Anti-social anthropology? Objectivity, objection, and the ethnography of public policy and professional communities*

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One legacy of Malinowski's ethnographic method is the separation of 'field' and 'desk'. What anthropologists know is inseparable from their relationship with those they study – the epistemology is relational – but ethnographic writing breaks fieldwork relations, cuts the network, and erects boundaries: it is necessarily anti-social. As anthropologists turn their interest in what people believe, say, and do (and the inconsistencies between these) to the inter-connected institutions that comprise the modern world, to policy and professional communities of which they may also be members, their method of entering and exiting social worlds becomes more difficult. Arguing for the particular importance of an ethnographic perspective on the practices of powerful institutions, this article uses recent research on international aid and development to show how influential informants object to ethnographic accounts, resist anthropological boundary-making, and attempt to unpack academic knowledge back into relationships.

At the end of September 2002, I completed the first draft of a book on international development policy and practice, taking as its focus a project in tribal western India funded by Britain's Department for International Development (DFID) (Mosse 2005*a*). This was an unusual piece of research, complex, long-term, multi-sited, and initially unintentional, drawing as it did on insights as a participant-insider within international aid; and its conclusions questioned prevailing assumptions about development policy-making and project practice. The book manuscript provoked unusual controversy. Objections were made by my co-workers and informants to the publisher, to my university research ethics committee, my Department convenors, the Dean and the academic head of my university, as well as to my professional association the ASA (Association of Social Anthropologists of the UK and the Commonwealth) on the grounds that the book was unfair, biased, contained statements that were defamatory and would seriously damage the professional reputation of individuals and institutions, and would harm work among poor tribals in India. Those of my project colleagues who raised these objections sought to interrupt the publication process and to ensure that many parts of the book were rewritten. In April 2004 I was called to defend

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my ethnography in front of angry informants – international experts and Indian project managers – in the presence of professional colleagues. The move to publication had strained and broken valued relationships of fieldwork.

Malinowski might have been puzzled by such a scenario. The young Malinowski might have wondered how anthropologists could turn ethnographic attention to the schemes and policies of those authorities upon whose good office they – and especially he – relied for fieldwork, rather than confining comment to letters and diaries (Young 2004); and the older Malinowksi might have been surprised that the 'practical anthropology' he so strongly promoted in the 1930s as the basis for a science of colonial social policy (Kuper 2005) could turn so controversial. However, what I want to suggest in this article is that the scenario in fact arises from the fundamental structure of the ethnographic method that Malinowski innovated nearly a century ago. That structure is the relationship between fieldwork and writing – between, for Malinowski, the empirical work of observation (of the actualities of Trobriand life) and the 'constructive work' of tabulation, inference, and theory (Malinowski 1922); a relationship now stretched on Leach's (1961) famous rubber sheet to the point where entirely new problems in the analysis of events are emerging. The challenge for anthropology today is not how to rearrange 'fieldwork' – that dubious category that has come to signify any shift in location that is the ethnographic pretext (Gupta & Ferguson 1997) – or how to re-frame 'writing', but how to get to grips with the changing relationship between the two: change, first, in how fieldwork relations shape writing, and, second, in how writing now alters relationships of 'the field'.

In many ways anthropologists are a lot closer to their 'other' than they used to be. Arguably Malinowski's fieldwork was a method of dislocation rather than of 'immersion'. His now legendary social distance from his Trobriand subjects, his loneliness, and his scientific isolation from the flow of social relations formed, Michael Young suggests, the bedrock of his 'synchronic functionalism' (2004: 523). What Malinowksi's successors lacked of his brilliant powers of observation and exhaustive description, they made up for in forging closer relations with their subjects, greater identification, equity, and dialogue, often through long-term and repeated fieldwork. But with this, ethnographers became socially bound into their field sites in a new way, or, as Parkin puts it, became increasingly 'templated' by the field (2000*a*: 101). In parallel, an 'unbounding' of the field so as to include webs of regional and transnational connections and communities means that all anthropologists now research to some degree as 'insiders' or 'at home'. Furthermore, with the higher education funding squeeze of the 1980s (at least in Britain), trained anthropologists (myself included) joined non-academic institutions – for example, in international development (Panayiotopoulis 2002; Spencer 2000) – and while meeting new professional obligations also began contributing to a growing body of 'insider ethnography' of organizations and public policy. As researchers, we resolved the intractable problems of access to closed organizational worlds through membership of the communities we ended up studying. But in doing we so substituted a set of boundaries that kept us *out* (the problem of access) with another set that kept us *in*. Those who made themselves professional insiders in this way faced the problem not of entering a different world so as to be able to imagine or infer the taken-for-granted (and therefore hidden) way in which 'individual action and collective illusions are interlinked' (Hastrup 2004: 469), but of exiting a known world for the same purpose.

In fact, closer relations in the field, long-term and insider research have all made exit rather than entry the significant shift in location that is ethnography's pretext –

including exit from the templates of our younger ethnographic or professional selves (cf. Parkin 2000*a*), when 'the field' is a dislocation in time. And as other boundaries fade, it is often the detachment of writing itself that has become the primary mode of exit.

