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Challenging Anthropology: Anthropological Reflections on the Ethnographic Turn in International Relations

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Abstract
Ethnography and anthropology are intrinsically linked, but recently other disciplines have started to draw inspiration from anthropological methods. The ongoing ethnographic turn in International Relations has spurred debate on what ethnography is, what it means and entails in practice, and how to apply it in International Relations. Some assert that the ethnographic turn could not have taken place without adopting a selective and antiquated notion of ethnography; others counter that this argument draws on a caricatured version of ethnography. This article offers one anthropologist's reflections on these issues, drawing on ethnographic work within an international organisation and a state apparatus – both of which are areas of study more common in International Relations than in anthropology. This is not an International Relations turn of anthropology, but the practical and methodological challenges it involves are relevant to the ethnographic turn of International Relations and the disjuncture between the ethnographic ideals and anthropological practice.

Keywords
Anthropology, ethnography, International Relations, method, methodology

Introduction

The ethnographic turn in International Relations (IR) draws inspiration from anthropological methods in its study of global politics. It evolved in response to the text-based-oriented

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analyses and the over-reliance on discursive interpretations of the political associated with the linguistic turn.[1] The turn to language and discourse has had a major influence on the social sciences, albeit marred by a problematic tendency to equate discourse with practice.[2] New developments are underway which suggest a more practice-oriented approach. The ethnographic turn seeks to direct greater attention to everyday practices and embodied actions, thereby countering the criticism of IR as a static and state-centric discipline ill-suited for grasping the complexities of political life. The turn to ethnography promises greater attention to agency and a focus on non-discursive practices and the potential of resistance to discourses otherwise seen as totalising and reductionist to individual freedom. Rather than being deductive, it signals a sense of openness, with political scientists increasingly interested in embracing anthropologically inspired ethnographic methods to grasp what goes on beyond the world of spoken and written words.[3]

Just as IR has been turning towards ethnographic methods, anthropology has started to show greater interest in issues commonly covered by IR, like the state and global politics. However, this is not an 'IR turn' of anthropology but more an expression of the increasingly widening scope of anthropology. Studying across scale – to connect micro and macro, the local and the global – is not new to anthropology,[4] nor is the study of the state and globalisation.[5] On the other hand, these are arguably abstract phenomena, whereas anthropologists have conventionally focused on more concrete objects of study. Both these new anthropological trends and the ethnographic IR turn give rise to concerns over how to appropriate and juxtapose academic traditions and identity with new empirical scopes or methodological approaches. This article looks into these concerns, drawing on the author's own experience from fieldwork within the World Bank and a Ugandan government ministry to study knowledge/power formations and indirect governance mechanisms between the two institutions.[6] As these are unusual sites for ethnographic exploration

1. Iver B. Neumann, 'Returning Practice to the Linguistic Turn: The Case of Diplomacy', *Millennium: Journal of International Studies* 32, no. 3 (2002): 627–52; Patrick Thaddeus Jackson, *The Conduct of Inquiry in International Relations. Philosophy of Science and its Implications for the Study of World Politics* (London: Routledge, 2011); Derick Becker, 'The Neoliberal Moment: Communicative Interaction and a Discourse Analysis of the Global Political Economy', *International Politics* 47 (2010): 251–68.
2. Theodore R. Schatzki, Karin Knorr Cetina and Eike von Savigny, eds, *The Practice Turn in Contemporary Theory* (London: Routledge, 2001); Jon Harald Sande Lie, 'Post-Development Theory and the Discourse–Agency Conundrum', *Social Analysis* 52, no. 3 (2008): 118–37.
3. Iver B. Neumann, '"A Speech that the Entire Ministry May Stand For," Or: Why Diplomats Never Produce Anything New', *International Political Sociology* 1, no. 2 (2007): 183–200.
4. See Eric R. Wolf, *Europe and the People without History* (Berkeley, CA: University of California Press, 1982); Peer Schouten, 'Theory Talk #34: James Ferguson on Modernity, Development, and Reading Foucault in Lesotho', *Theory Talks* (2010). Available at: http://www.theory-talks.org/2009/11/theory-talk-34.html.
5. Christian Krohn-Hansen and Knut G. Nustad, eds, *State Formation. Anthropological Perspectives* (London: Pluto Press, 2005); Thomas Blom Hansen and Finn Stepputat, eds., *States of Imagination: Ethnographic Explorations of the Postcolonial State* (Durham, NC: Duke University Press, 2001); Aihwa Ong, *Neoliberalism as Exception: Mutations in Citizenship and Sovereignty* (Durham, NC: Duke University Press, 2006).
6. I spent two months in the World Bank in Washington DC, and 10 months in Uganda, mainly doing fieldwork within the ministry responsible for relations with external donor-institutions. See Jon Harald Sande Lie, 'Developmentality: An Ethnography of the New Aid Architecture and the Formation of the World Bank–Uganda Partnership' (PhD thesis, University of Bergen, 2011).

and involve practical limitations for achieving the usual ethnographic ideals, my fieldwork led to contemplation as to these ideals and the conditions for fieldwork and participatory observation. Thus, the title of this article – challenging anthropology – is meant to refer not to whether the ethnographic turn in IR contests anthropological methods or trespasses on its turf. Rather, it refers to how anthropology, as constituted by its cardinal ethnographic method, challenges its own ideals by engaging new empirical scopes less conducive to classic fieldwork and participant observation. Anthropology is both challenged and challenging at the practical, methodological and epistemological levels when studying those at the top ethnographically from the bottom. Thus, I argue that the classic ideals of fieldwork, as outlined by Malinowski, are largely incompatible with an ethnographic study of the state or a multilateral organisation. Yet, these ideals are what we work with and towards in the field as they are constitutive to the anthropological identity. By offering an anthropologist's empirically based reflections on the ethnographic endeavour and its ambitions, this article questions the notion of any 'pure', idealised version of ethnography and thereby speaks to the IR debate on how to appropriate ethnography.

