

## Complicit Positioning: Anthropological Knowledge and Problems of ‘Studying Up’ for Ethnographer-Employees of Corporations

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### **Abstract**

Contemporary work by ‘corporate ethnographers’, as employees of businesses, offers a refreshing perspective on Anthropology’s ‘crisis of representation’ and its extensions—from neo-colonial concerns and reflexivity, to para-ethnographic and recursive approaches—that are increasingly characterized by complicit relations between ethnographers and their informants/‘collaborators’. This article focusses on the history and politics of ethnographers’ positionality in field research and the analytic products of, and audiences for, their work. It contrasts the often confounded labor of ‘anthropologists *of* business’ with that of ‘corporate ethnographers’, who work *for* businesses, while highlighting that, for both, the ‘studying up’ (Nader 1974 [1969]) methodology required for research at business sites disrupts assumptions surrounding the politics of traditional ethnographic fieldwork. Tracing shifts in core interests across general Anthropology, it is argued that close attention to new sitings and circumstances of fieldwork—including studying up in businesses—could productively drive reconsiderations of methodology, ethics and, therefore, epistemology in Anthropology.

In this context, corporate ethnographers, who are often formally

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trained in Anthropology, are specifically encouraged to analytically engage with the problematics of their perhaps-awkward complicities with their employers. It is suggested that, alongside the work of anthropologists of business, corporate ethnographers—should they choose to do so—are well-positioned to assist in exposing the black box of the culture(s) of secrecy through which the work of corporations intimately penetrates modern life.

### **Keywords**

Corporate ethnography, anthropology of business, positionality, 'studying up', 'culture(s) of secrecy', crisis of representation, complicity, para-ethnography

### **Introduction**

Anthropology of Business takes business seriously, as it does the ethical conundrums of engaging it ethnographically. Along with providing detailed, empirically-rich analyses of an arena that drives much of our contemporary modern condition, Anthropology of Business could also make foundational contributions to general Anthropology because it is in the nature—and I use that word specifically—of their subjects that ethnographic work on businesses pushes to the edges of Anthropology's ethical envelopes. In raising ethics, I flag contested terrain, and ethnography of business generates particular tensions, and suspicions. Along with other formal organizations, businesses or, more specifically, *corporations* are private, legally protected fictions with very real boundaries, forcefully guarded. From gaining access to such spaces, to the unpacking of their private/internal activities, to their external representations as brands, goods and services, and as persons—think 'Steve Jobs'—businesses are particular, and particularly demanding, anthropological sites. The ambiguity generated from ethnographic positioning in such edgy sites should be an asset to our thinking about anthropological fieldwork, and a source of analytic power. Along with making significant contributions to general Anthropology, it should constitute a means of speaking to power and providing commentary on and critique of the diverse socio-economic conditions constituting, for lack of a better term, the 'neo-liberal' regimes that structure most of humanity's contemporary circumstances.

Regrettably, however, while generating interesting soundings regarding 'another village heard from', with some outstanding exceptions the quality of ethnographic research on businesses is uneven. As a result, given the obviously central position of businesses in the larger picture of how the world looks today, this work has been less influential than one might have hoped to general Anthropology, as it has been in providing

pithy commentary for public discussion. Astride ethical considerations, there are real concerns regarding methodology, including the duration, depth and quality of ethnographic research on businesses, which of course impacts its analytic breadth and interpretive precision. This would seem to be the case especially among ethnographers employed by businesses who, as it happens, tend to dominate the field. As I will discuss at length, what can be claimed with regard to anthropological knowledge creation is impacted by the form and intentionality of any ethnographic work. Non-disclosure agreements (NDAs) that are commonly found at the center of the employment contracts of corporate ethnographers are, of course, relevant here. But the questions I raise are more far-reaching, and insidious: the extent to which some ethnographers may, inadvertently perhaps, perpetuate the culture(s) of secrecy pervasive in business. With the public psychologically branded by corporations' advertisements, products, services and, sometimes, their famous leaders, I argue that it is in illuminating the erstwhile 'black box' of corporations' inner workings that anthropological work on business would most productively focus. And while in this article I critique the work of ethnographers employed by businesses, I also raise the perhaps counterintuitive prospect that they may be able to offer particularly cogent insights into those very corporations with which they are complicitly engaged.

In this article, then, I address the conduct, representation and ethics engaged in ethnographic fieldwork on businesses, all of which revolve to a significant degree around questions of positionality between ethnographers and their subjects in business contexts. As a proxy for these issues I flag 'studying up' (Nader 1974 [1969]) as a form of anthropological knowledge production, emphasizing the problems entailed in engaging ethnographically with informants of equal or greater status than the ethnographer. The core thrust of my argument is the following. If not necessarily generating an inversion of authority, studying up would seem to confound the supposed politics at the core of classic ethnographic fieldwork in Anthropology:<sup>1</sup> the *structural* dominance of ethnographers (as representatives or embodiments of their home community) over informants (as representatives or embodiments of their local context). Traditional fieldwork relations unfolded, and continue to unfold, within overarching political frameworks—that were sometimes colonial, and are now occasionally described as neo-colonial—which have

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<sup>1</sup> The model is, of course, Malinowski's fieldwork among Trobriand Islanders, beginning in 1914 (Malinowski 1922). Doing extensive, 'immersion' ethnographic fieldwork—usually for a minimum of one year: an annual seasonal cycle—is understood as a rite of passage in Anthropology, and is ordinarily the basis for writing an 'ethnography', which, if successful, allows for a PhD qualification. There is obviously considerable variation in the fieldwork experiences of anthropologists who have come after, but whether or not they have done extensive, immersion fieldwork like Malinowski, all anthropologists are aware of and must contend with his model as an ideal form.

allowed anthropologists the extraordinary privilege of going to sites, often very far away from home, staying there for a considerable length of time and, often, returning to those, or related, sites in the years to come. If construed in the negative sense implied by neo-colonialism, the interpersonal positionality implicated in the structural dominance of ethnographers over informants allowed anthropologists to impose themselves on local communities.

I will expand on this over-simplistic rendering below, but it serves to make the key point that in studying up, the fundamental positionality of the ethnographer vis-à-vis persons in the host community under study is unorthodox according to the traditional anthropological norm. If so, these new forms of relations pose important problems regarding the particularities of ethnographic research produced through fieldwork in these unorthodox contexts that, in turn, generate significant epistemological questions. As the number of ethnographers in such sites, e.g., studying up, proliferate, engaging with these new circumstances of fieldwork is important, in terms of methodology, ethics and, therefore, epistemology in Anthropology.

### **Studying up, the structural politics of positionality, and the day-to-day work of ethnography**

I define studying up as conducting ethnographic research among persons within, or who are closely affiliated with, organizations.<sup>2</sup> As I understand them here organizations are, among other possible criteria, formally constituted legal entities. They stand within, and are therefore reproductive of larger institutional contexts that have, or have the potential for, powerful effects on the larger society in which that organization, and its institutional context, is found. To unpack this organizational/institutional nexus, an example of what I mean by an organization is a hospital, which stands within the institution of the health care system in such-and-such national context. Another example of studying up, familiar from my own work, is ethnographic research in foreign subsidiary factories, affiliated local communities, as well as at headquarters of multinational corporations. The work of these corporations generates products and forms of symbolic capital with reach across globally dispersed sites in the vast institutional context of contemporary capitalist production and consumption. I might also

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<sup>2</sup> While 'studying up' has become the moniker associated with Nader's (1974 [1969]) proposal that anthropologists should conduct fieldwork within organizations effectively constituting America's 'military-industrial complex', as Nader herself points out (1974 [1969]: 292), methodologically this would obviously include studying 'down' and 'sideways' in those same organizations and among communities closely associated with them.

include in the definition of studying up or, in any case, its use as methodological technique, conducting ethnographic research focusing on the condition of persons who are more directly affected by their relations with particular institutions (and their attendant organizations) than would, perhaps, be the case of more mainstream citizens. (Of course, it must be acknowledged that the State is profoundly implicated in the lives of all modern persons.) For instance, anthropological analysis of the situation of Afro-American male urban youth in American cities would, necessarily, require both knowledge of the police at a local organizational level as well as the larger legal/institutional system in which the work of the police, which so intimately affects those under primary consideration, takes place.

