima. She sat stone-faced; yet she sheepishly kept looking at her mother, who was crying, and the three women. I wondered if she was used to this conflict and violence in her everyday life. I remained seated, paralysed, not knowing what to do. Something 'strange' was happening that was unknown to me but it was about my 'entry' into their world (Rabinow 1977). And I realised that I was at the centre of what was happening. Fatima's mother thought I was there to 'give something' to the children. She refused to believe otherwise. Mamtaj and the community members eventually escorted Fatima and her mother out of the room.

In light of this conflict, it is perhaps reasonable to think that Korail community remains at the mercy of the influence of the powerful community leaders and their problematically crafted role in decision-making (Morshed and Asami 2015). An NGO officer, whom I met after the incident and who oversaw the programs and services in Korail, told me that internal decision-makings regarding aid and other resources for Korail residents are relegated to the community members who were not necessarily incompetent but were inept, often seeking to serve the, *pori-chito lokh*, known people, in the community. I wondered if Fatima's mother had had any issues with Mamtaj and the community members. The NGO officer also told me that the community expectation about, *kichu pawa*, getting something, is a manifestation of NGO, civil society, government, and individual practices in Korail where they give away tangible things to children and their families, and as a result, residents in Korail were unaware of the limits of their expectations. These benefits have created perpetual expectations among the Korail residents who encounter, *oporichito manush*, a stranger, in their communities, like me, he added. The NGO officer had told me that it was common for the community members to intervene during meetings and interviews between children and outsiders. My identity as a researcher from a Western university also reminded me that I was an outsider in their community. I had to be cognizant about my plans to interact with the children as well as to offer gifts and lunches. Yet, I came to understand from my experience that I might not always have the upper hand during my interactions with the children (Willis 2014). This was necessary for me to realise and learn about how relationships and negotiations in the field were materialised and negotiated (Sparrman 2014). In the case of the conflict during my interview, Fatima's mother did not register her

daughter for the interview. She had heard about my arrival as a researcher and decided to show up without enrolling her daughter's name through Mamtaj and other community members. But the violent interjections of Mamtaj and her colleagues during the conflict illustrated the power of the community women in order to maintain equity of aid and resources among children and residents (Hossain 2013). The authority of Mamtaj had indicated her influence in the selection process of the children in Korail. But her authority also arises from her responsibility as a community leader to provide resources for the children from outsiders.

The expectation of children and their families about 'getting something' worried me as I had plans to give gifts, buy lunch, or take the children out to movies/site visits as a way of showing my gratitude to the children for their time to take part in the research. The readings from literature on compensating research participants prior to arriving in the field had provided some theoretical guidelines for me. However, the incident in Korail cautioned me about offering any form of compensation to the children. In addition, I began to harbour contradictory thoughts about offering gifts and lunches or taking the children to the movies in my future encounters with the children. Before I came to Korail, I had told Mamtaj that I might offer children gifts and lunch for their time in various linguistic terms: *jodi taka thake*, if there's funding; *jodi ami pari*, if I can; *jodi amar samortho thake*, if I have the capacity. The purpose was to ensure that I was not committing to any promises while, on the other hand, my offer indicated my goodwill towards the children. Yet, the contradiction of compensating children left me interrogating my consciousness about further consequences that I had yet to encounter.

After the incident, Mamtaj asked me to come back at another time to interview the children. 'I will call you', she promised. I left Korail with a sense of uneasiness about my own capacity and my identity as a researcher in a city that was both my 'home' and 'field' (Sultana 2007). And this dichotomy had placed me in an odd position, a temporal state of mind, in which I needed to be reflective about my 'own positionality' within the 'grids of power relations and how that influences […] interpretations, and knowledge production' (Sultana 2007: 376) from my research. On the one hand, my position as a researcher with the children in Korail gave me an opportunity to discover about their lives. On the other hand, my position was also somewhat restricted by the powerful presence of the gatekeeper and the community members in charge of the children. When I received a call from Mamtaj the next day, she apologised for the incident. She invited me back to her home to have a chat with her. So, I returned to Korail a few days later with a sense of purpose to find out more about the incident with Fatima's mother. I asked Mamtaj why Fatima's mother and Fatima were present at the meeting. She said they had randomly showed up and Mamtaj just could not kick them out. 'Does it happen often?' I asked. 'Just leave it', she said. I sensed a trace of irritation in her voice. I did not want to risk offending her so I moved on. As we continued our discussion, Mamtaj told me that I needed to learn about the *riti-niti*, politics, of Korail. 'You came here to research but you need to learn, and understand', she plainly said. 'But I am here, don't worry', she also assured me. While her assurance was comforting, my instincts also reminded me that to engage with the children and other adults in Korail would require

disengaging with my own discretion, decision, and will as a researcher. Mamtaj's role as my central gateway to the children reminded me about her authority for the well-being of the children and people under her care (Reeves 2010). I had to accept it because not only did I need to gain her trust, but also to understand the 'cultural indication of trust' (Norman 2009: 73).

My meeting in Korail did not necessarily produce any in-depth discussion with the children. Because of the conflict, the meeting had to be ended. Yet, it provided some key insights into power dynamics and compensation to participants, both of which were necessary for me to reflect on conducting my research in an ethical manner. While compensation to research participants has raised ethical issues, it can be a necessary tool for gaining trust and participation (Morrow 2009). Yet, in this case, the violent encounter reminded me to become aware and cautious about the rules of engagement with compensation and expectation. Observations of Hammett and Sporton (2012: 498) have provided some relevance into the necessity of offering gifts or payments as a form of 'reciprocal exchange relations' to seek and to establish favourable relationships with the participants. My follow-up discussion after the incident with Mamtaj provided some insights into the politics of Korial community — the *riti-niti*, as Mamtaj called it. 'We know how to manage these people but you are new, *notun*, here and you will learn', Mamtaj told me. The introduction of the rules of politics was both critical and unknown to me. Yet, these revelations helped me to adapt strategies in order to negotiate further interviews and encounters with the participants (Hammett and Sporton 2012; McAreavey and Das 2013). In order to cultivate a trusting relationship (Blix and Wettergren 2015; Norman 2009) with Mamtaj, I had sought assurance from her about interviewing the children without any further conflicts. I told her that it'd help for the children to offer their perspectives without the presence of the adults. She had agreed to allow the children to talk to me without their parents, herself, or any other adults. I also sought advice on appropriate compensation for the children.

6.4 Conclusion

In this chapter, I have provided some insights into ethical tensions of conducting research with children in a marginalised community through the lens of power relations and compensation against my own research experience in Dhaka. While ethical considerations remain a linchpin of children's research, these considerations in Bangladesh are not so transparent. However, it is not to say that there are no protocols for ethics. In fact, Abebe and Bessell (2014) discover that many countries in the Global South have their own ethical protocols and requirements. Regardless of where research with children is undertaken, power dynamics between adults and children need to be considered during research (Gallagher 2009). These power dynamics and the adult interactions during the research process demand that particular consideration be given to ethics (Abebe and Bessell 2014). Although ethical guidelines often set the tone for the researchers to engage with vulnerable

children, the rigour of ethical requirements can hinder encountering the reality of the field, making it difficult for researchers to address the 'ethical uncertainty' (Palmer et al. 2014) of research within its context. However, evidence of countering ethical uncertainties exists, where researchers are able to adapt the ethical guidelines to local culture and context. In other words, the rules of engagement with ethics are not necessarily immune to discretion and judgment within the context of research. The experience of conflict in Korail highlighted the rules of engagement with the community as a researcher and enhanced my understanding of the expectation of the community members and participants from the researcher. My experience did not prepare me for the nuances of community relationships that played out among the residents in, *bosti*, informal settlements. It took an entire episode of violent encounter between a parent and the community leaders for me to learn about how a researcher engages with the participants through a vetting process done by community members prior to the engagements. The episode also reminded me to think deeply about the reality of the power dynamics between parents and children in Korail that embodies protection of children from outsiders. The intention of this chapter, through the experiences of my research in Dhaka, has been to reflect my own experiences about the ethical tensions and to open grounds for other researchers to learn about reorienting and localising ethical practices with children during field studies, which may not be native to them. Furthermore, the intended contribution has been to offer some new insights into how practices of power relations and compensation in marginalised communities are politicised, perceived, and practiced from my ethnographic encounters in Dhaka.

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Chapter 7
Fieldwork Poetics: The In-Betweenness of Ethnographic Alterity and Researching with Music

Diego Astorga de Ita

Abstract This chapter is a reflexive exploration of ethnography. Of the issues that arise when we consider its problematic history, the asymmetric relations between those involved, and the confusing positionalities that emerge when moving from the place of study to the place of research ('the field') and back again — even when research is done 'at home'. Throughout the text, I consider the theoretical and poetic ideas of diverse authors and disciplines — from anthropology, to poetry, to geography — discussing how they can provide a framework for ethnographic work, and for understanding our positionalities, as well as how they can help us answer the questions that arise while in the field. How do we navigate the otherness of research? What ethical issues come up when dealing with Others from what often is a position of power, and how do we (try to) overcome these dilemmas? What are the stakes for us, and for the Others we are researching (or 'researching with')? Who benefits from our work? Where lies the fealty of the academic? In this text, I explore how these questions looked like in my PhD research, and try to elucidate these issues through a lens of relational poetics.

Keywords Ethnography · Alterity · Poetics · Relation · Mexico

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7.1 A Kettle of Fish

'I don't know why we write....
And sometimes I wonder why later.
we publish what we have written.
(Pacheco 2007)

Ethnography is a strange thing — a kettle of fish. Its practitioners know this, and many have written about it. Is there a need for yet another reflection on the subject? I don't know, like I don't know if there's a need for yet another ethnographic text. Still, here I am writing my experiences of doing ethnography while researching music in south eastern Mexico: the strangeness, self-consciousness, and ethical qualms that came over me while working in the region of El Sotavento ('The Leeward'). My current research looks at *son Jarocho* — The Leeward's folk music — and its relation to landscapes. I am interested in how culture/nature is/are conceptualised in this particular region, especially through Sotaventine music-culture. Given the subject of my research, my fieldwork consisted in following musicians and visiting music-making communities around the globe — from Paris, to small cities and ranches in El Sotavento, to Mexico City, to L.A., to Tijuana, and back again — chatting and playing the *jarana* (a small cedar eight-string guitar) with them.

Most of my fieldwork took place in Mexico, my country — 'at home' — but in a region that is not my own. I had been to The Leeward before as a tourist or passer-by, but I had not worked there until now. Being there as a researcher made the place new to me. Part of the novelty was the in-betweenness of being foreign-yet-at-home. A good example of this uncanny feeling happened the first night I was in Santiago Tuxtla, a small city in The Leeward.

In the city centre of Santiago, amid palm-filled gardens and park benches, stands a colossal pre-Columbian Olmec head carved in basalt. Olmec heads are the sort of archaeological marvel we learn about during primary school but forget exists somewhere beyond our textbook until we come across it while casually strolling down the street. Finding it there felt how I imagined it felt to find the bones of King Richard III under the tarmac of a Leicester parking lot, or how it would feel if they'd made a permanent exhibit of the monarch's remains in a little square across the street from a Boots and a Tesco.[1] I'd heard of Olmec heads, I'd seen them in pictures and museums, but never so unexpectedly in such an everyday spot.

Is that an Olmec head?!

I asked, stunned.

Oh, yeah.

Someone answered quite matter-of-factly, as if I'd asked if a tree was a tree or if a bench was a bench, and as if it were perfectly normal to find trees, benches, and colossal Olmec heads in little town squares.

[1] Boots and Tesco are common franchises throughout the UK, the former a chemist, the latter a supermarket/convenience store. Like CVS and 7–11 in the US, or *Farmacias Guadalajara* and OXXO in Mexico.

With strange meetings like this, one quickly realises (or remembers) that ethnography is not only a kettle of fish, but that each ethnography is a different kettle of fish altogether. Ethnography is some fuzzy thing[2] that, under different circumstances, becomes a different *some*thing. Partly because of this, the notions of foreign and home quickly dissolve when going into 'the field' — even into a field you (somewhat) know. This is particularly true when one is based in a 'foreign' university doing research 'at home'. What follows is my attempt to unravel the questions and ideas that grew from this particular kettle of fish (many still unanswered), starting with what arguably lies at the heart of such strange encounters, or rather, what makes these encounters feel strange: the question of otherness.

7.2 In-Between Alterity and Alter Egos

The central question of anthropology, and indeed, all human disciplines, is alterity. At least according to Krotz (1994). To him, otherness is the starting point of all anthropological explorations and is emplaced in cultural contact.

This otherness is particularly strange when we are placed in-between 'here' and 'the field'. Especially when 'the field' is supposed to be home and 'here' is supposed to be foreign. 'Here' and 'the field' seem like two different worlds; one imagined, the other real. Identities become muddled and we no longer know where 'foreign', 'field', 'here', and 'home' are anymore. Alter egos emerge as we move (in-)between worlds.

I was born 'here', in England, but I have never been English. I have always been Mexican, even before setting foot in Mexico as a toddler. Returning 'here' makes my otherness palpable. In the UK, I'm seen as a somewhat exotic, brown (or brownish) character that doesn't comply with expectations of Britishness and/or whiteness: I have a strange accent ('Oh, but your English is SO good!'), I have a strange name, I have been told to 'speak in English' and to 'shut the fuck up'. I am clearly Other in this island.

On the other hand, I'm Other in 'the field' as well — when 'at home' — as the following interaction illustrates. One afternoon, in the town of Tres Zapotes, I was sat on the curb, under the shade of a tree, waiting for a *versador*[3] to return from picking maize in the field — an actual field. Near where I was waiting were two older ladies and a girl, sitting in their porch. After a while, the girl shouted:

> *¡Ey Güero! Que si no quiere sentarse acá en la sombrita.*
> [Oi Blondie! They want to know if you´d like to come sit in the shade.]

[2] Some would speak of ethnographic methods (Malinowski 1932, for instance), others of ethnography as a discipline (Crang and Cook 2007), others would say ethnography is the writing that results of anthropological research (for example, Ingold 2014). In this chapter I will use the term as all of these interchangeably. While this definition is a fuzzy definition, it seems befitting, seeing as ethnography is a fuzzy changing thing.

[3] A *versador* is someone who knows/makes and declaims/sings verses.

I shyly accepted their invitation and sat in the porch from where I could still survey the street for my would-be interviewee. They asked what I was doing and we started talking about my research and music and life in Tres Zapotes. At some point in the conversation one of them said:

> We were looking at you sitting on the street and we took pity, I said:
> "He´s going to get sunstroke!"
> So I told my granddaughter to call you in, but she said:
> "We don´t know him! I don´t know his name, how am I supposed to call him?"
> And I said:
> "Just call him *güero* [blondie]".
> And she did,
> and you replied,
> and here we are!

I should probably mention now that I have dark brown hair. Although it's not uncommon for people in Mexico to use '*güero*' as a catch-all term — like 'fella' or 'hey you' — this little field incident made me realise that I was seen as a somewhat exotic white (or whiteish) researcher prone to sunstroke: I have a posh(ish) accent, I speak two foreign languages fluently, and I am studying in a foreign university. Not that being white is unusual among Leeward musicians — renowned Sotaventine guitarist Andrés Vega is nicknamed '*El Güero*' — but whiteness is another thing altogether. My alterity is palpable even 'at home'. It sometimes feels as Lévi-Strauss (1961: 58) says that the ethnographer 'acquires a kind of chronic uprootedness from the sheer brutality of the environmental changes to which he is exposed. Never can he feel himself at home anywhere'.

This alterity of the in-between is both consequence and driver of social research, but it 'has a high price: it is not possible without ethnocentrism' (Krotz 1994: 9). This can be problematic given our disciplines' past. Geography and ethnography were used repressively to reassert the centrality of the Western *ethnos* against the colonial Other (Smith 1999; Glissant 2010), who was often seen as a savage and a Cannibal (de Certeau 2000; Jáuregui 2008). While we do not adhere to this imperialist and totalising vision anymore, historic implications cannot be dismissed on the basis of good intentions (Krotz 1997). Even if we do not use our research to construe otherness as wildness to be civilised and subdued, we still imagine and organise the world from a place of power-knowledge (Smith 1999; Crang and Cook 2007). But alterity need not be totalising, as Glissant points out: '[t]otality's imaginary allows the detours that lead away from anything totalitarian' (2010: 18). Likewise, we can deploy Said's (1994: 161) counterpoint, wherein we consider both imperialism and resistance, and 'read ... retrospectively and heterophonically with other histories and traditions counterpointed against'. In this sense, rather than continue writing alterity from a place of power we can attempt a more poetic approach where we acknowledge our otherness, partiality, and privilege, and the kettle-of-fishiness of it all (Clifford 1986). As Glissant (2010: 29–30) points out, 'the power to experience the shock of elsewhere is what distinguishes the poet'. Similar things have been said of the ethnographer as we read in Lévi- Strauss (or as we can read in Rosaldo 2016). We shall then look for theorists of alterity in the world of poetry.

7.3 Poetics of Otherness

Many poets have spoken of otherness. One of the better-known ones (at least in Spanish) was Octavio Paz. Throughout his writings he explored questions of identity, solitude, and alterity (Xirau 1970; Wilson 1979). Some images from his poem *Piedra de Sol* ('Sunstone') can be useful to us for thinking of otherness in ethnographic work.

However, first an important caveat must be made. While Paz is one of the best-known Mexican poets, he has also been criticised for his treatment of certain Others. His cavalier writing on rape, and his treatment of women and the feminine is problematic — both in his writings and personal life — as is his treatment of homosexuality and of Chicanos. Paz has been criticised for this (Vera Tudela 2018), and called 'racist, misogynistic, and homophobic to the extreme' (Gaspar de Alba 2014: n. 50: 222–223). With this in mind we must read Paz contrapuntally; considering not only his voice, but also the voices that speak with and against him, so as to not perpetuate the ideas that make him 'dangerous ... to those ... that he maligns' (Ibid.).

But Paz', own words — at least these words — seem to counterpoint his own problematic side:

para que pueda ser he de ser otro,
salir de mí, buscarme entre los otros,
los otros que no son si yo no existo,
los otros que me dan plena existencia
[for me to truly be I must be other,
get out of me, seek myself in the others,
the others that are not if I am not,
the others that give me a full existence.]
(Paz 1957: ll.515–518, all translations are the author's own, unless otherwise stated.)

The idea that our very existence is linked to the Other and to our search for the Other is clear in these verses. In Paz' poetics our being depends on alterity. This poetic ideal can be enacted in ethnography: the search for Self in the Other drives us, for only in knowing the Other can we get to know ourselves.

muestra tu rostro al fin para que vea
mi cara verdadera, la del otro,
mi cara de nosotros, siempre todos
cara de árbol y de panadero
de chofer y de nube y de marino
cara de sol y arroyo...
[show me your face at last so I may see
this true face of mine, the face of others,
my face of ours, always everyone,
this face of tree and this face of baker
of chauffeur and of cloud and of sailor,
face of sun and of river...]
(Ibid.: ll.526-531)

Our true Self is revealed in the Others', our face is like the Others' and is indeed the Others'. This idea is not new and we run the risk of falling into platitudes and

commonplaces if we continue this way, but maybe that's precisely what we need. Glissant (2010: 31) proposes that 'amassing commonplaces is, perhaps, the right approach to... the entanglements of ... relation'. We see this relational dialectic with the Other in the work of many writers. Reyna's sixteenth century translation into Spanish of the biblical Proverbs puts it this way:

Como vna agua ſe parece à otra,
anſi el coraçon del hombre àl otro.
[As one water is alike another,
So is the heart of man to the other.]
(de Reyna 1569 Prov. 27: 19)

Looking at these poetics, a dialectical relationship emerges, in which there is no Self without the Other. We need the Other to fully understand; we cannot build knowledge (or *be,* for that matter) in isolation. This has implications for ethnographic research. If we need the Other in order to know, then ethnography must strive to truly be an intersubjective understanding of the world (Crang and Cook 2007).

Nevertheless, searching for the Other only for the sake of (Self-)knowledge can also be problematic. Would that not be, once again, a return to totalising alterity rather than an enactment of poetic otherness? Again in a proverb, though this time from Antonio Machado, we are admonished against this:

Busca en tu prójimo espejo;
pero no para afeitarte,
ni para teñirte el pelo.
[Find in your neighbour a mirror;
though not for shaving,
or dying your hair.]
(Machado 2018, CLXI § XXXIX)

We must assume our ethnocentrism poetically, rather than as totalising expansion. We listen and make space for the voices of Others. We try to find the mirror of the Other's heart, and to show the mirror in our own. And yet, the temptation of a clean-shaven face lingers. The tension between poet and explorer remains in spite of our best contrapuntal efforts, and we struggle to relate to the Other in the uncanny in-betweenness of alterity.

7.4 Beware the Researcher My Son!

Regardless of the new outlooks that poetics of otherness may bring to our ethnographic pursuits, the researcher is approached with caution. When I started my fieldwork in the 4th Encounter of *Jaraneros*[4] in Paris, someone who knew about my research came up to the group I was chatting with and said jokingly:

[4] Jarana players; *son* Jarocho musicians.

Beware! He´s a researcher! He´ll investigate you!

Perhaps he said it half-jokingly. Either way, it seems that we researchers are someone of whom the Other should be weary. We are nosy characters, sometimes interesting, sometimes intrusive. In poetry we find numerous images that could well be a warning against our intrusions; for instance, Thomas' verses (1938):

O make me a mask and a wall to shut from your spies
Of the sharp, enamelled eyes and the spectacled claws

Am I a spy with 'sharp enamelled eyes' and 'spectacled claws'? Are we like Carroll's Jabberwock? ('Beware the [researcher] my son! / The jaws that bite! The claws that catch!'). Are we building masks or trying to catch a glimpse of what's behind? Paz' poetics point towards unmasking (Paz 1957: 363–368):

...las máscaras podridas
que dividen al hombre de los hombres,
al hombre de sí mismo,
se derrumban
por un instante inmenso y vislumbramos
nuestra unidad perdida...
[...the rotten masks
that divide man from men
man from himself,
crumble down
during an immense instant and we half-see
our lost unity...]

Is unmasking then a violent act or a necessary process? Even if it were beneficial, don't we put a mask on the Other through our writing? Ethnographers, like poets, make up worlds with their words. Our accounts are always partial; it's all a fiction, as Clifford points out (1986). Even if we do not 'strive to estimate [our] fellow men from a lofty and distant point of vantage' as Lévi-Strauss has proposed (1961); even if we do otherness poetically and get a glimpse of 'our lost unity' — isn't it all lost in the printed page? Isn't the counterpoint of the Other's voice silenced in our fabrications? We listen to them and capture them in tape — or mini SD cards — only to code and use their voices in our fictions. Perhaps we must re-read Paz' request (Paz 1957: 526–527) as a plea not just for the Other but for ourselves: 'show me your face at last so I may see / my true face...'. We too wear masks — sometimes we're the makers (is this chapter not a mask?), sometimes it's the Other ('Hey, *güero!'*). We all play this game of hide-and-seek, masking and unmasking; we are all trying to reach this intersubjective understanding of Other and of Self, this 'lost unity'. We must take off the mask we wear and show our face, if we are to encounter the Other, even if only for one 'immense instant'.

