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Collaborative Anthropologies, Volume 13, Number 2, Spring 2021, pp. 56-99
(Article)

Published by University of Nebraska Press

DOI: <https://doi.org/10.1353/cla.2021.0003>

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The Ethnography of Collaboration

Navigating Power Relationships in Joint Research

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Abstract: We came together to write a paper on the devaluation of field researcher labor as an entry point into the broader domain of research ethics to unpack what collaboration may mean in settings of incommensurable inequality. These motivations were grounded in the materialities of our involvement within an international research project focused on post-earthquake reconstruction processes in Nepal since 2015. However, since we started writing this piece, some of us felt that the paper did not adequately reflect their experiences, others felt it put them in the hot seat too quickly, and some thought that it mimicked the faulty modes of collaboration we wanted to unsettle in the first place. Realizing the power dynamics within our own writing collective, we stepped away from a centralized narrative to make room for our diverse, sometimes complementary, sometimes contradictory experiences. The paper is a bricolage of reflections that focus on issues such as the division of labor, coauthorship, and community engagement. We use these reflections as a way to think critically about the current juncture of transnational, collaborative research and propose a series of open-ended reflections that prompt the problematization of the inequities, tensions, and emotional labor inherent in collaborative work.

Keywords: Research ethics, collaboration, ethnography, reflective writing, research partnerships, Nepal, disaster research

Introduction

This paper dwells in the polyphonous perspectives of researchers attached to a transnational research project based at a Canadian university. “Expertise, Labour, and Mobility in Nepal’s Post-Conflict, Post-Disaster Reconstruction: Construction, Finance, and Law as Domains of Social Transformation” explored Nepal’s post-earthquake reconstruction and state restructuring following the devastating earthquakes in 2015 that brought immeasurable damage to lives, property, and infrastructure.¹ The project’s main objectives included launching a partnership through research on three domains of expertise, collaborating on research design and dissemination through knowledge-sharing workshops, bringing Canada-based Nepali diaspora organizations into dialogue with academics and humanitarian agencies, curating an online research database, and publishing a series of jointly authored papers based on the research findings. These efforts were supported by a Partnership Development Grant, a generous funding scheme from Canada’s Social Sciences and Humanities Research Council (SSHRC) that encourages the development of new and existing partnerships for social science and humanities research and supports the involvement of emerging scholars and students. Keeping these objectives in mind, the project was designed as a transnational, transdisciplinary collaboration between Canadian and Danish universities, a Canada-based Nepali diaspora community group, and local research institutions in Nepal, as well as between over twenty formal project members in Nepal, Canada, Denmark, and the United States, and affected communities in Nepal. The reflections in this paper stem from the notion of “collaboration,” specifically in the context of disaster research, and the multiple complexities that emerged over the course of this three-year project.

Collaborative research is an increasingly common practice within anthropology (see, e.g., Banks et al. 2019; Childs et al. 2017; Menzies and Butler 2019) as well as in many other disciplines. To enhance research impact, a growing number of universities and funding agencies strongly suggest collaboration with actors outside the university. Fluehr-Lobban (2008) claims that collaborative research is not only ethically but principally “better” because it “emphasizes multiple, polyphonic perspectives, which will leave a richer heritage of ethnography to subsequent gener-

ations of ethically conscious researchers” (175). While researchers have long collaborated with their interlocutors during the research process, we note that the outputs are not always coproduced. Similarly, we acknowledge that most forms of collaboration produce richer research findings through a more ethically sound process, but the complexities of such practices are not always made transparent and are therefore worth exploring in detail as we do here.

Taking our point of departure from the contemporary turn towards collaborative research, we consider the ways that the language of collaboration may be co-opted for self-legitimization and placement *outside* the purview of critique. This is not to say that all such efforts are insincere or facetious, but to argue that just because a research endeavor ticks all the constitutive elements (“check boxes”) of some prescriptive arrangement of collaboration, this does not render it outside the realm of ethical scrutiny. In fact, there are many assumptions and underlying arrangements that prop up practices of collaborative research which, when closely examined, can reveal patterns of betrayal and exploitation (see, e.g., Scharff 2013; Ibáñez-Carrasco 2012; Islam 2000). This includes an over-emphasized coherence and egalitarianism of “community,” “community-based organizations,” and “local partners” (see, e.g., Pudup 2008; McCarthy 2005; Joseph 2002), as well as the neoliberal university’s equivocation of community engagement as a means to meet strategic institutional goals, which almost always privileges the university over the community (see, e.g., Bortolin 2011). The ways in which local and Indigenous procedures of consent are often dismissed by institutional ethics review processes are also noteworthy. Discussions on the unintended violence of collaborative research are often suppressed or kept out of public purview and therefore rarely contribute to honest conversations on research ethics (see, e.g., Arieli et al. 2009). These forms of “sanctioned ignorance” (Visweswaran 1994, 98) silence particular types of knowledge in favor of imperial knowledge production and institutionalized ways of thinking.

A manifesto was recently drafted by a group of disaster researchers which, as an aspirational document, calls for the privileging of the “local” (in terms of epistemology, partners, research dissemination, and beyond).² We build on this document by critically examining the power dynamics that shape the “local” and how the “local” can equally be a site of exclusion. Efforts to examine the “local,” particularly in disaster

and development research are typically outward-looking and oriented towards the communities in which the research takes place. This is because research is often imagined as that which takes place *between* the “researcher” and the “researched.” *What about the power relations that exist within research teams who are engaged in collaborative research? What can be learned from their experiences as they navigate the messy contours of collaboration and the unexpected tensions that inevitably emerge between research teams and the communities with whom they work?* It is precisely at this juncture that this paper sits.

As the following reflections will show, we attempt to deconstruct the ethical implications of conducting research in this multiyear collaborative project. We reflect on our experiences and social locations as researchers, students, participants, and colleagues and on the interpersonal dynamics, cross-cultural tensions, and ethical ambiguities that have emerged and continue to emerge. We problematize the hierarchies of expertise between “junior” and “senior” researchers and other overlapping binaries (i.e., “experts” and “field researchers”) and take issue with processes of knowledge production that unfairly rely on the bodies and subjectivities of field researchers and the communities in which they work. Issues such as coauthorship and the division of labor are at the core of these discussions, as are the underlying processes that normalize the separation between field research and writing/thinking. We also problematize the prioritization of English as the language of internal communication and public-facing forums and publications. We point out the unease that arises when research partners in Nepal are expected to act as cultural brokers and when unrecognized labor and invisible structures that form the foundation of ethnographic research as well as academic administration are taken for granted. Together, we question the ethics surrounding collaboration and the shadow that neoliberal institutions cast over the affective dimensions of research.

The paper broadly contributes to literature on ethics in collaborative research and attempts to highlight those dimensions that often go unrecognized and undiscussed. Put differently, we hope that this paper can bring to the forefront the “next generation of methodological and conceptual troubles” pertaining to research ethics (Fine and Weis 1996, 253). Grounding the paper within the materialities and transactions of the very project we are employed in often feels like walking on eggshells: will we offend someone? Will writing this hurt future career prospects? Will this

mean that we will never be invited to another partnership? And worse, will we lose friends or mentors? But after much deliberation and some intragroup tension we have nonetheless decided to take these risks.

The various dilemmas raised in the paper are not unique to the project that brought us together. We hope that readers can also catch glimpses of their experiences within these conversations. In other words, we want to use the particularities of this arrangement to tap into wider discourses of research ethics: to raise questions and open lines of inquiry that exceed the confines of this project alone. At the same time, we also realize that reading this paper may be disorienting, regardless of where readers are in their academic careers. We suggest that disorientation is part of the burden and responsibility of being a researcher. As Jones and Jenkins (2008) remind us: to rethink research collaboration “is both to desire it and to ask troubling questions about it” (471). Thus, despite this paper’s somewhat critical tone, it is written from a place of generosity, and its aim is to not to generate critique for its own sake, to search for “better” research methodologies, or to provide resolutions and remedies to existing dilemmas, but to open honest conversation and dialogue around the shift from more hierarchical models of research to one based on an ethos of equity, transparency, and reciprocity. In doing so, we argue that collaboration does not necessarily equate to a more equitable research arrangement, as we will elaborate on in the proceeding sections, but that a sustained commitment to supporting and valuing joint research requires uncomfortable and, at times, inconclusive dialogue between all members of a partnership team.

Methodology

We, the authors, are attached to the project as graduate and undergraduate students, a postdoctoral fellow, research associates, and tenured faculty. The project commenced with a workshop at the University of British Columbia in Vancouver, Canada in September of 2017, where partnership members from Canada, Denmark, Nepal, and the US came together to generate a research framework and organize the hiring of three research associates through Social Science Baha, a research institute in Kathmandu. We then set out to structure the ethnographic research in three districts of Nepal. The fieldwork unfolded in two phases between March to May 2018 and September to January 2019, with a fo-

cus on households, institutions, and governmental and nongovernmental agencies. The research sought to understand how the professionalization of expertise intersects with daily life as we explored access to housing grants, processes of documentation, perceptions about reconstruction policies and building codes, construction materials, cultural knowledge, and new government structures. The fieldwork was punctuated by a second international workshop held in Kathmandu in 2018, which coincided with short field site visits. The second round of fieldwork concluded with a final international workshop and public forum in Denmark in 2019.