In a recent exchange between Wanda Vrasti and Jason Rancatore on the relevance and applicability of ethnography in and to IR,[7] Vrasti asserts that IR's ethnographic turn 'could not have taken place without adopting an impoverished and somewhat antiquated definition of ethnography'.[8] Whereas Vrasti questions parts of this turn for drawing on a selective and instrumental concept of ethnography,[9] Rancatore criticises Vrasti for implying a 'pure' notion of ethnography. The pure version draws on an idealised version of ethnography – commonly understood in terms of 'fieldwork' and 'participant observation' that seek to grasp 'the native's point of view'. Such a stereotyped understanding of ethnography leads to a selective view of the complexity of ethnographic method. Underlying this problem is the confusion as to what ethnographic method entails in practice; moreover, anthropologists, who tend to claim ownership of ethnographic methods, seem to share an implicit understanding of ethnography that is hard to grasp for outsiders. This draws on the view that 'anthropology is what anthropologists do'[10] – and what anthropologists do is intrinsically linked with the methodological endeavour of hyphen break should be as ethnography so central to the anthropological identity.[11] As Geertz puts it:

> if you want to understand what science is … you should look at what the practitioners of it do. In anthropology … what the practitioners do is ethnography. And it is in understanding what ethnography is, or more exactly *what doing ethnography is*, that a start can be made toward grasping what anthropological analysis amounts to as a form of knowledge.[12]

7. Wanda Vrasti, 'Dr Strangelove, or How I Learned to Stop Worrying about Methodology and Love Writing', *Millennium* 39, no. 1 (2010): 79–88; Jason P. Rancatore, 'It Is Strange: A Reply to Vrasti', *Millennium* 39, no. 1 (2010): 65–77.
8. Wanda Vrasti, 'The Strange Case of Ethnography and International Relations', *Millennium* 37, no. 2 (2008): 279–301, 294.
9. Vrasti pays particular attention to Neumann, 'Returning Practice to the Linguistic Turn'; Iver B. Neumann, 'To Be a Diplomat', *International Studies Perspective* 6 (2005): 72–93; Vincent Pouliot, 'Towards a Constructivist Methodology', *International Studies Quarterly* 51 (2007): 359–84.
10. James L. Peacock, *The Anthropological Lens: Harsh Light, Soft Focus* (Cambridge: Cambridge University Press, 1986), 8.
11. Tim Ingold, 'Anthropology Is *Not* Ethnography', *Proceedings of the British Academy* 154 (2008): 69–92.
12. Clifford Geertz, *The Interpretation of Cultures* (New York: Basic Books, 1973), 5–6, emphasis in original.

Ethnography appears as an esoteric anthropological practice, and the lack of a clear idea of what this practice is makes other disciplines' adoption/import of it challenging and subject to contestation – not least since their versions tend to involve an extreme or caricatured ideal of ethnography, fieldwork and participant observation. Without seeking to define ethnography, let me simply remark that it typically involves going out and getting close to the activities and experiences of other people, observing and interacting with informants preferably over a longer period. What I wish to do is to call for greater introspective reflectivity as to the researcher's own methods and approaches, by offering some thoughts and reflections from my own ethnographic work.[13] These deliberations speak to the exchange between Vrasti and Rancatore in raising concerns, as seen from an anthropologist's perspective and drawing on empirical experience, as to what ethnography is and how to achieve its methodological objectives.

This article is, however, not the story of an anthropologist arbitrating an IR debate by telling others what ethnography is and how to employ it. Rather, in providing an empirically grounded methodological contemplation based on practical fieldwork, I seek to show that anthropologists also, with their ethnographic methods, may have an uneasy relationship regarding how they understand, employ and attain their own methodological ideals. Hence, this article responds to Rancatore's call for a reflexive exploration of what might be considered 'pure' ethnography in order to gain a deeper understanding before making use of it.[14] It does so by comparing the established methodological ideals, as once heralded by Malinowski,[15] with the practice of ethnographic research in sites fundamentally unlike the village studies from which the methodological ideals of ethnographic fieldwork 'to grasp the native's point of view' evolved.[16] In his formative work, Malinowski expounded on certain 'methodological ideals which soon became general ethnographic objectives, and still remain so'.[17] Despite the epistemological and practical challenges of seeing the informant's world from his own perspective, and the dilemma of reconciling participation *and* observation,[18] Malinowski's ideals remain important for the anthropological identity. As Marcus notes, these ideals constitute the '"classic fieldwork" [which] has become the key ideological trope of identity of the discipline congenial orthodoxy'.[19] Although these ideals are rarely achieved in full, they have a regulative effect for scholarly identity and practice – not least since Malinowski 'is read by all students before they embark on their first fieldwork' and no alternative authoritative operationalisation of the ethnographic

13. Lie, 'Developmentality'.
14. Rancatore, 'It Is Strange: A Reply to Vrasti', 65.
15. Bronislaw Malinowski, *Argonauts of the Western Pacific* (Prospect Heights, IL: Waveland Press, 1961 [1922]).
16. Ibid., 25.
17. Signe Howell, 'Doing Fieldwork in your Own Back Yard: Some Reflections on Recent Tendencies in Norwegian Anthropology', *Norsk Antropologisk Tidsskrift* 12, nos 1–2 (2001): 16–24, 18. My translation.
18. Neumann, in referring to Steven Turner, calls this 'the Mauss problem', that is, 'how to square the roles of participant and observer. Too much doxa means too little observing distance; too fluent practices spell too little observing'. Iver B. Neumann, *At Home with the Diplomats: Inside a European Foreign Ministry* (Ithaca, NY: Cornell University Press), 184.
19. George E. Marcus and Judith Okely, 'How Short Can Fieldwork Be?', *Social Anthropology* 15, no. 3 (2007): 353–67, 362.

method exists.[20] It is thus against these ideals that the ethnographer measures and reflects upon her practice. Comparing ethnographic ideals and practice demonstrates that it is not only the apparently strange case of IR and ethnography that warrants a reflexive exploration. Also anthropological ideals and practice have an uneasy relationship, particularly with regard to unconventional sites and scopes.[21] In an exchange with George Marcus on the idea and practice of ethnographic fieldwork, Judith Okely privileges and defends 'the classical field practice, while recognising it was never fully what it seemed. We have inherited an idealised, caricatured model, rarely lived.'[22] When Vrasti holds that ethnography in IR 'cannot accomplish the goals it was set out to realise … [being] a return to empiricism',[23] the argument rests on what she sees as IR scholars' ignorance of the methodological contemplation inherent to anthropology – which results in an importation of an idealised version of ethnography rarely practised by anthropologists themselves.[24] In fact, the discrepancy between ideal and practice is not exclusively problematic. As Rancatore notes, 'the inherent ethical dilemmas that confront the researcher using ethnographic methods are themselves potentially useful findings'.[25] It is not only the ethical dilemmas that may yield results, but also the practical challenges and reflexivity that arise when working with and towards the ideals of ethnographic fieldwork – as I experienced when studying the World Bank and a Ugandan ministry ethnographically.

