Agency and Agendas: Revisiting the Roles of the Researcher and the Researched in Ethnographic Fieldwork

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Abstract: Drawing on the authors' research experiences, and using Anna Tsing's (2005) concept of "friction," this article considers how ethnographic research is an essentially collaborative project. Ethnographic knowledge is generated by researchers and their (intended) participants – our agencies and agendas – come together to co-create a field of research. We argue that how these contextually embedded agendas align, differ, and/or diverge deeply shapes ethnographic knowledge. We also consider the effects of ethnographic legacies: of past ethnographers on their contemporaries; of the ideal, "slow" ethnographic approach for researchers working outside of academia; and of the future afterlives of our own work.

Keywords: agency, ethnographic method, observer effect, positionality, reflexivity, slow ethnography

Résumé : À partir de l'expérience des auteurs, cet article présente la recherche ethnographique comme un projet essentiellement collaboratif grâce au concept de « friction » de Anna Tsing (2005). Le savoir ethnographique procède des chercheurs et leurs (éventuels) participants, de leurs activités et agendas de part et d'autre, et de leur rencontre naît le champ de recherches. Nous proposons que la manière dont ces agendas, profondément inscrits dans leurs contextes respectifs, s'alignent, diffèrent et/ou divergent, façonne le savoir ethnographique. Nous considérons aussi l'impact des pratiques ethnographiques : de celles d'hier sur celles d'aujourd'hui ; de l'approche « lente », dite idéale, par rapport à la pratique hors académie ; ainsi que le futur de nos propres projets de recherche.

Mots-clés : agencéité, méthode ethnographique, impact de l'observateur, positionnaliteé, réflexivité, ethnographie « slow »

One of the key strengths of socio-cultural anthropology and our central methodology – ethnography – is its ability to identify and make sense of ways of knowing and being in different social worlds. During the latter half of the 20th century, anthropologists have become ever more conscious and critical of how our own ways of knowing as social researchers affect what can be known through our research (Clifford and Marcus 1986; Scheper-Hughes 1992). Through the works of anthropologists such as Rosalie Wax (1971) and Jean Briggs (1970), questions of reflexivity, positionality, and representation have become not only more than simply *de rigueur* in ethnographic writings but also integral to the scholarly rigour of ethnographic research and writing. Since the 1980s and 1990s, ethnographic accounts have routinely come to include reflexive descriptions of the researcher's position vis-à-vis their research participants, as well as their own feelings and reactions during fieldwork, as a way to acknowledge the inter-subjectivity of the research (Rabinow 1977; Salzman 2002, 806). Important threads in ethnographic work conducted and produced over the last few decades include considerations of how our agency as researchers, and the complex relationships we build with our participants and in the context of our field sites, matter during and following field engagement. These questions, so hotly debated in our recent past, continue to matter for ethnographic fieldworkers.

We argue that this is especially the case in the contemporary moment when what it means to "do" ethnography – from the cultural contexts under study to the types of reports produced and the training of the researchers employing ethnographic methods – is changing. Given the valuable insights that ethnographic methods produce, the use of this approach is becoming more commonplace in research across a variety of disciplines. These include not only fields of study closely related to anthropology, such as sociology and sociolinguistics but also the kinds of applied contexts that

anthropologists have traditionally excelled in outside of academia, such as working with non-governmental organisations (Long et al. 2015) and in policy creation (Ferrara 2015). We are also witnessing a growing use of ethnographic research in fields like education and nursing. Ethnographic methods and the "thick data" that they produce are increasingly employed in the private sector (Ladner 2014) – for instance, in business innovation, marketing, and product design (compare Battersby 1981; Pettigrew 2000). This embrace of ethnographic research in the public and private sectors coincides with the intensifying precarity that those of us with advanced training in anthropology now face in the university sector job market (Cummings 2015; Henderson 2015; Thorsen-Cavers 2015; Waldram and Graham 2015). Given that ethnographic methods are increasingly employed by non-anthropologists, and by anthropologists facing distinct challenges using ethnographic methodology in non-academic contexts, we argue that it is important to revisit the roles of the researcher and the researched in ethnographic fieldwork. The insights brought forth in this article include an exploration of how the social position of ethnographers has shifted and how relationships with interlocutors have changed in increasingly different contexts of "the field."

The idea to revisit how the impact of the various agencies and agendas shape ethnographic knowledges in the present moment first emerged from discussions across the work of five scholars brought together on a panel during the 2015 Canadian Anthropology Society conference in Quebec City. This panel sought to understand how our own choices and perspectives – as researchers and otherwise socially situated subjects – present a starting point for understanding ethnographic research. This panel began by questioning how the anthropologist affects the point(s) from which projects may begin and/or profoundly influence the direction or outcome of the investigation. Together, however, these papers offered more than a simple contemplation of reflexivity and positionality through their discussion of the multiple agencies and agendas at play in ethnographic research. This question becomes even more important when acknowledging that not all ethnography is done by anthropologists or anthropologists working in the academy. Drawing on the authors' diverse ethnographic contexts and perspectives, this article seeks to honour panellist Pierre Maranda's enthusiasm for locating what he called the "general cognitive impact" of this discussion.[1]