This paper emerged alongside our conversations throughout the project and our direct experiences traveling as a group to two of the three field sites in the summer of 2018. Its origins can be traced to our discomfort during the field site visits. Initially, we carried our discomfort individually, unsure about how much to share, where to share, and whether it even mattered. It was only when we came together in unstructured time (outside of conference calls, workshops, and meetings),³ first during field visits to Nepal and later in between the contours of a writing retreat in Denmark, that our individual unease coalesced into a collective political and ethical voice. This is variously a voice of protest, of speaking truth to power, and of critical self-reflection. Our stance is that of “uncomfortable reflexivity” (Pillow 2003), which, as mentioned earlier, is not about identifying “better” methods, but rather about thinking through how we can hold ourselves accountable to the research process and the communities we research, while recognizing the structural inequalities inherent in North–South relationships and between “junior” and “senior” researchers (see Jeffery 2014). Although power dynamics exist even *within* our collective, we are attempting to write together, nonetheless.

In what follows, we present individual narratives of our personal and professional experiences in relation to the dynamics of collaborative ethnographic research. We tease apart the ethics of cross-cultural, multi-lingual collaborative research, the invisible and labor-intensive administrative work that drives these projects, and the complexities of community-based fieldwork in a post-disaster context. We problematize the internal hierarchies of academic partnerships, which are structurally built into large partnership grants such as this one. Although race and gender are not extensively discussed in this paper, we understand that these and other markers of difference function as mechanisms to regulate

“insiders”/“outsiders” and the “right”/“wrong” type of researcher; they also operate less overtly through everyday interactions and preferences that dictate things like funding opportunities and citational practices. Taken together, our experiences are both unique and familiar; drawing on our varied positionalities, we show how collaboration, on the one hand, exacerbates certain inequalities while, on the other hand, it provides an opportunity to find new and productive ways of working together. We argue that while there are no clear-cut answers to the dilemmas we pose here, there *is* a need to address the messy and uncomfortable nature of collaboration as the foundation for knowledge production, academic or otherwise. We conclude with a series of open-ended points of reflection, based on our experiences working and writing together that we hope will be useful to other researchers grappling with the multitude of ethical issues inherent in collaboration.

Working as a “Junior” Research Associate in a Research Partnership

BINA LIMBU

International research collaboration in Nepal strikes an image of foreigners—usually, white researchers—coming in and collaborating with Nepal-based researchers or research organizations to enter local communities in pursuit of their professional interests. Depending on their level of engagement and expertise, collaboration with Nepal-based counterparts is essential, not only to overcome linguistic barriers but also to navigate different socio-cultural contexts. Partnerships with Nepal-based organizations or researchers also become a way to outsource the time-intensive requirements of research, something that not all researchers want to commit to. Meanwhile, their Nepali counterparts benefit from the influx of research funding, the jobs created, and the networking made possible, which could lead to future collaborations. Such partnerships are common and mutually beneficial for the most part, but matters of credit-sharing are rarely clear, depending entirely on the power dynamics within such collaborations. I was hired as a “research associate” in one such arrangement. Having been a part of one of such collaborative research project for two years, and altogether, worked as a “research associate” for three years, the experience has made me grow and work with many wonderful people. At the same

time, I also found myself in the difficult position of balancing my professional obligations with my ethical sensibilities.

Generally, in my job as a “research associate,” the line between assisting with someone else’s research and doing the actual research is blurred. In past jobs, I had partaken in tasks as diverse as data collection and analysis, literature review, policy briefing, and report writing, yet, authorship credit was never up for discussion for any of the publications that resulted from this work. Often, being given credit depended more on the person’s rapport with those leading the research, rather than one’s contributions in it. These practices, although unethical, are an open secret, a normalized practice that is rarely contested.

What made the project under discussion different was how the role of the “research associates” was clarified at the onset. In 2017, when we were hired for this position, the “senior researchers,” both the internationals (hailing from Canada, Denmark, and US) and their Nepal-based counterparts, welcomed us into the core team. We were not just assisting but also partaking in the research and in doing so, were given joint ownership over the research data and a right to claim authorship over the publications resulting from them. This also meant we would be doing most of the “donkey-work” (as my friend often calls it) such as fieldwork, data collection, transcriptions/translations, as well data analysis and structuring of findings to the point where the tangled mass of data became coherent enough for the “senior researchers” to conceptualize research papers. These are key background responsibilities that we, as “research associates” were hired to carry out. However, the fact that I would also be owning and coauthoring various publications energized my efforts and work hours beyond the job requirements. As the work responsibilities of the “research associates” evolved within the partnership, overtime, we were referred to as “junior researchers” by the project’s senior members.

Who Counts as an Author, and Who Doesn’t?

I hoped that working with more established scholars and coauthoring articles with them would enhance my skills and advance my career—I believe my fellow “junior researchers” also shared similar expectations. However, the issue of coauthorship is a sensitive one. While available in principle, the modalities of actually getting writing credit are not clear.

Despite all the hard work I had put into this project, I was rather uncertain as to how the process of authorship would play out and what kinds of fruits it would bear.

To understand the discrepancies I explore here, we first need to understand the structure of the project. The project consisted of three domains; each domain was led by one “senior researcher” who conceptualized the publications and a “junior researcher” who provided the empirical data to substantiate the publications, with consultation from other team members. Initially, we were told that we would coauthor the respective domain-specific article that we contributed to. After numerous meetings, it was eventually agreed that all the junior researchers should have joint authorship in all publications, owing to their contributions in collecting and refining the primary data for all the research papers. However, this was not evenly understood by the team members. In the end, each domain-specific paper proceeded rather differently given the usual messiness of fieldwork and multi-partner collaboration including changes to team membership and the unpredictable aims of the international partners.

The conceptualization of the papers relied heavily on the senior researchers, giving them the power to have the final say regarding what version of the empirical data befits the narrative of the paper. Having this power also means having a responsibility to do justice to the data and fair recognition to those who generated it, but the degree to which a “senior researcher” complied with this responsibility differed according to their interests, biases, sense of ethics, and sheer generosity. For example, in the writing process of one paper, all the researchers were consulted on the theme of the paper, invited to participate in the writing process, and the authorship was openly discussed and shared. Meanwhile, in the case of another paper, the lead authors worked through private emails, using the collective data but not sharing the initial drafts nor discussing the authorship openly within the team. Being in the position of a junior researcher, I was deeply uncomfortable with this, but did not have the courage to speak up for fear of offending a senior member, or worse, being labelled as someone who is “difficult” to work with. I suppose similar fears existed in other team members who chose to ignore the elephant in the room so as not to create discord within the team.

Now that my contract has come to an end, I am happy that we managed to copublish two working papers authored by the researchers based

in Nepal. Meanwhile, other papers are in the process of being refined. However, I find myself wondering about the future outputs of this project. Being the ones who actively engaged with the communities in Nepal, there are numerous intricacies of the data that are only familiar to us. I wonder if my contributions will also be credited in the future, and if I will continue to be consulted, now that my formal contract is over. And if I am, will my fellow “junior” colleagues receive the same treatment? But, *as long as the output is useful, does it matter whose labor it was based on? Does it matter who found an interesting linkage between issue A and issue B? Who agreed to write the most difficult section? Or, who painstakingly went through six-hundred pages of field notes to find that one quote that paired so well with a given narrative?* These questions, I feel, are difficult to answer.

Although authorship issues have now been addressed to some extent, and everyone is onboard with the inclusion of the research team as co-authors, I wonder what would have happened if these issues were left unchecked? Whose responsibility is it to ensure that authorship issues are discussed and agreed upon? Had we not openly discussed the details of authorship for each paper, would I and my fellow junior members have been given fair recognition?

“Pots Tend to Clanker”: Conformism and Hierarchies

As I noted earlier, the working modality between “senior” and “junior” researchers is such that, after working together to design the research instruments, the “junior researchers” supplied empirical findings and based on this, the “senior researchers” framed conceptual arguments. However, the research findings are rarely absolute or clear-cut, and are always subjectively interpreted to one’s preferences. These interpretations become inconsistent when one does not have the first-hand field experience of the ground-level intricacies. Hence, when the interpretations of a senior researcher began to stray from the community’s voice or the reality that we, as field researchers, encountered on the ground, a lingering feeling of unease nagged my sense of ethics.

