

Trading Up: Reflections on Power, Collaboration, and Ethnography in the Anthropology of Policy

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ABSTRACT: This article constitutes a pragmatic consideration of how to orchestrate access to ‘powerful’ individuals *and* a theoretical reflection on what efforts to negotiate access reveal about the anthropologist’s subterranean assumptions about power, collaboration and ethnographic data. Too frequently, powerful actors and the contemporary settings they inhabit appear to be obstacles to ethnographic research. In contrast, I propose that we explore the ways in which working with powerful actors can enhance, rather than inhibit, the possibilities of anthropological data collection. In this article, I present several examples from my field research in the Mexican government to show how the ethnographic encounter can be constructive of the political process, not just an appendage to it. By directing attention to the ways in which our actual research practices (and not just our findings) intervene in the political space, we can re-orient our expectations about data and the ontology of anthropological expertise.

KEYWORDS: Anthropology of policy, collaboration, elites, interviews, methodology, Mexico, research design

In the summer of 2006, a young Canadian named Kyle MacDonald succeeded in an ambitious endeavour to transform a red paperclip into a small home via a series of internet exchanges. The logic of the project was simple: by engaging in a series of asymmetric trades (potential traders had to be willing to offer something that was slightly bigger and better than the object MacDonald offered them), MacDonald would ultimately be able to secure the deed to a residence. The red paperclip was initially traded for a fish pen, which was then exchanged for a hand-made, ceramic doorknob, an object that attracted the attention of a doorknob enthusiast, who offered a gas grill in its place, and so on and so forth (see <http://oneredpaperclip.blogspot.com>). Four-

teen swaps and one year later, Kyle was the new owner of a home at 503 Main Street in Kipling, Saskatchewan.

The success of his venture owed to the confluence of multiple factors—the spirit of generosity (and perhaps novelty) on the part of his trading partners, his acumen in identifying auspicious trades and no small measure of luck. For example, Hollywood actor Corbin Bernsen’s penchant for snowglobes played a pivotal role in Kyle’s pursuit. Bernsen traded a cameo appearance in his upcoming movie for a particularly unique snowglobe to add to his collection. The cameo appearance attracted the interest of the residents of Kipling, who ultimately offered a piece of property in exchange for the opportunity to feature their community

in Bernsen's film. Absent any one of these factors, the series of trades would have ground to a disappointing halt.

This foray into asymmetric exchanges reminded me of my own experiences 'studying up' in the Mexican government. In order to conduct an ethnography of the policy making process in Mexico (and the role of economic ideas within it), I had to persuade government officials to talk to me without offering much in return (or so it seemed to me). I did not undertake the venture empty-handed—I had met a handful of mid-range officials while they and I were still students at the University of Chicago, before they had traded in their shredded backpacks for smart leather briefcases. These connections alone, however, would not be sufficient to procure access to the upper levels of the Mexican government because of the strict protocol of patron-client ties. Referring me to colleagues occupying lateral or subordinate posts would not diminish my contacts' political capital, but making similar requests of their structural superiors would. So while I could interview individuals at the same level of the bureaucracy *ad infinitum*, I would have to get a bit more creative if I wanted to creep upwards. I would have to find a way to trade up—to convert my red paperclip of mid-level contacts into upper-level contacts.¹

Over the past ten years, I have managed to climb the ladder. This article constitutes a pragmatic consideration of how to orchestrate access to 'powerful'² individuals *and* a theoretical reflection on what my efforts to negotiate access reveal about the anthropologist's subterranean assumptions about power, collaboration and ethnographic data. As scholars become more strident in their calls for anthropological studies in and of contemporary settings, many research agendas now stipulate ongoing contact with policy makers, government officials, employees of international financial institutions, heads of major corporations—people who can claim to be experts in their own right, sanctioned by the same

structures that underwrite our professional legitimacy (Wedel et al. 2005). Striking out in the world of boardrooms, government offices and corporations, anthropologists are confronted with situations that challenge many of our folk understandings of ethnographic field research. These range from seemingly mundane concerns about how to secure access to powerful actors, to ethical considerations of the potential vulnerabilities to which this type of research exposes the anthropologist and her interlocutors, to the epistemological ramifications of what it means for our own expertise when we work with experts.

The general tendency has been to regard these concerns as obstacles to ethnographic research. In contrast, I propose that we explore the ways in which working with powerful actors can enhance, rather than inhibit, the possibilities of anthropological data collection. Much of the anxiety about studying up derives from a tacit assumption of our own powerlessness in these settings. Well aware that ethnography involves an asymmetry of power, we have transposed the terms of the old model—anthropologist as powerful, interlocutor as marginal—on to field relationships with powerful actors, except in this case it is the anthropologist who becomes the marginal figure. However, we can only realise new forms of fieldwork relationships if we rethink this powerlessness. What are the assumptions about our own powerlessness that we bring into the fieldwork experience? Upon what implicit understandings of power do these assumptions rest, and how might this conception of power be revised?

In this article, I present several examples from my field research in the Mexican government to show how the ethnographic encounter can be constructive of the political process, not just an appendage to it. By directing attention to the ways in which anthropological research practices (and not just our findings) intervene in the political space, we can re-orient our expectations about data: data are not simply

the facts gleaned in interviews, but rather, the cross-cutting dynamics that permeate the political field. Our successes *and* failures in enrolling interlocutors can help adumbrate the political field by exposing the dynamics that are not always apparent in formal models. Quite simply, how and why (as well as why not) we achieve access to privileged actors and spaces tells us a lot about how those actors and spaces operate.