New Scope, New Method? Anthropological Reflexivity

The anthropologist's reflexivity – the constant and reciprocal relationship between fieldworker and informants, underscoring that the fieldworker's position in the field influences the data that she gains access to and acquires – is intrinsic to ethnography.[26] My reflexivity concerns became intensified through my choice of theme and field, which required multi-sited fieldwork.[27] The research focus was knowledge/power formations as constructed, consumed and articulated by and in the interface between the World Bank and Uganda, which drew attention to issues of state formation, sovereignty and globalisation. Studying international organisations and the state from within has not been common in anthropology for two interrelated reasons: although studies of complex societies have become more common during the last decades, the anthropological identity has traditionally privileged villages and villagers (preferably in remote and 'primitive' locations); and studying the social spheres of

20. Howell, 'Doing Fieldwork in your Own Back Yard', 18. My translation.
21. Jean Comaroff and John Comaroff, *Ethnography and the Historical Imagination* (Boulder, CO: Westview Press, 1992).
22. Marcus and Okely, 'How Short Can Fieldwork Be?', 357.
23. Vrasti, 'The Strange Case of Ethnography and International Relations', 281.
24. James Faubion and George E. Marcus, *Fieldwork Is Not What it Used to Be: Learning Anthropology's Method in a Time of Transition* (Ithaca, NY: Cornell University Press, 2007).
25. Rancatore, 'It Is Strange: A Reply to Vrasti', 67.
26. Geertz, *The Interpretation of Cultures*, see particularly the final chapter 'Deep Play: Notes on the Balinese Cockfight'.
27. George E. Marcus, *Ethnography through Thick and Thin* (Princeton, NJ: Princeton University Press, 1998); Mark-Anthony Falzon, ed., *Multi-sited Ethnography* (Farnham: Ashgate, 2009).

the state and international organisations entails certain restrictions that impinge on the anthropologist's ability to achieve participatory observation, as these sites have a political, bureaucratic and formal character that restricts access and limits what informants can say. These methodological challenges have made anthropologists hesitate to approach the state and international organisations ethnographically, so most research has been undertaken from an IR perspective, with any 'ethnographic' twist limited to sporadic interviews in addition to the analysis of official discourses, formal self-representations, documents and texts.[28]

All the same, I see my study of an international organisation and a state apparatus from within not as an 'IR turn' of anthropology, but rather as an expression of the once-heralded but rapidly abandoned anthropology of what Laura Nader introduced as 'studying up'.[29] While anthropology has often been conceived of as the study of marginalised people and processes of marginalisation,[30] the call for anthropologists to 'study up' meant shifting the focus to 'the colonizers rather than the colonized, the culture of power rather than the culture of the powerless, the culture of affluence rather than the culture of poverty'.[31] Although Gusterson rightly notes that 'studying up' never took off,[32] there are a few examples – even before Nader's call – as with Powdermaker's 1951 ethnography of Hollywood movie-makers.[33] Reflexivity regarding their own position in the field, their access to information and their ability to collect ethnographic data appears intrinsic to those studying up. So was it for me, during and after fieldwork: was it anthropology I was doing when conducting fieldwork within the World Bank and a Ugandan ministry, given the challenges and limitations that these social spheres emplaced on my chances of achieving the ethnographic ideals of fieldwork and participatory observation? It became clear that studying up and 'new' empirical scopes gave rise to practical and methodological challenges that might undermine the scholarly embodied ethnographic endeavour.

Such considerations and reflexivity are not new to anthropology, as shown by the ambiguity in how anthropologists have approached the state as an object of inquiry. Anthropology faces at least two fundamental problems when engaging the state

28. See, for example, Catherine Weaver, *Hypocrisy Trap: The World Bank and the Poverty of Reform* (Princeton, NJ: Princeton University Press, 2008); Michael Barnett and Martha Finnemore, *Rules for the World: International Organizations in Global Politics* (Ithaca, NY: Cornell University Press, 2004).
29. Laura Nader, 'Up the Anthropologist – Perspectives Gained from Studying Up', in *Reinventing Anthropology*, ed. Dell Hymes (New York: Pantheon Books, 1972), 284–311.
30. Wolf, *Europe and the People without History*.
31. Nader, 'Up the Anthropologist', 289.
32. Hugh Gusterson, 'Studying Up Revisited', *POLAR – Political and Legal Anthropology Review* 1 (1997): 114–19.
33. Hortense Powdermaker, *Hollywood: The Dream Factory. An Anthropologist Looks at the Movie-Makers* (London: Secker & Warburg, 1951). But see also Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (Berkeley, CA: University of California Press, 1998); Ulf Hannerz, *Foreign News: Exploring the World of Foreign Correspondents* (Chicago, IL: University of Chicago Press, 2004); Karen Ho, *Liquidated: An Ethnography of Wall Street* (Durham, NC: Duke University Press, 2009); Neumann, *At Home with the Diplomats*; Stephen Nugent and Cris Shore, eds, *Elite Cultures: Anthropological Perspectives* (London: Routledge, 2002).

ethnographically. First is that the state – as conceived by political scientists, who see it as a distinct entity with particular functions – appears to be a pre-empirical entity external to what may be grasped by ethnographic methods without violating its inductive and empirical hallmarks.[34] The second problem concerns ontology and what kind of object the state is.[35] Surely the state is not a 'material' object, but is it an object of the second degree, like kinship or marriage? Radcliffe-Brown does not seem to think so when he denounces the state as an object for anthropological analysis: the state, he argues 'does not exist in the phenomenal world; is a fiction of the philosophers. … There is no such thing as the power of the state'.[36]

This 'death by conceptualisation' in the formative period of anthropology served to write off the state as an object of study. Over the past two decades, however, anthropologists have increasingly begun studying the state.[37] But instead of seeing the state as an entity in its own right before engaging it empirically, the focus is now on the practices, state effects and processes that reproduce and manifest the *idea* of the state – and not on the state as an ontologically given object.[38] These perspectives have gained further momentum through the governmentalisation of the state and our study of it.[39] Although anthropology has managed to overcome the impasse and conceptual challenges of studying the state, there remain many practical and methodological problems of access to the field and experience-near data. These relate to the ethnographic premises for studying the state idea and how it sustains the state system, and cannot be simply brushed aside. This has implications for the production of empirical knowledge and thus anthropological analysis, since an *a priori* application of the state-concept infringes upon the anthropological ideal of inductiveness. Malinowski warned that pre-empirical knowledge might blur our understanding of the field.[40] According to that view, the production of anthropological knowledge is premised not only by the choice of method, but also by the choice of empirical field. However, such a restrictive view is not in line with anthropological practice today. Anthropologists are arguing against this narrow ideal,

34. Timothy Mitchell, 'The Limits of the State: Beyond Statist Approaches and their Critics', *American Political Science Review* 85, no. 1 (1991): 77–96; Timothy Mitchell, 'Society, Economy, and the State Effect', in *State/Culture: State-Formation after the Cultural Turn*, ed. Georg Steimetz (Ithaca, NY: Cornell University Press, 1999), 76–97.
35. Christian Krohn-Hansen and Knut G. Nustad, 'Introduction', in *State Formation: Anthropological Perspectives*, eds Christian Krohn-Hansen and Knut G. Nustad (London: Pluto Press, 2005), 3–26.
36. Alfred R. Radcliffe-Brown, 'Preface', in *African Political Systems* eds Meyer Fortes and E.E. Evans-Pritchard (London: Oxford University Press, 1955 [1940]).
37. Michel-Rolph Trouillot, 'The Anthropology of the State in the Age of Globalisation', *Current Anthropology* 42, no. 1 (2001): 125–38; James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, CT: Yale University Press, 1998).
38. Philip Abrams, 'Notes on the Difficulty of Studying the State', *Journal of Historical Sociology* 1, no. 1 (1988 [1977]): 58–89; James C. Scott, *Seeing Like a State*.
39. Michel Foucault, 'Governmentality', in *The Foucault Effect: Studies in Governmentality*, eds Graham Burchell, Colin Gordon and Peter Miller (Chicago, IL: University of Chicago Press, 1991), 87–104. See also Iver B. Neumann and Ole Jacob Sending, *Governing the Global Polity: Practice, Mentality, Rationality* (Ann Arbor, MI: University of Michigan Press, 2010).
40. Malinowski, *Argonauts of the Western Pacific*.

favouring a broader concept for the scope of ethnography, one that can encompass subfields like auto-anthropology, anthropology at home and studying up.

