Article

Ethnography

Spies like us

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Abstract

The discipline of anthropology recoils instinctively at the idea that its researchers' labor might contribute to the national security state; other disciplines celebrate the same contributions as evidence of policy impact. In this article, we examine the seductions of espionage for professionally vulnerable (untenured) researchers that employ ethnographic methods but are operating in the shadow of market incentives and the Global War on Terror. We define "extreme fieldwork" as a research design likely to yield the kinds of data that Price identifies as "Dual Use Anthropology." The bulk of our essay is devoted to providing warrants for the claim that there are strong incentives to brand oneself as an "extreme" fieldworker – which may be the post-9/11 equivalent of chasing what Trouillot called the "savage slot." We argue that for some topics in certain research settings, uncomfortably, the more care and effort one invests in ethnographic best practices, the more likely it is that the researcher will engage in behaviors that could be confused with spycraft.

Keywords

activism, ethics, fieldwork methods, research design, risk, security bureaucracy

> *There are many parallels between anthropological fieldwork and espionage. Both involve looking, listening, eavesdropping, taking notes, recording conversations, snapping photos, and establishing trusted confidants. We call it participant-observation; they call it spying. (John Borneman and Joseph Masco, 'Anthropology and the Security State')*

> '*It's going to be tough for her to compete with Somali pirates and Jihadis!' (overheard on the academic job market, 2014)*

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Slow and painstaking fieldwork is often the only way to uncover hidden dimensions of, and allow deeper engagement with, dangerous social worlds. Graduate students who demonstrate a capacity to transverse language and class barriers, and convince locals that they are trying to 'get the story right', have been traditionally rewarded with professional recognition in disciplines utilizing ethnographic methods. As early career researchers who are also mentors and advisors to high-risk PhD projects, we (the authors) face a conundrum. On one hand, we feel duty-bound to support students engaging in projects that we see as likely to offer original contributions to scholarship and position our students to be competitive on the job market. On the other hand, we find ourselves grappling with a host of issues that arise from 'extreme fieldwork'.[1] This collaborative essay is an earnest attempt to synthesize some of the implications of this kind of extreme fieldwork that were not obvious to either of us at the early stages of research design, but are now some of the main things that we think about as advisors. We can follow Joanne Passaro (1997, 147) in questioning "how subjecting oneself to physical danger might still be a rite-of-passage aspect of fieldwork [that continues] to romanticize the 'young' ethnographer and his/her ethnographic project."

This essay began as an interdisciplinary reflection on 'extreme fieldwork' from the perspectives of ethnography in anthropology and comparative politics. Our discussions underscored a taboo subject: that some of the status that both of our respective disciplines confer on the type of work that both of us have undertaken is based on a risky gamble. The gamble is especially vexing because real harms would be borne not by us but by local affiliates who do not have the social protections afforded social scientists. While participant observation can feel like "couch surfing and observing", as one colleague put it, in some contexts that ethnographic access jeopardizes the safety of our generous hosts, friends, and assistants: We get to do this only because we are backed by Western research universities and political institutions.[2] Rather than trying to develop ethnographic methods that are appropriate to risky research settings – topics that have been well-covered elsewhere (Tittensor, 2016; Sluka, 2012; Goldstein, 2014; Theidon, 2014; Felab-Brown, 2014) – we aim to step back and consider how to advise students on research design for projects crafted around extreme fieldwork. Our practical suggestions are meant to initiate a difficult conversation. Power differentials between researcher and subject may be inevitable, but they are especially potent when researchers are assumed by observant locals (who may not be one's human subjects) to be conducting social science in the service of the national security state.

More broadly, given the prestige and policy-relevance attached to scholarship that describes and models social worlds that are widely stereotyped as unruly, corrupt, or ungovernable, it might be worthwhile to wonder, in a public forum, whether the current incentive structure is adequate – or ought to be – and if the current regime is going to be sustainable. Michel-Rolf Trouillot's (1991: 48) work on the demands of creating 'publishable' and tenure-worthy books within the thematic universe of Otherness inherited by anthropology confronted us with the prospect of 'ABDs relegated to cab-driving status when their lines could not see

the light of day'. Is extreme fieldwork chasing the same old alterity – the 'savage slot'[3] of the contemporary War on Terror? And at what price?

We started our careers doing research under the shadow of authoritarian regimes in Central Asia and the Caucuses and in a notorious contraband hub in Latin America. Ethnography in these sites invites a level of detail and intimacy with human subjects, and the cultivation of networks with extra-legal entities, that is really quite unwelcome from the perspective of the security bureaucracy of the host country (Verdery, 2014). The categorical confusion between ethnography and espionage is foregrounded for the various actors around the world who follow US security policy with careful attention and interest – and reasonably so (see Borneman and Masco, 2015).[4] The intimacy demanded by ethnography, and the decision to pursue research agendas that involve collecting qualitative data from criminals, terrorists, rebels, militia members, gangs, and the like, even when all goes well, bring social scientists into close proximity to real danger.[5] The multidimensional relationships and activities ethnographic researchers develop in order to stay safe often, functionally, mean flaunting the inability of state security services – often supported by professional social scientists – to enforce the law. It can, and does, raise questions about researcher motivations that are not easy to answer. Meanwhile, the existence of many thousands of credentialed social scientists whose existence depends on grants that originate in the military, or salaries and pensions guaranteed by state security services, is a constant reminder that the knowledge we cultivate about distant societies can be repurposed for seek-and-destroy missions. We are reminded that the American Anthropological Association would not need to issue an ethical statement prohibiting covert work with the military if professional anthropologists were not doing just that.