In this text, we reconsider the role of the ethnographic researcher, how one's position as a socially situated subject and our choices in (relation to) the field deeply shape that field, the research one is able to undertake, and what can be known through such study (Briggs 1970; Rabinow 1977; Wax 1971). Ethnography remains necessarily adaptive, with our research profoundly directed, enabled, and curtailed by the (often unanticipated) insights and influence of our participants. In what follows, we seek to re-open a discussion of the issue of access to field data and offer a consideration of how this access and what we come to know through ethnographic research is part of a dialogic and "frictive" process between ourselves as researchers and those we seek to engage in our research (following Tsing 2005). During our panel, Maranda raised the question of the "observer effect" – namely, that our presence as researchers can influence the very actors and phenomena that we wish to study (LeCompte and Goetz 1982; see also Monahan and Fisher [2010] for a discussion on the benefits of the "observer effect"). This concept has long been an issue with which anthropologists and other social researchers have wrestled. In this article, we take the opportunity to reflect on what might instead be called the "observ*ed* effect" – that is, we consider how the agency and the agendas of our participants and their sense-making of us as social actors and as researchers with specific agendas affect what can be known as the result of our research.

We suggest that Anna Lowenhaupt Tsing's (2005) concept of "friction" can be helpful in making sense of what we come to know as ethnographers as well as how this knowledge is generated through our agency and agendas as social researchers as it aligns with, supports, or diverges from those we (seek to) engage as a participants. Here Tsing's friction refers not to conflict or poor relations among the researcher or researched. Instead, friction refers to the idea that our interlocutors are agentive individuals who wilfully take part in, and influence, our research. How our interlocutors participate (or refuse to participate) deeply affects our work as ethnographers. As Tsing writes,

> a wheel turns because of its encounter with the surface of the road; spinning in the air it goes nowhere. Rubbing two sticks together produces heat and light; one stick is just a stick. As a metaphorical image, friction reminds us that heterogeneous and unequal encounters can lead to new arrangements of culture and power (5).

To think of the interactions of ethnographic research as producing friction, even if taken in the hard science sense – that is, as "the resistance to motion of one object

moving relative to another" (Ghose 2013) or the effect of one object on another and the other's trajectory – is the moment – the point – that we wish to discuss.

Beyond Reflexivity: Considering Multiple Agendas in Ethnographic Research

Ethnographic research design is a messy process that emerges not only from careful planning based on prior knowledge but also through the continuous feedback loop of the qualitative research process itself (that is, purposive sampling, inductive data analysis, and emergent design). Many ethnographers use or reference an inductive and iterative approach to analyse data collected from and, importantly, during fieldwork (Charmaz 2004). The flexibility of this methodological approach is one of ethnography's main advantages in that it allows ethnographers to shape their research according to "what they find in the field" (Briggs 1970). Ethnography is useful in its flexibility to fit and explore questions in various cultural contexts. What we find in the field, however, is part of how this field of study is defined, delimited, and co-created by and between the researcher and the researched. Beginning from this perspective, Tsing's concept of friction is therefore instructive. In Tsing's work on the multiple local and global actors and interests invested or embedded in the forests of Kalimantan, friction becomes a way of thinking about how culture – and, as we argue here, the study of culture – is continually co-produced. In Tsing's (2005, 4) sense, friction describes "the awkward, unequal, unstable, and creative qualities of interconnection across difference."

Our consideration of ethnography as an essentially collaborative project also speaks to the generative and transformative energy of friction. In this sense, collaboration in ethnographic research refers to the processes through which "knowledge is made and maintained" through diverse encounters and relationships, where goals or agendas may or may not be held in common by the ethnographer and those we wish to recruit to our projects (Tsing 2005, 13).[2] Thus, in our analysis (regardless of whether this occurs in the context of ongoing research or after data collection has ended), ethnographers must acknowledge the continued role of our participants on our reading of the data. In so doing, ethnographers can ask important questions about their research, including: how we learn what we learn; what we learn from the tensions, contradictions, and "truisms" that we observe; and how working toward a representative and multi-vocal perspective of our field data can help us to recognise and address the various forms of "baggage" (associated with our discipline, our field sites, how we access our data, and ourselves as researchers) in our ethnography? In asking these questions, we necessarily reflect on our positions as actors with specific agendas engaged in certain social contexts and in particular ways. What we intend in the discussion that follows is a consideration of how the multiple agentive participants involved in our research facilitate and/or circumscribe access to the data we hope to gather, affect the perspective through which we make sense of this data, and inform the results that we eventually disseminate. In so doing, we seek to reinvigorate the idea that ethnographic research can be, as Tsing has described globalised acts, a "zone of awkward engagement" and an inter-subjective project, by pointing to those less often disclosed ways in which the agendas, agency, and perceptions of our (potential) participants – and of us – direct, shape, and cause friction in our research (xi).

