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Jason Cons

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What is This?

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Field dependencies: Mediation, addiction and anxious fieldwork at the India-Bangladesh border

Jason Cons
University of Texas, USA

Abstract

This paper explores the politics of dependency in researcher–assistant relationships. By doing so, it reflects on how these dynamics are always already predicated on broad personal histories and a range of emergent dependencies. Taking the politics of dependency in fieldwork seriously charts a path towards more fully understanding the quixotic production of ethnographic knowledge. Specifically, this paper reflects on the author's relationship with Saiful (a pseudonym), who worked with the author during research at the India-Bangladesh border. Saiful was addicted to heroin. This addiction both compromised and enabled a productive research engagement in an unstable place. But Saiful's heroin use was only one of a series of dependencies that structured our relationship and this research project. Using the lens of dependency to unpack the construction of the field and of ethnographic knowledge more broadly, this paper reflects on a range of questions, including access, anxiety, insider-outsider politics, and entanglement.

Keywords

Introduction

Over the past three decades, the 'reflexive turn' in anthropology and related social sciences has led to myriad investigations of the politics of writing and representation in ethnography, the complexities of fieldwork, the possibilities for community-based and participatory research, and more.[1] Despite these interventions, there remains little attention to the politics of collaborative fieldwork with research

Corresponding author:
Jason Cons, Lyndon B. Johnson School of Public Affairs, University of Texas, Austin, TX 78713, USA.
Email: jasoncons@gmail.com

assistants. Surprisingly few ethnographers have taken the relationship between researcher and research assistant, and the processes of mediation that necessarily occur within this dynamic, seriously as an object of ethnographic analysis and theorization (Middleton and Cons, this issue). Indeed, discussing this relationship appears to be a veritable ethnographic lacuna, if not a taboo (Sanjek, 1993).

Part of the reason for this, I suggest, is that the very notion of a research assistant as more than a cipher of information raises a range of anxieties about ethnographic knowledge production. The idea that data is shaped by or in dialogue with a collaborator who has not been vetted by rigorous processes of graduate training, research proposal evaluation, and internal review boards problematizes the already assaulted 'scientific-ness' of ethnography as social science practice (Sayer, 2000; Thomas, 1991). In undermining the idea of the ethnographer as the sole vessel of observation and analysis, it suggests that our data is compromised at best, 'polluted' at worst. More importantly, particularly for those working in a more interpretive vein, the idea of a collaborator in the field raises troubling questions about the notion of authorship (Charmaz and Mitchell, 2011; Coffey, 1996; Stacey, 1988). How are the interpretations of culture and cultural practice that are part and parcel of ethnographic work shaped and translated if the relationship between an ethnographer and his or her informants are mediated by another? Moreover, the use of a research assistant conjures questions about the ethnographer's own skills, abilities, and qualifications in the field. What are the specific needs for working with a research assistant? Do they signal a lack of linguistic capability, on-the-ground savvy, confidence, or some other trapping that is or should be part of the intrepid ethnographer's toolkit?

This essay plays upon these anxieties so as to explore the analytic possibilities opened by confronting them, as it were, head on. This is the story of my relationship with Saiful,[2] my research assistant during much of my fieldwork on enclaves along the India-Bangladesh border in 2006 and 2007. Saiful was a heroin addict, a condition I discovered shortly after we began working together and which, despite my attempts to intervene, eventually forced me to end our working relationship. Our time together raised a range of questions central to the researcher–assistant dynamic – questions of anxiety, of dependency, and of mediation. Such questions were particularly formative of my relationship with Saiful. However, I suggest that they also regularly shape the terrain of engagement between researchers and research assistants. In engaging these issues, I hope to show how they paradoxically placed certain limits on fieldwork, while also opening up new ethnographic possibilities.

Concerns over the research assistant's role in the production of ethnographic knowledge stymie more open dialogues about the researcher–assistant relationship. As Middleton (this issue) argues, such silences preclude many productive possibilities for imagining a postcolonial ethnography and for unsettling the subalterneity of research assistants in ethnographic production. As many contributors to this issue demonstrate, the productive relationships between research assistants and 'their' researchers are foundational conditions for field engagements old

and new. Yet, in interrogating our relationships with research assistants, I suggest that it is critical to explore limits and tensions as well as successes and possibilities. In other words, if our exploration of this relationship is to be more than a revival of the well-worn trope of celebrating ethnographic key informants (Casagrande, 1960), we must be equally as concerned with the processes and meanings of 'failure' as with 'success' (Ross Owens, 2003).