The uneasy relationship between anthropology and the security state has been thoroughly discussed by researchers such as John Borneman and Joseph Masco (2015). Their analysis of state and global security apparatuses challenges us to ask: How can we register the changing relationship between ethnography and state surveillance activities 'in our efforts to understand another person or culture without succumbing to the unmooring effects of working in our era of terror politics' (Borneman and Masco, 2015: 781)? As a provisional answer to that question, Borneman and Masco conclude that, 'As anthropologists respond to these changing conditions, we might hope that they, we, continue to choose objects, relations, sites, and projects that *engage security risks* and *resist the seductions of espionage*' (p. 785, our emphasis). We use our position as junior scholars to reflect on the particular stakes of these issues for early career researchers. We find that the answer may be much more ambiguous (and, to the extent that our analysis is correct, frankly quite a bit more disturbing) than their injunction: 'Don't be a spy.'

First, we examine the seductions of espionage in a global research context where ethnographic fieldwork presses at the disciplinary boundaries of the qualitative research. In our analysis, the seductions of espionage gloss several interrelated processes: the appeal of extreme fieldwork that might be misconstrued as spycraft, the ambiguous collaborations that make dangerous fieldwork possible, and the

uncertain career options that can transform fieldwork that is conducted as critical academic scholarship ex-ante into espionage ex-post (perhaps years down the road). Given the polyvalent meanings of both seduction and espionage, we use our cross-disciplinary conversation to consider the broader career stakes of both pro- and anti-state ethnographic collaborations (p. 784). While the long history of reflexivity in anthropological practice may help guard against these seductions, there are also aspirational scholar-practitioners who are completely aware that they are learning strategic languages, and developing an ability to speak credibly about silent societies, because they are building resumes for potential work in the intelligence community if the academic job market does not pan out. More than we admit, state agents are often ethnographers' co-travelers and facilitators. Second, we examine two dimensions of engaging security risks: 1) focusing on the security state as a research object; 2) undertaking research in dangerous social settings. We suggest that a third form of engaging – via mentoring and public discussion on research design and methodology – will be crucial for ethnographers navigating dangerous and politicized fieldwork settings.

The seductions of espionage

The constitutive relationship between ethnographic fieldwork and imperial expansion goes to the heart of disciplinary anthropology and has been a robust topic of discussion. Leading anthropologist Franz Boas initiated the discussion with an early warning in 1919 about 'Scientists as Spies', published in *The Nation*, for which he was censured by the American Anthropological Association (reprinted in Boas, 2005; see also Borneman and Masco, 2015: 784; Price, 2012). Since then, disciplinary anthropology has attempted to maintain distance from the various national security services both at home and abroad (Sluka, 2010). However, the long history of military appropriation of anthropological knowledge documented in David Price's research on the Human Relations Area Files in the 1950s and M-VICO project in the 1960s attests to the fact that the original producers are often unaware of what is happening to their data (Price, 2012: 20). Given that ethnographic fieldwork is claimed as a core methodology of social science disciplines beyond anthropology (see Ingold, 2014; Schatz, 2009), it bears reflecting on how ethnography is discussed more broadly. Our interdisciplinary discussion of fieldwork methodology revealed that the conversation about fieldwork ethics has been somewhat less robust in comparative politics, even though the field produces cultural experts that can, and often do, collaborate with the military, domestic and international intelligence services, or both (González, 2007).[6]

But even within the field of anthropology, popular reporting – often focusing on marginal research at what might perhaps be described as the fringe of disciplinary norms – paints a more ambiguous picture of collaboration between ethnographers and various national security services. It is telling that of the three articles specifically addressing qualitative fieldwork in *The Chronicle of Higher Education* in the past year (February 2016–17), two were about white male ethnographers in

militarized contexts (Bartlett, 2016; Voosen, 2016). The third article, written by a doctoral candidate in American studies, Mingwei Huang, offered a direct challenge to the romance of risky fieldwork by addressing its gendered vulnerability. In Huang's framing, perceptions of fieldwork risk, on the one hand, reward a type of 'muscular' masculine ethnography while, on the other hand, stigmatize gendered vulnerability (Kulick and Willson, 1995 ; Willson, 1995; see also Moreno, 1995). After analyzing her experience of sexual assault in the field – research undertaken while studying mafioso Chinese businessmen in Johannesburg – Huang (2016) offers the following reflection:

> When ethnographers can access and immerse themselves in worlds unknown, such as illicit ones, their work is valued and rewarded. Within the academic version of celebrity, the risk-taking, intrepid, normatively white and male ethnographer is a star. The price that many ethnographers pay in pursuing their fieldwork is not always recognized, and rape carries a particular stigma.

We share Huang's sense that the work of ethnographers who can access and immerse themselves in dangerous fieldwork settings is compared favorably to the Malinowskian archetype of the lone ethnographer who displays courage and perseverance as a disciplinary rite of passage (see Seizer, 1995: 74; Sanjek, 2014). We also agree that both the rewards and potential harms are – often invisibly – gendered, due in large part to the individualized heroic narrative tropes that frame extreme fieldwork.[7] The individual ethnographer is thought to shoulder both the risks and rewards of field research (Kuklick, 1997). It would be difficult to prove whether the prestige of fieldwork in risky settings is empirically borne out in academic hiring. However, it is worth noting that Huang's argument about the prospective career payoff of risky fieldwork was published in *The Chronicle of Higher Education*, a key portal for academic job-seekers.