Co-Creating the Field: Accessing Data

There has been much discussion of how the ethnographic field itself is continuously being redefined in both scholarly literature (Amit 2003; Gupta and Ferguson 1997) and the increasing number of reports and reflections on the use of ethnography in non-academic institutional contexts (Ladner 2014). Yet through all of these discussions and this repositioning within the discipline of anthropology, how one accesses this data, and through whom often remains integral to the social positioning of the researcher in the social field under study. This positioning, we argue, enables, shapes, redirects, and limits the kinds of research we may ultimately produce; this is in no small part due to the agency and agendas of those we recruit as participants in our studies. In the context of the 21st century, the legacies of past ethnographers increasingly come to bear on how one is socially positioned within the field. Our experiences, addressed below, underscore how the multiple positions and identities that researchers simultaneously inhabit influence the starting points and development of our projects. This exploration highlights how these positions and identities operate in relation to those we seek to recruit for our ethnographic studies. Rhiannon Mosher, Jennifer Long, and Lauren Harding draw on their fieldwork experiences to speak to the ways in which the decisions we make – and the decisions made by past ethnographers – can variously help grant or bar access to participants, generate a particular perspective, and inform the tacit knowledge we bring to our analysis of the everyday. Where Mosher and Harding speak to the challenges they faced undertaking long-term academic projects in the context of their doctoral studies, we use Long's research experiences as an "ethnographer for hire" to investigate this use of ethnography and explore how researching on behalf

of private industry or a non-profit organisation affects one's social positioning as a researcher.

Identifying a site or point of entry for ethnographic fieldwork hinges on a researcher's ability to gain and maintain the (ongoing) consent of those who become our research participants. The point where an anthropologist or ethnographer enters the field is critical; however, access to field data should be considered an open-ended practice shaped by our interlocutors, their (and others') agendas, and their perceptions of us. This access to data and how we are positioned in relation to those we study is frequently affected by how we communicate with these groups of people. Whether we are "native" anthropologists or engaging in sites far from "home," as Lanita Jacobs-Huey (2002, 793) has noted, "'[k]nowing the languages' of a research population is a mantra to which all ethnographers are socialised before conducting fieldwork." Learning the language of your research community implies much more than literal understanding; learning the languages of the communities we study is a key methodological tool for making sense of cultures in context. By learning and communicating in another language, we learn through experience, gathering tacit knowledge of the different ways in which those we study understand social categories and processes, expectations, and etiquette. More than this, the languages we use often communicate important social cues to potential participants about how to make sense of us and our (research) agendas (Jacobs-Huey 2002), as detailed in Mosher's work below.

When Mosher began her doctoral fieldwork in Amsterdam, the Netherlands, she was interested in how native (that is, white, ethnic, or "autochthonous") Dutch citizens imagined the national community and in their views on the integration of newcomers into Dutch society. The language-focused "civic integration" legislation that was first enacted in 1998, the growing voice of the xenophobic populist Right in Dutch politics, and subsequent scholarship on issues of immigrant integration all indicated that the Dutch language itself would be an important issue in Mosher's research (Björnson 2007). Yet Mosher found that it was her own position as a Dutch language learner that most dramatically shaped the choices she made as her project unfolded. Mosher's research focused on Dutch language coaching projects and their volunteers in Amsterdam as a group of "gatekeepers" to the national community. This focus developed from Mosher's contact and involvement in one such organisation as a language-learning client and her own voluntary work in educational and care initiatives elsewhere in Amsterdam. Working as a volunteer, Mosher came to see voluntarism as a rich site for ethnographic analysis of notions of morally and culturally attuned citizenship practice (Muehlebach 2012; Putnam 1995). Yet, as Mosher conducted participant observation and interviews with volunteers across these sites, it became clear that Dutch language coaches, whose voluntary work effectively positioned them as frontline citizenship educators, provided a unique lens to the research questions she wished to explore. Not only were these language coaches almost exclusively native Dutch, but, in helping newcomers learn the national language through informal conversation partnerships, they conveyed particular ideas about what it meant to participate and belong in the neighbourhood, city, and nation (Mosher 2015).

Mosher anticipated that her position in Amsterdam as a white, anglophone, Canadian researcher would affect how her participants related to her and her research interests. Canada was not such a "foreign" country in the minds of many of her participants, and, indeed, many native Dutch she spoke with had vacationed in the country or had relatives who had emigrated to Canada after the Second World War. Most people she interacted with in Amsterdam, as well as elsewhere in the Netherlands, were also conversational (if not fluent) in English. She found that most of her participants assumed that Mosher would share similar life experiences, have similar historical and cultural reference points, progressive values, and expectations for behaviour as themselves. These points made Mosher less of a stranger for these participants than foreigners (or Dutch citizens) with non-Western heritage, and, indeed, her efforts to learn and speak Dutch were frequently read by these participants as conveying a commitment to (understanding) Dutch culture, which was often seen as lacking in those deemed "foreign." Due to this common perception of Mosher as an interested and sympathetic observer of Dutchness, she frequently found herself engaged in discussions about cultural difference and belonging that were critical of those perceived by her interlocutors as "foreign." Speaking Dutch – and even conversations about learning Dutch – became a point of entry for Mosher to the broader discussions circulating around (the problems of) immigrant integration, cultural difference, and national belonging. In such conversations, the Dutch language itself was shown to be a powerful symbol or idiom used to demarcate the boundaries of the national community – but one that was also layered over other culturalised notions of belonging (Mosher 2015).