Focusing on the trials and tribulations of orchestrating access to powerful officials, then, underscores the vital role that forms of participant observation play—such as attending conferences, informal get togethers and so on—in setting up interviews in the first place. Thus, anthropologists of policy—as well as anthropologists from a variety of sub-fields who conduct research in multiple sites or in contemporary settings—maintain a commitment to participant observation through the very ways in which they manoeuvre through these sites. This article is an enjoinder to anthropologists of policy not to forget that the processes by which they gain access are just as significant in their ethnographic accounts as the information that they obtain through the interviews themselves.³ By no stretch of the imagination am I implying that we are as powerful as our interlocutors. Instead, I am proposing that we are not as powerless as we quite often think.

Trading Up, or Why Our Informants Talk to Us

For people whose livelihood depends on personal interaction, we anthropologists can be an insecure bunch. George Marcus and Douglas Holmes point out that ‘ethnographers trained in the tradition of anthropology do not approach the study of formal institutions such as banks, bureaucracies, corporations, and state agencies with much confidence’ (2005: 236). Abiding by the countless admoni-

tions to seek out new spaces for an ethnography that is responsive to and directly engaged with the forces of globalization (Merry 2001, Hoffman et al. 2006, Ong and Collier 2005), we are prone to wring our hands over why anyone ‘powerful’ would want to talk with us. Certainly this preoccupation is not unfounded—anxiety about access has a long history in anthropology. For years, anthropologists have landed in far-away places and relied on the benevolence of local communities to help them set up shop, circumstances that expose both the community and the anthropologist to potential vulnerability (Nelson 1996, see also Powdermaker 1966). In this regard, anthropologists who study ‘up’ tread upon not entirely unfamiliar ground.

That said, there is a way in which this anxiety, when it accompanies the enterprise of ‘studying up’ seems palpably new, for reasons that are both pragmatic and epistemological. First, if we ever did have captive informant audiences, we most certainly do not when ‘studying up’. Instead of being greeted as a curiosity, the anthropologist might be altogether ignored, for reasons of insufficient time or interest on behalf of a potential interlocutor. Moreover, even when the anthropologist manages to gain access ‘to study powerful national and transnational organizations (including the World Bank, the IMF, the BBC and the European Commission)’ this access often hinges ‘on the condition that the organization retain[s] a veto on what can be published’ (Nugent and Shore 2002: 11). Even among those who are willing interlocutors, the anthropologist might face at best disappointment or at worst potentially damaging reprisals if her conclusions do not match the perceptions and/or expectations of her informants (Forsythe 1999, Wedel, personal communication). At the level of epistemology, the anthropologist’s interlocutors may occupy institutionally sanctioned positions from which they can credibly critique the practice of ethnography and the validity of its data (Marcus and Holmes 2005). Thus, the list

of concerns runs the gamut, from the mundane to the epistemological.

It was with these concerns in mind that I meekly tiptoed into the offices of Mexican policy makers in July 1997, fairly certain no one would talk to me and afraid of what they might say if they did. Slowly, I came to the realisation that my initial timidity and apprehension were largely unwarranted. Though my research was not without its ups and downs (more of this below), the majority of my interlocutors was reasonably receptive to my research and willing to participate. In rare cases, several of them sought me out. As good technocrats armed with PhDs in economics and well acquainted with the disciplinary configuration of social science in the US academy, they were vocal in their skepticism of the 'objectivity' of anthropological field research, but were accommodating with their time and insights. So much so, in fact, that they prompted me to re-examine my somewhat misguided assumption that access would be an interminable struggle, one that I think many anthropologists who work with powerful actors share. The mismatch between my assumptions and my actual experience of access raises the question, where does this feeling of marginality come from? According to what implicit calculus of power is this presumed powerlessness based?⁴ With this question I do not intend to imply that a differential does not exist, but rather, to probe the precise ways in which anthropologists *apprehend* this differential and its significance for how they understand the challenges of access. Since this line of questioning engages self-understanding as much as it does interlocutors' perceptions of the anthropologist, in addition to exploring *why* people talk to anthropologists, it is necessary to examine why anthropologists of policy often suspect that they *will not*.

This persistent anxiety is rooted in the spatial metaphor that underlies the notion of 'studying up.' 'Studying up' implies that the anthropologist occupies a permanently subordinate position in an ineffable hierarchy. Our

best ethnographic sensibilities have heightened our awareness that the field encounter necessarily involves a differential of power. The self-reflexive turn in anthropology in the mid-1980s (Clifford and Marcus 1986), and the consequent post-modernist emphasis on the fundamental partiality of representation (Jameson 2002), have drawn attention to the way in which anthropologists give voice to people who do not have one, thereby unwittingly reproducing, rather than transcending, patterns of colonialism and modes of representation. I contend that we have inadvertently transposed the differential between ourselves and the subaltern to the context of powerful institutions, imagining that if our 'powerful' interlocutors are the ones who have the real voice, then we must properly have none at all. Ironically, anthropologists are transformed from unwitting imperialists to helpless victims by virtue of who they happen to be talking to. This view reduces the anthropologist to a supplicant, rather than an active agent in the ethnographic encounter. The roles of 'powerful' and 'powerless' are cast *ex ante*, admitting no possibility of a protean gradient between the two poles.