In public forums, it is not hard to find the version of academic celebrity criticized by Huang. The epitome of extreme fieldwork is described at length in *The Chronicle*'s long-form journalistic review of Scott Atran's work on violent conflict, published under the title 'The Road to ISIS: An Unorthodox Anthropologist Goes Face to Face with ISIS Is the Payoff Worth the Peril?' Weighing in on the payoff of perilous fieldwork, the essay concludes with a resounding, 'yes!', suggesting that 'scholarly quibbles aside, there is near-universal admiration, bordering on awe, for how Atran is able to collect data in the midst of a violent conflict' (Bartlett, 2016). On the one hand, Atran's own descriptions of his methods only bear superficial resemblance to what readers of this journal would recognize as rigorous ethnographic fieldwork.[8] On the other hand, Atran's research is published in prestigious anthropology journals such as *Current Anthropology* (Atran, 2016), which indicates that his extreme fieldwork is indeed rewarded with disciplinary recognition. Meanwhile, the *Chronicle* lists off the arms of the US security apparatus that have sought out Atran's expertise: the White House, Congress, the UN Security Council, the former CIA director, and the former Prime Minister of Britain

(Bartlett, 2016). And for the professionally-vulnerable untenured members of academic disciplines, under constant pressure to demonstrate 'impact', it is hard to ignore that Atran's example generates impact that is easily legible to deans and administrative committees via indices like Google Scholar. The work also yields countless 'Indiana Jones stories' that sell well to large undergraduate lecture halls, filling course rosters and providing a boost to course evaluations.[9]

The allure of this kind of work was not lost on either of us. The dissertation prospectus that eventually produced the empirical data for Schuster's research was a straightforward plan to use ethnography to document how life works in the smuggling networks that traversed the Tri-Border Area in Latin America. In a discussion of methodology (post-candidacy, prior to fieldwork in Ciudad del Este), a senior advisor asked wryly if the plan was to 'find' Paraguay's contraband economy 'by standing on a street corner with a machine gun to ply your trade as a participant-observer'. Much later, when an unexpected encounter in the field gave the author the opportunity to conduct interviews with smugglers and gangsters, she decided that doing so would put her network at risk. There is no question that proximity to these networks lent a richness and depth to the theory-building that could not be replicated without ethnography. The dissertation prospectus that led to Driscoll's research was similarly self-aware (about a desire to use one's 20s to go 'find the edge' of governed spaces) and also similarly naïve (about how difficult the experience would ultimately be). The decision to spend extended months conducting fieldwork in Tajikistan in the years when NATO forces were attempting to stabilize Afghanistan, just across a very porous border, raised reasonable questions about the true motives and identity of the researcher.[10] In a memorable conversation, Driscoll was accused by a Russian military officer of being an agent of the U.S. government (in the normal course of an interview). Driscoll presented his credentials – business cards from graduate school and the like – and the officer responded: "Of course I believe you are a Stanford graduate student. That just means you'll go in as a G-13." In the end, after returning to the United States and coming to understand that he was not crazy to fear that there was a demand for his microdata from within the national security apparatus of his government, Driscoll decided it was necessary, in order to limit his future liability, to destroy many notebooks in which anonymous subjects had voluntarily divulged other people's names in an effort to help the author map the human terrain and model consolidation.[11]

Since the disciplines of economics and political science pride themselves on the production of actionable policy-relevant knowledge, and heap rewards upon scholars capable of maintaining dual careers as policy practitioners, qualitative area studies knowledge – which requires really learning languages and cultivating long-term trust-based relationships with subjects– often safeguards its privileged niche with the claim that area expertise is necessary to provide guidance to policymakers. This can mean engaging in research that complements the functions of the intelligence community or engaging in research that is explicitly prescriptive with respect to high-stakes social policy issues. All of this is to say that there are simply no

taboos in political science or economics – none at all – about using one's graduate school training to acquire critical languages, collecting high-integrity data via original fieldwork, trying out the academic job market and, if it does not yield academic rewards, going to work for a federal agency.[12]

It is tempting to follow in Franz Boas' footsteps and use a public forum to spotlight – and criticize – ethnographic research that crosses or blurs the line to covert intelligence work. However, righteous condemnation of scholarship that produces knowledge relevant to the security state has two disadvantages.

First, it leaves the wider scholarly and professional appeal of extreme fieldwork largely un-interrogated. Painting with such a broad brush obscures, for instance, that a great deal of extreme fieldwork, and almost certainly the majority within anthropology, aligns more closely with *anti-state* interests than covert work for state security services. Indeed, Borneman and Masco highlight the important ways anthropologists have been enlisted into advocacy roles that require them to 'take sides', often in fluid settings where 'the conditions of collaboration [frequently] change once in the field and upon return from fieldwork' (Borneman and Masco, 2015: 784).

