Fieldwork, Failure, International Relations

This is an excerpt from *Fieldwork as Failure: Living and Knowing in the Field of International Relations.* Get your free copy from E-International Relations.

This volume aims to unsettle the silence that surrounds fieldwork failure in both methods training and academic publications. Speaking from the practice of Ph.D. research to (postgraduate) researchers embarking on fieldwork-based research projects, it seeks to problematise the notion of fieldwork-based methods as mere instruments for data-gathering. In doing so, it joins volumes that deal with practical aspects of fieldwork (Cerwonka and Malkki 2007; Nordstrom and Robben 1995); more recent reflections on fieldwork in specific contexts (Kapiszewski, MacLean, and Read 2015; Bliesemann de Guevara and Bøås 2020; Glasius et al. 2018); and novel experiments with forms of writing (Pachirat 2018). We further build on works that have successfully brought methodological discussion to the centre of International Relations (IR) debates: feminist reflections on the political and ethical investments of researchers (Wibben 2016; Ackerly, Stern, and True 2006); thinking about the role of methods in critique (Salter and Mutlu 2012; Aradau et al. 2015); probing of the ethical dilemmas of immersion (Dauphinée 2007); and calls for reflexivity (Steele, Gould, and Kessler 2019). The authors assembled here complement this rich literature by addressing the specificities of conducting fieldwork within IR and speaking *from*, rather than about, the difficulties of living and knowing in the field.

Learning Methods

We, the editors, thought ourselves to be 'responsible' IR scholars when we started our Ph.D.s in 2014. We took methods seriously: we read the heated debates on ethnography, we were aware of the colonial underpinnings of the terms 'field' and 'home', and we expected fieldwork to be power-laden and challenging. We approached these problems solemnly, reflectively, and with trepidation. And, importantly, we met at a place that promised to give us the tools to solve them – a summer school on ethnographic methods in Ljubljana.

The idea of method that underpins the possibility of a 'methods school' is one of a *recipe*. To use John Law's (2007, 9) formulation*,* as students we are told that doing methods 'properly' will ensure 'a healthy research life'. Approached in this way, methods are a series of *do* and *don't* instructions. They are given as advice, summaries of best practices, and lists of things we should do to turn our research design into knowledge. As we shared our fears and excitement with colleagues in Ljubljana, we were relieved by these promises: if we followed the instructions, we too would become successful researchers.

In addition to this 'healthy' research, fieldwork-based methodologies promise *exciting* research. Within IR, they were introduced with worthy aims: to 'resolve the aporias of textual representation', work towards 'emancipation', and cultivate reflexivity (Vrasti 2008, 284). The lure grew stronger as we learned about the political investments of methods themselves: how we approach the field, design our research, position ourselves among our collaborators, and ways we write are all processes shaped by power. Methods are an opportunity to situate ourselves in relation to that power, they allow us to enact some and disrupt other worlds (Aradau and Huysmans 2014). The excitement of confronting these politics was as attractive as the promise of the many secrets we imagined finding in the field.

After our methods school in 2015, we had not seen each other again until November 2017 when we both attended a

Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

workshop in London. In this new meeting, we were not supposed to learn about methods, but present findings that we had reached through practicing them in the two years spent in our respective fields, Katarina in Serbia and Jakub in Israel/Palestine. Catching up, we quickly concluded that we had not succeeded in doing 'real ethnography', and, despite perhaps earning us degrees, our fieldwork had mostly been a 'failure'.

In subsequent informal chats with colleagues and friends, 'failure' increasingly appeared to be part and parcel of fieldwork methods and knowledge production more generally. Stories multiplied: botched interviews, sexual harassment, broken limbs, ruined relationships, political inadequacy, inescapable guilt – failures were everywhere, yet conspicuously absent in public debates about fieldwork, publications, and conferences.

Our informal conversations continued at an Early Career Researchers Workshop at the EISA convention in Prague. The workshop demonstrated the potential of talking about failure: it allows us to be more transparent about the many material and emotional factors that shape our research, exposes long-standing academic conventions that form academic subjectivities, and provides an opportunity to challenge academia's obsession with productivity and the narrative of disembodied research. This volume is an outcome of this open, trustful sharing.

Failing in/to… What?

