Doctoral Journeys
From Field Diaries to Institutional(ised) Authorship

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The ways in which three doctoral scholars engaged in ethnographic research in differing social worlds are explored here. Accounting for the ethical–political dilemmas engendered by "fieldwork" and the ways in which we grappled with them, this paper reflects upon methodology and questions of power pertaining to disciplinary boundaries, social identities, and researcher–practitioner binaries that have marked key debates within scholarship on the Indian social. This reflection draws from our vantage point as doctoral students, particularly addressing our preparedness for the messiness of field participation and converting field notes into authorial accounts. The arguments in the paper feed into larger conversations around representation in the social sciences. By foregrounding our ethical–moral positions and the institutional spaces (or the lack thereof) to act upon such imperatives, the paper raises important questions about the dilemmas of authoring social worlds.

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This paper is an exploration of ethical and political[1] tensions engendered by institutional(ised) hierarchies in knowledge production. We specifically investigate our own experience of field-based research undertaken as doctoral scholars at an interdisciplinary research institute. We articulate these tensions in conversation with a long history of scholarship from India that has sought to discuss the hierarchies in academic engagements on the social, ranging from the forceful argument of "theoretical Brahmins and empirical Shudras" (Guru 2002) to a more recent piece by doctoral scholars critiquing the normative understanding of "students" as consumers rather than producers of knowledge (Reddy et al 2019); a discussion on questions of social power and hierarchy in academic practices, including doctoral supervision, forms of mentoring, and publication channels; the ethics of theorising the "lived experiences" of others, particularly socially underprivileged others (Sarukkai 2007), and politics, as the power dynamics underlying such theorisations (Satyanarayana 2013).

In this paper, we call to attention what it means to be in the "field," recognising a long history of debates in ethnographic research on ethnography as both field method and interpretive writing (Geertz 1973; Ghodsee 2016; Gupta and Ferguson 1997; Narayan 1993; Srinivas 2002; van Maanen 2011). Such attention is necessary, we believe, at a time when "fieldwork" as a form of academic labour, involving observations and interviews with specific social groups (more often, "studying down") marks the majority of knowledge production within the social sciences. Such fieldwork in the Indian social sciences is performed most often by research associates and doctoral students.

In this context, we believe it is important to explore the ways in which doctoral researchers are not only fieldworkers but also, crucially, authors or writers of the field. As academic scholars committed to producing a thesis and other publications, we "author" social worlds. How do we do this in ways that do not objectify or render our research respondents' articulations as mere "data" for analytical claims by "theorists" outside the field? In writing ourselves into the field encounter, we found partial answers to the challenges of the ethics of writing. However, forms of self-reflexiveness, including writing oneself into the text, created new ethical–political challenges. Several of the challenges that we explore have been reflected upon critically by ethnographic researchers. However, few accounts by doctoral scholars (within India) exist about the ways in

which these challenges are grappled with in the course of doctoral studies.[2]

'Confessional Tales'

By bringing together three different journeys of fieldwork, fieldworkers and writing, this paper engages in a conversation on research methodology to foreground these challenges. The proposed conversation is being contextualised within the larger experiences of Indian doctoral scholars, to explore our preparedness for the messiness of the field and its articulations. Given the large enrolment of PhD students in humanities and social sciences in India (many of whom perform some variety of "field research") as also public lamentations of "declining research quality" (Sharma and Agarwal 2019), we believe it is important to foreground challenges faced by doctoral scholars "working in" and "writing about" the field. We hope to do this by acknowledging both the affective angst of established scholars lamenting the poor quality of "student" work at one end and lack of institutional(ised) support for doctoral scholars at the other.

In some ways, our paper is a "confessional tale" (van Maanen 2011) by which we mean accounts of fieldworkers (rather than the field itself) that accounts for the complexity of ethnographic work. Our account also responds to van Maanen's (2011: 81) sharp argument that most confessions, like most theses, are never published unless the author is "known" and that "authors of unknown studies, while they surely have much to confess, will rarely find an audience who cares to read their confessions."