While fieldwork has changed beyond recognition – becoming ever more intensely social – ethnographic writing (interpreting, objectifying, and textualizing) remains a solitary process that disembeds knowing from its relationships, denying (to varying degrees) the social its claim to power, to ownership, to negotiation.[1] For Malinowksi himself, writing was a necessarily anti-social process incompatible with intense social intercourse, frustrated by the conversational (Young 2004: 544) and '[t]he unnecessary communion of souls' (Young 2004: 552, citing Malinowski 1967). What we have inherited is not so much a particular practice of fieldwork, as an ethnographic method premised on the division of field and desk – the social and the anti-social – experienced by every returning researcher. The changing nature of fieldwork – its closeness – both intensifies this division and surrounds it with tension.

Closeness makes writing more difficult, not just because of an 'exponential sense of incompleteness' that David Parkin (2000*a*: 103) notes of accounts which are always partial and provisional, but because ethnographic writing begins to have significant social effects of its own. The detachment of writing is now socially experienced by others. Of course, those reading about themselves may be intrigued, amused, or pleased;[2] but turning relationships into data, and placing interpretations in public, can also disturb and break relationships of fieldwork. It may be 'anti-social'.[3] Those interlocutors – neighbours, friends, colleagues, or co-professionals – who directly experience ethnographic objectifications now surround the anthropologist at her or his desk; they raise objections, make new demands to negotiate public and published interpretations. The relationships of the field persist, the capacity to exit through writing is in question, and ethnographic representations have become unavoidably part of the world that is studied. When desk collapses into field, something important has changed in the structure of ethnographic practice. We are starkly confronted with the essentially relational nature of anthropological knowledge,[4] no longer an object in our possession. That is to say, what anthropologists know is inseparable from their relationship with those they study. Consequently, the issue at hand is not just ethical but epistemological.

The 'narrative ethics' of the 1980s that followed Edward Said's critique of Orientalism and the 'writing culture' debate left some anthropologists imagining the problem could be solved by a retreat from representation altogether, allowing subjects to speak in their own words through personal narratives. Such replacement of description with *evocation* (Parkin 2000*a*) and the honouring of individual agency implies, Kapferer suggests, a broad 'shift away from concerns with social relations and interactive structures' (2004: 152) that has made the discipline 'anti-social' in another sense. Recently the metaphor has shifted again from dialogue to collaboration in ethnography (Lassiter 2005; Marcus 1998). Of course, the notion that power inequalities between the interpreter and the interpreted can be dialogued away, or 'written out', is too obviously false; and, as Spencer (1989) points out, the analytical cost of this pretence is considerable. But equally problematic, in the politics of representation debates, and the professional ethical guidelines that they influenced, were the assumptions made about the gradients of power across which research takes place. Relying on informant self-representation and allowing subjects to speak in their own words are not self-evident solutions for anthropologists of public policy whose informants are officials at the World Bank, the International Monetary Fund, DFID, or any group with a strong organizational need to produce and protect authorized views. Moreover, if they do critically investigate organizations or public policy of one kind or another, anthropologists begin to face not just personal unhappiness, but also public and formal reprimands, or even the threat of defamation proceedings, for their ethnographic accounts;[5] threats which may none the less still be framed in terms of research ethics set out in guidelines drawn up from an earlier generation of ethnographic practice in order to protect those lacking other means of redress.

Here I am concerned with this kind of ethnography, where field/desk, self/other, subject/object, here/there distinctions do not apply in the same way; where reflexive concern about epistemological privilege gives way to worry about epistemic capture or co-option; but which is no less based on extended fieldwork and participant observation of the social and symbolic transactions of communities, even if these are epistemic communities (e.g. of policy experts) interacting electronically, 'organized transnationally with their own public spheres, and with contractual or other exchange relations with similar groups' (Friedman 2004: 164). Why undertake such study? Well, ethnography offers particular insights into relationships mediated by policy ideas within contemporary 'network society' (Castells 1996). It also offers another means of public engagement with powerful institutions whose knowledge systems constantly organize attention away from the contradictions and contingencies of practice and the plurality of perspectives. Or as Burawoy puts it, 'by highlighting the ethnographic worlds of the local, [anthropology] challenges the postulated omnipotence of the global whether it be international capital, neoliberal politics, space flows, or mass culture' (1998: 30). But my concern here is not to explicate or justify the anthropology of policy, global governance, or international development (see Mosse 2005*b*) but to examine the dynamics of such research in the light of the ethnographic practice of exit and objection.

In what follows I will first explain my approach to a piece of insider research and outline the main argument that resulted. Second, I will examine the nature of the objections made to this ethnography, and the epistemological divide between an anthropologist and his informant-readers that they reveal. Third, I will track the unfolding controversy to see how the boundary between field and desk is contested, and how 'rightness' is both countered and claimed in an ethnographic encounter. My concern with representations in anthropology here points not to reflexive poetics but to the politics (and ethnography) of objection.

I worked as an anthropologist-consultant on a development project from its initial design in 1990 until 2001. Because of this rare continuity, and the *particular* importance of this project as a 'flagship' within the 1990s British aid programme – demonstrating a new commitment to participatory and poverty-focused interventions – the DFID agreed to support a study of the project experience from my particular anthropological perspective. This would be a critical analysis of policy and administrative rationality and modes of expertise in aid and development – including those of social anthropology itself. It would be based on the best available evidence, but would not cease to be an interested interpretation, a personal analytical account; an ethnography in which I was myself a key informant.

