

Research Note: Fieldwork, supervision and trust

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Abstract: *In this research note I reflect upon my different experiences as a researcher with ethnic minorities in Southeast Asia, from my years as a postgraduate student to my current work as a professor. From being a graduate student, to supervising them, I have learned many lessons about graduate fieldwork. Nowadays, I pass these on to supervisees as best I can. I consider it my professional and personal duty to actively warn, inform and prepare young anthropologists about to leave for their masters' and doctoral field research in China, Vietnam and Laos about the field locations to which they are travelling, the political negotiations that they will need to undertake, and the cultural, economic and political differences they will encounter. I also stress that the anthropology that is played out in the field in socialist settings is not necessarily that which we read about in textbooks.*

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What have I learned over more than 20 years of practising fieldwork and ethnography among highland ethnic minorities of Asia, with 15 of these years devoted to socialist countries? What can I tell younger generations that could be of help to them in understanding the issues at stake when organising their field research? This note begins with reflections upon my own graduate work in Thailand in the early 1990s. Later, a move from Thailand to socialist Laos, Vietnam and recently to China opened my eyes to sharp contrasts in fieldwork conditions and the negotiations required with all kinds of state agents simply to conduct research. In a nutshell, throughout, I assess that trust is a core element of the research process.

The author as a keen postgraduate student

It was in the early 1990s that I embarked on my doctoral fieldwork in a remote minority village of northern Thailand. Of my 'preparation' for that journey, I can now declare that the best thing was that I did not know much about the local situation I was about to enter. This may seem somewhat paradoxical, so let me explain.

In the French anthropological tradition in which I was educated, discussing fieldwork in

detail prior to the task was considered trivial by many of my professors. It must be remembered that in French anthropology, unlike other Western anthropological traditions, before Marcel Mauss published his *Manuel d'ethnographie* in 1947, fieldwork was seen as a rather mundane activity, better left to colonial administrators, missionaries or waged assistants equipped with rudimentary ethnographical training (Mauss, 1967; Stocking, 1991). The colonial times over, data collection was still a necessity, and the researcher finally had to do it him or herself; but it still occupied a minor part of anthropological research. The latter was more focused on crafting a stylish theoretical discussion, preferably with strong philosophical overtones; therein lay the authentic talent. During my training years, I especially remember the lectures of a professor who argued with unshakable poise that being in the presence of the subjects in their exotic locale, becoming accepted by them and extracting the relevant data from their daily life was simply a matter of showing the right amount of personal moral fibre. Learning more precisely *how* to do that was not actually a subject matter; what really counted was to hold the right kind of character (from birth? From God? He did not tell). This

could be one way of accounting for my earlier statement that not knowing much about the local situation was ‘a good thing’. But this is actually not what I meant.

I studied anthropology at universities in French-speaking Canada that boasted no specialists of either Thailand or minority studies in Southeast Asia. In those years, that was not considered cause for concern, the essence of superior research being elsewhere as I just mentioned. True to this vision, my doctoral department did not provide any graduate coursework on fieldwork methods or research design. I must say – to my utter disappointment today – that I was also convinced that at age 35, with a professional past that included rubbing shoulders with many kinds of youth offenders and having performed daily miracles as a tour guide in faraway places, I would be more than able to cope and show ‘the right moral fibre’.

Much like those clueless missionaries sent to French Indochina in the late 19th century, to whom I devoted a book a couple of years ago, the research for which had been a splendid opportunity to be reminiscent of my own ignorance as a young ethnographer, I arrived unabashed in the field with a couple of scholarships in hand, grand ideas, a modest methodological toolbox and only a slim idea of what Thailand, the country where I had elected to live and conduct fieldwork for a year, was about. Trusting that what I had learned back home was important and comprehensive, I was convinced that I would conduct good ethnography. The exercise would require me to stay in one highland minority community – ethnicity to be determined at a later stage – for a lengthy period of time and conduct an in-depth study based on participant observation. My past experience in ‘adventure’ tourism inclined me to adopt tourism as an entry point to study the impact of modernisation in that village.