In a recent reflection on the 'experience and agony' of fieldwork, a particularly suggestive set of terms when reflecting on my time working with Saiful, Hammoudi and Borneman argue for a re-centering of the episodic encounter in the writing of ethnography. As they write, 'episodes engulf the anthropologist [sic] in occasions and practices outside her control, forcing her into a deeper and more quixotic relation to knowledge' (Hammoudi and Borneman, 2009: 271). Quixotic, here, is a suggestive term, implying both an impractical and idealist notion of the production of truth. Part of the entanglements of knowing, the quixotic nature of ethnographic truth, is bound up not just in the episodic relationship between the ethnographer and his or her subjects, but also the episodes and encounters mediated with and often by research assistants. These mediations, I argue, are structured by the not always straightforward dependencies that emerge within this fieldwork relationship. Bringing the research assistant 'back in' clarifies that these dependencies are conditioned not just by the contexts of ethnographic engagement or by the ethnographer's own intersection with biography and history (Mills, 2000 [1959]). Rather, they are also conditioned by the peculiarities of the dynamic between specific researchers and specific research assistants. The issue here concerns the many things that research assistants *also* bring to fieldwork. Personal histories that at first sight would appear beyond the pale of ethnography proper can become constitutive of the field itself – often in ways unforeseen at the onset of a 'working relationship'. The results can be rewarding, problematic, even volatile. The field, as the object and location of ethnographic analysis and practice, emerges thus not just as a place but as a particular intersection of interpretations, histories and contingencies (Reddy, 2009).

In what follows, I rethink questions of mediation, fieldwork, and dependence that often shape fieldworker/researcher relationships by offering an episodic account of my own painful experiences with fieldwork. By dependency, I signal a set of co-emergent connections that socially, emotionally, and, in this particular case, chemically tie people to each other and to things. As I hope to show, my relationship with Saiful has instructive, if uncomfortable, things to say about the many dependencies that emerge in the context of such relations. I am particularly interested in two dynamics. First, I suggest that dependency in fieldwork is a complex, but key, relationship in need of critical exploration. In my account, dependencies – chemical, personal, and otherwise – marked both the limits and possibilities of collaborative field engagements. Second, I join a host of others in exposing the field as an especially leaky container, one in which a broad array of histories and experiences bleed into each other to constitute ethnographic encounters. My purpose is not to re-inscribe the problematic understandings of the 'field'

as a zone of distinction between the subjects and practitioners of ethnography. Rather, I suggest that complicating our understandings of field relations through the consideration of research assistants is critical to shifting attention towards 'epistemological and political issues of location' (Gupta and Ferguson, 1997: 41).

Meeting Saiful

I first met Saiful in late 2006, as I was finishing archival work in the Bangladesh National Archives in Dhaka. My dissertation project, an ethnographic and historical study of a series of enclaves – literally sovereign pieces of India inside of Bangladesh and vice versa – had brought me to Bangladesh with high hopes and more than my fair share of trepidations. Though aware of the political complications involved in researching border areas in South Asia (Axel, 2002; Krishna, 1996), I had imagined, somewhat naively, that I would be able to figure out some way to navigate around a range of concerns to study these sensitive border zones.[3] Thus far, it seemed as though the world was conspiring to correct this naivety. For several years, I had been talking to a range of people who had told me, in no uncertain (though often highly uninformed) terms, that the project would likely fail due to the difficulty of access and the danger of the area. Statements such as *you can't do research there, those are sensitive spaces full of criminals and smugglers,* became a familiar refrain of my pre-fieldwork planning. These disheartening messengers included people in Dhaka and Kolkata with whom I had been casually discussing my project. It even included one of the organizations offering generous funding for the work, which sent me a congratulatory letter explaining that they had decided to support the project based on its merit, but doubted that it could be carried out.[4] I had a number of difficulties in securing a visa to start my work and was delayed getting into the field by eight months. Upon arrival, I continued to face difficulties in getting the project underway. It was not my topic alone that was posing challenges. My arrival in Dhaka corresponded with a series of increasingly violent and contentious disturbances, strikes, and demonstrations leading up to the general elections scheduled for December 2006.[5] Consequently, since arriving in Bangladesh, my movement outside of the capital had been limited.

My plan called for several months of historical research in the National Archives in Dhaka, which I eagerly threw myself into while waiting for a change in the political conditions. Yet, here too, I was having difficulties. While I eventually was able to identify a series of holdings related to the politics of the India-East Pakistan border in the years following Partition in 1947, references to the enclaves in these were sparse. While intriguing, they did not provide enough grist for a chapter, let alone a whole dissertation, even when read, as Stoler, puts it, 'upside down' (1995: viii). Within the archive, archivists regularly told me that I was struggling because my research was on a sensitive topic and that I was unlikely to be allowed access to these spaces themselves or to information about them. Indeed, several of them openly, and apologetically, refused to let me see materials that were, as far as I could tell restricted because of the sensitivity of my topic, as

opposed to the sensitivity of the materials contained therein.[6] In short, I was far from confident that my proposed course of research would be successful. Failure loomed large in my mind.