In 1966, Claude Lévi-Strauss asserted that the modernisation of traditional societies posed a challenge to anthropology as a discipline and its method.[41] Reflecting on anthropological method in relation to the empirical field, Lévi-Strauss wrote that 'the new threat to our studies is not, then, so much quantitative as qualitative: these large populations [i.e. the 'primitives', as Lévi-Strauss calls them] are changing fast, and their culture is resembling more and more that of the Western world. Like the latter, it tends to fall outside the field of anthropology'.[42] The 'savage societies' seem to have been perennially vanishing since the outset as Malinowski already in *Argonauts* moaned about this concern. Yet, Lévi-Strauss calls for anthropological methods to be more responsive to the empirical field. And as 'we have less and less material to work with',[43] he also calls for a widening of the empirical scope – even for reversing the roles of researcher and informant to allow ourselves 'to be "ethnographized" by those for whom we are solely the ethnographers'.[44] Although the traditional problems of anthropology are assuming new forms, none of them can be said to be exhausted. Both the study of the remnants of primitive culture – 'the fundamentals of mankind', in Lévi-Strauss's words – and new emerging empirical fields require anthropology, or its ideals, to undergo a deep transformation: 'Anthropology will survive in a changing world by allowing itself to perish in order to be born again under a new guise.'[45]

In *Reinventing Anthropology*,[46] Hymes argues along similar lines as Lévi-Strauss in asserting that the conditions in which anthropology found a niche as an academic profession around the turn of the 19th to 20th century no longer exists: 'The implicit division of labour – anthropology on Indian reservations and in uncivilised places abroad, sociology at home … – has quite broken down'.[47] If, he goes on to say, the study of man were to be invented in the early 1970s, the result would not be an entity corresponding to anthropology as we know it today, because its scope, method and organisation are basically arbitrary expressions of the context in which the field was born. Thus, we can note a steady reconstruction aimed at remoulding the reified view of anthropology to contemporary empirical realities.

In the same volume, Nader argues for *studying up* as a new direction in anthropological studies.[48] By studying up, she argues, 'anthropologists have a great deal to contribute to our understanding of the processes whereby power and responsibility are exercised'.[49]

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41. Claude Lévi-Strauss, 'The Scope of Anthropology', *Current Anthropology* 7, no. 2 (1966): 112–23.
42. Claude Lévi-Strauss, 'Anthropology: Its Achievements and Future', *Current Anthropology* 7, no. 2 (1966): 124–27.
43. Ibid., 126.
44. Ibid., 125.
45. Ibid., 126.
46. Dell Hymes, ed., *Reinventing Anthropology* (New York: Pantheon Books, 1972).
47. Dell Hymes, 'The Use of Anthropology: Critical, Political, Personal', in *Reinventing Anthropology*, ed. Dell Hymes (New York: Pantheon Books, 1972), 3–82, 4.
48. Nader, 'Up the Anthropologist'.
49. Ibid., 284.

This relates to the perspective of anthropology as a discipline concerned with the study of marginalised peoples and the processes behind that marginalisation, but also with anthropology concerned primarily with the subalterns.[50] Studying up, by contrast, would enable the researcher to grasp those marginalising others by studying those who have the 'power to exclude themselves from the realm of the discussable'.[51] This is akin to what Marilyn Strathern calls 'auto-anthropology',[52] which refers to anthropology conducted in the intellectual context that produces it, or 'anthropology at home'.[53] Auto-anthropology entails a form of analysis that draws on analytical concepts belonging to the society one is studying, so these concepts cannot be applied to contrast or illuminate cultural difference from the realm of the researcher. Hence, 'anthropology at home' directly concerns the emic–etic schism of anthropology (see later). Moreover, it relates to Hastrup's distinction between cultural knowledge and anthropological knowledge – the former being practical and implicit, the latter theoretical and explicit.[54] The analytical process concerns the transition between these two, the shift from implicit knowing to explicit understanding.[55] These propositions draw on 'culture shock' – which Strathern embraces as an important methodological strength – as a means to gain cultural knowledge, to contrast and identify what is relevant in the empirical field, and thus to assist in the analytical process of translating between implicit knowing and explicit understanding, that is, the passage between ethnography and anthropology.[56] How, then, do we gain cultural knowledge, experience culture shock and attend to an emic view when conducting anthropology at home or studying up? These issues emerged as methodological concerns during my own fieldwork stays and relate to the disconnect between what I was doing and the ethnographic ideals of fieldwork and participatory observation.

With *Argonauts of the Western Pacific*, originally issued in 1922, Malinowski pioneered anthropological method.[57] It draws on fieldwork undertaken because and while Malinowski was stranded on an island as an alien on his return to Europe from Australia due to the First World War. This classic ethnographic work set the standard for later anthropologists in terms of methodology and the temporal and spatial depth and structure of collecting data through the now-familiar but still somewhat woolly ideas of 'fieldwork' and 'participatory observation'. Malinowski's emphasis on long-term fieldwork, the importance of learning the local language(s) and becoming familiar with native customs and categories through the researcher's participation in them became normative for

50. Wolf, *Europe and the People without History*.
51. Frederick Cooper and Randall Packard, *International Development and the Social Sciences: Essays in the History and Politics of Knowledge* (Berkeley, CA: University of California Press, 1997), 5.
52. Marilyn Strathern, 'The Limits of Auto-Anthropology', in *Anthropology at Home*, ed. Anthony Jackson (London: Tavistock, 1987).
53. Howell, 'Doing Fieldwork in your Own Back Yard'.
54. Kirsten Hastrup, *A Passage to Anthropology: Between Experience and Theory* (London: Routledge, 1995).
55. Kirsten Hastrup and Peter Hervik, eds, *Social Experience and Anthropological Knowledge* (London: Routledge, 1994); Anh Nga Longva, 'When Everyday Life Is Fieldwork: From Experience to Anthropological Knowledge', *Norsk Antropologisk Tidsskrift* 12, nos 1–2 (2001): 85–93.
56. Ingold, 'Anthropology Is *Not* Ethnography'.
57. Malinowski, *Argonauts of the Western Pacific*.