As a foundation to further examination of positionality when studying up, I emphasize the phrase 'structural dominance' in relating the uncomfortable perception of neo-colonialism in anthropological practices that, some claim, informs classic and, indeed, much recent work in Anthropology (Rosaldo 1989a). This is in order to make the point that we would do well to take a disaggregated approach to this problem. On the one hand, we should obviously take seriously the overarching geopolitics that inform the historical circumstances that allowed for anthropological fieldwork in colonial times, and may continue to structure anthropological fieldwork in notionally neo-colonial and other sites. On the other hand, in whatever historical period, we should appreciate the practical circumstances and the emotional engagements accompanying the anthropologist's day-to-day practice of conducting fieldwork on the ground, as part of a community, in the company of local people, over a long period of time. The traditional, Western-educated elite anthropologist studying, say, a tribe in the 'Global South' may inevitably be a cog in the wheel of a larger set of unequal geopolitical relations that may, tangibly, allow him certain freedoms that his informants may not enjoy: including mobilizing resources that allow him to arrive at, remain in and leave the field on his own terms. That said, the particular circumstances allowing field ethnographers to gain access to field sites in the first place (and remain in them) has never been as simple as this domination model suggests, nor is life on the ground during fieldwork unproblematic, including the personal upheaval of anthropologists uprooting themselves from the relative physical and emotional comforts of their 'home' lives.

Along with any overarching politics, it is valuable to be reminded, then, of what jobbing anthropologists got up to and what they get up to in the field while conducting ethnographic research. My sense is that across the discipline's history, as today, the vast majority of anthropologist fieldworkers have empathized genuinely with the circumstances of members of their host communities. If possibly seen here through overly rose colored glasses, successful ethnographers are inevitably caught up in

important relationships with persons who we professionally call informants but who are usually friends who we treat as erstwhile equals, at the very least when we are sharing 'coeval time' (Fabian 1983). In the field, as at home, we devote ourselves to personal interactions that, in the nature of human relations, are complicated and, so, rewarding. Thus it is my own experience, which I suspect is shared by most anthropologists, that fieldwork is undergirded by the long term construction of a practical and emotionally-enriched lifeworld between anthropologist and hosts that literally makes space for shared community. We might understand this as a process that allows the strange to become familiar: from the perspectives of both the anthropologist and members of the host community, and, so, a two way process. As such, a collaborative movement across time, feeling and apperception is central to sound anthropological analysis. (I will revisit this point later in the article.) Meanwhile, in the field, ethnographers very often find themselves humbled by the knowledge and practices taking place in the communities they are allowed to join. (In such circumstances, who is the dominant party? How and when do dominance and/or questions of inequality matter?) If knowledge of such practices is what we go to the field to understand, I would suggest that, along with respect for and interpersonal commitment to those among whom we study, knowing little and being willing to learn a lot about the local situation is what constitutes competent ethnographic practice.

At risk, 30 years on, of revisiting a hackneyed debate, I raise these matters as a mild retort to what seems to have been an overemphasis, especially in (North American) Cultural Anthropology, upon guilt, and subsequent angst, regarding the circumstances of traditional anthropological fieldwork. As Rosaldo, as understood by Marcus, would have it, especially in the morality tale driving his noted piece, 'Imperialist nostalgia' (Rosaldo 1989b), field anthropology has reached an 'impasse': it is a 'tragic occupation', so 'paralyzed' (Marcus 1997: 95) and tainted that it should, effectively, stop, at least in its current form. I suggest, *contra* Rosaldo, that rather than tarring every anthropologist with the brush of neo-colonial operative, wherever ethnographic fieldwork takes place, including in situations of studying up, we recognize that structural differences between ethnographers and members of informant communities are present. And, despite the likely fact of structural inequalities, we nonetheless work toward an ideal—that, of course, is not in all cases achieved—of communicative equality with our interlocutors in the field.

Anthropologists are conscious, or soon become so, of the political circumstances through which their work is made possible, and they recognize how the politics accompanying interpersonal relations in the field, including their capacity to gain local knowledge, affects their fieldwork experiences. In turn, they should be explicit about how those

particulars of the field are subsequently represented, or translated, as they must be, for other audiences. And should it be that ethnographers are invited to produce knowledge representing their field experience in particular forms for particular audiences, they should be conscious and explicit about how and why that field experience is being re-translated, or translated differently, for that different audience. If all corporate ethnographers were so engaged, I believe their work would be more powerfully rendered, more widely read and their very important subject matter more deeply appreciated in general Anthropology. Without explicitly representing such engagements in their work, however, the question is raised to what extent their ethnographic renderings can lay claim to producing in-depth anthropological knowledge.

### **Unorthodox sitings: the production of the current wide-open ethnographic moment**

While it has largely shared the same intellectual trajectory, the fact is that in the history of Anthropology, the study of businesses—and other formally organized contexts, such as public bureaucracies—has been unusual, and sporadic. Thus, although there have been significant studies of particular organizations, sometimes with anthropologists working in multidisciplinary teams, the Anthropology of Organizations (including, therefore, Anthropology of Business) lacks intellectual momentum as a subject of study.<sup>3</sup>

Asking why studying businesses has been unorthodox takes us right back to the beginnings of Anthropology. Briefly, classic sites in Anthropology were asked to bear the *de facto* weight of extreme, if usually implicit, analytic comparisons with anthropologists' own, read, Western societies. I say 'implicit' despite some late 19<sup>th</sup> century work that explicitly distributed the world's societies in a highly elaborated hierarchy, with Victorian Britain at its apex (Stocking 1987). Historically, work in such extreme or, from a Western perspective, extremely different sites, among hunter-gatherers, nomads, slash and burn agriculturalists, etc., has been variously, if sometimes-unfortunately and often-tenuously described as studying among 'primitive peoples' or 'savages' living in 'tribal' or 'simple societies' or, in more recent articulations, among 'marginal communities', say, urban slum communities, in 'less-developed'

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<sup>3</sup> For competent surveys, see Baba's (1988) discussion of both collaborations and antagonisms between 'anthropologists of work' and large American corporations across the 20<sup>th</sup> century; and Wright's edited volume, where her opening chapter (Wright 1994: 1-31) nicely covers the history of 'anthropology of organizations', some of it overlapping with Baba's treatment. Sedgwick (2007: 9-20) provides a survey of the very substantial and longitudinally rich anthropological literature on Japanese businesses, from craft producers to major global corporations.

nations. In any case, if under British social anthropology ‘social *organization*’ was a construct accounting for various forms persons might take in organizing themselves, i.e., kinship groups, tribes, markets, etc., those studying formal *organizations*, of which businesses are a quintessential ‘modern’ example, have traditionally been a marginal community in Anthropology.

In recent years there has been an explosion of ethnographic fieldwork in locations considerably different from traditional anthropological sites. This has to do with general changes in our *external* environment, including the proliferation of communications technology and dependence on the internet for all sorts of relations, simplifying, for instance, the maintenance of communities that are literally globally-dispersed. Technological developments have also generated prodigious opportunities for increasing numbers of people, often including our informants, to experience personal displacements, e.g., mass travel. These phenomena collude in complicating—not displacing—our common sense notions of time, space and place in social relations built up, as they fundamentally remain, from face-to-face contact and spatial relations unfolding physically in the present.

The rise of new and more creative sitings for ethnographic fieldwork also has to do with *internal* changes in Anthropology, including severe self-critique regarding the neo-colonial pretenses of traditional anthropological practices, as outlined above. These matters were combined in Anthropology’s ‘crisis of representation’: a fully justified assault on traditional forms of, and concern over the audiences for, anthropological texts, as the basis for overall critique of the politics of (representation in) Anthropology. Hence, its foundations shaken, with no consensus as to what should happen next, theoretical debate in Anthropology has splintered, as have forms of fieldwork: there is little agreement as to what now constitutes a proper site for anthropological research. More recently, meanwhile, albeit a far smaller discipline in terms of number of staff employed, Anthropology has shared with other Social Sciences a severe downturn in resources. That said, in my view, despite multiple, on-going intellectual crises, Anthropology is increasingly popular with students and, judging by the high quality of talent that it attracts and its influence across ‘the conversation’ between the Humanities and Social Sciences, e.g., in the ‘human sciences’, Anthropology is an extremely successful discipline.