But how do we go about doing this? How do we take off our masks? How do we enact a relational poetics in our work? Smith (1999: 16) suggests we ought to 'share the theories and analyses which inform the way knowledge and information are constructed and represented'. Though when I talked of 'geopoetics' and

'ecopoetics' with master musician, writer and luthier Patricio Hidalgo, the conversation didn't quite go as expected:

> Oh such terms! Such big terms laddie! — he said.
> Yeah, well there´s this book by a bloke called Bate (2001)... — I replied apologetically.
> Ah — he laughed — and here´s me thinking you´re making words up!

Funny as it may be, this interaction makes me wonder how much of our 'big terms', 'theories and analyses' are relevant outside of academia. They appear to be (or easily become) yet another mask we wear as researchers. After this, sharing theories and analyses was not a central part of my research[5]; however, there were two things that did help to break down walls and take off masks, if only for a little while. The first one: sharing my Self.

If, as Crang and Cook (2007) say, social research happens through social relations, shouldn't we share our own lives as the Others do? After all,

> *El ojo que ves no es*
> *ojo porque tú lo veas;*
> *es ojo porque te ve.*
> [The eye that you see is an eye
> not because your eye can see it;
> but because it can see you.]
> (Machado 2018: CLXI § I).

This approach might lead us somewhat astray from Lévi-Strauss' detached ethnographer. However, as feminist scholars have pointed out, detachedness is neither achievable nor desirable (see England 1994; Rose 1997; Pink 2008, 2009). In the field I tried to be open about my life with interviewees, as they shared theirs. I welcomed the Others' questions, even if they made me nervous and I ended up rambling about strange theories and big terms. This is, to me, part of taking off the mask.

Besides sharing my Self and my story, the best way I found to traverse the tension of ethnographic alterity was music. Rather than merely researching music, I researched with music. Music is a language in itself and a constant dialogue, which has been recognised and utilised methodologically in ethnomusicology for a long time (Hood 1971). Music — at least folk music — is a communitarian endeavour; and so, music provides both a literal and theoretical counterpoint. Call-and-response verses counterpoint each other, and the syncopated beats of dancing shoes respond to cedar chordophones. At the same time, a moment of relational creation shifts the focus from rigorous academic sapience to traditional knowledge — from observing and estimating, to listening and making a joyful noise — this overturns the knowledge-power asymmetry of research, even if only temporarily. When making music the literal tension of the strings turns into sound; likewise, in sound the tension of alterity finds purpose in community. Sotaventine music requires alterity: while one can play alone, music produced in isolation is never on par with music made communally. This communal musical praxis embodies the ideals of poetic

[5] Though discussions on *topophilia* and the meanings of 'space' did flourish in other interviews.

alterity better than any ethnography ever could. But music is bound to come to an end. Is that enough? Does a moment of community compensate for our ethnocentricity, and for all that we take with us? Is this all we can leave in the field?

7.5 Life and What We Leave Behind

We can speak of poetic otherness all we want, we can talk about counterpoint and music, but in the end we are still writing about the lives of Others for our own benefit, even if we leave bits and pieces of ourselves behind. After 3 or 4 years of research (and only a fraction of that in the field), we get a piece of paper that says 'Doctor of Philosophy'. We make a living by laying bare the lives of Others for all to see. And what do they get out of it? Are music and anecdotes enough?

Even if we return the outcomes of our research to the communities we work with, what good is a thesis in academic English to an ageing rural Mexican musician who never learnt to read? It can be a gesture, and a powerful one in some cases, especially when one gets to know a community quite well. And it can be useful when research is planned and undertaken with the community. But when research is done all over the map, meeting people only a few times, what can we give back, and to whom?

Our research is seldom useful to the Other, particularly doctoral research. 'Your PhD research is not going to change the world', my supervisors told me in our first official meeting. Add to that the barrier of our academic lingo. Why do we research then? Only for the title? Can we (should we) be so cynical? What do we leave behind us in the field? If we manage to take off our mask and see the Other, perhaps we'll leave some memories — a little curio in the corners of their mind. Perhaps we give ourselves too much importance thinking people we meet once or twice will remember or care about us. This,[6] however, shouldn't dissuade us from trying to give back. For me, a simple, yet powerful way of giving back was photography.

As part of my research, I took portraits of the people I interviewed, and of the people I met. As I did this, I asked for their permission to use their portraits in my work. Photography is a lot like ethnography; it is too '[g]azing on other people's reality' (Sontag 2005: 42), and like ethnography, it is sometimes aggressive and predatory, turning 'people into objects that can be symbolically possessed' (Ibid.: 10). But in photography it is much easier to go from the totalising to the poetically relational by returning the mimetic object to our ethnographic Other.

Given the above, returning portraits to those portrayed became an important part of my fieldwork dynamics. I would walk around with the printed pictures, looking for my interviewees, returning their portraits to them. Returning photographs meant giving back something tangible, even if only a piece of paper, and leaving something that can be touched and kept seemed important. Furthermore, given the

[6] And much of this self-consciousness, I think, might just be in my (or our) mind(s).

kinship between photography and ethnography, returning a photograph seemed like a symbolic way of returning my research, or part of it. Those of us who grew up before the advent of digital images will remember having frames and photo albums at home, and how flicking through old pictures brings people together. I hope that my pictures may become artefacts that allow for this sort of encounters. Though, again, I don't know if it's too arrogant of me to think this might be the case. What makes me hopeful were the reactions of the people upon receiving their portraits: their smiles, laughter, and comments. In many cases, the reaction was surprise: the astonishment of getting something back from a researcher.

One man I talked to spoke of a person who had lived in Santiago Tuxtla for some time, interviewing and recording many musicians:

> They came and made records, and asked questions, and stole everything from us.

I have met this person. They seemed perfectly decent. I've heard their wonderful field recordings and seen their excellent work. Still, in spite of all this, their memory is one of mistrust.

> Beware! He´ll research you!

Said the man in Paris.
Perhaps he wasn't joking.

7.6 Death, and What We Take with Us

Spy, thief, child playing hide-and-seek, portraitist: what part does our ethnographic mask bestow upon us? We take away from the field our notes and recordings and bring them back to the real world. Or were we in the real world all along and are now retreating from it? Who knows? We listen and write, and time moves on.

Some months ago, I sat in England listening to the voice of one of the great guitarists of the city of Tlacotalpan — Mr. Cirilo Pomotor — who, I'd just learned, had passed away a few hours back. There is a particular strangeness to hearing the dead speak. The aural records of past masters reverberate with voices now gone: of players and of instruments. I feel both as a guardian and as a thief. I have some minutes — perhaps an hour — of a man's life encased in an mp3 file. It's a higgledy-piggledy interview: there is no music (he couldn't play anymore because of his aches), and the conversation is somewhat hectic (he couldn't quite hear my questions and I couldn't quite phrase them right); and yet, it's his voice and part of his story. What am I to do with it? Transcribe it, use the data, and delete it, all in due time, as per the accepted protocols of our trade? The same ethical procedures would have me anonymise this master musician. But wouldn't this erase him from memory as well? Should I share this recording? Upload it some place where it may be heard by those who knew his voice better than me, and by those who never had a chance to hear it?

While I was 'in the field', another great musician passed away: Andrés Flores, a *jarana* and tambourine player, exceptionally skilful, amiable and funny. I met him briefly in the *Luna Negra* 'Seminar' — a music retreat that takes place every Easter. We spoke over breakfast about carving spoons and played in the same nightly gatherings that week, but I didn't get to know him well on a personal level. Later that year he passed away, almost at the same time as another very young Sotaventine musician. I was in Santiago at the time. These losses hit the musical community very hard. They were both sudden and took everyone by surprise. Andrés' wake and funeral — a musician's funeral, with processions of musical instruments playing alongside the casket — were to take place in his hometown, a couple of hours away from Santiago. I was unsure about going.

On the one hand, I felt the need to go and pay my respects to this great musician I'd briefly met. I was curious to see and be part of this rite of farewell that happens when a Leeward musician dies. At the same time, it felt wrong to go for the sake of sating my curiosities. I felt like I would be imposing on the mourners. I could already see the grief and confusion of my friends in Santiago, and felt it myself even though I hadn't known Andrés well. How could I possibly take this and write it into my work? How could I even think about it? Here my ethnographic otherness weighted heavily; it felt wrong, like I'd be profiting from the pain of Others if I turned this into 'data'. Yet, am I not doing now what I hoped to avoid? Here I am, writing Andrés' death into an ethnographic 'reflective' text, using it like I thought I shouldn't.

A few months after the funeral I found among my recordings four files from a night when Joel Cruz Castellanos and Claudio Naranjo Vega — two notable Leeward guitar players — were jamming together. As they played, a group started to gather around them, listening as they wove their melodies.

In the second of the four recordings, one can hear that a *jarana* starts to sound, strumming softly in the background. By the third, Andrés Flores is heard singing verses and answering refrains. I'd forgot Andrés was playing that night in that impromptu ensemble. It felt strange to listen to his voice and instrument, particularly singing *Las Poblanas* and *La Lloroncita,* both tunes that speak of death and dying:

Nacer es un sacrificio,
morir no tiene igualdad,
al ser supremo le aviso:
no estoy de conformidad
aunque morir sea preciso
[Being born´s a sacrifice,
and there´s nothing quite like death,
to the supreme being I say:
I do not agree with this
though I know my life must end.]

Re-encountering this voice in County Durham hit me hard. I listened to the recordings on loop. I edited the files a little and sent them to Joel. I thought he'd like to have them. It felt like perhaps my intrusions in the field and my ethnographic

ramblings — my otherness — might not be entirely totalising or devoid of sense; like there might be something valuable I could give back to musicians and friends back in 'the field'.

7.7 Return to Alterity

How then do we research? Or how should we try to research? Here I propose that poetics can help us enact alterity in our work in such a way that we may overcome some of the issues of ethnography. The idea of doing poetic ethnography is not new — Clifford and Marcus (1986) edited a whole volume devoted to it decades ago — this is merely my attempt at gleaning out of poetry a means for ethnographic fieldwork that might answer some of the problematic aspects of the method that have conflicted me in the past (and that still do). To me the poetics of fieldwork ought to be relational and reflective and can be aided by creative practices, particularly collective ones, such as music and poetry. It is poetic not only because it is a fiction — our version of events — but because like poetry (Pacheco 2007), it ought to be a profound and collective endeavour, even if encounters are brief.

I'm not pretending to claim these ideas of poetics or alterity as my own, nor to say that my version is the final one; numerous scholars have written and discussed these things before (Clifford and Marcus 1986; Taussig 1993; Said 1995; de Certeau 2000; Todorov 2007; Glissant 2010; McKittrick 2019; etc.). What I've tried to do here is to share my attempts to enact alterity poetically in the field, and my reflections upon these attempts. Although, as Pacheco (2007: 70) says, 'personal opinions / are really not very interesting'. Still, I hope my reflections may be useful to Others.

Looking at poetry and at poetics gives us tools to think of and enact ethnographic research differently, it might even help us narrow down the fuzziness of what sort of *thing* ethnography is. If we conflate the poet with the ethnographer (Rosaldo 2016), we can take Pacheco's words on what poetry is and apply it to ethnography:

> ...it is another thing:
> a form of love that only exists in silence,
> in a secret pact among two people,
> almost always unacquainted.
> (Pacheco 2007: 70)

Pacheco writes of reading (or being read by) a stranger. We could place our poetic ideal of ethnographic work along these same lines, following a relational alterity in which we seek community and knowledge alongside the Other. We should not look for relationality for the sake of Self, nor for the sake of knowledge in and of itself; as another old Hebrew proverb says, 'of making many books there is no end and much study is weariness of the flesh' (Eccl 12: 12). Or as Pacheco would put it, 'we throw/a bottle to the sea, filled and overflowing / with rubbish and messages in bottles' (Pacheco 2007: 69). Still, 'it is not useless, this shipwrecked gesture' (Ibid.); poetically we can take our alterity and our encounters beyond mere

'black signs in the white page' (Ibid.). But how do we do this? 'Only in love is it possible to capture what is radically Other without reducing it to consciousness', wrote Paz of Machado's work (Paz 1976: 147). Poetic ethnography, then, should be 'a form of love', though not necessarily silent. This might sound naive, corny or commonplace, but — as mentioned earlier — we must amass commonplaces to better grasp relationality. A poetic ethnography, then, must forget the self-absorbedness that often characterises academia and seek to encounter the Other earnestly and lovingly (for more on the idea of love in critical geography see Mould 2019). Granted, this might not be the right approach always — for instance, this idea becomes quickly implausible (or impossible) in institutional research, or when dealing with questions of corruption or violence; here a different poetics would have to inform our alterity. Nevertheless, at least for this kettle of fish, this particular poetics of otherness seems like the best framework.

Perhaps Machado (2018, CLXI § LXVI) said it best:

Poned atención:
un corazón solitario
no es corazón.
[Pay attention! Hark!
A heart that is all alone
is not a heart.]

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Chapter 8
Doing Ethnography with a Dual Positionality: Experiences in Spanish and Taiwanese Governmental Institutions

Yu-Shan Tseng

Abstract This chapter offers an account of how I conducted ethnographic fieldwork into two digitally-mediated policymaking processes by applying for an internship in Madrid City Council and in Cabinet Office of the Taiwanese government. My dual positionality as an intern and a researcher made it possible for me to circumvent the challenges of studying hard-to-reach elite institutions and to navigate through the power imbalance existing between myself as a PhD researcher and political elites within the two governments. This dual positionality created a semi-insider identity, which allowed me to build reciprocal and trustful working relationships with Spanish and Taiwanese practitioners. Based on this working relationship, I was able to collect insightful perspectives on two policymaking processes enabled by two 'Digital Platforms for Political Participation' (DPPPs) in Spanish and Taiwanese governmental contexts.

Keywords Dual positionality · Elite ethnography · Comparative ethnography · Participatory observation · Interview · Power dynamics

8.1 Introduction

This chapter outlines a set of participatory ethnographic practices I used to conduct a comparative study of Decide Madrid and vTaiwan, two 'Digital Platforms for Political Participation' (DPPPs). These DPPPs were developed and implemented by Madrid City Council and the Taiwanese Government respectively for improving democratic participation in policymaking processes. This comparative study was part of my 3.5-year PhD project that explored how the two DPPPs were designed,

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deployed, and operated within policymaking processes embedded in the Cabinet Office of the Taiwanese government and the Department of Citizen Participation at Madrid City Council (*Participación Ciudadana del Ayuntamiento de Madrid*).

I adopted three ethnographic practices — making an initial approach, conducting participatory observation, and interviewing — that were based on the dual positionality of an intern and a researcher that I created for myself. These practices can be generally understood as a set of participatory methods that requires a researcher to practise immersive observation and/or take part in the community they are researching (Prasad 1997; Cook 2005; Kesby et al. 2005: 144).

As a Taiwanese PhD student affiliated to a British university, I was an outsider to the two governmental institutions under study, which speaks to the wider issue of academics encountering difficulties in gaining access to elite communities (Harrington 2016). To get over this problem, I decided to apply for an internship at both institutions to immerse myself into their working cultures, and hopefully to create reciprocal and trustful relationships with policymakers, software engineers and civil servants. From a practical perspective, this dual positionality made it possible for me to collect contextual information on policymaking processes which are often 'gated' by elite groups and consequently have traditionally been 'out of reach' for researchers (Ibid.: 13).

In the following sections, I describe how I persuaded gatekeepers from the two governmental institutions to grant me internships by cultivating a sense of intrigue about my comparative study. I depict how the dual positionality assisted me to gain trust and help from both Spanish and Taiwanese colleagues throughout my practices of participatory observation and interviewing. I also discuss some tensions that emerged from interactions with political elites in the two ethnographic fieldworks due to the dual positionality.

8.2 Commencing the Ethnographic Fieldwork: Making an Initial Approach

Obtaining access to a given community under study using ethnography and qualitative methods such as participatory observation (Cook 2005; Kesby et al. 2005) and interviews (Valetine 2005) is essential. In particular, gaining access to elite institutions — for purposes of conducting interviews and participatory observation — has long been considered as a challenge for ethnographers (Harrington 2016). Elite individuals and communities are often particularly hard to research due to their powerful status in society (Ibid.: 135). As the aim of my research was to examine and compare the impacts of using digital platforms on policymaking and political participation, I needed to gain access to two elite institutions, the Spanish and Taiwanese governments. From the outset of my PhD, it was clear that it was these two governments that had the power to grant me access to potential research participants who would help to answer my research questions, and also held considerable power to regulate the information I could acquire in the field. It was more

challenging because I was conducting a comparative study. I needed to gain access to *both* Spanish and Taiwanese governmental institutions, each of which was situated within a specific cultural and political context.

Given this challenging circumstance, I found that creating a dual positionality as an intern and a researcher helped me gain access to the two elite institutions, as it helped ease practitioners' concerns about my presence. When I expressed my desire to work as an intern for Spanish and Taiwanese practitioners, I consciously and carefully located myself in a lower power position vis-à-vis these governmental officers whom I not only had to learn from, but also committed my time to working for free as an intern. From my previous working experience in Taiwanese government (prior to studying for my PhD, I worked as an urban planner for Taipei City Council for a year), I understood how governmental officials in Taipei represent and protect the sovereignty of the government that they work for in a way which prevents any challenges to their political power and authority. Therefore, I envisaged that governmental officials in Madrid City Council and the Taiwanese government could perceive researchers as either jeopardising their political positions or as bent on criticising their projects, without making any practical contributions of their own. By gaining a position as an intern, I attempted to assuage their concerns and worries, as this position indicated not only my lower power position, but also my dedication to work for them.

To aid my attempts to persuade the Spanish and Taiwanese gatekeepers to grant me internships, I researched both institutions thoroughly to demonstrate my understanding of the cultural and political contexts of the two DPPPs. I specifically wanted to point out what circumstances were common to both cases. Before contacting the gatekeepers, I investigated the two DPPPs by visiting their websites and conducting desk research on how Decide Madrid and vTaiwan were portrayed in various external research reports, news articles, and websites. I discovered common cultural and political contexts between the Spanish and Taiwanese governments, which would serve as useful starting points for conversation. Specifically, I found that the appointed heads of both programmes (Pablo Soto and Andrey Tang respectively) as well as the associated policy advisors and software engineers, all self-identified as 'civic hackers', many of whom had previously been involved in social movements in Taiwan and Madrid broadly located under the 'Occupy Movement' (the Sunflower Movement and the *Indignados*). As they were part of a wider shared civic hacking/social movement culture they held strong commitments to reforming and improving their governmental mechanisms for political participation.

Obtaining this initial understanding was particularly important to me as it allowed me to present them with 'culturally relevant knowledge' (Ting-Toomey 2010: 21, 32) which had the ability to convince and intrigue political elites in the Spanish and Taiwanese governments. During my interviews with Spanish gatekeepers (for the purpose of my research and for my internship application), I introduced my comparative study to them with an emphasis on the culturally relevant knowledge of Decide Madrid and vTaiwan that I had obtained via my initial desk research. I directly pointed out what was shared between Decide Madrid and vTaiwan by using terminology they were familiar with:'civic hacker', 'social movement' (the

Indignados/the Occupy Movement), 'open source software' and 'direct democracy'. After the interviews, both gatekeepers — the chief officer and the leading policy advisor at the Department of Citizen Participation at Madrid City Council — were pleasantly surprised, not only by my choice of a rare comparison of the Spanish and Taiwanese cases in the context of their shared connection with previous social movements in Taipei and Madrid, but also by my in-depth understanding of Decide Madrid. One gatekeeper explicitly told me that through my interview questions I had shown that my interest and knowledge of Decide Madrid was deeper than other journalists and researchers: typically, he said they were contacted only for one-time interviews. As a result of their 'liking' of my comparative study, I received an oral agreement to proceed on the spot, which was followed up with an official document accepting me as a three-month intern at Madrid City Council.

In the case of vTaiwan, the way in which I approached the relevant gatekeeper in the Cabinet Office of the Taiwanese government was more straightforward — it only involved sending an email. After demonstrating what I had learnt about vTaiwan with my research purpose in an email, I received an official confirmation that accepted me as an intern in the Cabinet Office without the need for any further meeting or interview. I assumed this was because I was a Taiwanese national whose PhD research was funded by the Taiwanese Ministry of Education. In addition, my intention to work with the Taiwanese government as an intern seemed to be appealing to the gatekeeper because an internship status implied that I would be in a lower power position than to policymakers. The Taiwanese policymakers warmed to the idea of an internship so much that they even established a formal internship scheme to enable other Taiwanese graduates to apply for temporary positions in the Taiwanese administration.

To summarise, despite using different methods to approach the two governments, I convinced both Spanish and Taiwanese gatekeepers to grant me internships. I did this by accepting a relatively lowly position as an intern and researcher, and by intriguing them with the commonalities between the two DPPPs that I had uncovered during my desk research. This indicates that applying for an internship, together with a thorough initial background study of both cases, can assist outsiders like myself in gaining access despite a lack of pre-existing connections with these elite governmental institutions (Cook 2005: 172; Harrington 2016). As conducting any participatory method is highly political and infused with complicated power relations (Cook 2005: 177), it is essential for researchers to recognise and navigate the specific power relationships that exist between researchers and the researched in each instance.

8.3 Being a Semi-Insider: Conducting Participatory

8.3.1 Observation Within Spanish and Taiwanese Governments

During my ethnographic fieldwork within the Spanish and Taiwanese governmental institutions, I carried out participatory observation as an intern and researcher who wished to learn about their everyday working cultures as manifested in daily

activities, events and meetings. This participatory observation was particularly enabled by my dual positionality fusing the roles of researcher and intern, which made me a semi-insider. As an intern, I was allowed to observe and take part in institutional events and activities in a way that other researchers could not as they did not have an official position in the Spanish and Taiwanese governments. I was not only given an ID card and an individual desk inside Madrid City Council; in Taipei, I was given access to the heavily-restricted Cabinet Office, where I was allowed to follow high-ranking policy advisors and consultants wherever they went. This access gave me a rare chance to work closely with governmental officials and immerse myself within their working cultures. From this specific experience, I was able to build a mutual and reciprocal relationship with the Spanish and Taiwanese governments, because I not only conducted my own research activities as a researcher, but also carried out tasks that my Spanish and Taiwanese colleagues assigned to me as an intern. This allowed me to build trust and helped me to gain multiple perspectives on the policymaking processes surrounding the two DPPPs.