So why was it a good thing that I did not know much about the people I was about to study? Because if I had known from the start as much about the local situation as I did after one year, to be honest, I would have freaked out. The perspective of facing that ordeal without closely focused preparation or much institutional support would have been too much to bear. It was only *after* the fact that I grasped more fully what had not been told to me beforehand, that

is, the range of repercussions a foreigner residing amongst a highland minority Hmong community in Thailand can trigger during a period when forced sedentarisation and the phasing out of the opium poppy cash crop were in full swing. It was already late in my fieldwork when I came to realise that for a few months – that is before my behaviour finally laid their worries to rest and allowed some trust to take over – I had been seen by my hosts as a spy sent by the Thai state and its American friends to check on their communist sympathies. Only then did I finally get a picture of all that the village was trying to conceal from the authorities, from unreported murders, kidnappings, possession of military weapons, the hiding of illegal migrants from Burma, to the production of vast amounts of opium and its trade, and the fact that doubtful characters were constantly congregating in the village to get high. Oh yes, and how ridiculously cheap it was in Thailand to hire a gunman to have a quick chat with anyone you did not like; the forested trail to the village was long and seldom busy, and the monsoon was incredibly effective at wiping off traces of goods or folks gone missing. Only then did I realise that without investing time early on in developing a trusting relationship with my reluctant hosts, the situation could have turned unpleasant. Likewise, only then did I come to realise how much of a liability I must have seemed to these non-Thai, non-Buddhist, wary minority people wrestling to cope with harsh living conditions and an authoritarian state bent on turning them into something they did not exactly wish to be.

But I lived to tell the tale. I returned home, wrapped up my thesis – which turned out to be significantly less classy than I had initially fancied – and I became a certified anthropologist. I got promoted to the rank of bona fide academic, and I found myself in a position to expand my research horizons.

The green academic

My fieldwork experiences with graduate research in India for my master’s and Thailand for my doctorate had led me to reach what I thought to be solid conclusions regarding how one conducts field inquiry in Asia. For instance, obtaining the necessary official research permissions in these two settings had

been a formality, even if not always a speedy one. Meeting with local academics in Delhi or Bangkok to 'pick their brains' and discuss research was realistic and gratifying. Access to field locations was only hindered by physical distance and what James Scott (2009) calls the 'friction of terrain'. Furthermore, thanks to the well-ordered and dependable civil society that prevailed in these two countries, the subjects in the field could always decide to speak their mind or, on the contrary, refuse to talk to me based solely on their assessment of where their best interests lay. Repeated visits posed no particular problem, nor did moving field location, finding additional informants, exploring documentary collections, archives and libraries, or physically getting the data out of the country and back home safely.

It was with this state of mind that I made my first visit to Laos and Vietnam in early 1995. At that time, America-Vietnam diplomatic relations had not yet been restored (that happened later the same year) and the obvious distinction between Thailand, which was familiar to me and unambiguously comfortable for the overseas researcher, and these two socialist countries was the latter's comparative lack of options in every aspect of daily life, starting with food and lodging. Professionally speaking, what immediately struck me as the main distinction with what I had known before were the unusual work relationships with local colleagues and the unexpected fieldwork conditions. In both Laos and Vietnam, my institutional interlocutors were not academics any more but state officials. Their role, in addition to filtering who would or would not be allowed in the field, was also to keep an eye on all contacts between me, as an outside researcher, and the locals, be they urban dwellers in the capitals or peasants in the hills. Field sites had to be vetted, often chosen, by these state agents, as well as the duration of the stay (a matter of days), the dates of the visits (as few as possible) and the exact identity of the informants, who more often than not turned out to be local officials and People's Committee spokespersons.

This first step passed, authorisations to go to the field could take weeks to be delivered and if they were, restrictions were attached such as having to report to the police and to provincial, district and communal authorities before talking

to any potential informants. These local level authorisations could also take weeks, or simply not materialise depending on the local political situation and a collection of other circumstances I was not to be told much about. 'Colleagues' specially selected by the state and sent with me to the field were actually paid – by me – to monitor my activities and report back to their superiors on the phone day by day. They also doubled as interpreters, actively directing the flow and content of information between informants and me. Needless to say, the few local peasants I was eventually able to meet and talk to were perceptibly aware of the eye of the state on them and their answers to my questions, conveyed by my 'colleague', were invariably succinct and unrevealing. I could tell that for these informants, trust was not part of the picture; those stressful moments were instead conducive to unease and concealment.