One day, while I was pouring over dusty accounts of post-Partition border skirmishes, a young, extraordinarily skinny reporter, who was in the archives conducting background work on a story, approached me. Saiful introduced himself and inquired as to what I was working on. I quickly found myself in an engaged conversation about the politics of the border. Saiful made an immediate impression on me for several reasons. As an MA student at Dhaka University in International Relations, he had briefly visited the enclaves. As such, he could offer a first-hand account of a space that, because of the pre-election political disturbances, I had not yet been able to visit. More importantly, he immediately grasped, and was excited by, the theoretical and conceptual, as opposed to the purely empirical, dimensions of my project. In our brief conversation, Saiful identified and engaged the very questions that I was interested in exploring further – the imperfect fit with these enclaves and various notions of nation and state, their political and historical constitution as contested spaces, and the precarious conditions of life in and around them. To top all of this off, he was enormously charming. I sensed that here was someone who I might easily come to call 'friend'.

We made arrangements to meet later that evening in a park near my house to chat more. After several hours of discussion, I invited Saiful to come back to my apartment to meet my wife and share a meal with us. Over a chicken curry, I asked Saiful if he might be interested in making an exploratory visit to the enclaves with me. The visit, which happened a week later during a momentary lull in the election *hartals* (strikes), was surprisingly fruitful. During the trip, I found myself drawn to Saiful and we became fast friends. Saiful had an incisive and insightful outlook, a sharp and sardonic wit, a near photographic memory for details, and, to my mind, a healthy and lively disrespect for authority – including, often, mine. I felt that we worked well together in initial discussions with informants at the border and found our discussions following a day's work and composing shared fieldnotes enlightening.

All along, it had been my intention to work with a research assistant during fieldwork. I wanted to do this for several reasons. First, I was concerned that my language skills would prove insufficient to the task of keeping up with rapid conversations in the dialect of Northern Bangladesh. I hoped that a research assistant would help in the transition process while I become more confident in raising issues that were key to my research. Second, I was aware of the vagaries of doing research in a 'sensitive' border zone and thought that by working with someone who 'knew the terrain', even if only a little bit, I might avoid compromising situations that would have grave implications for my research (see Hoffman, this issue). Yet, arriving in Dhaka with relatively few contacts, I had no idea how to go about finding and hiring someone to work with. Meeting Saiful seemed an enormously fortuitous coincidence. On our return to Dhaka, I asked him if his position would allow him time to work with me. As it turned out, Saiful's position was a

short-term contract which was soon ending. The next day, he returned to his hometown of Chittagong to tie up loose ends with plans to return the following week to begin fieldwork.

Early concerns

So far, so good. I was thrilled. I had met someone who I felt would not only be an excellent partner in research, but who I was growing to like as a friend. Yet things were not quite as smooth as they initially seemed. While Saiful was returning to Chittagong, as he told me, he was mugged and severely beaten. Saiful lingered there, supposedly recovering from his beating, for several weeks, during which time we occasionally spoke on the phone. The more we spoke, the less his accounts of what had happened made sense. The circumstances of his beating, his injuries, and his plans changed not just from conversation to conversation, but even over the span of a single phone call. Moreover, he was constantly changing mobile numbers. Saiful had multiple SIM cards that he would swap almost daily. I would frequently receive new instructions from him about what numbers I could use to call him, and when. Saiful's emaciated frame had led me to wonder about his recreational activities and lifestyle before. As our discussions became more and more confused, I began to suspect that there was more to my newfound research assistant than he was letting on.

After long discussions with my wife – who was also charmed by Saiful, though perhaps not as charmed as I was – I decided that the best thing I could do in this situation was confront Saiful before we became too embroiled in fieldwork and try to find out the truth. I did this as soon as Saiful returned. Saiful, after an uncomfortable few moments, admitted that, following his university days, he had graduated from abuse of codeine-based cough syrup to heroin. He now regularly freebased 'brown sugar' – so called for its brownish color and high number of impurities – that was smuggled into Bangladesh over the Burmese border and was increasingly becoming a critical problem in Bangladesh's urban slums (Uddin, 2007). He told me that he had been struggling with a low-level addiction for some time and was looking for an opportunity to kick. Whether ill-considered or not, I decided that I wanted to help.

Partly this decision was empathetic. Saiful was far from my first friend to slip from casual use of various chemicals into dependency. I could readily imagine the path that Saiful had followed to arrive at his addiction. We had discussed his desire to attend graduate school, and I (paternalistically and naively) imagined that I was in a position to help him break his habit and, possibly, help him get into a graduate program in which, I had no doubt, he would thrive. Partly, this decision was also selfish. Saiful's presence was a ready-made balm for my growing anxieties about my own ethnographic potentials. I could not imagine finding another research assistant who would be as engaged and resourceful, not to mention as enjoyable to work with, as Saiful. Moreover, Saiful's unflappable savvy in the field lent me a fair amount of confidence. Working with someone who was sanguine about navigating

things like border security officials and suspicious informants made my work feel more concrete and feasible.

We reached an agreement that Saiful would move into our apartment in Dhaka, and kick his habit before we began fieldwork. Saiful was clearly a high-functioning user and, based on his own account, his use was fairly recreational. Saiful locked himself into our spare room, coming out only occasionally for food or more often to buy cigarettes, and after a long week, announced that he was ready to begin work.