In the discipline’s current wide-open moment, among the proliferation of new sites, increasing numbers of anthropologists study in or around formal organizations. Perhaps this is to be expected: after all, despite postmodern pretensions,<sup>4</sup> formal organizations—including

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<sup>4</sup> In ‘late capitalism’, often understood as coinciding with the turn toward postmodernity, ‘progress’ may seem thoroughly stalled across a decade or more



businesses, government agencies, voluntary agencies, etc.—are central to the reproduction of the complex modern societies that dominate the planet. Furthermore, it is perfectly obvious that the reach of modernity has touched the supposedly isolated, smaller, ‘simple’ communities that were traditionally sought out for classic anthropological fieldwork. Happily, many anthropologists continue to work in such communities, but their porous and often-contested boundaries—the comings and goings of its members, the interactions of parts of the community with the outside, etc.—are now essential to contemporary anthropological analyses.<sup>5</sup>

While current circumstances, including networks of connections imagined across the internet and individuals and families calling multiple parts of the world ‘home’, suggest a plethora of new contexts informing anthropological work, we would want to recall that the world has always been linked up, if at times more dynamically than others. In collusion with technological development, networks of trade and, of course, migration, have reconfigured the globe (Wallerstein 1979), sometimes with tragic results. One thinks here of the devastation of many African tribes through the marketing of slaves that constituted the middle leg of the 16<sup>th</sup>-19<sup>th</sup> century Atlantic ‘triangular trade’: an enormously complex example of the effects of Western colonialism, and with far reaching consequences still prevalent today. Given this history, as a product of Western academia that developed in the mid-late 19<sup>th</sup> century, but based in earlier forms of accumulation—exploration, missionary work, colonial administration, trade, etc.—Anthropology could not have arisen outside of a colonizing framework. It is natural that acknowledgement of the historical linkages between colonialism and Anthropology should be articulated, then. The question is Anthropology’s intersections with that historical record.

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of stagnation for Europeans, for the Japanese—over 20 years of recession here—and, to a somewhat lesser extent, in North America. But even putting rapid economic growth in China and India across the last two decades or more to the side, the progressive project of modernity continues to capture imaginings of ‘the future’, at a minimum at least as far as the institutional configurations that drive that very modernity in which our lives are deeply embedded are concerned. As a thought exercise we might ask, where are ‘no-growth’ economic relations—that might, incidentally, help preserve the planet—being seriously discussed? Furthermore, the current period of Great Recession is hardly historically unprecedented. Rather, such dips are anticipated—normalized in capitalism—and, so, indicative of the project of modernity. It is not for nothing that the naming itself, *Great Recession*, refers directly to the Great Depression from which, in conventional understanding, we ‘recovered’.

<sup>5</sup> By no means are interactions between communities new to Anthropology. One thinks immediately of Edmund Leach’s pathbreaking work in Burma here (Leach 1977 [1954]). Nonetheless, the dominant trope in Anthropology until the mid-1990s has been the analytic unpacking of single communities that were considered bounded, at least as a methodological, ‘scientific’ and/or interpretive convenience.

A different, more optimistic alternative to the colonial/neo-colonial imperatives of Anthropology that Rosaldo raises, then, might consider the early development of American Anthropology as a reaction to that very colonialism: for instance, the early 20<sup>th</sup> century American ‘salvage anthropology/ethnography’ undertaken by Boas and his colleagues (Stocking 1974), among Native American tribes devastated by white America’s western expansion. Of course, by this time, across North America’s vast, verdant stretch—from the Atlantic Coast to the Mississippi—most Native American tribes had already been decimated through contact with whites that began in the 17<sup>th</sup> century. (Far earlier, of course, in Central and South America.) The practical exercise in salvage of (at least, information about) remaining Native American communities was based not in a romantic aesthetic, or nostalgia (cf. Rosaldo 1989b), but an effort to provide substance to Boas’ forceful theories about cultural relativism. While subject to possible negative extensions, the cultural relativist view is that no culture is any better, or any worse, than any other: each has arisen in human history as an equally-laudable human accomplishment within its own particular environmental context. These views, which deeply informed North American Cultural Anthropology until the 1980s, to say nothing of the American civil rights movement, perhaps arose in Boas both as a reaction to the appalling destruction of Native American populations and his experience as a Jewish immigrant from Germany, educated in the German intellectual tradition. In any case, driven by this uplifting, egalitarian ethos, it is unsurprising that the idea that anthropologists ‘should do no harm’ to their ethnographic interlocutors should constitute the foundation of anthropological ethics itself, guiding both ethnographic work in the field and the subsequent representation of communities studied.<sup>6</sup>

### **Positioned engagements**

As suggested above Anthropology’s more recent rethink regarding its colonial roots has in some quarters been morally debilitating: it has attempted to hoist responsibility onto the shoulders of Anthropology as/of the Western/dominant system in which it was first institutionally constructed. I would suggest that an outcome of the discipline’s recent concern over its colonial roots, and fears regarding its possibly on-going neo-colonial disposition, has been an extension of the missive that anthropologists ‘should do no harm’ toward the view that

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<sup>6</sup> At a minimum this has been expressed through maintaining the anonymity of specific persons and communities studied ethnographically. While a comparative discussion worth pursuing, as it may serve to respectively elucidate both contexts, note that the anonymity typical of public representations of anthropologists’ field research should not be confounded with the non-disclosure agreements (NDAs) typical of for-hire ethnographers’ work for businesses.

anthropologists' work 'should do some good'.<sup>7</sup> What might constitute 'doing good' or being 'politically engaged' of course varies. For some, the act of conducting competent research or educating students well—on the basis of a rich and varied literature in Anthropology to which one's own specialist ethnographic knowledge is appended—is sufficient. For others, meaningful good is only achieved through direct political action.<sup>8</sup> Meanwhile, it is worth recalling that, traditionally, as in the present, anthropologists have variously supported their host communities outside of the frame of those communities as subjects of the anthropologist's enquiries. (Not considered of academic or 'scientific' merit, such activities between anthropologists and hosts remained largely private.) Without intending to suggest a sea change in the left leaning and sometimes radical politics of anthropologists, however, in its contemporary guise 'doing some good' seems often to be articulated as an explicit desire that the anthropologist's work should engage politically-relevant subject matter. By this I mean political relevance from the personal viewpoint of the anthropologist,<sup>9</sup> as opposed to the discovery, among other things, of material relevant politically to the community that the erstwhile naïve anthropologist is studying.<sup>10</sup> Of further interest is the fact that the many sites now studied, as communities, are often institutionalized within the ethnographer's own society, or familiar national context: in shorthand, they are conducting 'anthropology-at-home'.

While possibly worth celebrating, collectively these new

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<sup>7</sup> This is further suggested in the cover blurb of the recent edited volume, *Writing culture and the life of anthropology* (Starn 2015). In his cover blurb for the book, Arturo Escobar says, "To the question posed twenty-five years ago of "Why write, and how," some of the essays now pointedly add "Why act, and how do we act?"

<sup>8</sup> In my experience, most anthropologists in fact do both, but balance them differently, i.e., as public or private activities, including in their scholarship.

<sup>9</sup> For instance, currently there is broadly-shared concern over environmental decline with increased consciousness of the linkages between local contexts—that would typically be the sites of anthropological work—and global impacts, and *vice versa*. Pertinent questions that arise might include: How can anthropological knowledge assist in understanding how we got here, what institutions sustain such damage and, by implication, what forms of engagement would be relevant in how we are going to get out of our environmental mess? Corporations, national and local governments, multilateral agencies, universities and NGOs linked up through technology, finance and the consumption of goods and depletion of resources collectively engage this 'environmental problem'. As its creators, understanding the human dimensions of any one part of this puzzle and, better yet, generating the capacity to describe the links across its complex (organizational) contexts is interesting, and important. How can anthropological knowledge, as means, be deployed here, and to what productive ends?

