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Theoretical Contributions

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Staring out my window: Reflexivity and relationality in research in a Covid-19 world

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Abstract
In March 2020 I faced a research crisis. Like many PhD students, my project quickly fell apart and I was faced with writing a PhD on migration while stood still in London. In this article I explore the techniques I used to try to write about movement through conscious stasis and what this can tell us about the nature of fieldwork. I explore my attempts to answer these problems through experiments in autoethnography and communal storytelling. Both of these methods were attempts on my behalf to think through what it means to do ethical research in the face of a climate crisis and a pandemic. As such, I explore the ways in which using them changed my thinking not only about my project, but about the nature of research itself. I argue that moments of rupture such as COVID shine a light on the structuring of 'normality' in research. I write against a return to that normal. A normal that has justified extensive international travel in the face of a deepening ecological crisis, a normal that celebrated knowledge extraction and created material realities which governed 'who' the researcher could be.

Keywords
Archives, autoethnography, communal storytelling, colonialism, historical fieldwork, reflexivity, relationality

Introduction

In the spring of 2020 I bought a hammock. I am a slow thinker. Staring out of windows had long been an integral part of my method as a historian and so, in this moment of slowing down, I decided to lean into it. I installed it right next to my bedroom window. There is not quite enough space, which means that when I lie there my head collapses into my shoulders and my feet wedge against the bed, but the discomfort is nothing to the beauty outside. Over that summer I watched fox cubs learn to hunt, waited patiently to hear the local woodpecker and was even blessed by a family of wrens who chose to build their nest in the cracks above the window. It was, in many ways, idyllic. It saved me from myself in the 2 months of a stay at home order.

In the time I lay in that hammock I wondered what to do with my research. I was half way through the first year of my PhD when the threat of covid became visceral in London. I had recently submitted my upgrade portfolio to change from an MPhil student to a full PhD student. I was surprised that the portfolio passed, as I had submitted it in the middle of the project falling apart. I study Indian indentured labour in the 19th century and my original proposal had involved significant international travel to use different archives. These plans became virtually impossible in the space of a couple of weeks. I could not access

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the archives here in London for 6 months let alone arrange to visit other archives around the world.

These closures gave me time to lie in my hammock and think. I read as much theory as I could, I attended zoom lectures from all over the world, I spent too much time on twitter. While it initially felt like the reasons for the disintegration of my project were recent and outside of my own control, I grew to see that the issues were long standing, pre-dating masks, lock-downs and self-isolation. I had entered my PhD uneasily, not entirely convinced by my project or by myself, both in terms of capability but also in appropriateness. In those first few months of lockdown, I attempted to reconcile this, to bring who I am and what my project was into a single frame. I attempt to make sense of these musings, to sketch the result of these decisions and indecisions here.

This article is both defined by covid and yet covid is conspicuous in its absence. I am not focussed on what happens to research in the immediate throws of disaster. Instead I write about what comes next, what is built back, what is returned and what is made anew. When international travel returned as a possibility for those of us resident in the UK, I chose not to restart my previous plans but to remain in London. In doing so I forced myself to contend with the extractive research practices around which I had once designed my PhD. Instead of utilising colonial archives around the world, extracting 'data' from the South to be turned into 'knowledge' in the North, I sat asking myself what a community driven history would look like and what place, if any, academics could have within it.

I consider my attempts to rework my project through autoethnography, communal storytelling and concealment, which in many ways failed. While I write about the changes I made to my project, the thinking that went behind them and my hopes for their utilisation, I am aware of the limitations of the method. I come to no conclusions on how to overcome the problems my project faced and the problems I see inherent in historical research methodologies. I am wary of narratives that speak of the benefits to research from the pandemic, as if we are somehow separate from the world it continues to threaten. Instead I explore the ways in which academics can, in Hartman's (2008) terms, 'sit in' the problem.

While I speak from the position of a history PhD student in the UK, the issues I raise have wider resonances. Research in the Global North has, for many years, relied on extensive international travel in the face of a deepening ecological crisis, on treating people who live in the Global South as 'data points' and relied on overworking and created material realities which governed 'who' the researcher could be. I write against a return to this status quo in research, a normal that was 'predicated on disease' (Brand, 2020, n.p.). The pandemic continues to be a brutal reality, but it also challenges us to think beyond the neo-liberal trappings of our present to centre our collective inter-dependence. In this exploratory article, I extend the call made by Brand (2020) to use the pandemic to imagine the world otherwise.