anthropological holism and the comparative approach, which are seen as imperative for reaching the ultimate anthropological goal 'of which an Ethnographer should never lose sight. This goal is, briefly, to grasp the native's point of view, his relation to life, to realise *his* vision of *his* world'.[58] This goal separates the life-worlds of the informant and the researcher by constructing an epistemological distinction between the two. But what happens then, as Geertz asks, with the researcher's 'psychological closeness, a sort of transcultural identification, with our subjects? What happens to *verstehen* when *einfühlen* disappears?'[59] This balancing act is intrinsic to anthropology and reveals a general methodological and conceptual problem, variously formulated as: '"inside" versus "outside," or "first person" versus "third person" descriptions; "phenomenological" versus "objectivist," or "cognitive" versus "behavioural" theories; or, perhaps most commonly, "emic" versus "etic" analyses'.[60]

Emic and Etic

The concepts of *emic* and *etic* derive from the distinction in linguistics between phonemics and phonetics – 'phonemics classifying sounds according to their internal function in language, phonetics classifying them according to their acoustic properties as such'.[61] The usage of the emic–etic divide within anthropological analyses points to two distinct approaches to the ethnographic field. An emic approach, which denotes the use of a conceptual apparatus being verbatim to that of the informants, makes it possible to depict the social world as experienced by members of that society. By contrast, the etic approach draws on the researcher's own concepts and understanding, making it possible to apply anthropological concepts and tools on the empirical material in order to enable comparative analysis – but in this, it is argued, there is a danger of losing the emic and native perspective. Hence, this balancing act is also a matter of epistemology and knowledge, how to grasp it, and, indeed, what the anthropological project is all about. The emic–etic divide is thus a divide between what is seen as either concrete or abstract; and whereas the emic relates to the ethnographic *description*, the etic relates to the anthropological *analysis*. It is a question of classification and how pre-empirical knowledge is utilised in the analysis.

The emic approach builds on Franz Boas's pioneering work on cultural relativism.[62] His historical particularism seeks to counter what he saw as simplistic notions of unilinear evolution and the idea that all of humankind evolves through the same cultural stages. Historical particularism and the associated cultural relativism are doctrines that cultivate the emic approach by underlining uniqueness: each culture has its own internal logic which must be understood in the particular temporal and spatial context. Boas was highly sceptical of generalisation or intercultural comparison, which, by employing etic concepts, 'too easily establishes artificial similarities between societies that were fundamentally

58. Ibid., 25, emphasis in original.
59. Clifford Geertz, '"From the Native's Point of View": On the Nature of Anthropological Understanding', *Bulletin of the American Academy of Arts and Sciences* 28, no. 1 (1974): 26–45, 28.
60. Ibid.
61. Ibid.
62. Franz Boas, 'Recent Anthropology', *Science* 98 (1943): 334–37.

different'.[63] The emic approach is aligned to that of Malinowski, as illustrated by Boas's statement: 'if it is our serious purpose to understand the thoughts of a people the whole analysis of experience must be based on their concepts, not ours'.[64] This view yields an epistemological position that seeks to know the world through engaging with it rather than classifying it with etic concepts.

To apply our own concepts in the analysis, then, belongs to the etic realm. Marvin Harris, who initially coined those conceptual terms, offers a comprehensive critique of the emic view when he asserts that the emic analysis provides an inadequate general theoretical framework even though it seeks to give a framework for the analysis of *all* human behaviour.[65] Harris argues that internal to the anthropological project is the aspiration to grasp how and why people act as they do, something the emic approach misses by giving pre-eminence to language over action. Harris's project of classifying human behaviour in terms of pre-empirical categories of body motion illustrates the fundamental discrepancy between the etic and emic approaches as regards epistemology and means of inquiry. With an etic approach, the researcher applies her own concepts and categories onto the ethnography. That in turn requires pre-empirical knowledge of the empirical field, to make cross-cultural comparison possible. The emic approach remains closer to the native's point of view – but grasping only the contextual and particular renders comparison impossible.

How anthropologists employ the emic–etic divide is thus akin to the distinction, originally made by the psychoanalyst Heinz Kohut, between 'experience-near' and 'experience-distant' concepts. An experience-near approach draws attention to concepts which an informant 'might himself naturally and effortlessly use to define what he or his fellows see, feel, think, imagine, and so on, and which he would readily understand when similarly applied by others'.[66] Conversely, the experience-distant approach draws on concepts employed by various specialists, such as ethnographers, 'to forward their scientific, philosophical, or practical aims'.[67] Whereas 'love', for example, is an experience-near concept, 'social stratification' is experience-distant. 'Religion' is more ambiguous; 'caste' and 'nirvana' might be concepts distant to the researcher but experience-near to Hindus or Buddhists. The point Geertz makes, which also applies to the emic–etic division, is that these distinctions are matters of degree and not polar oppositions: 'Confinement to experience-near concepts leaves an ethnographer awash in immediacies as well as entangled in vernacular. Confinement to experience-distant ones leaves him stranded in abstractions and smothered in jargon.'[68]

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63. Thomas Hylland Eriksen and Finn Sivert Nielsen, *A History of Anthropology* (London: Pluto Press, 2001).
64. Boas, cited in Michael Quinn Patton, *Qualitative Research and Evaluation Methods* (Newbury Park, CA: Sage Publications, 1980), 307.
65. Marvin Harris, *The Nature of Cultural Things* (New York: Random House, 1964); Marvin Harris, 'Why a Perfect Knowledge of All the Rules One Must Know to Act Like a Native Cannot Lead to the Knowledge of How Natives Act', *Journal of Anthropological Research* 30 (1974): 242–51; Pertti J. Pelto and Gretel H. Pelto, *Anthropological Research: The Structure of Inquiry* (Cambridge: Cambridge University Press, 1978).
66. As presented by Geertz, '"From the Native's Point of View"', 28.
67. Ibid.
68. Ibid., 29.

Usage of the emic–etic divide has become somewhat banalised since it was imported from linguistics in the 1960s. First of all, the conceptual pair is just as valid at 'home' as it is 'abroad', applicable not only to fieldwork conducted in distant places, but also to ethnographic work in our own backyard. Secondly, the relationship between emic and etic is not dualistic. The concepts constitute one other: it is impossible to identify anything as emic without seeing it through an etic grid – even a native's point of view is constituted from an etic viewpoint. This distance between the researcher and the informant is no less tangible if the researcher uses the informant's own concepts: the emic concepts are less familiar to the researcher while they are not made explicit and expounded upon when used for descriptive and analytical purposes. Thirdly, life-worlds cannot be separated. People and knowledge interact and move, their life-worlds coalescing and collapsing into each other. Empirically, it is impossible to compartmentalise emic and etic perspectives. The two overlap and hybridise.