The beginnings of my relationship with Saiful point to ways in which the research assistant relationship is already over-determined, or at least co-constituted, by things beyond the specific research engagement. From the start, my working relationship with Saiful was bound up in complexities that punctured the notion of research as a discreet activity. Speaking from my perspective alone, these included my liking for Saiful, my empathy for his condition, and my anxieties about my role as a foreign researcher at the India-Bangladesh border. Moreover, it hints at the complexities of notions of access and barriers to entry. As Sally Falk Moore observes, 'there is something inherently weird about many a fieldwork situation. Intruding on the lives and affairs of communities of strangers, even when they are cheerfully willing to have one do so, is not a normal way of making friends' (Moore, 2009: 152–3). Moore's narrative concerns the 'getting in' of fieldwork, the awkward ways that one initiates a series of relationships with people who come to occupy the uncomfortably hazy category of 'informants'. The weirdness of fieldwork, and the anxieties that it conjures, is itself often a particular kind of barrier of entry, one that structures the encounters and possibilities involved in the early phases of research. These awkward beginnings, of course, shape the possibilities of long-term engagement. Yet, the weirdness of fieldwork begins long before the 'field', as a conceptual or physical location, is reached. It is framed by and through a range of experiences that both directly and indirectly lead up to it. My relationship with Saiful, for example, was already deeply imbricated in the weirdness and interpersonal complexities of ethnographic engagement even before we began the project of co-constituting the field with our informants.

Covert empathies

Ethnographies of addiction have much to say about issues as various as the social norming involved in representations of addiction, the social suffering of addicts, the struggles for 'respect' and dignity in the face of often extreme structural violence, and ways that addiction itself produces particular forms of community.[7] My experiences with Saiful open interesting windows onto these discussions. But here, I wish to raise a slightly different point about dependency. As my argument above suggests, the beginnings of our relationship certainly contained the seeds of various forms of dependency – Saiful's chemical addiction and my own anxieties about fieldwork. As we worked together, these dependencies both deepened and became intertwined, forming new working resonances and shaping our evolving

professional relationship and friendship. Crucially, these dependencies provided more than just limits, impediments, and failures. They also, and particularly in the unstable context of the India-Bangladesh border, opened up new possibilities and shaped our research in often productive ways.

Over the next six months, until I finally and forcefully ended my relationship with Saiful, we explored both these limits and possibilities. While at the border, we shared and digested numerous troubling and often hair-raising experiences. When we would return to Dhaka to reconnoiter, transcribe and translate interviews, and conduct further historical work, Saiful lived in our apartment. My working relationship with Saiful during this time was often astonishingly fantastic. My working relationship with him was also horrible and painful.

In the field, we developed an intuitive partnership. At the border, my 'foreignness', coupled with Saiful's natural charm and ability to make even the most nervous informant feel comfortable, made us objects of productive curiosity. The enclaves where we worked are subject to high levels of regulation and observation.[8] They are also frequently the target of various short-term visits and research projects, many of which are designed to collect haphazard information about abstract concepts such as the enclave residents' 'human security'. Both forms of observation and monitoring have exoticized the enclaves as 'self-evidently' problematic spaces filled with 'self-evidently problematic populations' (Cons, 2013). Over the weeks and months that we worked at the border, it became clear that few people had asked the residents of this sensitive border zone to share their own histories and stories. The curiosity of a white foreign researcher coupled with Saiful's approachability proved to be an ideal admixture. Both of us were outsiders, but our varying 'outsider' statuses were productive. We were strange, yet approachable. While initially, we struggled to find ways to get enclave residents to share their history, over time, our own exoticism proved a way to work past the stilted often canned phrases and stories, seemingly ready-made for outsiders, that we heard repeatedly in our early days in the field.

It was clear that there was an intimate link between Saiful's gifts in the field and his marginal lifestyle out of it. Saiful was many things during fieldwork. He was a brilliant listener. He had an intuitive ability to ask the right questions in the right order, an ability that far outweighed my own. He also quickly and easily conveyed a sense that he understood and was not likely to judge accounts that strayed outside of the margins of formal, legal practice. This turned out to be invaluable for working in the enclaves. The realities of life within these complex border zones meant that many of the informants that we worked with had in the past, and often continued in the present, to pursue means of living that regularly violated both borders and legal norms. The enclaves emerged, in our work together, as zones of both danger and ambiguity – grey areas in which residents of the enclaves regularly negotiated complex spaces and politics. Saiful brought an enormous range of empathy and shared experience into these spaces. Saiful's background – including not just his drug use, but also his time in student politics at Dhaka University[9] – involved regular brushes with, and dodges around, the law, local

power-brokers, and other petty sovereigns (Butler, 2004). Perhaps in part because of his own experiences in grey spaces at the margins of law, Saiful seemed to inspire an almost confessional attitude in many of our informants.

