

Title: Trouble in Para-Sites: Deference and Influence in the Ethnography of Epistemic Elites

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Abstract: Through his enduring efforts to interrogate the regulative ideals of fieldwork, George Marcus has empowered doctoral students in anthropology to rethink their ethnographic encounters in terms that reflect novel objects and contexts of inquiry. Marcus' work has culminated in a charter for ethnographic research among 'epistemic communities' that requires 'deferral' to these elite modes of knowing. For adherents to this programme of methodological reform, the deliberately staged 'para-site' – an opportunity for ethnographers and their 'epistemic partners' to reflect upon a shared intellectual purpose – is the signature fieldwork encounter. This paper draws on doctoral research carried out among the overlapping epistemic communities that comprise London's market for mining finance, and reviews an attempt to carve out a para-site of my own. Troubled by this experience, and by the ascendant style of deferent anthropology, I think through possibilities for more critical ethnographic research among epistemic elites.

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'First, "our methods," that is the practices of ethnography, have been assimilated as key intellectual modalities of our time' (Holmes and Marcus 2008: 84).

'What should we do so that our understanding of the world does not purely and simply coincide with the spirit of capitalism?' (Galloway 2013: 352).

Introduction:

In this article I engage with George Marcus' efforts to rethink the design of ethnographic fieldwork such that it keeps pace with changing objects of study in anthropology. I start by reviewing Marcus' evolving approach to what he calls the ethnographic *mise-en-scène*, or 'staging' of fieldwork. One of Marcus' key contributions has been his attention to the regulative ideals of fieldwork, or the disciplinary norms regulating ethnographic research, that take account of the cognitive *and* ethical implications of carrying out anthropological research at different moments in world history. I go on to review my own anthropological education and the regulative ideals I was exposed to as an undergraduate and taught postgraduate. Encapsulated in the notion that anthropologists are 'for the little guy' (Graeber 2002), my internalization of these ideals made designing doctoral fieldwork to be carried among elites in London's mining market particularly challenging. To do so, I returned again to Marcus' later work on collaborative research in 'epistemic communities' – or communities of experts whose members are always already carrying out research. Despite my reservations regarding the notion that adequate and effective fieldwork carried out among contemporary epistemic elites should involve 'deferral' to their modes of knowing (Holmes and Marcus 2008), I attempted to carve out space for exploring intellectual partnership with the members of one of the mining market's epistemic communities – what Marcus terms a 'para-site.' The productive, but hardly collaborative or deferent form that this para-sitic encounter took led me to question the viability of Marcus' agenda for the ethnographic exploration of 'harm industries' (Benson and Kirsch 2010) like mining, and I conclude by gesturing towards a slightly more provocative form that the ethnography of epistemic elites might take.

Becoming an Anthropologist

While George Marcus is perhaps best known as the primary proponent of ‘multi-sited ethnography’ (Marcus 1995), his apparent advocacy for a globe-trotting anthropology was but one moment in a much broader project. The purpose of this has been to rethink the regulative ideals of fieldwork. Treating the fieldwork encounter as a *mise-en-scène*, Marcus has made it his business to diagnose and interrogate, for successive generations of anthropologists, the ethical and cognitive ideals that underprop ethnographic research. Thus Geertz’s (1973) fabled account of gaining acceptance into his Balinese fieldsite describes the achievement of ‘rapport’ born out of complicity. Geertz’s rapport with his ethnographic subjects arose from an incident during which he became, along with his erstwhile hosts, a fugitive from police intent on disrupting an illegal cockfight. Rapport of this type became, for those trained from the 1950s to 1980s, a key trope in anthropology’s ‘meta-method’: the professional ideology that determines what might count as appropriate ethnography (Marcus 2001a; 2011). Meta-method, for Marcus, has both ethical and cognitive dimensions. For Geertz’s generation, rapport meant attaining ‘insiderness’, allowing the anthropologist to orchestrate the traditional *mise-en-scène*, and carry out fieldwork within a decidedly local, more-or-less hermetically sealed, setting (Marcus 1997). For Marcus, a revolutionary shift in meta-method came with the recognition that anthropologists were complicit not only with those among whom they studied, but with powerful colonial and post-colonial agencies impacting upon their fieldsites. So, anthropologists were pressed to bring this world-historical context *into* their understanding of the fieldwork relation. This required that *ethical* alterations be made to the setting of the ethnographic stage – for instance, by incorporating an awareness of historical change into ethnographic writing. But only with *multi-sited* ethnography, that aimed to ‘represent something of the [world] system itself’ (Marcus 1989: 9), could the *cognitive* implications of this recognition be fully incorporated into

anthropology's meta-method, calling curtains once and for all on the traditional ethnographic *mise-en-scène*.