The discursive comfort and familiarity that Mosher communicated through her efforts to learn and speak Dutch also allowed her participants to make sense of her questions on issues of national belonging and immigrant integration in a way that aligned with their own

views or agendas. This was especially the case for her key research participants, the Dutch language-coaching volunteers. That Mosher was learning Dutch resonated with these ideas as expressed by her key participants. In her position as a participant in a Dutch language-coaching program, and as a volunteer for other initiatives in the city, language-coaching volunteers viewed Mosher as someone sympathetic to their views and projects. Language coaches and their organisations all agreed that by learning the language, newcomers to the city or country would also learn about Dutch cultural norms and values, including how to be a good neighbour and member of the local community. Mosher's learning Dutch, and interviewing Dutch language-coaching volunteers on these topics, often spoke to these participants' interest and motives for engaging in these programs. As Mosher's experience underscores, the agendas of our participants may deeply colour and direct the research we conduct and the data we collect.

This question of whether or not our research agendas align with the agendas of those we wish to study is important for the acceptance of ourselves and our work among particular groups and at particular sites (Ferrara 2015; Rabinow 1977). We must therefore acknowledge that in some contexts ethnographers may in fact be denied entry to particular "sites" or data. Gone are the days when an anthropologist can simply decide to set up their tent or be dropped into the lives of a local community by colonial authorities or local missionaries and effectively demand the participation of locals in their study.[3] Now it is widely understood that anthropologists and other social researchers uphold a code of ethics that requires informed consent. While some ethnographers may find that the issues they intended to study are taboo and not discussed (Briggs 1970), gatekeepers may deny an ethnographer's participation in their communities, or community members may refuse their consent for their own reasons (Wax 1971). Moreover, contemporary concerns for decolonising anthropology – especially among those of us who work with Indigenous communities – may in fact mean that ethnographers actually negotiate access to particular (physical, social, political) sites as a researcher (Smith 1999).

Working on the West Coast Trail on Vancouver Island, British Columbia, Harding recognised that her chosen field site is situated among local communities who have a historical and fraught relationship with anthropologists. As such, negotiating an entangled web of research questions, professional ethics, and identity politics has been key to accessing this particular site. In her research, Harding investigates issues of national park formation in the context of settler colonialism as well as both tourist and local place-making in a so-called "wilderness" setting. As a settler Canadian studying settler colonialism in Canada, her research focused on Canada's most renowned backpacking trek, the 75-kilometre-long West Coast Trail (Pacific Rim National Park Reserve). This trail is located within the traditional territories of the Huu-ay-aht, Ditidaht, and Pacheedaht First Nations. The lack of historic treaties, the historically large Indigenous population, and the relatively late colonisation and settlement all factor into the region's significantly dynamic politics of colonialism and Indigenous resistance throughout the 20th and 21st centuries. These factors also account for anthropology's interest in the region and its peoples since the early 20th century. Although the majority of Harding's research focused on park visitors and locals who facilitated tourism in the region, the legacies of past anthropological research on Indigenous people's oral history, storytelling, and practices more typically designated as "culture" had notable effects on her research.

When Harding first met with a representative of the Huu-ay-aht First Nation to discuss her request for research permission, the legacies of past anthropologists in the area were immediately clear. The first thing the Huu-ay-aht First Nation representative said to Harding was: "So you're an anthropologist, eh? I've read Sapir. He got a lot of stuff wrong."[4] Even though Harding assured the representative that she was "not that kind of anthropologist," she nonetheless faced a unique set of challenges by virtue of her choice to work in a site that has long been under the anthropological gaze. Echoing Harding's experience, Maggie Cummings (2005) notes the surprised reactions of her research participants upon learning that she wanted to study practices surrounding women's clothing in Vanuatu, rather than more traditional anthropological interests in this context, such as religious belief or economic systems. "But aren't anthropologists supposed to study *kastom*?" was a common refrain during Cummings's fieldwork in Vanuatu, which, like the Northwest Coast, is a place with a salient anthropological history (52). Key to reflecting on how anthropologists are themselves centred within their own research is understanding and acknowledging what being an anthropologist or ethnographer means in certain cultural contexts. This is particularly the case in regions such as Oceania and the Northwest Coast where the historical legacy of past anthropological and ethnographic encounters continue to mark present and future relationships in powerful ways.