On fieldwork

The clearest aspect of my project that fell apart was the sudden inability to conduct research outside of my home, something that my university insists on calling 'fieldwork'. Not all universities use the term, but it is increasingly bleeding through into the PhD vocabulary. There could be several reasons for this proliferation, such as the growing cross-pollination between 'history' and disciplines that take spatiality seriously and thus emphasise the importance of the 'field' as a location in the production of knowledge, particularly geology, geography and anthropology (Kuklick & Kohler, 1996). Equally, the rise of cross disciplinary specialities such as science and technology studies and gender studies has hinted to a growing importance to situate knowledge within its contexts (Haraway, 1988). Within the context of my university, these developments are entangled with the linger

effect of the boom in area studies in the 1960s in UK higher education, something which in turn was heavily reliant on imperial infrastructure (Kuklick & Kohler, 1996).

The idea of the field, while relatively new to the discipline of history, has been part of a central methodological critique across many disciplines. Harrison (1991) mounted a challenge to anthropologists to consider the dual formation of the field and the capitalist market and to move beyond the concept of 'fieldwork' as mining for raw data. In the ocean sciences, Max Liboiron and their fellow researchers at the CLEAR laboratory have established feminist and indigenous processes to more ethically engage with the concept of the 'field', including re-matrification of fish samples, community peer review and well considered guidelines for working with indigenous groups (CLEAR, 2021). There has been equally invigorating work done to consider what the field might be for academics engaged with digital methods and in media studies to map how the field intersects with aesthetics of technical modernity and privatisation (Wershler et al., 2022).

The notion of the field is however difficult for historians. Where is the field of the 19th century? How does one access it? The answer for most historians lies in the archives. Although I had not used the term 'fieldwork', I located my field within the documents left behind and preserved by colonial states. My original plan had been to augment the colonial archives I was using in London (the India Office records at the British Library and the Colonial Office records at the National Archives) with archives from centres of indentured labour. I had put together an admittedly over-ambitious plan to visit India, Mauritius, South Africa and potentially Trinidad to piece together archival fragments from around the world. I thought that, despite these archives also being colonial in nature, I would find some voice, some marginalia, some case which would open up access to a different subjectivity. To some extent this is possible. Voices do emerge unintended in the archives. The paranoia of the state ensures the collection of materials produced by dissenters, malcontents, victims and a plethora of other non-state historical actors.

For historians of the British empire for example, the method I originally intended on is the most common form of sketching the path of colonialism, uniting the imperialisms of the colony and the metropole into one analytical framing. It is part of the reason why histories of 'difference' (a generally used but problematic term for histories of race/gender/class/ability) have been so successful. As Joan Scott highlights, it fits comfortably into the disciplinary boundaries of history, 'working according to rules that permit calling old narratives into question when new evidence is discovered' (Scott, 1991, p. 776). In other words, histories of empire which rely on established historical methods operate within the same epistemic economy as histories they are supposedly speaking against.

Again this is hardly new, the violence of the archives is well trodden ground for historical methodologies. For those of us who regularly use archives though, the discussion rarely goes beyond that. There is plentiful work on the violence of the archive, but the majority of this work is focussed on accessing the archive in spite of its violence, on techniques to read through discourses to render the archive useful. There is little thought given to the material harm inherent in the method. As archival studies broadens beyond the historical discipline, with excellent work being conducted out of archives in fields as broad as literature, geography and the natural sciences with a range of qualitative and quantitative methods, the need to assess the method has become ever more paramount (see Lowe, 2015).

While there are multiple reasons to be wary of using the term 'fieldwork', the term itself is closely linked to imperial methodologies constituting a 'here' and a 'there', in some ways it is because of these issues that I found myself reconsidering it. There has been a lot work produced over decades questioning the research methods employed by academics and debating

the nature of the 'field' in fieldwork, particularly the history of scholars obfuscating the knowledge and labour of the people who call the field their home (Lindee, 2005). The problem is, at least in my experience, these discussions have not filtered through into the training of history PhDs and the production of historical knowledge. Thinking of the archive as a field opened up new questions for me. What exactly would constitute the field of the archive? Would it follow lines of communication or institutional lines? Where does the field end? Whose home is the field and how are historians working within it responsible to them?

It took me some time to see clearly the contradictions in the method, particularly for historians concerned with the production of knowledge in empire. While I knew abstractly when devising this original plan of the imperialism of academia, I had not contended with the ways I was feeding into a global system of knowledge extraction. As the pandemic continued to be mapped unevenly, as white countries forgot about the pan of the demic and focussed instead on the epi, I became more and more wary of this model of research. For one it struck me as deeply odd that in order to overcome the coloniality of the metropole I should immerse myself in the coloniality of the colony. I began to think about how extractive research models continue to feed into extractive economies in the ruins of empire. It was through reading Max Liboiron that the concept and the process finally clicked into place for me. In their words, '*extractive economies, including colonial ones, are about taking value from peripheries (where people live) and relocating it to the centre (where power lives), rather than reciprocating the value to its place of origin*'. (Liboiron, 2020, p. 95.)