I will return to Marcus' work on multi-sited ethnography below. For now I wish to take up his interest in the regulative ideals of fieldwork (or 'meta-method') and explore how my own exposure to anthropology's professional lore, at three different British universities, coloured my efforts to design doctoral fieldwork, among the epistemic communities of London's mining market. Having been introduced to anthropology at Durham by outspoken proponents of 'indigenous knowledge in development' (Sillitoe 1998) and academics committed to working with grassroots sustainability projects in the vicinity of the university (Henfrey 2014), I moved to Kent, where my postgraduate studies were carried out under a similar sphere of influence (Ellen and Harris 2004). My exposure to anthropology had certainly led me to believe that if nothing else, 'we are definitely not on the side of whomever, in a given situation, is or fancies themselves to be the elite' (Graeber 2002: 1223). This was reflected in the conception of my first two research projects: on preparations for mine closure in Papua New Guinea (Gilbert 2012), and on informal seed-saving networks in south-east England (Gilbert 2013). If we wanted to identify, using Marcus' terms, the 'meta-methodological' correlate of the politics that Graeber imputes to anthropology, we could look to Ernest Gellner's dictum that anthropologists in the middle of the twentieth-century were 'roughly liberals in their own society and Tories on behalf of the society they were investigating: they "understood" the tribesman but condemned the District Officer or the Missionary' (Gellner 1973: 29).¹ Gellner implied that in an effort to enact their loyalty to their ethnographic subjects, and disloyalty to elites 'at home' (and in order to sidestep accusations of ethnocentrism), mid-twentieth-century anthropologists would *seek out* the appropriate context in which to render the rituals and utterances they came across in the field both intelligible and

meaningful. Here Marcus' notion that the fieldwork encounter is a *mise-en-scène* – a theatrical staging – becomes particularly potent. If Gellner was right, then ethnographers who, like Geertz, sought 'entry' into a fieldsite via the cultivation of rapport, would go on to conduct their fieldwork with the explicit intention of creating something that would elicit a pre-determined form of approval from their supervising anthropological audience. And if it remains trueⁱⁱ that 'anthropological writing in the end remains committed to the elucidation of contexts' (Strathern 1987: 261), we might need to think especially carefully about how the ethnographic *mise-en-scène* could be reworked to enable fieldwork among elites 'at home', and what this means in the context of an implicitly anti-elite disciplinary politics.

Such was the dilemma I found myself facing while I attempted to plan for my doctoral fieldwork at Sussex. My decision to try and carry out research in London's market for mining finance came as a response to an unfolding and all-encompassing financial crisis that I little understood – but about which anthropologists seemed to have something unique to say (Tett 2009). In addition, I had the sense during my earlier short fieldwork in Papua New Guinea that it would be worth exploring the 'inner world' of the mining capitalism that had dictated an endless deferral of mine closure, such that workers and local residents experienced constant anxiety about the near future. And this is where we might return to George Marcus' work on multi-sited ethnography. While his initial (Marcus 1995) sketch of a multi-sited ethnography that would involve 'following' objects, peoples and concepts through the world system seemed to imply something like Caroline Knowles' (2014) painstaking efforts to trace the production and movement of flip-flops through multiple cultural and economic contexts, Marcus was quick to distance himself from this 'obvious' version of multi-sited ethnography. The more interesting 'non-obvious' version of multi-sited ethnography involved situations 'where there is very little actual contact or exchange between two sites but where the

functioning of one of the sites (the more strategic one?) depends on a very specific imagining of what is going on elsewhere' (Marcus 1999: 7). This new kind of 'non-obvious' multi-sited ethnography would involve 'turning the traditional *mise-en-scène* inside out' (Marcus 1997: 101). Thus the ethical adjustment to staging fieldwork that began with the recognition that anthropologists were complicit with powerful agencies that reshaped the fieldsites in which they studied was provided with its cognitive complement. It seemed I had found the perfect batten from which to suspend my proposed fieldwork design: I would carry out fieldwork among the epistemic elites of London's mining market – investigating how the functioning of the market for mining finance depended upon *their* imagination of a distant locale. In response to the frontier market frenzy gripping the mining market as I began my fieldwork, and its unique extractive industry politics, Bangladesh (often designated a 'frontier' jurisdiction) became that second locale.ⁱⁱⁱ I would explore how participants in the mining market discussed, ranked and represented particular jurisdictions as sites of 'opportunity' or hazardous hotbeds of political risk, and how this affected the valuation of individual mining projects, as well as negotiations with sovereign states over mining licenses and royalties (Gilbert 2014; 2015a). But there was a glitch. How could I reconcile this non-obvious multi-sited fieldwork, to be undertaken among the epistemic elites of London's mining market, with the anthropology in which I had been trained, and that demanded I not be on the side of those who fancy themselves to be the elite – *especially* elites 'at home'?