This became particularly clear when in the summer of 2018 a group of the international project members (approximately ten) accompanied us to the field sites for some two days. This trip was supposed to be an opportunity for the international members who had never visited the

field sites to get a sense of the community. However, some took it as a fast-track data collection opportunity and started interviewing community members without clarifying their vested interests in these conversations. Such exchanges did not align with the parameters of this project that required informed consent of the interlocutors. The authenticity of the experiences being shared was also questionable, as the community members who conversed with the international members perceived them more as tourists than as researchers. This also drew unnecessary attention and put “junior researchers” in an uncomfortable position, as we were perceived as the mediators bringing in the foreigners. In another instance, I also noticed that some community members who were less interested in talking to Nepali researchers in the previous field visits were more willing to share their plight, even exaggerate, to the foreigners, in hopes of benefitting from these exchanges, either materially or financially—a trend set by foreign officials from numerous INGOs that provided in-kind and cash relief to the people after the 2015 earthquakes. These intricacies were more apparent to some of the “senior researchers” who had been working in Nepal for a long time, but less so to others who seemed less aware of how they were being perceived by the community. However, as a junior researcher, I was confused as to how much I could or could not intervene in the knowledge-seeking process of the senior partners who had more experience, authority, and status than myself.

Within our Nepal-based team also, many differences have arisen among the members. As “pots tend to clanker,” it is normal to have disagreements and a sense of competition among the coworkers. Adding to this equation, each one of us is motivated to gain connections and more so, to impress our senior colleagues. I admit, I am no different. Yet there is a limit to how often you can say “yes” to please someone without feeling that the very nature and substance of the research findings become too bogged down in the politics of research. Given our “junior” roles within the project, it is difficult to disagree with the “senior researchers.” Things get increasingly tricky when there is a need to maintain balance between conforming to the “senior researchers’” preconceived ideas and our own personal perceptions. I say “personal perceptions” because the understanding of the field sites and research data tends to vary among us junior researchers as well. Disagreements frequently occurred as we gleaned differing insights from the same data. After many rounds of diplomatic arguments and efforts to find a middle ground, I feel we have

overcome some of the hurdles I have described above, while others are left open-ended.

Ethical Issues among Disaster Researchers

MANOJ SUJI

The earthquakes in Nepal destroyed people's homes, took away their sources of livelihood, and strained the infrastructure of the country, crippling everyday life and intensifying political instability. As the process of rebuilding began in 2015, those who were affected voluntarily shared their stories and experiences to I/NGO workers, university students, and related professionals. In highly affected districts, people spent hours responding to questions hoping they would receive assistance in exchange for their stories. During our fieldwork for this project, many participants shared their concerns about previous researchers who collected their stories but never came back nor shared the results of their work. In fact, they did not know who these visitors were, where they came from, or what had been done with the information that was collected. Moreover, relief materials provided by the Nepali Government and I/NGOs were unfairly distributed due to the political linkages and personal networks among I/NGO workers. This left people with large amounts of anxiety and uncertainty, fostering anger, frustration, and animosity towards I/NGO workers and researchers.

I joined the project in February 2017 when one of the original members left the team. I took over his responsibilities as a Nepal-based field researcher. Before this project, I was involved in several other post-earthquake rehabilitation and reconstruction research efforts. During my previous work, I encountered groups of enumerators deployed by I/NGOs in the field and observed the perceptions of local communities towards these outsiders. In my reflection, I explore how the accumulating frustration and anxiety amongst earthquake-affected communities impacted our collaborative research project and what lessons can be learned for future disaster-research partnerships.

Community Perception and Essence of Ethnographic Work

During our first round of fieldwork, people were busy rebuilding their homes or laying the foundation for new houses, while some were still

living in temporary shelters. The field sites we chose for our research were severely affected by the earthquakes and had become hubs for aid workers and researchers alike. We noticed that many people seemed reluctant to talk with us because they had been inundated with visitors. Community members we spoke to had expectations of receiving support in return. Many people viewed us as NGO workers who were only mining communities for “free” information. In one of our field sites in the Kathmandu Valley, a young man who introduced himself as a ward secretary, ignoring our explanation of the research and its objectives, angrily stated: “What good is this research! You are just wasting people’s time and creating even more problems for us.” His comments did not surprise me as this sense of anger and frustration was a reflection of how communities throughout Nepal have been treated by the influx of data collectors who come to learn about their lives, living conditions and vulnerabilities, while providing little or nothing in return.

As I spoke with interlocuters, the extent to which affected communities had become dependent on I/NGOs for support became clear. When potential participants were told that our research was hosted in Canada and that participation was voluntary, they expressed their frustration and asked: “How will we benefit from your research? Would Nepal materially support foreigners if they were in a vulnerable situation like ours?” Some people asked me to assist them in building a kindergarten for their children who currently have to travel long distances to get to school. They viewed us as donors, and like the other I/NGOs that brought programs and projects to their communities, they wanted us to do the same. Moreover, when we briefly visited these sites with the international members of the project in 2018, people’s expectations were piqued even further. For example, as we were conversing with a schoolteacher during a group field site visit with our foreign partners, a man came up to me and called me a *bideshiko dalal* (broker for foreigners), a trend also noted by Amburgey’s section that follows. A *dalal* is a pimp/agent/broker, in this case implying someone who earns by mediating between local communities and foreigners. In some cases, it is used pejoratively to refer to NGO workers.

Our experiences in the field raised an important ethical question: *how can the research process contribute towards the alleviation of community members’ vulnerabilities?* In collecting so-called lifesaving data following disasters, vulnerabilities of survivors are all too often overlooked. People’s frustration and anger towards us and other researchers hints at the

importance of balancing data collection with efforts to alleviate existing suffering. My field experiences suggest that it is essential to think more purposefully how the project and its outcomes will benefit communities, both in the short-term and long-term, and throughout the research process. This might be a way to minimize research extractivism and improve the lives of our interlocutors. International partnership projects, like ours, might have ample funds for workshops, seminars, and writing retreats, which are essential parts of the research process, but *would it be unethical if some of this money was set aside for communities' immediate post-disaster needs, either at the individual or community level?* Research outcomes may contribute to the researched communities in the long run. Still, even the smallest contributions, when there is great need, can play a significant role in shaping the relationships between the project, researchers, and the communities.⁴

Junior researchers are the ones who bridge the gap between local communities and university partners. As Amburgey's and Aijazi's sections similarly point out, the international team heavily relied on local partners. However, the absence of the majority of project members during the fieldwork process affects the relationships that we, as "junior researchers," have with the people we have come to know personally. We must juggle between the role of representing communities to international partners as "insiders," and describing the project to local communities as "outsiders."

Power of the English Language in Knowledge Production

Generally, English is the primary language of knowledge production, reflecting the colonial arrangements of Western academia (Said 1978). More importantly, proficiency in English gives the advantage to control how findings are presented, which affects the authenticity and legitimacy of information generated by different people in different contexts with different experiences. We acknowledge that, on the one hand, researchers in anthropology and other disciplines often conduct research in the language of the communities with whom they work; on the other hand, the research outcomes of collaborative work are more often published in English. When communities are isolated from or unaware of the produced knowledge, it raises questions about the legitimacy and ownership of the generated knowledge itself. For example, during an in-

formal visit to one of the field sites with my foreign colleague, people raised concerns about the accountability, transparency, and legitimacy of our work, as the communities remained mostly unaware of our project's outcomes mainly due to its inaccessibility and language barriers. In collaborative work that attempts to engage with communities, it is important to share findings with the respective communities and request their feedback on the research outcomes. Moreover, our experiences show that it is essential to publish research findings, in this case, on disaster preparedness, in the local language. Doing so will not only expand the project's influence but can better prepare communities for future disasters.⁵

Unwieldy Collaboration and Ethnographic Presence

EMILY AMBURGEY

As the graduate student grant coordinator for this project, my responsibilities ranged from facilitating communication among the various nodes of the international partnership team and assisting with financial matters on the back-end, to coordinating workshops and field site visits in Nepal. At its core, my position rested on the maintenance of relationships. I reflect on issues stemming from my personal experience and sense of ethical responsibility surrounding hierarchies of knowledge and expertise, and the structural burdens that characterize administrative processes. I ground these discussions within my role as an organizer and participant during a team workshop held in Nepal in 2018.