In my early observation, I quickly noticed that the two governments shared a working culture that valued hard work. I then showed through my endeavours an attitude of respect towards and active engagement with this working culture. I immersed myself within everyday routines. In Spain, every day I went to my office in Madrid City Council at 9:00 a.m. sharp, where I started my day by saying *Hola, Buenos Dias* to the security guard and my Spanish colleagues. In Taiwan, every morning I passed through the wired security barrier to the Cabinet Office, after which I greeted my Taiwanese colleagues and was informed of the day's schedule. My tasks as a researcher and an intern were often entwined. When I attended different events, from face-to-face consultations to private meetings with overseas visitors and workshops, I observed these events as a researcher, as well as learning the practical and technical perspectives of the two DPPPs as an intern. In addition, I helped both institutions to deal with specific administrative tasks as an intern, such as observing the off-line public participation process (in the Taiwanese Government) and helping Decide Madrid to market themselves in Taiwan and Japan by phoning the relevant local authorities.

I discerned the fact that being seen at almost every institutional meeting and event *meant a lot* to both my Spanish and Taiwanese colleagues; they witnessed how I was actively living up to being an intern who worked hard at becoming part of their team. In Madrid, some civil servants liked to joke that they saw me everywhere, from small private internal meetings to semi-private usability tests. In Taiwan, other policymakers nick-named me as a 'hacker' or a 'shadow' who wanted to 'hack the Taiwanese Government'. Getting a nickname from both teams not only served as a sign of my close working relationships with my colleagues, but also showed that they recognised and appreciated my efforts as a hard-working intern. I started to forge mutual and reciprocal relationships with practitioners through my hard work.

As time went on, I found that my position as a semi-insider became even more important, as it provided me with an enhanced ability to collect multiple perspectives on the political impacts of Decide Madrid and vTaiwan. Firstly, by being

mindful and respectful towards both working cultures, I was able to gain trust from my temporary colleagues who were willing to speak to me further about particular issues facing the DPPPs. Achieving such trust, as argued by Prasad (1997: 113), can encourage practitioners to confide in the researcher or reveal aspects of their viewpoints on researched objects which are not typically shared with other colleagues. Being embedded within both teams, I initiated private and informal conversation with policymakers, software engineers or civil servants. I spoke to them about the DPPPs in small talks in the corridor, in parties or in the cafeteria. Having such impromptu conversations with practitioners matters because, as Cook (2005: 177) has noted, often the 'juiciest' information is acquired when practitioners let their guard down. By taking advantage of informal conversations, I acquired important information about Decide Madrid and vTaiwan and obtained official and non-official documentation from colleagues. These included usability test reports, internal reports on digital campaigning or the digital divide in the case of Decide Madrid, and draft policy documents for conducting face-to-face public participation in the case of vTaiwan.

Secondly, being immersed within and trusted by both Spanish and Taiwanese teams, I was able to trace and recruit former practitioners — typically software engineers and policy advisors who either used to work or worked remotely for Decide Madrid and vTaiwan, and held crucial information. Harrington (2016: 136–137) noted that immersive and participatory observation can provide deep insight into what has previously been taken for granted; in my context, this involved increased insight into concepts such as 'political participation', 'digital platforms' and 'algorithmic decision-making'. I identified these former practitioners via informal conversations and formal interviews with my colleagues and persuaded them to be interviewed. I did this by showing them how much I knew about the policymaking processes in which Decide Madrid and vTaiwan operated, and incorporating such information into my subsequent interview questions. For instance, I would name some of my Spanish/Taiwanese colleagues and use specific quotes from politicians or policy documents during interviews. In doing so, interviewees began to see that I could verify my *bona fides* as the interview went on. They could see specific information that I held had gained directly from my working relationships with both Taiwanese and Spanish government officials. As a result, I obtained a couple of interviews with off-site software engineers and researchers which would have been difficult to conduct if I had not worked within the governmental institutions. Off-site practitioners accepted my invitations to interview mainly because of my dual identity as an intern and a researcher in the Taiwanese and Spanish governments.

Inevitably, not being a native Spanish speaker created language barriers in my practice of participatory observation and interview (I will focus on cultural challenges that I encountered when interviewing Spanish and Taiwanese policymakers in the next section). However, being a semi-insider in Madrid City Council, I received tremendous help from colleagues who spoke fluent English and acted as my interpreters. They were extremely helpful in translating proceedings of meetings and sat in with me on interviews with civil servants who did not speak English. In

some rare and special cases where there were colleagues who did not speak English, their presence as interpreters was less intrusive in comparison with hiring an outside interpreter. They also understood technical jargon in relation to Decide Madrid better than outside interpreters. As I took part in a lot of meetings and events, I became aware that some information I received on highly technical and law-related issues was not clear enough, because they entailed Spanish usage of legal and administrative jargons, such as Spanish governance structure, public procurement and political participation legislation, which were often difficult to be translated. However, I was able to clarify such information by conducting further interviews and by obtaining political documents from policymakers and researchers if they were highly relevant to my research focus. As both Decide Madrid and vTaiwan were government-led projects, information related to policymaking, regulatory and digital divide issues were well-documented. With these internal or external documents in hand, I was able to cross-reference written materials with information that I acquired from interviews and observation.

Despite the fact that I tried hard to immerse myself with the two governmental institutions, I would only consider myself as a semi-insider precisely because my other identity as a researcher made my colleagues treat me with caution to a degree. For instance, my access to internal documents was still limited, in particular a specific report about Decide Madrid that I asked for. I was told by off-site practitioners about this particular report about assessing the performance of Decide Madrid, but not able to obtain it through my Spanish colleagues. This report may contain important information for my research. Having said that, as I continued to follow up with off-site and former practitioners after my internship, I managed to gain other crucial information and reports that used data-driven analysis to tease out Decide Madrid's impacts on political participation.

8.4 Interviewing: Negotiating Communicational Cultures of the Taiwanese and Spanish Governments

As a researcher, my interviewing practices were governed by various ethical principles (Centre for Social Justice and Community Action 2012) which required me to obtain informed consent and keep information anonymous and confidential. I had to apply these ethical principles within the context of the Taiwanese and Spanish governments, taking into account their different cultures of communication and political norms. During interviews, specific tensions emerged between myself and Spanish/Taiwanese interviewees, due to cultural differences in communication and power dynamics springing from my dual positionality and the higher status of policymakers. Importantly, these tensions were not 'open quarrels', but rather involved back-and-forth negotiation on precisely how much information I could acquire from them, and of what I could use in my PhD thesis. Tensions were not obvious but implied in the subtle usage of body languages and careful choice of spoken

languages. However, as I had built reciprocal working relationships with Spanish and Taiwanese practitioners, they could understand and sympathise that it was my job as a researcher to explore and examine the impacts of using DPPPs in citizen empowerment. The Spanish and Taiwanese policy advisors and senior officers expressed to me that they also respected my identity as a researcher since I had contributed much more than other the researchers they had encountered by virtue of working for them.

At Madrid City Council, some politicians, policymakers, and civil servants were defensive when being questioned during interviews, and made it clear that I could not use certain pieces of information they offered to me for my PhD research. Such information was related to a range of issues, such as party politics, and strategic and resource prioritisation issues within Decide Madrid. In response to this tension, I followed up lines of inquiry with off-site or former practitioners, whose ability to articulate issues related to the DPPPs was not inhibited by restrictions stemming from having political positions inside the government. With regard to critical information which I was not allowed to explicitly use in my PhD project, rather than using this as specific evidence I used it as 'insights' which could be used to inform my future postdoctoral research. I used information only if it fitted the aforementioned ethical protocols.

In the Taiwanese government, when I sensed officers trying to dodge answering some parts of questions during interviews, I would repeat questions to them with a lot of listening, patience and guided encouragement during the interviews until they provided me with clearer answers. Dealing with such tensions required the use of my understanding of the communicational culture within the Taiwanese government that I had gained as an intern. During my internship, I understood that Taiwanese policymakers tended towards modulating their speech to guard against potential adverse political impacts, and did not value critique and conflicts in opinion. When I noticed interviewees — from senior officers to politicians in the Cabinet Office — not directly answering my questions which were intended to explore where the problems within the digital mediated policymaking processes are, I did not immediately chase such questions up with them. Instead, I listened to their 'defence' of the governmental narrative of vTaiwan without judgement. I waited for a gap in their speech when I could ask the same question again with a clearer explanation and patience. In one instance, an official was guided and encouraged to answer questions which he considered difficult, such as 'To what degree does vTaiwan empower citizens?' and 'What are the key issues of vTaiwan in citizen empowerment?'. Similar to the Spanish case, here, I acted as a researcher who used my power to ask critical questions that some Taiwanese officials would find difficult to answer for cultural and political reasons. As I expressed how I learnt these questions from my internship and their importance for my research, the Taiwanese official understood my contribution to the Taiwanese government and my other job as a PhD researcher. As a result, he became more direct in revealing information as I continuously asked him the same questions in different ways during the interview.

In response to these tensions, the bottom line was that before I conducted interviews, I made sure all interviewees acknowledged that the data and information they

provided would be treated anonymously, transcribed and only used for my PhD research (Ibid.). In particular, I let them know that all interview data would be destroyed 1 year after I finished my PhD thesis and that they could withdraw from participating in my PhD research *at any time*. This information was provided to interviewees via a written consent form in both English and the local language (traditional Chinese or Spanish) before the interview was conducted. During the interview, I also explained this information again to the interviewees. Very rarely, in a situation where I initiated impromptu interviews with software engineers and policy advisors (often at conferences), I verbally explained what was written in the consent form and asked for verbal consent from them.

8.5 Conclusion

This chapter has discussed how I conducted a set of ethnographic practices — gaining access, and conducting participatory observation and interviews — with a dual position as an intern and a researcher, in the context of a comparative study of Spanish and Taiwanese digitally-mediated policymaking processes. I assert that it is this dual positionality that made it possible for a PhD student like myself to obtain access, recruit high-profile interviewees and collect insightful information from two hard-to-reach elite institutions — Madrid City Council and the Taiwanese government. Of course, there were limitations and challenges that came alongside this dual positionality in terms of partial access to internal reports, long-working hours and heavy workload in comparison to adopting a single identity as a researcher, some of which is beyond the scope of this chapter. This is mainly because I find these challenges less important than the pragmatic benefits that such dual positionality can offer for a PhD researcher.

This dual positionality made it easier for me to immerse myself into Spanish and Taiwanese working cultures and thus helped me to create a trustful and reciprocal working relationship with Spanish and Taiwanese practitioners. By working hard for the two institutions, I was able to ease some concerns that often exist between researchers and elite institutions, such as those related to the use of sensitive data. I was able to collect rich and diverse information from policymakers, software engineers, politicians and other practitioners in a way that I respected their institutional cultures and normative modes of communication. Building reciprocal working relationships meant both Spanish and Taiwanese officials could better understand the tensions playing out in interviews when I acted as a researcher who asked questions that they might find difficult to answer. Practitioners from each institution could see and understand that I was doing my other job at those times — a researcher who needed to collect insightful and different perspectives to examine the impacts of two DPPPs.

Without being a semi-insider, I would not have been able to gain access to the elite communities which I needed to research for my study (Cook 2005; Harrington 2016). I would not been able to hear diverse viewpoints on the issues I was studying

(Prasad 1997) and would have potentially misinterpreted the respective institutional cultures. As any ethnographic practice is an intervention in a specific community, it is crucial for researchers to pay attention to the different power relationships that their presence produces with local actors when using ethnographic methods in comparative studies.

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Chapter 9
(M)otherhood, Identity and Positionality In and Out of the Field

Mildred Oiza Ajebon

Abstract In this chapter, I demonstrate that researcher positionality such as motherhood status matters beyond data gathering activities during fieldwork. I reflect on how aspects of my identity as a parent, directly and indirectly shaped the topic of my research enquiry and influenced the ways in which I experienced fieldwork and the postgraduate research process in general. I argue that researcher identity, positionality and power relations play important roles in establishing researcher credibility and reconfiguring insider-outsider status, data interpretation and representation. Drawing on encounters from the field, I examine the dilemmas of doing fieldwork as an accompanied researcher with an infant child and a dozen research assistants. The aim of this reflection is to highlight the invisible dilemmas of motherhood/parenthood status, especially for early career female researchers who combine caring, earning, and learning. An improved understanding of these issues is important for providing relevant support for researchers with young families in the workplace, especially in academic institutions.

Keywords Motherhood/parenthood · Identity and positionality · Insider-outsider status · Power relations · Nigeria · Accompanied fieldwork

9.1 Introduction

> Who you are, who you think you are, who you present yourself as being interact with who other people think you are, who they think you want to be. The combination of presentation and perception makes the difference to what you experience in social interactions and what you can possibly learn from them (Townsend 1999: 88).

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Qualitative researchers have increasingly reflected on issues of identity (Hewitt 2007), positionality in research and 'the subjectivities that are inscribed in our work' (Mullings 1999). Researcher positionality refers to the ways in which a mix of multiple aspects of researcher identity — in terms of motherhood, ethnicity, race, gender, class, nationality, educational qualification, age, and disability — relationally shapes, within power hierarchies and in a given space and time, our perspective to data generation, interpretation, and representation (Frost and Holt 2014; Mullings 1999). Social science enquiries must pay attention to this potential bias and the researcher, as an actively involved observer and participant, rather than an objective recording instrument. It is important to consider the ways in which aspects of the researcher's multiple identities, and the specific circumstances in which knowledge is produced (Rose 1997) make important contributions to the fundamental goals of social science research (Townsend 1999). Researchers need to examine critically how aspects of their own positionality in relation to shifting hierarchies of power relations are shaped between the researcher and the researched (Skelton 2001).

It is recognised in the academic literature that different aspects of a researcher's multiple identities may be projected or concealed, depending on the context, in order to gain credibility on the part of research participants (Godbole 2014). However, there are aspects of a fieldworker's identity that may be difficult to conceal, such as gender and parenthood, especially when accompanied by a child during fieldwork. 'Not-concealable identities' have varying implications for data collection and analysis. According to Townsend (1999), parenthood is a gender-based identity and fieldworkers' gender has been the most observable aspect of researchers' multiple identities to be recognised and examined. He notes that anthropologists have long argued for the implications of parenting and parenthood for fieldwork to be made more explicit in academic research (Ibid.). Weiner (1976) has shown that an approach that pays attention to the role of gender-based identities, not only in the people being observed but, in the people, doing the observing, does not obscure reality, but illuminates it. In order to illuminate the implications of motherhood for field research in actual practice and the fluidity of the insider-outsider positionality, this chapter presents reflections on fieldwork-related encounters as I negotiated my own position in my own eyes and in the eyes of my research participants. Drawing on insights from the academic literature and verbatim comments from my research participants in Nigeria, I highlight the mixed implications of motherhood status in gaining researcher credibility; negotiating insider-outsider status; and the ways in which child health risk knowledge is produced, field data interpreted and how representations of research subjects emerge. This chapter is sectioned into three. First, the role of my motherhood status in shaping the topic of my research enquiry is presented. I then examine the practicalities and impacts of doing accompanied fieldwork on researcher positionality and power relations. Lastly, I discuss subtle and complex power dynamics using examples of typical encounters with other mothers in the field, and the need to rethink insider-outsider identity claims.

9.2 Can I Interview Mothers of Young Children as a Non-Mother?

In this section, I discuss how changes in the context of the research project occurred due to changes in my identity as a PhD researcher. I originally designed my PhD research to investigate the resilience factors in selected communities within southern Nigeria where under-five mortality rates were significantly lower than the national average, using a mix of quantitative and qualitative methods. The early stages of the research project involved the analysis of publicly available secondary datasets from the Demographic and Health Surveys (DHS) in Nigeria. Based on the secondary data analysis, I identified 15 urban and five rural field communities in Edo State, southern Nigeria, for more detailed questionnaire data collection and semi-structured interviews.

During the planning phase for my fieldwork, one of the main concerns of conducting interviews with mothers of children aged under 5 years was whether they would be opened to sharing their child's health risk experiences with someone who was not a mother. A friend had asked me this question. I did not have an answer. I could not fully anticipate what my non-mother status would mean for my research, but I was determined to do my best. The vast literature notwithstanding, Merriam et al. (2001) have pointed out that given the issues of positionality, power, knowledge construction and generation in social research, it is almost impossible to anticipate fully the roles and implications of our shifting and multiple identities, positionality and power without actual fieldwork. My friend then said:

> It should not be too bad. At least you are married so you have something in common with your potential participants. I can only imagine what it would mean for a single woman like me to do your kind of research back home.

That question was a very sensitive one because it had implications beyond my research. It was personal in many ways. It connected my body, my personal and academic life, and it plagued my mind throughout the planning phase of my fieldwork. It was personal because I was a married woman in my 30s with no children. During the planning phase of my fieldwork, I was anxious about potential attitudes, usually reflected in phrases like 'it takes a mother to understand', getting in the way of honest conversations. Doing fieldwork in my home country Nigeria, I was well aware of the subtle stigma of not having a child at my age in a culture where many women tend to have their children in their 20s. I was also worried that my many years of education as a woman might be considered as amounting to almost nothing without children, as a family member did tell me that in the same month I left to the UK. However, I became pregnant in the second year of my PhD. For health and safety reasons, and in line with the advice from my supervisors who showed me incredible support throughout the PhD process, I then had to postpone doing fieldwork until my long-awaited son was born. My positionality then changed from a non-mother to a mother.

I spent most of the period of pregnancy doing further literature reviews and exploration of the DHS data. This intensive investigation marked a significant shift in the course of my research. The additional statistical analysis I conducted unveiled high levels of inequalities in under-five mortality rates across different population segments in Nigeria in ways that I had not previously paid attention. I became particularly drawn towards understanding why the wide health gaps I observed between geographical areas and social groups occurred. The focus of my PhD research evolved dramatically from health resilience research to understanding the determinants and perceptions of child health risks in Nigeria. As a mother-to-be, I could not bear the thought that the 14.5% under-five mortality proportion, derived from the DHS data I worked with, represented real children and actual tragedies for many families. I wanted to do something about it. I saw it as my role as a social scientist to illuminate societal problems such as avoidable causes of and inequalities in child mortality until they can no longer be ignored. I became interested in and explored child health risks by asking 'why' and 'how' types of research questions. My new research focus became twofold. First, to statistically examined the main geographical and social determinants of inequalities in under-five mortality across different geographical scales using quantitative methods. Second, to utilise qualitative methods to understand how mothers perceive and experience child health determinants and resilience factors in their everyday lives as discourses of resilience and risks often go hand in hand (Cairns et al. 2012). I designed my semi-structured interviews to reflect this new focus.

However, the need to narrow down my research focus to health-risks and to leave out completely issues of health resilience became very clear to me as I began to interview my participants. Mothers were more inclined towards discussing the health-risk factors within the neighbourhoods in which they lived and what they did to address these challenges, rather than talking about resilience. Economically and politically, Nigeria was in a bad state at the time of my fieldwork. In the year preceding my fieldwork, there was a global recession in the crude oil market, which is the mainstay of the Nigerian economy. Exchange rates had changed dramatically and the Nigerian Naira had weakened significantly against the USA dollar. Given that Nigeria is a country with high dependency on imported goods and services, the prices of many products including food and groceries had tripled and unemployment rates had doubled. There was a general sense of economic hardship and insecurity. These were reflected in the narratives of many research participants. Women were more inclined to talk about their experiences of risks than their strengths. This experience demonstrates the need for mixed-methods researchers doing semi-structured interviews to be flexible. The predetermined interview guide could change depending on prevailing socio-political circumstances of the fieldwork context.

I was a new mother when I arrived in Nigeria to conduct fieldwork. All of a sudden, I had not just shared a marital status but also a shared parenthood status with my research participants. I assumed that I was an insider because of this shared status, and this simplistic assumption was challenged in significant ways, which are discussed later in the chapter. My identity as a new mother significantly changed the

trajectory of the study. As Crang and Cook (2007: 3) have rightly observed social research processes, in practice, do not fit into a linear model of planning, doing and writing. They noted that the practicalities of research involve a messier process, and fieldwork plans sometimes 'go off the rails'. Research designs evolve and sometimes, new fields, both physical and conceptual, are discovered. Researchers must be prepared to deal with unexpected twists and turns, both personal and theoretical, which will inevitably be experienced. 'The field is not a bounded entity separate from the everyday life of the researcher. The field is rather a constructed entity that does not have identifiable borders' and the multiple blurrings between the body of the researcher, personal and professional life could shape the knowledge produced (Cupples and Kindon 2003: 212).

9.3 Doing Accompanied Fieldwork

What then happens if you need to gather data in the company of or with other people? What happens when your closest research assistant is 5 months old? Accompanied by my son, I no longer fitted into the dominant image of a lone researcher doing fieldwork in far-flung places. The image of an accompanied researcher is rarely documented in social science research methodology. The accounts of Cupples and Kindon (2003) and Lunn and Moscuzza (2014) are exceptions. In sharing experiences of the different dimensions of accompanied fieldwork in India, Lunn recognises that fieldwork images in the Global South are dominated by those of solitary and foreign researchers doing fieldwork in distant places. The reality in the field is that accompanied status — family member, friends and colleagues — is more common. Godbole (2014) rightly argues that the experiences of social science researchers during fieldwork might vary depending on whom they are accompanied by. The accompanied researcher status might be more significant for female researchers with children than their male counterparts because women are perceived as the primary carers of infants in society. It might not always be the case that researchers with families have the power to decide whether they are accompanied for fieldwork or not (ibid.). In support of their argument, I demonstrate that the power of female postgraduate researchers to choose whether to be accompanied by their partner and children may depend on a complex mix of factors, such as the ages of the children involved, availability of partner, institutional restrictions, funding conditions, immigration rules, access to social capital and affordable childcare resources.

My son was 5 months old and exclusively breastfed when I began my fieldwork in third year of my PhD. Conscious of my strict funding and UK Tier-4 visa conditions, I had no choice but to begin my fieldwork whilst my child was still little. I had no power to choose otherwise. My husband could not come with me to the field because, as a PhD student himself, he was also bound by strict visa restrictions on leaving the UK. The regulations indicated that he could not be away from his studies for more than 4–6 weeks annually. These strict regulations also deprived me

of access to paid maternity leave beyond 6 weeks. Home students are allowed to take up to 6 months of paid maternity leave but international students in the UK who required more than 6 weeks of maternity leave — paid or unpaid — had to return to their home countries with their visas confiscated. They then had to reapply for another Certificate of Sponsorship and pay for a new student visa when the period of leave was over. For me, there was no home to return to because my husband was in the UK doing his own PhD. I had taken study leave from my job in Nigeria and therefore had no extra income outside my PhD stipend. I could not afford the extra physical, emotional and financial costs of travelling between different cities in Nigeria in the company of an exclusively breastfed baby to apply for replacement student and dependant visas for my baby and myself. It was against this background that I imposed the decision on my child to accompany me for fieldwork.