The knowledge across disciplines has been, and still is, an extractive economy. Universities in imperial centres are bestowed with greater wealth, greater opportunity and enjoy greater recognition. Academic journals are overwhelmingly based out of Europe and North America. Conferences, papers, research talks, books and lectures all rely on English as a de facto world language. Scholars should, and usually do, learn multiple languages to access a greater variety of sources but the knowledge produced from those sources is almost always produced in English. Knowledge is taken from the 'periphery' and translated into use in the 'centre'. The problem lies beyond the mere search for further primary sources and extends into theory. The work of theorists and scholars from the Global South is often treated as giving additional value, not something to seriously trouble the inner workings of what it means to do research. As Eve Tuck has written about extensively, the turn to 'decolonise' disciplines has not led to a true engagement with systems of coloniality, but instead has involved academics reading indigenous writers and indigenous archives 'extractively, for discovery' (Tuck, 2018).

The coloniality of this process, of researchers based predominantly in the Global North undertaking 'fieldwork' to mine for 'data' in the Global South for intellectual work produced for consumption in Europe and North America, was reinforced when the COVID vaccine roll out began in late 2020. I watched as academics in the Global North exploited their access to early vaccination to once again begin research trips in the Global South. For bell hooks, 'to travel is to encounter the terrorising force of white supremacy' (hooks, 1997, pp. 343–344). The implications of this statement took on a sudden new dimension for me as researchers from the UK resumed work in India, South Africa, Mauritius and other locations that I had intended to travel to all while these countries remained on returning red lists for anyone who is not a British citizen. Even once lockdowns within the UK had eased, racialised notions of who carries disease ensured the continuance of a border policy that was quick to contract in some areas and yet remained conspicuously open in others as new variants emerged. When in 2021 the Omicron variant emerged, the UK moved to ban entry from southern African nations, including countries which at that point

had no confirmed cases of the variant. In contrast, borders were kept open across the global North for European and North American nations, even for countries like the UK with confirmed Omicron cases.

The framing of COVID as a point of rupture ignores systems of racial capitalism that were strengthened during the pandemic (Papamichail, 2023). In the UK, 'controlling the virus' became a slogan to enforce stricter border controls and bring more elements of society into the remit of the Home Office. Under the guise of emergency planning in 2020, the Home Office began interning asylum seekers in the Napier army barracks in Kent, using the language of quarantine to enact stricter lock down protocols on migrants than on citizens even as the inhumane conditions of the barracks allowed COVID to run amok. Such measures were supported by a quelling of dissent, as the spectre of the virus and ever changing 'COVID-guidelines' were weaponised to shut down in person protests.

The tightening of border control methods during the pandemic produced a strengthening of the categories of 'citizen' and 'migrant' in Europe in a way that profoundly affected the way research could be conducted. Opportunities to vaccinate everyone across the globe (and thereby make everyone safer) were lost as intellectual property was reinforced and the vaccines, produced largely through publicly funded research, were financialised for private profit (Storeng et al., 2021). Rich nations in the Global North used advance purchase agreements to secure a far larger supply of vaccine than they needed, around 70% of the doses of the five most promising vaccines in 2021 according to Wouters et al. (2021). My project considers the violence of immigration and emigration legal systems and the fracturing nature of citizenship. What I saw in that first year after vaccination roll outs started in Europe was such a clear enactment of Du Bois's global colour line that the racial capitalism of the research economy became all too clear (Quisumbing King, 2022).

Research as extraction is of course not simply a metaphorical concern. Kanagasabai's (2023) concept of the 'forever fields' in research, places which are continually framed as sources of data rather than producers of knowledge, demonstrates how funding opportunities, visa regimes and disciplinary methods alongside the neoliberal university demands that researchers attempt to locate themselves within an accessible field without questioning the material realities afforded to researchers that makes fields accessible. The uneven mapping of the COVID vaccination programmes added yet one more element to make the field 'accessible' for some and ever distant for others.

Every year researchers with access to economic and academic capital in the Global North establish their careers through fieldwork and attendance at international conferences, often involving high rates of intensive travel. This is in spite of the many implications and warnings of the climate emergency. In the time since I started my PhD there have been catastrophic oil spillages off the coasts of Trinidad (Johnson, 2021) and Mauritius (Sandooyea et al., 2021). In 2014 Vunidogoloa became the first village in Fiji to have to be relocated as sea levels rise (Lyons, 2022). According to some projections, if greenhouse gas emissions continue at their current rate, Georgetown is expected to be underwater by 2030 (Strauss & Kulp, 2018). It is not news that global warming is disproportionately affecting the Global South even as it is disproportionately produced by the Global North and yet the idea that academics *need* to travel is still paramount. The brief lull in international travel from Global North universities throughout the pandemic has come to an end. Conferences have already started to spring back to in person, requiring people to fly from around the globe to attend. The small gains we made in the vital digital access for Disabled scholars during the pandemic have already started to wither, to the detriment of speakers, participants, the planet and the circulation of knowledge.