We write our confessional tales attentive to particular questions that foreground knowledge production, academic hierarchies and ethnographic research as instutional(ised) practices, in a manner that hopes to be more than confessional: (i) What were certain dominant ethical and political tensions in our respective field-based experiences? (ii) How could one attempt to reconcile them? and (iii) What is the role of institutional(ised) mandates in foregrounding and resolving these tensions? We explain these tensions below as arising from our specific locations of socially privileged, urban women researchers and recognise that they are not universal.

Our doctoral projects evolved from professional trajectories and experiences, that led us to individual research problems, which were diverse with no necessary overlap in terms of central foci or disciplinary orientation. Based on deep immersion at the Hampi World Heritage Site, one of us critiqued heritage practices through the interdisciplinary frame of critical conservation studies. Another examined the practices and production of "giftedness" by following children through diverse elite spaces using tools at the interface of the sociology of education and psychology, while the third engaged with life experiences of young women in government welfare hostels in Kalaburagi and Bengaluru, Karnataka, within the ambit of sociology of education. The methodological tools of inquiry, however, overlapped—qualitative and ethnographic—as we were each attempting to "experience" certain lifeworlds and field dynamics. While we did not come to these tools through similar trajectories,

our previous experiences led us to methods that would help us understand "lived experiences" of voices we believed were subdued in scholarly writing, whether resident communities of heritage sites, gifted children, or marginalised young women students.

What overlapped also were questions of social power and privilege in interpretation, as we sought to question, for instance, the established hierarchies of heritage expertise, social construction of giftedness in children, and ideals of academic reflexiveness. We realised that, at a certain level, power and privilege in engaging with the "researched" were inescapable, which led us to debates around the ethics of theorising. We reiterate that the question of ethics arose not just in the process of representing our field through writing, but also through how we entered the field to what we observed and experienced.

Our forays were intended to find and foreground the "voices" of our central protagonists. We recognised that any extraction of the other's voice only occurs through the authorial account and thus already creates an uneven power terrain. Through the course of fieldwork, we also realised that the voices we wished to foreground were enmeshed within sociopolitical structures and were not necessarily extractable. Experiences of resident communities could not be taken apart from the heritage regimes they inhabited, voices of the gifted were enmeshed with expectations of adult-driven social worlds, and the ways in which women students experienced life in social welfare hostels were connected to higher educational practices and social welfare bureaucracy. Acquiring a doctoral degree, we acknowledge, is part of building academic capital. While we set out to foreground voices we believed to be marginalised, through reflection and writing, however, we recognised that the main audience of our writing was not those who lent us their voice. Rather, it was our peer community within academia. This recognition also created several ethical–moral dilemmas, which we address at the end of the paper.

How were we, as field researchers, to account for enmeshed social worlds while retaining our political impulses that had sought to centre certain voices? There were no ready answers, but constant deliberations with each other and peers (as part of a semi-formal collective of doctoral scholars conversing about the challenges of field-based research) helped us realise that ethical–political tensions, even as they were contextual, could be a common ground to help our projects speak to each other—in the course of fieldwork, analyses, and thesis writing—well beyond the limitations imposed by disciplinary mandates.

With this background, through auto-ethnographic accounts of field research, we lay out some dilemmas we faced and our attempts to resolve them. We explain the accounts as three separate narratives, not only to retain the voice of each author but also methodologically account for one's presence while writing the field. While drawing attention to the fact that neither the tensions nor attempted reconciliations were necessarily similar, this style of authoring the paper was a way to bring up issues around authorship of fieldwork data; potentially

a way to facilitate conversations around authorship and method across disciplines.

Fields of Heritage by Rajangam

I largely draw upon my own experiences as a heritage practitioner–scholar, researching the field (field as both constructed and dynamic) of heritage, to discuss emergent tensions between institutional(ised) research ethics and ethics in practice when undertaking field-based research. I commenced doctoral research with the aim to understand if "dissonance" (Tunbridge and Ashworth 1996) understood as contestations and conflicts is inevitable when "official" (Harrison 2010) heritage trajectories intersect with peoples' everyday lives. As a practitioner, I was involved in "community-based work" related to heritage management–conservation, which made me dimly aware of certain tensions between official heritage practice and "resident communities," that is, people who live in and around heritage sites, but had no reason to consider its day-to-day realities. In the course of my doctoral study, based on deep immersion, facing up to the everyday "structural violence" (Gupta 2012) consequent to conservation "for public good" proved wholly destabilising.