Over ten years I was part of an extended project team including consultant colleagues (experts in forestry, crops, irrigation, soil and water conservation, or gender), project managers, and field staff, all with whom I spent time in meetings, on long journeys, at their homes, with farmers in the scattered villages of this Bhil *adivasi* ('tribal') region of western India, and in drafting and redrafting collaborative reports. These intense periods of interaction from which grew personal relationships of understanding, trust, and respect were the first source for my research. The second was the series of studies and reports produced from our various engagements, and a large body of contemporary project documentation (for 1990-2001). As a third layer of research, in 2001 I returned to India to carry out interviews with project workers and ex-staff, as well as with India- and UK-based DFID officials and consultants. 'The aim was to test and verify my understanding of project processes, to decentre my own view, and to extend the analysis to the wider context of British aid in India' (Mosse 2005*a*: ix).

Now, there was another, fourth, methodological level, and this concerned the response to my analysis from those who shared the experience and about whom I write. Such ethnography courts controversy and is likely to produce objections. I would like to suggest that these objections are themselves part of research which emerges from, and reflects on, relationships in development. And here I concur with Bruno Latour's view of 'objectivity', which derives not from standing above the fray or suppressing subjectivity, but from maximizing the capacity of actors to *object* to what is said about them (2000). So, I shared my writing with 'informants', collaborators, colleagues, and friends, who possessed a capacity to object.

Summary of the argument

Before outlining these objections let me explain, at the risk of oversimplifying, the nature of the ethnography at the centre of the controversy (see Mosse 2005*a*). It is an exploration of the relationship between international development policy and project practice, and focuses on 'participatory approaches' prominent in the 1990s. The argument unfolds around five general propositions.

The first is that policy in development functions to mobilize and maintain political support as much as to orientate practice. I show how, in this case, the work of project design served to negotiate relationships and bring together diverse and quite incompatible interests around a causal model that justified the allocation of resources. I explain how the conceptual and linguistic devices that enrolled support also built contradictions into the design that made its straightforward execution in practice impossible.

The second proposition is that development interventions themselves are driven by the exigencies of organizations and the need to maintain relationships rather than by policy. My book (Mosse 2005*a*) describes in some detail how the informal everyday practices of project workers constituted a system of relationships shaped by the political logic and culture of the project agency, and by its demand for administrative order (i.e. by what may be called 'system goals'; Quarles van Ufford 1988), and routinely contradicted the prescriptions of official policy, substituting bureaucratic rules, targets, and controls, or relations of patronage, for anticipated community self-reliance (see Mosse 2005*a*: 109-31). In fact, the project was a world comprised of different autonomous spheres (of the village, fieldworkers, office, managers, consultants, donor advisers), mediated by institutional brokers, in which policy models *could* not organize practice. Which is not to say that policy was irrelevant, but that as a kind of mythology it was only partly a 'charter for action', since it had symbolic functions – accounting upwards, legitimizing expertise, signifying alliances, or concealing differences – that were 'at least as relevant as pragmatic ones' (Leach 2000*a* [1957]: 59).

Although their practice often contradicted the prescriptions of participatory project design, staff (including consultants, and Bhil villagers too) none the less worked hardest of all to sustain and protect official interpretations of actions, so that they articulated with higher DFID policy, matching events to theory in many and sophisticated (though not always conscious) ways – because, thereby, success and their (our) own interests were secured. Here were several project 'rituals' in Leach's sense of procedures 'to overcome the anxieties which are generated by [the] lack of fit between how things are and how we would like to think about them' (2000*b* [1976]: 87).[6] My third proposition, then, is that development projects work to maintain themselves as systems of representations as much as operational systems.[7] The work of international development consultants such as myself, while appearing to assist development operations, in fact mostly serves to produce/sustain policy models offering an authoritative *interpretation* of events that result from quite different logics; not (as we imagined) preceding or directing action but following it. Through such expert discourse unruly practice is stabilized, and the gap between policy and practice constantly negotiated away. Again, I have to refer the reader to the ethnography which shows how such interpretations have, further, to be sustained socially by enrolling supporters and building an interpretative community that made the project successful; and how policy designs also provide the framework of self-objectification for project actors accounting for themselves to each other and to outsiders.

The fourth proposition is that, correspondingly, project failure is not the failure to turn designs into reality, but a certain disarticulation between practices and their rationalizing models. Failure, as I discovered, is a failure of interpretation. And this I demonstrate ethnographically through the exploration of a project crisis in the context of evaluation and the rapid shift in DFID aid policy after 1997. Such policy changes have 'the effect of making the chains of translation in development more complex and harder to negotiate' (Mosse 2005*a*: 216). The final proposition concerns the way in which policy discourses of 'success' and 'failure' conceal the local social effects of development interventions, not only perpetuating misleading explanations, but also concealing valued outcomes, which, in the case of Bhil villages, include new forms of patronage, access to resources (subsidies, agro-inputs), and the means to articulate new aspirations of progress and cultural re-valuation (Mosse 2005*a*: 205-29).