As Bautista-Puig et al. (2022) highlight, Higher Education Institutes are increasingly likely to sign declarations on sustainable development and yet have been slow to actively implement sustainable practices. In spite of the growing emphasis on ethics in PhD research, the idea of 'do-no-harm' is rarely extended onto ecosystems in the context of the humanities and social-sciences. Lawlor and Morley (2017) show that ethical codes for research rarely go beyond the bare minimum legal requirements and, as such, fall short of the complexities of ethical responsibility in the face environmental crisis. This certainly extends into the humanities. There are of course debates to be had on the relative ethics of climate change and the idea of individual responsibility, but the issue here is not individual responsibility, it is profession. Academics are as a rule not good at thinking of their research on a professional scale (Hayward, 2012). The question for the researcher should go beyond the idea of the carbon footprint of a particular research project and should bring into focus how the academy in the Global North is complicit in neo-colonial research that contributes to environmental breakdown.

These two major issues affecting academic research, the rapidly encroaching climate catastrophe and the realities of visa regimes enhanced through COVID, should be matters for research ethics, and thus issues which every researcher must think through. For those of us engaged in archival methodologies across the disciplines, thinking ethically around these topics requires us to actively view the archive as a field and therefore consider not just the violence inherent in the archive, but the violence in the method of accessing in the archive. To put it in Kanagasabai's (2023) terms, we have to consider how the archive is made accessible to us and how this 'accessibility' is both mapped unevenly between researchers and how our 'access' contributes to systems of colonially produced climate change.

To think of archival research as 'fieldwork' is to situate yourself within a growing interdisciplinary literature that aims to denaturalise the field and academics access to it, to insist on viewing the field as peopled which therefore must be understood through histories of relationality between academics and communities (Simpson, 2014). This demands us to rethink our ethics, to bring more into focus the question of to whom is the researcher is responsible?

In reading about the 'field', I decided to undo my work and consciously choose to work with reciprocity (Liboiron, 2021). So when travel opened up as a possibility once more, I made the decision to remain in the UK and sought to understand the archives I was working with in London more concretely as a field. While this decision mollified some of the concerns I had with conducting research in the middle of a pandemic, it left me with another major problem. How is it possible to write a history of the British empire, entirely from London? Where and how could I put this field in context?

Autoethnography

Restricted to London, the 'field' I was working in changed. Dent's (2022) question of whose home is the field suddenly inversed as my home became my field. I sought to understand what it means to write a history of empire from London, which looms so large in the background of so many histories of this type and yet rarely comes into direct focus. To understand the implications of this for my research and my thinking, I turned first to myself, to unpick the uneasiness I had felt at the research I was doing and to think of myself relationally. I thought that if history is the fruit of power, in the words of Trouillot (1995), then the start of detangling my own connections to power systems lay in the uprooting of my own past and present, my own particular history. I turned to autoethnography as a means of understanding myself in the context of my research and my research in the context of myself.

As I sketched my own entanglements with power, both historically and contemporaneously, I started to think about what had led me towards the PhD. I was 17 when I left my home in Scotland and travelled to India for the first time. It was, as the cliché implies, a revelatory experience for a white person with very little experience outside of my hometown. I had already been accepted into university before travelling but half way through that year I wrote to change my degree from BA History and Politics to BA Hindi and History.

The combination of these two movements, one a physical journey and the other a journey between academic disciplines, became crucial to sense of self. They allowed me to define myself differently from the whiteness in which I was raised. I thought of myself as different to other British visitors to India as I worked and lived in small village, not travelling in which 'India' becomes a mere backdrop. This continued when I was in university. In my year abroad in Jaipur I learnt Hindi alongside several white people from the USA who were learning the language to boost their employability in the State Department. There too I thought of myself as different, interested in Hindi not as a language of utility but as a language of literature. As I look back now I realise how much of this was bound up in a particular defensiveness of my own whiteness – yes I am white, but I am not that white person I thought.

I had in short always defined myself in the negative. I was this because I was not that, but I never really thought to look too deeply at what I meant by 'this'. This is course exceptionally hollow, but it is a pattern that is remarkable prevalent in academia. I found this type of thinking replicated in the structure of the PhD itself. As students move through the conditioning of an undergraduate to a postgraduate, academic writing is defined by thinking in negative spaces. PhD proposals, which demand that we assert the novelty of our work while acknowledging the work that has already been done, inspire a focus on the critical, in digging holes in the literature to establish ourselves as different. I research like this because I do not research like that.