In the enclaves, as I have argued elsewhere, a public narrative of 'suffering for territory' tends to dominate and frame the way that enclave residents interact with government officials, NGO workers, and other short-term visitors (Cons, 2013; Moore, 2005). This narrative position enclave residents as hapless victims who have, over the long period since Partition in 1947, persevered in the face of overwhelming opposition from residents of their bounding state. While not untrue, this narrative also masks a more complex history in which enclave residents, often violently, asserted their own authority and agency in defending territory, framing community, and staking claims to rights and lands. Over time, people began to share stories that compromised public narratives of belonging and entitlement and presented the enclaves as a significantly more complex and problematic space. Saiful's uncanny ability to lend an empathetic ear was, in no small part, responsible for this.

On more than one occasion, for example, in interviews in which both Saiful and I had been speaking in Bengali, informants would turn to Saiful and say, 'Now, Saiful, you will have to decide whether or not to tell this to Jason, but . . . '. In such moments, our informants would seemingly allow me, as a foreign researcher, to drop out of the picture, on the one hand conveying information that they knew I could and would hear *as though* they were simply conveying it to a sympathetic interlocutor. This is not to suggest that our informants were naive or uninformed about why we were there, what we were asking, or how we intended to use the information. Indeed, we took great pains to make this clear. Rather, Saiful's familiarity provided a way to reframe conversations about sensitive subjects. Often, our discussions were thus transformed from formal interviews into chats with a fellow-traveler: an outsider who understood, without judging, life lived in constant negotiation with projects of state rule.

Powers of dependency

Saiful's brilliance as a fieldworker was linked to more than just his elective affectivities with people at the border. The complex border configurations that we were studying were also part of our daily working life. Saiful had a knack for exploring the limits of control and regulation and, in doing so, getting us into and out of all kinds of productive trouble. He put this knack to use in our fieldwork, working around and past security officials who periodically became alarmed by our presence. Such encounters required deft navigation of state and personal power. For example, after we had been working in the enclaves for several months, a plainclothes military official – who we later found out was from the Bangladesh Rifles' (BDR, Bangladesh's border security force) intelligence division – began following us. The intelligence officer haunted our movements for the better part of a day before he actually approached us. He began speaking to Saiful, not to me, asking a

number of questions about what we were doing there. After several minutes, the security official, who presumed that I spoke no Bengali, asked Saiful if I had permission to be there. To my horror, Saiful responded, 'of course he has permission to be here. You don't think he just fell out of a plane and landed here do you?'

I was sure that this would prompt immediate action from the officer, but he appeared to accept Saiful's narrative and let us proceed on our way. Our encounter with the intelligence officer had a number of interesting outcomes. We were interrogated by a cadre of armed BDR troops in our room later that night. Over an extremely tense hour, we were obliquely threatened, questioned, and quizzed. The primary focus of discussion was what we were *doing* in the enclaves, as opposed to whether we were *allowed* to be there. To my surprise, following this conversation, the regional BDR commander sanctioned our presence in the region so long as we would keep him informed of our arrival in the area. This involved an informal conversation with him over a cup of tea whenever we left for Dhaka or returned to the border. It is interesting to note, however, that throughout all of these encounters no one ever again asked if I had research permission to be at the border. This is in part because Saiful, in addition to deploying brashness, also skillfully deployed deference and charm in ways that ultimately allowed us to continue to work in the shadow, and indeed with the endorsement, of armed and professionally suspicious paramilitary groups.

If Saiful's occasionally brash interventions helped keep us out of (some) trouble, he also used it to get us into other kinds of productive trouble. He had a knack for slowly piling on provocations during conversations in ways that often allowed us to understand key points of tension and anxiety for various different actors engaged within and with the regulation of the enclaves. The primary Bangladeshi enclave that we worked in, Dahagram, is connected to Bangladesh by a land bridge running through Indian territory and known as the Tin Bigha Corridor. This space is open for passage from Dahagram into Bangladesh but is heavily regulated and monitored by the Indian Border Security Forces (BSF). One day, early in our research, as we were passing through the Tin Bigha Corridor, we were hailed by a BSF *jawan* (soldier), who stopped us to chat. This, in and of itself, was not terribly unusual. I usually regarded our interaction with these *jawans* with some trepidation, as I suspected (incorrectly as it turned out) that the BSF could stop us from passing through the corridor if they so wished, thereby preventing access to one of our main fieldsites. Despite this anxiety, we regularly spoke with *jawans* who were on duty as we passed through the corridor and had come to know several of them.

Our interaction that day was somewhat different than usual. The *jawan* asked us to get down from our rickshaw and take a cup of tea with him. We were immediately joined by an officer who began to ask what we were doing there. I patiently explained my research interests in the enclave's history and current condition of its residents. The officer eyed us with an expression of suspicion and disbelief, making it clear that he thought our story somewhat thin. A cordial conversation with somewhat tense undertones followed, in which the officer, not so subtly, began

trying to figure out what our 'true' intentions were. Saiful, not to be outdone or bullied by an officer who he clearly did not like, began to subtly push back and provoke. First, he suggested that he run to the tea stall just outside of the Tin Bigha Corridor on the Indian side of the border. It would be no trouble, he suggested, for him to procure some tea and snacks so that we could enjoy a morning respite together. With a forced smile, the officer patiently explained that we could not enter Indian territory outside of the Tin Bigha as we had come from Bangladesh and had not passed through an official border crossing. He sent the *jawan* off to fetch tea and continued to ask a series of probing questions about what we were up to. I focused on providing clear, straightforward answers to these questions while Saiful parried by asking the officer about things that we had been observing in the enclave, consciously avoiding catching my eye, with which I was trying to tell him to back off.