Terms like “partnership,” “collaboration,” and “capacity building” embedded in the discourse surrounding transnational research imply a certain kind of knowledge production that relies on hierarchies of expertise. Such hierarchies can easily displace forms of knowledge, the intimacy of storytelling, and the affective dimensions of ethnographic research. In the context of this research project, nested power asymmetries facilitated the flow of data from communities and cities in Nepal via a well-known research institution in Kathmandu and eventually to scholars at various international locations. As Suji points out above, the research associates in Nepal were responsible for carrying out interviews, surveys, and participant observations in the three field sites. “Senior” members of the partnership team worked alongside the research team through many late-night Skype calls and other forms of online communication to imple-

ment a research plan and formulate interview questions. I question how the research process is influenced by these multiple layers of expectation and what role these relationships play in the generation of knowledge. In other words, *what is lost and gained through the process of translation via transnational partnerships in lieu of smaller-scale research initiatives? How does the separation between doing ethnography and writing ethnography impact the outcome, longevity, and validity of such work?*

I recall an interaction during our first international workshop held in 2017 before fieldwork commenced. A colleague pointed out the top-heavy nature of the project; there were the numerous participants brought on from outside Nepal (of varying disciplinary and geographical backgrounds), who outnumbered the smaller team from Nepal, who were hired to manage and carry out the research itself. There seemed to be an echo of agreement among the whole partnership team, some nodding of heads and exchange of knowing glances—however, these interactions did not lead to further discussion. The following year, I found myself recounting this scenario as I embarked on a field site visit with many of the same team members to an earthquake-affected area of Sindhupalchowk, Nepal. As we hiked through the steep valley with over ten members of the partnership team, I observed firsthand as the research team from Kathmandu, all of whom had been hired on to the project to conduct the fieldwork, were tasked with leading and mediating interactions between foreign partners and community members. Hoffman and Tarawalley (2014) use the term “cultural brokers” to describe the way field researchers are often mediators between communities and collaborators. Gupta (2014) argues that access to the field depends on the affective and bodily practices of field researchers, and these dynamics intersect with issues of gender, language, ethnicity, age, and class. In other words, research partners are at once expected to be “inside” enough to navigate local social structures and linguistic practices while remaining “outside” enough to maintain a level of professionalism on par with international academics. The liminal space our team occupied during this visit created a tension that was noted by my fellow coauthors and it was this positionality that raised questions central to this paper: *What were we really doing there? What expectations and promises did our presence bring?*

What had been organized as a short visit to the field to observe the effects of the earthquake quickly escalated to a research trip as described

by Balaz-Munn below. I found myself thinking about the countless hours and tedious planning that went into this trip, and despite our best intentions to create a culminating learning experience, our visit was beginning to distort ethical boundaries. Blurring the lines between tourists and researchers, our involvement brought up the issue of *access* as both an asset and a liability with long-lasting effects—not only for the future of our collaborative project but for the communities whose participation relies on transparency and respect. On a personal level, I sat awkwardly with the concept of “access.” I was technically a tourist but also involved intellectually and affectively in the research process;⁶ on the one hand, I was an active team member involved in shaping parts of the project; and on the other hand, I was a student who relied on this paid position. My discomfort stemmed not only from the observed rupture between our initial intention and what actually unfolded but also from the undue pressure this put both on the communities with whom we stayed and the research team we relied upon. Although the trip was successful in bringing together team members in a way that moved beyond the academic formalities of conferences and workshops—ultimately leading to this paper—I reflected on how our temporary involvement in the field impacted communities still noticeably recovering from a disaster.

Although most of what I have focused on here emerged from my relationships with other team members in the field, a large part of my role within the project was spent working on financial matters. This experience gave me a small glimpse into the administrative pressures of such grant projects and left me thinking about *legibility* and the distinct political and cultural worlds the grant attempted to bridge. For example, something as seemingly straightforward as a bank transfer between our Canadian funders and the research institute in Nepal (a core purpose of the grant) took months of taxing communication, research, and paperwork. Part of the tension seemed to lie in what Canadian institutions read as legible forms of documentation and research outputs, and the inflexibility of these structures to adapt to practices in Nepal. Not only did these issues obscure the extra labor required to make these transactions happen, as Shneiderman and Rankin reflect on below, but they took away from moments of cocreation. I am left feeling certain that there are better ways to support faculty such that administrative procedures are able to nurture productive transnational collaboration rather than impeding it.

I am reminded of an interview with Dr. Faye Harrison, who urges those of us operating within neoliberal institutions to remain “very vigilant and careful that the progressive projects that we do are not appropriated and then refashioned in forms that really undermine the very epistemological and political agendas from which our projects have actually emerged” (Harrison 2016). I question whether short-term field site visits end up being more harmful than beneficial to the research process and communities involved, and contemplate how the unexpected outcomes of collaborative research, with even the best of intentions, can create long-lasting anxieties. I hesitate to support large (and at times, unwieldy) international research partnerships formed in the spirit of “collaboration” or “capacity-building,” and wonder if the tens of thousands of dollars and environmental costs of such projects, coupled with the undue administrative burdens, overshadow those whose labor is at the core of ethnographic research, and the stories and lives that make this work possible.

When Stories Become Unmoored

OMER AIJAZI

I met Sara Shneiderman at UBC. Noting my interest in disaster studies and ongoing research in Northern Pakistan and Kashmir, she invited me to join the project. I started as a project coordinator before Emily Amburgey took over the position, then as a graduate research assistant, and finally as a collaborator. I sensed my refusal of disciplinary boundaries and disregard of institutional hierarchies would likely cause some friction in the team. For the most part, I sucked it up and took this as a learning experience, which it was. It seemed the issues that were causing me significant discomfort didn't matter to others—or so I thought. That all changed once I blurted out my concerns one afternoon. It has been a hurricane since then.

By now it should be clear that the partnership project brought together contrasting positionalities and research aesthetics. Perhaps this was prompted by the alluring seductions of interdisciplinarity. Or maybe, it was the culmination of a very generous reading of the academy, of academics, and of good intentions. I choose to leverage this space to un-

pack the many different ways one *can* be devoted to the academy. My reflections might seem abstract, but they are very much rooted within the labor of thinking and writing with others; when you don't see eye to eye, and when you do. In my reflection, I ponder over the desire to tell stories which don't belong to us, and why this is a pressing question for some, but not for others.

Stories can offer meaning from experience and can elaborate the political from the personal. They can propose new ways of mooring and unmooring the world. Amy Shuman (2010) writes that the representation of experience in stories is often inadequate. The failed promise to represent and understand experience or the "almost fulfillable promise" of stories hinges on relations between the private and the public; where the individual and collective are contested and recombined (Jackson 2013, 1). Robinson (2014) asks: "Who owns the story, the person who lives it or the person who writes it?" Welch (2009) presses further: Can ownership be considered synonymous with the right to tell that story?

I find these questions important, ones that should be considered in light of contemporary arrangements of knowledge generation where writing and thinking are sometimes considered separate from field research as Amburgey similarly notes above. These curious separations enable researchers to write with/from experiences they themselves have not nurtured. However, relying on others for fieldwork is not new and in most cases is considered fairly uncontroversial. There is ever growing literature that attempts to elaborate the communal nature of qualitative research to adequately value the affective and bodily practices of field researchers (see, e.g., Gupta 2014; Gupta and Ferguson 1997; Hobson et al. 2005). After all, it is the field researchers and the relationships *they* create that bring the field into "being" (Middleton and Cons 2014), which makes writing possible.

The increasing reliance on field researchers to serve as principal mediators, or *bideshiko dalal* as Suji notes above, is partially a response to demands for "high productivity in compressed time frames" by the neoliberal university (Mountz et al. 2015, 1236). Oddly, this arrangement also bears remnants of strange considerations of access and safety within humanitarian and development practice, which also relies on local people for fieldwork and social mobilization. The discourse in humanitarian and development literature is mostly on the efficiency and efficacy of remote management, not so much on the ethical repercussions of such

arrangements (see, e.g., Donini and Maxwell 2013). Perhaps, it is not too outrageous to think that some busy and over-committed academics also hide behind the language of efficiency and efficacy to protect themselves not only from the time investments demanded from fieldwork but also from the messy and sticky relations that unfold across vast differences in power. This prompts me to ask: *At what point does remote management of fieldwork become just another technology of control, expediency, and ontological safety?*

This is not to say that all scholars have ulterior motives but to point out that increasingly university positions are not conducive to conducting long-term ethnographic fieldwork, and scholars, therefore, scramble to put in place different collaborative arrangements. Thus, the motivations for instituting research partnerships are not necessarily only egalitarian, but also rooted in a calculus intended to render ethnographic research more convenient and conducive to the changing expectations and roles of scholars in Western universities. Collaborative research then serves the interests of universities and faculty primarily, and not necessarily research partners or research communities. Therefore, specific safeguards are required.

It is possible that a heavy reliance on fieldwork conducted by others can introduce a certain distance from communities under research, giving form to a mode of writing that carries minimal ethical repercussions. Such a separation allows the writer to feel comfortable with very sanitized notions of responsibility and reciprocity. I noticed this in our project where despite repeated rounds of fieldwork, there is a continued debate amongst the more senior researchers about the usefulness, practicality, and efficiency of directly channeling resources into a community project. During a review meeting, a Nepal-based project member remarked, “If all the money spent on the project had supported earthquake reconstruction, the kind of resources we had could have easily rebuilt an entire village.” While one can argue that as academics, our primary sphere of influence should be knowledge generation, it is difficult to consider knowledge as disjointed from the material conditions, which govern the lives of those we seek to learn from, even improve. *Why should a binary be maintained between a “material” intervention such as direct resource transfer and knowledge production?*

A reliance on stories collected by others and the separation that allows, can lead to expectations that one might reconsider, if they themselves had

been more directly implicated in the field. During our visit to one of the field sites, a senior researcher instructed the research associates to change the nature of their discussion questions so that community members could speak more purposefully about the research topic at hand: “I am not getting exactly what I need,” they said exasperatedly. Demands like these are indicative of an instrumental gaze bent on mining people for specific forms of information at the expense of respecting relationships and appreciating the limits of qualitative inquiry, or even just listening.