The ethnography explains all these as general and inherent features of the system of international aid not as the failings of one particular project. It is not an evaluation. Indeed its central concern is not whether development projects are successful, but *how* 'success' is socially produced or constructed. Its supposition is that in the hugely complex cross-cultural world of development, most actors (including apparently powerful ones) have very little control over events.[8] What is usually more urgent and more practical is control over the interpretation of events; and as Bruno Latour reminds us, the success of policy ideas or project designs is not inherent (not given at the outset) but arises from their ability to continue recruiting support and so 'impose their growing coherence on those who argue about them or oppose them' (1996: 78; Mosse 2005*a*: 8). Since it is prone to being misread, let me stress that my argument is not intended as a criticism of aid and development. Rather, my analysis of the practices of a participatory development project, including my own role within it, aims to understand the micro-social processes of policy. So, first, it does not imply a rejection of this enterprise (or of aid projects more generally). My book makes clear that the project did have a significant positive effect on the lives of many thousands of disadvantaged Bhil *adivasi* people; but often in unscripted and unintended ways (Mosse 2005*a*: 227). However, the point is that the project did not 'work' because it was well designed. Stability in the world of action did not come from coherent policy; good policy was not implementable. None the less, policy is absolutely central to what happens in development arenas (Mosse 2005*a*: 20). Second, this is not a case of cynical disengagement. My argument does not express regret at my own involvement in this aid project, nor warn other anthropologists against working in development. On the contrary, the implication is that that there is greater need than ever for anthropologist involvement in the complex and contradictory arenas of international aid. But in order to grasp the social and political processes through which aid policy is made and transformed in practice, and which have a major bearing on outcomes, anthropologists have to negotiate space for their involvement to be more ethnographic and resist institutional pressure to conform to dominant policy-driven or economics-based knowledge systems (see Mosse 2004). Anthropologists do not have to choose between optimism and scepticism. In relation to development, as Quarles van Ufford puts it, they participate in three separate domains – the domain of hope, of politics/administration, and of critical understanding (Quarles van Ufford, Kumar & Mosse 2003). That is to say, our engagement is with the discourses of moral responsibility and policy vision (about what is to be done), with the strategic politics of programme action, and with critical reflection. These are not exclusive modes of thought and action, but neither are they entirely reconcilable. The danger is in asserting any one over the others (Mosse 2005*a*: 240-3; Quarles van Ufford *et al.* 2003).

Of course my ethnographic analysis is a *positioned* interpretation, which does not preclude other accounts. It is also one in which I place myself alongside others who worked on the project, taking my responsibility for shaping the project's design, for its construction of success, for the naïvety, over-ambition, and wrong-headedness of my own contributions. I can admit these as personal failings, but also see them as prefigured by the structural and discursive conditions of a development project without doubting my own or others' sincerity or commitment.

The nature of the objections

As I said, I shared my writing with my colleagues, collaborators, and 'informants'. Now, most who responded to the drafts over eighteen months – especially my social development and field staff co-workers – in fact gave strong endorsement to my analysis, describing it as 'balanced', 'truthful', 'insightful'. However, my attention became preoccupied with those key actors (including UK technical consultants and those in managerial positions) who took strong exception to my 'too negative and unbalanced' account, which was 'unfair and disrespectful', 'out of date', and even 'damning of all our work'. This group, represented by a UK consultant and DFID Project Adviser,[9] disagreed fundamentally with my conclusions and wanted the book re-written. Such a reaction should disturb any ethnographer; the more so for me because these were my close colleagues, co-workers over thirteen years.

The objection unfolded in stages: personal disagreement and friendly editorial advice gave way to questions of abuse of contract (was I entitled to use information acquired as a consultant) and appeal to the principle of participation in research – the notion that collective experience has to be collectively analysed. Here there was an issue both of method – by failing to make the research fully participatory I reduced its validity and laid myself open to criticism for being unbalanced; and of morality – I was at fault for the individual appropriation of shared experience, substituting 'stand-alone arguments' for collective experience. How could I, one exasperated colleague asked, 'the *participation consultant*, not want to review the project experience in a fully participatory manner?' But worse, by making myself an outsider, I was disinvesting from categories of explanation that I had myself promoted, fed back, and routinely used. Finally, in graver and angrier tone, the ethnography was challenged as potentially seriously damaging to the professional reputations of individuals and institutions (the Indian agency, a UK agricultural research institute, the DFID); it would adversely affect the ability to attract finance and undo the work of those who had given their best years.

I was astonished by the strength of emotion conveyed through e-mail, telephone conversations, and eventually face to face about a book which to all its independent readers did not appear to defame or malign the reputation of anyone. Nor was there any plausible explanation of how the book would damage organizations (DFID or the project agency), destroy the programme, or its routes to funding.[10]

Of course, I rose to defend the truthfulness of my account and the soundness of my research methodology; but it also occurred to me that my critics were themselves enacting the very argument they objected to, offering extraordinary confirmation of the key point that authoritative actors work hardest to defend projects as 'systems of representations', not only against the destabilizing contingencies of practice, but also now against competing (ethnographic) representations existing potentially within the same public space. Was there in the angry accusation that the book 'questioned our professionalism' an implicit recognition of the truth that managers and experts are involved in the organization of interpretations as much as planned outcomes? More generally, what reading of the ethnography did these emotive encounters reveal? And what could they add to my understanding of the culture of aid projects? Let me take up some key points.

First, my colleagues did not read my ethnography as an exploration of a general theme (perhaps a theory of policy) through the particular. Of course, this was a story about them. Theory and citation became a duplicitous hiding behind others. Second, my colleagues did not share the ethnography's interpretist view of project reality as a multiplicity of truth composed from different points of view. It would be read as a singular statement about the project *as it is*, taken as objective (the team leader added) because written by someone from a world-renowned institution. From their positivist perspective, talk of alternative points of view simply dealt in the currency of 'spurious facts' or 'biased interpretations' which, as I was told, 'fail to meet the normal standards of social science research'.