In the period before we began working in Dahagram, the BSF had implemented a cattle ceiling limiting the number of cows that could be taken out of Dahagram to the local market in Patgram, the nearest market town in Bangladesh, on any given day. The ceiling, which was nominally designed to prevent smuggling, was currently causing a significant amount of tension and misery within the enclave itself. After several minutes of probing, Saiful said, innocently, 'we have heard that there is a cattle ceiling in the enclave right now'. The officer immediately exploded at us and began shouting 'so that is it! That's what you are here to study.' His sudden anger shocked and alarmed me and I quickly assured him that we were only interested in cattle because it was something that enclave residents regularly brought up. I hastily thanked the officer and the *jawan* for our tea and made to depart with a smirking Saiful in tow. I was, initially, furious with Saiful for jeopardizing our ability to continue working in the enclave, though as I cooled off, I came to realize that Saiful had skillfully highlighted a key issue in the regulation of the enclave. The BSF was anxious about the sensitive space of the enclaves for a range of different reasons. However, they were most anxious about exposing the tenuous limits of their ability to extend sovereign control over the contested space of the corridor and the enclave (Dunn and Cons, 2014). The BSF's cattle ceiling was currently under a fair amount of criticism from both within and outside the enclave. Through his probing of sensitive topics, Saiful had ways in which this particular exercise of authority, which was to become a focus of our research, was itself producing anxieties of rule.

The limits of entanglement

'Research assistant' feels a wholly inadequate term to describe Saiful. He was a collaborator, a trickster, a mediator of myriad complex relationships. Moreover, in the countless hours we spent together, discussing what we had seen, traveling from enclave to enclave on rickshaws, sharing uncomfortable night buses, we grew closer and closer as friends. Or perhaps it is fairer to say that I grew closer and closer to

him. Yet, if things in the field were always ethnographically interesting, in Dhaka, things would go from excellent to a disaster. He would compulsively lie to me about what he was up to. When he would return to Chittagong to visit friends and family, Saiful would completely fall apart and often fall out of touch for days or even weeks, only to return in miserable shape. At these times, returning to the field was a way to get Saiful back on the wagon, as well as a place that we returned to for work.

At the outset of our working relationship, I was uncomfortable with the employee/employer dynamic between Saiful and myself. This was not because I wanted to trick myself into imagining that our relationship was something other than what it was, but more because I could not think past the exploitation that applying the notion of surplus value to my fieldwork implied. The commoditization of ethnographic knowledge marked a transformation from use value to abstract value, a transformation that by definition refigured the production of ethnographic truth (Marx, 1992 [1867]). To what extent did this relationship shape and shift my data? To what extent would it color my interpretation of it? If these were questions to which I had no immediate answer, there were others that become more troubling, specifically: Saiful's own conversion of the salary I was paying him into another commodity with a different kind of use value that, in and of itself, was in the process of dramatically transforming and increasingly structuring our relationship.

I had tried to assuage my own discomfort with employing a research assistant by paying Saiful an extremely generous salary. It was painfully obvious that Saiful was using it to fund his habit. This translation of salary into heroin appeared to me to be a confirmation of the most dire Marxian analyses of capital, alienation, and its opiatic qualities. The overt dependencies emerging in our relationship began to make me profoundly uncertain about the direction and possibilities of our ethnographic partnership. These concerns deepened as Saiful gradually moved from asking me to hold on to his salary and give it directly to his family – his own proposed stop-gap for converting money to drugs – to eagerly demanding his pay the moment we stepped off the bus from the border.

As we worked together, Saiful became more and more erratic. If he had a knack for getting me into compromising situations that were productive for my research at the border, he also developed a tendency to put me in compromising situations away from it. He became more and more sanguine about taking advantage of me, financially and otherwise. Whenever Saiful wasn't with me, I would frequently receive emergency text messages, asking me to wire him money. Often, while we were working in the archive, Saiful would go out for a cigarette break and not return. While translating and transcribing interviews, we would put in three or four productive hours of work and then he would visit the bathroom and return incapable of working for the rest of the afternoon.

It was difficult to determine whether Saiful's heroin use was becoming worse, or just more visible to me. If Saiful's use was marginally clandestine when we first met, it

became all but open as we worked. One time, Saiful borrowed my digital camera to take photos of several documents that he thought were relevant to my research. He returned it full of photos not of the archives but of him and his friends free-basing in a Dhaka slum. Another time Saiful brought me to meet his family in Chittagong, and as we were headed to the bus station to catch a return bus, left me sitting on the borders of a *basti* (slum) while he disappeared into the back alleys to score.