The intimate experiences of field researchers have important bearing on the stories they collect. There is real risk that these valuable entanglements may get lost as fieldwork is coded into transplantable commodities with little or no affective weight that can be transmitted via email, Dropbox, and Skype calls. As Binks expresses in the subsequent section, research artifacts such as journals and field notes are meant to mediate this dampening effect, but to what end? I think it is safe to say that something is lost when the sweat, tears, blisters, handshakes—the very gestures of encounter—are rendered subservient to the data itself (Aijazi 2018a). Such forms of data which are a step removed from the palpability of being present in the field bring us dangerously close to the kind of extractivist relationships that postcolonial critique has worked so hard to dismantle (see, e.g., Asad 1973; Nagar 2002; Nagar and Ali 2003). In these arrangements, “data” takes primacy over the heartbreak and chaos that are experienced in the field itself (Aijazi 2018b).

My participation in this collaborative research project has prompted me to reconsider whether data that has been collected by someone else is qualitatively *different* from stories collected by oneself. Stories that are only experienced and received in a textual or photographic form or as an audio recording, deserve a category of their own, one that captures the in-betweenness of “primary” and “secondary” data. Similarly, the writing that is enabled by these different approaches also needs to be better distinguished and parsed. This is not to ascertain which modes are better or worse, but to make clear the relationalities and forms of reciprocity each can enable. This enables us to press further on the question: *Can we write ethical stories when the relationships which bring them to light are fraught and contradictory?*

More language is needed on the ethics of writing from primary data collected by others. Both the writing and how it is represented should be

rethought. It is not sufficient to simply list field researchers as authors on potential publications (which is nonetheless a welcome step as pointed out by Limbu) or request them to write initial drafts which are then exacted, polished, and contextualized within academic literature by more seasoned researchers.⁷ Rather, I believe we are likely to find more honest answers by directly diving into the disequilibrium of power, value, and labor that remains concealed *within* research teams, imperfectly reflected by authorship credits on a published piece. This can possibly provide us an entry point to ponder over questions of shared ownership of stories as coconstructed and constituted projects, and the many struggles for legitimacy they represent. Otherwise, we are not doing much to speak truth to power against the colonial saturation of knowledge production, which although not always secure in its reach and depth is still profoundly destructive (Radcliffe 2017).

Gender and race also require careful centering. I noted the circulation of power within the project in two additional ways. Through a certain homosociality that manifested almost daily, such as the sharing of expensive spirits amongst some of the (mostly white or senior) male researchers at the exclusion of others, the separation of the “care labor” of working in a team from more masculine considerations of strategic data collection and high-impact publishing, or the “heroic” impulse to “let” the women researchers occupy “safer” accommodations in the village. Whiteness stuck out even more. Not only as a form of unquestioned and unrecognized privilege but also as an underlying optimism in Western academia, the researcher’s overstated role in knowledge production, and the brazen instrumentalization of relationships as a logical give and take, which left little room for actual reciprocity. While these considerations may appear as minor when read in the context of a collaboration and the research questions at stake, both gender and whiteness can function as mechanisms to orient participating bodies in particular ways, regulating the limits of one’s inclusion in the intellectual construction of a project, and creating various “outsiders” and “insiders” that exceed the categories of “senior” and “junior” researcher. Rather, it is at the intersections of these hierarchies where various exclusions manifest. The dilemma of unequal power sharing within research teams will likely persist regardless of how cleverly we throw in words like “collaboration” or “self-reflexivity” (see, Allen and Jobson 2016). Collaborative research can itself become a form

of structural violence (Bouka 2018). And, can enable the extraction of stories, normalize distance in writing, and conceal power dynamics—the very harms it seeks to undo.

Tasting the Field: Reflections on Blurred Boundaries between Online and Physical Fields of Research

JAMES BINKS

The reflections in this paper often consider fieldwork to occur only at physical field sites in Nepal. In this large partnership, I perceived research “fields” occurring in intra-partnership contexts too, especially online. By expanding the boundaries of where the fields of knowledge creation are encountered, I consider how intra-partnership interactions influenced the partnership’s behavior during short tastes of Nepali field sites in summer 2018.

Encountering the Field

When I joined the project in the summer of 2017, I was a third-year undergraduate student. As a research assistant, I was charged with supporting various project activities, but as an anthropology student, I felt that it was also my duty to examine the very practices of research. I was a “research tourist” or perhaps a “research researcher,” as Balaz-Munn discusses below. My experience as a student consisted primarily of reading completed ethnographic work and considering thinkers and theories in class discussions and essays. Fieldwork—the celebrated research method of anthropology—was treated as sacred and, therefore, as an abstract mythology off-limits to us non-initiates. I wasn’t privy to how to uncover or generate knowledge through transforming lived experiences into empirical data and then publishable data.

I felt privileged to be hired into the partnership and to peer behind the curtains to engage intimately with scholars conducting ongoing research across the world. I cannot overstate the value of the enriching experiences I had as part of this large project. Indeed, perhaps because I was wide-eyed with excitement and freshly engaged in the training of anthropological theory and method, I was just as excited to be involved in even mundane research activities as I was to experience fieldwork in Nepal. For this reason, I was primed to be on my best “ethnographer”

behavior. At all times I was listening, imitating, and learning—after all, was I not being tentatively initiated into a new social milieu?

As I worked on my initial role in the partnership setting up the infrastructure for future collaboration, I paid attention to how scholars (anthropologists and others) acted and interacted with each other and the process of research. While creating the project website, transitioning the partnership's communication to a new web-based project management system, and helping to organize a start-up workshop in Vancouver, the communication I shared with others showed me that I was far from the only one experiencing growing pains. The diffuse partnership development nature of the grant itself was new for many, while others had never communicated across such vast disciplinary divides, and a few members had never been to Nepal before. Even the Principal Investigator, Sara Shneiderman, faced several new and unexpected tasks (as she discusses in a later section of this paper).

I observed that the borderline unwieldy range of the partnership members' disciplinary interests and excitement were channeled productively into planning to write publications. Intra-project research teams around themes such as law, finance, and construction were formed to that end. Indeed, written publications are the first two items listed as partnership goals on the approved application submitted to the funding body as (1) "traditional scholarly publications" and (2) "online case studies and literature reviews." The initial concepts, hierarchies, and goals of the partnership had been organized and these social complexities would drive the partnership forward, in particular to produce publishable writings, and eventually cause some friction and concern amongst the partnership during visits to the Nepali field sites in summer of 2018.

Shaping the Field

By the end of 2017, several "junior" Nepali researchers had been hired into the partnership. Their role, as described by Suji and Limbu in this paper (who are themselves two of those "junior" researchers), respectively, was to "bridge the gap between local communities [in Nepal] and university partners [outside of Nepal]" and to "supply empirical findings." That is, to conduct field research in and with earthquake-affected communities in Nepal and communicate these findings to other members of the partnership. As these researchers began going to three select-

ed field sites across Nepal, the hierarchy and conceptual themes of the partnership guided their activities.

The junior members conducted observation, interviews, surveys, and other qualitative and quantitative research methods to help understand topics such as infrastructural reconstruction, changing cultural practices, financial reforms, and the newly promulgated Nepali constitution. The fieldwork in Nepal was conducted over several phases, and between these phases members of the partnership logged onto computers (or sometimes met in person) and participated in digital interactions over email, video calling, and word processing software to discuss, negotiate, and guide the research process. Partnership members not only discussed findings or translations of data from the field sites in Nepal; these digital interactions constituted essential relationships between members of the partnership where conversations flowed in many directions to receive field-based or disciplinary context and sharpen analysis and approaches. Through these platforms, the junior members' field notes and draft papers were distilled into conceptual research themes and fine-tuned in order to transform empirical data into publishable material.

As Suji notes earlier in this paper, at times his role in the partnership felt like being either an “insider” to the Nepali field sites for the senior members of the partnership and an “outsider,” or even a foreign broker, from the perspective of those at the Nepali field sites. The interactions with the senior members of the partnership in the digital fields drew upon his experiences and memories of his experiences at the Nepali field sites in a similar manner, as he himself was an “outsider” who elicited and transformed community informants' memories, lived experiences, and Nepali-language words into English-language empirical data.