For the Little Guy

Given that the regulative ideals of ethnographic fieldwork were devised through engagement with those *outside* elite institutions, the fact that anthropology is often reliant upon 'tolerance-engendering contextual interpretation' (Gellner 1973: 30) presents challenges for research

carried out among non-conventional ethnographic subjects (see Gusterson 2013). Indeed, after expressing the view, discussed above, that anthropologists are by convention ‘for the little guy’ David Graeber asserts that, in practice, ‘this comes down to a ritualized declaration of disloyalty to that very global elite of which we, as academics, clearly form one (admittedly somewhat marginal) fraction’ (Graeber 2002: 1223). During my own preparation for fieldwork, I was confronted by such ritualized declarations of disloyalty, on the list-serv for the Society for Economic Anthropology (SEA). In 2012, a thread was stimulated by the prospect that Gillian Tett – author of an anthropological book on the financial crisis (Tett 2009) and editor at the *Financial Times* – might give a keynote address at an Interest Group for Anthropology and Public Policy (IGAPP) conference. In the words of one list-serv member: ‘might a Financial Times keynote speech to IGAPP/SEA be kind a sorta akin to a Wall Street Journal keynote speech to the Occupy Wall Street gathering down the block? If not, it might be worth a clarification for some of us.’ And another contributor, despite proposing an openness to the likes of Tett (and the financial anthropologists of the American Anthropological Association) ultimately could not avoid delivering, straight from Graeber’s postulated hymnsheet, a ritualized declaration of disloyalty to the global elite: ‘After all, what side do we usually take when Walmart eyes a new area and sizes up the small individually or communally operated businesses therein? We are usually on the side of the underdog, as we should be.’^{iv} It would be foolish to deny that exposure to this kind of explicit – ritual even – disciplinary moralizing did not affect my ability to conceptualize my planned fieldwork among financial elites. So if I was not to fall in line with this ritualized disloyalty, what would the alternatives be?

George Marcus has throughout his work rejected the notion that multi-sited ethnography should entail ‘getting the ethnographic goods on elites.’ Anthropologists are, in his view not

‘temperamentally suited to be so clearly oppositional at the outset in relation to who they studied’ (Marcus 1998: 27 n. 9). Furthermore, mapping out the ‘good’ and ‘bad’ guys would dispense with the ‘one trick’ that anthropology has up its sleeve: a methodology based on ‘the deliberate attempt to generate more data than the investigator is aware of at the time of collection’ (Strathern 2004: 5). And so, Marcus entreats non-obvious multi-sited ethnographers to ‘pose the ambiguity and messiness of any moral position mapped onto social life across communities of difference’ (Marcus 1998: 28 n. 9). A willingness to embrace ambiguity has been a hallmark of recent writing on the regulative ideals of financial ethnography. Bill Maurer has argued that attacks on financial anthropologists who may, from certain perspectives, seem to have ‘gone native’ (e.g. Riles 2011) indicate nothing more than a failed critical imagination, according to which “‘impurity’ still means “‘danger’” (Maurer and Mainwaring 2012: 181). At this point in my own preparation for fieldwork, I was starting to feel convinced: if rapport was the watchword of anthropology in Geertz’s day, then *ambiguity* must be the foremost regulative ideal governing contemporary financial ethnography. Ambiguity I could live with. There may not be continuity with Gellner’s tolerance-engendering contextual explanation, but an embrace of ‘the messiness of the actual’ (Cowan 2010: 104) did seem to run as a continuous thread connecting the anthropology in which I had been schooled, and the type of elite ethnography I was planning to conduct. The ritualized declarations of disloyalty, I supposed, could be put on ice for eighteen months, but at least I didn’t have to ‘take sides’ during fieldwork. Ambiguity rules.