Third, the ethnography was read as an evaluation. My colleagues felt judged. They did not, as others had, read a description that exonerated their struggle with the real contradictions of development, but a commentary on the gap between the actual and the ideal; a judgement against norms or best practice that critically assessed their professional competence, and dismissed their effort and enthusiasm.

But the book was unfair not just because it was evaluation, but because it was *bad* evaluation. For one thing, the ethnographic genre, unclothed in the official etiquette of praise and indirect comment, appeared unacceptably critical. For another, it did not judge the project in its own terms, but added complexity, clouded issues, and introduced diversions and irrelevant details. At a meeting, 'one of the objectors referred to the book as a field of mixed crops when everything is sown higgledy-piggledy – "it's all a confusing muddle" '.[11] Moreover, while judging performance as falling short, I offered no scale. 'Show us the model of true participation'; 'what did we do wrong?' But most important, the book was bad evaluation because it had not involved the usual negotiation between evaluators and project actors over *who* is qualified to construct knowledge about a project, *how* it to be done (methodology), and *what* is to be said. The ethnography failed to be what all good evaluations are, namely an acceptable story that mediates interpretative differences in order to sustain relationships and the flow of resources (Phillips & Edwards 2000).[12] Ultimately it was from this expectation of a shared definition of the truth about the project that the objectors took their right to propose changes to the text.[13]

The next point is that the ethnography was read from a managerial perspective. It was unnecessary and embarrassing because it refused to explain outcomes in terms of design, and evaded the expectation that problems should really be analysed only in relation to solutions. It did not provide a proper project history of implementation, learning, and improvement, which should reveal a progressive narrowing of the gap between intention, action, and outcomes.[14] I had not explained 'the steps taken', 'how the project [had] responded to particular events and problems'. In my book, difficulties and contradictions were not, as they should be, dissolved by the unfolding project, which makes history a dustbin of irrelevant errors and solved problems. It interrupted a managerial view that accelerates history so that the aspirations of the present constantly erase the experiences of the past, where it is always 'too early' to judge the success of new technology, whose disappointments are contingent (drought or monsoon excess), whose latest results are always the most promising, and whose full advantage (upon which justifying economic analysis is based) lies in the future. The ethnography was not just 'out of date', dealing in moribund problems and ignoring the unfolding success, it was also *out of time*, in Hastrup's sense of giving attention to the routine and ordinary, the out of sight, so avoiding the normal historical narrative of temporal causation which accounts for events in terms of 'the most recent and most extraordinary precedent' – that is, the project and its technical innovations (2004: 462).

Next, there was a problem with the ethnographic treatment of my colleagues' data. On the one hand, the ethnographic account denied interpretative power to scientific data: for example, to data on the genetic and generalized advantages of improved seed technology, derived from context-free models, which were unravelled in the relational world of debt-bound tribal livelihoods. On the other hand – and this is something which recalls a division in the earlier years of our own discipline recorded in Edwin Ardener's 1971 lecture in this series – in my analysis, project success was anyway not just a matter of the measurement of achievement and empirical or statistical evidence (of yield increases, trees planted, functioning groups). Success was a matter of *definition*, a question of meaning, of sustaining a particular interpretation of events through the categorizations and causal connections established by the policy model. And this model was not itself empirically falsifiable. But it *was* replaceable. So when DFID policy changed in 1997, the project became, *by definition*, a failure (Mosse 2005*a*: 184-204). The socio-economic studies we undertook then to 'demonstrate impact' were in fact orientated to re-model tribal livelihoods so as to show *how* the project, re-aligned to a new policy framework, would improve them. The research served more to clarify and justify a new development model than to demonstrate its effects. After all it was on this that project survival urgently depended. Showing the need for further action is always politically more important than demonstrating results (Quarles van Ufford 1988: 25). So, the ethnography dismissed empirical evidence and implied that the project was a self-verifying system in a way that was considered damaging to scientific reputations.

A further point is that the objections to the book reveal a particular representation of agency in the project world. The multiplicity of actors and intentions is concealed as agency becomes deflected as the property of ideas or expertise, design, technique, good policy, or the problem-solving project. Project workers (including consultants) hide their own contingent actions and the wider politics of aid so as to, as Timothy Mitchell puts it, 'allow reason to rule, and allow history to be arranged as the unfolding of a locationless [policy] logic' to which expertise is attached (2002: 15). Managers insisted upon precisely the kind of essentialized abstractions and transcendent agents that anthropologists have come to berate themselves for constructing (Hobart 1996: 7). But while success sustains the project as a unified source of intention and power (successful projects are well designed), failure fragments into the dynamics of blame (Latour 1996: 76). As a project worker put it to me, 'We always appreciate *our* successes, but failure is always seen as the failure of an individual'.

An ethnography which draws attention to diverse intentions and motivations by this detail involves unethical disclosure; and even where (as with mine) it eschews the individual personality, action, or event – in order to reflect on systematic effects or outcomes beyond intention – even though it distributes agency, it becomes a source of personal offence. My colleagues unpacked structure into *their* agency, claiming personal damage to professional reputations. Now, the tactical need to discredit the account in terms that would register as defamation or a contravention of research ethics (harm to informants) had some part in this, but it seems to me also to derive from this paradoxical way in which agency is framed in project arenas – the collectivizing of success and the individualizing of failure[15] – when confronting an ethnography that by contrast aims for a symmetrical treatment of 'success' and 'failure'.