I quickly came to appreciate the limits such a structure was going to impose on my research objectives, and I started doubting the concrete value of working within such constrained research conditions. In essence, I was faced with a choice of three directions: give up, move on and hope things might get better later; take on the current arrangement and challenge its limits; or 'go with the flow' and see where that would take me. I confess I did test the system a bit, which led nowhere, and thankfully not to the local gaol either, a fate a few of my more intrepid colleagues had the opportunity to experience first hand. All the same, I was attracted enough by the prospect of working with some of the poorly known 12 million or so highland minority people in these two countries – compared with a well-researched half-million in Thailand – to decide that going with the flow was worth a try, which is what I elected to do.

The professor

Since those early years of getting *au fait* with anthropological fieldwork in socialist Asia, I have continued research in Vietnam and Laos. Soon, I was also sending graduate students to conduct their own fieldwork there. In the meantime, the Lao and Vietnamese regimes have somewhat mellowed towards Western anthropologists labouring on their soil, although still not to the point of allowing unrestricted access

such as what Western anthropologists consider normal for academic research elsewhere. Lately, I have also ventured into Yunnan and Guizhou in southwest China. Conditions for research there are virtually the same as that in Vietnam and Laos, albeit perhaps slightly more relaxed, thanks presumably to China's confidence in its own might.

In 2006, Luong Van Hy (2006), a diaspora Vietnamese anthropologist working at the University of Toronto, stated that authentic ethnographic fieldwork was not yet possible in socialist Vietnam. Luong was right of course if one considers authentic fieldwork to be what the discipline had devised in the West. This heritage is based on particular historical conditions within democratic regimes, while more often than not, the locales for field research were once important colonial settings where the practice of ethnography was pioneered and thrived for decades (Stocking, 1991; Pels and Salemink, 1999). Yet, for me and many of my Western colleagues working in China, Vietnam, and Laos, anthropology is not solely about being classical and fitting normative models. These colleagues and I, including the contributors to this special issue, share the opinion that academic anthropology has something to learn from its encounter with socialist countries. In particular, I judge that fieldwork that adapts in creative and productive ways to restrictive local conditions such as those in socialist Asia is still worthy of our consideration, even when it appears to challenge established wisdom. If we were to restrict ourselves to what is commonly held as the 'true', that is, Western forms of ethnographic fieldwork, a large proportion of the world's population would remain outside of our scope. Indeed, to me, anthropological practice has always been dynamic and experimental, and here is a fabulous case for testing further its versatility.

In short, the Western tradition of ethnography needs to find a way to adapt to and fit the challenges on the ground in socialist Asia. One instance of this possible acclimatisation is multi-sited ethnography (Marcus, 1995). Despite the fact that I am not a strong advocate of this type of ethnography as a mainstream form of fieldwork to be promoted to graduate students, I do see its value when long fieldwork periods are not readily possible, and political

conditions (communism, but also other sorts of authoritarian regimes, war zones and failed states) make it virtually impossible for an outside observer to be based in one given community and conduct a more mainstream form of inquiry. I do not expand further on this theme here, but several other similar examples could also be brought in (see McKinnon, this issue).

The concerned supervisor

Nowadays, when discussing potential fieldwork settings and circumstances with graduate students preparing to embark on their research in China, Vietnam and Laos, I explain that mainstream anthropological principles will not apply untouched for their work lying ahead. To make the most of their options within a controlled and politically sensitive context, I emphasise the importance of reacting creatively to local conditions, of being reflexive at all times and of banking on trust.