According to representatives that Harding spoke with, Nuu-chah-nuulth and Ditidaht First Nations found themselves approached for permission to conduct ethnographic research several times a year. These encounters were characterised as tales of naive and starry-eyed graduate students, enamoured with classic Northwest Coast ethnology, who approached Indigenous people to learn and write about "their" (with a possessive emphasis) culture. In her encounters with these representatives, Harding learned that many First Nations people question the ability of outsiders to not only understand but also represent Indigenous cultures. This trepidation reinforced their concerns that an outsider's agenda (such as an anthropologist's or a public servant's) will align with their own, which for the last few decades has primarily focused on supporting local Indigenous governance, land claims, and treaty negotiations (Ferrara 2015). Instead of supporting these agendas, these Indigenous communities regularly complain about non-Indigenous social scientists "coming here to tell us our culture" (Harding, private communication).

This is not to say that collaborations between Indigenous peoples and researchers cannot be highly productive spaces for some parties but, rather, that there remain ongoing frustrations in these communities with outsiders "getting it wrong" when it comes to local histories, stories, and experiences (Smith 1999). The ongoing treaty process has made such tensions particularly fraught in the past two decades. In spite of these misgivings about anthropologists, Harding was permitted to conduct research by the Nuu-chah-nuulth and Ditidaht First Nations as she was interested primarily in tourists, outdoor recreation, and the history of outsider visitation in the region. In her work, Harding has incorporated Indigenous practices of place-making (for example, oral storytelling) but has decided not to make Indigenous cultural practices or cultural property such as stories the primary object of her study. In discussing the practice of storytelling as an act of place-making, Harding has sought to destabilise her role as academic expert/knowledge keeper and to reposition her role and work with respect to these interlocutors. By focusing on the nuances of colonial and post-colonial relationships with territorial visitation, local Indigenous representatives approved her access to areas and situations from which she feels she would otherwise have been barred. As was once half-jokingly put to her, "you're okay because you're here to study the white people." Harding's decision to research tourism was seen as acceptable and a welcome relief by people used to being interrogated, often in legal settings, about their language, ritual, and "traditional" culture. Harding's choice to study tourism and not to self-identify as an anthropologist was strategic, reflecting on, and recognising, the local legacies of past research as well as larger discussions around the politics of representation.

Unlike Harding, Long's doctoral research has followed more traditional relationships with interlocutors where her role in the community was as the "local anthropologist." Long first conducted research in Rotterdam, the Netherlands, to understand how local inhabitants were building ideas of community and belonging following what Mosher has described as rising Islamophobia in a country internationally and locally perceived to be tolerant (Long 2015a). Long participated in a Rotterdam neighbourhood over a year-long research stint, enabling her to build rapport with a variety of community members/research participants. In many ways, this doctoral research project reflected the expected norms (or ideals) of ethnographic research, with the anticipation of ongoing connections to this community throughout Long's career. Yet, the ability to conduct ethnographic research in a community where one lives and works for an extended period is arguably no longer how ethnographic methods are applied within and outside of the discipline of anthropology today.

Following her doctoral research, Long embarked on three separate projects with various academic, community, and industry partners. These projects used her ethnographic skill set, yet she could not develop the same sorts of relationships with her interlocutors due to the short timelines of the research projects and the required outputs for her clients. Significant differences for Long in these private sector projects included the process of gaining consent from participants and investigating how particular data sets were accessed. As Sam Ladner (2014, 118) has indicated,

> private-sector ethnographers can underestimate the subtle social reciprocity of gaining access because they usually pay participants for their time. But payment alone is not enough to ensure access is true access; being polite, respecting your participants' space and time, and guarding ethical practice carefully must also be on the ethnographer's mind. Getting and keeping access adds an entirely new layer of difficulty in ethnographic practice. Staying aware of your research agenda while being physically in your participants' context is indeed difficult. But maintaining an even keel while in the field is also critical in order to ensure you will continue to get access to your participants.

Ladner's reflection speaks to Long's experiences working for private industry as a contract ethnographer. For example, working on contract for a market research

firm, Long employed ethnographic methods to explore patients' first-hand experiences with chronic illness on behalf of a client involved in health care provision. Hired for her past experience conducting ethnography, Long was part of a team of ethnographers located around the globe seeking to provide a representative understanding of how these first-hand experiences differed worldwide for the client. Long was flown overseas where – using in-depth interviews and reflexive focus group practices – she conducted research with patients, other health care providers, and caregivers. These meetings were pre-arranged by a third-party contractor, and participants were paid for their time, which allowed researchers and representatives from the hiring firm access to their homes.

As described by Ladner, payment alone was not enough to gain what ethnographers typically consider "true" access to these participants' experiences of dealing with such a sensitive topic as serious illness. Building ethnographic rapport was complicated due to the limited timeline of the project and the method of access to these willing participants. In this scenario, Long's access to the field was brief, arranged third-hand, and not as organic as her previous long-term field research experience. As ethnographers, the temporality of this research is not what we think about as the ideal form of ethnography. Many would not consider this approach to ethnographic methodology appropriate training for students of anthropology in that many of the hallmarks of fieldwork are not included, such as the length of fieldwork and the process of ingratiating oneself in the community. As mentioned above, the ideal long form of ethnography is not the way ethnography is most often practised today – but this does not preclude the more recent application of ethnography in rapid research contexts (Ladner 2014).[5]