Except, I discovered, that is not quite what Marcus and his colleagues had in mind for an anthropology of contemporary elite formations. Building on his notion that non-obvious multi-sited ethnography should entail an awareness of how the functioning of one locale (in which the ethnographer is most ‘thickly’ embedded) depends on the imagining of or focus on

another, Marcus argued for a reworking of the ethnographic *mise-en-scène* that frames the anthropologist and her ethnographic subjects as *partners*, sharing a common concern with a third ‘elsewhere’ (Marcus 2007). Hence, multi-sited ethnography would by necessity come to be organised by a ‘problem cognitively shared’ (Marcus 2011: 23) with the ethnographer’s subjects recast as ‘epistemic partners’ (Holmes and Marcus 2008: 82). Furthermore, Marcus’ agenda for multi-sited ethnography is explicitly formulated as a way for carrying out research in communities of *experts*, whose lives are already structured by a commitment to research – such as the central bankers studied by Holmes (2009) and the World Trade Organisation (WTO) executives studied by Marcus (Deeb and Marcus 2011)^v. In many such expert communities, argue Holmes and Marcus (2005; 2008; Holmes et al. 2006), the ethnographic is already a familiar way of knowing. In other words, central bankers, WTO officials and countless other epistemic elites already employ ‘ethnographic’ – or open-ended, participatory modes of inquiry – in their professional lives. These found sensibilities they term ‘para-ethnography’ – which may take the form of unauthorized, critical, qualitative analyses in contexts where statistical or quantitative knowledge is privileged – and all that is needed is ‘an anthropological staging, a *mise-en-scène*, to give them articulation within complex discourse on the nature and operation of the contemporary’ (Holmes et al. 2006). And what form would this *mise-en-scène* take? That of the ‘para-site’, a deliberately staged opportunity for ethnographers and their epistemic partners to reflect upon a shared intellectual purpose, in the midst of fieldwork that is nonetheless carried out and conceived by an individual ethnographer (Deeb and Marcus 2011). Only the required mode of engagement with these epistemic partners is ‘deferral to subjects’ modes of knowing’ (Holmes and Marcus 2008: 82). As I came to my final understanding of Marcus’ programme for ethnographic reform, I once more experienced discomfort. Ambiguity I could live with, but this was one step further. Deferral to elite modes of knowing? What could be further from the regulative ideals of

ethnography gently mocked by Gellner, but which had shaped my introduction to anthropology at university? Then again, the notion of para-ethnography seemed heaven sent. To go into the mining market, where quantitative ways of knowing (based on geological, actuarial and econometric modelling) are undoubtedly privileged, with something *qualitative* to look for, was certainly reassuring. And it seemed a great deal more comfortable than following the research programme laid down by the actor-network theory-inspired ‘social studies of science,’ for whose protagonists it is simply misguided to try and ‘thicken’ descriptions of economic agents or impute to their actions ‘meaning’: the goal instead is to explain how calculative agencies, as described by economic theory, are formatted and organized into existence (MacKenzie and Millo 2003). To be told *not* to look for meaning in the organisation of financial markets, *not* to thicken the descriptions of financial agents produced by conventional economics, is enough to give any well-brought up anthropologist disciplinary indigestion. It would have to be para-ethnography. I would have to defer to the modes of knowing active in London’s mining market. And I would have to find a way to stage the most up-to-date genre of ethnographic *mise-en-scène* available, the para-site, in order to bring those modes of knowing – and their relationship to ‘frontier’ jurisdictions like Bangladesh – into view.

The Age of Conversation

My fieldwork experience was, all in all, not entirely pleasant. For all that Marcus (1997; 2001; 2007) perceives rapport to have been superseded by epistemic partnership as the organising sign of contemporary fieldwork, there is no doing away with finding an ‘in’ to your fieldsite. Anthropology has certainly been opening itself up to novel objects and contexts of inquiry for some time now. But where novel research involves conducting