Thinking about failure in academia is confusing. As Sarah Naumes (2015, 827) notes, 'to admit to failure may as well be a cardinal sin in the academy'. At the same time, failure is emerging as a political concept capable of challenging the neoliberal imperative of success and productivity. Failures are retold in efforts to 'provide some perspective' by publicising the many rejections that make an average academic career.[1] Recently, Laura Sjoberg encouraged us to embrace failure in critical scholarship, as critique 'is always and already failing and failed' (Sjoberg 2018). Even within methodological discussions, failure can mean a variety of different things: from projects that were prevented from ever being (Smith and Delamont 2019), to those that changed dramatically in response to changing conditions in the field (Kurowska 2019; Daigle 2016).

After many conversations, we recognise that the failure we observed in the London workshop connects the affective experiences of the researcher with larger political and epistemological investments of our research. We do fieldwork looking for different perspectives, political relevance, and engaged writing. In her review of IR ethnographies, however, Wanda Vrasti (2008, 284) points out that these aims do not only remain unachieved, but having those expectations in the first place is proof that IR scholars have *failed* to understand what ethnography really is (cf. Lie 2013). Many of the failures recounted in this volume relate to these great expectations, despite all of us having read both Vrasti's work and the anthropological debates that inspired it.[2]

No matter the management of expectations and the humility that we profess, most of us do fieldwork because we believe that the stories we are told by those we meet 'in the real word' are worth telling. Ethnography is animated by a commitment to an epistemology that recognises our necessarily limited and partial positionality, and by extension the political value of attending to others' perspectives (Haraway 1988; Harding 1992b; 1992a). Even while being aware that fieldwork is in no way free of the problems of representation and the powers that shape it (Dauphinée 2007), we travel to field sites because it is an exhilarating encounter with other perspectives that challenge our concepts, change our directions, and make us confront first-hand the power that we study.

Mobilising these perspectives in our research and our knowledge claims necessarily means making people and things legible: we note and transcribe, draft and structure, analyse and write, follow-up and reconceptualise in order to make the social world we encounter comprehensible to us and our peers. This aspiration to represent sets us up for a complex failure. It inextricably connects us with a desire for mastery: we represent in order to make the 'authenticity' of being there legible to our epistemic communities, even when we know that any such claim to authenticity is impossible.[3] The stories we tell *necessarily* fail: they are incomplete, situated, and imbued with the power of our own interpretation (Page 2017; Daigle 2016). In short, as we do not have privileged access to ourselves *or* our interlocutors, all our understandings have to be accompanied by an 'awareness of fracture and partiality' (Dauphinée 2016, 48).

Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

While the dismantling of the ideal of mastery and representation is an ongoing and never-finished process, we also fail on a daily basis when dwelling in the field. Rebecca Hanson and Patricia Richards (2019) have recently shown how these everyday experiences are powerfully shaped by three 'ethnographic fixations': solitary research, danger, and intimacy. Even after decades of work that feminist, postcolonial, and critical race theorists have done in academia, these fixations still shape not only what happens to us in the field and the wider academe, but also how we interpret it. Drawing on Hanson and Richards' volume, we argue that both the failures and the absence of these failures in talk and writing say something about the state of our discipline and academia more broadly.

The failures we discuss in this volume are not simple rejections, closures, or endings: they are continuous negotiations in the practice of doing and writing research. In their focus on fieldwork experiences, the contributions are then similar to 'confessionals' – tales in which we are told 'what really happened' in the field and the many failures that accompanied fieldwork (Van Maanen 2011; Rabinow 1977; Jemielniak and Kostera 2010; Schatz 2009; Thomson, Ansoms, and Murison 2013; Yanow and Schwartz-Shea 2006). In addition to 'telling the story', the chapters also uncover and challenge the disciplining structures of science that need to be both navigated and challenged. In this volume, we consider them together to highlight the power that disciplinary expectations have and the ways in which they guide our positioning as researchers – even when we are explicitly warned against such illusions.

In discussing fieldwork failures, we might be once again setting a goal that is out of our reach: to shake up the idea of an always and already knowing researcher, to bring in emotional and bodily experiences of academic work, and to help move towards different, more caring, and less conclusive ways of knowledge production. Yet even if we are bound to not (fully) succeed, our collective experiences require us to work towards this goal. With this in mind, we now turn to two phases of fieldwork-based projects which are usually marked by perceived failures: being in the field and 'writing up'.

Doing Fieldwork

Fieldwork is never a straightforward application of methodological ruminations and instructions: researchers are unable to control all the factors that determine access to sites and people; they struggle with finding time to do fieldwork; they try to juggle it with teaching responsibilities, administrative duties, personal and family-related considerations; and they depend on ever scarcer funding. In this section, we review three issues that underlie the failures recounted in this volume. We talk about the difficulty of ensuring access and the major role that luck plays in it, the expectation of intimacy, and the still dominant notion of solitary research.