In turning to autoethnography, I attempted to undo this thinking in myself. Autoethnography constitutes both a 'process and a product' (Ellis et al., 2011, p. 273). At its roots it is concerned with the problematising of experience as a historically constructed modality (Scott, 1991). Autoethnography has been used across fields to bring the processes of research into tension, bringing researcher and reader into an analytic frame. It allows for research to be 'witnessed', bringing the power structures that support it into focus and giving space for author, subject and reader to read the text in differing theoretical frames (Denzin, 2006). Carolyn Ellis and Arthur Bochner argue that the benefits of autoethnography lies in understanding 'some aspect of a life lived in a cultural context' (Ellis & Bochner, 2000, p. 742) thus allowing the elucidation of that cultural context through the personal. Adams and Holman Jones (2008) extend this argument to claim it as a fundamentally queer methodology, which Holman Jones (2018) has used to understand activism as a way of life and queer subjecthood. Autoethnography as a method is primarily associated with the social-sciences, however there have been excellent forays into it within history. Carby (2019) has used it to speak to the intimacies of empire in Britain and Jamaica through familial memory and pushes it to explore more widely history of growing up Black and British, using it to unstable concepts of home and belonging.

I found that autoethnography, to think in Ellis's terms (Ellis et al., 2011), had a more profound effect on the processes of my research than the product of my PhD. My experiments with autoethnography were both retrospective and contemporary, stretching backwards through time in order to ruminate on the present. The process of understanding myself autoethnographically occasioned a shift in my thinking, by making myself an active participant in my own work. To engage with autoethnography is a fundamentally relational process

precisely because 'the self is porous, leaking to the other without due ethical consideration' (Tolich, 2010, p. 1608). The result of this was an expansion of my own ideas of ethics. This had an unusual effect on my mental health. It brought me into closer contact with my work, blurring the boundaries of labour and the self as I sought to recover and politicise my memories, but it also demanded that I think of myself ethically (Edwards, 2021). If I was a participant in my research, then I had to be careful of the questions I was asking myself, allow myself time to recover and consider what it means to consent to my own research. The situations I was thinking through with autoethnography were not happening in some separately located sphere of 'research' where my PhD resided, they were happening in my own life, an unending introspection. This demanded that I view the process of my work ethnographically, not simply looking at the method with which I approached documents, but the situatedness of my work within the material world and of my own embodied practice.

The autoethnographic process held some uncomfortable realisations for me. By understanding the archives I was working in into 'fields' and thinking of myself relationally, I began to see that in confining myself to London, I had unwittingly put myself in the position of the colonial officials whose records I was accessing in the archives. I read a secretary in London attempting to understand colonial migration systems at the turn of the 20th century and I saw my own confusion. I found myself writing about and inhabiting the lives of the people I said I would never write about, powerful white men.

The fundamental outcome of my experiment with autoethnography therefore was to understand the relational construction of myself – something I was disturbed to find located not only in the queer and postcolonial theorists I read but in these colonial officers. I had known prior to this of course that to write about the empire is to situate yourself in relation with colonialism in some way, but the proximity of the relation surprised me. In attempting to understand movement, enforced colonial movement, through my own stasis, I came face to face with my own whiteness, the unspoken and often looming presence that had made me so uncomfortable with my PhD when I first started. I had thought that a reflexive autoethnographic practice would be a means of interrogating my own epistemology (Ackerly and True, 2006) and as such change the nature of my research to being 'with' rather than 'on' my research subject. Instead I found that the coloniality I had been attempting to escape was in fact the true subject of my research, the whiteness that loomed in the background was in fact the focus.

While I wrote many thousands of words, little of it will end up in my final PhD, for reasons both ethical and tonal. The process of autoethnography had brought me into more emotional contact with my work, it had demanded that I consider myself ethically and it had brought questions of whiteness and research into new relief. The product however brought with it new ethical questions. Ethical guidelines for autoethnographies are relatively new and, as Wall (2016) contends, there is a frequent lack of awareness of ethics or evidence of applied ethical principles in published work. The relationality of the self speaks to its inherent co-constitution, making it impossible to conduct auto-ethnography without implicating others in our stories (Roth, 2008). Unlike community engaged methodologies however, the responsibilities of the author to those mentioned in their stories are not always clear. I came against questions of how to go about the process of gathering informed consent from those mentioned in my autoethnography – at what point in inviting others to comment on the constructions of the self does the 'auto' vanish?

The second, and perhaps the most pertinent objection I have to including my autoethnographic work in my PhD, was that the product often read as self-indulgent and inappropriate. As Enloe states when thinking about reflexivity in research, 'the real discomfort comes when

trying to draw the line between reflexive candour and unwitting self-absorption' (Enloe, 2016, p. 258). Gani and Khan (2024) go further in their critiques, particularly regarding positionality statements, arguing that reflexive practices not only do nothing to change the colonial relationships at the heart of research, but that they actually reinscribe hierarchies through legitimising the role of the researcher. Although Gani and Khan specifically omit autoethnography from their critiques, I still find it valid.