I was also accompanied by 12 research assistants (RAs). Part of my fieldwork plan was to administer about 2000 questionnaire to adult women of reproductive age (18–49 years) in the selected fieldwork communities within 4 months. The plan to work with 12 research assistants to meet this questionnaire target was not entirely determined by being accompanied with a child, but the decision to double the number of RAs from six to 12 was influenced by my motherhood status given that the physical labour of carrying a young child with me on a day-to-day basis in the field would limit my speed and extent of data gathering. My RAs were my ex-students, and I felt the pressure to ensure their health and safety because I considered myself as their guardian in the field. The involvement of my ex-students as RAs constituted a two-way relationship of 'helping me' and of a continued teaching and mentoring process for them. My motherhood status made it easy for me to be identified as the leader of the group, as being a parent is often considered to be synonymous with maturity in many settings (Baker 2010). A more detailed discussion of the ethical implications of doing fieldwork with research assistants is beyond the scope of this chapter.

9.4 Encounters in the Field: A Mother Interviewing Other Mothers

As a new mother and an accompanied researcher interested in understanding the agency of women in relation to inequalities in child health-risks, I must acknowledge that my research participants and I were not mere research objects, but subjects with agency, distinct histories, personal idiosyncrasies and power (Godbole 2014). Our shared identities of motherhood, beliefs, and values influenced the interview trajectories, perceptions, the memories created, and how child health-risk knowledge was shared. My identity and positionality shaped my 'research eyes' and the lenses through which I observed and understood. It is evident that having a shared motherhood identity with my participants earned me credibility and empathy from my research participants, especially in the participant recruitment phase. Participant

recruitment for the one-off interviews I conducted was through door-to-door knocking. This face-to-face recruitment method is aided by the informal nature of community relations in Nigeria, and because of limited infrastructure for prior communication. Whilst it was easier to access the homes of mothers, living in rural and urban poor areas compared to mothers in wealthy urban neighbourhoods, characterised by high fences and massive security gates, showing up in front of people's homes in the company of a 5-month-old earned me significant receptiveness from mothers. However, I have to admit that with my motherhood status also came a myriad of ethical challenges during fieldwork. These challenges occurred from what Merriam et al. (2001: 209) have described as 'the interlocking [changing] nature of culture, gender and balance of power'. It is difficult to create a static balance of power during interviews. Researchers have to constantly negotiate power dynamics with their participants at every stage of knowledge construction (Harrison et al. 2001).

An awareness that power is subtle, fluid and complex, and has to be negotiated during fieldwork, and not given, broadens the 'understanding of and responses to the subtle, changing and shifting balance of power that pervade and impact on the social research process' (Frost and Holt 2014: 1). In my experience, such subtle and shifting power relations were manifested through perceptions of the researcher's identity in terms of age, class and educational attainment. For example, I was often asked by participants, who perceived me as a poor student, whether I needed to conduct the interviews to graduate from university. I was not sure if these participants understood the difference between a first degree and a postgraduate research programme, but I tried to explain. I was also asked by many participants to specify how I wanted the questions to be answered, because they thought that survey and interview responses were to be assessed as either right or wrong like elementary school homework. I was again not sure if I succeeded in convincing all participants that I was genuinely interested in understanding the health-risk issues that children faced in their everyday lives. Perhaps they merely saw me as a mother who was eager to learn for the sake of my own child.

The ages of my research participants and those of their children meant different things and influenced the research process and my positionality in many ways. My research participants were adult women aged 18–49 who are parents or guardians to at least one child under the age of 5 years. Some of the women who had other older children in addition to the under-five child felt the need to instruct me on childcare risks and ways in which to respond. Many of these women easily assumed they were older than I was and therefore were more knowledgeable about child health risks than I was. I did not expect these tensions. My transition from a non-mother to a new mother made me assume that I was an insider because of my shared motherhood status with my participants, but such encounters reminded me that issues of positionality are much more subtle and fluid and have to be negotiated. My positionality was not something that I simply ascribed to myself but what my participants ascribed to me on the basis of who they perceived me to be (Reeves 2010). It led me to pay attention to other aspects of my own positionality and those of my participants such as levels of education, class, power, and privilege and other qualities.

Difference in educational status was also a significant area of discomfort. Whilst my educational attainment appeared to be a threat to the husbands of many of the rural and less privileged urban mothers, it was not an obvious threat to the mothers who voiced similar educational aspirations for their children. Many of my interviewees in the rural areas and poor urban areas were keen to support my research to 'sow good seeds' in the hope that their children would become well educated enough to conduct their own independent research in the future. On my part, I was very moved by the poor literacy levels and living conditions of some of my participants, especially those in the rural areas. I was particularly moved that many lacked adequate access to potable water supply, which was a major risk factor for childhood diarrhoea. I felt guilty that whilst they contributed to my research, I had nothing to give back to these women. I decided that I would reciprocate their kindness by drafting a brief report to the state government to highlight the child health risks issues that had emerged from my research.

Comparatively, wealthier women in Benin City were different. First, my educational attainment was not a threat, they implied, to many of the wealthier urban mothers in high-class neighbourhoods. Some of the wealthier women pointed out that they did not bother to study further since they had all they needed to live a good life. Furthermore, they, despite having lower levels of education in comparison with me, did not perceive a postgraduate qualification as a priority. I was not sure if they discounted the relevance of postgraduate education because a more educated woman was interviewing them. In the interviews, they emphasised their socioeconomic privilege and thought that their children faced significantly lower health risks compared with children born to poorer mothers. My encounters with these mothers demonstrate that inequalities in power are a critical factor in social encounters during fieldwork (Merriam et al. 2001). On the one hand, my position as an academic staff of a university in Nigeria facilitated access to gatekeepers and participants. On the other hand, my research participants subtly repositioned my 'powerful' status by determining when and where to be interviewed, the information they shared and how they chose to perceive me as a researcher. I could not help but admire their affluence and wished that my academic career could afford me the same. I usually returned home from such encounters feeling discontented with 'low' remunerations of academic careers, especially in Nigeria. I constantly reassessed what my own priorities were, and, questioned what my priorities ought to be, a mother, a researcher or 'making' more money.

9.4.1 'Get Your Priorities Right: Be an Academic or Be a Proper Mother'

As an accompanied researcher with an infant, I was perceived by many mothers as having the credibility to conduct research on child health-risk. Most of my research participants who agreed to get involved in the research saw me as both a poor

student and a brave mother who needed their support to fulfil an important requirement of earning a degree. There was a general feeling of pity for my son, who many thought was being inconvenienced in the process. However, some participants did not hesitate to express their disapproval for what several described as 'going up and down' with a young child. The term, 'going up and down' in that context was derogatory. It suggested my failure to prioritise domestic life over earning a living, as captured in the following quote by the husband of a research participant who vehemently suggested that mothering was more essential:

> Get your priorities right, be a stay-at-home mother or wife or be a student. You are a woman, what exactly are you chasing after?

No one asked why my husband was not looking after the child whilst I was in the field. I wondered if a father would have been expected to prioritise childcare over a PhD programme. To many of my participants, doing a PhD was too ambitious for a woman and meant neglecting essential family responsibilities. Some of the husbands of my participants, who were sometimes present during interviews, did not support women pursuing 'too much education'. I, on the other hand, also received encouragement from many men, mainly the partners of research participants, who expressed their desires to educate their own daughters to a postgraduate level.

As a result of these encounters, many questions plagued my mind. Do women always have to prioritise family life over career? Do we always have to choose, to retain, and to let go? In practical terms, managing a young family and academic research simultaneously was extremely difficult as well as rewarding for me, but are we even allowed to own up to a mix of emotions? Will admitting the everyday struggles of blending both personal and academic life be considered as weakness? A colleague once suggested to me over coffee,

> Be careful how you own up to the challenges of managing a young family and PhD life around here. People will think that you are weak or incompetent. Academics do not show weakness, they let it stay at home.

As a researcher, I felt similar emotions too, not just during fieldwork but also throughout the research process. I constantly questioned whether I was getting my priorities wrong. I was always conscious of the implications motherhood for my career. This is what Baker (2010) refers to as the 'child penalty' — a term used to conceptualise the earning gaps between young mothers and women with no children. I felt like I was not a proper mother because I spent many long odd hours doing research at the university away from home. I did not feel like a proper student either. I had to work at odd hours and late night on most days including weekends, which left me feeling even guiltier. Although flexible working is common amongst PhD students, caring for a young child made it difficult for me to maintain a more structured routine like many of my cohort had. There were times I wanted to quit and be a proper mother to my child. Other times, I drew strength from the need to be a role model for every girl child out there. Many times, the gentle encouraging words from my husband were priceless. 'Babe, I know this is hard but you can do it. I believe in you.' They were the much-needed oxygen for my PhD lungs.

Whilst shared motherhood and accompanied researcher status undoubtedly granted me social access, and earned me credibility and empathy with many of the mothers I interviewed, these often placed me in what Greer-Murphy (2018) describes as uncomfortable positions. These uncomfortable positions reminded me of the complexity of my own position and the danger of assuming that I was an insider simply because my field sites were in my home country. These incidents point to the need for researchers doing fieldwork in multicultural sites to be sensitive to fluid positionality and the importance of behaving in a culturally appropriate manner during fieldwork. I recall two independent conversations that I had with separate women during my fieldwork in one of the rural areas during May 2017.

'Being accompanied means being observed in those relations by those we study' (Cupples and Kindon 2003: 223). The first instance that still resonates strongly with me borders on what Chong (2008) described as the quandary of conformity and involved a reaction from a potential participant, which made me rethink my insider status. This participant was an older woman who turned down my request for an interview because she was offended that I used a baby carrier like an '*oyibo woman'* (white woman). She thought it was culturally inappropriate to strap my baby in a baby carrier in front of me, rather than using the traditional method of strapping babies to one's back with rectangular pieces of fabric known as 'wrappers' in Nigeria. She was particularly upset by how my child's hands and head were positioned with the carrier. To me, my baby appeared comfortable enough and was sleeping soundly at the time of this encounter. She said that she had observed the baby and me for some time before I approached her but could not hold back her anger. She felt I was being too 'western' and that I had put my elite and '*oyibo woman'* status before my child's comfort. She felt that my 'innocent baby' was being 'punished' because the baby carrier did not appear comfortable to her. I apologised and tried to explain that I genuinely did not know how to 'tie wrappers' in the Nigerian way, let alone strap a baby with it. I related how I had tried over the years to learn without success, and that it was not very practical to travel around my fieldwork communities with a baby strapped to my back with Nigerian wrappers. My explanations annoyed her even further. I was not sure how convincing my reasons were. Many people, including friends and relatives back home in Benin City had expressed their disapproval of the baby carrier on various occasions. My reaction was usually 'this is what works for me so mind your own business.' Although baby carriers are widely used and considered safe in the UK where I was a PhD student, most people who verbally voiced their disapproval in Nigeria thought the opposite. Their disapproval came across more as a negative judgement of my competency as a mother. Their opinions mattered very little because I was sure that it was safe for my child. However, the perception and reaction from a potential participant who refused to get involved in my study because she perceived me as being a culturally inappropriate mother in a rural setting carried more weight and hurt more than the opinions of my friends and relatives. This speaks to the importance of power-relations and cultural access in social interactions during fieldwork. After the incident, I decided to strap my baby on my back with the 'oyibo woman' carrier but had to put a wrapper over it (Fig. 9.1) for the rest of the fieldwork period in the

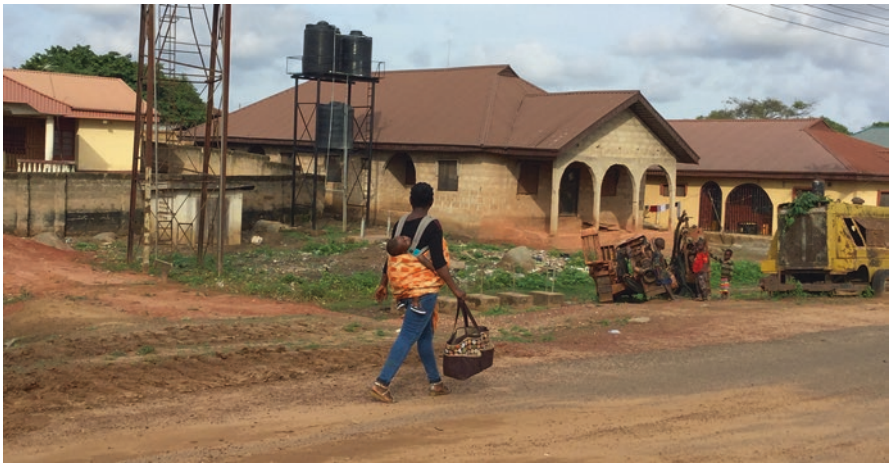

Fig. 9.1 My child was strapped with both a baby carrier and a traditional Nigerian wrapper, May 2017. (Source: Author's)

rural areas to avoid similar damaging judgements. My perception and utilisation of baby carriers changed significantly after that encounter. I remembered that embarrassing incident every time I used the carrier afterwards until I stopped using it altogether. The carrier did not return with me to England.

9.4.2 A Prim and Proper City Mother

Another encounter with a participant in a rural case study area during a questionnaire survey reminded me that I did not totally fit in as an insider. She, who finished secondary school, was more educated than most other research participants in that rural community. She wanted me to stay with her a little longer before leaving because she was concerned that the heat from the 'hot sunshine' would cause illness for my baby. I sat back to please her. Her daughter was about a year old and was playing in the wet bare ground in front of her small grocery shop. It had not rained that day so I was not sure where the wetness in front of the shop came from. Then she said:

> Participant: You have been carrying him in your hands since you got here. Put your son down and let him play with my child.
>
> Me: … Thank you Ma'am, he is still too young, he cannot sit without falling over yet.
>
> A few minutes later, she offered me a sachet of packaged water to give to my child. I was hesitant and she noticed it.
>
> Participant: Give your son some water to drink, [handed me the packaged water]. It is very hot today, you need to give him water regularly to avoid illness.
>
> Me: Thank you so much madam for your kindness, but he cannot drink water yet, he is on exclusive breastfeeding. [I began drinking the water]

> Participant: [Laughed] … You city mothers eh, always wanting to be prim and proper. You really don't want him to drink the water, do you?

I was not sure if she was offended, because we both laughed, and I carried on explaining until one RA came to interrupt the conversation with work matters. I used the opportunity to excuse myself, thanked the participant profusely for her kindness, and departed.

I realised that although I was doing fieldwork in my home country and near my hometown, Nigeria was too ethnically diverse for me to be entirely considered an insider, or for me to feel completely 'at home.' Although my RAs and I had 'dressed down,' and communicated with many research participants in Pidgin English, I could not entirely fit into the informality of rural society. Perhaps it was also obvious to that participant that I was being over-protective of my child. Power dynamics and positionality appeared much more complex as an accompanied researcher with a baby on top of the culturally diverse context of Nigeria. As an accompanied researcher in the field, my participants were observing my practices not just as a researcher in relation to the cultural context of the communities I researched in Nigeria, but also in relation to my baby. Cupples and Kindon (2003) are right, those we study not only observe us, but also observe our relations with those who accompany us.

During fieldwork, I was very careful about my accommodation, childcare and feeding. I tried to attend to every detail in caring for my son. I was very selective about what he ate, drank, or touched. My son and I judiciously slept under bed nets and attended the best nurseries. Yet, he still suffered three severe episodes of malaria that landed him in A&E each time. He had one episode of diarrhoea that halted my fieldwork for 2 weeks, in addition to developing skin rashes on his face. The rashes left two major scars on his nose and were yet to fade as I wrote this chapter. My husband and I call these scars, PhD scars. Although researchers, and those who accompany them, do not always get scarred physically from doing fieldwork, a series of encounters and varied experiences, both expected and unexpected, planned and unplanned, influence the ways in which we derive meaning from the research output, our world views, who we are and, perhaps, who we become.

The encounters presented above clearly indicate that issues of identity, positionality, and power are complex and subtle. My experiences demonstrate that it is too simplistic to assume that researchers doing fieldwork in their home country are insiders and those doing fieldwork abroad are outsiders, and that each status has inherent implications for research outcomes. Critics of positionality in social research have noted the perpetuation of an imposition of western bias, which tends to inhibit more fluid 'positional' engagement in fieldwork. In identifying the limitations of common assumptions about what it means to be a member of a socio-demographic community, Frost and Holt (2014) have argued for the complexity surrounding insider and outsider states to be made more explicit. Vanner (2015) has demonstrated that it is overly simplistic to talk of membership of a community in terms of stark socio-demographic identities without addressing questions of power and privilege. Arguing for the fluidity of researcher positionality and power,

Merriam et al. (2001) have pointed out that the real world of data collection is complex, and that in practice researchers tend to slip between insider and outsider states and shifting positions of power and privilege. It becomes problematic to conceptualise insider-outsider status in terms of neat and binary positions into which researchers can be predominantly categorised. In my case, for example, I largely assumed that I was an insider, but this simplistic assumption was challenged by the subtle way in which I was repositioned by some research participants as a cultural outsider. My position could not be categorised simply as a mother interviewing other mothers, or a black female researcher interviewing other black females in her home country. My experience demonstrated that there are differences between an international student researcher and a local researcher; an elite researcher studying in one of the top 100 universities in the world interviewing mothers in a rural setting in a developing country; or a local mother interviewing her own people at home. Critical and feminist theory, participatory (action) research, postmodernism, multicuturalism and the many of the other philosophical 'isms' have demonstrated that it is more meaningful to frame insider-outsider positionality within and across one's culture in terms of gender, class, race, ethnicity, culture, educational attainment and other identities. Although academic work on positionality and associated dilemmas is accumulating, only through doing fieldwork can researchers personally encounter these dilemmas (Frost and Holt 2014; Merriam et al. 2001; Mullings 1999; Rose 1997; Skelton 2001).

Beyond the field, issues of positionality and power transcend data gathering activities into analytical spaces. 'When it comes to thinking about how to analyse our data, we cannot ignore the phenomenologies — the experiential materialities — of bodies and places during fieldwork' (Matsutake Worlds Research Group 2009: 201). In my case, experiences of infertility, being accompanied by an infant child, and dealing with childhood diseases such as malaria and diarrhoea in the field as a mother first-hand, contributed to my analytical perspectives and research understanding. As a researcher doing research in her home country, I was drawn towards perspectives that are less emphasised in global health research the role or women agency and the need to understand ways in which mothers in Nigeria assume responsibility for child health risk management. It is important to recognise that research perspectives from insiders, outsiders and those in-betweens are needed to gain a broader understanding of child health risk perception and management in Nigeria. Researchers must acknowledge the usefulness and limitations of diverse ways of knowing depending on where a researcher is situated within the insider-outsider spectrum. No position should be elevated above the other because what an insider sees and understands will be different from, but will be as valid as, what an outsider understands at the end of the spectrum. These positions evolve, and are fluid and relative to the cultural norms and values of both the researcher and the participants (Merriam et al. 2001: 415). As a mother researching child health risks, I must recognise that my experiences in the field and my personal subjectivities were inscribed in my work and influenced the object of my research enquiries in significant ways. According to Townsend (1999), parenthood status, in addition to other factors, can influence the practicalities and theoretical objects of research.

Like him, my status as a new mother influenced the choices I made in the field, the timing of my fieldwork, my theoretical perspective and research understanding, the research themes that emerged, the positions that I assented and critiqued, the voices I made visible and those that I unconsciously let slide into obscurity. It influenced the interpretation and meaning that I derived and the representation of the voices of my generous participants.

9.5 Conclusion

Through reflecting on my fieldwork experience as a PhD student and a mother, this chapter has demonstrated that the salience and significance of different aspects of a researcher's positionality at different stages of the research process might depend heavily on the topic of research enquiry and the identities of research participants (Frost and Holt 2014). Undoubtedly, the relevance of the researcher identity to the research enquiry does not remain static throughout the research process. The influence of unexpected changes and the ever-shifting personal subjectivities of the research work may begin as early as the construction of a research enquiry. This chapter has also discussed the practical implications of researcher identity for gaining access, credibility, and legitimacy on the part of research participants. For researchers doing child-related social research in particular, motherhood status can interact with the timing of fieldwork, the way in which the research enquiry is framed, what the research participants choose to share, the nature of data collection, and the kind of interpretations that are made visible. In line with Townsend (1999), parenthood may have different implications for male and female researchers, and experiences of researchers in the field might depend on whether researchers are accompanied, the ages of their children, and differing access to social networks, supporting services and available funding. It raises pertinent questions about whether the intersection of motherhood status with other personal identities may enhance credibility or produce discomforts during fieldwork practices. By reflecting on encounters from the field, I have illuminated the intricacies, complexities, power dynamics, the fluidity and interlocking nature of insider-outsider positionalities that I encountered in knowledge construction and representation. I challenge the notion of stark positionality and ready-made identity boundaries into which researchers are often situated. This chapter has provided stories and evidence to show that the field is a site where the professional and personal lives of the researcher converge. It is a leaky space in which 'relationships with participants shape and are shaped' by the relationships with those who accompany the researcher (Cupples and Kindon 2003: 212). We might need to reposition ourselves as our participants in turn reposition us. Being able to recognise and renegotiate our shifting identities and power remains one of the most important reflexive tools at the disposal of human geographers at all stages of the social research process.

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Part III
Writing the Field

Chapter 10
Recognising and Addressing Secondary Trauma: Stories from the Field

Zuriatunfadzliah Sahdan

Abstract This chapter investigates my reflexivity as a Malay-Muslim woman and early career researcher, conducting participatory research with women traumatised by experiences of domestic abuse in Malaysia. This chapter focuses on how I experienced secondary trauma due to prolonged empathetic engagement with violence stories, and how this sheds light on the ways in which researchers move between positions while processing research data. It further discusses the importance of monitoring the symptoms that are understood to indicate secondary trauma, for intervention purposes. It offers a detailed personal insight into the ways in which secondary trauma can be recognised, and concludes with suggestions for coping.

Keywords Domestic violence · Secondary trauma · Symptoms · Empathy

10.1 Introduction: Understanding Secondary Trauma

The experiences of early career researchers have been the subject of discussion in numerous reflexive accounts that examine secondary trauma in different traumatic settings. Among them are researchers' experiences of undertaking research in post-tsunami (e.g. Calgaro 2015) and post-war (e.g. Drozdzewski and Dominey-Howes 2015) scenarios, and those following gender-based violence (e.g. Pio and Singh 2016; Bell 2003). These scholarly works place emphasis on how early career researchers are often not trained to anticipate, recognise and respond to trauma while working in trauma landscapes.