fieldwork among elite epistemic communities (Crewe 2010; Deeb and Marcus 2011; Holmes 2009; Riles 2011), the differential distribution of social and cultural capital that marks these groups off as elite within wider social contexts becomes doubly relevant within the discipline itself: those able to activate connections with such communities are availed of greater fieldwork opportunities, a sociological fact barely acknowledged in undergraduate or postgraduate methodology seminars. To gain access to the overlapping epistemic communities that make up London's market for mining finance, I began by joining a series of professional associations, including the Association of Mining Analysts. As well as attending regular professional masterclasses, career development seminars and networking events, I enrolled on courses designed for analysts and non-executive directors new to the mining industry. And finally, I bought my way in, by attending events such as *Mines and Money London 2012*, at the cost of £1342.50 for a two day pass. At *Mines and Money* (discussed in greater detail in Gilbert 2014; 2015c), I attended an executive masterclass on initial public offerings (IPOs) for mining companies on the London Stock Exchange. One of the sessions was run by Catherine, a partner in one of the mining industry's leading communications and public relations (PR) firms^{vi}. Catherine introduced her presentation by announcing that 'we are within what we call the Age of Conversation...reputation is no longer controlled from within a citadel.' Social media had a big part to play in this, it seemed. 'Right now, everyone is a journalist, everyone is an activist...people you wouldn't expect can be overly cynical about business and overly interested in your personal wealth.' The role of the communications firm in the mining company's IPO was, she said, to deal with the 'profusion of stakeholders': identify the key audiences (those interested in driving the valuation up or down), built a 'solid equity story' and 'secure the right to valuation.' In Catherine's view, valuation is a narrative process: 'it is *very* different to price'. The role of the 'PR machine,' as she called it, was to counter efforts to drive the price down: 'you price, but you can't just go

away and get on with your day jobs.’ As long as there is conversation, there is a contest over price. In closing she argued that ‘shaping opinion’ and ‘getting truth’ were one and the same – especially when you’re ‘looking at it from a broader societal view, as we have entered into this Age of Conversation.’ I was fascinated. The notion that reputational risk arose from social media – the idea that ‘everyone is a journalist’ – was not, of course, novel, and I had and would encounter it many more times during my fieldwork – at London’s Reputation Institute, and at a masterclass for lawyers and PR professionals concerned with managing reputational risk. What was interesting was the notion of the Age of Conversation and Catherine’s attitude towards valuation as a narrative process that could be ‘secured’ by establishing ‘truth.’ Catherine’s idea that reputation was once managed from within a citadel, but now circulates more freely – but not without contest – in a networked world seemed to resonate with analytical perspectives I was using to make sense of other developments in the mining market, such as the decision to partner with a small company developing an algorithmic reputation analysis system that was made by the International Council on Mining and Metals. I had been thinking through Deleuze’s (1992) notion of a society of control, which he offers as the successor to Foucault’s (1991) disciplinary society. Whereas for Foucault, power operated via enclosure and the organizing power of the panopticon, in Deleuze’s control society, it is data management and network protocols that encode power. There seemed to be clear resonances with Catherine’s language of the citadel succeeded by the Age of Conversation. Clearly, hers was a vibrant epistemic community, committed to understanding emergent social forms. And so, I approached her and asked if we could meet. To my delight, she not only agreed, she was enthusiastic – she made me feel like I had found an epistemic partner, and promised to organise a series of meetings with her and her firm’s associates, in which we could engage over our apparently shared intellectual project:

understanding the changing significance of reputation for the production of value and pursuit of profit in the mining market. I had scheduled a para-site!

Trouble in Para-Sites

Like Deeb and Marcus (2011) in the run up to their para-sitic encounter with the director of the WTO, I prepared slavishly for this staged opportunity to situate Catherine's para-ethnographic commentaries on valuation and conversation within broader discourses on the nature and operation of the contemporary. I read her firm's briefings on the 'Age of Conversation' in which everything from Occupy to the Arab Spring was presented as the *result* of new media, since new media means there *must* be a conversation – not just somewhere, but *everywhere*. To illustrate the effervescence of conversation, her firm pointed to TED Talks, RSA Animate, the World Economic Forum (WEF), and *The Times* CEO Summit. Being aware of the WEF's exclusivity, and having been 'vetted' out of attendance at one natural resource-focused CEO Summit organised by a large newspaper, I began to feel wary of the notion of conversation deployed here. But I continued. There were twenty-two conversations, all relevant to every corporate, they said – and the one that most engaged me was the 'Environment & Resources' conversation, which directly touched upon the politics of 'frontier' jurisdictions. Introducing the terms of this conversation, Catherine's colleagues wrote: 'Our search for resources takes us to increasingly remote and inaccessible places – which pushes up the social, environmental and financial cost. There's a lively debate about how to minimize the impact, or whether we should extract at all.' Later in my fieldwork I would come to recognize this as part of a distinctive twist in the mining industry's approach to corporate social responsibility (CSR). Whereas programmes in the early 2000s were designed to 'uplift and empower', and carried out with a missionary zeal (Rajak 2008), CSR

was increasingly seen as a form of ‘social risk’ management. The plastic notion of social risk could variously refer to the risk posed *by* mining *to* local communities, as well as the risk *to* the profitability of mines caused *by* fallout from poor social relationships. For now, though, I was earnestly attempting to prepare for my para-sitic encounter, unsure of quite how to do so in a non-confrontational way.