Getting There

In most fieldwork accounts, access problems stay on the margins. Researchers tell us how they established contact, chose their locations, and made their way into the field, but they rarely talk about the obstacles encountered and the affective dimension of the process. The lack of discussion around these issues does not only make researchers fear and suffer failures alone; it also contributes to a limiting vision of fieldwork in which our decisions are based solely on rigorous research design, rather than on the messy relations that we are investigating. Several chapters in this volume speak to these issues. Johannes Gunesch, in his chapter with Amina Nolte, talks about completely changing his Ph.D. research due to security issues: abandoning the planned fieldwork in Egypt, he re-designed his question to study the Egyptian diaspora. Sezer İdil Göğüş notes how she had to start her project anew after the failed coup in Turkey made her focus on AKP activists in Germany instead of doing fieldwork in Turkey. In the final chapter, Berit Bliesemann de Guevara recounts the difficult start of her research project in Colombia. After initially being granted access to work with political prisoners, a bomb attack in Bogotá for which the prisoners' group claimed responsibility quickly turned them into 'terrorists' and closed them off from research.

Issues of access do not end upon arrival in the field. When confronted with institutional, bureaucratic, and spatial restrictions that can easily derail any outlined project, flexibility and ad-hoc decisions become the norm rather than an exception. This became evident to Holger Niemann at the beginning of his fieldwork at the United Nations headquarters in New York City described in his chapter. From the onset, it was clear that his aim to understand

Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

decision-making practices in the Security Council was significantly hindered by the way in which global power relationships were transposed onto the architectural blueprints of the UN headquarters.

As these and other accounts testify, confronted with limited and/or changing access, researchers have to deal with the possibility of the whole project failing, the feeling of inadequacy, and even fears of financial liability.

Empathy, Intimacy, and Connection

Fieldwork operates with an expectation of empathy – ethnography seeks to humanise the 'other' as a strategy for achieving positive social and political change. In this expectation, intimacy becomes crucial. It is intimacy that is supposed to give us a glimpse of different points of view – we might not *become* the people we study, but by living, thinking, and feeling close to them we should be able to understand how they see the world. In short, we expect to feel 'with and for another'.[4] This quest translates into anticipating personal bonds, solidarity, mutuality, and perhaps even love which are supposed to form during fieldwork. These expectations insidiously set up another trap: as feminist scholars have explored in detail, not only can we never truly feel for another, but sympathy can also easily slip into appropriation (Sylvester 1994; Ferguson 1991). The relationships on which fieldwork depends are complex and fluid – we inevitably fail at intimacy and then deem ourselves incapable of producing valuable ethnographic insights.

In this regard, ethnography failed many of the contributors to this volume, and instead of intimacy they encountered alienation. We feel detached not only from those we knew we would find disagreeable, but, at other times, we feel distance even from those with whom we would expect to build solidarities. In her chapter, Emma Mc Cluskey talks about the decision to abandon her research in a refugee camp after being unable to come to terms with a public whipping that had taken place there. Even though her research was meant to challenge the dehumanisation that happens through EU security practices, she could not generate the empathy that is supposed to underpin this process.

The bonds that we do make are ambiguous and may change quickly. Ewa Maczynska's chapter shows this complexity when her relationship with a research participant becomes almost impossible: in her responses to him, she must take into account his position of a non-European migrant, but this is exactly the position that he wants to escape. Similarly, 'the way forward' can easily come about from mundane misunderstandings. For Lydia Cole, deeper understanding of the intersection of politics and psychological care for victims in Bosnia and Herzegovina was made possible by what many would consider a researcher's failure – laughing and crying (inappropriately) in front of her interviewees. Lydia treats these 'failures' as openings – academic, personal, and affective – rather than closures, and shows how 'empathy' can be generated through unexpected means.