On a personal level, many – or likely most – social scientists are invested in gaining a deeper understanding of the experience of oppressed and victimized communities (cf. Gusterson, 2017).[13] Indeed, anthropologists who have transitioned to ethnography from a sustained advocacy role suggest that, 'as an anthropologist engaging in activism and with activists, it's a negotiation that is continuous, with others and with [themselves]' (Ross, 2017). As anthropologists such as Hugh Gusterson have noted, there are few professional incentives in anthropology to check the tendency towards over-correction in a progressively more anti-state direction. In fields such as political science, the trade-offs include a willingness to forgo certain grant opportunities and possibly future security clearance. There are certainly some intrepid practitioner-scholars, however, especially young scholars at the beginning of their research career, who make the decision to go to extreme settings in the first place out of a charitable desire to help and to engage personally with a relevant and timely topic with real world importance, and only secondarily by a desire to send what political scientists would call a 'costly signal' to the academic market. Some engage in a hybridization of social science and activism and authentically want to help locals fight injustice, resist state domination, or empower themselves.

A second disadvantage is that retreating to well-rehearsed scripts that pit caricatured 'co-opted intellectuals' against 'authentic advocates for social justice' squanders valuable energy that we think is better spent identifying shared best practices for qualitative research in dangerous settings. Debates over the requirement to 'take sides' access longstanding anxieties about strategic funding priorities – from complicity with colonial administration to Human Relations Area Files and Cold War era Title VI FLAS language training (see Guyer, 2004; Price, 2012) – have only intensified in the contemporary moment when debates about national security have been shaken by the possibility of mass-casualty terrorism. And while

this essay is calibrated towards the professional dilemmas faced by us as junior faculty and novice advisors, Scott Atran's late-career pivot from researching relatively obscure folk biology to high-profile political and religious violence suggests that this quest for scholarly recognition does not just afflict junior scholars. In light of the career rewards of risky fieldwork, it is useful to weigh its potential consequences.

Engaging security risks

So far we have discussed the perception – whether misplaced or not – that extreme fieldwork will be rewarded by disciplinary recognition. To weigh the consequences of this trend we attempt to widen the lens to consider some of the ethical challenges posed by extreme fieldwork. Anthropology has long relied on complex relationships with field assistants as the core methodology of participant observation.[14] However, as part of the wider reflexive turn in anthropology, Roger Sanjek (1993) has warned that the under-theorized relationship of 'assistants and their ethnographers' was part of anthropology's hidden colonialism. In the ensuing 25 years, a wide range of theoretical and methodological works have spotlighted the ethical and interpersonal implications of close ties to local communities. Some projects are explicitly framed as 'engaged research' (Low and Merry, 2010; Aiello, 2010) while others take a wider view of collaboration (e.g. Sanjek and Tratner, 2015). The very fluidity of these interpersonal relationships makes it difficult to pin down anthropology's 'methodological stance of privileged witnessing' (Kuklick, 1997: 63).

One of the points of entry for this collaboration was a shared appreciation by the authors of the quandaries our fieldwork assistants continue to face, even after the 'privileged witness' is long gone. The idea that our presence might *intensify* these quandaries poses a direct challenge to the anti-state bias that is implicit, and sometimes explicit, in most national traditions of ethnography (Samimian-Darash and Stalcup, 2017).

The primary problem is that in the semi-authoritarian social settings where we have conducted work, espionage, treason, 'insulting the president' and the like, can be charged retroactively and carry long jail sentences. Katherine Verdery's (2014) research in Cold War Romania highlights the long afterlife of security files, epitomized in the disorienting experience of confronting her own file years later. One of the major contributions of her work is to track the security file as an ethnographic object without vilifying the state bureaucrats and institutions that were systematically collecting information on her, as they surveilled communities she was studying for the purposes of maintaining social order. An obvious, but ethically uncomfortable, second-tier implication of the long afterlife of the artifacts of the security service is that there may well be implications for research assistants or implementation partners as well as their ethnographers. Neither practices of informed consent (since those at risk are not always the 'subjects' of the research) nor the multifaceted 'rapport' approach advocated by ethnographers flying under the radar of security services (Tittensor, 2016) address this potential area of harm.

Especially as more and more states require pre-approval of research in order to grant research permits and visas (often under the wider pretense of 'human subjects protection'), engaging in certain kinds of scholarly projects may put our assistants at risk, perhaps even years down the line.

Given this wider set of ethical issues, we find it troubling that these crucial research design issues are too often cast as researchers' personal or psychological dilemmas. Our general sense is that the most common way of dealing with the double-binds that are inherent in ethnographic research, when it occurs against a backdrop of surveillance and authoritarian political systems, is to be satisfied with an *ex post facto* sense of relief that everything turned out alright, as far as we know. Some of us, in our honest moments, also share, with each other, a sort of sheepish sense of having gotten away with something. We would experience (sometimes, after the fact) a guilty sense of longing for the rush that came from escaping close scrapes – and these 'war stories' are a kind of currency that qualitative researchers use to assess each other's credibility.

We also emphasize Borneman and Masco's (2015) cautionary note that the relationship between ethnographic research and the security apparatus persists long after fieldwork and even the publications that disseminate its findings. The language of seduction that we employ earlier in this essay is surely appropriate, here, as a type of seduction that can be deferred into the future based on successive career decisions (a 'Plan B' that may never be actualized). Driscoll employed ethnographic methods to study post-conflict politics in both Georgia and Tajikistan, but he made a set of relatively conscious decisions to work a little harder with his Russian and Tajik (Persian) flashcards at the expense of Georgian or Kyrgyz flashcards. This is not to say that he was planning to bootstrap the dissertation writing process into a career in intelligence, exactly – he was using ethnographic methods because it became obvious that these methods were the best way to understand, and subsequently mathematically model, the peace processes in a way that was actually consistent with locals' understandings, and then write a book about it that would be legible to his academic community. The process meant collecting many not-for-attribution anecdotes, slowly sifting truth from the vapor of nuance and street stories, learning to say local names correctly, and proceeding very slowly under the watchful eye of state security personnel. And there is no question that at least some of his respondents talked to him *because* of, not in spite of, a prevalent unfalsifiable theory that he might be conveying their stories to powers-that-be somewhere in northern Virginia.