Yet as Foucault pointed out so long ago, power is not a one-way street. While the encounter between anthropologist and her interlocutor is undoubtedly framed by relations of authority, it is not entirely pre-determined by them:

[P]ower, if we do not take too distant a view of it, is not that which makes the difference between those who exclusively possess and retain it, and those who do not have it and submit to it. Power must be analysed as something which circulates ... It is never localised here or there, never in anybody's hands, never appropriated as a commodity or piece of wealth. Power is employed and exercised through a net-like organization. And not only do individuals circulate between its threads; they are always in the position of simultaneously undergoing and exercising this power. They are not only its inert or consenting target; they are always also the elements of its articulation' (Foucault 1980: 98)

Placing these comments in the ethnographic context suggests the need to think not only about how power is exercised *in* research, but also about how power emerges *through* the research encounter. If power is understood to be prefigured and unidirectional, then the anthropologist *is*, indeed, relatively powerless. What I react to when I consider my powerlessness in relation to the Minister of Finance of Mexico or the head of a Fortune 500 Company is not a patently obvious difference in status, but rather, power in its crude sense. The Minister of Finance and the CEO have the force of bureaucracy standing behind them, which implies that they possess more extensive means to enforce their will than I do. Their individual actions have greater global significance than mine do. And if I irritate them, they can direct that power against me. Thus, the sense of powerlessness to which the anthropologist responds when she regards herself as situated somewhere 'below' a powerful actor is predicated on a narrow, one-sided conception of power—the same mechanical definition of power that the anthropological account of governmentality and disciplinarity has steadfastly resisted (see Foucault 1991, Dean 1999).

Yet, if power in ethnographic research is, instead, regarded as partially emergent, then the anthropologist gains significant ground. Accordingly, power does not reside in the hands of one person directed against another, but rather, emerges through complexes of power and knowledge (Foucault 1980) and is simultaneously repressive and constructive. To be sure, repressive power exists and can be harmfully directed against the anthropologist. But the constructive element of power in the ethnographic setting has, in my estimation, been overlooked. Central to recuperating our self-esteem is recognizing how this constructive, emergent and contingent dimension of power re-aligns the fieldwork encounter.

The asymmetry between the anthropologist and her interlocutor cannot be fully mitigated, but it can be put to productive uses that facili-

tate ethnographic knowledge rather than simply obstruct it. In his seminal contribution to the methodology of interviewing, Briggs (1986) noted that because informational frames vary by culture, the communicative assumptions embedded in the Western method of interviewing may not be shared by the researcher's interlocutors. This disjuncture does not render the communicative effort impossible; rather, it requires the researcher to reflect upon culturally specific communicative practices and how they might be bridged. Similarly, 'studying up' should not become a pretence to abandon our attentiveness to cultural and communicative differences (Coe 2001). Just because one's interlocutors may be cosmopolitan does not mean that they are not enmeshed in institutionally specific strategies and norms of conversation. Recognising this fact enables the anthropologist to experiment with communicative frames and practices not with the aim of extracting otherwise privileged information, but rather, to expand the domain of negotiation between herself and her interlocutor. The outcome of these negotiations of power will vary according to the research context. Ascertaining these processes not only enables new and creative strategies for gaining access, but they also expose emergent fields of social and/or political interaction of which the anthropologist may otherwise have been unaware. Quite simply, anthropologists do not arrive empty-handed, and much of the challenge of ethnographic field research among powerful actors is learning what, precisely, the anthropologist brings to the table.

In my own case, there were several dynamics that greatly facilitated my quest to 'trade up'. Several are obvious, others less so, but they all bear pointing out in order to disembell the anthropologist's nagging inferiority complex. The first of these dynamics is something that I might call the 'prestige effect'.⁵ I was surprised to learn that once my study had reached a critical mass, talking to 'the anthropologist' acquired a certain cachet. I did

not directly witness this effect, but I learned through intimations by my interlocutors that such conversations had taken place. Several of my interlocutors reported to me that at various soirées, participants in my research were asking one another, ‘Have you talked to the anthropologist? What did she ask you?’ Even the most educated elites cannot quite figure out what an anthropologist is doing out of the jungle, and they seemed to gamely engage the topic of what it was that I was trying to figure out. While I am sure my research provided rich fodder for cocktail party conversations, I gradually learned that aside from being left out of social chatter, being excluded from the research could have the unintended consequence of suggesting that an individual is not important enough to be considered, a conclusion that most political actors would prefer to avoid. One day I walked into a lush executive suite that looked like a page out of *Architectural Digest*, awash in deep jewel tones and luxurious textures—supple leather, rich velvet, polished hardwoods. Before I could open my mouth to comment on the impressive décor, the occupant smiled wryly and said with a contented sigh, ‘I’ve been waiting for you to come’. This brief but memorable opening heightened my awareness of how my movements through the political field were being carefully monitored by my interlocutors. I began to pay closer attention to these cues in my conversations, and they helped me sketch the contours of functioning networks.