Lastly, axes of race, gender, class, and ability intersect to shape our experiences in the field and the relations that make it. Jan Daniel, for example, discusses how gendered norms of what a security researcher should be like shaped his ability to connect to his interlocutors and fuelled feelings of inadequacy. In his conversations with military officials in Lebanon, which he conducted as part of research on interactions between local actors and global organisations, he was perceived as failing to satisfy the ideals of militarised masculinities that were hegemonic in the spaces he conducted his interview in, a situation that significantly shaped his research. In Sezer İdil Göğüş' case, her position as a Turkish secular woman based at a German institution, yet conducting research on Turkish politics and later the Turkish diaspora, made an imprint on her doctoral project in several ways. Her chapter makes clear that one cannot be sure which of the myriad identity markers condition encounters in the field, in what way, and with what emotional impact. The same is demonstrated by Amina Nolte's research on Israeli infrastructure: at times she was able to use gendered imaginaries of women as harmless to her advantage, yet her Muslim name also brought about complications even before she entered the designated field.

We should note that the discussion of the researchers' identities in this volume has a very specific limitation: all the authors included are racialised as white. This is telling of a larger problem of whiteness that is still at the heart of academia. Although the volume thus misses an important aspect of 'what makes a researcher', we hope that the discussion started here will encourage people to engage in further conversations, including those on race and

Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

fieldwork (see e.g. Henderson 2009; Loftsdóttir 2002; Hanson and Richards 2019).

Solitary Research and 'Having What it Takes'

The expectations of integration into the community in the field and of intimacy are closely related to an expectation of being otherwise alone. Even though early ethnographers often relocated with their families who provided both emotional support and research assistance,[5] the fieldworker we usually imagine is a lone hero.[6] During fieldwork, the researcher 'must cut his or her life down' (Van Maanen 2011, 151): not only do we have to suffer through fieldwork as a sort of initiation, but we have to suffer it alone (Ibid., 29) – as if asking for help or working in teams would both detract from our skills and somehow ruin the methodological process.

There are some obvious questions here. Who are those who can afford to 'cut their lives down to the bone'? Caring responsibilities, class and racial locations, and a spate of other factors dictate who is 'allowed' or 'able' to cut themselves off from their 'real life'. And what are the costs of such 'cutting off'? The material and emotional capacities needed to turn a research design into a finished project need to be carefully considered (D'Aoust 2013). As Jakub Záhora's chapter shows via his disclosure of struggles with depression throughout his fieldwork in Israeli settlements and beyond, there are many emotional and personal experiences and qualities that shape what we access, what we do, and how we come to understand our interlocutors.

The current 'epidemic' of mental health issues in academia affecting both staff and students significantly raises the costs of doing research.[7] The fact that many people would endure mental illness, but also plain loneliness and sadness for prolonged periods of times, harks back to the idea that fieldwork is supposed to be somehow challenging, even dangerous – as committed researchers, we should be ready to do anything for data. By talking about our failures to do so, we want to question the ideal of the fieldworker as a disembodied vessel of knowledge smoothly navigating new relationships.

Afterlives of Fieldwork

Fieldwork also shapes our expectations of written outcomes – we want others to be equally thrilled about our findings, do justice to the people we spoke to, and give back to those who gave to us. And we have to publish our findings and secure jobs. We, as many others, have learned the hard way that failures do not stop when leaving the field. There are two issues we want to investigate further in this 'post-fieldwork phase': the expectation of engaged writing and the increasingly neoliberal environment in which early career scholars conduct research.

Fieldwork as Textwork

The social sciences are going through a process of raising their 'textual consciousness'. Within IR, discussions of voice, positionality, authorial presence, and reflexivity are employed to fight the fetishisation of objectivity, the soullessness of jargon, and the absence of a positioned author (Doty, 2004). In this context, fieldwork emerges not only as a method, but also as a path towards a different kind of writing. Such expectations set the stage for another failure.

Translating impressions, experiences, and narratives that excite us while in the field into academic texts, judged by their rigour and coherence, often causes deep frustration. Renata Summa addresses this issue in her chapter through a reflection on her struggles to 'capture' her impressions of Sarajevo, where she conducted research on everyday bordering practices. A city that she found mobile, dynamic, and illusive was forced to stand still on paper. This inevitable gap between experience and writing leaves one doubting the value of the text we produce.

In its reflection on the 'textwork' involved in turning fieldwork into academic outputs, we consider this volume to be an intervention. We recognise that both fieldwork and writing that it inspires require a constant 'construction and production of self and identity' (Coffey 1999, 1), and the chapters examine these processes through narrative, dialogue, and self-reflection. We thus contribute to the ongoing project of developing novel forms of writing IR and approaching the world differently (Ravecca and Dauphinée 2018; Dauphinée and Inayatullah 2016; Inayatullah

Fieldwork, Failure, International Relations

Written by Katarina Kušić and Jakub Záhora

2011). The stories presented are necessarily partial, yet also constitutive of the researchers' selves; by disclosing our failures, the connections between our biographies and our theories are made more accessible to others.