<sup>10</sup> The classic example of this sort of serendipitous discovery is Clifford and Hildred Geertz' sudden, and inadvertent, injection into the politics of the Balinese village where they were conducting ethnographic fieldwork during a raid by Indonesian authorities on the village's illegal cockfight (Geertz 1972).

commitments among anthropologists also confound the intellectual premises upon which anthropological fieldwork was established and the forms through which it was predominantly practiced up until, say, the 1990s. *First*, anthropology at home may undermine the analytic advantages of studying others—commonly describe as ‘the Other’—usually expressed, as discussed above, as an implicit form of comparative method operationalized through the personal experience of the ethnographer. Typically this has been articulated as learning about another society ‘from the bottom up’. While this characterization perhaps suggests the ethnographer *as if* innocent child, anthropologists are adults upon whom interpersonal experience, usually through co-habitation within a society quite different from their own, makes its mark: emotionally, intellectually and, often, physically. In practice, despite preparation—in the form of language study, reading ‘everything you can get your hands on’, etc.—this rite of passage usually begins with the ethnographer entering the field, at least for the first time, with extreme naïveté. If that is the case, among those studying ‘at’ or ‘in’ their own society, what is the impact of prior knowledge—the *de facto* lack of naïveté—before conducting ethnographic work at/on/with it? I understand, of course, that modern societies may be defined by the enormously complex range of social roles and circumstances entailed by its members, as well as a vast range of ‘sub-cultures’ about all of which no individual could possibly be truly knowledgeable. That condition, however, does not mean that individuals within a society, in this case an ethnographer, does not have a viewpoint, however unsophisticated, on other parts of their own society. Meanwhile, obviously an anthropologist’s decision to study a particular part of the complex puzzle of their own modern society would strongly suggest their interest in and an at least implicit viewpoint upon it. Good or bad, how does that ‘pre-positioned’ aspect affect their access to and their methodological engagements with their sites, their analyses and their interpretive work?

*Second*, ethnographic work in such sites may invert or, in any case, significantly complicates the inequality common to the erstwhile neo-colonial relationship of anthropologist of the ‘cosmopolitan center’ toward their ‘peripheral subjects’. While I have argued for the ideal of *de facto* equality in the day-to-day, coeval relations of informants and ethnographers across the history of Anthropology, in new, modern sites, and especially in studying up in formal organizations, ethnographers are very often structurally subordinate to their informants. This matters. If overly simplistically rendered, a practical articulation of these changes in field circumstances is the following. It seems that in traditional sites anthropologists could remain *in situ* until they themselves judged it was time to go. In studying powerful organizations, the head of the organization under study, if not many other members of that organization, can show the anthropologist the door at any time. Problems raised in such new circumstances of fieldwork, now increasingly common

in Anthropology, have been nicely spelled out by Marcus (1997: 100):

The fieldworker often deals with subjects who share his own broadly middle-class identity and fears, in which case unspoken power issues in the relationship become far more ambiguous than they would have been in past anthropological research; alternatively, he may deal with persons in much stronger power and class positions than his own, in which case both the terms and limits of the ethnographic engagement are managed principally by them. Here, where the ethnographer occupies a marked subordinate relationship to informants, the issues of use and being used, of ingratiation, and of trading information about others elsewhere become matters of normal ethical concern, where they were largely unconsidered in previous discussions.

How would such conditions affect the day-to-day conduct of ethnographic fieldwork in businesses? What knowledge do we lose, or gain, by working ethnographically under such conditions?

One may celebrate, as, indeed, I do, shifts in Anthropology as reflections of new realities in an evolving world, including changes in the political relations between anthropologists and informant communities. One may also want to problematize the details of received wisdom in Anthropology as I have expressed it, including Anthropology's recent crises and to what extent they have exercised the discipline. (This may also look somewhat different in Santa Cruz, CA from how it does in Cambridge, UK.) Nonetheless, these historical forms remain extremely well travelled ideas in Anthropology, driving crucial methodological premises, with foundational theoretical implications. As I have suggested above, at their heart lie profound questions regarding the positionality of ethnographers in the production of their work.

And, yet, as fieldwork sites have increasingly diversified, becoming unorthodox by previous standards—including corporations and other formal organizations—engagement with fundamental questions of positionality and their effects on the quality of ethnographic research have quietened. In the uncharted territory of the expanding repertoire of sites, are ethnographers just getting on with new studies with the intention of working out questions regarding positionality later? Have the throes of self-examination following the crises of the 1980s generated disciplinary exhaustion in Anthropology, discouraging any hope for agreement? Or, perhaps, is avoiding these questions just as well because much contemporary ethnographic work rubs uncomfortably against some of the ethical concerns raised specifically by those important crises in Anthropology?

**Cultures of secrecy: exposing power/knowledge in businesses**

As my title suggests, I am concerned that questions of positionality remain insufficiently explored in the ethnographic study of businesses. For one thing, a large proportion of the ethnographic work on businesses—including that available for public/academic consumption—is based on research conducted on behalf of those firms, e.g., with ethnographers working for the corporation. Indeed, as I will further examine below, in this ‘vendor’ context ‘ethnography’ has become part of a saleable methodological toolkit. This has obvious implications for interpretative and other analytic work produced for businesses/organizations, and for interpretative and other analytic work that may, or may not, stem from those ethnographic experiences for an anthropological audience. This is not about hand wringing regarding the authenticity of analysis or, necessarily, an argument, *per se*, that employment by an ethnographer in a business should rule out that work’s credibility to the academic Anthropology community. It concerns the provision of clarity in revealing the context through which anthropological knowledge is derived. This seems particularly important to analysis of businesses, which, I suggest, are contexts that are subtly loaded, especially for the ‘native anthropologist’ working ‘at home’. Allow me to expand on this point.

It is not unusual that, like other normal persons, informants are unconscious of their own apperceptions. That said, the powerful interlocutors who ethnographers work with in studying up are often able to control how their labor is perceived, and in ways that may significantly affect the pith of ethnographers’ analysis of that labor. It is part and parcel of both the ethos and the explicit knowledge of those with organizational power that, if provoked, they can draw on the resources of the State, both physical and otherwise: say, through their capacity to deploy legal resources to derive favorable outcomes. Notably, however, the day-to-day work of persons in positions of authority intimately depends on the ‘invisibility’ of their means of deploying power (Herzfeld 2015). Meanwhile, of course, it is precisely in uncovering what is below the surface that anthropological methods thrive. ‘[T]he enduring, lived consequences of events taking place in the centers of power...’ can be recognized ‘...in the way that [Anthropology] extracts hidden and highly significant social realities in tiny local details... [and in] keep[ing] both the detail and the larger picture in focus (Herzfeld 2015: 18)’. To extend Herzfeld’s discussion of ‘invisibility’, the non-disclosure agreements (NDAs) under which much corporate ethnography is conducted is but a literal articulation of the culture of secrecy typical of the mobilization of power in formal organizations. But the ‘corporate veil’, if a legal fiction, may have quite real analytic effects. As I will expand upon in detail below, it may well be that ethnographers employed by businesses are positioned, inadvertently or not, to enact the invisibility and secrecy—the mythmaking—characteristic of managing the tension between the public and private work of corporations. To what extent are ethnographers of

business willing to elucidate for anthropological and public audiences those very black boxes which they themselves may be complicit in constructing?

### **Profligate naming: boundary trouble and the uneven carving of ethnographic claims**

While there are some important anthropological studies, much of the ethnographic work on business is driven by ‘corporate ethnographers’. Many corporate ethnographers have formal academic training in Anthropology but they are clear about their non-academic, professional position *in the workplace*. ‘[P]articipant[s] in corporate settings in such roles as researcher, consultant, manager, and designer, the anthropologist operates as a mutual corporate actor with other members of the corporation (Cefkin 2012: 5)’. In their favor, then, corporate ethnographers do not pretend to the quasi-objectification of their work that is typical of academic Anthropology.<sup>11</sup> Rather, corporate ethnographers’ goal is to produce ethnographically-sensitive ‘deliverables’ for the profitability of the corporations they work for. That said, while clarity is provided, or implicitly understood, regarding the fact of their employment by, or their erstwhile membership in, corporations, when it is made publicly-accessible the implications of the *de facto* lack of detachment structured into their ethnographic engagements tend to be distanced. Most simply, the effects of the empirical context of the work go largely unacknowledged. It seems that in producing ‘deliverables’ the middle-level guts of these ethnographic projects are carved out. As the