After being buzzed through even before I rang the bell to Catherine’s unmarked building (I must have looked the part), I waited for her first in a waiting room – all clean lines and glass – and then upstairs in a room in which hung an original Picasso, and three surviving prints from a now incomplete series, produced by William Blake. ‘Our chairman loves art!’ I was told, when I commented upon them to Catherine and her two young associates. And I could not dispel the impression that in drawing attention to the artworks, I had violated what Miller (2005) terms ‘the humility of things’ – the taken-for-grantedness that is precisely what allows material culture to format our experience of space and social interaction – thus revealing the frailty of my efforts to perform as a member of this epistemic culture. But we sat down, and Catherine was friendly. One of her associates had recently left behind a career in investigative journalism, the other was trained in the UN system. We talked about the age of conversation, about the importance of maintaining and facilitating a conversation ‘between companies and society’. Feeling bold, I asked about the recent events in Marikana (see Breckenridge 2012; James 2013), as I knew her firm to be dealing with the mining company which had employed the striking – and now dead – workers. This, she said, was a *legacy* issue. All such events were ‘legacy issues, bad decisions that were made 20 to 30 years ago, when there was no internet.’ Remarkably, Catherine seemed to suggest that because of the disciplinary power of social media in the Age of Conversation, companies today *could not* be unethical. Thus, any ‘social risk’ arising at a mine site would *necessarily* be a legacy issue.

Rio Tinto's ongoing problems in Utah? 'Legacy issues is what they are. Projects don't go ahead nowadays if they do harm – if you're not gonna get buy-in from the community, you have to walk away.' Since Catherine had introduced the concept of 'harm', and I was inclined to push my 'epistemic partner' a bit on the notion of legacy issues, I asked how she would respond to the view held by some mining researchers in anthropology (e.g. Benson and Kirsch 2010) that mining is always and necessarily *predicated* on social and environmental harm? Her reply was telling: 'I don't know – when was that published? But I can only assume it was some time ago!' In other words, *before* the mining industry's rebirth which precipitated the paradoxical treatment of a 'corporate legacy' as something totally isolated from the contemporary corporate body. Later I would meet Catherine's young associate – the former UN employee – at a week-long ethical mining summit in a west London hotel. She had just returned from Marikana, she said. I asked what it had been like, speaking to the mineworkers as a representative of the firm. 'Oh, no', she replied, 'well we don't speak to everyone, only the *influencers*.' At that same summit, I met with mining executives who were working with a Wharton School professor to develop a methodology for identifying and traversing 'influencers' such that mining companies might be able to 'expand a coalition of local supporters without appearing to undermine a powerful tribal leader's authority' (Henisz 2014: 53). I discuss this in more detail elsewhere (Gilbert 2015b) as part of a wider shift from 'corporate social responsibility' to 'corporate diplomacy' in the mining market. For now it is perhaps sufficient to note that the Age of Conversation might be better termed the Age of Influence. And this has relevance not only for how I experienced and made sense of my parasitic encounter at Catherine's firm, but for how we might evaluate – and move beyond – calls for a 'deferent' ethnography of epistemic elites.

Conclusion: Trickster in the Age of Influence

George Marcus' contribution to rethinking ethnography has been invaluable. He has empowered doctoral students to refashion their projects such that the form taken by fieldwork keeps pace with changing objects and contexts of study – especially with regard to elite epistemic communities. By asking anthropologists to take account of how distant locales occupy the attention of those who populate our more strategically chosen fieldsites, Marcus' programme for ethnographic reform glides seamlessly from multi-sited into collaborative ethnography. As such, members of elite groups – and Marcus is explicit in his distaste for subaltern-focused 'resistance' studies (Marcus 2012) – become our epistemic partners, and we must 'defer' to their already-present ethnographic sensibilities, drawing out their para-ethnographic insights within carefully staged para-sitic encounters. I have discussed my discomfort with this research programme, but my ultimate acceptance that a (partial) embrace of cognitive *and* ethical ambiguity may be necessary for successful fieldwork among epistemic elites (as, no doubt, in less elite contexts). Certainly, through my own attempt to stage a para-site, I generated more data than I was aware of at the time, something I no doubt could not have done if I had 'taken sides' before beginning my fieldwork, and refused to dwell in the messy actuality of my (ultimately abortive) epistemic partnership with Catherine. But the most significant outcome of my para-sitic encounter was the recognition that the so-called Age of *Conversation* works as a strategically-driven Age of *Influence*. I would argue that likewise, the para-site, which Holmes and Marcus frame as a staged *conversation*, could be viewed as an attempt to stage the exercise of *influence*. Just as Catherine and her colleagues are only interested in speaking to 'the influencers,' Marcus is put off by the anthropological interest in those whom Graeber refers to as 'the little guys.' Marcus' research programme has been developed as part of an explicit attempt to escape resistance studies and the draw of the 'virtuously subversive' (Marcus 2001b: 8). The blueprints for the para-site were drawn up precisely in order to escape an 'overdetermining moral economy and