In the aforementioned example, my presence became inserted into the social relations of Mexican government officials. Yet there was another way in which my presence became an active force in the making of the political field in Mexico. In my first extended field research trip in Mexico, I interviewed nearly 150 government officials, domestic political actors, and representatives of international financial institutions. The bulk of my data consisted of interviews with Mexican government officials, as well as direct observations of policy making

discussions, in order to reconstruct the meetings between officials that occurred during the social security reform process (approximately 1993–1995). Since I did not go into the field until well after the process had been completed, I had to interpolate my observations of contemporary policy discussions with the content that was recounted to me by the people who had been present at the actual meetings.

At the end of each interview, without fail, the officious and straightforward bureaucratic tone melted into coquetry as my interlocutor would demurely ask, ‘Who else have you spoken to?’ Clearly, this constituted more than an attempt at small talk, for I could see how my interlocutors focused intently on the names I listed. Occasionally, an interlocutor would directly ask me whether I had talked to a specific official. At first I thought it a striking coincidence, but as the scenario continued to repeat itself, I knew that there had to be another explanation for this uncanny correlation. I slowly came to grasp the significance of this question when I started mapping out the chains of referrals through which I had moved. I had made a practice of indicating in my field notes who had referred me to whom, and after several months in the field, I thought it might be useful to have a visual rendering of these networks. It was only once the diagram was complete that I recognised its significance, and why my interlocutors were so interested in the pattern of referrals.

To appreciate the value of the diagram requires a bit of background information about the Mexican political system. *Camarillas*, or political teams, are central institutional forms in contemporary Mexican politics (Smith 1979, Camp 1996). *Camarillas* are pyramidally structured, client-patron networks established through academic, professional, and/or social connections, and two major *camarillas* dominate the political landscape, cross-cutting the bulky Mexican bureaucracy (Camp 2002, Gledhill 2002). Paradoxically, despite their legendary significance for Mexican

politics, the shifting membership of *camarillas* is elusive, even to the most seasoned political actors. *Camarilla* membership may change, and no one is privy to all of the details of the movements of these political actors. Much time is spent trying to ascertain the shifting dynamics of *camarilla* membership, yet I had inadvertently generated a map of connections that roughly approximated *camarilla* membership. I was not fully convinced of the meaning of these data until a few months ago when I delivered a presentation to a group of public policy students at a prestigious public institution. Several Mexican students came up to me after the presentation to discuss various aspects of the presentation, and as our conversation turned to the issue of access, I joked that it had taken me several years to get a chance to talk with certain people. The two students did not appear surprised. When I mentioned that I had compiled an exhaustive list of all of the various connections, one of the students look at me in shock and exclaimed, 'You could plug all of that information into a social networking program and make a fortune!' His comment confirmed my suspicion that intensely local information was not only bounded and made legible through my physical movements, but achieved the status of a commodity. In the course of field research with Mexican policymakers, I came to realise that my physical movement through the bureaucracy was creating a living archive of the structure and membership of *camarillas*. The conventions of access in Mexico are highly regimented. As I mentioned in the introduction, it is fairly easy to secure an interview with laterals, a fact that had not escaped my interlocutors as they tried to excavate the *camarillas* through my real-time movement through the bureaucracy. My movements left footprints, and my interlocutors were quite eager to see exactly where those footprints led.

Though I had never imagined that I would silently tiptoe through the Mexican bureauc-

racy, I was nonetheless surprised by this unintended consequence of my research. I was trying to learn about the Mexican bureaucracy through my interlocutors, and they were trying to learn about it through me. Our agendas did not perfectly intersect, but there were distinct ways in which we were mutually useful to one another. These overlapping agendas suggests that both I and my interlocutors were adhering to an understanding of the ethnographic encounter 'as a form of learning, rather than absolutely, as a form of representation' (Whitaker 1996: 1). Just as I was experimenting with techniques to gain access and information, my interlocutors were doing the same.

The practical consequence of this finding is that relationships shift and evolve over the course of field research, and thus, so do the conditions of access. In the anthropology of policy (as I imagine in other sub-fields as well), persistence pays off. If the door gets shut in one's face the first time around, then the anthropologist should look for alternative entrances—side doors, windows, back doors. It took me nearly six years, on and off, to establish contact with a well-known Mexican politician. As with many prominent politicians, he was a rather controversial figure. At the time I conducted my initial field research in 1999–2000, I had been told by a number of people that he would be difficult to contact and hesitant to talk to me for fear of questions about the 1994 peso collapse in Mexico, which some had attributed to his mismanagement. Nevertheless, I dutifully started harassing his secretary and sending detailed faxes, to no avail. My work was proceeding well in other directions, so I eventually let the campaign lapse. Six years later, as I was preparing my book manuscript, I noticed that he was scheduled to deliver a keynote address at a conference hosted by the Graduate School of Business at the University of Chicago, my home institution. The conference was held in an intimate venue,

and presenters and attendees were encouraged to mingle at roundtables set up around the room. I arrived at the conference a few minutes late, only to discover that one of the few remaining seats in the room happened to be at the table where a number of presenters, including the former official I was interested in meeting, were seated. I nervously weighed the pros and cons of perching myself in the seat, and ultimately concluded that I did not have anything to lose. I sat down, and was graciously welcomed by the various presenters. At the break, I went right up to the official in question, handed him a business card, and declared, 'I have wanted to meet you for a very long time'. He responded in a kind and avuncular manner: 'But my dear, why didn't you call me?' to which I replied, 'I did. You never returned my calls!' Not missing a beat, he politely grasped my hand and promised that from then on, he would. He is now a faithful phone and email correspondent! The great convenience of interviewing powerful people is, to be blunt, that they appreciate a fighting spirit. Tenacity and ingenuity can reap benefits that they may not as easily reap in other fieldwork contexts.