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<sup>11</sup>As discussed above, the interpersonal linkages of anthropologists and informants, that is both typical of the experience of fieldwork and emblematic of Anthropology’s fieldwork model, generates tension in anthropologists’ later representations—the analysis and interpretation *elsewhere*—of that field experience. This makes the production of anthropological texts highly challenging. It is *quasi*-objective, I claim, because while, on the one hand, anthropologists attempt to make an accurate portrayal of the world ‘out there’, on the other hand, our research processes, if radically-empirical, are hardly scientifically-objective. Rather, the field is so subjectively experienced by the anthropologist—the subjective-objective gap so artificial—that what we write are real fictions. As such, given the already fully-loaded condition of reflexivity at the heart of anthropological practices in/as foundational to interpretive work—that is to say, at a minimum, deep reflection on one’s position as an anthropologist in the field—it seems that moves toward even more reflexive representations are likely to involve more complex objectification work than that typically undertaken by anthropologists. (What I have in mind here are attempts to make the explanatory power of anthropological texts more evocative, as, say, art: for example, as poetry.) As a result, given the representational challenges anthropologists already experience it is no surprise that the air has rather gone out of extending ‘reflexivity’ beyond it informing a personal ethic surrounding our translations of fieldwork: it is a nice idea to push things further aesthetically, but so difficult to do convincingly.

ethnographic descriptions often feel hollow, analysis runs thin: it lacks the *intent* toward holistic richness driving anthropological ethnographers' highly contextualized work. The impression of a hollowed out ethnographic/empirical center, meanwhile, is encouraged by corporate ethnographers' publicly-available presentations and writing sometimes being accompanied by a (compensatory?) veneer of high theoretical abstraction.<sup>12</sup>

Corporate ethnography takes many forms—as above, 'researcher, consultant, manager, and designer'—but ethnographic work for business has been especially prolific in consumer research. According to Malefyt (2009), at the intersection of consumption research and 'ethnography', a plethora of interventions by consultants using various forms of qualitative methods has been established under the 'brand' of ethnography. Guided no doubt by the adage 'time is money', it appears that in this sphere there are a number of consulting firms where 'technomethodologies', that claim to improve on ethnographic methods, are deployed as a matter of course (Malefyt 2009: 204-06). As I understand it, here erstwhile 'informants' provide information through still cameras and/or video that interface with computer-driven operations or programmes that are perhaps custom-built ('tweaked') for the needs of a particular client. In the analysis of consumption patterns, it would seem that such 'ethnographic' interventions may have become an alternative to the focus group: instead of gathering isolated sets of people together to discuss their likes and dislikes, e.g., regarding a product, an advertisement, etc., here *individual* preferences are literally recorded as they take place. Of course, this data is raw—arguably, therefore, it is pure—compared with the garnering of group opinion. That is, rather than analyzing the work of a group of consumers in considering what appeals to them, it is the analysts or, perhaps initially, their computers, to whom or which this information is fed, and who/which, in combination, do the work of interpretation, i.e., at the other end of a technological tunnel from the action as it takes place. It seems that consumers are not even invited to talk about their choices while they make them, which furthers the point that there is no visceral ethnographical knowledge of the space in which consumers' actions take place. In short, there is nothing *ethnographic* about this. Meanwhile we know nothing of the capacities of those, back in the 'ethnographic' consultancy, working through this already highly abstracted material. Here, technology-driven interfaces have replaced 'inefficient' face-to-face interactions.

In noting these cases Malefyt does not directly problematize this 'shift in the work of ethnography from anthropologist-fieldworker to

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<sup>12</sup> Such inclinations are typical of several of the entries in Cefkin's (2009) edited volume. See, for instance, the chapter by Nafus and Anderson (2009).



technology-enhanced ethnography vendor' (Malefyt 2009: 206), nor the attractions their work may hold for clients.<sup>13</sup> But I believe he is implicitly policing what is at stake. Such research operations water down the meaning of ethnographic methods among the larger, if overlapping, community of corporate ethnographers, many of whom, as mentioned above, are formally trained in Anthropology. While operating professionally as employees or consultants for corporations, and positioned in their use of ethnographic techniques in ways that, I argue, are variously problematic, most corporate ethnographers respect the ideas behind proper anthropological inquiry, and are keen to self-identify as anthropologists. Quick-and-dirty, computer driven 'ethnography' represents a commercial threat to this community. It should be noted, however, that, following Malefyt's lead, I have purposefully exposed methods that have so stripped the 'brand' of ethnography that they risk denuding it altogether. It is unfortunate that Malefyt, as a corporate ethnographer himself, does not make the point that these technologically-enhanced examples of ethnography-emptied-of-content undermine the idea of ethnographic inquiry as we have, up until now, understood it: that is, participating in the observation, in shared time and space, of other persons' actual behavior and, in turn, putting co-experienced events at the center of analysis.

Taking our discussion of ethnography well beyond the sphere of corporate ethnographers, as an academic discipline Anthropology may rightly claim to have invented (field) ethnography or, in any case, the participant-observation method strongly associated with Malinowski's work among Trobriand Islanders. That said as a matter of record, or anthropological congratulation, it is perfectly clear that 'ethnography' has gone its own way, carving out methodological space across the Social Sciences and, to some extent, in related, practical fields. Indeed, the state of ethnography is such that in its proliferation across the academy, and in claims to its use elsewhere, there is by now no general consensus as to what ethnography is. I have forwarded a most cursory definition above but, generally, ethnography's contours have been so intellectually depleted that it rests, precariously, on negative definitions, i.e., of what ethnography is not. In practice, ethnography has become an increasingly large receptacle for all sorts of qualitative methods: again, 'ethnography'

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<sup>13</sup> Malefyt suggests—rightly, so far as I know—that the (well-known) advertising firm where he works, or worked, is not a 'technology-enhanced ethnography vendor'. That said, along with more traditional, face-to-face ethnographic, as well as various technologically-enhanced, methods, Malefyt is frank about the use in this firm of 'deprivation' techniques in gathering information on behalf of clients (Malefyt 2009: 204). These are activities that academic anthropologists would recognize as manipulations of informants and, so, entirely unethical. But, then, the intention behind these techniques is not at all academic: they are directed toward 'client deliverables'.

is *not* quantitative methods.<sup>14</sup> Meanwhile, I note with some irony that Malinowski, in arguably treading a scientific line of enquiry, did plenty of quantitative work or, at least, counting, as do most anthropologist fieldworkers today.

Corporate ethnographers, meanwhile, also describe themselves as ‘practicing anthropologists’. (If broadly accepted, this wording confounds the common sense meaning of *practice*, i.e., anyone who practices anthropology, whether in the academy or elsewhere.) So, although there

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<sup>14</sup> I suspect that the rise of ethnography is a consequence of frustration with the limits of the types of questions that quantitative methods could ask and, so, the quality of answers that they produce. That is, situations encountered among highly complex subjects, i.e., the behavior of human beings, are not easily parcelled into simple variables that can be meaningfully correlated. (Regular tooth brushing and reduction of tooth decay are strongly correlated, but this is not surprising. The interesting problem is why some people brush regularly, while others do not.) Ethnography has furthermore become attractive among ‘soft’ social scientists as they have increasingly recognized that the forced packaging of their quantitatively-oriented *qualitative* methods—structured interviews, postal surveys, and so on—yielded insufficient ‘data’ to account for circumstances that interested or concerned them. Such researchers might have an intuitive understanding that attracted them to their research problem in the first place and about which they were observant on their passages through the production of ‘data’.

Those opinions stated as a matter of conjecture in explaining the rise of ethnography as method, the fact is that the parcelling of variables into manageable packages is generally what is sought in the Social Sciences: quantitative methods remain overwhelmingly dominant. This is especially so in Economics, in Policy Studies—where interested parties, such as governments, increasingly demand ‘fact-based evidence’—and in North American Sociology and Political Science. Perhaps of greater relevance to our interests in the ethnography of business, is the field of Business/Management Studies, where its highly complex subject matter seems particularly well-situated to attract analysis via qualitative methods. Unfortunately, this field also remains America-centric in terms of scope, with quantitative methods overwhelmingly dominant.