These encounters, however, affirmed my resolve "to act," which is when institutional mandates proved to be both a stumbling block and an enabler. While scholarly engagements opened up critical questions, an exclusive focus on the acquisition of a doctoral degree as part of material academic practices, in my case, appeared "institutionalised." That is, institutional mandates, based on a normative understanding of a doctoral thesis as nothing more than an exam and the first step to an academic career, hindered attempts to act in keeping with overlapping professional–personal ethics–politics. Such ethics not only demanded making one's position explicit but also action towards social change informed by critical scholarship. I set down a few instances below and then discuss whether an interdisciplinary lens was useful in attempting reconciliations.

Ethical–political tensions in field research: An early destabilising encounter with the field occurred during a project to study regional craft traditions, when a craftsperson asked me: "Why should I talk to you? What's in it for me?" (personal interview 2011). A seemingly simple question posed to me brought home the salience of researcher privilege and hierarchies of knowledge production. During my (pre-PhD) stint as a consultant, whilst undertaking an impact assessment of new constructions next to a protected monument, a resident asked me: "Madam, tell me is it fair? You and ASI [Archaeological Survey of India] can build as you like but I can't even add a toilet because I happen to live next to a location that was declared a monument?" (personal interview 2012). This struck me as a fair observation from his standpoint.

Can, and if so to what extent, institutional(ised) forms of research prepare (action-oriented) scholars for such encounters? I ask this question because the principal reason I undertook doctoral study was to understand and address dilemmas

engendered by such encounters. Though the result of the process was productive, it was not always positive. Institutional forms of research, particularly feminist scholarship, gave me a whole new set of tools to articulate problems and gain critical insights into field-based research. These mandates, however, proved limiting as I was variously advised: "keep activism for later, get the degree first" or "disengage self from the field and attempt to write more objectively/academically" to a rather sarcastic "only 'new' researchers worry about ethics" (field notes 2016). The first two were intended to be sympathetic while not entirely understanding my reasons to formally engage with scholarship. They were intended to caution me about certain academic practices if I wanted a doctoral degree. The last was more worrisome from an ethical–political stance; do old(er) researchers not worry about ethics then? Do they inevitably become institutional(ised)? As I continued to grapple with such tensions, I was simultaneously attempting to root my work in interdisciplinarity as a way to reconcile frictions. Did the approach help? Both yes and no as I elaborate below.

Reconciling tensions: I begin with a brief background of the disciplinary approaches I commenced with and/or gradually adopted. Broadly, critical heritage and conservation studies suggest that the scope of heritage is ever widening, resulting in increased entanglement with social, cultural and political factors, perforce, making its domain interdisciplinary (Meskell 2011; Smith 2004; Winter 2013a,b). Given the practice's epistemological limitations to "people engagement" (UNESCO 2012), I found sociocultural anthropology and human geography extremely productive for both methodological choices and critical insights into heritage as a contested domain. These included identity, developmental or cultural politics, role of expertise in knowledge/power nexus, and questions of political economy.

Where earlier I might have wondered about the seeming indifference of local people to official heritage, now I was able to recognise it as a consequence of their ongoing alienation from such locales. In this instance, an interdisciplinary approach helped reconcile some divides. It, however, brought up fresh concerns, particularly through the lens of institutional mandates. At a pragmatic level, these included questions of how to root my thesis in a particular body of literature or discipline when the field, through a researcher's construct, is enmeshed. When disciplinary boundaries work more like borders to gatekeep whose knowledge and what forms of knowledge are legitimised, how is one to reconcile tensions which speak to each other beyond such borders? Merely acknowledging one's debt to anthropology for methodological insights or human geography for certain conceptual frameworks seemed counterproductive to a critical understanding of heritage as a reflection of societal structures that can only be comprehended interdisciplinarily.