Beyond consideration of the riskiness, both projects also made an effort to undermine pervasive stereotypes about the fieldwork contexts. In the Paraguayan case, these stereotypes were especially troubling for an anthropologist (see Jusionyte, 2015) because they justified state military operations and surveillance in collaboration with the US War on Terror (Abbott, 2004; Hudson, 2003; Stanislawski, 2006) The *triple frontera* where Paraguay, Argentina, and Brazil intersect, is one of the most active border economies in the hemisphere. The Paraguayan free trade zone of Ciudad del Este is often cast as its lawless capital.

For Schuster observing how microfinance loans were packaged, sold, and put to use in a local context awash in money – much of which flowed across borders through illicit or contraband ('dark') exchange networks – involved cultivating the trust of both borrowers and lenders. Research proceeded by slowly tracing the pathways of circulation that stitched different sectors of the commercial city together into temporary alignments, often through systems of formal and informal credit. By necessity, the work involved licit and illicit border crossing by the researcher, and meetings with various parties engaged in money laundering, customs evasion, and counterfeiting. Without signaling a wider commitment to supporting the mundane and everyday livelihood activities of economic actors who make their profits in the gray interstices of state legal regulations, this sort of research would not have been possible. While the region had been a site of surveillance and state violence in the past (Folch, 2013), today private security services take a leading role in governing Ciudad del Este. In this sense the *triple frontera* is significantly different from the political context of Central Asia and the Caucuses. The persistent worry for Schuster has been that foreign intelligence services would turn her work into "dual use" anthropology (Price, 2016). Despite the best intentions to conduct research in such a way as to keep those private and illicit networks at a safe distance (both practically and analytically), she was aware that the distinctiveness of the site opened the door to future academic and non-academic collaborations focused on security and illegality. This crossover appeal now helps her demonstrate research impact, most recently commenting on a spectacular bank heist that took place well after fieldwork concluded (Schuster, 2017).

Comparing these two projects side-by-side revealed a crucial feature of extreme fieldwork, or at least how it is perceived by disciplinary gatekeepers. Stated bluntly, it is consequential that one of these books is about men weaponizing their charisma to build private armies, and the other book is about women monetizing their social interdependency to collateralize loans. Academic publishers, however, recognize that both projects service the same market. However, in the journey from dissertation to book, Schuster was routinely advised to lead with the sensationalist aspects of the research. Tellingly, an editorial decision was made by the press to keep 'smuggling' in the title of the book even after reviewers noted that it was not a central focus of the research. Again and again, career advice presumed the importance of outlaws and downplayed the research questions that made original contributions to scholarship on gender and sexuality.

How are we to understand this in light of our wider discussion of the risks of our research being misrecognized as covert intelligence gathering? Trouillot, writing on the 'savage slot', observed that 'we are far from the days when five Eskimos caused an uproar in London. The Primitive has become terrorist, refugee, freedom fighter, coca grower, or parasite' (1995: 46). By narrowing our focus to home in on the Otherness of the terrorist, warlord, freedom fighter, mobster, and the like, we may well be still at the task of 'melancholically preserving the slot itself, empty as it may become' (1995: 51). One implication of Trouillot's warning is that our disciplinary self-preservation puts local populations directly in harm's way – e.g. when our

fieldwork, even if it is focused on a feminized topic dismissed as 'unimportant' by some key academic gatekeepers, is at risk of being misrecognized as espionage.

Between 'do no harm' and 'see no evil'

A simple conclusion might be that the next generation of ethnographers ought to reconsider extreme fieldwork. If you just stay away from dangerous places, your conscience will be clear. This would be disingenuous, however. Our collaboration began when we were in residence at a postdoctoral interdisciplinary research fellowship which awards positions based on one-of-a-kind fieldwork. Advisors have a responsibility to guide students towards dissertations that have a good probability of turning into impactful first books. So, while it is tempting to think that this problem would go away if only students were more aware that going to stateless spaces is dangerous, both of our lived experiences suggest that this optimism is misplaced. The fact that doing this kind of work draws attention and that accolades accrue to those few of us who demonstrate an ability to do it – to have actually worked in a famously lawless space – was part of the attraction. There is absolutely no reason to think this will not also be the case for the next generation.

Rather than bounding the ethical space of risk and consent to the interactional context of ethnographer and informants *in situ*, we regard as absolutely essential to also cultivate a conversation within our disciplines regarding the ethical dilemmas that come from social misunderstandings of the professional aims of the ethnographer. These misunderstandings are magnified by the fact that social scientists cannot, in the contemporary research environment, credibly promise not to repurpose data in the service of state policy. This is a reoccurring conversation in the discipline of anthropology,[15] but as Malejacq and Mukhopadhyay (2016) emphasize, best practices for safe data collection in war zones are constantly being improvised in contemporary political science. If our experiences are any guide, early career researchers are quite poorly prepared to navigate these waters alone. If it were not for the help of our advisors, the same intrepid spirit that was so necessary for us to manage difficult fieldworks situations, and the same energy to be recognized for our excellence, could have gotten someone seriously hurt.