The missive ‘follow the money’ provides the necessary evidence of this overarching phenomenon. In terms of number of staff employed and number of academic departments, while increasingly popular with students, Anthropology is a miniscule discipline compared with any of the quantitative-methods-heavy Social Science disciplines listed above. In the Social Sciences, qualitative methods, and the turn toward ethnography in particular, provides but an addendum to core quantitative methods. And, in the supposedly scientific thinking driving most of these fields, *qualitative* methods are an easy target, with the vagueness associated with what it is that constitutes ethnography as method making it particularly vulnerable to critique. Rightly so. Ethnographers do not do themselves any favors here. At one extreme an ‘ethnographic observation’ might include a social scientist noting, on the way to conducting a structured interview, that in the cafeteria workers wear different clothes from managers. At the other extreme is the (traditional) anthropological ethnographic experience: participant-observation for at least one year, ordinarily in a foreign, unfamiliar location, far away from home: a *de facto* rite of passage both personally and professionally for the anthropologist and, no doubt, for some members of the host community.

seems to be some formal disagreement and, certainly, confusion, we can define 'practicing anthropologists' as a community using ethnographic techniques who are employed at or working on behalf of, e.g., as consultants to, private enterprises, as well as other formal organizations.<sup>15</sup> (Albeit less visible, the 'practicing anthropologist' community also includes anthropologists working in 'development': in public and private sector agencies, consulting firms and NGOs. The community of anthropology-oriented 'development practitioners', however, is more closely associated with the terms 'applied anthropologist' or, more specifically, 'development anthropologist'.<sup>16, 17</sup>)

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<sup>15</sup> The community of 'practicing anthropologists' who work in business settings is most parsimoniously represented in two formal groupings. Founded in 1983, the National Association of Practicing Anthropologists (NAPA) is a formal section of the American Anthropological Association (AAA) and has the remit: 'promoting the practice of anthropology, both within the discipline and among private, public, and nonprofit organizations' (<http://practicinganthropology.org/about>). I think, however, that NAPA is understood as representing the interests of non-academic anthropological work. In furthering that point the Society for the Anthropology of Work (SAW) is also a section of the AAA, but with an explicitly academic focus. (With most other sections of the AAA focussing on subject or regional subfields within academic Anthropology, it is extremely valuable to have sections representing particular interest groups, in this case, the professional concerns of non-academic practicing anthropologists.) Meanwhile, the Ethnographic Praxis in Industry Conference (EPIC), as the name suggests, is a group specifically oriented to ethnography for industry. It started its annual conferences in 2005 and has an active web-based forum for those pursuing careers in this area, especially 'corporate ethnographers'.

<sup>16</sup> Again as a matter of common sense one would assume that those with anthropological training (or other training that includes in-depth ethnographic techniques) who are employed by private consulting firms servicing development agencies, or who are employees at (State-run) development agencies or NGOs, would also describe themselves as 'practicing anthropologists'. When articulated in contrast with academic Anthropology, some of them might agree to that nomenclature, but their more common, specialist self-description is 'development anthropologist'. (Meanwhile, at the UK's Department for International Development (DfID), anthropologists and other qualitative methods-inclined social scientists, i.e., non-economists, are called 'social development officers'.) In terms of formal representation, this group is most closely associated with the Society for Applied Anthropology (SfAA), which is *not* part of the AAA and runs its own annual conferences. (It publishes the journal 'Human Organization' and, just to confuse my argument regarding development anthropology, has 'a career-oriented publication' called 'Practicing Anthropology'.) Founded in 1941, SfAA's remit is extremely large, describing itself as 'a worldwide organization for the applied social sciences'. It '...promote[s] the investigation of the principles of human behavior and the application of these principles to contemporary issues and problems. The Society is unique among professional associations in membership and purpose, representing the interests of professionals in a wide range of settings - academia, business, law, health and medicine, government, etc. The unifying factor is a commitment to making an impact on the quality of life in the world.' (<http://www.sfaa.net>)

I am purposeful in supplying a range of overlapping nomenclature for those laying claim to ethnographic methods in the service of their (non-academic) employers. Meanwhile, in addition to those sited above, I have also found the following representations of ethnographic and/or anthropological work *for business*: ‘corporate anthropology’ (Cefkin 2012: 2) and ‘corporate anthropologist’ (Malefyt 2009: 202); ‘consumer ethnography’ and ‘branded ethnographic practices in consumer research’ (Malefyt 2009: 201); and ‘professional cultural anthropologist’ and ‘professional ethnography’ (Powell 2015). No doubt there are other names, or soon will be. Meanwhile, among trained anthropologists who, due to their work on business contexts I would describe as studying up, the contrast between those who ‘study businesses as sites *for Anthropology*’ (as a discipline) versus those who ‘work *for business*’ is most parsimoniously defined by the respective labels ‘anthropologist of business’ versus ‘business anthropologist’. Meanwhile, although authors of articles for the *Journal of Business Anthropology* come from both communities, it generally publishes articles by academic anthropologists on business-related topics, i.e., written by anthropologists of business.

My point is to highlight the boundary trouble in this profusion of naming: between what I call ‘anthropology of business’ and those working for businesses, in ‘business anthropology’, ‘corporate anthropology’, ‘professional anthropology’, ‘corporate ethnography’, ‘professional ethnography’, etc. I condense what I have suggested above in noting that there are significant differences between these respective communities’ approaches where it comes to, a) intent regarding the gathering of knowledge, b) the position of the ethnographer in gaining knowledge and, c) the dissemination and core audience(s) for that knowledge. And I would suggest that if there is confusion among specialists about the meanings of different names for the application of ethnographic methods in business contexts—when the fundamental intent of work among those who do ethnography for business, and those who study businesses ethnographically as sites (for academia) is so different—this important distinction is even more confusing for general Anthropology. The result, I believe, is that in general Anthropology it is thought that most studies of businesses by anthropologists are not

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<sup>17</sup> There is a notable literature in Anthropology animated by the tensions between anthropologists who critique the politics and practices of ‘development’—as, for example, a neo-colonial exercise—and anthropologists who use anthropological techniques and/or claim anthropological sensitivities as analytical assets in providing ‘assistance’ in the developing world. While I do not consider that literature here, the discourse between ‘anthropologists of development’ and ‘development anthropologists’ should be pursued comparatively in unpacking the similarly-structured tensions between anthropologists who study businesses academically, as research sites, and those using ethnographic methods as employees of businesses.

academic projects, but the exercises of consultants or in-house employee/ethnographers. This is suggested by academic work on sites that are perfectly obviously businesses, but use different nomenclature from Anthropology of Business. For instance, there is robust academic work in Anthropology of Finance, and a whole plethora of research at sites affiliated with businesses under the rubric of Science and Technological Studies (STS). Projects in Economic Anthropology, as an overarching subfield of the discipline, meanwhile, are often based in work that could easily be called Anthropology of Business.

The ambivalence, at best, toward studies of business by the discipline of Anthropology is driven both by the confused naming I have highlighted above and by the dynamism of the ‘practice’ communities working for businesses which are, perfectly-justifiably, formally represented within the American Anthropology Association (AAA), to a somewhat lesser extent in the (British Commonwealth-based) Association of Social Anthropologists (ASA), and elsewhere, i.e., the Society for Applied Anthropology (SfAA), etc. Meanwhile, in the larger move toward ‘relevance’ in the context, it must be said, of significant declines in academic employment, the management of the AAA constantly advocates a larger public voice for anthropological knowledge as well as the role of anthropologists in non-academic work. Some university Anthropology departments of course explicitly emphasize applied work. (Development Anthropology is particularly strong in the UK, for instance, with its intellectual training set in relation to the Anthropology of Development, as outlined in Note 17.) Implicitly, among those working in the discipline, and perhaps occasioned with some regret, the decline in opportunities for careers in academic Anthropology encourages awareness and concern regarding the practical, non-academic use of anthropology. These are all perfectly rational, and reasonable, reactions to a changing environment. However the scepticism that accompanies perceptions of the role of anthropology with regard to business is different from perceptions of Development Anthropology versus Anthropology of Development. In my view this comes down both to a normalized, if thoroughly myopic, hostility toward ‘business’ among left-leaning anthropologists—that tends to make corporations unpalatable research sites—and to problems of positionality in ethnographic work on businesses. The former may take care of itself as Anthropology becomes increasingly engaged with analysis of corporations and other formal organizational forms, i.e., as core drivers of modern capitalism and, so, of foundational relevance to anthropologists’ political concerns, whatever they may be. The latter, however, requires a serious and self-conscious engagement with method as epistemology in the ethnography of business.