Lynn Meskell's (2005: 85) argument for archaeological ethnography helped address some dilemmas in writing. She suggests that when archaeologists undertake ethnographic

research, the preliminary training (that is, knowledge of archaeology) is already taken care of. Hence, such work could productively be positioned as an ethnography of "us," that is, of what archaeologists do as practitioners at particular field locations. Thus, while I located my doctoral work in Hampi, Karnataka, the thesis building on a body of literature could become a critique of heritage practice. However, in my case, much as in Meskell's at Kruger National Park, the immediacy of wanting to intervene when one hears compelling accounts of forced evictions in the name of site management–conservation are hard to set aside, given the "insider" knowledge of the field in general and that location in particular. In addition, an archaeologist's casual encounters with field as local people is very different from a practitioner–scholar's deliberate seeking of people engagement as part of ongoing attempts to democratise heritage-making mechanisms.

While conducting interviews, I reached out to a rights-based non-governmental organisation (NGO) working with displaced residents of the Hampi site. When I indicated my willingness to engage with them beyond any academic limitations, the lady replied tersely: "You saying so is all fine but are you willing to take a stance?" I was surprised; surely my offer indicated that I was so willing. She explained: "I have worked with many researchers. I agree they need to understand all sides and are not writing sensational journalistic pieces. But in the end, they back off from making their position clear in the name of academic objectivity" (personal interview). She was unhappy over knowledge being extracted from the field, including their work as a rights-based NGO, while nothing was given back. Having previously faced academic distance (not confusing it with objectivity or non-reflexivity) on many occasions, I understood her frustration. But now, as part of academic structures myself, I had become aware that institutional(ised) scholarship is meant for colleagues within academia and not the "researched"—a divide I was anyway grappling with—again making me question if, and so how much, institutional(ised) approaches can help one to act beyond academic scholarship. What, then, is the role of institutional structures? Is it merely to legitimise one's scholarship and knowledge? How does one move forward in both heritage scholarship and practice in relatively democratic ways? While I navigated dilemmas arising out of "calls to act" by negotiating various frameworks, including academic and civil society's, the significant question is: How can one enable or even think about ways to bridge social change and knowledge production? In my case, they largely operated in distinct social worlds.

Landscape of 'Giftedness' by Mahajan

Envisioned as child-centric, my research sought to provide a layered account of "gifted" children's lived experiences, as they were often seen simply as isolated sites of growth and development within the discipline of developmental psychology (Burman 2001). The term giftedness is mostly used to describe children who show extraordinary abilities, skills, and talent in academic and, sometimes, non-academic arenas. There are close to a hundred contrasting definitions of giftedness (Hany 1993 cited in Laine 2010), despite which psychological perspectives dominate. Initially interested in understanding socio-emotional experiences of gifted children, I commenced fieldwork by following a few gifted children identified by the Indian Research Institute through their Gifted Children Program. As fieldwork progressed, new social actors and roles began to emerge, making the field diffused. What began as an exploration focused on gifted children, situated within their specific sociocultural contexts, soon became a quest to probe ways in which my field understood gifts socially, thus shifting my attention to the practice of giftedness-making. This shift from a psychological to a sociological lens was not a linear process really and developed through much back and forth.

Reading a diffused field: I draw attention to specific ethical–political dilemmas by summarising interactions with two gifted children and their lifeworlds.

Eleven-year-old Rahul belonged to a Telugu Brahmin middle-class[3] family and lived with his parents, sister and grandmother. On deciding to homeschool his children, the father, Sridhar, terminated his employment with a prestigious American software company. I spent long hours observing Rahul in his home environment. While Sridhar was deeply invested in enhancing Rahul's intellectual capabilities, he and his wife were hesitant to acknowledge that Rahul was gifted. Sridhar was focused on organising his son's life as a series of productive engagements and kept expecting me to validate his ideas. He saw me as a psychologist who could help diagnose and address parental concerns. I tried to remain neutral, treading a careful path of taking neither his side nor Rahul's. At times, I intervened so Rahul could voice his opinions. I am unsure if this was because of my psychological training or my moral/ethical compulsion to act or both. Sridhar had the best of intentions for Rahul. He believed it to be his ethical duty to perform forms of parental work, like concertedly cultivate Rahul's potential that would help him forge a close bond with his son (Lareau 2003). As I tried to grasp the middle-class family structure, including class, caste, and parenting practices sociologically, my psychological training would occasionally influence *how* I read the familial dynamic that was deeply invested in their child's "potential"—sometimes, at the cost of his overall well-being. Even as I write this, the seeming compartmentalisation of field experiences makes me uncomfortable as there is no room for fuzziness, and periods of incomprehension that I encountered consequent to reading a diffused field through dual lenses.