As I mentioned, my colleagues insisted that 'objective' truth has to be collectively defined; to become 'facts', interpretations had to be subject to group appraisal and agreement. An ethnographic approach which interviewed people individually or in groups and then collated and compared diverse opinions, events, and experience in an independent interpretative analysis simply did not qualify as proper social science. I was even reprimanded (in a meeting) by one junior manager for using unreliable private conversations instead of statements made in public about events, on the grounds that informally people will invent stories, confuse, and conceal, but publicly they will speak the truth. Team discussion would compensate for the failures of individual self-censoring, and this, of course, is why it was not a good means to research such complex and contested social processes.

But there is a broader point here, namely that for these project actors social research has to preserve and honour its social context. At one level this means simply that there is an ethical obligation to those who helped you and 'gave their time and materials'. At another, it is an epistemological position that implies that the limits of what can be known, revealed, or written about are determined by social relationships. Perceived harm, risk of damage to reputations, or embarrassment to institutions[16] invalidates the analysis. 'Fairness' in research is a question of respect (and unfairness, disrespect) rather than verifiability. Research data and analysis are 'correct' (and mine was incorrect) in the normative sense of socially appropriate (as in 'correct behaviour') as well as factual: 'I am sorry, but we are not talking about simple factual errors ... what we are talking about is incorrect statements about events or decisions made on the project ... we are very disturbed by your draft'. Concerned with 'correctness' rather than 'fact', the objections were epistemological not ontological. Indeed, my colleagues' positivism concealed an essentially relational epistemology which rejected the notion of 'evidence' as external to the situation.

As a final point, they felt that the moral nature of their actions should shape the way they were described. It was wrong to appear to criticize those who work selflessly, enduring hardship for the poor. Moreover, it was construed as irresponsible to question participatory methodologies (e.g. in crop research) lest the state take against them. The project's *strategic* truth had to be preserved.

Now I see a triple significance to these objections. First, the reaction to my ethnography reveals, indeed elaborates, the same framework of expectations that organized the project system, further clarifying its conceptions of time, agency, or evidence. Second, the objections were not about what was to be known but about *how* knowledge was to be arrived at. They revealed an apparent divergence of epistemology between an ethnographer and his interlocutors – or rather one dominant section of them – that suggests limits to any collaborative ethnography.[17] Third, and more importantly, the process of 'objection' showed how ethnographic writing threatened the project as an 'epistemic community' – a set of relationships around shared meanings – drawing anger from those dominant figures whose prestige was most closely tied to authorized representations.

Let me consider the latter point further. The official-textual view of the aid project as an explicit system of rules and procedures (regularized in brochures and training manuals), a scientific order, a replicable model, and a history of significant actions/ events had a certain *necessity*. It was necessary (a) given the high degree of uncertainty in international development involving people who do not know or understand each other; (b) in order to put back together the worldview of project staff that was constantly fragmented by the everyday contradictions of practice; and (c) because actors invested in these objectifications, through which systems of expertise, status, esteem, and reward operated. Official views and habitual objectifications were (to different degrees) part of people's assertions of power, self-definition, and representation to each other *and* to outsiders – government, donor, other experts and researchers – who would read *this* version of project reality (cf. Geertz 1999: 53).

Such self-objectification as structure, rule, or replicable model is not unlike the native offerings re-inscribed by anthropologists and so keenly criticized by Bourdieu (among others) in his call to penetrate the strategies of practice, the temporality and indeterminacy of social life (1977; Jenkins 1994: 443). But what 'objection' reveals is the social (and emotional) effects of such acts of ethnographic description that pull apart socially constitutive knowledge,[18] particularly when they take similar (here, textual) form and potentially exist within the same public space. We may not realize it, but our analyses can be experienced as profoundly disempowering; they may provoke claims of serious 'damage to professional reputations'.

Then, there is the general problem that knowledge born of inter-subjective experience, when re-contextualized for a different audience within a broad analytical schema (Descola 2005), can produce a disconcerting misrecognition among those who shared the experience. 'In David's research we, his colleagues, have become objects of study', they complained. As Hastrup puts it, our ethnographic work involves objectification, as the gradual transformation of fieldwork relations into object-knowledge apart from relations (2004: 456).

My writing, then, ruptured relations and broke the rules of fair play within a professional team of which I was a member. Undoubtedly this is partly what lay behind the strong expressions from project managers and consultants who wrote (and at our last meeting spoke) of the loss of trust, of being hurt by a valued friend and respected colleague of long association. Of course, I did not intend this – it upset me too; but perhaps it is inevitable to what anthropologists do. Like others, in producing the ethnographic account I refused the roles allocated to me, 'cut the networks' of fieldwork (Strathern 1996).[19] I had to disembed myself, erect boundaries, or put distance between myself and the social worlds I described such that the academic individual was seen to deny the moral person of fieldwork.[20] Moreover, I now made inferences which my subjects could not share while holding their own; based on 'evidence' which their schemes of understanding would never generate (Hastrup 2004: 463). I made connections from aspects of experience not captured in current categories (Hastrup 2004: 469) that were damaging. Meanwhile the disagreement that I had brought within the community itself weakened the 'hardness' of project facts (cf. Rorty 1991). And the further my ethnographic representations travelled from arenas of social negotiation – from the consultant report, the conference paper, the journal article, to the book – and the wider and more public their consumption, the more strands were broken and the greater the anger and anxiety.[21]

Unfolding controversy

Let me now turn from the question of what my colleagues objected to, to that of how they did so between January and May 2004. My principal observation is that my critics reacted not by engaging with my text, but by challenging the boundaries that my ethnographic writing introduced. They refused a *textually* mediated process in favour of a *socially* mediated one that would, in some way, re-embed the production of representations (research outputs) into the fields of power, the moral community, or 'the family' of the project.