Previous scholarly works have theorised that researchers who listen to the experiences of traumatised individuals suffer from 'vicarious trauma', where they become traumatised by the 'process through which the inner experience of those

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empathically engaged with clients' trauma material, is negatively altered' (Pearlman and Saakvitne 1995: 31). In vicarious trauma, the symptoms experienced by researchers are deemed as strikingly similar to their traumatised clients (Blair and Ramones 1996; Robinson and Markowitz 2019). The traumatised person's story, affect, or behaviour affects the researchers so powerfully that they take on this same trauma (Coddington 2016).

However, trauma experienced by the researchers is different because they are not directly exposed to the atrocities or disasters studied by them (Ibid.). In fact, trauma affects everyone differently. What contributes to one unique individual experience of trauma may not affect someone else in the same way. The impact of trauma can be subtle, insidious, or outright destructive. How traumatic data affects an individual depends on many factors, including the characteristics of the individual, developmental processes, the interpretation of the trauma, and sociocultural factors (Substance Abuse and Mental Health Services Administration 2014). Therefore, difficulties in anticipating, recognising and responding to trauma is not limited to early career researchers. Many researchers who have long been working in the field of trauma have argued that the complexity of the trauma itself makes it difficult for the researcher to deal with it (see Vachon et al. 2016).

While acknowledging the complexity of trauma, research literature is clear regarding empathy as one of the main causes of trauma. Empathy is the processes of sympathetic engagement, through which one person comes to feel and know what the traumatised people are feeling (Gerdes 2011). Figley (2002) contends in his studies of healthcare workers that researchers who have a great capacity to feel and express empathy tend to be more vulnerable to secondary trauma. For Bondi (2003: 73):

> It is a key task of the researcher to make empathy available. Empathy is a form of psychic space in which movement between positions is possible. This space is one in which interviewees are able to express themselves relatively freely, and in which they may move beyond familiar and well-rehearsed accounts into spontaneous self-exploration.

This chapter is about the reflection of an early career researcher who ensures that empathy exists, as well as vulnerability to the complexity of secondary trauma. Focusing on my own research experiences, I use the term secondary trauma to refer to my indirect exposure to the atrocities of domestic violence, and to reflect on the experience of processing the research data with empathy and the process of change in my psychological and physical well-being. This chapter does not aim to review the concept of trauma, but rather explores the transformations that may be experienced by researchers who suffer trauma.

10.2 Research Context

This chapter elucidates my experiences of encountering survivors of violence during my PhD research. My qualitative research aimed to explore the spatially and culturally specific experience of domestic violence by focusing on female survivors

from different ethnic and cultural backgrounds in Malaysia. It employed participatory methods, which utilised fragmented storytelling, in-depth interviews, photovoice, documentary analysis and participant observation as the main methods of data collection.

The fieldwork took place over 3 months from 1st July to 9th October 2015 while I was an intern at a safe and confidential shelter located in a large urban area in Malaysia. The shelter is managed by the Women's Aid Organization (WAO), a Malaysian non-governmental organisation that has been helping abused women since 1982. My sample included 10 married women who were residents of the WAO refuge[1] in 2015. My participants were of Malaysian nationality and their ethnic groups included four Malays and six Indians. They were aged between 21 and 41 at the time of the research and were from various socio-economic backgrounds. The majority of the Malay participants in this study were Muslim. Of the Indian participants, one was Christian, and the rest were Hindu.

All participants contributed to the data every day over periods ranging from a week to 3 months, throughout the fieldwork (Sahdan 2019). The fragmented storytelling emerged spontaneously from the survivors whenever they wanted to share anything with me during the 3-month period of fieldwork. Each fragmented storytelling session was recorded with their consent, as were the interviews. This might have involved one or several participants at a time. Photovoice provided another route for the women to speak about domestic violence. Participants took photographs of things that they felt illustrated their experiences, and wrote a reflection sheet for each photograph. All the data such as transcripts, photovoice reflections and my research diary — in which I had logged details of the informal aspects of the research including encounters and observations — were analysed using a Computer Assisted Qualitative Data Analysis Software.

10.3 Positionality and Empathy

As a woman and a mother, I was an insider to all participants. As a Malay-Muslim, I was an outsider to the Indian participants, especially in terms of culture and religion. As a wife to a non-abusive husband, I had different experiences from all of the women. However, some of the Islamic values that have been instigated in me since childhood, such as avoiding pride (*riya'*), have shaped my character, and this has helped me to connect with and understand my interviewees. Pride is an egoistic trait that includes characteristics such as being conceited, looking down upon and underestimating others, and being censorious or fault-finding. Avoiding pride is important because some survivors feel inferior when they are communicating with other survivors, who claim that their case is 'not that bad', and their feelings of inferiority

[1] Their pseudonyms are Harini, Ashna, Shalini, Chumy, Rekha, Usha, Mariam, Fika, Faizah and Fazlin.

can be further heightened when they are communicating with other women who have never experienced domestic violence.

Furthermore, avoiding pride also helps me to build empathy. The researcher's empathy is a key requirement for gaining trust from survivors. In any relationship that exists after separation from a perpetrator, in every encounter, trust becomes the ultimate question (Herman 1997). For my research participants, there are limited numbers of roles that reflect their life in domestic violence: one can only be a perpetrator, ally, passive witness, or sometimes rescuer (see Ibid.).

However, many of the survivors admitted that they started sharing stories once they trusted me, because I showed them care and empathy. Chumy recounted:

> Because you care, you can accept what we tell you, you can understand, even you're not in our shoes, you can feel us. But for some people, they use that for their own advantage. So, they just smile at you, but behind, they'll tell others about us and our issues. There are people like this, we tell them stories, but they cause us problems. In fact, we want to vent out our feelings, but they abuse that.

Empathy is about 'how a researcher can feel their experience'. That feeling excludes any differences such as religion, skin colour and class. Empathy allowed these women the freedom to say whatever they wanted to share through storytelling, photovoice and other methods of my research. Although I was an outsider to Indian women, they were free to talk about their religious and cultural practices without fear of being marginalised. Empathy, therefore, enabled me to obtain detailed religious and cultural information. According to the women, I am a researcher, so they trusted that I had no bad intentions when I wanted to listen to their story, they just needed a listener who could understand them. They also once told me that they just wanted their story to be heard through my research so that other women would not suffer the same fate. Other people may have been seen to manipulate their stories and use them to look down on them, but I was trusted. They also realised that while I was just there for a while, I would, in fact, continue to think of them.

10.4 Unknown Signs of Trauma

After the field study ended, I started processing the qualitative data, which required me to remain immersed in the data over lengthy periods, through the iterative processes of transcription, analysis and writing. I had not anticipated the emotional complexity and traumatic effect of working with the research data during transcription. It can be distressing for the researcher, and their voices tend to elicit more emotional reaction through repeated listening (Shopes 2013; Kiyimba and O'Reilly 2015), which can have a lasting impact (Gregory et al. 1997). The voices of the women, the depictions of what happened, the words uttered by the abusers, the sexual abuse, physical torture (including the weapons used), and the abusive treatment of their children continued to 'haunt' my mind. Their storytelling left very clear mental images, which were partly reinforced by the visual data in the

photovoice reflections, and then reinforced yet again throughout the process of data analysis and thesis writing.

During the transcription process, I experienced anxiety or severe fear, guilt for leaving them in a situation still in need of help, sadness, helplessness, feelings of being overwhelmed and vertigo-like headaches. According to Substance Abuse and Mental Health Services Administration (2014), most of these symptoms occur at the initial stage of dealing with traumatic materials known as 'immediate response of trauma'. The vertigo-like headaches happened so often that when I was walking, I sometimes had to cling to the nearest object to avoid falling. When I closed my eyes, I felt like I was spinning or falling from a very high place. I assumed at the time it was because I was not getting good-quality sleep.

Each of the immediate symptoms can be attributed to other factors. This made it difficult for me every time I tried to access treatment. For instance, when I went to see a doctor about vertigo-like headaches, I was told to rest because the condition could be caused by stressful work. How the symptoms started did not arise as a question during the diagnosis of the disease. Often, I would temporarily abandon the process of immersing myself in the data in order to reduce the severity of this kind of headache.

10.5 Dreaming of Being a 'Saviour'

During the data analysis process, reading the stories repeatedly, as well as completing the fragmented stories by connecting them all together with certain themes, made me feel depressed. Over time, my data unravelled how helpless and powerless the abused women felt. At first, I recognised the desire of the abused women, who are mostly mothers, to have a happy life once they were out of the shelter. Part of their wish was to ensure their children received a good education and grew up as decent human beings, unlike their fathers. However, these desires are related to their need for greater support and proper intervention.

In the case of my research participant Shalini, for instance, she had a dream of providing a decent education and showering her children with affection, so that they could live free from the culture of violence in her husband's family. However, she was not able to act on this. The only option available to her was to surrender her children to their father because of economic constraints, threats against her life by the perpetrator, and the lack of efficient support once she left the refuge. This example from her story brought me emotional distress — hearing the heartfelt dreams of women and knowing the less-than-happy outcomes in the future was at times too hard to bear.

Feelings of empathy really struck me because I could do nothing to help alleviate the women's situations, even though I had witnessed how totally helpless and powerless they were from their stories. Evidence from my research suggests that survivors coming out of the shelter very often return to their perpetrators and live in family houses which are known to the perpetrators. They have to hand over their

children to their perpetrators because they are unable to take care of the children on their own, or have to be separated from their children because they lose child custody or because their children are simply taken away by the perpetrators. Others live alone in trauma and fear after leaving the shelter. A safe space that provides the conditions of a stable recovery is very hard to obtain. Herman (1997) suggests that one person cannot do the task of establishing a safe environment; instead, it requires societal support. Realising the fact that coming out of the shelter did not guarantee their safety was extremely painful. At this point, I was not just a trusted researcher with responsibility for making their stories heard. I felt as if I was a witness who was responsible for saving them. As a witness to women who just wanted to live normal lives, I was gradually questioning myself more and more. Why was I doing nothing to help when they were totally helpless and powerless?

At this point, I was totally immersed in the research data, and my sleep was disrupted by nightmares. In most of the nightmares, I played the role of saviour to all the participants. I can still clearly recall the scenes in which the survivors stood behind me as if to seek refuge from evil forces. The dreams seemed to imply that I was responsible for fixing their terrifying situations. Most dreams were about ghosts, the evil forces, and the process of escaping from them. According to Freud (2001), dreams are the link between waking and sleeping conditions. My dreams were full of abstract subconscious messages, such as the desire to save abused women and their children, the fear of imminent danger, and the intense desire to do so in a state of awakening (Ibid.).

Nightmares disturbed my daily routine because they happened too often. They caused me sleeplessness, extreme tiredness and sometimes fear of going to bed. As I remember, I began to experience nightmares during the data analysis process, and they happened almost every day while I was writing empirical chapters. However, I did not consider them as a symptom of trauma, because dreams are often said to be ‘a sleeping toy’ as corroborated by Yuminah (2018).

10.6 From ‘Saviour’ to Traumatic Researcher

After about 6 months of writing empirical chapters, my trauma symptoms became unbearable. To make matters worse, the writing process coincided with difficult phases such as becoming pregnant, giving birth to my third child and returning to Malaysia after the period of PhD funding had ended. Together with the traumatic effects, feelings of empathy for survivors and qualitative research demands, my productivity slowed down. It then gave rise to a sense of loss of purpose, hopelessness, and cynicism. As far as I knew, I was in a very troubled situation, but I did not know what the problem was. It was difficult to explain what I was going through. Vachon et al. (2016) illustrate this condition as living with an ‘invisible wound’ in a state of relative chaos that may be left unnoticed.

To make matters worse, my relationship with my husband was also affected. The behaviour and relationship signs such as difficulty in setting boundaries and

separating work from personal life can increase conflict in relationships (Substance Abuse and Mental Health Services Administration 2014). According to Mathieu (2012), this is when my fundamental beliefs about the world were altered, and possibly damaged by being repeatedly exposed to traumatic materials. My life at that time was an unsettling mixture of immersion in stories of violence and my loving and caring role as a wife and mother. I thought about this disturbing data while drinking coffee with friends, serving my children, and having breakfast with my husband. I got so involved with the participants' experiences, that I related them to my own life, especially to my relationship with my husband. My mind was always trying to make sense of participants who were tortured for many years, but why could no one help them? My empathy caused me sadness, tears and stress, and I found no effective intervention. Over time, I saw the negative side of husbands in general, and wondered how a husband could treat his wife so cruelly many times, every day, for so long? How could a husband be so cruel? In my mind at that time, any husband could be a cruel person. I was convinced that my husband could also be abusive at some point, even though he has never abused me over the course of our marriage. Small irrelevant matters started to bother me, consuming much of my energy and time and significantly affecting my marital relationship. Sometimes I became angry towards my husband unnecessarily.

One day I noticed a flashback symptom (maybe before this particular incident, similar moments had happened, but I was not aware). One morning after I finished preparing coffee that was unusually weak, my husband jokingly remarked 'that's it, you just did this without caring'. My husband is a nice man and never uses harsh words, but I interpreted this as a form of aggression because those words were precisely the same as those my research participant (Mariam) had reported that such statements were used by her perpetrator. In my mind, I thought my husband would beat me after drinking the coffee (just like what had happened to Mariam). It was hard to explain my fears at that time. Perhaps they are best captured in this account by another secondary trauma sufferer, namely Kimi (in Substance Abuse and Mental Health Services Administration 2014: 65), who describes her flashback experience by stating, 'the best way I can describe how I experience life is by comparing it to watching a scary, suspenseful movie — anxiously waiting for something to happen, palms sweating, heart pounding, on the edge of your chair'.

This was one of the incidents that made me wonder how Mariam's experience appeared to have been replicated in my daily life. How can I feel so sure that abuse will happen to me too, and so afraid that my husband will abuse me? How did these feelings arise? This question helped me realise that the mysterious problems I was going through were related to my prolonged studies and immersion in testimonies by women who are always sensitive to danger and continuously monitoring potential warning signs of violence and abusive behaviour.

In November 2016 I shared my anxieties with my supervisors. After listening carefully, they suggested that I was experiencing flashbacks and we discussed the idea of secondary trauma experienced by researchers, as discussed by Coddington (2016). In her account, she asserts that 'trauma is characterised by repeated flashing back to a traumatic episode at unexpected times and places' (Ibid.: 1). It became

clear that my flashbacks between October 2015–November 2016 were symptoms of secondary trauma, and I sought help.

10.7 Coping with Secondary Trauma

10.7.1 Recognising Secondary Trauma Symptoms and Its Triggers

'Trauma and recovery are messy, non-linear and subject to different retellings' (Bondi 2013; Tamas 2009, 2011, cited in Pain 2014: 541). To build resilience to trauma, the most important factor for an early career researcher is to recognise secondary trauma symptoms. Therefore, researchers into trauma need to be aware of how the symptoms of trauma are closely associated with stories of violence.

In my case, frequently I experienced flashbacks triggered by repeated contact with my research data and my participants' stories. My flashbacks were symptoms that arose after my other traumatic symptoms had reached a very severe stage (such as regularly experiencing nightmares). I now cope better with secondary trauma by recognising its triggers. For me, the trigger is when I see, hear or inhale anything that reminds me of abused women and violence stories, when reading the transcripts in my thesis and looking at photovoice pictures.

10.7.2 Support from Supervisors

Supervisory meetings helped me to build resilience to secondary trauma and to finish writing up my research findings. My supervisors always provided support and understanding about how heavy the emotions I had to bear in the study of domestic violence were. Their magic words, that 'you can do it' and 'your research is important for the abused women' always convinced me that the study itself had a valuable impact. Supervisory meetings allowed me to deal with the guilt that often arose because I did not help the women to flee from domestic violence. My supervisors always reminded me that the findings of my study can be translated into various forms of impacts such as publications, policy papers and activism in providing long-term intervention to the women. The realisation that my thesis can help abused women has supported me to deal with the trauma that I perceived as a 'bad' feeling from being a passive witness.

10.7.3 Building Connections

Knowing that I had symptoms of secondary trauma, I was also better able to communicate with my husband about the trigger factors that could send me into a state of stress, anxiety, or fear, all of which related to my research participants' accounts of life in an abusive relationship. My husband played an important role in reassuring me that not all husbands are cruel and was fortunately supportive.

I also sought counselling through friendship networks from a Muslim woman who runs a non-governmental organisation that provides support to minority ethnic female survivors in the UK. She was familiar with experiences of trauma among humanitarian workers and was able to listen to my problem. I was free to confide in her and share my feelings without shame and guilt. She also taught me how to calm down through prayer. My confidence that God would hear my prayers allowed me to feel at ease. In every prayer, I ask that the abused women be protected by God and given safety.

It is important to focus on finding trustworthy and compassionate individuals who validate one's feelings, which supports the building of skills of resilience. From this, and other forms of support, I am now better able to understand about what to expect, and when to pause during data processing. This is particularly important because secondary trauma can result in early career researchers being unable to pursue their goal of positive impact through their research.

10.8 Conclusion

This chapter has discussed how hidden and unspeakable trauma can be, due to the nature of the trauma itself, which is difficult to identify. This chapter began with the positionality and empathy of the researcher throughout the study from fieldwork to data analysis and thesis writing. This chapter has showed how prolonged empathic contact with domestic violence stories can put the researcher into contradictory and shifting positions as a trusted ally, a saviour or a passive witness, which can induce secondary trauma. It also argues that secondary trauma is an unintended health consequence and takes a long time to detect because it affects everyone differently. Furthermore, secondary trauma is hidden because early career researchers who conduct research with traumatised people are often not well-trained in anticipating, identifying and responding to it. In this reflexive account, secondary trauma symptoms are often misunderstood as overworking or stress. Therefore, this chapter concludes that early career researchers should build resilience to secondary trauma by being alert to and recognising its symptoms, particularly flashbacks triggered by repeated contact with traumatic materials. It is crucial to acknowledge triggers and to seek support from institutions, supervisors, counsellors, family and friends in coping with secondary trauma.

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Chapter 11
From Shining a Light to Making an Argument: A Thesis Writing Journey

Lucy Smout Szablewska

Abstract Academic writing can be a challenge for doctoral researchers, who know they need to collect, analyse and write about data, but sometimes struggle to acquire the rhetorical moves and scholarly language needed to develop and express an academic argument. Grasping the unspoken social scientific conventions underpinning the writing up of social research is a further skill. This can particularly be the case for mature and international researchers in the UK, who may not have undertaken social science courses as undergraduates and been exposed to scholarship in the UK, or, like the author, may have worked outside academia. This chapter draws on my early career background in teaching and communications, and midlife experience of writing a thesis part-time, to examine some of the challenges. I outline my journey from wanting to 'shine a light' on the importance of unpaid care labour to our understanding of migration, to getting started, and gradually learning to make an argument. I then make a preliminary case for thinking through ways of demystifying 'tacit' social research principles, and equipping researchers with a social scientific thinking 'toolkit'.

Keywords Academic writing problems · Scholarly argument · Demystifying unspoken rules · Tacit social research principles · Mental toolkit

11.1 Introduction

> 'Writing is an exhaustive activity, very difficult, filled with anxiety.
> I'm always afraid of messing up;
> naturally, I mess up, I fail all the time.'
> (Foucault 2016, cited in Thomson 2019)

Turning findings from fieldwork into succinct academic writing can be a challenge for all scholars, even Foucault, as shown above, and especially for

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postgraduate researchers. This is not least due to intense feelings of discomfort, inadequacy, disillusion, enthusiasm and personal involvement, and the challenge of dealing with ethical dilemmas and personal positioning, all expressed in earlier chapters in this book.

In particular, some researchers struggle to grasp the rhetorical moves and scholarly language needed to develop and express a clear argument, and, further, to fathom the unspoken conventions and 'ways that things are done' in social research that are not always made explicit. This was my particular predicament as I set out to explore the linked lives of Polish migrant worker-carers and their children and parents across borders and over the life course. I was driven by the desire to show that Polish migrants to the UK after Poland joined the European Union in 2004 (which Britain then belonged to) were not a threat, as depicted in anti-migrant discourses. I wanted to show that they had rich and complex personal lives outside work that shaped their decisions, and that they contributed to the British economy and society. However, researchers have to marshal their stories into a rigorous argument buttressed by evidence, as my supervisors said when we first met, and to whom I am eternally grateful for taking me on, and to sticking by me during my long, slow, taxing and ultimately enriching 7-year journey.

There is a growing body of literature on ways of recognising academic writing as a literary genre of its own and tackling the challenges. Some have explicit titles, such as 'How to write a thesis' (Eco 2015) and 'How to write a lot' (Silva 2007). However, the literature on the fundamental issue of what Thomson (2015) calls 'text work/identity work', which is the process of crafting a professional identity through crafting written work, is smaller. As Thomson puts it, 'when we write we not only produce text, we also produce ourselves as scholars'. There appears to be even less literature on the unspoken conventions that govern 'the way things are done', partly because established researchers have absorbed and internalised them to the extent that they become second nature, which can be difficult to explain to novices.

This chapter suggests that a contribution could be made to this growing and useful body of work by thinking further about how doctoral researchers themselves make sense of their writing journeys, and which strategies and resources they use to demystify tacit rules and develop their own 'mental toolkits'. I start by laying out the broad context of doctoral social science writing problems. I then reflect on my own personal journey from wanting to 'shine a light' on a topic to getting started, and gradually learning to make an argument anchored by evidence, thread the argument through the thesis and acquire scholarly language. Lastly, I reflect on some initial ideas about unspoken social scientific principles, a work in progress. The section on the bumps in the road on my journey is written in a narrative style in order to ground the chapter in everyday lived experience that readers can relate to. It evokes my particular personal challenge in making the transition from an undergraduate degree in social anthropology and early career in English language teaching and communications, to midlife career development through a social science doctorate. The former needed clear speaking and writing. The latter needed clear thinking.

The chapter is not advocating that other researchers draw on all the resources listed, or develop the same strategy in order to complete their PhD journey. Nor is it arguing that it is possible to boil the PhD down to a simple set of linear steps, or that I have all the answers. The point is to inspire fellow researchers to reflect on and identify what they might need and to develop their own unique and flexible social science writing toolboxes.

Further, it is written out of a commitment to making space for researchers who bring different knowledge and voices to doctoral study, particularly mature and international researchers from different academic systems to those in the western world. They may have missed out on opportunities to pick up patterns of scholarship in the UK through, for instance, not having undertaken contemporary undergraduate social science courses in the UK. They bring valuable insights, but writing in a second language means they sometimes struggle to capture and express their novel ideas in grammatically perfect English, or argument structures that meet disciplinary standards.