Critically engaging with two features of ethnography – its complicity with local political dynamics and its social justice impulse – *before* conducting fieldwork can enhance the situational awareness of ethnographers engaged in participant observation *in situ* among communities whose survival probably depends on remaining illegible from the state. As a preliminary rubric for developing situational awareness about extreme fieldwork, we propose that advisors and PhD students discuss a few basic issues, and ideally develop shared answers to a few focal questions, long before the prospectus is approved. Ideally, this would take place outside the classroom, in a one-on-one setting, before even beginning the process of pre-fieldwork site scouting and topic formation.

Our specific call is for *situational awareness in research design and mentoring*. We believe that it is crucial to supporting the kind of research we value – detailed,

empirically grounded, sensitive to community-based concerns, built out of local idioms, theoretically rigorous – while also grappling with the non-zero probability that affiliation with the ethnographer could expose the local research team to charges of espionage (and it should not be taboo to discuss research constraints in this way). There is nothing extraordinary about our shared experiences of the field as a 'rite of passage'. We are certain that many early-career scholars, holding their first book, look back at the version of themselves that received initial grant funding and IRB approval as dangerously naïve. What was vital in both of our cases was a sense of situational awareness, cultivated over many years of project design and research. Crucially, this awareness was also supplemented by mentoring that *explicitly engaged* with the trade-offs between research transparency, the protection of human subjects residing in authoritarian or unstable regimes, and the potential public goods associated with either activist or investigative journalist sensibilities.

This is a different type of situational awareness than the field-site-informed intuition that generates pithy anecdotes of intrepid research (e.g. the awareness that certain neighborhoods in Ciudad del Este or Dushanbe are safe, or that certain fieldwork contacts will be friendly rather than hostile, or that it is a bad idea to assume that email correspondence is private, or that it is safer on balance to pay a bribe at a security checkpoint than it is to wave your blue passport, etc.). The awareness we have in mind is more of the question:

- If you do this work with integrity, and design an ethnographic project deeply engaged in the intimate and everyday dilemmas of people in a political hot-spot, you are probably going to end up on a list of 'persons of interest' that you cannot get off of – and you might want to think carefully about why you want that, if you do, and the stakes involved for others.

Early career researchers ought not to shoulder the risks and rewards of situational awareness alone – as a disciplinary rite of passage (Seizer, 1995) – because they are not actually qualified to pass judgement on whether the research has the potential to make a meaningful academic impact. We appeal, therefore, to mentors to develop strategies to build situational awareness into all stages of the research. For some – and probably for most – this is a natural extension of the work mentors are already doing; for others, it may require introspection about whether life experiences from their established research programs are really analogous to the experience likely to be faced by today's graduate students. Some of our students are considering confrontational projects that will put them into direct opposition with *very* high-capacity state security bureaucracies. We view our proposal as overall complementary to the more commonplace notion of risk-assessment and skillful management of situations within fieldwork relationships. In an academic environment that is going to continue to reward risky and dangerous fieldwork, we are increasingly concerned that this approach to research training is neither easy nor straightforward.

Candid conversations about the seductions of espionage are vital. Private reasons for undertaking extreme fieldwork are rarely dealt with head-on and were largely inchoate (for ourselves) during the early stages of research. Whether the prospect of a good job in the public service or the appeal of, those motivations were only ever questioned obliquely, and only long after the fact. Indeed, often these journeys are treated as a retrospective 'ethnography of ethnography' (Bourdieu, 2004), as was the case for Bourdieu's Algerian fieldwork. We argue that a frank conversation about the relevant figurations of 'courageous fieldwork performance' that shape expectations about the specific project, the personal ideals that motivate the work, and the specific templates of research relevance, should all be examined closely as standard reflexive practice. These reflections are very personal, and the classroom is the wrong forum. Part of pre-fieldwork engagement can also provide a framework for guidance as researchers 'repair their ship at sea'; if they reach the field and find other templates, or find that the 'side' they are on has changed, there is no one other than the advisor to play this crucial role. The specific parameters of these questions are often highly context and question specific. Senior scholars have a better intuition for whether the cost-benefit ratio is favorable for certain kinds of research conducted in a difficult field site. Specific lines of inquiry might include the following:

- What are the standards and criteria for ethnographic visibility in this project? What do you reveal about what you are doing, under what conditions, and for what reasons? What are your criteria for taking sides in a political hot-spot? Can you write-up *without* taking sides – and, if not, how do you think that your subjects' awareness of the fact that you are eventually going to be on one side or the other will influence the data you collect?
- IRB boards may caution against collecting and recording any identifying information (of any parties discussed in the research) but offer less guidance on disclosing the researcher's maneuvering among groups – how will this tension be managed?[16]
- What criteria will guide the ethnographer's decisions with respect to 'naming names' in violent conflict zones, contexts of state surveillance, or areas of interest to the intelligence community? How will those decisions be made, and with how much consultation from disciplinary mentors?

Second, there needs to be an equally candid conversation that takes place within departments and at public meetings among senior scholars about engaging security risks that does not simply pretend away the market incentives that are going to bring practitioners of 'extreme fieldwork' into direct confrontation with the security state. This is particularly the case for research that is designed to blur the line between anti-regime activism and qualitative social science knowledge production, but may extend further. Two points of consensus emerged from our own experience of research in a surveillance state and in a zone criminalized by the security apparatus. Anthropologists and sociologists have long engaged in debates about the relationship between ethnographic validity and ethics of confidentiality.