While these lenses enabled the reading, they did not entirely account for my individual or social position, which were composed not only of my social locations (caste, class, educational background, and geographical location), but also individual beliefs and ideologies. The challenges of field research included accounting for the "I" in the picture. As a middle-class parent, I often had to shift between being a parent to my five-year-old son, understanding his challenges, negotiating middle-class

norms, and being a researcher who looked at the field as a canvas of social-class dynamics interfacing with educational structures. How does one inhabit an elite social world, as a person and parent, and author a critique of parental practices of the same group? In the quest to offer a social critique, I sometimes wondered if I was being hypocritical just "capturing data" at the cost of those who had permitted me into their worlds.

Using interpretive frameworks that acknowledge any knowledge production as influenced by the researcher, and justifiably so, were there other modes of thinking through the ways I, as a person rooted in my own experiences, made meaning of my field? As a teacher, I had come across children with different ability levels categorised as "learning disabled," "slow learners," "academically bright," and gifted in accordance with psychological framings. While I tried hard to create comfortable learning objectives for all of them, I constantly found myself falling short of options to accommodate those who were either lagging behind or felt unchallenged. Such challenges led to an interest in gifted children. The desire to understand their world led to this doctoral research project and subsequent field experiences compelled me to embrace new lenses in understanding giftedness and ability.

Navodaya Centre for Learning, an elite but inclusive school, which began as an experiment in homeschooling, was one of my field sites. My training in psychology helped me examine peer relationships in the classroom and dynamics between children. Teacher–student interaction in class and larger conflicts concerning school philosophy also became apparent. Many teachers felt Krutika, a 10-year-old from a forward-caste middle-class family, did not exhibit psychologically described characteristics of giftedness: evidence of rapid learning, problem-solving ability, phenomenal memory, to name a few (National Association for Gifted Children nd). Teachers suggested that there were other gifted children in the school, but they had not been recognised as such. Such crucial field experiences moved my research beyond looking at individual experiences to examining the larger social phenomena of giftedness.

However, the sociology of education holds an ambivalent relationship with intelligence quotient and ability, reading them as embroiled within social class, leading to educational inequalities (Nash 2001). I recognise that individual capabilities could be products of social privilege and power, but the pressing question of how one accounts for them at the level of the individual remains unresolved, even as I, the researcher, attempted to make sense of the field through dual lenses. Each time I shifted *how* I read, I needed to change vocabulary. Though this process seems rather straightforward on paper, it had deep emotional and ethical repercussions. Limitations in conveying the conceptual shift to those I wrote about made me introspect on the challenges of making one's analytical tools transparent to respondents. Was I delegitimising Rahul and Krutika's gifts by foregrounding parental cultivation or teacher perceptions? The questions were further complicated by my background in teaching and my position as a researcher belonging to the same social class I was researching.

Partial reconciliations: I have sought to unpack what it means to do multidisciplinary research. I now argue that, alongside the lenses one deploys to make meaning, the researcher and their acquired capacity to read and write the field need accounting for. As this capacity requires the exercise of authorial power on the part of the producer of knowledge, it comes with the responsibility of representing the voices of all social actors who negotiate the phenomena of giftedness on a daily basis. This, then, is my attempt to acknowledge the unequal relations between researcher and field participants in constructing the field. Being an author remains a space of discomfort as respondents' voices come to be *represented* in ways that speak to the writer's analytical choices. Within these choices, marked by authorial power, how does one account for faithful representation of respondent voices? This question gains significance in the face of changing analytical lenses that could not always be communicated on the field. Alongside, I struggled to foreground the voices of children in the instances described above, and neither psychological nor sociological lenses allowed for the centring of their voices.