11.2 Getting Started: Theories and Resources

It is well documented that every researcher starts from uniquely individual positions, in a huge range of different fields, all stretching in numerous directions into multiple domains across borders and generations (Crang and Cook 2007). Yet, despite all these different starting points and unique research projects, research into postgraduate learning highlights perennial challenges, as documented by professional associations such as the UK Council for Graduate Education (Wilson 2020). For instance, making sense of and writing up research in a scholarly fashion seems to pose particular challenges for researchers used to 'doing' in earlier careers. Kiley and Wisker (2009: 437) explain the problem in the following quote from one of the supervisors interviewed for their research on 'threshold concepts', a term for new ways of understanding.

> For many of them [who have come from business] to make a jump from their very practical applied business ways of thinking about the world to thinking in academic terms, theoretically, conceptually is quite a leap. So you can ask them to read a book on research theory method … something like that, but here you might be saying to them you've got a topic, great project but you have to have a way of theoretically and conceptually framing this. Now that's a leap for them because all they want to do is concentrate on the doing of it because that's how their head works.

For others, moving from writing as a private to a public activity can be emotionally and intellectually demanding. This is particularly so when exposed to rigorous scrutiny and critique after putting oneself and one's stance on a topic 'out there', so that it can be debated and contested, as part of a conversation seeking to create new knowledge. As Basbøll (2019) explains: 'As a scholar you are writing for other scholars whose thoughts you have access to through the literature. In addition, you are writing for *them* because they are qualified to help you think more carefully

about things. To put it bluntly, they are qualified to tell you when you are wrong.' Being told that the work one has tortuously laboured over has fallacies can be painful, even if peer review stimulates better thinking and writing. For some trainee early career researchers the whole process can provoke intense stress and anxiety, as noted by Wilson (2020). 'We began to think about the links between writing and well-being when we noticed an increase in applications for suspension of studies or mitigating circumstances applications around the time of the annual progress review deadlines' (Ibid.)

Traditionally, academic supervisors are the first port of call for guidance. However, in modern marketised universities, academics frequently juggle exhausting workloads (Collini 2013) and doctoral study is enmeshed in complex power structures. This means that apprentice researchers find themselves drawing not only on their official thesis mentors, but also on in-house training courses, structured writing sessions and intensive writing programmes nick-named 'Thesis Boot Camps' (Freestone 2020). Equally importantly, and often under the radar, postgraduate researchers support each other formally through peer support writing groups, and informally during innumerable conversations. They also use their research skills to dig out a range of supplementary guidance online and in the library. A whole host of resources have both flowered and been commodified over the past two decades in response to the rise in the number of doctoral researchers worldwide, and the opportunities opened up by the internet.

Many accessible online materials such as blogs and chargeable coaching, are underpinned by scholarly research. This includes Becker (1986) on social science writing, Bolker (1998) on writing for 15 min a day, Gardiner and Kearns (2010) on becoming a prolific writer, Murray (2011) on developing helpful habits, Goodson (2016) on warm up writing practice, Dunleavy (2003) on structuring a thesis, and Mewburn et al. (2018) on fixing troubles. There is also some thoughtful debunking of rigid rules such as writing for 15 min a day (Sword 2016), plus helpful debates about good and bad social science writing (Billig 2013; Dunleavy 2014; Jones 2014) and the ways in which procrastination is an opportunity to think and untangle. There is also exquisite humour, such as 'Search Procedures for Geographers, By Geographers and Using Geographers' (McNoleg 2004).

Whatever the individual issue and broader institutional context, PhD researchers have to 'write down what they know, in coherent prose paragraphs, during well-defined moments, for the purpose of discussing it with other knowledgeable people' (Basbøll 2019). Alternatively, more brutally, they have to deliver a written output. Publish or perish, write or withdraw.

11.3 My Bumpy Start: Messy Lived Experience

Classically I struggled with getting started on writing. The 15-min-a-day (Bolker 1998) or 25-min bursts of free writing did not work, as I did not know how or what to write, and the minutes passed without anything appearing on a blank sheet. I

brainstormed, mind-mapped and drew endless spider diagrams in order to devise a plan. I devised a chart setting out the number of words written and the number of words remaining and wrote about how I felt. It felt like wading through treacle. Professionally in communications I was used to writing short, structured, tightly worded pieces, and to condensing what other people said, without inserting my own views, not scholarship which required me to have read, made senses of and have a clear opinion on other scholars' work. I wanted to shine a light, expose injustice, and explain my topic in an interesting, colourful and engaging way accessible to non-specialists as well as academics. I liked storytelling, personal narratives, listening to life stories and views about everyday life, not dry scholarly materials. The words 'I argue x' sounded almost confrontational. The work bled out.

This struggle tied into other observations about being an academic and disciplinary outsider (Thomson 2018). When I attended presentations and asked questions, I kept them short and was puzzled by how long other questioners would talk for when asking their question. Sometimes I did not have a clue what some people were talking about (Kiley 2015), for instance in discussions around topics such as 'performativity'. I swung from admiring and feeling intimidated by thought-provoking brilliance to wondering if some participants really knew what they were talking about, or if they were just mimicking key phrases in an attempt to fit in. Even words such as 'gendered' and 'spatiality' troubled me. I had never heard or used them before. My imagined audience were well-informed lay people who could think spatially about how and why places are where they are on a map, and understood 'gender' and 'spatial inequalities', but not scholarly variations.

Nevertheless, I began to sprinkle words like 'flows', 'emerging', 'shifting', 'ruptured', 'lens' and 'unpacking' around my writing, but baulked at playing around with words by splitting them apart with brackets, hyphens and prefixes such as 're', or adding 's' to create, for instance, the term 'knowledges'. The 'geographies of x' with x meaning anything one wants to highlight, was a useful phrase to fall back on — but that was as far as I wanted to go. I could not go 'over to the dark side' of dense writing that a friend who had done a social science PhD joked about (the dark side being the place in Star Wars films where one learns about evil ways). When giving a compulsory first-year progression presentation and trying to avoid repeating the same words about my topic over and over again, I used the term 'transnational'. I was taken aback when a senior academic jumped on it midway through the presentation and snapped 'Why are you saying transnational now when you didn't mention it earlier?' My mind went blank. I tried not to look like a rabbit caught in headlights. After an embarrassing silence I picked up my presentation where I had left off. The question was left hanging in the air, unanswered, and there was no opportunity to seek a one-to-one chat at the end of the seminar as busy staff dispersed quickly. I thought to myself that, like any good writer, I had simply been trying to vary my vocabulary, and 'transnational' was surely a very geographical adjective that captured the idea of migrants and their homeland relationships.

Comfort came in the shape of pearls of wisdom from supervisors, doctoral peers, academic associates, friends, family, books and online resources, during training courses, long conversations and casual encounters. Memorable advice included the

words of a friend ‘if you don’t know what to write you can’t write’ and from a writing course ‘if your head is not straight you can’t write’ (Tolia-Kelly 2013). A story about an anthropology professor who switched to law school was reassuring. ‘She said that during her first six months she wrote so incoherently that she feared she was suffering from a degenerative brain disease. Of course she was not: she was going through a temporary aphasia that afflicts us when we try to write about matters we do not entirely understand for an audience we understand even less’ (Booth et al. 2003: 118). Similarly, a confession by a professor that a reviewer had commented that his article contained more holes than his grandfather’s vest was enlightening. It was so nice to hear that even professors could be laid low by unthinkingly brutal hyper-criticality that does little to help other scholars improve their thinking. I also agonised a lot about how to evoke participants’ individual voices and the warmth and humanity of our interactions when ‘packaging’ their quotes to conform to the ‘the unitary register of storytelling in social science’ (Katz 2013: 8).

The penny finally dropped that my problem was not so much about words and language as about thinking. It dawned while chatting to an engineering professor at a social event, who sighed and said: ‘There’s no point doing a thesis without an argument’. Others had been saying the same for months, but I had not processed it properly. Gradually I felt as if I was finally crossing the mythical ‘threshold’ into academia. “Crossing a conceptual threshold involves a transformed way of understanding, interpreting or viewing ‘something’. Without this new way of seeing, the learner cannot progress at the level required for more advanced study or research” (Kiley and Wisker 2009: 437). Bit by bit I grasped that I was taking part in a conversation (Graff and Birkenstein 2018) conducted in formal ways so as to allow differing views to be voiced and scrutinised (Bastow et al. 2014), not a journalistic question-and-answer session. I came to understand that the word ‘transnational’ that had troubled me in my first-year progression seminar was not just a word, but stood for a body of theory about migrants’ homeland relationships, a theory painstakingly developed over the years through collective endeavour. One had to use words with precision, and tread carefully with scholarly theories, as if walking through a minefield (Kamler and Thomson 2006). It dawned that trainee researchers were not so much mimicking established academics as using ideas they had only half formulated in their heads. They were trying to express themselves as ‘insiders’ in order to lay claim to a professional identity in a symbolically enclosed profession (Fullick 2015). I recognised that scholars’ concerns about the dumbing down of scholarship were as legitimate as my concerns about obfuscatory malaise (Reisz 2010) and exclusion (Nagar 2002). I even began to open my mind to Butler’s ideas about the social construction of gender through speech acts and non-verbal communication (Birkenstein 2010) and got as far (but no further) as writing in my thesis that ‘embodied’ personality and emotion shape the personal biographies that researchers bring to their work.

11.4 Getting the Argument Clear

Understanding that a clear argument was needed was only the first step. Getting my own thesis argument clear and rooted in evidence was easier said than done. I could not see the wood for the trees. All I could bring myself to say was 'here is an example of what I found' and 'it might mean this'. What I really wanted to look at was a step by step guide revealing how other researchers had moved from tentatively expressing their views and battling with the inner editor in their heads to making a bold argument. Reflecting now on my own step by step journey there were three milestones. These were: synthesising an argument out of theory, method and data; threading the argument through the thesis; and developing a scholarly tone. I reflect on them below in a preliminary effort to 'show and tell' other researchers.

11.4.1 Synthesising the Argument

Much has been written about synthesising theory, method and data into an argument (Kiley and Wisker 2009). Understanding that a thesis needs to be anchored in a conceptual framework rather than a 'laundry list' of theories (Thomson 2017) is one thing. Putting it into practice and relating abstract terms such as space, place, time, scale to everyday life stories from one's own research in a meaningful way is a big step up. I could quote, for instance, that local processes map onto maps of global change, as that sounded 'geographical'. I could also quote that structures of inequality intersected with everyday practices as that sounded 'social scientific'. But I did not know how to explain in my own words how the complex processes I came across in fieldwork actually 'mapped' onto or 'intersected' with an abstract phenomenon. I tried drawing a diagram composed of a small circle with the word 'individual' in, surrounded by a larger circle with the word 'Poland' in and then another larger circle with the word 'EU' in. I stuck it next to a map of Europe, but realised it was not exactly thesis-standard knowledge. I tried thinking about typologies, such as a frequently cited typology on Polish migration to the UK (Eade et al. 2006) which divided Polish migrants into storks (circular), hamsters (long stay), stayers and searchers, the latter being the largest group composed of people keeping their options open. This was easy to grasp, but over-simplistic and potentially offensive.

The Dictionary of Human Geography (Gregory et al. 2011) and undergraduate textbooks, such as Introducing Human Geographies (Cloke et al. 2005), proved marvellous resources, filled as they are with concise definitions of key terms in glossaries and cogent 'in a nutshell' explanations. Gradually I pieced together a jigsaw of interlocking ideas, building on vital keystones from supervisors. One set was about 'a progressive sense of place' shaped by routes, mobility and 'things from outside' as much as roots or fixity (Massey 2012). Another breakthrough emerged from Gibson-Graham's (2002) ideas about a diverse economic life beyond capitalism represented in an iceberg. The tip visible above the water represents wage

labour, market exchange of commodities and capitalist enterprise. The larger submerged area represents diverse alternative economies, including household, voluntary, gift, informal and social economies. This got me thinking about how the term 'economic inactivity' — used in employment statistics — disregarded unpaid care work. That in turn led me to ideas about social reproduction, 'the fleshy, messy and indeterminate stuff of everyday life' (Katz 2001: 711) that props up the economy and underpins society; and also ideas about the life course and linked lives (Elder 1998). I had long discussions with one of my PhD friends about the ways in which life stories were not irrelevant, but a crucial part of research (Dutta 2016, 2019). Theory became exciting when it shed light on everyday real-world injustice and the ways in which broad structural forces shape lives (Staeheli and Mitchell 2005: 131).

A conceptual framework gradually took shape. Drawing on the ideas above, I started to write that my PhD was about migration and social reproduction. Specifically, it explored the 'linked lives' between multigenerational household members who stayed put, moved and circulated across borders within Europe at different times over the life course. Their caring activities were unpaid labour, not 'economic inactivity'. Their labour was unequally distributed, with women bearing a heavier burden. Eldercare was a major consideration, given population ageing, low fertility and low levels of social protection in Poland.

The next step in synthesising theory, method and data (it was not a linear process, but for clarity's sake is laid out step by step here) was pulling out key themes to be shaped into findings. I went through the transcripts and mind mapped ideas. For instance, a theme associated with 'linked lives' was 'intergenerational'. Sub-themes were 'closeness' and 'distance'. I cut up a pile of small rectangular pieces of paper, wrote key words on them, shuffled them around (Crang and Cook 2007; Rumble 2011) and gradually worked out what was important. One particular principle shared by a PhD peer kept coming back to me — not everything is the same for everybody all of the time in all of the places. I repeated it to myself endlessly, a mantra. It prompted me to think that my interviews showed that not everybody cared all of the time in all of the places, and that intergenerational-linked lives, which were constantly evolving, featured both closeness and distancing, which were both positive and problematic. Women did more care work both willingly and unwillingly and the older generation both cared and were cared for, like all of us. This fed into my emerging final argument that social reproduction should be a major narrative, rather than a hidden sub-text, in discussions about migration.

11.4.2 Threading the Argument Through the Chapter

Having got thus far, the next step was to try to get the argument clear in my head in order to thread it through the thesis AND condense it into a short abstract. Furthermore, the 'so what' question had to be answered in my thesis, or as Booth et al. (2003) put it, the 'warrant' underpinning my argument. I kept asking myself — what am I trying to claim given that my research findings are complex

and contradictory? How can I do my participants justice, while not veering away from writing about 'difficult things' (Thomson 2016)?

More wracking of brain. I needed a template. Templates can be seen as formulaic in social science, with its avoidance of reductionist 'cause-effect' ontologies and epistemologies, and creation of space for innovative expositions of inductive arguments. However, Birkenstein and Graff (2008) make a compelling case for spelling out 'the moves that matter' through templates. Similarly, Sword (2016) recognises that some researchers are planners, who have to plan before writing, rather than plunging straight into free writing.

Adapting the news triangle used by journalists was one option for spelling out my big idea. In the news triangle, the 'what' is at the narrow top of the triangle, followed by 'where' and 'when', then 'why' and 'how' in the wide bottom of the triangle. Adapting public relations approaches was another option, for instance by filling in the template 'we are doing x to find out y and make better sense of z'. Learning from science writing was a third option, as it is clearly structured (Mensh and Kording 2017), as shown, for instance, in the guidelines for formulating an abstract that the journal *Nature* gives to contributors (Nature 2020).

Although these ideas were helpful, they did not do the trick. Ideas from doctoral peers (and tea and sympathy) proved more useful, such as the six sentences exercise. That involved writing six lines — introduction (area of study); the problem (that I tackle); what the literature says; how I tackle this problem; how I implement my solution; the result. I refined this into my own version — the broad issue in the research, the specific issue, the puzzle, what my study investigates x, what it argues x, how it addresses the gap/contributes to x, where it is theoretically positioned in x, what it is methodologically based on x. Other useful templates included 'tiny texts' or 'mini-me' versions of a bigger text (Thomson 2019) and sentence skeletons (Thomson 2014).

I wrote and re-wrote six sentences and 'tiny texts' repeatedly until I ran out of steam. I had got there. Population ageing and migration are creating complex new patterns of care and work across Europe. We should pay less attention to types of care and carers and more attention to the crucial role of governments in helping people care for each other. A final argument of sorts. It was good enough, a work in progress.

11.4.3 Acquiring a Scholarly Tone of Voice

The final challenge was writing up in a scholarly tone of voice and avoiding description, journalese, hyperbole, over-emoting, exclamation marks and so forth.

There are no colourful anecdotes about this final leg of the journey. It was an exhausting battle, or perhaps dialogue is a better word, between one's everyday conversational and visceral self, and one's more thoughtful and measured scholarly self. Perhaps it was even a dialogue between six selves wearing six different

thinking hats symbolising different directions of thought, as evoked by De Bono (2017) as part of his mission to help people develop the habit of constructive thinking.

The easy part was going through the text and cutting out every last exclamation mark (except for those in participants' quotes). Much more tortuous was the grinding labour of checking and re-checking page after page to see where I was overstating or over-simplifying the case, and paring down paragraph after paragraph. This labour also involved examining what others had written, scouring the Academic Phrasebank (2020) for phraseological 'nuts and bolts', and re-jigging sentences.

A 'good enough' scholarly voice came and then the great day when the thesis was finally finished and sent off to be turned into three neatly printed and bound copies ready for submission to the academic office. Except it was not quite finished. Katz's 'fleshy, messy and indeterminate stuff of everyday life' (2001: 711) intervened. Five minutes after I put on some music and got into the bath the phone rang. It was one of my children on their teacher's phone. They had lost their mobile phone by a river in the woods on a geography field trip. Instead of submitting the thesis we ended up driving to the back of beyond to look for the phone, without success. When we got home my husband idly cast his eye over one of the copies and said quietly: 'You've forgotten to put your name on the front cover.' Clearly some things are just not meant to happen on certain days. Happily, the phone was discovered on the school bus, and the corrected thesis was finally submitted a few days later, at a desk next to a colourful sculpture of a globe aptly named 'The Sphere of Redemption'. It formed a backdrop to photos with a few of the wonderful Durham doctoral researchers who had got wind of the date and time, and came along, full of smiles and laughter, viscerally sharing my success in the hope that it would be their turn soon, just as I had done with my predecessors.

11.5 A Work in Progress

As this chapter has demonstrated, acquiring the skill of making a well-argued case, bolstered by evidence, and written in a scholarly fashion, can be a gruelling iterative process. Demystifying 'tacit' social research principles and developing a social science 'toolkit' also takes time and effort.

One possible approach to acquiring such skills is to imagine a set of underlying principles which could be turned into deceptively simple questions when needed. These principles and questions might help provoke the deep thinking needed to seek out appropriate ideas and evidence, which could then be argued through in a thesis or article, and also boiled down to a 'big idea' in an abstract. Being able to carry around a set of key principles and questions in one's' head can also be helpful for work in and outside academia, where the ability to communicate to non-specialists in social research is vital. Inside academia there are moves towards interdisciplinary research and demonstrations of impact and relevance. Outside academia social researchers need to be able to explain the value of a social science lens in a

multitude of non-academic contexts. One key underlying principle could be the one I repeated to myself endlessly — not everything is the same for everyone all of the time in all of the places. It can easily be turned into a question, such as 'is this particular issue the same for everyone?'

Lists of principles do not need to be set in stone. The point is for researchers to identify and explain their own guiding principles and questions as part of the process of developing a confident voice in and outside academia. Good luck!

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Chapter 12
Open Inquiry: Fielding the Field

Zahra Hussain

Abstract This chapter explores what it means to work in the field. Its contours, textures, and often-unruly behaviour that holds the capacity to shape and mould the researchers' mode of engagement as well as affect the kinds of materials that can be produced whilst doing the fieldwork. It argues that within an open inquiry framework, the field cannot remain within spatial and temporal bounds but rather it slips away. The field also unsettles one's positionality as a mere researcher and demands and obligates certain modes of conduct which require the researcher to assume more than one position, stay with the mess and somehow never really be able to leave the field behind. This chapter begins with discussing how a field may be understood and what it means to conduct an open inquiry in the field. It goes on to mention three stories from fieldwork conducted in a post-disaster landscape in Northern Pakistan. These stories pronounce my attempt at fielding the field; assuming positions and adapting methods in response to the field in order to be able to produce more engaged accounts from the field.

Keywords Open inquiry · Temporality · Spatiality · The field · Positionalities

12.1 Introduction

Defining the boundaries of the field is not an easy task. No matter how much one tries, the field always seems to slip away, seep in, expand, and transgress the spatial and temporal bounds in which we attempt to contain it. A researcher too often encounters the same dilemma; one cannot just be a researcher in the field, being in the field requires establishing certain relations and these often do not fit neatly into the category of a researcher. I faced such a dilemma when carrying out fieldwork in a post-disaster landscape in order to understand how local actors and communities re-built their lives and landscapes in the aftermath. Conducting the fieldwork with multiple communities in different geographic locations in five affected villages and

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two shelter sites became a journey in learning how to embrace the field and its complex temporality and spatialities, and come to terms with the messiness of my own presence and positionality in the field. In this chapter, I discuss my experience of conducting the fieldwork in Northern Pakistan where a landslide disaster that occurred in 2010 displaced 400 households and partially inundated three villages. In particular, I discuss modes of negotiations in the field as it never seemed to stay passive in the background. The field somehow managed to disrupt, unsettle and challenge my forms of engagement in the field as well as the methods I used for generating materials for my research.

12.2 The Field

It is important to understand what the field is and what it consists of; is it the physical location that we visit or also the spatial imaginary in which we place it? Can it really stay within the bounds of temporal frames, which could make our analysis somewhat easier and less complex? Massey (2003: 84) discusses these concerns and explains that establishing a relation to the field has consequences for how we frame the material generated from the field, what kind of power relations are involved or what position is taken by the researcher. She explains that often in the process of research, the field is thought to be a 'bounded space separated from the academy' (Ibid.: 84), on the other hand she quotes Katz (1994: 72) who writes about the difficulty of separating the field from the other ongoings of life, arguing that she 'is always, everywhere, in "the field"' (cited in Massey 2003: 84). Massey (2003) explains that imagination of the field is significant in articulation of the relationship between the anthropologist and the people being studied. We do not just encounter the field but we construct it imaginatively. The field is encountered and constructed 'open and porous, and connected by a chain of practices' (Ibid: 84). The imagination of field surpasses any temporal bounds; it does not begin or end with our physical being in the field. The field leaves impressions and triggers emotions, which come to bear upon how we negotiate our engagements in the field and how we reflect upon, remember and reconstruct the field in our research analysis and writing.

For the purpose of this research, the field must not only be understood as sites visited during the fieldwork or locations on the map, but also the overall experience of imagining, remembering, being in and reflecting upon the field. An important idea to stay with is using an open inquiry framework, which allows us to approach and understand the field as a dynamic entity rather than a static background for activities and encounters to take place. Hence, the field is not a passive entity that is waiting to be read and discerned by the researcher; rather, it has the power to affect what materials or data that we are able to generate from the field (Whatmore 2003). This dynamic aspect and the 'everywhere-ness' of the field can also be approached from Ingold's (2002: 229–230) concept of 'wayfaring' as knowledge production through movement along paths and trails, which he explains as a 'way of knowing ... a path of movement through the world'. The rest of this chapter discusses how the

field features in an open inquiry framework and how that shapes the researcher's encounters. It argues that the field plays a crucial role with reference to the kinds of materials that are possible to generate from the field elaborating the deeply entangled nature of research and engagement in the field.