However, some of those recommendations bear repeating. The first is that it is trivially easy for you to be compromised by the security state without your knowledge or permission. Crudely, someone can read your emails or stop you at a checkpoint and take your notebooks. There are also subtler ways for agents of the state to collect information from you, 5 to 10 years later – for instance, by enrolling in your university, taking your classes, and asking a lot of good questions. The simplest solution is the best, which is to not write down people's real names, period – even if means absorbing whatever minor professional risks are associated with charges of data fabrication. The second is that, in order to avoid covert research, it is almost always a mistake to lie about who you are or what you are doing. At the same time, we should recognize that state security agents are often our doppelgangers, in the sense that they are trained social scientists and they are studying us, trying to discern our real motives by reverse-engineering the real reason we are asking the questions we are asking of their citizens. They often engage us quite transparently, not under a cloud of subterfuge, and from within altogether mundane state bureaucracies. To open a conversation about engaging the security state, the following issue areas might usefully guide a roundtable:

- What criteria would (or should) render a project a non-starter? More broadly, what level of ethical scrutiny is appropriate for this project? At a minimum, should we be made aware of the fact that work perceived as being needlessly dangerous, or callous towards human subjects, will invite a level of ethical scrutiny far out of proportion to 'normal' ethnographic projects, and that the social status rewards that ultimately accrue are rather weak compared to the potential reputational costs associated with exemplifying 'cowboy' behavior? But where is the line drawn? And why?
- If the governments of certain high-capacity states – like contemporary local authorities in the Russian-controlled North Caucasus, for example – have a theory that all social science research amounts to espionage, is the implication that no ethical research can take place at all since it puts interlocutors at risk, possibly long after the researcher departs the field site?
- What sort of access are you prepared to give up? Has your assessment of access changed, given the seductions of espionage that have been identified for this project?

The future of extreme fieldwork

In conclusion, we wish to emphasize that our motivation for this 'ethnography's kitchen' should not be interpreted as a blanket call for the abandonment of research in high-risk settings. To the contrary, as we have emphasized throughout, these issues both emerge out of the intimacy of fieldwork and simultaneously challenge ethnographic sensibilities. Ethnography is uniquely positioned to detail comprehensively and to elaborate theoretically these dangerous social worlds, even

while we should face uncomfortable questions about the perceptions of courageous fieldwork that inspire work in such areas to begin with. Sophisticated state agents will understand that ethnographers, including anthropologists, cannot credibly commit to being academics forever. Even when such fieldwork reveals something crucial about gender politics rather than about the war on terror, there is a danger that the research in high-risk settings will be misconstrued as espionage. Mentoring can helpfully clarify what the range of career options are envisioned to be, and how these options about taking sides in the future might shape everyone's perceptions of the seductions of espionage more clearly.

The nub of the concern that motivates us is that scholars chasing a certain kind prestige are conducting research that contributes to the perception, by local security services, that roving social scientists conducting fieldwork are, in most functional respects, voluntary NATO or US spies conducting information operations. While anthropologists have warned that we 'have a professional obligation to one another not to conduct slash-and-burn fieldwork' by colluding with the short-term interests of military and intelligence services as it will undermine social science research in the long run (González, 2007: 19), we have been largely silent on the question of whether extreme fieldwork has similarly deleterious potential. For some kinds of research, the more care and effort one invests in getting the story right, the more likely the researcher will engage in behaviors that can be confused with spycraft. We may be overdue for a very high-profile clash of values.

What is most troubling to us – the real reason that we decided to write this – is that there are many scholars who have reason to believe that symbolic confrontation with state security services is, on balance, a good thing. It may be good for both their short-term fame and their long-term careers, and also be normatively defensible, to engage in espionage to advance the cause of marginalized, under-resourced, and victimized communities. What may be emerging is a kind of arms-race where students are incentivized to imitate the practices of guerrilla journalists.[17] If the kinds of research practices that this confrontational ethos engenders can be harmful, in the aggregate, to our human subjects and research associates residing in semi-authoritarian states, then it is important that we confront that possibility head-on.

The bottom line is that it is going to become incumbent for every researcher who collects qualitative data related to contemporary terrorism, criminality, the drug war, or civil war violence in general, to self-define their role with a great deal of self-awareness and awareness of how their roles might be viewed from the outside. Conducting scholarly work in authoritarian or unstable environments on politically sensitive topics while staying safe requires keeping one's eyes open and responding flexibly to highly local and contextualized variables. How does one mentor and advise early career researchers on this type of situational awareness? While the ethics and power relations of ethnographic fieldwork in 'anthropological locations' (Gupta and Ferguson, 1997a, 1997b) have been a core concern to ethnographers across the social sciences, our main suggestion is that advisors and mentors consider a related set of questions about what sort of research

projects we should encourage our students to undertake. This may come prior to asking *how to* undertake them ethically and appropriately. We appreciate that this is a big ask at a moment when research impact measures are the primary criteria for hiring, promotion, and department resourcing, but it is useful to open an interdisciplinary space for an honest conversation about these important issues.