The old adage about being in the right place at the right time definitely holds true, but it means that the anthropologist has to be awfully strategic in finding the right place. Often the right place is a professional conference,⁶ but sometimes the right place may be a completely unofficial locale. While waiting to conduct an interview with one of the managing partners of a major brokerage house in Mexico, I decided to grab a cup of coffee at the local version of Denny's, known as VIPS. This particular VIPS had an unusual nautical décor, and I was focusing on one of the life preservers nailed to the wall when I walked a rather tall, grey-haired man in a dark suit. I would not have paid much attention to him, except that his presence had generated quite a bit of enthusiasm among several groups of young people seated in the restaurant. He

came up to them and shook their hands, and I wondered if perhaps he was the owner of the brokerage house. My suspicion was that he was, but I did not have enough chutzpah to approach him. Since I had never actually seen a picture of the owner of the brokerage house, I could not tell for certain if the man before me was, indeed, he. I would have saved myself a lot of time had I simply struck up a conversation with him right there, but I did not. Several months later, when I returned to the U.S., I stumbled upon a picture of the owner of the brokerage house and, guess what? He was the one at the coffee shop. The moral of the story is never forget the practical details, like pictures.

I have sprinkled this section with bits of pragmatic advice and methodological insights gleaned from my field research experiences in Mexico to stress the improvisational character of the anthropology of policy. A successful anthropology of policy involves strategy *and* serendipity, not one or the other. Ethnography allows for flexibility in research methods, but that flexibility is not practicable if the ethnographer is not constantly keeping abreast of how his 'powerlessness' is manifested in specific contexts. Practical issues of access to powerful figures force us to unearth deep-seated assumptions about issues ranging from the mundane details of ethnographic fieldwork to the epistemological position of the ethnographer. Many of these concerns are not the exclusive province of the anthropology of policy, nor do I wish to imply that they are. Rather, I wish to suggest that the institutional structures in which anthropologists of policy may locate their research should not always be viewed as obstacles. Instead, these structures may be generative of novel forms and strategies of engagement that not only enable the anthropologist to press forward more confidently, but also offer a window to dimensions of power that might have otherwise remained hidden from view.

Rolling Up Our Sleeves and Getting Dirty: A New Metaphor for Collaboration

Last year in New York, I had the privilege of engaging in an extensive and thought-provoking discussion with a well-known political figure in Mexico. He spoke frankly and unself-consciously about his successes and failures in government, as well as those of his colleagues. While he was clearly proud of the accomplishments of NAFTA (North American Free Trade Agreement), he lamented the ongoing difficulties experienced in the southern states of Mexico:

In many ways, NAFTA was a success. We had two objectives with NAFTA. The first was to increase foreign investment, which we did by 400%. The second was to increase foreign trade, which we did by 350%. You may say that it should have been 500%, but these percentages are pretty good. But what didn't NAFTA do? Well, if you look at the map of Mexico and at the numbers, 80% of trade is from Mexico City north. NAFTA did absolutely nothing for the South, and this is what we hear from the election results. It is not about blue states vs. yellow states. In the U.S., you already fought the civil war. In Mexico, we must find a way to bring the results of NAFTA to the southern part of the country. There is a real lack of infrastructure there. That is the big problem (personal communication, 25 July 2006)

In some ways, his comments do not depart dramatically from the paradigm of economics in which he was trained. By framing the difficulties of southern Mexico in terms of lack of infrastructural development, he implies that progress for Mexico entails more closely aligning the rates of economic development in Mexico's north and south. But as our conversation unfolded, he conveyed a surprising openness to other forms of expertise: 'You know, there are some things that you can't understand in terms of relative price. We must never underestimate the importance of individual actors, and this is what you, a social anthropologist,

can tell us about' (personal communication, 25 July 2006). I found it ironic that at a time when anthropologists are experiencing something of an identity crisis my economist friend could articulate such a clear vision of the nature of anthropological expertise. His implicit regard for the limits of economic knowledge seems to fit rather cozily with Marcus and Holmes' notion of the 'para-ethnographic dimension in such domains of expertise—the *de facto* and self-conscious critical faculty that operates in any expert domain as a way of dealing with contradiction, exception, facts that are fugitive, and that suggest a social realm not in alignment with the representations generated by the application of the reigning statistical mode' (Marcus and Holmes 2005: 236–37). Para-ethnography concerns those moments when the facts do not fit and the (usually) positivist policy maker, economist, central banker, and so on relies on decision-making criteria that are not legitimated by the positivist paradigm—intuition, hunches, informal information. The para-ethnographic framework admirably seeks to transcend the dogged oppositions between the inside and outside of knowledge that re-inscribe the uncomfortable and deeply problematic distinction between us and them upon which the epistemology of anthropology of knowledge has traditionally rested. In this endeavor, they are in good company (Riles 2004; Miyazaki and Riles 2005).