12.3 An Open Inquiry and the Field

My fieldwork entailed engaging with communities affected by a landslide disaster in Northern Pakistan. One of the objectives of the research was exploring the reconstruction of a post-disaster landscape. This was a daunting task as post-disaster landscapes are imbued with ambiguity where I could not know for certain, at what levels, and which actors were involved in the re-organisations of the disaster-stricken landscape. This required deeper engagement to explore the 'situatedness' of the field; to take things as they are, not as they may seem to appear, but find links as to how they have come to be, and what they tend to become through the relations and processes that constitute them. The task was to try and capture the linkages between actors, things, realms and formations from as many angles as possible in order to create a some-what sense of the 'multiplicity' of the field (Simone and Pieterse 2018) to conceptualise how resilience, displacement and rehabilitation emerge and play out in the post-disaster landscape. To engage with how communities affected by a disaster continued to respond to the disaster event required loosening up the established norms around traditional ethnographic methods (such as surveys or interviews generally used with communities affected by disasters) and opening up to the field and local contextualities. It required an approach that could sense the different ways in which the disaster event was dealt with by the different actors and stakeholders in practice, policy and everyday life.

An open inquiry desires openness yet requires some fielding to achieve some sense of coherence. Whilst following calls for an open inquiry and engagement with the field, I also realised that it was practically impossible to take account of all actors' entities within the post-disaster landscape. An open inquiry demands a particular kind of openness, to allow the situation you confront to 'move' you and open you up to the possibilities that situation may present (Clark 2003). Openness means to be moved or affected by the field, and to open up to the possibilities of how things emerge, without aligning or reducing it to our framework of inquiry. An open inquiry calls for a practice of engagement that is susceptible to asking questions and embarking upon conversations that may not fall under its domain but seem relevant. Thus, an open inquiry is about opening up the territories of our research investigations, rather than closing them off through particular disciplinary frameworks and methodologies. This means, staying open to the idea that our methods and modes of engagement in the field may not always be welcomed or appropriate for the situations we confront. What helps proceed with such an approach is to delve deeper into how these situations come to be, who is involved, how are they linked, and so on. One way of doing this is by asking 'how' rather than 'why', which allowed

this research to explore ways in which engagement in the field could be re-arranged and retrofitted. Investigating the 'how' meant exploring processes of engagement, attachment, resentment, fragmentation and fragility that displaced communities encountered in the aftermath of the disaster. Here varying methods of research and engagement were required in different contexts and situations in order to be able to stay in the midst of things. Mitchell (2010: 51) eloquently describes the quality of openness as,

> not to be open at a point and closed off at another, it is to be open through and through, so much so that everything about oneself is destabilized, translated, emergent. […] Openness means existence in the midst of things.

Openness in research requires different modes of conduct. It means to remain open to the tools and practices of engagement with the entities and the field in order to begin recognisings entities, networks and processes. Alongside this, it requires attending to the environment, context and situation one confronts, which means the researcher might have to tread divergent paths on unknown terrain in search of the possible forms that things may assume. Here, the researcher must be reminded that the field is active and dynamic, and things must be seen in their entangled relations with surroundings as opposed to the controlled and bounded environment of the laboratory (Stengers 2011). Massey (2003: 75) echoes Stengers' approach that 'being there' in the field is about 'doing […] one's science in the field itself' in order to capture the continual movements of the world. She explains that this claim to knowledge production is radically different from the 'objectivity (supposedly) lent by distance' (Ibid.: 75). Stengers (2008: 44) suggests that upon entering the field, the researcher must question and deliberate on 'which kind of attention, concern and care are required'. The fieldwork is the researcher creating a particular understanding of the world, and it would rather be contextually situated and grounded — an engaged version of reality (Law 2004) that surfaces the relations that make up the worlds we are exploring.

For example, within the post-disaster landscape, issues of displacement were not only tied to physical displacement from one's home and land, but also to the different ways in which affected communities experienced displacement within everyday interactions, memories and relations with their landscape. This meant paying closer attention to the relational as well as temporal aspects of how contexts and situations were encountered and observed in the field. As I entered the field and began to engage with its contexts and dynamics in more depth, certain kinds of relations and negotiations were required as I proceeded to gather research materials. My presence and mode of engagement had to be negotiated as every encounter produced a particular set of materials and eventually became a lens through which a narrative could be constructed. For the purpose of this chapter, the field must not only be understood as these locations on the map but the overall experience of being in a place and conducting research. Various research methods such as participatory mapping, drawings and semi-structured focus groups were employed, adapted, and discarded during the period of the field research to create a condition of engagement that was favourable for conducting an open inquiry, subsequently allowing research

participants to lead the process of sharing their accounts of the disaster event. Whilst conducting the fieldwork, moments of hesitation, estrangement, confusion or realisation led to a continuous process of method adaption in response to what the field/context/situation required. These were also valuable for engaging with questions that were aimed at understanding how disaster event became present and lived on in the everyday lives of affected communities. Critical questions were explored. For example, how does a person want to, or not want to talk about, describe or demonstrate, attach importance to the event? In order to address these questions, research methods were adopted, transformed, adapted and fused in response to the field dynamics and context. This process mimicked what Law (2004: 143) calls the 'method assemblage', 'a continuous process of crafting and enacting necessary boundaries between presence, manifest absence and Otherness'. Law (2004) does not support the idea that methods are a set of procedures that report or represent a given reality, but that they are performative and help to produce realities.

Hence, an open inquiry is performed through two commitments, first, that the field is open and porous and not a passive and bounded entity that exists, rather it occurs (Ingold 2008) through relations (Massey 2005) and practices (Cresswell 2004). Second, that the field does not stay within the confines of space or time, rather it stays with the researcher allowing her to re-configure and re-write the field in different ways. The following section discusses how the dynamics in the field shaped my engagement with the research materials, and my persistent attempts at fielding the field to get some semblance of a coherent fieldwork plan. Fielding the field means curating our response in relation to what the field demands in order to be able to produce more engaged accounts of reality.

12.4 The Field, Sites and Stories

One aspect of my research with disaster-affected communities was to understand their imagination and understanding of home, displacement and rehabilitation, as experienced, negotiated and narrated by locals themselves in response to the disaster event. I employed community mapping (Kitchin 1994; Grasseni 2012) to investigate local inhabitants' (displaced or affected by the disaster event) ideas, 'sense of place' and notions of dwelling, interpretation and moving about in a landscape, which they have acquired through their association with their landscapes.

I began engaging with communities through collective spatial mapping exercises (a daily circuit), and to capture the relations between communities and their landscapes, performed through practices of everyday life, expressed in the spatial arrangements of houses and objects of belonging and association in a shelter site. For example, in the daily and weekly circuits of some displaced communities living in shelters showed that they divided their time between the shelter and lands in ways that might not always fit the harvest calendar (growing and harvest times spent at land and winters in shelters). So, what else was happening (migrations for jobs/study/exploring other livelihood options) and how might that relate to processes of

rehabilitation and resettlement? The task was to surface these practices and processes that were made present and absent (Law 2004), as each method brought a certain reality to the fore. But more than the method, it was my position in each situation that enabled the production of research materials. On several occasions, open inquiry challenged my position as a researcher and I had to negotiate between different positions to generate relevant materials.

When I arrived at the disaster-stricken landscape close to Hunza Valley in Northern Pakistan, it was just after the tourist season (summer months when people from the south visit northern areas in Pakistan). The local people were busy in preparation for the winter season; collecting grass and wood, and digging pits to store vegetables. In terms of the mapping exercise, I actively resisted the idea of having pre-planned conversations or focus group sessions. This meant that I wasn't going to set any time or place for the mapping exercise; instead, I would capture their narratives by entering their environments; homes and the fields, walking by the lake, or encountering local people at the market or van station. In the spirit of ethnographic practice, and staying committed to the idea that nothing comes without its world (Bellacasa 2012), I tried to avoid any extractive behaviour and let the field guide me through the process in order to create an engaged version of the reality of the post-disaster landscape. I conducted mapping exercises with the communities and most of them were pretty straightforward, lending breadth and depth to the issues I wished to explore. However, there were certain instances where the field and the relations with it seemed to disrupt and challenge my mode of engagement as a researcher. I discuss three instances below to give an insight to how an open inquiry was deployed in the field, the methods used, and the kind of challenges encountered.

1. In the village of Shishket, closer to the Attabad Lake, I started the mapping exercise with a group of eight middle-aged women. As we gathered around the blank A0 paper, one of them commented, "we own a lot of land, it won't fit this paper'. I had not expected this response and felt that the exercise was challenged. She continued, 'we need four times more paper than this even if we make a tiny house'. My immediate response was to save the method and add more sheets. With four sheets neatly pasted together, I invited them to draw their houses and mark their daily circuits. They made a few circuits and proposed to have tea instead. There was more to be done on the map — I would rather have had tea later. But I took a moment to remind myself about the control over the research process and of staying open, hence accepted their proposition. The tea session became a very interesting mode of engagement to explore how territories were produced and negotiated in the field. During the tea session, I became a guest in their territory and they set out the conversation for me to feel welcomed; however, on the map sheet, I was the host inviting them to tread in an unknown territory.
2. It is a long walk uphill to reach Attabad Bala, if you're from down areas, it can take you 4 h they say. Luckily a jeep gave us a ride mid-way. Upon reaching the village, I was reluctantly invited to sit in a house. I seated myself by the only window in the dark room and looked out to see a panorama of the valley. A few women came in and sat around me. After introducing my research, I asked about the disaster incident and there was no response; I assumed there was a language

barrier until a woman told me to drink tea. So, I asked them again, to tell me the story, they murmured something and told me to drink tea. It was absolutely quiet; there was a strange silence in the air, perhaps due to the height of this village. In my head, I was telling myself to be patient, and slow down and try to match my temporal rhythm to theirs; perhaps people who lived on top of mountains were very patient, and I abandoned the thought — telling myself I shouldn't be presuming. So, I sipped more tea, looked around the relatively dark room, saw the women looking at me... and then giving them a smile, I asked again, 'so what happened that day'. Pause. While no one was rude, there was just no way were they ready to say anything. After about 45 min of awkward silence, three cups of tea and my occasional insistence, an older man began to narrate the event. There was silence again. The silence was filled with an undertone of displeasure or even resentment. I could strongly sense it was time for me to leave, and the departure wasn't so easy — although I wanted to disappear immediately, it took some time to get up, gather my stuff, say goodbye to each woman and make my way to the door where I had to put my shoes on. I picked up my shoes and went outside and found a spot to wear them. I felt relieved to be out of there, but at the same time I was confused because either my presence made them uncomfortable or their obvious silence made me uncomfortable. I couldn't exactly guess what had gone wrong and how a tea session, which was meant to be warm and welcoming, could become so awkward and hostile.

3. I walked down the main road from Attabad Bala and reached a pathway going towards Attabad Payeen. After taking a quick rest at the crossroads, I cross roads, I walked down the path and saw two women at the end of the road who enquired about our presence. I briefly introduced my research about the landslide disaster and tried to establish a comfort zone — 'we can sit and talk about this over tea if you like', I offered. But there was no mention of tea, they started talking to each other in *Burushiski* language and I could sense some urgency, and we started walking towards the village where we met more women who were asked to join us. By now, I was expecting to sit down and rest a while after the 2-h long hike; I was looking forward to a tea session. But as I followed them, crossing a stream, entering a vicinity of houses and moving beyond that, my thoughts of a tea session diminished as we passed the houses and entered the fields, and walked for a good 7 min until we approached a barren area, 'this is it', the woman said pointing to the ground beneath us. A man who accompanied us, started narrating the event. I was looking at the landslide debris, which was visible due to the land formation, silt and clay hues and the absence of any cultivation or trees. I was overwhelmed, being present and standing on the site of the landslide. With the increasing sound of the river flowing beneath, it was hard to focus on what the man was saying. Fuzz. His wife added on to this, 'there was dust and a very bad smell… smell of gas… I was thrown to another side of the village, but I am alive… it became dark, we were all covered with dust'. I struggled to listen to these stories; there was this man telling his story and there were my thoughts about the debris, the location where the wrath unfolded and blocked an entire river. I was immersed in the sheer presence of the debris and its surrounds.

12.5 The Full Field: Negotiating Modes of Engagement

Each story is embedded in its field with a set of relations that could not be untangled, or extracted, and they demanded a particular obligation that I was to follow. The field required me to take up multiple identities whilst conducting research especially with reference to guest-host relations (discussed in the next section). An open inquiry not only enabled an engagement in the field, but also allowed the non-human and material entities to partake in the research. These entities emerged through observations, conversations and drawings. While walking in the barren silt scape and listening to the creaking dead trees, and when the landslide debris would not let me listen to the old man's story, or when tea would stage conversations around it. Research was carried out by following these actors; the shelters, abandoned boats, submerged houses, dead trees, silt, rocks, debris experienced through observing, listening, smelling and feeling, by being present in the field.

In-depth engagement with communities required embracing their environment: not pinning them down in a frame or context, but to get to know them through their practices, stories and aspirations. This entailed observation of how practices of everyday life were adapted to make do and live in constrained environments (such as shelters). Such an engagement required considerable delays to the daily research schedule and detours from the planned research enquiry. Producing circuit and season maps gave an insight to their sense of place, identity and belonging and enabled me to follow actors through their daily and seasonal routines. In certain instances, listening became a form of following actors through tone, pitch and plot of their stories and conversations. Other times, the voice in my head deafened me to their stories (recall the old man at the landslide debris site). However, an open inquiry is not an absolute openness; it operates within conditions of power relations (how it is distributed within research engagements), silences and lapses (of not getting access). Whilst mapping allowed more freedom to respondents in identifying their practices, routines and sense of place, I had not realised that the methods I used could be adapted and transformed in such different ways, to the point of getting discarded. Points of transition in the method were exciting and I felt as if I was treading on the peripheries of my method and staying open, but when the mapping exercise was discarded, I felt I had lost an important ground since mapping was anchoring the research in different sites. This sense of anxiety, loss and uncertainty accompanied me while I adapted the method, until I began to get comfortable with the idea that 'tea sessions' worked and they usually came to the rescue during engagements in the field. Yet again, I was taken out of my comfort zone when a tea session couldn't have been more awkward (recall the Attabad Bala story). As a researcher, I would crave a certain amount of semblance and certainty; but to conduct an open inquiry, constant negotiation is required to genuinely attend to the situations we confront.

12.5.1 Taking More Than One Position; The Fielding the Field

Host-guest relations played an important role in my research. This hospitality allowed me to go into houses and communicate with the local people. However, a deeper insight elaborates how these engagements were laden with emotions, power and cultural and social norms that subtly dictated the methods. This is evident in my encounter in Shishket when I was burdened with the hosting women's hospitality and had to discard my mapping exercise to embrace a tea session. I was treated with great respect in terms of my identity as a 'guest' in their house; however, my identity as a 'researcher' was completely undermined as the locals silently refused to engage in any conversation related to my research enquiry. This entailed a difficult negotiation; being a guest and a researcher, between the three cups of tea and failed conversations. In Attabad Bala, for example, I recognised my responsibility as a guest; it was not the 45 odd minutes of silence, but rather the three cups of tea burdened with their hospitality, which signaled me to disengage and move on to the next site without having gathered any material for my research enquiry. Moreover, I could also relate to my position as a 'host' whilst conducting the mapping exercise by inviting local women and children to take part and share their stories. I became a listener too, as I listened to many stories that did not have much to do with my research, but ethical conduct required that no conversation was cut-short. In this sense, an open inquiry in a host-guest context offers unique challenges; ethics of hospitality in Northern Pakistan allowed me to go into houses and communicate with the local people but the open inquiry depended on following cues of the hosts. So, while the researcher may get access, there is no guarantee you will acquire the material from the field site. An open inquiry requires the researcher to assume different positions that can be multiple, even conflicting, therefore careful negotiation is required in keeping in view the demands of the situation we confront.

My field engagements show that uncertainty is not only tied to our research questions but also to the ethics of encounter. The ethical relation was activated when one chose to welcome or not and to what degree into one's home (Diken et al. 2005). Encounters moderated by hospitality can be uncertain as they operate within certain constraints and 'remains forever torn between complete openness and degree of closure' (Ibid.: 188–189) for the host as well as the guest. Within a research environment, the notion of hospitality lends power to both parties in a particular way and a constant negotiation occurs between being a guest/researcher and the host; to deal with what is sought, offered, accepted and followed. Therefore, an open inquiry operates within certain constraints where we embrace strangers in our encounters through particular methods, and there is no guarantee that they will bring what we seek (Bulley 2015). Within this context, the researcher must look for which methods of engagement are on offer by the host, and adopt these rather than forcing their own methods. As such, the researcher's identity and modes of engagement are malleable entities in an open inquiry and must assume a form in response to the contextual conditions of the field.

12.6 Conclusion

When I reflect upon my fieldwork, I would describe it as a series of pinhole cameras installed in different situations to slowly capture and expose scenes from the post-disaster landscape, illuminating the obvious and evident as well as the subtle and discreet. Slow exposure enabled capturing movement, dislocation and disturbance (however blurred) in a scene lending 'depth of the field' to the image (or narrative) produced. In this, a camera's lens focuses on a single point, there are areas that stretch in front and behind it — this zone is the depth of the field. An open inquiry too, may focus on specific points but it ventures forth to capture the depth of the field in order to understand the relations that constitute a particular condition. One cannot be entirely open to the situations they confront; there are always certain positions one has to assume. Alongside this, an open inquiry also depends on the openness of the actors and entities being researched with, as they might not want to talk, share or have anything to say to the research. The difficulty of 'letting go' always accompanies one in the field; hence, the encounters in an open inquiry are defined by these negotiations. In this sense, I was neither able to fully capture the field nor dis-associate from it completely. Through reflections upon my own positionality and re-working the research materials, the field remained with me, although I had supposedly left it to write up my PhD. I realised that even before entering the field physically, it was there in my head, I had imagined it, and whilst I was physically in the field, I wasn't entirely there, and when I left the field, I couldn't really leave the field behind. The field stayed with me in different ways; through the people I engaged with daily and who continued to stay in touch with me. More than anything, the field with all its nuances stayed with me and troubles me from time to time especially with reference to power relations and guest-host relations within fieldwork. What the field continues to remind me is that no field, no context and no relations can be assumed, taken-for-granted or remain stable. The field produced through our relations is unstable and continues to assemble certain kinds of power relations that a researcher needs to be vigilant and careful about. Every field requires a vigorous response and an open inquiry is one way of achieving that.

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Chapter 13
Conclusion: Navigating Social Research — An Emotional Journey

Yim Ming Connie Kwong, Mildred Oiza Ajebon, and Diego Astorga de Ita

> *We do not store experience as data, like a computer: we 'story' it.*
>
> (Winter 1986: 176)

One of the key motivations for producing this edited volume, was to critically present reflections on a range of conceptual, methodological and practical issues permeating the social research process from the perspective of postgraduate students who have most recently experienced fieldwork. It highlights a wide variety of fieldwork issues which cannot be read off easily in research method guides or covered in postgraduate training courses and supervisors' comments. We did not write this book to simply present findings or research methods; each contributor has endeavoured to show the reader how they navigated through their encounters in the field, using stories from their own research processes. Literature on research methods reminds us that things never happen the way we planned. This has been fully demonstrated through all chapters with different levels of changes, uncertainties, possibilities, restrictions, and even denial. The contributors have reflected upon how they manoeuvred in the field with flexibility, looking back with an honest and open attitude. They have illustrated that doing fieldwork does not only entail or is bounded with a definite set of data-gathering activities (forget the neat and tidy yellow brick road), but

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rather it is part of a more fluid and iterative research process. Mason discussed the field in the context of access, navigating mobilities/immobilities and geopolitical conflict. Kwong conceptualised the field as a site on the move during which field sites and social relations can be temporary or temporal, transcending the physical material locations of data gathering activities. The field in Rella's case was hybrid, constituting fluid networks of both offline and online spaces and people, which are 'as much produced as they are discovered'. Tseng navigated in a field consisting of two elite governmental institutions where taking up a dual positionality as a researcher and an intern could allow better access and enhance immersion. Sadhan and Smout Szablewska focused on how they engaged with the field posteriori, reflecting on the intensity of emotions incurred when the 'field' stays with the researcher beyond fieldwork. Ajebon, Ahmed, Astorga de Ita, and Hussain, together with Doppelhofer and Todd, demonstrated that our experiences and encounters in the field could not be fully and easily anticipated, even when 'the field' is 'home', because every fieldwork experience is different. The ethical dilemmas encountered and the confusing positionalities that emerge may re-orient research practices, open grounds for re-thinking power relations and allow new questions to be asked. Some methods work while others do not; the open inquiry framework adopted by Hussain suggests a way of working in/with the field and entitles doing research with high flexibility.

'The field' is diverse. As shown by various contributors, in some circumstances the field was encountered before the researchers entered it physically, while in other cases the field stayed with them along the process of data analysis, writing, or even longer. The field is so dynamic that it becomes challenging to compartmentalise the fieldwork process into stark temporal or spatial marks of being 'in' and 'out'. We are in-between. The 'in-between-ness'and 'fold-over-and-back' are exactly what make us anxious. Taken together, the 11 chapters in this book show the need to adopt an 'open' approach to social research. Through this book, the contributors have challenged the normative definitions and sheer conceptualisations of what the field is, what fieldwork involves, the reoccurring impact of social encounters from the field, and making sense of fieldwork data. This volume has hopefully sparked a fruitful discussion around questions of the field — how it may be conceptualised, discovered, encountered and represented.

This book has also emphasised the importance of reflexivity. We have put forward that it is essential for researchers to constantly consider their own positionality, access and saturation of data, and, more broadly, the process of knowledge production in terms of contexts and relations with the field and with others. Various issues flagged up and discussed in this edited volume are long-standing in the literature, with which not only inexperienced or early career researchers (ECRs), but researchers of all levels often struggle. One prominent issue that cuts across the 11 chapters is emotional management at different stages of carrying out a research project. Whether we feel like we are lacking brains, heart, courage, a sense of home, or something else, emotions are present throughout the field. Understanding and managing the emotions of participants during and beyond fieldwork can be daunting for researchers (Holland 2007). Although this is an issue concerning researchers

of all levels and genders, junior and female researchers have been portrayed as the vulnerable groups (Caretta and Jokinen 2017). Hitchings and Latham (2020) have pointed out that it is still a taboo to reflect on failures and weaknesses while presenting our work in qualitative human geography. This is especially the case when 'the idea that sharing emotional fieldwork circumstances will undermine one's scientific credibility and career still prevails within the discipline' (Caretta and Jokinen 2017: 276). It is undeniably challenging to gain both physical and cultural access to the field, and deal with our various positionalities within it. It is almost impossible to define when we have left the field — if we ever do. Perhaps the question is whether it is necessary to clearly indicate being 'out' of the field, given the various rhythms of 'in' and 'out' that we have highlighted above. It is also strenuous managing post-fieldwork emotions in relation to our researcher identity, looming around the question of what kind of researchers we are in and out of the field, and at every stage of the research process in-between.