In the written product of my autoethnographic experiment, the whiteness which I found and which I read in tandem with the whiteness of the archive was being unequivocally centred. In some ways this was understandable and unavoidable; my subject is the British empire, whiteness should be a topic of focus. The introspection I gained from autoethnography however seemed insufficient. It seemed to me that as a product it could only really be beneficial when viewed alongside multiple autoethnographies, to gain a multitude of voices and a complex of standpoints. I came to realise that if positionality were to truly be the starting point of my research, I had to contend with the work my research would do in the world. Not in terms of impact as narrowly defined by research councils, but how my research ratified certain notions around what scholarship is, what it is for and who it is built by.

Communal story telling

The shift to thinking of myself as a participant in my own research caused me to dwell further on the distinction between researcher and researched established in fieldwork. Fieldwork is ill defined, but generally evokes a sociological/anthropological framework. Fieldwork is qualitative, often interview based, intensive (both in time and effort) and, most importantly, fieldwork is located outside of the 'normal' realm of the academy. The field therefore, is generally denoted as 'elsewhere', whether that be in a different country or a different community that exists in a different cultural context from the university. While there are a great many interesting and exciting takes on the researcher as inside/outside the research dynamic and attempts to undo this binary, the fact that these issues are often controversial speaks volumes to the supposed 'objectivity' of research. By naming the research element of a PhD as 'fieldwork' there is the implication of a certain distance between who researches and what is researched.

This 'distance' is enforced in many of the materials produced around doing fieldwork in doctoral study. There are a plethora of articles online, ranging news reports on the need to protect students on fieldwork to self-help forums where students report on their own 'field' experiences. The tone of these writings remains curiously positivist. The authors recount stories of the 'field' wryly, the tone veering towards the farcical and amusing, before signalling to some larger point of supposed importance to PhD researchers. Jourdane's (2017) illustrated collection of 'Fieldwork Fails', for example, tells the story of being detained by NATO security with the 'affective mode' of 'the amused chuckle' (Victorian Studies for the 21st Century, 2015). Stories like Jourdane's reify the field as strange and unfamiliar, the lives of people who live in the field are mere peculiarities, amusing anecdotes about the PhD experience.

Such stories betray the particular imagined community to whom the author is writing, one which their research subjects can never be included within. I doubt any of these writers really considered that the people they were speaking with, the imprisoned young person for example, were their readership. The analysis in these writings always comes after the story of the field. The location, and the people who live within it and call it home, serve as vignettes, indicative and yet unaware of larger theoretical connections. The geography of this knowledge production, that data is collected there and analysed here, is in turn mapped onto the bodies of the researcher and the researched. Thus even in situations where the researcher thinks of

themselves as 'inside' the community they are researching, the process of turning people into research subjects and into researchers retains this geography.

The distinction between researcher and research subject lies at the heart of many academic disciplines and, as Narayan (1993) notes, it is a binary that emerged from the colonial origins of 'disciplines' as universities sought to distinguish between 'native' researchers (usually termed informants) and 'real' researchers. This dynamic, as Schmeidl (2024) highlights, leads to an implicit expectation for 'locals' to tell stories and for the researcher to interpret these stories, belying the processes of collective sense making and the co-production of theory. Even within participatory research methods, although it is often used in the social sciences as a panacea for all forms of colonial knowledge production, the position of the researcher is often reified and can lead to an entrenchment of power inequalities (Darroch & Giles, 2014). To borrow from the framing of Gani and Khan (2024), many such attempts at reflexivity in research do little but to legitimise the particular academic enacting them. The fundamental logic of these exchanges remains capitalist. It is at its core, transactional. Participants may be rewarded with time, research and even occasionally money but they are kept at bay from being considered a genuine contributor. A co-author of equal standing. The credit, the acknowledgement of work and the cultural capital of production all lie firmly with the researcher. Even within participatory research methods the relationship remains one of extraction, although it does open the opportunity for participants to extract resources or policy shifts from researchers (Lenette, 2022).

There is an element of this which plays out within the academy as well as in its relations to the broader world. As Mbembe (2016, p. 38) asserts, there is a 'global Apartheid in Higher Education' which continually ignores and devalues thinkers and epistemologies from the Global South. There is a current of exchange which undercuts all of academic work, but it is often reserved to those at institutions with historic cultural and economic capital in the Global North. Researchers work together to produce theories, think reciprocally with ideas and come together in conferences to collectively further their knowledge. The very idea of a PhD should be an exchange, built on the supervisory relationship which over the years extends outwards. It was this economy of exchange that actually made me want to do a PhD in the first place.

If economies can loosely be thought of as regimes of value, I started to consider what was being valued and devalued in this closed current of exchange. Who was absent in the theoretical workings, the conversations, the questions and the audience? Todd's (2016, n.p.) understanding of reciprocity is vital to my thinking on this particularly the need to pay attention to 'who else is speaking alongside us'.