Important to this discussion of "true" access is the relatively new phenomenon of labour precarity and the higher unemployment rates of anthropology (and other social science) graduates in the university sector. In this particular moment, taking a year or more to conduct ethnographic research – while a staple of most graduate training – is all but impossible for researchers and students located on the fringe or outside of academia; it is also highly impractical for ethnographers working with short industry timelines and client-focused outputs. The use of ethnography by non-anthropologically trained ethnographers begs the question of whether this lengthy trip to, and stay in, the field is a luxury now enjoyed only by (funded) graduate students in anthropology and so-called "slow professors" (Farr 2016; see also University Affairs 2016). At this moment in time, it is important to explore how anthropologists can incorporate these wider uses of ethnography and the role of ethnographers for hire into their discussions about methodology in all of the practical ways that ethnography is now being used. For example, as a contract ethnographer, Long took part in data analysis and write-up activities as part of a team of ethnographers once she was back in Canada, using anthropological theory to interpret and translate ethnographic insights into client-friendly materials. Drawing on this experience, Long argues that collaborative discussions about findings and use of reporting documents developed from her own and others' qualitative research experiences conveyed ethnographic insights meaningful for the client. Through her past training and her use of anthropological and ethnographic theories to understand her data, Long ensured that her research insights achieved the depth of culturally contextual meaning and experience that are the hallmarks of ethnography. Producing quality scholarship, like that based on lengthy experiences of an extended field-based project, was possible through the collaborative breadth of this project. This included Long's access to both the research and findings of the other globally based ethnographers working on this project (each conducting their own interviews, focus groups, and environmental assessments) and the group's collaboration on data analysis after leaving the field. It is therefore important to try to better understand the ways in which our clients' (and/or interlocutor's) agendas and specific agencies are, and can be, integrated into private and public sector projects like those experienced by Long.

Although each of these researchers faced different issues in how they located and negotiated continued access to their field sites, their experiences in conducting ethnographic research amplify that how we navigate, co-create, and engage with participants in what we describe as "the field" facilitates and directs our studies in particular ways. Through the iterative process of qualitative research, our explorations developed from the choices we made throughout the research process to pursue particular sites, participants, and lines of questioning. Our positions as socially situated actors in complex social fields generate the conditions through which we relate to our (potential) research participants. When we reflect on the agency and agendas of those involved in our studies the structures and frictive encounters that direct and limit our choices as researchers come into focus.

The Researcher as an Object of Scrutiny: The Agency of Participants

As ethnographers, we are often acutely aware of participants' agency in relation to the phenomena under study, but the agency of our participants also directly shapes our work, the questions we ask, the findings we discover, and so on. In May 2015, Pierre Maranda (2015) categorised his panel discussion as "Paleoanthropology," speaking to data he collected in 1966–68 on change and conflict among the people of Lau Lagoon in the Solomon Islands.[6] Maranda used this example to introduce the "observer effect" and the many ways in which anthropologists affect those with whom they work and the phenomena observed. Maranda also spoke of the importance of acknowledging the role of the participants' agency in shaping the data ethnographers collect. Drawing from ethnographic notes recorded by some of his literate informants, Maranda surmised that, like ethnographers, our participants' interpretation of reality, shown through their divergent accounts of significant events in the daily life of the village, are partial. Maranda's discussion spoke to the importance of attending not only to the agency of our participants but also to their agendas; not in a manner that seeks to find an authentic ethnographic reality, but one that seeks to understand why participants wish to take part in our research endeavours beyond simple good will or interest in projects we, as outsiders and researchers, designate as important. Overall, a focus on the partiality of perspective given by any one interlocutor also brings into light the dialogic relationship of learning and shaping realities both during our time in (and after we leave) the field.

This dialogic relationship of learning and shaping reality was also tantamount in Elisabeth Le's (2010) research experiences in Alberta, Canada. For Le, personal relationships established before the start of her research among a Russophone amateur theatre troupe deeply affected the professional relationships that she would later cultivate in this site and the questions she felt she could address in her research.[7] Le's ongoing research project on immigrants' life adjustment through arts began as a private social activity in her position as the spouse of the theatre group's director. Although from the beginning, she had privileged access to information concerning the formation of this community of practice, this access evolved with time. Le's early involvement with the group as the only member not from the former Soviet Union and as "only" the spouse of the newly appointed director (who himself had to earn acceptance in this particular community) placed her in a position "twice-removed" from the group. Since this early contact with the community, she gradually became a peripheral member thanks to her (imperfect) knowledge of the language and due to her regular participation in the community's menial and social activities. With these commitments to the group, she had come to be affectionately called the group's "mother" and "guardian angel."