But the question is does my Mexican colleague want to inhabit a para-ethnographic space with me? His comments rather suggest the opposite—that the bridge that links us in collaboration exists because of the disciplinary boundaries that separate us. In my experience, it is the range of responses to anthropology and the methodology of ethnography that has provided the most fertile ground for discussion and debate with my interlocutors. Despite varying degrees of receptivity to the concept of ethnography, my interlocutors had a surprising amount of faith (sometimes more than I did) that I would be able to ascertain aspects

of their daily practice that they did not already know. While some of them voiced doubts about the nature of my expertise as it pertained to policy decisions (e.g. whether ethnographic data could be put to productive use in the realm of policymaking; see Apthorpe 1997), they did not openly challenge my status as an expert. Other anthropologists—especially in the field of science and technology studies, where engaging contrasting expert epistemologies has a comparatively longer history—have reported similar experiences (Rabinow 1996, Traweek 1992). If my authority had not been respected, then I doubt I could have achieved such high rates of participation. Mexican technocrats are not known for having a lot of time on their hands, and their willingness to sit and talk with me, at times actively reflecting on many of the same issues, was encouraging.

I propose a somewhat different model of interaction to address the epistemological challenges raised by conducting anthropology with powerful, often expert, interlocutors. Instead of trying to transcend the distinctions that structure the epistemology of anthropology—for example, self versus other, powerful versus powerless—we put them into dynamic and contestable play. As my previous discussion has indicated, we almost never find ourselves in the land of absolutes—the anthropologist is never totally powerless, the expert never completely ‘inside’ of his own expert knowledge. Instead, each ethnographic encounter exists on a gradient, and the position on that gradient can shift within a single encounter as well as between encounters. If we abandon the absolutes and, instead, regard ethnographic situations as involving sets of equalities and inequalities, sets of shared notions and dissonances, then new zones of ethnographic engagement and collaboration emerge. Instead of looking for an epistemological convergence, we would be seeking points of epistemological ‘friction’ akin to the zones of awkward engagement that Anna Tsing describes in relation to spaces of globalization. For Tsing, ‘cultural diversity brings a

creative friction to global connections’ (2005: x). The same might be said of epistemology: the moments of friction offer the greatest potential for permutations, for novel combinations that cannot be anticipated through the lens of an *ex ante* methodological stance.

The epistemological engagement I propose echoes Kwame Anthony Appiah’s stance vis-à-vis universal liberal values and relative local values. In *Cosmopolitanism*, Appiah ponders the inconsistencies of liberalism and pluralism, asking the simple question of how we reconcile the universals of liberalism—which, despite all of its failings, many still regard as admirable—with the particulars of the world. For him, the answer is simple and poignant at the same time. They do not fit together easily, but that does not mean that we should discard both in favour of a synthesis:

There are some values that are, and should be, universal, just as there are lots of values that are, and must be, local. We can’t hope to reach a final consensus on how to rank and order such values. That’s why the model I’ll be returning to is that of conversation—and, in particular, conversation between people from different ways of life. The world is getting more crowded. ... Depending on the circumstances, conversations across boundaries can be delightful, or just vexing: what they mainly are, though, is inevitable (2006: xxi).

Appiah’s perspective is refreshing for its pragmatism. Rather than trying to reconcile diverse perspectives *ex ante*, he contends that tolerance, respect, and understanding for one another can only emerge through contact, conversations, and the very simple act of rubbing shoulders.

I had the experience of rubbing shoulders at a recent conference about NAFTA. Much to my surprise, I was a presenter on a panel that included several of the original architects of NAFTA. Though I have conducted limited field research on the negotiation of NAFTA, my findings were preliminary and I did not particularly feel like testing my hypotheses on such a well-informed group. Instead, I

presented a paper in which I argued, a la Polanyi, that there was a world of difference between the concept of free trade and free trade agreements, and thus, that our goal should not be to make trade as free as possible, but to understand the ways in which the very idea of free trade requires negotiation among different cultural, political and economic registers. I fully anticipated being ambushed by the free trade advocates in the audience, but when it was time for questions, several of the original negotiators concurred with my points, and several indicated to me that they would like to read Polanyi. Ultimately, what matters to me is not whether they agree with Polanyi, but that they regard the text as worth reading. This is one way of extending one epistemological space to include another.

The physicality of rubbing shoulders is perhaps what makes it such a useful metaphor. It is a clumsy act that does not always end cordially. Appiah and Tsing are the first to admit that conversations across difference are inevitably fraught, sometimes explosive, and always messy. These types of encounters invariably run the risk of misinterpretation, hard feelings and misunderstanding. Often the anthropologist may not be able to comply with the hopes of his interlocutors (Miyazaki 2006). But the very direct collisions that result from conversations across differences have the advantage of being explicit, with enormous potential for improvisation. Unmoored from an *a priori* scheme of how epistemologies can merge, creative friction allows for the development of new, hybridised epistemologies that complement, rather than supplant one another. The form of collaboration that emerges from such experiences need not involve a coincidence of ultimate aims or final conclusions. Alternatively, it is experimental and contingent by virtue of its epistemological elasticity. I am applying the term collaboration liberally to encompass active participation, or reflexive engagement with the limits of specific epistemologies, be they anthropological, economic,

sociological, technological, political. As Tsing observes, 'There is no reason to assume that collaborators share common goals. In transnational collaborations [of capital], overlapping but discrepant forms of cosmopolitanism may inform contributors, allowing them to converse—but across difference' (Tsing 2005: 13). As the examples I have adduced suggest, collaboration occurs across multiple axes and may involve factors that remain out of view to the anthropologist, at least initially (cf. Lasziter 2005). I was inadvertently collaborating with them, as they were with me. It would be a mistake to foreclose these types of collaborations by prematurely pigeonholing the form they may assume.