This meant, firstly, that for over fifteen months my colleagues failed or refused to send written comments on the text. Many deadlines passed. They could not give written comments, they said, 'because we disagree so fundamentally with your version of events and the conclusions you draw ... it would take weeks and [would] not be an effective way of communicating'. In fact the text itself was dismissed as an independent object – '250 pages of difficult academic writing' that development professionals and practitioners cannot find time to read, noted the team leader, adding that 'it is only by me writing to "X" [a manager] and asking him to read specific paragraphs that I got him to respond'. Nor were they interested in making a substantive response, putting on record reactions and alternative points of view; something which – fully accepting their 'right to reply' – I offered to do by means of a postscript, or opening a web-site.

What my colleagues repeatedly demanded was that we meet *as a team* to discuss my draft and how it should be re-written, section by section over a period of three or so days, and that, before we meet, I declare my preparedness to make changes. As the dispute unfolded, they were careful to say that they did not insist on particular changes, but that they were confident that I would be persuaded by their point of view and would 'want to re-write many sections of the book'. What they insisted on was not textual change *per se*, but the social-emotional process – a persuasive team workshop – that would produce such change through my re-inclusion in the moral community and history of the project. Theirs was a moral critique of, and practical challenge to, ethnographic exit. Only through being socially re-embedded could the text become 'fair and balanced', and the project as an 'interpretive community' be re-constituted; if not by appeal to friendship, loyalty, or obligation, then by implicit threat (damage and defamation). Correspondingly, for my manager-critics to concede to a socially disembedded academic process (that disallowed moral pressure over the author) – through textual engagement, written responses, and the debating of alternative points of view – would be profoundly disempowering.[22] And I could not have re-subjected myself to the rules and power of field relationships, or dismantled the boundary between the dynamics of project life and writing, without abandoning my ethnographic project or the integrity of my analysis.

So, I resisted the fallacy that the social has to be analysed socially and that evidence is a matter of consensus (at least in this context), and did not accept that after one-and-a-half years the objectors had had insufficient opportunity to share, discuss, and respond to my draft. It was when I made it clear that I was unwilling to subject my account to the adjudication of a select group of informants (or to meet and suspend publication with that objective) that they protested to my publisher and academic managers, now adding that the team considered that my study breached the ASA Ethical Guidelines on at least four points concerning both the *basis* (negotiated consent) and the *outcome* (harmful effects) of the research.[23]

But this remained a second-order negotiation: not about facts or interpretations, but about the terms of exchange – textual or social, about whose rules would apply. My senior academic colleagues who were approached were mystified by the objectors' intensely personal efforts (through their leader) – by the repeated phone calls, emails, offers to travel to London personally to show them sections of the book that disturbed, to fly team members from India – and yet their persistent refusal, as one put it, to 'comply with our request that you supply a written list of what you assure us are substantial objections regarding the text'. He continued: 'You have referred to these objections repeatedly in written communication. Yet you remain unable to specify to those ... you have involved in the procedure (pro-Director, Ethics Committee, Departmental Convenors, not to mention the ASA) the precise substance of your objections. This is a most serious matter'.

But, of course, the objectors' appeal to academic authorities was not intended to open up the text or concede to conventions of scholarship, but rather to augment the social persuasions of the 'moral community' by bringing a disciplinary power to bear on me, the author. The appeal was for adjudicators of disagreement in an editorial process that should be collective. But to expect senior professors to play roles which erase the boundaries that preserve academic independence rather than defend them was a misjudgement. Which is not to say that there was no equivocation, for example, on whether the university had a primary 'duty [to] respond to [an] accusation that a publication will seriously damage third parties ... or to support academic freedom' (senior academic, internal correspondence on the case). However, those to whom my critics appealed – including the ASA Committee that met in March 2004 – concluded that they had no remit to adjudicate, or act as a court, for what were only ethical *guidelines*. And there was a private view that neither the basis nor the outcome of my research contravened them. Consent to research had been given, and as a senior colleague put it, 'the absence of flattery is not harm'.

The university authorities did agree that there should be a one-day meeting at which the university, the ASA, and the two parties would meet, but that this would not be 'any kind of court of arbitration', that it did not have to resolve differences, that it could not impose on me any obligation to make particular changes to the book. They insisted that this meeting would take place only on the basis of a full and detailed list of errors and objections provided by the book's critics – showing evidence of inaccuracy or unargued bias that would support 'allegations [made] against the professional standing of a [an academic] colleague'.[24] Academic rules of procedure had been enforced and with them a quite different sense of 'open and fair' which re-constituted the boundary between social life and ethnographic analysis (field and desk). The meeting did go ahead on that basis, and it *was* preceded by a detailed list of comments. The shift to formal academic process was irreversible. 'Very sorry it has come to this', the team leader said to me on the day. In truth, he was.