Gaining access and recruiting research participants are not uncommon challenges; however, the emotions incurred have not been explicitly reflected upon and discussed widely in the case of ECRs. Mason experienced the shock of denied access and deportation, and anxiety of developing a new PhD project. The recruitment process, as experienced and delineated by various contributors here, is extremely emotionally demanding, in terms of the amount of labour involved in identifying gatekeepers and contacting interested participants, and the subsequent worries of not getting enough data. Alternatively, concerns over the way the data is gathered, the extractive nature of research, and the impacts it has in the communities that permanently inhabit 'the field' are present, as we can see in Ahmed and in Astorga de Ita (Chapters 6 and 7). Even when we are past the anxieties of gathering 'data' and have got more than sufficient materials, we are overwhelmed by the stressful feelings of inadequacy in writing in a scholarly enough way and making a strong enough argument, as Smout Szablewska has openly pondered. This could be attributed to an academic culture in which failure is never embraced and where positive results and significant contributions to knowledge are expected (Caretta and Jokinen 2017; Hitchings and Latham 2020; see also Doppelhofer and Todd in this volume). The level of anxiety is even higher when researching a fieldless field with mounting uncertainties, and concerns over waiting, despite tons of background research and preparation in advance (see Kwong; Rella; Doppelhofer and Todd).

Anxiety abounds, whether doing research in a new country or our home country. Throughout this volume, a few contributors have challenged the simplistic assumption of the stark dichotomy between what it means to be home or foreign, especially in terms of power in the research process. They revealed their cultural uneasiness when conducting fieldwork back home, yet feeling foreign. Ahmed encountered profound discomfort when his gatekeeper handled a recruitment situation in a way which escalated into an embarrassing fight. Astorga de Ita critically contemplated notions of 'home', 'field', 'here' and 'foreign', and his experience of otherness in the field and home country — Mexico — as well as in the place of study. Ajebon reflected upon the quandary of conformity (Chong 2008) and how her 'western' ways of taking care of her accompanied new-born son were considered culturally

inappropriate back in Nigeria, which impacted recruitment. This awareness of cultural and social norms brings us back to the issue of fluidity in researcher identity, positionality and power, and the sort of uneasiness that arises when negotiating our different roles and identities. In line with Willis (2014), these chapters revealed that the politics of power, positionality and ethics of research become embedded in ways in which researchers approach their work. Together, Ahmed, Ajebon, Astorga de Ita, Rella and Tseng discussed the difficulty of creating a static balance of power between the researcher and participants. They propose that researchers must constantly negotiate ever-shifting power relations at every stage of knowledge construction (see also Holland 2007). Navigating our interactions with our research others entails dealing with subtle power dynamics fraught with emotional tensions and uneasiness, for which there are no clear-cut ethical guidelines (Reeves 2010).

Hussain encountered this kind of uneasiness when negotiating between local hospitality and planned methodologies, and uncertainty about what would come next as she shifted between being a guest and a researcher. On the one hand, local hospitality allowed her to go into houses and talk to women. On the other hand, it implied that the hosts took the lead in that space and that she, as the guest/researcher, had to discard her plans and follow the norms, wherever they led. Similarly, Ahmed had no other choice but to allow the gatekeepers to determine which children were enlisted into his research project. For Kwong there was confusion and awkwardness around the shifting nature of her relationships with her research participants as they moved from one field site to another: team members when volunteering in Cambodia; friends/researcher-researched when hanging out in Hong Kong and Taiwan, while she was observing and interviewing them; and then friends once again, after fieldwork was 'done'. These constant changes pose further emotional challenges in the maintenance (or not) of established relationships. This kind of emotional challenges can also be complicated when the researcher takes up a more committed role in addition to being a researcher, since this requires extra efforts in navigating the power relations and fulfilling responsibilities. Mason and Kwong explored this respectively as they laid out their roles as volunteers in collaborating organisations, as did Tseng who was an intern in two governmental institutions. In these cases, taking up an additional role helped alleviate some of the awkwardness and anxiety inherent in the trade-off of not getting work done in the field, for making a 'contribution' instead.

Many times, such anxieties are linked to the felt need to give back to research participants, within the boundaries of research ethics (Willis 2014). This felt need drives researchers to think about the value of their research to the researched community, and how to reciprocate. Ahmed discussed the tensions around compensation and the politics behind and over it when researching with children in Bangladesh. Kwong has suggested friendship as both a form of reciprocity and a way of softening some of uneasiness of having multiple identities. Doppelhoffer and Todd talked about the awkwardness of gate crashing someone's holiday and of how sharing tips about places to visit was a way of compensating participants. Astorga de Ita addressed the issue of reciprocity through moments of collective music-making, and by returning photographs to interviewees. Although using methods such as

participant observation and participatory (action) research may allow more time for the researchers to give back, how to do this carefully and appropriately still is a long-standing matter that confuses and worries researchers, as it should (see also Harrison et al. 2001).

Questions of responsibility have also been raised in this volume, particularly the issue of researchers' relations with family members during the research process, and the need to acknowledge the fluidity and convergence of the narrow boundary between researchers' private and work lives, which is increasingly being documented in feminist literature (Frost and Holt 2014; Lunn and Moscuzza 2014). Managing the roles of a researcher and a parent/partner can lead to self-doubt or even self-blame, especially when the research process causes impacts on their private life. Ajebon brought to the light the various factors that led to a situation in which she had to travel alone with her 5-month-old son for a sustained period of fieldwork back 'home' without being accompanied by her husband, who was also pursuing a PhD and facing visa constraints at the time. This led to questions of whether she had provided the best childcare to her little one, given the restrictions placed upon her by funding and residency rules and cultural norms. Likewise, the type of emotions demonstrated by Sahdan are directly linked to the nature of the research enquiry and the personal subjectivities of the researcher (Ramsay 1996). The physical and psychological impacts of the research process on the ECRs are rarely discussed or prioritised compared to those on participants. In this case, Sahdan experienced a trauma-induced disturbance to her mental state, which impacted on her role as a mother and a wife. This is particularly challenging for researchers at the stage of getting immersed into their data, which means continuous and emotional engagement with the field and their participants, despite being physically distant. When Sahdan transcribed and analysed the data, she internalised the experiences of abused women and unconsciously diverted such negativities and cynicism to her marital relationship. This could result in an enduring period of anxiety, self-doubt and discomfort alongside writing; questioning what to write and how to write. Again, this leads us back to the question of whether we have done justice to our informants and what benefits them.

Beyond the analysis of data, the writing process itself is a key aspect of research which keeps researchers emotionally bound to the field, leaving us with the feeling that we have never really left it behind. Astorga de Ita hinted at this in his chapter, and Smout Szablewska clearly demonstrated the researcher's ongoing emotional connectedness to the field, as well as the psychological costs of social science writing. She rightly pointed out that writing up social research findings can be emotionally challenging, especially for ECRs, who often struggle with grasping the unspoken but underlying disciplinary conventions for making a logical academic argument. Postgraduate writing is particularly unnerving for many international and mature students who might not have undertaken social science training in the UK. Through sharing her writing journey, she has not only showed that acquiring relevant thesis-writing skills is possible, she has also made the argument for the need to explicitly demystify writing principles, and for institutions to equip researchers with the necessary toolkit for communicating research findings in a social scientific way.

Unfortunately, there is no 'off' button from these emotional entanglements with the field. We have no ruby red slippers to get us out of 'there'. The level and kind of confusion, anxiety, uncertainty, stress, and even second-hand trauma overwhelming us are not something we could calculate, measure, put in the risk assessment form and prevent accordingly. Having said that, many times filling out such risk assessment forms before fieldwork and submitting a field report afterwards 'can give the illusion of a linear clarity to the often-frustrating fog of the research process, hiding the confusion, self-doubt, and many mistakes that are made along the way' (Harrowell et al. 2018: 231). Overall, this book has argued that emotional tensions associated with the social research process are not only central to the production of knowledge, but also that reflecting upon such tensions with openness has an undermined potential to add value to our understanding, analysis, and interpretation (Holland 2007). This volume provides honest and contextual accounts into the realities of conducting social science fieldwork by ECRs in both developed and developing countries. On the one hand, the honesty and openness of contributors in this volume is a positive step towards fostering a research culture where reflections upon weaknesses and failures are as welcome as presentations of successful fieldwork techniques and methods. Such acknowledged weaknesses and failures are part of 'the field' that cannot be erased, forgotten or left behind (Harrowell et al. 2018). These things stay with us; they inform our field experiences and our subsequent navigations, as we have argued throughout this volume. On the other hand, this volume has emphasised that it is important that ECRs receive a corresponding level of understanding, support and appreciation from their institutions, funding bodies, supervisory teams, peers, and academia at large. How should the institutional framework of risk assessments be revised to evaluate the long-term risks posed to researchers, regardless of their experience? In order to encourage existing and future researchers to navigate their research paths more realistically, there is a need for a system that has a place for debriefing as well as plentiful and open discussions, not only of major successes and little victories, but also of self-doubts, weaknesses and failures.

While the ideas put forward by the contributing authors cover a wide range of relevant topics, we do not claim that this is an all-encompassing volume on doing fieldwork in social research. Three questions are somewhat missing from this volume. Firstly, the question of languages and translation. Many of the contributing authors in this volume speak more than just English and have undertaken fieldwork in different languages: Arabic (Mason), Bengali (Ahmed), Bahasa Malaysia (Sahdan), Cantonese and Taiwanese Mandarin (Kwong), Nigerian Pidgin (Ajebon), Polish (Smout Szablewska), Spanish (Astorga de Ita), Taiwanese Mandarin and Spanish (Tseng), and Urdu (Hussain). With such a wide array of languages, one would expect language and translation to be front and centre in this volume, but this book was written in its entirety in English (with English spelling, of course!). In her chapter, Tseng explicitly spoke of the difficulties and consequences of multilingual fieldwork. Likewise, Ahmed wrote in a range of Bengali words, giving his text a rich texture, and Astorga de Ita interspersed quotes in Spanish. However, the question of writing fieldwork, and the transformation of

realities spoken in other languages into English text, brings up numerous issues ranging from the practical to the political that ought to be discussed further. If, as the adage says, poetry is what is lost in translation, it would seem that writing fieldwork experiences is anything but poetic (and what to say of what that other adage — *traduttore, traditore* — means for us). Still, some fragments of the text in other languages bleed through, breaking the Anglophonic hegemony. We hope this, alongside Tseng's discussions might suffice for now, and that later on the motives and mechanisms of the little man behind the curtain when it comes to translation will be more widely discussed, perhaps in another edited volume.

The second issue missing from this book are the lived experiences of 'the field' by scientists beyond the social sciences. While this second question might be beyond the scope of this volume, field experiences in 'hard' or 'natural' sciences are also rich, variegated and challenging, and we believe that they ought to be discussed. How 'the field' is conceptualised and encountered by those scientists gives rise to another set of theoretical, practical and ethical questions and reflections. The logistics behind biologists' transects, physical geographers' sample-taking and glaciologists' long Antarctic expeditions, for instance, are full of complexities, anxieties and challenges beyond the technical that deserve to be explored and presented to the academic community by those who experience them. This is particularly relevant given the recent increase in interdisciplinary approaches to research, through which 'the field' is engaged with and understood across-domains, resulting in novel and variegated insights into the nature of such fieldwork.

The last matter we did not touch upon specifically is 'leaving the country'. We have talked a lot about gaining access to field sites, but we did not have a coverage on how hard it is to get permission to leave the country of academic affiliation for a long period. Crang (2002) has mentioned the practical difficulties of securing funding for a prolonged period of absence for ethnographic research. This has proved challenging for students trying to get approval to leave their home country for such research. This challenge reaches a completely new level for international students who must deal with varying visa conditions. This is a pressing issue for students who need to calculate how many days they can leave the UK for, without breaking the terms of their visas. Foreign researchers become entangled in a catch-22 where their immigration status, which depends upon their work, becomes threatened by the work they need to undertake to maintain their visa. Ajebon's chapter in this volume touched upon the question of immigration challenges a researcher faces; in this case, her husband was unable to accompany her for childcare responsibility during fieldwork, due to restrictions related to his visa for studies in the UK. These issues on immigration have become a concern especially after two of our Durham colleagues, Dr. Arely Cruz-Santiago and Dr. Ernesto Schwartz-Marín and their daughter were nearly deported after spending 'too long' a time conducting fieldwork in their home country during what many (but not the Home Office) would consider a humanitarian crisis (e.g. Mitchell 2018). While the question of immigration is not clearly explored in this volume, and although some progress has been made in the regulations placed upon immigrant researchers, this is still a pressing issue and more explicit conversations on the subject are required. With all this being said, we

hope that the reflections in this book will be useful to those who come across it. This is not a guide; it is not a yellow brick road that will lead to magical solutions, nor do we offer any enchanted ruby slippers. However, we hope that by sharing our experiences we might help continue a conversation about the things that almost always remain hidden behind the method: the anxieties, the confusions, the flying monkeys. With any luck, this book will be a step away from the wizardry of the great and powerful academics, in deference to the recognition of the smallness and meekness of researchers all over the world.

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Afterword

Divya Praful Tolia-Kelly

Navigating the field is a welcome publication in these times of social, political and intellectual reflection on the politics of the production of knowledge. Reflection in relation to whose knowledge counts, gets cited, gets recognised and where the power of knowledge production should be and is shifting in a postcolonial, post imperial and decolonising era of dismantling academic hegemonies and associated violence. The researchers themselves that are embedded here, have all encountered challenges and tensions between their own understandings of knowledge production and notions of whose knowledge counts. Navigating the field iterates the fragilities of the process of production of credible, rigorous and competent research. It indicates the gap between supervisory expertise and the specificities of 'the field' itself; these are simultaneously shifting. It is only at the moment of encounter that the field is recognisable, bounded and researcher is navigator, their tools are co-created and co-dependent on the coordinates of the field; both held in relation temporarily. The spontaneous nature of research skills, tools and method that shape research is honoured here, along with the process of dealing with dynamic, contingent paradigms that are often seen as immoveable, and formulated before we step into 'the field'. Honouring the research process as it emerges, reshaped and co-created is a gift for researchers in the process of research design and navigation. We must not take for-granted any conceptual or material 'field' nor 'method', they are in flux, in the process of becoming. This edited collection is a testament to that very fact and process.

Overall, the field is not always ready, accessible and available. The researcher's body is not always welcome, and mobility is not always a privilege proffered to all equally (see Olivia Mason in Chapter 2, this volume). All terrains are unevenly experienced and unevenly accessible. For some researchers the 'site' is not static,

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tangible or indeed mapable, let alone navigable. The 'field site' itself for Kwong (Chapter 3 in this volume) is co-created through emotional attachments, relationships that are figured through temporal rather than terrestrial coordinates. The field site was not necessary in this account; the experience of 'tourism' was the connecting thread. As a result, the network of connections between folk in the research cohort and as such was not attached to a space or set of architectures that could be inhabited or known in a singular way. Multiple space-times are the basis of Kwong's field site, and this co-created site exposes the fractured nature of the geographies of research. The 'Fintech' field is interrogated in Rella's (Chapter 4 in this volume) where the assumptions about research 'network', 'field site' and 'platforms' of exchange and interface are constantly being redefined and refigured. Here, Rella argues that when interacting with financial networks — the 'field-site' is moveable both temporally and spatially, unending sequences involving transactions at internationally synchronised co-ordinates. In his research 'time' is also a moveable context, international space-time for digital monetary transactions requires simultaneous ethnographic data collection. Temporal and spatial notions of *field* and *data* are disturbed with the conclusion that the researcher needs to engage with the field as conceptualised as *rhizomatic*. An ethnography beyond bounded place-time can coincide with Fintech rhythms and mechanisms of exchange. This insight is recognisable in any research where data collection and the business of data is in constant flux temporally and spatially. Methodologies themselves need to shift to encompass the rhizomatic nature of real-world events and networks.

In the second section there is a shift from 'field' to 'the body' of researcher and politics of positionality. Dopplehofer and Todd (Chapter 5) expose the mechanics of getting the bodies together, which is often overlooked and assumed as straightforward. Their chapter focuses on making the 'failures, mistakes, and changed directions' visible, and tangible. This engagement enables them to conceive, shape and define their recruitment strategies as 'queering' recruitment (Todd) and ensuring a non-exploitative 'reciprocity' (Dopplehofer). These strategies critique the gaps of interface within the theory and practice of methodologies that have gone before, but also outline their priorities in translating research towards ethical success. In Ahmed's account (Chapter 6 in this volume), it is clear that ethical research practice is not always in our control as researchers. There are differences between the theory of having ethical values, expectations and notions of rightful and respectful engagement, and how these are unfolded in praxis. As outsiders to communities, we are beholden to community 'leaders' and gatekeepers, who themselves be inflicting uneven power dynamics and differential ethical practices that are not legible or transparent. This has led Ahmed to expose how conflict and violence can occur in interviews and groups despite our aims for their avoidance. The body of the researcher is relegated to secondary to community leaders and gatekeepers and as such become implicated in breaches of 'good' practice outlined in theory. There are hindrances and blocks to peaceful, respectful relationships between researcher and participants that are embedded in the already complex fabric of communities on the ground. These are continuously shifting and encountered as intangible and illegible. In Astorga de Ita's research (Chapter 7) the essence of researcher as Other is figured

as central to the constant struggle of being ethical whilst being mindful of politics, poetics and inhabiting the body of researcher. Alterity is constant in the researcher's experience at very many levels, proving interminable textured environments of interface between the researcher and those researched. These textures are co-produced and co-present in every interaction. The landscape of research is thus never reconciled and is in constant refiguration. Ethnography perhaps is contingent on being simply a story or tale of encounter and the production of knowledge is simply a body of the written poetics of this encounter. All that is made in the field is the notion that we are always being re-made as researchers and there is no bounded 'field' or 'body' for the researcher to inhabit. In Tseng's account (Chapter 8 in this volume) it is clear that to achieve immersive observation in the field, the researcher (outsider) often is in a position of a 'threat' given particular political contexts and as such has much work to do to 'fit'. Part of the work is to address power relations directly through re-figuring the body and practices of the researcher. The moral geographies, grammars and vocabularies of the institutions engaged with are mirrored and embraced to then over time create a liminal space of interaction, where less guarded conversations and more relaxed and trusting exchanges ensue. Tseng, argues clearly that the positioning of researcher is also about creating hybrid identity and rhythms of conversing, interacting and exchanging in field sites. The researcher is thus unfixed and unbounded to maximise gain in data in the fieldwork phase of research. Ajebon in Chapter 9 interrogates notions and assumptions about 'the body' of the researcher to a deeper terrain. Shifting identity, through motherhood is the focus here, with a focus on how body and thought affect the research process through shifting identity, values, priorities and needs in the field and in the analysis of research data. There are invisible and visible transitions between researcher and researcher as pregnant, researcher as parent, that are illustrated in this chapter outlining how 'body' and 'research' ideas are co-produced and reconfigured.

Section Three focuses on *Writing the Field*, Sahdan (Chapter 10) expresses how the process of writing is itself about negotiating trauma and violence. That these are not 'out there' in other stories or lives. We as researchers are interwoven and implicated in trauma and violence as affective subjects with triggers, and resonances with the narratives in fieldwork. Sahdan courageously asks us to reflect on the very idea of writing academically as a product of collusion and co-production with those in the field and the academy simultaneously. This chapter challenges the assumptions implicit in the process of academic writing and exposes the risks of trauma as tainting the production of knowledge. Smout Szablewska in Chapter 11 takes the assumptions about academic writing and its production as traumatising itself. There is a clear need for a 'toolkit' that demystifies the nature and grammars of writing up research. There are psychological and emotional costs to learning, understanding and executing the writing of the research. Smout Szablewska takes to task the gaps in the training environment for the researcher to become a writer. Hussain in Chapter 12 outlines how a stance of 'open inquiry' disturbs the usual retrofitting of theory, method and dissemination as smoothly intertwined and seamless. The framework outlined here requires shifting positionality, and a 'messiness of

presence' that continually co-creates the field and paradigms of research inquiry. The writing of 'the field' and 'the body' of research becomes assemblage. As does the writing up of research process. What is continuously engaged with is participants and their 'sense of place'; these anchor the rhythm of research and privileges research participants' everyday lived environments. Storying these narratives and engagements is the outcome of 'open inquiry' and as such becomes a form of write-up that is ethically mindful and faithful to the interactions in the field. This opens up further, the question of what counts as academic writing and dissemination.

Last Words

This edited collection is truly international and addresses many of the research quandaries that are experienced and pertinent yet overlooked in more parochial collections and approaches to fieldwork and research navigation. The collection reminds us that we have a grave responsibility in 'international' research institutions that those conducting fieldwork are adding to the repertoire of the possibilities of research methods, questions and the shape of the navigable field itself. These institutions also need to acknowledge that PhD students are in terms of time allocated to research, 100% research focused and as such have a suite of expertise not always recognised and acknowledged as discipline-defining in research discussions, conference communities, and publication realms. This collection is a celebration of the intellectual quality, competence and contribution of researchers not always on the radar of 'cutting edge' and 'blue-skies' research that is celebrated.

A minor gap perhaps in the collection is more of an advisory call to early career-researchers, to have the confidence to engage much more critically with the methodological literatures that have gone before. A second advisory note would be to actively recognise and cite academics doing this political work of re-figuring the field, body and politics of credible knowledge production, including each other. There is a difficult balance to be made between revering those already canonised in the disciplinary repertoire and to value those pioneering voices seen as 'other', who are challenging, who are seen as politically radical but not always powerful allies in the political field of research publications. In this collection, there is a consistent and clear outline of the problematics overcome in the field, positionality and other bodies of literature, but there is a reluctance to challenge and outline the limitations of the methodologies or ethnographic frameworks that are foundational to research design. This reluctance is often a material response to the 'gap' in their utility or 'fit', but also a reverence to their canonisation in the disciplinary repertoire. Challenging the repertoire is at the heart of this collection; both implicitly and explicitly.

Index

M. O. Ajebon et al. (eds.), *Navigating the Field*,
https://doi.org/10.1007/978-3-030-68113-5