I discuss these dynamics here to highlight how no matter where members were geographically situated, they were involved logistically in thinking about research production at different levels of “local” concerns. Some senior members may be focusing on trendy discourses within an academic discipline, while others navigated bureaucratic institutional politics to transfer project funds, while junior researchers in the field navigated how and when to contact certain key stakeholders. Senior members of the partnership were not only conducting “remote management of fieldwork,” as Aijazi discusses in the preceding section, but also the process of research and knowledge generation underwent a telescoping research process. This process passed through a hierarchical

system that involved negotiation and adaptation across the partnership, as earthquake reconstruction dynamics moved from lived experiences (in a Nepali field site) to empirical data (in a digital communication field) to words a reader can comprehend in a published article (on this page).

Tasting the Field

In the summer of 2018, most members of the partnership travelled to Nepal to take part in workshops, conferences, and—the catalyst for the writing of this article—short-term visits to two of the field sites. More than ten Nepali/non-Nepali and “junior”/“senior” members of the partnership attended. The field visits were designed to give the members a “taste” of rural and urban post-earthquake dynamics with which to better understand the ongoing research context and ground future publications. Our arrival to the field sites was preceded by many long days of rewarding discussions amongst the partnership on conceptual and publication topics across the disciplinary divides of the partnership. However, we did not have significant partnership-wide discussions about our strategy for conducting ourselves in the field sites. I was somewhat confused myself, but was excited to let the more experienced members of the partnership take the lead and to experience how abstract themes influence fieldwork and vice versa.

I continued my role as a “research tourist” during these field visits (a point Balaz-Munn discusses further in the following section). Like me, the partnership also generally proceeded from the digital fields to these physical fields without significantly adjusting the hierarchy or dynamic for conduct in the field sites. Guided by the conceptual thinking of senior members, we split up into groups based on research themes and our interactions with the field sites were often facilitated by the junior Nepali researchers’ connections, expertise, and Nepali language skills (where necessary). As I walked around with my colleagues, at one instance delving into an industrial tunnel and another visiting a political party’s office, I wondered about whether such short and relatively unplanned visits were standard for research or “tastes” of a field, or if there were ethical complications to our actions.

At what point do I take-off my observation hat and touch base about potentially problematic interactions? As a novice researcher, I wondered whether perhaps I didn’t have enough experience to identify what eth-

ical conduct looked like. I thought back to my anthropological training and wondered, like ethnographers in any new cultural or social field, do I need to swallow my hang-ups in order to understand the social group I'm learning about? Only here it wasn't the Nepali "locals" alone who I was studying, it was also the close-to-home group of experts I had spent a relatively significant amount of time over the past days and year growing and learning with—anthropologists, economists, geographers, historians, and engineers.

Focused on publications and set agendas, I and other members of the partnership carried our positionalities and actions from the intra-partnership digital field to the inter-social physical field. With minds on research outputs, conceptual thinking guided partnership members' interactions with those individuals who possessed relevant experiences, whether the junior Nepali researchers in the digital field or earthquake affected individuals in the physical one. However, during the short-term visits to Nepali field-sites, the conceptual and hierarchical style of interactions brought over from earlier intra-partnership communications stood out starkly. It is important to recognize the productive research and thinking that occurs in various research contexts or fields, and to jointly consider how best to proceed to utilize the multi-disciplinary skills of the partnership in an ethical manner.

Research Ethics and Tourist Anxieties in International Fieldwork

COURTNEY BALAZ-MUNN

In this reflection, I consider the fraught relationship between being a "tourist" and being a "researcher" and its implications for ethical research. As a master's student, I travelled to Nepal from Canada with a plan to spend my time taking language classes, getting oriented to a new city and culture, and eventually, conducting interviews that would form the basis of my master's thesis. During my stay, I was also fortunate enough to be invited to attend a series of workshops and field site visits organized by the research project with which the authors of this paper are affiliated, and I gratefully accepted the opportunity to learn about the behind-the-scenes of international research collaboration. These experiences prompted uneasy reflection on my various roles as a tourist/visitor in Nepal, a student affiliated with a large research project, and as a novice researcher working on my own thesis project. Below, I

consider a few key moments in which the boundary between research and tourism became blurred,⁸ and how this blurring may have obscured perceptions of ethical versus unethical methodology during team visits to field sites.

Tourists and Researchers

During my time in Kathmandu, I inhabited an ambiguous space between “tourist” and “researcher.” I had come to do research, but I was often perceived as a tourist and indeed felt like one (I was, after all, spending my first month settling in and exploring the city. And hadn’t I chosen my research project in part for the international experience?). When asked what I was doing in Nepal, I sometimes hedged around these issues, explaining that I was a student in Kathmandu to learn, do research for my thesis, and hopefully see some of the country. The tension I felt between research and tourism stemmed in part from an assumption that the two ought not to overlap—an assumption that became messily unmoored during the later field site visits.

Social scientists studying tourism have reflected on international fieldwork, probing the similarities and overlaps between ethnographers and tourists (Crang 2011; Crick 1995; Galani-Moutafi 2000). Some question whether there is “a basic difference between the tourist gazing at a spectacle and the theoretical gaze of the anthropologist” (or human geographer) (Crang 2011, 218). Relatedly, anthropologists have critiqued the ways the spatial imaginaries of ethnographic fieldwork have traditionally essentialized difference in terms of spatial distance, where the mobility of the researcher contrasts with an object of research that is rooted in place (Appadurai 1988; Gupta and Ferguson 1992). In this framing, the tourist and the researcher are similar: both may travel to relatively distant places to gain first-hand experience of something “other” and learn something about or from the “difference” of that place (Galani-Moutafi 2000). Scholars have also pointed to the shared colonial histories of tourism and ethnographic research (Bruner 1995; Galani-Moutafi 2000), overlaps in the resources and infrastructure used by foreign researchers and tourists (Saville 2019), and a parallel mobility of researchers and tourists epitomized by their ability to come and go from a place of interest (Hepburn 2002). The reflexive accounts of many of these scholars also observe anxious boundary policing by themselves and other researchers

who seek to uphold a fundamental distinction between fieldwork and tourism (Crang 2011; Crick 1995). Though the methods, motivations, and administrative requirements of researchers and tourists vary, there is clearly a relationship between international research and international tourism (Crang 2011; Crick 1995; Saville 2019). Here, I focus not on visas and other administrative aspects, but on behavioral and interpersonal dynamics that stem from a self-perception as a researcher or a tourist in everyday interactions.

However, the ability to be either a researcher or tourist depends in part on recognition by local people and institutions, and an individual's self-perception as a researcher may be a moot point if local actors do not perceive a meaningful distinction between tourists and researchers (Crick 1995; Hepburn 2002). This was illustrated during a group visit to one of the project's field sites, which contains a heritage site and popular tourist attraction. As our group approached the entrance to the site, we encountered a ticket counter at which all foreign group members had to pay a tourist entrance fee.⁹ Immersed in the world of research and fieldwork, some had forgotten that the field site was also a tourism site, and that some of us researchers were also functionally tourists. As we entered the area, a few vendors approached offering handicrafts for sale, as they did for other tourist groups entering. Despite perhaps a self-perception as researchers, not tourists, in these moments, this was a distinction without a difference. Our ability to be "researchers" as we entered the heritage zone was dependent on recognition by the systems and people we interacted with, to whom we were "simply a kind of tourist" (Crick 1995, 216).

Anxieties in the Field

A blurring of research and tourism occurred again during the second field site visit, a two-night stay in a rural town, as Amburgey discusses above. The dissonance first became apparent during a lunch break en route to the town, where we were encouraged to present ourselves as "visitors," not "researchers," in order to limit disruption to the research relationships established by the fieldwork team. Despite the good intentions, this seemed to imply a potential deception, or at least a deliberate vagueness, at odds with the principle of informed consent idealized by university research ethics. As we hiked the last leg of the journey to the town, some of the foreign team members expressed their discom-

fort at the idea of being perceived as tourists, joking about looking like stereotypical tourists with backpacks and cameras. There seemed to be an assumption that we were different from tourists, even as we hesitated to call ourselves researchers. Instead, we hedged around an ambiguous third ground of “visitors.”