**‘Complicit’ attractions: corporate ethnography and the potential diversions of contemporary theories of method in Anthropology**

It seems to me that in much of what I call corporate ethnography, as a catch-all for ‘ethnography/anthropology *for* business’, there is a fundamental lack of engagement with the ethnographer’s positionality in relation to the conditions under which the work is being conducted. This has crucial implications for its production of anthropological knowledge. While laying claim to responsibility for ‘the emergence of a nascent *canon* of corporate ethnography’ (italics mine) (Cefkin 2009: 2), I am concerned that these ethnographers do not acknowledge, a) their *de facto* lack of structural control over the direction of their work and, b) their lack of control over the use of that work, i.e. as the commissioned property of the firm. These structural conditions of work impact basic problems of positionality and often lead corporate ethnographers to c) thin out critical detachment in their interpretation of the ethnographic work they conduct.<sup>18</sup> As I have also suggested, such problems in *anthropological* relations with informants/ communities are sometimes disguised through an insinuation of anthropological credibility slipped in via engagement with sophisticated anthropological theory.

The parsimonious, if simplistic, approach among corporate ethnographers in addressing these concerns is to state frankly the limits of their fieldwork: the audience should consider the work for what it is, within its own framework of production. If insufficient for some purposes,

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<sup>18</sup> In his concluding chapter of Cefkin’s impressive edited volume, *Ethnography and the corporate encounter* (Cefkin 2009), Michael M.J. Fischer discusses the work of corporate ethnographers comprising the rest of the volume. He asks,

Could corporate anthropologists ever be allowed to produce the equivalent of such studies of the biotech industry as those by Barry Werth, Paul Rabinow, Kaushik Sunder Rajan, or Melinda Cooper; of the IT world such as those of Gabriella Coleman, Chris Kelty, or Hal Abelson, Ken Ledeen, and Harry Lewis; or (from the legal world) Lawrence Lessig [*sic*]; or of the financial world as those of Donald MacKenzie? Or is that request less in conflict just with NDAs [non-disclosure agreements], and rather more in conflict with the new ethnographic products being developed...? (Fischer 2009: 236-37)

Two points here. *First*, the serious and highly respected anthropological work of the scholars listed by Fischer is ‘Anthropology of Business’. Although studying business contexts as academics, i.e., not as employees of those businesses, they are also ‘corporate ethnographers’, at least in the common sense meaning of the term. They just do not wish their work to be described under either identifying rubric. As I have explained above, ‘corporate ethnography’ is now for-profit terrain, a problem that, I maintain, bleeds into the comparatively negative perceptions of ‘Anthropology of Business’ in the academy. *Second*, elsewhere in his chapter Fischer (2009: 232-33) misconstrues non-disclosure agreements (NDAs) in corporations as having equivalent effects on corporate ethnographers’ work as North American universities’ Institutional Review Boards (IRBs)—Ethics Review Boards (ERBs) in the UK—do on academics’ research plans and results. However, he is correct in highlighting, as I do, the centrality of corporate control over the output of corporate ethnographers’ research and what might be construed as the resulting limited impact it enjoys in academic Anthropology at present.

‘what it is’ is, nonetheless, a plentiful resource. The personal and intellectual effects (and affects) of working under conditions in which, for example, proprietary control over information allows supposed-ethnographic work to reproduce the ‘black box’ of business contexts, are a potentially productive route to unpacking the tensions inherent in the lifeworld of business.<sup>19</sup> Auto-ethnography comes with its own package of analytic difficulties—that I will address only tangentially here—but there is no reason why any number of corporate ethnographers, perhaps during a sabbatical from the workplace, or between jobs, could not engage in what, in practice, would be reflections on their work in businesses as ‘anthropology at home’.

More substantively I would suggest a collective, two-pronged analytical tack that acknowledges and examines businesses as familiar organizational forms that are, on the one hand, closed, bounded or ‘black boxed’: operations that serve to make them ‘foreign’ or estranged from day-to-day public life, except for their appropriately-processed members and properly-vetted visitors. These would be combined with analyses that, on the other hand, unpack both the penetration of many businesses’ products and brands into day-to-day public experience and consciousness, while the work of other businesses—perhaps most businesses—goes nearly entirely unnoticed in the public realm but, nonetheless, is likely to affect us all.

My earlier examples of the unfortunate mobilization of the ‘brand’ of ethnography through ‘technomediation’ are not representative of the broader community of corporate ethnographers. Many are trained anthropologists, fully sensitive to the emotional and intellectual rigors of their face-to-face work as ethnographers. Tracking, as suggested above, between corporate settings and within an active self-representing community with a particular (ethnographic) skill set—e.g., their formal Ethnographic Praxis in Industry Conference (EPIC) group—they are sophisticated operators, positioned to intelligently unpack the conditions of their ethnographic work and the corporate elaborations that surround it.

That stated as a matter of encouragement to this community, given the intellectual open-endedness, if not splintering, across the last 30 years of Anthropology, perhaps it is perfectly reasonable that such work is not undertaken by corporate ethnographers. As discussed above, anxiety among anthropologists regarding the colonial roots and neo-colonial implications of research among non-Western/‘less developed’ peoples provoked, along with an often guilty political consciousness, a crisis of representation regarding the content, style, production and

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<sup>19</sup> Brun-Cotton (2009) sensitively outlines the ethical problems and, thus, the interpersonal tensions entailed in her consulting work for corporations.

reception of anthropological texts themselves. In such an already-highly-contested representational environment, it is easy enough to leave the stone of the black box of business unturned, leave businesses' culture(s) of secrecy, secret. At least, that has been the prevalent approach in corporate ethnography to date.

In addition, my criticism of corporate ethnographers for insufficiently unpacking, and so insufficiently exposing to anthropological scrutiny the corporate contexts in which they work, is not assisted by highly theorized discussion in Anthropology that explicitly challenges traditional understandings of anthropologists' ethnographic work with informants. In an important intervention that specifically deals with contemporary modern contexts and, so, may be especially relevant to those working in or on businesses, 'complicity' is preferred over what has been previously understood as *rapport* with informants (Marcus: 1997). With his work already positioned to dislodge previous assumptions regarding anthropologists' work, i.e., the influential *Writing Culture* volume (Clifford and Marcus 1986) and the *de facto* movement that followed it, especially in North American Cultural Anthropology, Marcus' deployment of 'complicity' is surely a rhetorical strategy, a provocation: the common sense understanding of complicity clearly generates suspicion and, therefore, a discomforting response, perhaps especially among anthropologists who, whatever their faults, attempt to maintain a high ethical threshold.

'Complicity' is analyzed by Marcus both through an offering for the reader's consideration of its Oxford English Dictionary definitions and in reference to Geertz' and Rosaldo's canonical work. He proposes new ethnographic configurations through which anthropologists reposition their relations with informants toward one of collaboration in knowledge *production*. (Thus, on the surface of it, Marcus' idea would seem to map easily onto the consultative work of corporate ethnographers.) The goal, therefore, is not the erstwhile one-way 'sharing' of the informant's knowledge (of his or her own society) with the (visiting) ethnographer, however collaborative that 'coeval' (Fabian 1983) experience might be for anthropologist and informant alike (Rabinow 1977). Rather, for Marcus, informants are understood as persons operating in lifeworlds similarly complex to that of highly trained, cosmopolitan anthropologists or, perhaps, depending on how broadly it is construed, within the same overarching lifeworld, e.g., long term, elite, Western educational backgrounds, etc. Marcus claims that the anthropologist and his informant, as two *de facto* experts, 'complicitly' align themselves as an insider pair together confronting the outside world, articulated as a 'third' (Marcus 1997). The external 'third' world, amorphous and shifting, generates anxiety and is possibly threatening: it seems to be experienced as a form of postmodern anomie. In any case, the core point is that knowledge of the 'third' is an outcome, or a fabrication, of the relations of



the pair. As such, the complicit co-production of the outside, 'third' world, or parts of it, seems suggestively therapeutic—the external 'third' could certainly be a fantasy—if, unlike therapy's typical professional form (as a quasi-confessional space), it generates knowledge through the relations of two parties coming to this process as presumed equals. (I believe the point is that the privacy, and so the intimacy, of the pair's joint work in confronting or producing the 'third' encourages the complicity of the relationship.<sup>20</sup>)

Perhaps Marcus' exploration of complicity is an early move seeking to socialize, through externalization, the erstwhile internal world of the *reflexive*, highly self-conscious, anthropologist into, e.g., recursive relations with informants. That is, a revival as well as an extension of anthropologists' traditional, deep-seated social relations with informants. In this, my own conceptualization of the precursors to this extension, the 'traditional' anthropologist, through fieldwork in faraway climes, i.e., extreme commitment to personal displacement, was preoccupied with the 'Other', and perhaps at risk of losing himself in his social relations with his interlocutors' lifeworld. (Thus the warning to never 'go native'.) In some quarters, i.e., Cultural Anthropology, this self-other boundary was, perhaps inadvertently, made more explicit in the work of the 'reflexive' anthropologist. Here the anthropologist would indulge, rather than repress, the personal, private emotional labor of field experience, indeed, in its more narcissistic forms, becoming self-obsessed, while also—more helpfully—remaining aware of the political positionality of his work across its entire trajectory: from access to, work in and later representations of 'the field'.