Welfare Hostels as Educational Sites by Babu

My research involved understanding educational experiences of women students living in social welfare hostels of the Government of Karnataka meant for students from the Scheduled Castes and Scheduled Tribes, whose annual family income is less than ₹2.5 lakh per annum. I spent time in a hostel each in Bengaluru Urban and Kalaburagi districts, focusing on the inhabitants' experiences of entering, living in, and navigating education through the hostel space. As with all ethnographic research, my social identities were always marked, including as a married woman, doctoral student, urban Kannadiga, and Brahmin. My caste identity was particularly significant as state-based caste categories determined hostel admissions.

In some ways, then, I was a rank outsider, working with a group of young women whose social worlds were radically different from my urban middle-class and caste background. Alongside, however, we were all women students albeit in very different institutions. Respondents asking questions about the progress of my doctoral project, including "theory" and writing, meant we inhabited a common, even if hugely uneven, academic structure that enabled my interlocutors to ask questions that might not obtain in other fieldwork situations. By highlighting our limited similarities/engagements, I do not mean to overlook differences; rather it is to indicate that clear binaries are difficult to sustain. Apart from the political question of who holds social power to write about another, differences in social locations have implications for how confidences are built, crucial for understanding how social inclusion policies unfold within the lifeworlds of those for whom they are meant. It was only after I was questioned by potential respondents and my support for reservation policies confirmed that students opened up about hostilities expressed by upper-caste peers and teachers towards "reserved category" students in educational spaces. With

these preliminaries, I offer specific field episodes that posed ethical–political dilemmas.

Field episodes: Regulatory practices were part of hostel routines—students were expected to put biometric impressions on a computer placed in the warden's office and be back by 6 pm. Such mechanisms were tied to material provisioning. Although not strictly implemented, sometimes, in order to pressurise students into marking biometric attendance, the warden insisted that only those who had marked attendance would be provided weekly supplies of eggs and bananas. On one such day, she asked for my assistance in making a list of students who had marked attendance and I felt obliged to help. Although the actual provisioning was haphazard, leading to most students (irrespective of biometric markings) getting their rations that day, this experience made me acutely aware of being part of the power structure. Such silences and complicities created dilemmas, since, as a feminist researcher, I sought egalitarian relationships with young women in the hostel and, yet, recognised that the silences were necessary for continued access to the field and its workings.

One evening, a few male student activists came to the hostel to invite students for a "personality development programme" being held the following day. They asked students questions to make them aware of how caste-based inequalities persisted in urban spaces, although it may mask itself as class. One of the many questions posed was, "how many of your grandfathers are university educated?" This led to loud laughter, gesturing to a shared understanding that such an occurrence was impossible; a point of absurdity. In an attempt to pick one student to respond, the speaker pointed to me. I quietly said my grandfather was indeed university educated but felt the need to clarify that I was not a student but a researcher, in an attempt to substantiate the inequalities alluded to by the speaker. While the lack of education in previous generations was just one of the many questions through which activists sought to pay attention to unequal distribution of resources among different caste groups, it was an instance that brought home how structural realities of our social histories were reflected in the contemporary.

During the programme the following day, I found myself in a strange situation. While the speaker's anti-Brahminical articulations were familiar as part of my anti-caste political commitments, it made some girls uncomfortable. During each break, they would enquire if I was doing okay and emphasise the difference between Brahmins and Brahminism, a slip that sometimes occurred in the speaker's articulations. While these reassurances were comforting gestures of friendship, it was a moment of political unease—in an activity planned by student activists as "politicisation" for girl students, did my presence compel them to balance in some ways structural realities that they were presented (such as disproportionate landownership and employment in the corporate sector by Savarnas) and individual friendships (which I had been able to forge with some of them)? This is not to suggest that one cannot understand social worlds different from one's

own, but to think about how one's presence alters the social world one researches.

In early articulations of these field instances, I believed I had been reflexive in line with critical social research methodologies. In retrospect, I am no longer certain.