redemptive [ethnographic] function,’ and enable ethnographic engagement with ‘empowered subjects complexly entwined and complicit with major structures of power’ (Marcus 2001b: 2, 7) – in other words, ethnographic engagement with ‘influencers.’

If a para-site is a an encounter with epistemic elites seeking to exert influence over powerful institutions with the capacity to reorder the lives of those with less authority – and perhaps especially where ‘harm industries’ like mining are involved – Holmes and Marcus’ (2008: 84) claim that anthropologists ‘are not needed to add “critique,” moral injunction, or higher meaning’ becomes potentially rather alarming.^{vii} Aversion to revelatory, confrontational or ‘debunking’ critique has been made explicit in recent anthropological work on epistemic elites (Yarrow and Venkatesan 2012), though this ‘crisis of critique’ (Bessire and Bond 2013: 448) reaches a high-water mark in anthropology’s recent ‘ontological turn.’^{viii} If the para-site is premised on ethnographic deferral, then one unambiguous consequence is a parallel deferral of critique. And if we are to consider ‘whose anthropology’ Marcus and colleagues entreat us to carry out, we might do well to ask whether there is ‘a language ideology built into the project that mimics the language ideology that has sustained industrial order’ (Fortun 2014: 321). In other words, does the notion of the deferential para-site reflect the orientation Catherine and her colleagues have of the Age of Conversation, disguising a hierarchical relationship of influence as one of reciprocal exchange? I suspect that at times, it might.

I am not calling for a crude anthropology that sees impurity as danger, and sets out to ‘get the goods on elites,’^{ix} abandoning any ethnographic attunement to the cognitive and ethical messiness of the actual. And Webb Keane (2014: 444) is certainly right when he notes that ‘an anthropology that confines its efforts only to understanding those of whom the

anthropologist approves, and ignores what Susan Harding (1991) called “the repugnant other,” is hardly worthy of the name.’ But this subtle regulative ideal, and an interest in how elite lives are complexly entwined with powerful institutions, should not allow us to stumble into conceding that ‘anthropologists have little business grasping the bigger structural things’ in and of themselves (Kalb 2015: 11). If it was true four decades ago that ‘never before have so few, by their actions and inactions, had the power of life and death over so many members of the species’ (Nader 1972: 284), then it is only more so today (González and Stryker 2014). It is precisely those ‘few’ who have become epistemic partners and ethnographic counterparts in Marcus’ methodological programme. When the strategic deployment of research sensibilities by epistemic elites like Catherine (or the World Trade Organisation officials studied by Deeb and Marcus) can be viewed as the simple counterpart to our own open-ended ethnographic enquiry, then questions about accountability, elite expertise and the reproduction of structural inequalities are whisked out of site.^x In the search for alternatives to the overdetermining moral economy of anthropological interpretation at which Gellner poked fun over four decades ago, anthropology seems to risk losing sight of structural dynamics that drive power and inequality.

I have come out of my fieldwork convinced of the productive power of dwelling in discomfiting ethical and cognitive ambiguity, at least for a time. But anthropologists are not the only figures to embrace ambiguity. Consider the Yoruba trickster Esu-Elegbara, who teaches ‘the folly of insisting on determinate meaning’ (Gates 2014: 25) – or Coyote, the archetypal trickster of the American south-west, the ‘comic dissaranger who dissolves boundaries, unsettles certainties, shakes up fixed ideas and twists the tail of long faced moralists’ (Lincoln 1993: 142). Like the anthropologist, the trickster dwells in ambiguity, in messy actuality – but also makes it their business to shake up fixed ideas, certainties and

apparent transparency. Is there not a place for trickster anthropology in the ethnography of epistemic elites? When Marcus developed the concept of the para-site it was part of a deliberate effort to escape from ethnographic encounters with the trickster subject of resistance studies, the cunning and ‘wily transgressor within’ (Marcus 2001b: 7). We do not need to restrict our ethnographic encounters to engagements with these virtuously subversive subjects, but perhaps ethnographers studying epistemic elites could learn from them. Dwell in ambiguity during fieldwork, learn the folly of insisting on determinate meaning – but keep an eye on the big structural things, and, fieldwork complete, feel free to upset the efforts that are invariably made by your influential elite ‘counterparts’ to insist on determinate meanings of their own.