After a few months of personal involvement with this group, which had earned her growing acceptance among its members, Le declared her professional interest in the theatre troupe as a social researcher. Although initially uncertain as to how her research activities would be received, Le became gradually accepted for both types of involvement – personal and professional. After her interviews with all of the members of the troupe, Le was shown to be one of the privileged few who had not lost her social status by immigrating as an adult, in the way her participants had. This split between a socio-emotional belonging and a socio-professional distance at first seemed to Le to provide an almost ideal insider/outsider position through which to do ethnographic research. In practice, it restricted the scope of her investigation. Primarily drawn to that site because of the actors' remarks, her first intent was to look at whether acting – the opportunity to "live another life," be it only for a few hours – could help these immigrants create more fulfilling lives for themselves in Canada. Did their involvement in the theatre enable them to personify beings they could not in their lives as immigrants? Did this activity allow them to "re-invent" themselves, despite their immigrant status? Such a project meant getting to know the actors personally by learning what they were doing and saying (and not saying) and by observing how these discursive practices were performed and considered by the actors.

The data collected through observations and interviews revealed interesting instances of correspondence between Le's participants' lives on stage and their everyday lives. For example, the complex relationship between two spouses in the troupe was manifested in their play as a couple on stage. Le observed that acting out these roles on stage actually appeared to bring them closer together off stage. The difficulties of a woman in her forties in playing a woman in love on stage might parallel difficulties experienced in her personal relationships. In Le's observations, this role on stage also appeared to help the participant behave more freely in her personal life. To Le, how life "on stage" and "off stage" mirrored, diverged from, and interacted became the most intriguing research angle, yet it also posed several ethical dilemmas. Even though the university research ethics board had authorised her study, Le felt

that her role as a social researcher and her intention to analyse the personal lives of these actors was at odds with her established position as the troupe's "mother" and "guardian angel." Le questioned whether these competing roles and agendas would risk alienating her from the troupe and contradict the very purpose of her (and their) project.

With their shared immigrant cultural background, and as they socialised and worked together toward the production of plays, these amateur actors had become a "theatre family." As such, they told stories, rejoiced, and sometimes quarrelled with each other. In these interactions, they came to reveal sensitive information that informed several facets of their identities, some positive and others less favourable, which are not part of their public image or talked about outside of the group. This information was key to understanding relationships within the group, and, thus, Le first viewed it as valuable in her research into the importance of arts in immigrants' lives. While revealing these details publicly could be considered "delicate" for any researcher, Le concluded that as a group member it could also be perceived as "treasonous." The key to her privileged access to the group (that is, being the director's spouse) was also the key that closed the door to her research as first envisaged.

The problem Le faced in seeking to reconcile her agenda as a researcher with the motivations and agency of her participants has also been an issue of great concern in Long's work. As identified by all authors, the rapport and complex social relationships developed between the ethnographer and our participants – often complicated by the perceived social position of the researcher by our participants – affects the kinds of information we gather. Intimate details shared with the researcher, even when expected to remain confidential, can be useful for probing beyond the surface of the dynamics and relationships we study. Le began her ethnographic study with an interest in understanding how her participants' involvement with the theatre might facilitate their adaptation to a new life in Canada. In the early stages of this research, Le viewed her participants' acting as something in the background and not a key point for the analysis itself. As her project developed, Le felt that her dual position as a researcher and a member of the very troupe that she studied (their "mother" or "guardian angel") ethically constrained her from pursuing the project she had first conceived of.

In this process of information gathering, Le found herself constantly reflecting on the co-construction of the type and degree of her belonging to the group. In negotiating the "observed effect" of her participation in, and obligations to this group, Le was able to productively reorient and redirect her research efforts during the project's first stages. For Le, this has meant redirecting her attention from the confidential experiences of her participants to attending to their agendas in participating in the troupe through to their discussions of the links between their "stage identities" and their "life identities." By shifting the focus of data collection from the individual's personal lives accessed through theatre membership toward the roles they played on stage, Le found that her participants would speak about their adjustment to life in Canada in ways that did not require her to divulge information that was never intended to be publicly shared. In turn, this allowed her to reconcile her research agenda and obligations to her participants through their own agency and expressions. This attenuation to Le's transforming identity in the eyes of the troupe is a poignant example of the "observed effect" as it changes a researcher's role and place within a research context.

Discussion: Revisiting Ethnography Today

Throughout this discussion, we have sought to reconsider the ways in which the agency and agendas of both the researcher and researched are part of the frictive encounters that deeply shape and direct ethnographic investigation. Who we are and how we are situated within the complex social fields that we study matters in how our projects unfold, how we pursue new lines of inquiry, and how we come to the data that we analyse in our ethnography. This question of acknowledging and negotiating diverse, and perhaps divergent agendas in the context of research also finds traction in considerations of how we represent our participants (and ourselves) in our ethnography (Jacobs-Huey 2002, 800). Although we often consider the dissemination of results as a final stage in the research process, this practice also presents a productive space in which ethnographers may consider some of the issues we have raised throughout this article. Long (2015b) has argued that ethnographers could write their results, preliminary or otherwise, for different audiences as a means to interrogate their own, and their participants', biases and assumptions. For Long, presenting her research in the university classroom or during guest lectures for different types of audiences has brought home to her the lessons learned about her role as a researcher and suggested new insights into her findings. She argues this is the case because the audience for such talks – for example, her students – is substantially different than the intended

audience for her doctoral or post-doctoral work – namely, advanced researchers in anthropology and related disciplines as well as community stakeholders such as immigrant service providers in local communities. Following Long's insights, we argue for the benefits of ethnographers acknowledging the role of their identity and their position as a researcher in the eyes of their interlocutors – whether research participants or our research audience – including in the ways we disseminate our research results. Reflecting upon, and acknowledging, the various positions and the agendas we engage and encounter in these social fields will help to make ethnographic investigations relevant for those with (and for) whom we work as well as in academia.