Through these series of lapses, frightening disappearances, and failures to follow through (as well as ultimatums, confrontations, and strategies to change), I continued to convince myself that our friendship and our working relationship were salvageable. The tension that I felt between wanting to help Saiful as a friend and wanting to help him as an employee and key collaborator became more and more confused. Saiful's addiction was as imbricated in my research as Saiful himself (and our mutual relationship) was. Saiful proved unable, or unwilling, to kick his heroin habit. However, my capacity to imagine ways to address the blindingly obvious contradiction between Saiful's stated claims and his actions suggest ways in which my relationship to Saiful was *also* a dependent one. Indeed, long after it was clear that our working relationship was doomed, I was unable to separate my friendship with Saiful from our partnership in research and from my fieldwork more generally.

In Robert DuPont's study of addiction, *The Selfish Brain*, he writes of co-dependence as a warped mirroring of addiction:

> [The] behavior of codependent people contrasts with the outward behavior of active addicts, who often recklessly disregard the feelings of others. . . . Nevertheless, they are locked into a malignant synergy with the addict. The codependent is the mirror of the addict and is equally the suffering hostage of the selfish brain. The self-centeredness of codependents is more deeply hidden but no less fundamental or troublesome than is the self-centeredness of the addict. (DuPont, 1997: 258)

DuPont's description of co-dependence as self-centeredness uncomfortably maps to my relationship with Saiful. On the one hand, I continued to convince myself that I could have the positive outcome in Saiful's life that I had initially hoped. I continued to believe this even as it became clearer and clearer to me that Saiful's desire to get clean and attend a graduate program were, even if sincere, utterly ephemeral. On the other hand, I could not separate my desire to salvage our working relationship from my own desires to continue working with Saiful in the field and my own fears that a project that had gone, in my mind, from a seeming impossibility to a tentative success would be halted, transformed, or even shut off. I was aware that the friendships and relationships that I had in the enclave were formed in partnership with Saiful. While, abstractly, I knew that many of these relationships were not, in and of themselves, contingent on Saiful's presence, I knew that without him they would change and, I feared, be compromised.

Kicking

Ultimately, the strain of living with and working with Saiful began to jeopardize not just my research but my mental health. After six months that I remember as both some of the best and worst of my life, I fired him. A few weeks later, Saiful called to try to blackmail me into giving him money by threatening to compromise the working relationships we had developed with informants on the border. Thus, Saiful himself invoked the deepest anxieties embedded in the fieldworker/researcher relationship – that fieldwork itself is a project that, once begun in collaboration, is inexorably and irrevocably tied and produced through researcher and research assistant. I hung up the phone and have not spoken to or seen him since.

    The long afterlives of my time working with Saiful continue to reverberate. Periodically, Saiful contacts me via email. Often, he writes to ask forgiveness, signing his emails 'the repentant Saiful'. However, these contacts invoke the anxieties that he produced for me while in the field. One time, he contacted me claiming that a rickshaw puller we knew well had been seriously injured by a truck and needed urgent money for medical help. If I could wire money to Saiful, he would convey it to our friend. This claim proved not to be true. Another time, Saiful took out an email address in my name and started sending me emails from it, silently implying that he could represent himself as me to research contacts, friends, and a broader network of acquaintances in Bangladesh and beyond. Often, Saiful's emails invoke a different sort of engagement. For example, he recently sent me a concerned message asking if I and my family were safe in the wake of Hurricane Sandy, which swept the east coast of the US in October 2012. These messages summon all of the ambivalence of working with Saiful – an immediate desire to respond and reach out to him, mixed with a certainty that doing so would only open me up for further manipulations.

    After kicking my dependency on Saiful, I continued to conduct research at the border. I found that Saiful's absence made little difference in people's willingness to speak to me, though I suspect that the nature of what I heard was different than what I would have had I continued to work with Saiful. My memories of our success in research, excitements, and giddy and adrenaline-filled moments of navigating the border are tinged by melancholia, guilt, and regret. Our relationship was problematic and complex not least because the very things that posed limits on our ability to continue working together also were the conditions of possibility for our success in the field. In retrospect, the relationship itself seems ill-advised from the start, a set of questionable decisions made in anxious circumstances – decisions that I now would approach differently. However, reflecting on our dynamic at the border, it is hard to imagine how my fieldwork could have unfolded differently. For better or worse, my own analytic engagement with the enclaves is now, and forever will be, interwoven with the dependencies and possibilities of my relationship with Saiful.