Thus, the two approaches are more ideal types suited for reflection, 'being good to think with',[69] in the research situation than they are methodological straitjackets. As Pelto and Pelto argue, 'most anthropologists do not consider themselves to be either completely emic or unalterably etic, although many researchers see themselves as adhering to Malinowski's directive "to grasp the native's point of view"'.[70] A condition for conducting meaningful anthropology is that the researcher knows something meaningful that is not known to his informants: that is, that the anthropologist is able to connect a local reality and the natives' views to a level of abstraction through concepts that enable comparison. Nevertheless, Malinowski was employing an emic approach in arguing for the importance of understanding man and the society to which he belongs:

> If you remove a man from his social milieu, you *eo ipso* deprive him of almost all his stimuli to moral steadfastness and economic efficiency and even of interest in life. If then you measure him by moral, legal or economic *standards, also essentially foreign to him, you cannot but obtain a caricature in your estimate.*[71]

The challenge, then, becomes to balance between a straitjacketed emic approach and etic categories for the purpose of generalisation, aggregation and cross-cultural comparison, without caricaturing the field and informants. Malinowski was instrumental in shaping the methodological doxa of anthropology by, firstly, seeking to operationalise how to grasp the native's point of view and, secondly and interrelatedly, pioneering the ethnographic practice of long-term fieldwork and participant observation.

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69. Claude Lévi-Strauss, *The Savage Mind* (Chicago, IL: Chicago University Press, 1966 [1962]).
70. Pelto and Pelto, *Anthropological Research*, 63.
71. Malinowski, *Argonauts of the Western Pacific*, 157. Last emphasis added.

Operationalising the Ethnographic Ideals

According to Malinowski, successful ethnographic research 'can only be obtained by a patient and systematic application of a system of rules of common sense and well-known scientific principles'.[72] In the famous first chapter of *Argonauts of the Western Pacific*, titled 'Subject, Method and Scope', Malinowski presents three foundation stones of fieldwork:

> First of all, naturally, the student must possess real scientific aims, and know the values and criteria of modern ethnography. Secondly, he ought to put himself in good conditions of work, that is, in the main, to live without other white men, right among the natives. Finally, he has to apply a number of special methods of collecting, manipulating and fixing his evidence.[73]

In a close reading of Malinowski's first chapter, Howell summarises the methodological principles in eight points: (1) live together with informants under equal conditions; (2) interact in the local language; (3) participate in all activities to see what people do in order to grasp information not obtainable by questionnaires, interviews and so on; (4) grasp 'the imponderabilia of actual life'; (5) each phenomenon should be studied in the broad multitude of contexts within which it gets articulated; (6) each phenomenon should be perceived as singular and exclusive. These six principles lead to a seventh, which underlines the need to complement participatory observation with other sources of data, like statistical data, official records and so on. Finally, there is the overarching methodological principle – which also has become an objective – of grasping 'the native's point of view'.

These directives mark the methodological habitus of fieldwork as the *rite de passage* of anthropology and the paramount means of ethnographic data collection.[74] Central to the ritual experience, and likened to an epistemological passage for the researcher, are 'culture shock' – usually illustrated by the researcher's arrival story, which expresses the fundamental uncertainty upon which the anthropological enterprise is premised – and 'becoming'.[75] These two concepts are interlinked, and represent different stages of research. Culture shock refers to the researcher's immediate perceptions and tribulations when initiating fieldwork, and thus belongs to the realm of etic knowledge. The concept of 'becoming' denotes the researcher's (inter)subjective shift in overcoming such culture shock and being able to grasp a more native point of view. 'Belonging is a metaphor for a kind of participation that can never be complete … [which] implies that one gives in to an alien reality and allows oneself to change in the process.'[76] The process of becoming marks a transition in-between the realms of etic and emic – a transition that moves towards the latter but will never be fully complete.

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72. Malinowski, *Argonauts of the Western Pacific*, 6.
73. Ibid.
74. George Stocking, 'The Ethnographer's Magic: Fieldwork in British Anthropology from Tylor to Malinowski', in *Observers Observed*, ed. George Stocking (Madison, WI: University of Wisconsin Press, 1983).
75. Hastrup, *A Passage to Anthropology*.
76. Ibid., 19.

Fundamental to the directives provided by Malinowski is an emic approach disconnected from the researcher's own conception of the world. In his inaugural lecture at the Collège de France in 1960 titled *The Scope of Anthropology*, Lévi-Strauss argues that Malinowski, as an ethnographic experimenter, illustrated what could constitute proof in the ethnological sciences.[77] Here, Lévi-Strauss draws a distinction to Durkheim's analysis of *The Elementary Form of Religious Life*, which he denotes 'a theory of religion so vast and so general that it seemed to render superfluous the minute analysis of Australian religions which preceded it and – one hoped – paved the way for it'.[78] This distinction is reminiscent of the emic–etic divide and the kinds of background knowledge and concepts the researcher brings into the analytical process. The problem, he continues, is whether Durkheim would conceive of his theory without 'being forced, at the outset, to superimpose upon the religious representations received from his own society'.[79] The basic question, then, becomes: at what stage in the analytical process do pre-empirical knowledge and concepts enter the emic methodological approach? To return to Malinowski:

> Good training in theory and acquaintance with its latest results is not identical with being burdened with 'preconceived ideas'. If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of moulding his theories according to facts, and of seeing facts in their bearing upon theory, the better he is equipped for the work. Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies.[80]

Along with the epistemological property of an arrival story necessarily shaped by the researcher's etic knowledge, this quote indicates that perhaps Malinowski did not distance himself that much from the realm of etics after all. However, he sought to keep them separate – as shown by his posthumously published diary, in which he gives a less flattering and *non-native* account of his informants.[81] Flattering or not, the diary does show that 'you don't have to be one to know one',[82] and that it is possible to distinguish between personal and professional views – akin to the etic–emic divide. In the following, I offer some thoughts on the idealised methodological principles and the notions of culture shock and becoming by comparing them with my ethnographic study of the World Bank and its Ugandan counterpart.

Studying the State and Studying Up in Practice

My research was designed as multi-sited fieldwork in order to grasp both the donor and recipient side of aid relations and their partnership. Neither in Washington DC nor in

77. Lévi-Strauss, 'The Scope of Anthropology'.
78. Ibid., 114.
79. Ibid.
80. Malinowski, *Argonauts of the Western Pacific*, 8–9.
81. Bronislaw Malinowski, *A Diary in the Strict Sense of the Term* (London: Routledge, 1967).
82. Geertz, 'From the Native's Point of View', 29.