The anthropology of policy is uniquely situated to nurture these forms of collaboration because of the pragmatic bent of policy makers. As I have pointed out, respectful conversations with other experts need not involve a contest of epistemologies. Given the sociology of professions, we are more likely to encounter epistemological contests among our colleagues within the academy than those in the business of policy making (see Abbott 1988, Boyer this volume), if only because policy makers do not have much patience for petty disciplinary turf wars. Scholars argue about poaching on one another's turf; policy makers are far less territorial in this regard (though not in others). Policy is about negotiating pragmatic solutions and coming up with creative strategies for forging consensus. The conversational metaphor offers a new path for anthropological work to be relevant and speak to the needs of the public. By actively participating in the conversations through which these frameworks have been elaborated, we have made a central, though difficult to discern, contribution.

This solution is clearly not feasible in all fieldwork scenarios, and I do not offer it as a panacea. The anthropologist cannot avoid stumbling upon formidable roadblocks. But maybe there is something valuable in facing these roadblocks directly, rather than in try-

ing to avoid them. Perhaps the anthropology of policy can learn more from rolling up its sleeves and getting dirty than it can from trying to avoid messes altogether.

A Cautionary Coda: Is Up Where We Want to Be?

A coda is in order, one whose significance must not be underestimated. 'Up' may not be exactly where we ultimately want to be. Ethical entanglements notwithstanding (see Forsythe 1999), there is a deeply ingrained, Weberian tendency to misrecognise an institutional hierarchy as an informational hierarchy. I can never forget my own eagerness, early on in my dissertation research, to score an interview with a top official in the Mexican government. Almost instinctively, I reveled in the glorious details that would be revealed, the controversies that would be dispelled once I secured the panoptical perspective of he who had the ultimate authority. As the case may be, the interview did not live up to these heady expectations, and I doubt it ever could have. I expected the hierarchy to do so much work *for* me—I expected structural position to stand in for bureaucratic negotiation, I expected clear-cut power relations to eliminate grey areas and multiple interpretations. Of course I should have known all along the reality was far more messy, but I think that I am not the only one to succumb to the tantalising fantasy that structural position confers omniscience, that at the top of the pyramid stands a man (and in the case of the Mexican government, it is inevitably a man) who knows the outcomes of the political intrigues, who can refute alternative descriptions with concrete facts and dates.

Thank goodness this fantasy did not bear out, because if it had, ethnography in institutional settings would be a difficult prospect indeed. If we draw from Anna Tsing's notion that an anthropological project can be 'deeply ethnographic in the sense of drawing

from the learning experiences of the ethnographer' (2005: xi), then the anthropology of policy conducted within the rigid structures of hierarchy, even within multiple, cross-cutting hierarchies, would lose its allure. If the exercise becomes purely fact-finding, then it loses its anthropological basis.

It can actually be deeply disappointing to talk to the people who, by virtue of their structural position, should know it all. The project of modern forms of liberal statecraft is to entertain dissonance and to erase its traces. A policy can be hotly contested, yet once it is adopted, it has no less force of law than a measure that was less controversial. Laura Nader's (1972) invocation of the term 'studying up' was a historically specific manoeuvre that has been of enormous service to the field of anthropology by challenging its pre-conceived notions of appropriate research subjects. That said, now that these types of projects are gaining increased acceptance, it may be time to replace 'studying up' with Reinhold's (1994) notion of 'studying through' in order to remove the ingrained assumptions about the structure of information within governments (and other bureaucratic structures) that may inadvertently inform our research strategies (Shore and Wright 1997). When pursuing individuals in high positions, we should be careful to consider what we are hoping to gain from them. We must not mistake structural position for omniscience.

Conclusion

In this moment of reckoning about how anthropology will differentiate itself from other social scientific disciplines and conventions of knowledge production, fieldwork has been under intense pressure from all sides. The limitations of place-based studies have been carefully enumerated (Marcus 1998), and the need to directly address the global processes in which local communities find themselves enmeshed has been clearly articulated (DeHart

et al. 2006, Ong and Collier 1995). Nevertheless, the problematic persists: if other experts are now relying on forms of ethnographic field research and our own research agendas render 'thick description' and its associated modes of apprehension obsolete, then what is the anthropology that is used to foreground our investigations? What is anthropological, for instance, about the anthropology of policy?

The preoccupation is real and the problem more than any one of us can tackle single-handedly. Much more sustained discussion will have to occur. But a particularly fertile spot of soil on which to plant this discussion, I think, is the persistent silence about fieldwork. While anthropologists are keen to present the results of fieldwork in highly polished and sanitised monographs or journal articles, the nuts and bolts of field research—uncertainties, failures, misunderstandings—remain confined to tightly controlled, informal conversations (Rabinow 1996). Yet it is very often in the process of 'thinking through' these pragmatic obstacles in the field, or in the writing process, that we are led to the most engaging facets of social life. This process gets buried because there is no comfortable place within the discipline in which it can be openly discussed. Of course, the temptation is to mistake the anthropologist's own internal struggles for the social lives that she hopes to observe. Okely (1992: 2) carefully distinguishes this attentiveness from self-absorbed 'navel-gazing': 'the concern for an autobiographical element in anthropology is to work through the specificity of the anthropologist's self in order to contextualise and transcend it'.