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ⁱ It is perhaps worth noting that although Gellner was taking Evans-Pritchard and his work among the Nuer as the focus of his discussion about what we might now call anthropology's

‘meta-method’, Evans-Pritchard was in fact on good terms with – even reliant upon – both District Officers and Missionaries during his first fieldwork among the Nuer, even if he did clash with certain members of the Sudan Government (Johnson 1982).

ⁱⁱ I am aware of ‘anti-context’ approaches in anthropology, often inspired by Latour (2005: 148) who argued that context is ‘simply a way of stopping the description when you are tired or too lazy to go on,’ but I do not think attention to context has been anything like excised from the regulative ideals of ethnographic fieldwork in anthropology.

ⁱⁱⁱ My fieldwork was also multi-sited in a more obvious sense, in that 6 months of my 18-month fieldwork was spent in Dhaka. But this does not alter the fact that it was conceived in line with Marcus’ notion of ‘non-obvious’ multi-sited ethnography rather than as a plan to ‘follow’ a particular mobile commodity, concept or social group.

^{iv} Thanks to Rahul Oka and Philippe Marius for granting permission to quote their postings from the SEA list-serv.

^v It is illuminating to read Deeb and Marcus (2011) alongside Haugerud’s (2005) account of how anthropologists should and could study the WTO. Haugerud firmly marries the cognitive and ethical requirements of ‘good’ anthropology with a ritualised declaration of disloyalty to elites.

^{vi} The names of my ‘epistemic partner’ has been changed

^{vii} See Kirsch (2014) for a superb and unashamedly critical account of the extent to which scientists and anthropologists working with and for mining companies become embroiled in reciprocal exchanges with their employers and end up distorting their results. Kirsch (2014: 232) is explicit that in this context, the anthropologist’s regulative ideal of ‘suspending disbelief’ during fieldwork must be eschewed.

^{viii} There are remarkable parallels between the so-called ‘ontological turn’ and Holmes and Marcus’ programme for elite research. Where Holmes and Marcus (2008: 82) reject the need

to add critique, and ask that we shift ethnography from description and analysis to ‘a deferral to subjects’ modes of knowing, a function to which ethnography has long aspired,’ Marshall Sahlins (2013: xiii) writes of the ontological critique that in good anthropology, other people’s worlds do not revolve around ours; ‘good anthropology revolves around theirs.’ Bruno Latour (1996, 2002, 2004), whose work has been influential in the ontological turn, has explicitly rejected critique, and the ‘adding’ of structures or explanatory terms like ‘Capitalism’ to ethnographic descriptions. Debates over the ontological turn are heated, and I want to be clear that while I find its foundational objection (that anthropologists treat the explanations of ‘Others’ as mere cultural interpretations of the one natural world to which their own cultural understandings most closely approximate) compelling and provocative, I do not feel anthropology is served by transposing a mode of argumentation from Amazonian ethnography to work not with ‘Others’ but with elite ‘partners’ in powerful institutions. See Fortun (2014) for a detailed and sensitive critique.

^{ix} It is perhaps worth noting that while Marcus’ (1998: 27 n. 9) disparaging reference to emergent approaches towards ‘getting the ethnographic goods on elites’ has been read as a swipe towards Laura Nader’s (1972) programme for ‘studying up,’ (González and Stryker 2014: 3), Nader’s research programme is better captured in relation to her interest in mapping ‘vertical slices’ through hierarchical societies. Nader was aware that ‘if anthropology were reinvented to study up, we would sooner or later need to study down as well’ (Nader 1972: 292). Her aim was not to ‘get the goods’ on, say, the organizational culture of an insurance firm, but to understand how it might shape access to credit, residence patterns and life chances in inescapable worlds of scarcity and difference, structured in part through the actuarial gaze.

^x In relation to Deeb and Marcus’ (2011) work on the WTO, it is worth considering work by the geographer Richard Peet (2007) and the anthropologist-cum-political-economist Robert

Wade (2003), on the extent to which the epistemic practices of elites who are ‘always already researching’ in powerful institutions can be read instead as ideological commitments which have concrete and significant distributional consequences.