In an effort to break down the barriers between researcher and participant, I mapped out the places where discussion on indentured labour were occurring. I contacted doctoral schools, archivists and established academics in Fiji, the Caribbean, South Africa and India. The response rate I received was, unsurprisingly, low. I then turned my attention to those speaking outside of traditional paths of research and, most importantly, to community groups established to confront the detritus of the past, where I received a much higher response rate. Some of these groups were well established institutions, others were a single person running an Instagram page and others still were undertaking the complicated task of writing family histories. With some of these groups and individuals I have only ever managed to establish a conversation between two points, myself and them. With others we have created broad networks of communication between several colleagues. The idea in these conversations was to work together to imagine the past, in the spirit of mutual exchange. If autoethnography had taught me to consider myself as a participant as well as a researcher, it seemed only right that I consider the people I

spoke with not just as participants but as researchers and theorists. As such we work together as a group of theorists and historians, all of whom have a different relationship to the idea of history and scholarship.

I refer to the method we worked through together as communal storytelling, a term I have borrowed from the theories of Ono-George (2019) and from the theatre (Wood, 2022), but the central tenants of the methodology have older roots. Storytelling as method has a long history which is multi-centred and cross-cultural, but it has recently sprung to attention in scholarship. The way we tell stories, how we categorise them, the tone we use, the applicability and relevance we put on them and the manner in which they are received are all pivotal theoretical expressions (Escalas & Stern, 2003). Perhaps the oldest genre of understanding stories within contemporary academic literature is through narrative analysis, the attempt to produce 'paradigmatic' categories through structure, theme, plot and character (Polkinghorne, 1995). Such structuralist formations attempt to see through the embellishment and exaggeration inherent in story making, but in doing so they lose meaning. Such experiments with the concept of truth are precisely what make stories an important methodological tool. The ability for a story to be elastic and adaptable creates space for the storyteller to interpret, to imbue the story with their own theoretical utterances (Tatar, 2004).

At first this seems similar to life-story methodologies utilised in social sciences and oral histories. There is however a distinction. So-called participants come to sessions with an idea of what history is, what is important, what needs to be sifted and what needs to be unpacked. However, oral history training tells us to try to avoid these, to probe deeper, to ignore the narrative in order to get to something more 'authentic'. I decided instead to look at these not as problems to work around but instead to consider them theoretical utterances. I acknowledged that the so-called 'participants' I was speaking with were co-thinkers, co-creators, co-conspirators and co-authors of this project. Simply put as colleagues.

Communal storytelling techniques are most commonly found in the intersection of disciplines and, most particularly, in 'public engagements' that arise from research. The playwrights Alexandra Wood, Sonali Bhattacharyya and Gurpreet Kaur Bhatti alongside the historian Kavita Puri utilised a communal storytelling approach to create the play SILENCE (2022), a multi-vocal and expansive re-telling of Partition. The finished product represents a collaboration between historians, playwrights, archival materials and oral history testimonials and reflects the often chaotic and incoherent stories of the brutalities of Partition.

There are however considerable ethical considerations to take into account with this method, and a substantial criticism of communal techniques levied centre about the incommensurability of co-creation with the university. Contemporary academia and PhD programmes as an extension operate on the same self-maximising logics as neo-liberal capitalism. Jobs are scarce and so we are constantly producing, not to make any real meaningful contribution but to make sure our names are published. To put something on our CV. To 'publish or perish'. This ideology is ingrained in PhD students early. We are pushed to publish, to give conference papers, to look for ways to make our research relevant and to constantly build our profiles as researchers. It is one of the few ways to accrue value in the economy of higher education. We are at once traders and traded, creator and product. To acknowledge the people you work with as co-authors means fundamentally giving up some of your ability to build an academic profile. You have to admit that you are doing nothing new, unique nor ground-breaking. However, in the end, I am writing for the purpose of a PhD, something I cannot share or co-authorise.

My sessions with my colleagues also highlighted ethical considerations I had not taken into account and which apply to research beyond the method of communal storytelling.

As I continued to speak with these community groups and individuals interested in the telling of history, I encountered a deep sense of frustration with the past. Many people I spoke to were annoyed with the decaying state of archives, the obfuscations of colonial documents and the desire of previous generations to not speak about the realities of indenture. Others, however, were more cautious. One colleague spoke to me at length about the complicated feelings they had about utilising their own family in their histories, particularly their women ancestors. They questioned what right they had to reveal secrets about these women, secrets that often they had kept closely for their whole lives.

The PhD, and all academic scholarship really, hinges itself on legacies and metaphors of enlightenment, of bringing into light, of shining a new light. Where new subjects enter into the academic gaze, the question often centres around the idea of representation (or re-presentation), with good scholarship tending towards the former while negating the later (Spivak, 1988). To render someone, particularly an oppressed or colonised person, 'visible' is to undo colonial domination. But this visibility within the history of the university has always meant institutional visibility, colonial and neoliberal, and we cannot produce scholarship in ignorance of what institutional visibility can mean.