Reconsidering reflexivity: Let us reconsider the episodes to raise new questions: When I wrote of ways in which my silences were complicit in constituting the field, was I shifting focus away from the ways in which everyday lives of the research protagonists were monitored and surveilled? In discussing my discomfort at the programme, was I distracting the reader from a moment of transformation, at least for some students? Does this divert attention from the central narrative to the dilemmas of the field researcher/author?

Even as I ask these questions, I recognise that a story can only be told from *a* standpoint, but how much space must the field researcher and eventual author take? This connects closely to the power of the authorial voice. Accepting that authorial power was inescapable, how did I account for it in ways attentive to the politics of representation? I found Bourdieu's (2003) notion of "participant objectivation" a useful authoring strategy. He argues that we do not necessarily need to choose between the necessary fiction of participant observation, and the objectivist "gaze from afar." Calling attention to the conditions that makes research possible, he urges attention to "the social world that has made both the anthropologist and the conscious or unconscious anthropology that she (or he) engages in her anthropological practice—not only her social origins, her position and trajectory in social space, her social and religious memberships and beliefs, gender, age, nationality, etc, but also, and most importantly, her particular position within the microcosm of anthropologists" (2003: 283).

I found an interpretation of social reflexivity, that of objectifying the researcher, useful in reading and authoring the field. The field, as I came to understand it, was a product of social moments of interaction that included disciplinary orientations, class–caste positionalities, and ideologies of both the respondents and researcher. By narrating my efforts at reflexive writing, and questioning my own authorial practice, I hope to have emphasised the political work necessary to create more egalitarian ways to read, make meaning, and author knowledge about different social worlds.

Bringing the Fields Together

Our fields were diverse in terms of physical locations, central actors, disciplinary frameworks, and individual lenses in reading and writing. Yet, they were able to talk to each other on ethical–political tensions, attempted reconciliations, and the role of institutional mandates.

For Rajangam, a heritage practitioner, academic scholarship enabled the recognition of new critical questions, with respect to the expert gaze and community engagements. However, academic practices that emphasised conformity to institutional(ised) mandates foreclosed possibilities of action, creating personal–political dilemmas. Her partial resolution was

to shift the focus from studying "them," the resident communities, to "us," namely heritage practitioners, including herself and the ways that practice impacts the field of heritage. In many instances, though an insider, being seen as part-outsider by her field site, enabled the critique of us, especially given the time-bound nature of institutional fieldwork.

Mahajan's reflections point to challenges in critiquing the practices of the group which one belongs to socially and inhabits, both as a person and parent. Unlike "studying down," as is the case in much of social science fieldwork, where the researcher typically enjoys greater social power than her respondents, in her case, power relations were generally on par. For her, the central tension was an inability to foreground the voices of gifted children, which was her initial research agenda—a tension that speaks volumes about the limits of disciplinary readings, be they sociological or psychological.

Babu was an archetypal outsider, locating herself in a field where she enjoyed greater social power than her respondents. Even though she recognised that the insider–outsider binary might not sustain on all fronts, the question of power and representation remained central to her dilemmas. She attempted to offer a reflexive account of her presence on the field to find tentative resolutions. Eventually, however, she came to question authorial practices of reflexivity, finding "participant objectivation" useful.

Each of us articulated our research journeys as a way to initiate conversations around academic hierarchies and knowledge production. Focusing on certain challenges we grappled with—the classic insider–outsider question of ethnography connected to researcher privilege and academic–practitioner/activist binaries—helped explicate social power in knowledge production. What we bring to these long-running debates is this: certain dilemmas in field-based research can speak to each other across institutionalised divides, with ethics–politics as the interface or common ground. The question we now ask is: Can one resolve such tensions, while accounting for scholarly work as an ongoing interpretive process? All three authors recognised research as context-driven, yet reconciliation proved challenging; in Mahajan's case, several dilemmas remained unresolved, while Rajangam and Babu were provisionally able to attempt partial resolutions.