Once we arrived in town, some team members embarked on a flurry of interviews, field notes, and meetings. I began to feel increasingly uneasy with my part in what was now playing out very much as a research trip. On the one hand, there was an uncomfortable voyeurism inherent in positioning myself as a tourist or simple visitor in the post-disaster setting (for instance, Suji, above, critiques how similar areas were inundated with visitors). On the other hand, I was also implicated in the ethically messy research practices going on around me: the fleeting encounters with people and place, the attempts to interpret meaning in an unfamiliar context, the way the ad hoc interviews by the foreign researchers seemed to take unspoken priority over the potential interests of the Nepali researchers, and the casual way Nepali-speaking team members were allocated to accompany non-Nepali speakers. Several other group members also expressed unease over their uncertain roles as researchers/tourists/visitors in the activities of a research project with which they were formally affiliated but in which they had previously had little direct involvement with the field. *Were we tourists, researchers, both, or neither, and could we ensure ethical research practices in this ambiguous space?*

This ambiguity raised concerns around informed consent. There is a significant difference between an individual sharing their perspective with a curious tourist (or “visitor”) and an individual sharing their perspective with a researcher who later writes up field notes about the encounter that could form part of a research publication. Perhaps it was a result of my inexperience that I could not discern to what degree the conversations and meetings I observed were informal touristic conversations or strategic research conversations; I obviously cannot know how the participants experienced them. However, it seems to me that opportunities to be overtly transparent were missed, and instead the uncertainty around our roles was relayed into uncertain interactions in which both information and consent were muddled.

Of course, most social scientists conducting research internationally would agree with a need to remain self-critical and vigilant about how we may reinforce artificial boundaries around research and tourism. I do

not believe it belittles ethnographic fieldwork to notice and acknowledge a relation to certain forms of travel for leisure, and I do not intend this reflection to be read as a criticism of specific individuals or projects. I believe the individuals in our collaboration would consider it a priority to ensure research is conducted in ethical and appropriate ways—but this was hindered by the structure of the large collaborative group. In the messiness and unfamiliarity of the group site visit, anxieties may surface when researchers are confronted with the voyeurism of being a tourist and tourists find themselves assimilated among a group of data-collecting researchers—anxieties that may hinder thoughtful and self-aware fieldwork practices and distract from the foundational concerns of research ethics. *How do tourists/researchers/visitors uphold ethical research practices when conducting interviews as a researcher versus chatting with passersby as a tourist? When does a “visit” become research? How do we parse competing research interests and touristic interests in a large group that includes members with diverse relationships to a place of interest?* Such ambiguities may be particularly worthy of contemplation in large transnational collaborations in which group members have varying relationships to fieldwork and field sites and in which project roles and relationships may be uncertain and evolving.

Collaborations across Status and Space

SARA SHNEIDERMAN AND KATHARINE RANKIN

Writing this article has generated rewarding multilayered conversations across space and status that inevitably arise over the course of a large international research project. We, Sara Shneiderman and Katharine Rankin, have served as Principal Investigator (PI) and Co-Investigator (Co-I) respectively, of the project on “Expertise, Labour and Mobility in Nepal’s Post-Conflict, Post-Disaster Reconstruction” out of which this article emerged. In the second year of our grant, after an intensive few weeks together as a research team in Nepal, we were impressed, and ultimately relieved—if at first somewhat uncertain about how to respond—when several graduate students and research associates began to raise critical questions about the power dynamics embedded in our research relationships. They articulated in words a sense of discomfort that we had also felt in inchoate form. Their work to create the space for this conversation went beyond the formal scope of their responsibilities

to the project, and as such demonstrates through its own example the arguments we make here. We were honored to be invited to participate in this collective effort to document that experience, and grateful to our coauthors who initiated it for revealing with candor and grace some of the inequalities inherent in the research process that often remain hidden, or at least unspoken. Our section focuses on the collision between what we might call the romance of collaboration, and the contingencies of institutional and funding structures, as a disorienting but defining experience of leadership in collaborative, international research partnerships.

We begin by recalling our own journeys through undergraduate and graduate education. Researchers in both of our generational cohorts (separated by a decade or so—Rankin completed her BA in 1983 and PhD in 1999; Shneiderman completed her BA in 1997 and PhD in 2009) experienced tremendous angst about the relationship between the privilege of researchers from the West and the nuances of subaltern voice, whether driven by the publication of Said's *Orientalism* (1978) and the ascendance of postmodern social theory, or the disciplinary paroxysms of Clifford and Marcus's *Writing Culture* (1986). These considerations of representation and voice continue to haunt us, through our ethnographic engagements as well as more activist-oriented research in both Asia and the communities where we have lived and worked in North America.

We were both trained at Cornell University in the tradition of humanist anthropology, a tradition geared toward making the world safe for difference (e.g., Benedict 1989). We are aware that this stance might be perceived today as naive and old-fashioned, but it continues to motivate us, even as we eschew the elements of relativism. Sherry Ortner expresses the humanist impulse with recourse to the analogy of a “ship of capitalist history” that tracks a universalist account of global political economy. She advocates the advantages of instead seeing the world through perspectives afforded by engagement with life on the shore. To assertions that we can never really know what the world looks like from other shores, she responds unequivocally, “try”: “The effort is as important as the results. . . . It is our capacity . . . to take the perspective of the folks on the shore, that allows us to learn anything at all . . . beyond what we already know” (1984, 143).

The postcolonial/postmodern and humanist standpoints offer contradictory perspectives on collaborative, international research.

The ambivalence they generate is compounded by long-term, personal relationships developed in Nepal and with Nepalis that have infused both of our lives over three decades. Living with the ethical tensions with which we all grapple in this contribution requires continual navigation—but collectively, it seems, we have opted to abandon neither the ship nor the shore, in favor of the possibility for encounters across difference and collaborative engagement, combined with attention to the macro-social and political-economic processes that articulate cultural politics on the ground.

The contributions of our coauthors in this paper manifest the dilemmas of navigating ethical ambiguities sensitively as emerging researchers, experiences which we shared, however differently, earlier in our own careers. Now as established scholars benefitting from the security of tenured academic appointments, we are learning to recognize and mobilize the power that accrues with seniority. Taking a cue from J. K. Gibson-Graham's *A Post-Capitalist Politics* (2006), we see these forms of power as resources for forging “trust, conviviality and companionable connection” within academia. We are now in a position to ask: *How can we direct research funding toward collective endeavors that both redistribute opportunity and generate robust research? How can we decenter power within research collectives, while also wielding power to create their conditions of possibility? How can we create the safe yet uncomfortable dialogical spaces to continually scrutinize the unexpected consequences of our own actions?*

Invisible Structures

Serving as PI (Shneiderman) and Co-I (Rankin) of the SSHRC Partnership Development Grant out of which this article emerged over the last three years, as well as in flipped roles (Rankin as PI and Shneiderman as Co-I) on another SSHRC (Insight) Grant with collaborative research partnerships in Nepal that began in 2014,¹⁰ we have often been struck by the power of invisible structures to shape the ways that participants relate to each other. These include the structures of university finance and administration, which govern the international flow of funds necessary for our projects to operate.

As Amburgey mentions above, it took nearly a year to make the first transfer of funds from Canada to Nepal to begin the work that our Part-

nership Development Grant had funded. The transfer proved difficult for bureaucratic reasons. The process of administering the grant required the majority of the PI's research time, with little support provided in an institutional context that lacked familiarity with international collaborative modes of scholarship and the commitments it must make to sharing financial resources. Navigating these processes, and building the relationships at our home universities to sustain them, meant that there was little time to engage substantively in the research that we had envisioned, let alone write about it. Our day-to-day affective experiences of scholarly labor and time were radically restructured as we came to inhabit these leadership roles. We learned new forms of perseverance navigating the complexities of a system that was not really set up to facilitate genuine collaboration with institutions and individuals outside of Canada.

These experiences, and the structures that conditioned them, were invisible or only partly visible to most other members of our research team, as well as to colleagues in our home departments. From the vantage point of Nepal, the funding either arrived or didn't—but the pathway it had taken to get there was obscured. From the vantage point of our universities, it was only the research outputs that mattered, primarily publications, while the time and emotional labor devoted to administration and relationship-building were not valued in themselves.

Building a research team, of course, also requires building relationships, which itself has been an ongoing process beginning with the creation of research instruments, to visiting research sites, up through disseminating research findings in publications. To take the example of writing in groups of five or more, as this project requires, here again we found ourselves in the position of having to negotiate different priorities and writing styles within the group. Doing so creates opportunities to make stronger arguments and clearer representation of empirical material. However, it also requires time and care to ensure that all voices within the group are heard, and that collective decisions are made in a way that all collaborators can accept. In a singular written product with many coauthors, not everyone may be equally pleased about every sentence or paragraph. Someone has to manage the expectations around this process, and ensure that writing actually does proceed even in the face of differences. This is part of the PI's role, but it is distinct from actually doing the writing one's self. Yet systems of reward at our

universities devalue collaborative research in general: coauthored, not the mention multi-authored, articles are ranked below single-authored ones for merit, tenure, and promotion purposes.

In a politico-historical moment when academia is engaged in a long overdue struggle to come to terms with the precarity at its heart, we found that it was difficult to talk about the labor of navigating the research funding landscape from our positions, perceived by many to be at the top. While recognizing the power we do wield as lead investigators, it often felt like we were not really at the top at all, but rather midlevel managers within the often-invisible structures of a neoliberal research economy that has outsourced its administration to faculty PIs. These larger structural conditions, and the way that they transform the daily lives and intellectual capacity of midcareer scholars embedded in Global North institutions are also part of the story. Putting these realities in conversation with the experiences of emergent scholars in forums like this article helps to reveal the relational system in which we are mutually embedded, and point towards strategies for transformation.