Politics Within and Beyond Our Texts

The writing that is born out of (or despite) the failures we recount is engaged in politics: of academia and of the world we study. This engagement is discussed in Katarina Kušić's chapter where she warns that even those sympathetic to efforts to challenge disciplining academic norms should be wary of the proverbial good intentions. In discussing the perils of narrative writing in a neoliberal context, she highlights the need for careful calibration of writing style – something that all the contributions to the volume navigate.

Our texts are also immersed in the power structures they study. In addition to our investments – temporal, emotional, physical – we are in debt to countless people who gave us their time in the faith of us 'doing something' with their input (beyond hard-bound theses sitting in libraries). Desirée Poet's chapter shows how her research with urban indigenous and urban *quilombo* communities in Brazil had to navigate the internal politics of these movements, and wider academia. Her own dissatisfaction with the collaboration that she tried to integrate into her fieldwork shows that the often celebrated 'participatory research' is rarely capable of addressing the power disbalances between the researcher and the researched. Danielle House went through a somewhat similar experience in the course of her project on disappearances in Mexico. Working together with those affected by disappearances enabled her to get closer, and also repay some of the time people invested in her. Desirée's and Danielle's chapters then provide us with glimpses of hope: there might be a way forward – if we take our political commitments seriously and work together for transformations of the structures that inhibit academia as well as what we call the field.

Finally, these concerns are closely related to what determines academic fates today: there is the failure of not publishing, not publishing enough, or not publishing in the right outlets. This type of failure is by now all too familiar – which nonetheless does not diminish its impact on people's fates. The 'hidden injuries of the neoliberal university' (Gill 2010) are not that hidden anymore, but the pressure persists. The imperative to turn our fieldwork into 'successful' publications again points to systemic issues of the academic industrial complex. The current epidemic of 'atypical contracts' – short term, fixed term, or zero hours,[8] exacerbates the publish-or-perish rationality and simultaneously prevents early career scholars from devoting time to follow-up research, writing, and publishing. The 'academic market' pushes early career researchers to enact a never-ending mobility posture (Allmer 2018).[9] As a result, the same intimacy so valued in ethnographic research (and that helps us survive the taxing nature of fieldwork) becomes an obstacle to the competitive self-entrepreneurialism needed to navigate the neoliberal academia.[10]

Notes

[1] The CV that went viral belongs to Johannes Haushofer at Princeton (https://www.princeton.edu/~joha/Johannes_Haushofer_CV_of_Failures.pdf), but the idea was first developed in Melanie I. Stefan's (2010) article in *Nature.*

[2] We have in mind the 'crisis of representation' debates that started in social anthropology in 1986 with the publication of *Writing Culture* (Clifford and Marcus 1986) and *Anthropology as Cultural Critique* (Marcus and Fischer 1986).

[3] In her volume *The Queer Art of Failure,* Judith Halberstam (2011, 2) connects this idea of success to the discipline(ing) of social sciences: staying within well-defined parameters helps maintain order and depends on

Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

making things legible.

[4] We thank Xymena Kurowska for this formulation in her comment on an earlier draft.

[5] For example, James Scott's (Scott 2008) fieldwork for *Weapons of the Weak* included moving his entire family with him to Sedaka.

[6] Different authors discuss the existence of an 'Indiana Jones' image of the fieldworker, yet this image still persists (see Rock 2001, 33; Pachirat 2018, 78–83; Hanson and Richards 2019, 28–29; Clifford 1988).

[7] https://www.theguardian.com/education/2019/may/23/higher-education-staff-suffer-epidemic-of-poor-mental-health

[8] In the UK, one third of all academics have fixed contracts. See:

https://www.hesa.ac.uk/news/24-01-2019/sb253-higher-education-staff-statistics.

[9] See https://www.sciencemag.org/careers/2017/08/toll-short-term-contracts.

[10] It is noteworthy that even Katherine Verdery (2018, 297), an established American professor of Anthropology, writes that 'the gratifying durability of the connections' in a Romanian village where she repeatedly conducted fieldwork over the span of four decades 'stands sharply opposed to [her] life at home, fractured by multiple moves from place to place'.

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Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

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Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

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Fieldwork, Failure, International Relations
Written by Katarina Kušić and Jakub Záhora

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