Conclusion: The quixotic field

In a provocative recent theorization of subjectivity, Michael Fisher writes, 'the subject is made through dialogue and shared expertise; it does not preexist. . . . The subject does not vanish, it returns; and it repays its investments in unpredictable, shifting, and slippery-slope ways . . . as a constellation of complexities, emergences, and surprises' (Fisher, 2007: 442). Fisher's arguments are oriented towards an explication of subjectivity in the violent hypermodernity of the present. Also, it resonates with my experience with Saiful. Both my own and Saiful's subjectivity, specifically in relation to our shared research experience, was constituted in both problematic and positive ways through our overlapping experiences and the entwining of different dependencies. The 'we' of our relationship was constantly invented and reinvented in a space of conflicting and shifting goals, desires, and, most importantly, needs. These returns were often unpredictable and always emergent from multiple pasts in both dynamic and troubling ways. Our mutual and independent subjectivities continue to return and reverberate in my own work, my imagination of future projects, and, occasionally, in messages that arrive from Saiful, summoning the ghosts of the past.

My argument, in this essay, is that attending to the multiple dependencies in field relations is a productive way to rethink the field. It helps re-frame questions such as the co-production of subjectivity, the mediation of meanings, and the host of anxieties that intersect in field-based research. Dependency undergirded and structured the relationship between myself and Saiful in ways that are perhaps more visible than in many researcher–assistant dynamics. However, such politics are central to all such relationships, both in the ways that they are framed through wage labor and employment dynamics and in the linked ways that research sites emerge out of mutual engagements and multiple histories. Dependencies, as such, are an unavoidable reality of working in ethnographic collaboration. Yet such dependencies need not be framed (solely) as lacks, shortcomings, or pitfalls. Rather, they also offer possibilities and avenues that otherwise would be blocked, or significantly changed. The key here is to confront the ways that dependency structures research, rather than to avoid and sweep it under the proverbial rug. Indeed, confronting such realities is a precursor to rewriting the field as a mutually constituted space.

It is clear that the dependencies of fieldwork are deeply idiosyncratic and, to recall Hammoudi and Borneman (2009), quixotic. That is, they are caught up in the messiness that emerges from the collision of political and ethical idealisms that undergird ethnographic practice and the impracticalities, impossibilities, and contingencies that erode and transform them in the field. As such, it is equally clear that any static theorization of dependency and fieldwork would be woefully inadequate – as inadequate as a general theory of researcher/assistant relations. Yet they remain central to shaping the kinds of data that emerge from ethnographic encounters and, consequently, the claims that ethnographers can make through them. As my relationship with Saiful suggests, the encounter between researcher and research assistant, and the emergent dependencies that govern that

relationship, often encompass much broader spaces and engagements than the narrowly defined 'field' itself. If my experience with Saiful does not, in and of itself, point a clear path towards a full rethinking of dependency, it does suggest ways in which considering this relationship more seriously is a worthwhile project. Taking the politics of dependency seriously provides a way to expose the limitations and anxieties of research, but it is also a path towards more fully understanding the quixotic production of ethnographic knowledge.

Acknowledgements

Notes

1. An exhaustive list of these interventions is beyond the scope or purpose of this essay, but a few key contributions to this debate are Rabinow (1977), essays in Clifford and Marcus (1986), Wolf (1992), essays in Amit (2000), Greenwood and Levin (2006), essays in Borneman and Hammoudi (2009), and essays in Faubion and Marcus (2009).
2. Saiful is a pseudonym.
3. The methodological literature on conducting ethnographic research in borderlands is somewhat limited (Cons, 2013; Megoran, 2006), though conceptually it shares many issues with the literature on conducting research in conflict zones and unstable places. There is, of course, a broad emerging literature on such issues. For discussions that specifically focus on the practice of ethnography in such contexts see essays in Greenhouse, Mertz and Warren (2002) and essays in Nordstrom and Robben (1995). For a discussion that links the possibilities of conducting research in such zones to working with research assistants, see Hoffman (this issue).
4. This organization continued to advocate for me and has been enormously supportive of my research though, for reasons beyond the scope of this essay to explore, I eventually declined this particular fellowship.
5. These disturbances culminated in the declaration of an emergency and the heralding in of an emergency government in January 2007. The emergency government would remain in power until December 2008.
6. I discuss the politics of sensitivity in detail elsewhere (Cons, 2008, 2011). For an additional account of the challenges of historical research on the enclaves, see Whyte (2002: 18–23).
7. For exemplary studies along these lines see Bourgois (1995), Bourgois and Schonberg (2009), Garcia (2010), Vitellone (2004) and Redding (2009).
8. For histories of the enclaves and their instabilities, see Cons (2012, 2013), Van Schendel (2002), Jones (2009, 2010) and Whyte (2002).
9. Dhaka University has historically been both an epicenter and a microcosm of politics in Bangladesh. These student politics – especially in the context of rivalry between the two main political parties in Bangladesh, the Awami League and the Bangladesh National Party (BNP) – have often been violent and dormitories have frequently been sites of struggle.

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Author Biography

Jason Cons is Research Assistant Professor in the Lyndon B. Johnson School of Public Affairs at the University of Texas, Austin. His research focuses on state and space formation along the India-Bangladesh border and agrarian change in rural Bangladesh. His work has appeared in *Third World Quarterly, Political Geography, Antipode,* and *Modern Asian Studies*. He is currently working on a manuscript tentatively titled *Sensitive Space: Anxious Territories at the India-Bangladesh Border*.