Catch-22s of Opportunity and Labor

The start-up workshop for this project was held with “senior” participants only who were already named on the grant, as we couldn’t hire “junior” researchers until Terms of Reference were developed and the supervisory group formed. But this resulted in the Nepal-based research team—who were hired through grant funds—not having been involved in developing the research agenda, either at the conceptual level of drafting the application, or at the pragmatic level of developing the research program in the start-up workshop. This is a catch-22—especially within a context like Nepal where paid, contract-based research dominates the field. Under such operating conditions it is unfair to researchers to demand that they get involved in the very intensive labor of writing a proposal when their time cannot be compensated—and funding success is not even guaranteed. However, as multiple reflections in this paper have noted, to build genuine collaboration, it is also unfair to expect team members to uncritically advance an already well-developed research agenda.

This catch-22 also presents itself in terms of creating “student train-

ing opportunities” at Canadian institutions, to use the terminology in grant agency guidelines, which suggested that approximately half of the proposed budget should be allocated for such purposes. It is structurally nearly impossible to involve the same students in preparing grant applications (the stage at which research design is conceived) and in undertaking the actual research (both of which activities must be properly compensated of course). This time lag means that, like the Nepal-based researchers, student research associates and assistants who comprised part of our research team (and coauthor this article) had to enter into an already-conceived research design—much against the principles of true collaboration. Moreover, since funding is not necessarily available to involve *any* students at the research design stage, this critical step is often left to faculty lead investigators working on behalf of a not-yet-constituted research team.

There are two competing arguments here. First, that intellectual labor must be fairly compensated, both financially according to actually invested time at all stages of the process, and in terms of eventual authorship and recognition. Second, that research agendas should be genuinely co-created from the very beginning—at which point there are usually few resources. An open conversation about this paradox is necessary, particularly in international collaboration contexts where it is not possible to bridge such gaps with occasional (uncompensated) informal meetings or backfill with small amounts of other university resources to provide lunch or tokens of appreciation (due to distance and university finance regulations).

To conclude, we reflect upon some of the questions that we are left with as our current grants come to an end. *What would it mean to subject administrative labor to collective planning processes, so that it can reflect strategic considerations as they relate to ethical commitments? How could we make that labor explicit and equitably distributed, as a real opportunity for cocreation? How can we make visible this invisible elephant in the room in a way that allows us to share the meso-level forms of power to which we have access, while also pushing back against the centralized higher-level forms of power that are exerted upon all of us?* Ideally, we would seek to create a sense of solidarity along the way, which benefits all involved while acknowledging the real differences in our experience and addressing them openly.

Concluding Remarks

Collaborative research aims to dismantle hierarchies, unsettle colonial models of researcher and subject, and democratize processes of knowledge generation (see, e.g., Blaikie et al. 2015; Fluehr-Lobban 2008; March 2002). While collaborative projects are for the most part genuine attempts to go beyond treating people as mere objects and subjects of research, they are not perfect, nor are they outside the scrutiny demanded by critical, feminist, and postmodern turns in anthropology that call for a repeated and continuous attention to power within knowledge construction (see, e.g., Bishop 1995; Gallagher 2008; Grogan 1999; Haraway 1988; Harding 1986; Lather 1991).

The notion of “junior” versus “senior” researcher, and many other related binaries, have been problematized throughout the paper. We acknowledge that these categories are fluid, with more experienced researchers feeling junior at times and junior researchers feeling undervalued. Rather than reproducing a dualistic way of thinking, we have attempted to think through these entangled power dynamics to value the expertise and capabilities of all partners. We envision a collaborative style that benefits everyone without collapsing difference. We are not only referring to the so-called apprenticeship model in academia—a model that imagines learning as a one-directional process as researchers with less experience learn from researchers with more experience—but are advocating for a way of working together that calls for reciprocal learning. Collaboration can shift the nuclei of relationships, creating a balanced exchange of knowledge and experience within a context of mutual respect. In fact, building relationships is part of the research process; sociality of research is constitutive of the process, and collaborative research teams must embrace the time it takes to build relationships. In our own group, we have experienced the challenges of sustaining a collaborative process in the writing of this paper. We have juggling multiple time zones, schedules, and levels of engagement; navigating grey areas of leadership versus collective process; and experienced tensions, disagreements, and miscommunications. Yet despite its challenges, we remain committed to the idea that data that is generated collaboratively, deliberated collectively, and engaged to multiple purposes is likely to be more nuanced and relevant.

At the same time, it is important to remember that members of a research team may have varying modes of engagement with the project at hand, and each type of engagement needs to be recognized and valued. This is particularly relevant for those modes of labor that remain invisible or hidden within the overarching framework of collaboration. This includes an appreciation for the extra work of translation and writing in a language that may not be the author's mother tongue. Valuing different forms of labor is also key to the discussion about who is credited as an author. Similarly, sustained attention is needed to the dissemination of research across geographical locations, disciplines, and languages, including translating all research outputs into the local languages of participants.

In this paper, we have attempted to unsettle and disorient the stability and protective guarantees of collaborative research. We acknowledge that research is an intervention—a material and affective interjection—whose processes, modalities, and outcomes can never be fully predicted, and in that sense may never be fully ethical. Through our reflections, we have attempted to illustrate how collaborative research is not *outside* of asymmetries of power but is shaped by the very relationships that give rise to such asymmetries in the first place. In this way, we advocate for a stance of constant vigilance and scrutiny—a position of uncomfortable reflexivity. However, this doesn't mean that we do not try to collaborate, that we cease the work and wait for the perfect politics to emerge—because that, too, is unlikely to happen.

Acknowledgements

The authors would like to express their sincere gratitude to our community partners in the three field sites in Bhaktapur, Dhading, and Sindhupalchowk districts, and to our institutional partners in Nepal: Social Science Baha, and the Central Department of Anthropology at Tribhuvan University. We would also like to thank our fellow partnership members for providing their invaluable feedback during workshops in Kathmandu, Nepal (2018) and Sandbjerg, Denmark (2019), and the three anonymous reviewers whose comments allowed us to develop and strengthen the focus of this paper. Field research in Nepal and all associated workshops was funded by the Social Science and Humanities Research Council (SSHRC) of Canada through Partnership Development Grant Number: 890-2016-0011. Additional support was provided by the Peter Wall Institute for Advanced Studies, the Faculty of Arts, the

School of Public Policy and Global Affairs, and the Department of Anthropology at the University of British Columbia (UBC), and University of Toronto in Canada; and by Aarhus and Copenhagen Universities in Denmark.

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Notes

1. For a more detailed analysis of post-earthquake experiences and our research outcomes, please visit the project website: <https://elmnr.arts.ubc.ca/>.
2. The draft manifesto publicly circulated on several listservs can be accessed here: <https://www.ipetitions.com/petition/power-prestige-forgotten-values-a-disaster?fbclid=IwAR3uuHj8INnRrvNHgC7lFhgZHXGxXFk20f9zdjLoIXmX00uNrsIREZ30t9U>. Also see Gaillard and Peek, 2019.
3. Unstructured time included hiking in the hills of Nepal, chats between official tasks, getting lost, engaging with each other on social media, sharing accommodations and meals, heated disagreements, and debriefing after uncomfortable encounters with communities and researchers alike.
4. The original grant budget included small amounts for donations to community organizations. However actually making these funds available became challenging due to university finance complications in Canada, as described by Amburgey and Rankin and Shneiderman in subsequent sections.
5. In the final phase of the project that is still ongoing, we intend to publish a series of policy briefs in both English and Nepali, and deliver the latter to communities with which we worked.

6. Nepal does not offer short-term research visas. The only research visa currently available is for a full year and suitable for long-term, doctoral research, but cannot accommodate short-term field visits. Many foreigners come to Nepal on tourist visas and either conduct research or work with NGOs, due to lack of other options. This is not to say I agree with this process, but to clarify that this behavior is not unique to our project.

7. Equally troublesome is the very polishing of experiences into data to fit within existing literature.

8. Although the term “tourist” can sometimes have pejorative connotations, I employ the term simply to describe a person who travels for pleasure. There is no implied distinction between “tourists” and “travelers.”

9. Like some of my coauthors, I distinguish between foreign and Nepali team members in order to depict dynamics I observed, but also note that representing these categories as binaries falsely simplifies individuals’ multiple and overlapping identities.

10. “Infrastructures of Democracy: State Building as Everyday Practice in Nepal’s Agrarian Districts,” <https://infrastructuresofdemocracy.geog.utoronto.ca/>. Funded by the Social Sciences and Humanities Research Council of Canada (SSHRC Insight Grant no 435-2014-1883, 2014–2020).

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