Uganda did I live *together* with my informants, despite the first Malinowskian principle. In the former case, I lived under roughly the same conditions as my informants, whereas my living conditions and quality of life in Uganda could be characterised as inferior to those of my informants, but superior to those of the average Ugandan. It was, I would argue, practically impossible to live together with my informants, thus challenging my aspirations for an emic approach. Their position as informants, however, was largely delineated by their employment in a development-focused institution. After working hours, they withdrew from the field and shifted to another status, such as spouse or parent, which made them less accessible and relevant to me. Nevertheless, I regularly spent time with informants outside normal working hours, and that yielded information in the sense of informal data, focused discussions and follow-up questions on issues raised earlier. In general, in both cases of fieldwork, my routine involved going every day to the office I had been allotted to see what transpired. When people left their offices, so did I.

As to the second principle of interacting in the local language, the language barrier was not that much of a problem. Numerous different nationalities are involved with the Bank, but as a US-based institution, it uses English as the medium of communication, and English is also the *lingua franca* of institutional development. It also happens to be an official Ugandan language, albeit not an indigenous one. All meetings, policy documents and interaction between government and the various donor agencies are conducted in English. Luganda,[83] which I do not speak, was occasionally used among my Ugandan informants, but when there was a *mazungo* (white man) present, they usually shifted to English. More problematic to the emic approach was the developmental idiolect, with its many unfamiliar buzzwords, metaphors, metonyms, acronyms and abbreviations. These I had to learn and memorise, in order to ease communication and bridge the insider–outsider gap. In fact, these idioms were not so different in function from those that constitute other 'tribal languages', like that of anthropology.[84]

The third and fourth principles are interlinked – to participate in all activities to see what people do and to grasp the information not acquirable by formalised methods; and to attend to the imponderability of actual life. Here, I fell short of the ideal. I attended many formal, official and informal meetings within and between the Bank and the ministry, but was more an observer than participant. This illustrates the difficulties involved in 'studying up' – the formidable bureaucracy of a large international organisation and the informal bureaucracy of a postcolonial state – at least when the focus is on practice at the level of actors. Multilateral institutions and government bodies comprise actors and venues responsible both for negotiating a nation's development strategy – with effects on the lives of many people – and for the management of hundreds of millions of dollars. Indeed, I can understand that an outsider, also a PhD candidate, may be refused access to the inner circles of negotiations and politics, and I was regularly told that such meetings were 'too political' for me to be present. The increased political tension between

83. Luganda is a Bantu language spoken by the Baganda, that is, the people living in the Buganda region of Uganda where Kampala is situated.

84. James Ferguson, 'Anthropology and its Evil Twin: Development in the Constitution of a Discipline', in *International Development and the Social Sciences*, eds Frederick Cooper and Randall Packard (Berkeley, CA: University of California Press, 1997), 150–75.

the Bank and its Ugandan counterpart caused a deteriorating partnership with effects for my research as it closed off access to empirical sites I earlier had been promised access to. In short, the political tension was caused by President Museveni – long regarded as a donor darling for introducing democracy and attending to donors' privatisation and liberalisation policies in the 1990s – who altered the constitution to lift the presidential terms limit, thus enabling him to run for office a third time. In an election campaign marred by corruption, use of force and nepotism, Museveni went on and was unsurprisingly re-elected despite the donors withholding a considerable amount of development assistance pledged and planned for in the national budget. This impinged not only on the partnership relation, being the scope of my research, but also had detrimental effects for my study of it. But does this disqualify the Bank–Uganda partnership as an empirical field and anthropological scope?

Malinowski's fifth and sixth principles might serve as a possible solution, in that these underscore that the research should not be limited to one approach – or to studying a phenomenon in only one setting. Any phenomenon, regardless of its uniqueness, should be studied in the various contexts it gets articulated. This approach verges on triangulation as the study of one phenomenon from multiple perspectives and in the different contexts where it is articulated.[85] Triangulation has parallels to the recent turn towards 'analytical eclecticism' and the mixed-method approach,[86] which have been criticised for lack of philosophical cohesion.[87] I would not claim there is any magic in triangulation, but it does make the researcher aware that different perspectives can yield different information on the same case – as illustrated by my approach to the aid partnership as articulated in formal and informal settings among both donor and recipient institutions.

The idea of triangulation and thus the combination of emic and etic approaches are underlined by Malinowski's seventh point, where official data, statistical records and so on are highlighted as complementary sources of information to underpin the analysis and information obtained through participatory observation. These aspects largely parallel my approach: thorough scrutiny of policy documents, and formal interviews and informal talks with actors holding various roles in the production, negotiation and consumption of formal policies, based on the assumption that 'social discourses are also embedded, or expressed, through writing'.[88] Contrasting the policies and other guiding documents that reflect the formal order with the practices and narratives of various informants makes it possible to distinguish 'between what people say they do, what they ought to do and what they in fact do' – or to identify disjuncture in order to comprehend the interface between the formal order and agency in practice.[89]

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85. Norman Denzin, 'Strategies of Multiple Triangulation', in *The Research Act: A Theoretical Introduction to Sociological Methods*, ed. Norman Denzin (Englewood Cliffs, NJ: Prentice-Hall, 1989), 234–47.
86. Rudra Sil and Peter Joachim Katzenstein, *Beyond Paradigms: Analytical Eclecticism in the Study of World Politics* (New York: Palgrave Macmillan, 2010); Alexander George and Andrew Bennett, *Case Studies in Theory Development in the Social Sciences* (Cambridge, MA: MIT Press, 2005).
87. Jackson, *The Conduct of Inquiry*.
88. Eduardo Archetti, 'Introduction', in *Exploring the Written: Anthropology and the Multiplicity of Writing*, ed. Eduardo Archetti (Oslo: Scandinavian University Press, 1994); see also Annelise Riles, ed., *Documents: Artifacts of Modern Knowledge* (Ann Arbor, MI: University of Michigan Press, 2006).
89. Joy Hendry and C.W. Watson, 'Introduction', in *An Anthropology of Indirect Communication*, eds Joy Hendry and C.W. Watson (London: Routledge, 2001), 1–15, 4.

Did I then manage to grasp the native's point of view – the eighth and final methodological principle? Here, the distinction between the emic, native's point of view as the ultimate goal of fieldwork and as a means – or approach – for gaining information becomes relevant. I would maintain that it was difficult – if not impossible – to obtain the full 'native perspective' due not only to the epistemological concerns of separating the emic from the etic, but also to the practical challenges associated with studying up and doing fieldwork within highly formalised institutions. However, I never lost sight of the emic approach as a *means* for acquiring information and understanding the setting. I continuously aspired to grasp the emic and native's point of view by employing an actor-oriented approach to comprehend how actors in the aid chain understand each other, the development order, concepts, policies and, not least, their own position embedded in larger structures and discourses of development. The ability to grasp the native's point of view, I hold, is closely related to the empirical field the ethnographer chooses to engage.