Now the extension. Unlike the traditional anthropologist, who is apparently obsessed by the 'Other', the 'complicit' anthropologist need not lose himself, nor adopt the self-engrossed attributes of the reflexive anthropologist, but should *resist* the 'Other', conjoining himself with the reflexivity of his similarly-positioned informant vis-à-vis an othered 'third'. In a later articulation along these lines, informant-collaborators complicit with anthropologists are themselves granted the status of quasi- or 'para-' ethnographers (Marcus 2000). (Or, more simply, particular artifacts become 'para-ethnographic' and, so, worthy of our attentions—for example, 'anecdotal' comments, in this case in the minutes of formal meetings of bank officials—as they suggest the 'intuitive' 'structures of feeling' typical of anthropologists' sensitivities (Holmes and Marcus 2006).) Positioned in broadly overlapping worlds to anthropologists, para-ethnographers are serious, sensitive and thinking

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<sup>20</sup> If so, albeit moving away from our common understanding of expertise, perhaps the less this joint work is able to identify reality in the outside world, or have it confirmed elsewhere, the more the complicit relationship would be reinforced?

people who are, similarly, coping with the 'outside' over the long term. It seems that in their relations with anthropologists they are effectively ethnographers in the making, just waiting to break out of their shells. (Or, perhaps, given the proliferation of ethnographic methods generally, the anthropologist is no longer required at all.)

In any case, so far, the several ethnographic projects so operationalized indeed do unfold with collaborators positioned similarly to that of anthropologists vis-à-vis their own societies: nearly all Westerners, they are bankers, scions of wealthy American families, European politicians, scientists, artists, architects and civil servants. Marcus acknowledges these persons as 'experts with shared, discovered, and negotiated critical sensibilities' (Marcus 2000) similar to anthropologists. Although he makes little of it—perhaps because he is aware that many anthropologists would be embarrassed to admit to such a status—moving through the world as highly educated, cosmopolitan/metropolitan types (albeit, no doubt, far better paid than anthropologists), para-ethnographers enjoy positions of significant authority in their own society: they are, like us, elites. Pushing this thread even further, Riles articulates her work with financial regulators as '...suggest[ing] ways of thinking about problems of concern to them and ways of engaging their various publics, produced recursively and relationally, that at once strike at the heart of what matters to them and yet would not have been thinkable outside the ethnographic conversation.... [A]n ethnographic sensitivity can provide venues for market governance and a professional life worth living, to making proposals for how financial markets might be governed (Riles 2011: 6-7)'. Such configurations of relations between anthropologists and complicit collaborators, as erstwhile 'informants', sound remarkably like the sort of private interactions that, as ordinary, if elite, modern persons, we seek out, and pay for, from any number of knowledgeable persons from whom we require professional advice: physicians, tax accountants, therapists, business advisors, dentists, etc. 'Ethnographer' as specialist consultant with a broad remit.

### **The risks of the para-ethnographic to corporate ethnography**

While I have discussed some of Marcus' interesting provocations across the last couple of decades, by no means is my elaboration of 'new ethnography' exhaustive: that is for another context. After all, here we are unpacking the problems of positionality among corporate ethnographers. The point, rather, is that the problematics of corporate ethnography, and the positionality of corporate ethnographers, run parallel with techniques some anthropologists are suggesting regarding ethnographic complicity with elite informants in positions of authority. Indeed, in my reading through the work of corporate ethnographers, as well as listening to their

(academic) talks and, indeed, getting to know them, I have been surprised that they seem not to have acknowledged, evoked or, possibly, embraced the move to collaborative/'complicit' ethnography advocated in some corners of Anthropology.

Alas, while fully admitting that I may have missed something earlier, I have discovered that recently this has begun to be discussed in the community of corporate ethnographers. Without wanting to make too much of a project that is clearly in progress, I raise the interesting case of a PhD-trained anthropologist now '...work[ing] for a strategy and design firm... [as] a professional cultural anthropologist experienced in retail innovation and branding efforts for major food retailer companies (Powell 2015).' Presumably working *pro bono*, Powell commendably 'assembled a team of experienced retail designers with whom [he] had professional relationships to work alongside community development experts already at work on a small market makeover project' of a corner store in a poor, and mainly Hispanic, South Los Angeles neighbourhood. He, thus, 'helped facilitate an exchange of ideas between professional/corporate food retail discourse—which largely lacked an awareness of how to affect the health of low-income communities—and food justice discourse'. In addition, in due course, Powell serendipitously encountered an anthropologist who was working on the food justice movement in Los Angeles and decided to 'form [with her] an ethnographic collaborative team to study the project'.

The project is evidently 'doing good' and, of course, is intellectually interesting in its own right. What I want to highlight are the multiple roles that Powell plays and, indeed, celebrates in this one context, including his control over the production of that very nexus. That is, while mobilizing his expertise initially as a food retail discourse specialist, he states, 'I also had the ability to create an ethnographic field site, as well as the ability to ethnographically study it. [Thus] in my capacity as a key informant (to my own project), I am arguably a para-ethnographer who is co-creating ethnographic analysis.' Over time Powell continued his specialist retail consultations, and the redesigned shop is apparently a success. Meanwhile, as the ethnographic work on the site he has himself created is still unfolding, we do not yet know about the entirety of the project, which, after all, is perhaps meant to be disaggregated. This process further suggests, however, questions regarding the analytic efficacy claimed for 'the multiple roles inhabit[ed] in an ethnography and redesign project' unfolding in the same space.

I return to concerns regarding the possible loss of anthropological knowledge in the larger move toward complicit, para-ethnographic collaborations that I raised about Marcus' initiatives. Powell links the following statement about the rise of para-ethnography to the work of his corporate ethnographer colleagues who '...understand well and engage in [work], either as consultants to client groups or positioned inside of

larger organizations and corporations' and who, as discussed above, organize themselves publicly under the aegis of EPIC, the website on which Powell's article appears. He states: '[f]rom technology and finance to consumer-focused industries and the non-profit sector, a general trend toward diversification and collaboration is prevalent...[with]... these processes... increasingly including experts with "para-ethnographic" sensibilities—that is, people who think, act or analyze culture, community, identity and social behaviors in ways similar to anthropologists, but who may or may not necessarily have any formal academic training in anthropology.' I would ask, however, in considering the prospects for para-ethnography, if authority among anthropologists is derived from their sensitive deployment of substantive ethnography, do we want to give up claims on this expertise quite so easily?

### **Conclusion: reverse infusing the lifeworld of corporations**

Finally, rather than a sideshow, the tensions with Anthropology generated by the work of corporate ethnographers—in studying up, down and sideways in businesses—confront in altogether refreshing ways the problems generated by the 'crisis of representation' and its methodological extensions. Where ethnographers are subordinates to informants, as is typical of research in business settings, neo-colonial angst might be put entirely to the side. On the other hand, this apparent inversion of what is thought of as the traditional informant-anthropologist condition allows for interesting reassessments of anthropologists' work with interlocutors, and their work with us, past and present. Thus the potential exists for corporate ethnographers to speak to new formations in informant-ethnographer relations.

The relevance of unpacking the critical role of businesses to our contemporary modern condition cannot go underestimated. Rather than worrying about anthropology-at-home and the suggested loss of the efficacy through estrangement typical of traditional anthropological sites—working with 'others' in foreign spaces and, in the process, eventually making the strange familiar—I would suggest that the questions raised by that problem be inverted to deepen our insights. That is, we engage the really hard task of making our familiar strange. The work of corporations is entirely infused into our contemporary condition. Are we not, in fact, estranged from reality in imagining our modern lives as unfettered by corporations? Indeed the fantasy of our individual efficacy, and freedom, may be the most important work these organizations perform. Understanding that our resistance to the effects of modern corporations is porous at best, better that we get inside this problem through fieldwork of ourselves in the here and now. And better still if we do that through conducting substantive ethnographic fieldwork in corporations and exposing the results of our work.

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