Reacting creatively to local conditions involves, first, understanding the context, then assessing what the possible connections with one's own project may be, and focusing on the locales and connections that seem the most promising. Western-type ethnography may not yet be easy to undertake in socialist countries; yet, a large number of Western and Western-trained anthropologists are active in Laos, Vietnam and China; a recent increase in scholarly publications proves that eloquently. Rather than tampering with the very intellectual and moral cores of their disciplines, or compromising to the point of irrelevance, these pioneer anthropologists have realised that in order to conduct fruitful research there, one must find ways to adjust, circumvent and sometime bend the rules in modes that are somehow acceptable to both sides. Critical reflexivity (Prattis, 1996; Robertson, 2002) is a necessary safeguard to avoid the risk of naivety and a lack of self-awareness that can lead to serious trouble, for research subjects as much as for the researcher. Critical reflexivity is a foundation for responsible positionality (Rose, 1997) and, together with accountability, is a vital ingredient for engaged research (Scheper-Hugues, 1995). Trust, then, reigns supreme (Fukuyama, 1995; Dasgupta, 2000; Magolda, 2000). In China,

Vietnam and Laos, this is also called 'friendship'; in Western academia these days, we like to call it 'social capital'. Without a doubt, and wherever we are, making friends among our research subjects requires gaining their trust, or they will otherwise remain reluctant participants. However, just as importantly, trust must also involve state officials and collaborators at all levels – academics, interpreters, librarians, guides, jeep drivers – who are far more likely to agree to help once they recognise one is a trustworthy person. To all these friends, official rules still remain a constraint; but with trust, ignoring or bending the rules in ways that are tolerable to all parties almost magically becomes a distinct possibility.

Yet, there have to be limits to what friendship and trust can achieve. Socialist politics are predictable when it comes to restricting access to areas and peoples judged by the state to be temporarily or permanently disloyal – untrustworthy. In these three countries, security concerns run high in border areas where the majority of highland minority groups dwell by custom, and national political irritability often descends on people on the geographical, cultural and ideological fringes of the state and the Nation. Consequently, literally overnight, outside anthropologists have abruptly been disallowed to continue their research, let alone start new projects, in areas such as Tibet and Xinjiang, the Central Highlands of Vietnam and the former Xaysomboun Special Region in Laos. And there is nothing friendship can do about it.

Of the advice that I give supervisees, I stress above all that stepping outside the boundaries of what a foreign friend is sanctioned to do is a sure-fire way to see future requests for access denied. Local colleagues who trust us have a degree of agency and margin to play with, they can open doors, shrink delays, and they can vet for foreign researchers when the police call them in to check-up on one. Still, being part of an authoritarian regime that also regulates their security, salary, promotion, and health benefits imposes limits on what local friends can realistically hope to accomplish for/with the foreign researcher. On the other hand, one's own irresponsible behaviour can severely harm them as well as their family. Let me bring up a straightforward example: a foreign researcher might

decide to draw on his or her friendship with a local counterpart to lay eyes on sensitive documents. But instead of simply reading them and handing them back, perhaps he or she digitises them and copies the files onto a computer hard drive. A search of that computer, conducted by officials at the airport or border crossing, not an uncommon occurrence, then exposes not only the researcher but also his or her local counterpart. The prospects of the latter being able to help get the guilty party out of trouble at this stage are slim, as are the local collaborator's own chances of escaping the administrative wrath for him or herself and his or her family. In such a case, the researcher's behaviour engages (and endangers) not only him or herself, and the future capacity to pursue field research, but an entire household's well-being as well, and possibly the freedom of some of its members.

In short

I learned a few things from my own time as a postgraduate. Now, I work with master's and doctoral students who put their faith and trust in my capability to lead them through a meaningful and rewarding graduate experience and to a positive outcome, and I cannot plead ignorance. This has meant that ethically, I have decided to avoid accepting postgraduate candidates who intend to undertake long-term fieldwork in a location or a cultural context I have little acquaintance with unless someone familiar with it joins the supervision team. Furthermore, I believe that sending students on extensive fieldwork journeys in socialist Asia without making sure they first undergo preparation with detailed information on what is to be expected on the ground would be a show of negligence. It is my personal and professional obligation to ensure the success of their endeavour in the field as much as I possibly can. As a consequence, I have made it my responsibility to develop the local institutional linkages necessary for these students to be able to obtain research visas and find support when they need it. I also make trips on location when I can see with my own eyes how they are faring. Upon their return, I encourage students to pass on functional information and field experiences from one generation of graduate students to the next.

In sum, I mean to give graduate students reasons to trust me. In return, I trust them. Then, I can entrust them to my dependable friends in the field in socialist Asia. That of course does not positively guarantee that they will succeed, but their odds might become higher than mine were at the same stage.

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