A kind of culture shock emerged upon arriving in the field, despite having done auto-anthropology and studying up. Interestingly, this shock was much more pronounced upon arriving in Washington than in Uganda. That might be due to my biased expectations of what to expect from African state bureaucracies after previous studies in Ethiopia. But never before had I experienced such a vast, formal and complex bureaucracy as with the Bank's Washington headquarters. That made my in-field learning curve very steep in terms of data collection approaches. The extremely formal character of the field made it impossible to apply anthropology's traditional methodological ethos of 'following the loops', as anthropologist Fredrik Barth once called it. Meetings, interviews and participation in various activities all had to be arranged, to a much larger degree than in the less formal Ugandan bureaucracy. And then there were my encounters with figures of authority like politicians, senior officials and aid practitioners, many of whom held doctoral degrees. From their positions of seniority, they would frequently comment on me and my work, ranging from remarks on my haircut and casual appearance – asserting that a more formal look would yield more respect and thus provide me with better access[90] – and my choice of method, scope and theory. Such feedback was helpful, and a good source of information, although some informants' scholarly views and poststructural reflexivity effectively collapsed the emic–etic divide.

Walking into a village to conduct fieldwork is probably easier than entering a ministry or multilateral institution. Both my fieldwork contexts are highly bureaucratic settings involving strict formal codes. Following the loops and drifting with my informants was possible, but only to a certain point. The bureaucratic character of the field often meant that I was excluded from certain settings, especially those now deemed as being of a 'too political' character and which I had been promised access to earlier. The sudden rise in

90. One Friday, while in the Bank, I was to attend a meeting with some executive directors and later interview one of the Bank's vice-presidents. Hence, I found it appropriate to dress up, unaware that the day happened to be an American *casual* Friday! In Uganda, I received several remarks, both from government informants and donor representatives – even when I had put on the proper attire. I did not have an air-conditioned vehicle at my disposal, and by the time I arrived by the 'number 11 bus' (i.e. walking), *boda boda* (motorbike taxi) or *matatu* (crowded minibus), I was often sweaty and dirty, and thus seldom looked as impressively neat as when I had started out.

political tensions between the Bank and Uganda were not only detrimental to their partnership, but also inflicted on my research by curtailing my empirical sphere. Once, when attending a meeting in Uganda between government and donor officials, I was evicted after having introduced myself in the usual presentation round. That was the only time I was actually expelled, but I was refused access to other meetings due to political concerns or tensions between the Bank and its counterpart.

Although they challenged my research, such events told me something about the partnership, while it also served as a good entry point for meeting and discussing with informants – demonstrating that processes of exclusion and challenges involved in living up to the ideals of ethnographic fieldwork may prove just as interesting and rewarding for data collection as actual processes of inclusion. This is illustrated by one of my arrival stories: early in my Uganda fieldwork and on the request of a ministerial senior official, I had to present a letter stating who I was and that I was an independent researcher – meaning independent of the Ugandan government, the Norwegian embassy and the Bank. The official wanted the letter to be signed either by the Norwegian embassy or the Bank's country office, but none of them wished to do so. Both referred to my independence, but it was clear they now considered my research too political and had no wish to be formally associated with it, for fear of liability, even though what they were being asked to sign stated precisely the opposite. Finally, and after about a month of transcontinental 'micro-diplomacy', the ministry official signed the letter himself, after giving a tirade about development bureaucracy and how donors say aid is apolitical while refusing to sign the letter over political concerns. In effect, this yielded greater understanding and sympathy of my project from a gatekeeper to the Ugandan bureaucracy, and good ethnography to my analysis of the World Bank–Uganda partnership. As the official added: 'this story will provide you with information for at least one chapter'. Well, it did not come out as a full chapter, but made more than a parenthesis in demonstrating that the lack of practical access to data might also be a good source of information and could tell something important about the field.

Conclusion

One cannot study what one does not have information of. Needless to say, I felt rather demotivated in being excluded from important parts of the empirical field I earlier had been promised access to. The restrictions I faced demonstrate the practical challenges with studying up since the powerful at the top may simply refuse to be studied ethnographically from the bottom. This produced an ever-widening gap between the ideals of fieldwork and my practice of them. But this process had generative effects for my research. It revealed important aspects of the partnership relation, and in being a victim of state effects, it established a communion between me and my Ugandan informants who also faced a hard time vis-a-vis the Bank. But more profoundly, it demonstrated that even though I was cut off from certain aspects of the field, I was nevertheless entangled and interwoven into it. Turning challenges into opportunities is contingent on the researcher's reflexivity and ability to adapt to new circumstances. This, moreover, is premised on seeing neither the empirical field nor the methods applied to grasp it as

something static, objective and ontologically given. And that is vital to the ethnographic endeavour. A classic Malinowskian understanding of ethnography is incompatible with a study of the state seen as an ontological given entity. An idealised, pure approach entrenches a version of ethnography that valorises the local over its externalities. Such a myopic worldview leads to a sort of political impotence. To make ethnographic sense of the state calls for greater reflexivity by the researcher and implies a rethinking of both the static state concept and the methodological ideals of fieldwork. This should not be understood as a wholesale abandonment of the ethnographic ideals, but rather one should not confuse these ideals with practice. Although rarely lived, the ideals, or 'pure' version of ethnography, serve as gatekeeping concepts which the ethnographer works with and towards in practice.

Asked why IR scholars and anthropologists fail to communicate, James Ferguson holds that 'it is striking in many ways how differently political scientists and anthropologists will approach what looks like the same question … there hasn't been much explicit reflection … on the gap between the political scientists who "start from the top" and anthropologists who "start from below"'.[91] Starting from below is more a question of attitude. Instead of starting with a bunch of certainties and assumptions, it signals 'a sense of openness, a willingness to be surprised and learn something new and not to be so deductive'.[92] Neumann provides a similar argument. In the conclusion to his recent book he reflects on his dual identity as an anthropologist and political scientist: 'Anthropologists focus on the constitutive, political scientists on the outcome … anthropologists focus on the preconditions for political order, and political scientists on how that order is maintained.'[93] This, Neumann holds, is due to the disciplines' distinct ideal-typical styles of reasoning. Political science takes the existence of particular institutions and structures as the starting point of analysis with the aim to excavate some kind of objective truth about them. Anthropology sees structures and institutions as socially constructed and culturally defined which, in turn, necessitates greater reflexivity when studying them. This distinction might be simplistic, but it helps in demonstrating scholarly differences, in understanding why 'it is the blossoming of political anthropology that keeps the classical study of politics alive'[94] and in explaining the ethnographic turn of IR. Ethnography is, however, no quick fix. As I have demonstrated in this article, the kind of openness and reflexivity called for by Ferguson and Neumann refers not only to the empirical field, but also to the methods one applies for making sense of it. Reflectivity and intersubjectivity at the levels of empiricism *and* method are important to ethnographic knowledge production – particularly when ethnography is engaged in unusual anthropological fields or by other disciplines.

91. Schouten, 'Theory Talk #34: James Ferguson on Modernity, Development, and Reading Foucault in Lesotho', 3.
92. Ibid., 1.
93. Neumann, *At Home with the Diplomats*, 183.
94. Ibid.

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