Notes

1. By discussing these sorts of high-risk projects as 'extreme fieldwork' we do not mean to trivialize the research or suggest that it is undertheorized or journalistic. Like all ethnographers, we are acutely aware that most fieldwork can become dangerous – often unexpectedly. Rather, we want to flag the fact that many nuanced and thoughtful projects that take questions of fieldwork ethics very seriously (including our own research projects) also happen to be set in contexts that are globally recognizable as politicized 'hot-spots'. We consider the academic gatekeepers that categorize and reward work perceived as 'extreme fieldwork' as well as some ethical questions that might arise from even the most well considered and sensitive projects in such settings.
2. As we note below, ethnographers researching their own communities in high-risk political settings often face even more acute challenges in terms of disambiguating their research from covert intelligence operations and even higher stakes in terms of their personal protections; meanwhile, non-Western ethnographers often also have more robust networks of kin and community to help navigate those dilemmas. Although we do not presume a Western ethnographer, we focus on the incentive structures of careers in the Western academy and the link to what we call the 'seductions of espionage' when engaging security risks.
3. Trouillot's important critique is central to our analysis, as he went beyond the specific modernist and postmodernist tropes that characterize ethnographic prose to consider the 'larger thematic field' of savagery in anthropology, and the wider world in which the 'savage slot' in the trilogy 'order-utopia-savagery' takes shape.
4. Many political scientists would be the first to admit that, as a discipline, we are functionally 'co-opted'. Many anthropologists' research may be more complicit with statist agendas, at least from a certain point of view, than they would admit. The ongoing critical blowback related to the Human Terrain Systems program (that embedded social scientists with military units) continues to foreground the murkiness of the uses to which ethnographic data might be put (Kelly et al., 2010). Gusterson's (2007; see also Reichman, 2012) long engagement with the intersection of anthropology and militarism is highly valuable.
5. There are cases where the stakes are fatally high, such as the recent death of, a researcher at Schuster's university, who died while conducting ethnography.
6. To give a sense of the incentive gap between the ethical intuitions imparted to contemporary students of anthropology and political science, consider the wide array of active research programs in political science and behavioral economics investigating, in a very self-aware manner, the efficacy of selective assassinations on the organizational structure of terrorist groups. The suggestion that one's labor might contribute to this knowledge base, even by accident, would be anathema to most anthropologists. See also Desposato (2016) generally.
7. These are durable tropes that have been well documented in anthropology even before the reflexive turn and *Writing Culture* (Clifford and Marcus, 1986) critique. In cognate disciplines, the heroic template of masculine 'hairy-chested' fieldwork was contrasted to the

'hairy-chinned' work of theory-building, also gendered (e.g. Kidder, 1976). Both conceptual and methodological courage have long been cast as academic ideals.

8. In Atran's own words: 'I always tell people you can learn a lot in just one day on the ground, [there's] nothing like it', and lionizes research methods described as 'gallivanting about rural villages, getting sneaked into Iraqi prisons, pondering fake checkpoints and roadside bombs' (Bartlett, 2016).
9. In our experience, a great deal of altogether mundane post-fieldwork anecdotes, debriefing, and gossip among colleagues and classmates similarly admires suffering in the field and can work subtly to reinforce popular perceptions of the heroism attached to extreme ethnography.
10. Improvising an answer to the question "how are you supposed to do ethnography when your methods are convincing absolutely everyone – including your friends and loved ones – that you are a spy" posed challenges beyond anything that anyone at Stanford's IRB knew how to give advice on (though formal and informal advice was sought throughout the research). More than once between 2005 and 2008, advisors warned that the risks that he was taking were far, far out of proportion to any possible professional gain.
11. After Alex Sadikov was charged with espionage by the Tajik government, which ruled that his qualitative research was indistinguishable from espionage, Driscoll stopped working in Tajikistan completely. He continues to conduct research in Somalia, Ukraine, and Georgia.
12. This claim ought not be read to imply that there are no taboos in contemporary political science or economics against employing ethnographic methods. It is worth reproducing the dry summary of Wedeen (2009:83) on where ethnography falls in the status hierarchy of methods in "mixed method" dissertations in political science: "I am suggesting that calls for 'productive complementarity' tend to subordinate the epistemological concerns of 'narrative approaches' to the aims of science. Ethnography is often deployed in the service of the very sorts of objectivist aims that current ethnographic approaches in anthropology undermine. And ethnography is seen as the least prestigious method, treated as the 'summer intern' to the 'senior partner' of formal methods."
13. Gusterson (2017) describes the advice given to him as a graduate student, which he recalls directing him to take a position aligned with activists and against government scientists: 'When I began my original fieldwork as a graduate student an anthropology professor told me I could say whatever I wanted about the weapons scientists, but I had a responsibility not to undermine the worldview of the much weaker community of activists who were struggling with flimsy resources against massive national security institutions.'
14. Political science is in the process of rediscovering this tradition, as exemplified by the discussion of 'building a tribe' in Malejacq and Mukhopadhyay (2016: 1017–22).
15. See, for example, anthropological responses to the controversy surrounding Alice Goffman's ethnography and the issues of third-party identification. These have been summarized on the widely read *Savage Minds* blog: https://savageminds.org/2015/08/25/ethnographic-field-data-2-when-not-sharing-is-caring/
16. A formal treatment of this second dynamic as a strategic game of 'chicken' is available upon request for those readers interested in formal modelling and game theory.
17. A PhD project somewhere is almost certainly weighing the costs and benefits of committing to confrontation and running on a collision course with state agents, more-or-less in order to see what happens.

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