Rubis and Theriault (2020) highlight how indigenous communities selectively engage and conceal from conservation organisations as a means of survivance, a topic we discuss frequently in our sessions. For indentured labourers, when documentation of their lives was so closely tied to their own domination and surveillance, surely the ability to withhold information would have been precious. Simpson (2014, p. 107.) writes compelling on how the act of refusal produces its own meaning, 'a stance, a principle, a historical narrative and *an enjoyment in the reveal*'. In order for there to be an enjoyment in the reveal, communities need the power to speak on their own terms and their own timing, and certainly not within the timeline of a four year PhD.

Through communal storytelling, my colleagues and I created a different interpretation for the silences of the past and the meaning of scholarship. 'Silences' are often defined as an absence – a space in which there is no voice and thus a space which needs to be excavated. As one colleague highlighted however, there is violence in silence, but there is safety, anonymity and power in hiding. Archives are in many ways totalising places in which colonised peoples appear as re-presentations of themselves, distorted both through the documenting process and how they become 'known' to the state (Spivak, 1988). In rushing to fill the silences of the archives, historians should be wary that they are not themselves complicit in this process of abstraction. In this reframing, silence is not a void to be filled but a place to honour the subjectivities of the past and a place to consider alternative futurities. In one session, a colleague and I discussed an ancestors reluctance to speak about indenture even as evidence of their labour existed in the archive. Instead of attempting to commensurate these two things, of supplementing the archive with the snippets that they had discussed with their family, we chose instead to think more clearly about the act to not speak and to honour it. Instead we interpreted their ancestor's silence as a refusal to bend their lives to the colonial logics of the archive, of the ways they were known to the state. Together we practice a form of ethnographic refusal, collectively deciding what should remain hidden in scholarship, what does not need to come to light and we aim to create spaces outside of academia where this refusal can become generative.

Conclusion

When I do submit my PhD, I know that it has been defined by the interruption covid thrust upon it. My project is not what it could have been. I would like to think that it is better and more meaningful, but in truth I do not know

for certain. I also cannot pretend that the decisions I have made have not affected me. At the beginning of my PhD I had been looking forward to travelling for research, to leaving the UK for an extended period and the incredible opportunity of having this travel paid for by my funders. I was excited to see friends, particularly in India, who I have not seen since 2015. I am largely at peace with my decision to remain in the UK, it still feels like an ethically just decision, but it still brings me a great deal of sadness, of wondering what could have been.

My reflections on COVID started with the particular problematic of producing history during a time of crisis. I would, however, like to extend outwards and conclude by thinking with the future. Like many PhD students, my own understanding of the potential for change in academic scholarship changed drastically when I was confronted with the realities of the university. The collapse of my project through COVID gave me the chance to rethink. By choosing to not move I made the decision that my project was not somehow beyond the scope of planetary boundaries and material inequalities. In doing so, I have found a new hope in the transformative potential of research. Crises like COVID are catalysts, society is altered or entrenched through them. In the face of the death and destruction of the last few years, we can choose to think otherwise, to imagine a normal in academia that is not predicated on extraction and the myth of individual genius.

The process of autoethnography allowed me to consider myself relationally to the world and thus, ironically considering the narcissistic nature of the product, gave me a path to move beyond the individualism of academia. Communal storytelling has allowed me to stretch outwards, to decentre my own theory and put myself in relation to the world. In both of these methods, the process has proved in many ways more important than the product, forcing me to embody hook's (1994, pp. 59–75) bridging of theory as a form of liberatory practice.

As I now write up my thesis, it is easy to look at the many documents and many thousands of words I have written experimenting with autoethnography and communal story telling as failures. Little of it has ended up in my thesis as data, but this is kind of the point. The colleagues I worked with during my experiments may not feature much in the main text of my thesis, but they proliferate in my footnotes. Ultimately, these experiences have highlighted to me the inconsequential nature of my PhD. By not using much of the information I have collected, I have insisted on maintaining my relationality, not extracting from it. I hope to build upon these relations, these theories and these friendships in whatever comes next. These tools, and most importantly the centring of process over product, can be used across disciplines and, I hope, by stitching together theory relationally can be used to break down the artificial and colonial boundaries of disciplinary thinking.

It has been almost 2 years since I lay in my hammock, discomforted and nervous about myself and the project. I lay there again recently, after a late night call with a colleague based in New Zealand. Our conversation had been wide ranging and unrecorded. We had spoken about violence, family, love, joy and community. We ended our session as we always do by collectively blowing out a candle, sharing our thanks for each other's generosity and our dreams for tomorrow. I have no idea if this will end up in my PhD, I have no idea what we will discuss next. But I do know that the feeling in my chest that the project is wrong has gone, even if our conversations have raised new challenges for me. I do know that our conversations have been cathartic, generative and occasionally problematic for us both.

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