Reflecting on researcher positionality as well as the agency of our participants matters because we work with people who have their own agendas in participating (or not) in our research. Significantly, the relationships we experience during fieldwork, the effects of how multiple agencies and agendas come together in ethnographic work, can have afterlives not yet foreseen by the researcher. These perhaps unintentional legacies of past encounters may generate potential sites of friction for future researchers and their interlocutors to negotiate. Such encounters include not only the relationships built between various actors past and present (for example, anthropologists and other social scientists, our participants and their forebears, colonial administrators, public servants, and so on) but also in the records of these encounters that ethnographers leave behind. For example, for Harding, working in a site and among peoples that have a long (and fraught) history of contact with researchers (like anthropologists) and other foreigners with specific agendas, and negotiating access to certain field data and the participation of certain actors was challenging in ways not experienced in other sites. As Harding notes, such challenges may be due to the expectations potential participants have regarding our research interests (Cummings 2005) or to a (perceived or justified) misalignment of our agendas as researchers with those of the people whom we wish to study (Ferrara 2015; Wax 1971). Alternatively, as Maranda's experience of fieldwork among the Lau in 1968–69 illustrates, ethnographic and other researchers may be welcomed by the local community for the ways in which our interest and approach to local customs serves local purposes or goals.

Friction, as Tsing conceives of it, engages not only the "awkward zone of encounter" but also the potentially generative results of diverse, even divergent, agendas and agencies coming together. This was clear in Maranda's (2015) discussion of how the only white foreigners in the region before his family's arrival in the lagoon had been Christian missionaries who actively shamed the Lau for cultural practices and beliefs that they saw as being immoral and backward. Maranda's ethnographic interest in Lau beliefs and practices was interpreted by the Lau as proof that foreigners could understand how healthy their traditional lifestyle was. At the end of his first period of fieldwork, the Lau Elders explained to Maranda that the recently arrived medical researchers would be welcomed to the lagoon as Maranda and his family had shown them that not all foreigners shared the missionaries' negative view of, and attempts to, intervene in their life and culture. Maranda's work among the Lau underscores how culture and our study of culture through ethnography is continually co-produced – a process that possibly continues beyond the life of the project or the life of the ethnographer. Through the Fonds Pierre Maranda Archives at the Musées de la Civilisation du Québec, Maranda began to digitise the substantial field records from his work among the people of the Lau Lagoon. By May 2015, these records were already being used not only by other professional researchers but also by a new generation of Lau in search of records about their traditional cultural practices. The digitisation of these records and their use by contemporary Lau to meet their own agendas was certainly unforeseen by Maranda in the late 1960s. The remarkable afterlife of this ethnography in these local people's lives highlights how friction is part of the making and maintenance of knowledge through encounters with difference, even when goals and agendas are not shared or even yet imagined.

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Notes

1 Pierre Maranda (1930–2015).

2 This meaning of collaboration is therefore quite different than what is usually implied in the context of activist anthropology or in research that seeks to directly involve participants in the development of the project's direction (for example, participant action research).

3 Classic fieldworkers such as E.E. Evans-Pritchard, Bronislaw Malinowski, and Margaret Mead come quickly to mind here. More recently, Jean Briggs (1970, 19) describes how she embarked on her 17-month field study of an Utku group in the Canadian Northwest Territories in 1963. Although she had brought letters of introduction from Anglican missionaries, Briggs described her embarrassment when the plane that brought her to this remote inlet "began to disgorge [her] gear without so much as a by your leave or any sort of explanation offered to the Eskimos" who, it was expected, would adopt her.

4 Through Edward Sapir's many publications about the Indigenous peoples on the western coast of Vancouver Island, the "Nootka" peoples entered the anthropological canon and became a classic case study taught to undergraduate students. They were named the "Nootka" by none other than Captain Cook, who, the story goes, mistook the word that meant "turn around" in the local language for the name of the local people. This misnomer from a colonial encounter is one among many legacies of past visitors and researchers that Harding encountered while conducting her own research.

5 Rapid assessment and/or action research as a discussion is not included in this article due to our focus on the role and changing relationship of the ethnographer and anthropologist. The authors acknowledge that this would make for a fruitful discussion in a future article.

6 Our goal here is not to speak on Maranda's behalf, as his work does so for itself, but, rather, to bring forth the most representative and important perspectives concerning the role of anthropologists as catalysts in the larger process of data gathering raised during our panel in May 2015 (see Woodhead and Maranda 1987).

7 Le found that studying the discursive practices of her participants and "theatre family" during her recent ethnographic research posed far different challenges than in her discourse analysis of print media (Le 2010).

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