By articulating certain tensions and their (always partial and contextualised) resolutions, we have attempted to consider both institutionalised research practice and ethics-in-practice, including power dynamics in field engagements and authorship. We do not seek to represent doctoral students as field researchers at large. At the same time, we are cognisant of our social locations as forward-caste, middle-class urban women. We offer our accounts as a tale of how a select set of students from an interdisciplinary research institution navigated the field and the writing of doctoral research to raise questions concerning the preparedness of doctoral scholars for fieldwork and the interpretive act of writing. These questions need attention, particularly because the "doing of fieldwork" is rooted in material practices of funding, institutional locations, academic hierarchies, and publications that are inevitable parts of any institutional academic endeavour. While scholars acknowledge that academic capital is required in being able to occupy institutionalised sites of knowledge production, the general tendency within the social sciences is to frame inquiries around the marginalised other. In many ways, then, our endeavour as doctoral scholars is about acquiring this capital.

However, a common thread across our accounts was that of coming to terms with how to be on the field, not just in terms of research access or methods, but to account for each of us as individuals and researchers, positioned within our specific biographies and personal–political commitments.

Given that critical scholarship often commits to certain social ideals, be it egalitarianism or social change, materialities that include institutional(ised) academic hierarchies acquire greater significance in field engagements. Often, as part of the larger social fabric, universities and academia reproduce the very hierarchies and structures they critique. The explorations of these materialities have led scholars to ask how egalitarian are the social sciences in India? Gopal Guru (2002) revealed that, while social scientists might engage with the concerns of marginalised castes and tribes, Indian social science academia nevertheless, has largely continued to be dominated by the forward castes and fluent speakers of the English language. Guru forcefully argued that the few social scientists who did come from socially marginalised contexts produced empirical data that privileged others used to theorise, sustaining a distinction between theoretical Brahmins and empirical Shudras. Since the publication of the piece, there have been animated debates around the ethics of theorising, particularly of the privileged theorising about the non-privileged (Guru and Sarukkai 2012), with other scholars insisting that the question of representation was not only about ethics but politics, understood as social power whose voice holds social weight (Satyanarayana 2013). While caste might have been the locus of power differentials in much of this discussion, other aspects, including region (Delhi–non Delhi) and seniority, have been explicated in conversations on who becomes empirical Shudras and theoretical Brahmins.

In Reddy et al (2019), a group of sociology-trained doctoral scholars question whether they were producers or simply consumers of knowledge. They discussed how institutional mandates (for instance, University Grants Commission guidelines) required them to produce materially through journal publications. However, in the face of widespread belief in poor-quality student work, they discuss the real challenges of getting published. Yes, there is little writing support in most universities (even if this is gradually changing in a set of relatively elite institutions), and for a large number of students for whom English is not a language of comfort, publishing in English language journals remains a challenge. Given these structural constraints, it is disappointing that, though senior scholars recognise these challenges, they rarely attempt to translate this recognition into pedagogic support structures for doctoral scholars or research associates.

The reflections by doctoral students from the University of Hyderabad struck a chord with many peer researchers as we

could relate to the frustrations of lack of publications, often based on implied understandings of doctoral student work as poor and at the bottom of academic evaluations of rigour. Often, we hear doctoral scholars lament the need of a "big name" for publishing as their scholarship is presumed unsatisfactory and disappointing. While not denying the affective angst behind such articulations, we sought ways to move with and beyond it through this paper, an attempt to explore the possibilities of pushing this conversation beyond antagonism, at both ends.

NOTES

1 By ethical tensions, we allude to protocols of researcher conduct, including disclosing one's presence in the field to observe, analyse, and interpret respondents' day-to-day lives, obtaining consent and respecting participants' rights and autonomy. By political tensions, we acknowledge that all research occurs within a larger social universe infused with power. While we understand all research as political, irrespective of whether it comes to be recognised as such, our projects were intended to be political, foregrounding what we believed were subdued or marginalised voices.

2 A notable exception to this includes a special issue of *Café Dissensus* titled "Writing in Academia" (Dasgupta and Lohokare 2019).

3 The middle class is a complex, layered social category. Here, I use the term to indicate how the family becomes a site of shaping the middle-class child (Kumar 2016; Malik 2019; Nambissan 2010).

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