Retooling anthropology partially depends on acknowledging and making explicit the ways in which the pragmatic, methodological and theoretical concerns are intertwined. For example, an anthropologist's interview is not like a political scientist's interview. The anthropologist is looking at the interview as a register of multiple orders of information, not just the 'facts' contained within.

The anthropologist probes the instabilities, the inconsistencies and foregrounds them in relation to similar experiences in order to discern salient patterns, whereas the political scientist is generally interested in the factual content of the interview. Just because we use interviews, then, does not mean that we have ceased to be anthropologists, or others more so. When 'site' is no longer defined by geography, but has to be simulated or improvised, the dynamics of facilitating research become part and parcel of ethnographic data. In this article, I have reflected on the variety of ways in which I have attempted to trade up, sometimes successfully, sometimes unsuccessfully. The dynamics that punctuate the Mexican political field certainly do not exhaust the range of possibilities, and I expect that other anthropologists have much more to offer. The purpose of this article is to bring these discussions into the open and to focus attention on the everyday experiences of fieldwork and how they inform the conceptual apparatuses with which we apprehend the field and the dynamics within it.

Returning for a moment to the opening epithet about the red paper clip, the series of exchanges proceeded neither entirely from the largesse and/or altruism of the various participants, nor from pure self-interest. A number of factors conspired to enable the young Canadian to obtain the title to a home with the initial investment of a red paper clip. The anthropology of policy, especially as conducted in institutional settings, involves the same messy combination of motivations. With a little bit of whimsy, creativity and serendipity, a red paper clip can become something more. With a little bit of whimsy, creativity and serendipity, the seemingly powerless anthropologist can become something more as well.

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Notes

1. This article takes as a point of departure the observation that the anthropology of policy relies heavily on interviews as a source of data. There are multiple reasons for this, but perhaps the most oft-cited include the difficulty of obtaining access to real-time policy discussions as well as the analytical problem of being able to fully elaborate the effectiveness of various networks and actors as the process unfolds. By foregrounding the issue of access as it specifically relates to interviews, however, I am not making a normative claim that interviews *should* supplant participant observation. Instead, I intend to critique specific notions of participant observation and interviews that regard them as separable moments in the ethnographic experience. The examples I provide suggest that much of the work anthropologists do to gain access is itself a form of participant observation.
2. I have somewhat reluctantly chosen the rather loaded and fraught term 'powerful' instead of 'elites' to draw attention to the conventions by which we differentiate ourselves from our interlocutors. While very few anthropologists would consider themselves 'powerful,' 'what could be more elitist than anthropology itself, a profession steeped in the traditions and practices of Western middle-class academics, most of whom possess doctorates from the most exclusive universities, whose scholarly output is aimed primarily for consumption by other, middle-class intellectuals?' (Shore and Nugent 2002). Though we may qualify as elites, we do not generally, as a group, consider ourselves 'powerful'. In fact, much of the anxiety about studying 'up' involves the very simple fact that we see the power of our interlocutors as a barrier to the ethnographic project.
3. There is a long tradition of interviews in anthropological research, and my comments here should not be understood as suggesting that interviews are somehow new to the field research experience (see, for example, Nash 1979). What concerns me here is the significance that the issue of access has for anthropologists of policy and their sense of what constitutes data.
4. The self-perception of powerlessness of which I write must be qualified by Peter Pels' (1997: 164) astute observation that 'the discipline [of anthropology] descends from and is still struggling with techniques of observation and control that emerged from the colonial dialectic of Western governmentality'. The very structure of the global political economy is such that many anthropologists hail from relatively powerful states, with the implication that they may be more powerful than they feel themselves to be. The vagaries of geopolitics play a significant role in the position that an anthropologist assumes in a particular community. At wartime as well as throughout the Cold War, anthropologists had both 'witting and unwitting links to intelligence agencies' in the U.S. and the U.K., thus rendering the anthropologist more spy than supplicant (Price 2002). Laura Nader (1997: 109) and David Price (2002), among others, have been particularly vocal about the need for the discipline of anthropology to confront 'the undocumented relationships of co-dependence between anthropologists and the military-industrial complex'. Thus, I am not seeking to make the generalised claim that

anthropologists are structurally powerless but, rather, to explore those situations within the sub-discipline of the anthropology of policy in which anthropologists consistently perceive themselves to be.

5. The specific nature of this effect depends on a number of factors, including the anthropologists' gender, age, nationality and educational background. Nearly all of my interlocutors in the Mexican government were men, and I have a distinct sense that part of my ability to gain access was related to the perception that a woman is somehow less threatening. Several other women anthropologists with whom I have spoken have had similar experiences. Once again, this suggests that access can be negotiated along a variety of axes.
6. Attending the conference is not enough. I have been known to attend conferences armed with a hit list of names and systematically proceeding through the list during coffee breaks, cocktail hours, and miscellaneous receptions. Prior to attending the conference, I have mentally reviewed all of the potential connections that might help me establish a link to a prospective interlocutor. What schools has he attended? What ministries have they worked in? Do they know anyone who I know? Have they published on topics that I am researching? I rehearse my brief introduction so that I get right to the point. It takes only a few minutes to interest someone in a research project, and only a few minutes for them to lose interest entirely.

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