The written comments for the meeting were in their way extraordinary. They filled fifty-six pages, and were categorized into a scale of 1 to 4 in decreasing seriousness, starting with statements regarded as 'defamatory or potentially damaging to professional reputations of identifiable individuals', most of which concerned allusion in the text to motives, compromises, or conflicts that departed from official representations of roles and processes. While the language of defamation was sufficiently serious for me to take legal advice, and while the objections had been helpful in indicating alterative points of view or correcting certain factual errors, the more I examined these comments, the less they seemed to be matters of substance (evidence or argument) and the more they invoked the moral community. That is, their concerns were matters not of ontology but of relational epistemology. The personal and accusatory tone was unmistakable. They were about a person not a text: 'David should know this ... David, quite frankly this comment is not worthy of you ...', etc. One independent participant in the dispute commented, after the event, 'They had one, big fundamental objection [an epistemological one] but they did not know how to write it so they provided fifty-six pages of minor objections in its place'.[25]

Certainly, assertions of the moral community, anger at the breach of its codes, the hurt of being judged, the bafflement of diverging epistemologies, were palpable in the highly charged day-long 'ritual of objection' chaired on 2 April by a former DFID Chief Adviser, an anthropologist well placed to look both ways in the divide between academics and development managers. She allocated a balance of time for the complainants (now including four who had flown in from India), for myself, and for independent comment from representatives of the university and the ASA, among others. Since the format – presentation and response – was academic and did not require or allow a drive to resolution, it restrained the social control of an anthropological text. I listened carefully to the objections raised, responded, and undertook to review my text in the light of the proceedings. In the end, I did not change my analysis, although I clarified its purpose, and modified phrasings that offended, where *I* judged this appropriate. Thus concluded an improvised procedure that gave a green signal for the publication of the ethnography. It did so ultimately through re-affirmation of the Malinowskian boundary between field and desk. Those who travelled to London expecting that moral and persuasive pressure would result in substantive changes to my ethnographic text were deeply disappointed.

Implications

For me, this was a significant series of events raising important questions for ethnographic practice and the status of anthropological knowledge, and I would like finally to come to some of these.

The first concerns the truth of an ethnographic account. The re-working of field relations and the contest over boundaries that I believe this dispute to have been about were none the less expressed in terms of a challenge to the veracity of my evidence and interpretation. And there is no question that 'objection' disturbed the analysis. Questions were raised in my own mind: had I got this or that wrong? Another round of research and reflection began, taking me back from the text to the interview tapes, my field notes, the source materials, the studies, and the shared experiences that underpinned it, to verify and clarify. I emailed and phoned those who I had worked with on the project and whom I had interviewed. An analysis that exists within a field of objections has to be sure of itself. Moreover, to borrow Albert Hirschman's (1970) terms, if anthropologists of development are going to turn ethnographic *exit* from the self-deceiving *loyalties* of policy and projects into *voice* in public, they have to be able to defend their accounts.[26]

But how was I to defend the 'rightness' of my ethnography against those who could say, 'you are wrong, I was there', or 'what evidence do you have to back this statement?' or even, 'come on, we know you!' On the one hand, I did not feel it was sufficient to say, 'I was there too, and this is my subjective interpretation; take it or leave it'. The matter of factual accuracy remained. Surely, as Malinowski taught us, it was necessary to allow some separation of facts from interpretation, to be explicit about the actual work of inquiry and the material and experiences upon which my generalizations were based; to unpick that seamless Geertzian interpretative web spun from field notes to ethnography (Spencer 1989: 150). But on the other hand, should I be drawn into defending this or that statement with reference to this or that specific piece of supporting evidence (event or conversation) *on its own*? It is not just that this might compromise the confidentiality of informants – including those in the meeting-room contradicting themselves – but that, as in any ethnography, the case and discursive material was illustrative, chosen for its succinctness, but drawn from a long interactive experience over years – the many encounters, events, memories, notes, reports, conversations that make up fieldwork. After all, this is where ethnography differs from investigative journalism.

In any case, as Kirsten Hastrup points out, anthropologists can never actually prove the rightness of their generalizations with reference to evidence or experience ('as an independent measure of validity'), since these are neither separate from, nor prior to, the anthropologist's own frame of interpretation, the pre-existing scheme of objectification that transforms facts into 'evidence' or imputes causation (2004: 456, 461).[27] At the very least, anthropologists need to examine the social basis of their own 'evidence-making'. They need to examine their own 'point of view' – their personal and academic predilections, judgements, and aesthetics – as the product of social conditions (and professional location), something that Bourdieu (2003) referred to as 'participant objectification'.

Since it cannot prove through evidence (the more so in relation to institutions which in-build deniability), what ethnography aims at, Hastrup suggests, is 'a kind of *explanation* beyond the truth of events themselves'; it 'is not simply knowledge about particular events, practices and ideas, but about the processes by which these come to appear meaningful, perhaps inevitable or mandatory, possibly contestable or even mad' (2004: 468). The kind of connections I made between individual statements, actions, events, and larger schemes of policy in a development project (and so the kind of explanations I offered) came, moreover, from my being implicated in its processes (Hastrup 2004: 466). As an anthropologist I do not have knowledge or experience of 'culture', but experience contingent events like everyone else and make sense of them (Hastrup 200: 468). 'Rightness', Hastrup insists, is an epistemological awareness not an ontological certainty (2004: 466).[28]

Others, too, have concluded that ultimately fieldwork is a kind of social apprenticeship, often beyond language, through which anthropologists negotiate the opacity of social life – its mutual interpretations and concealments – along with everyone else (Bloch 1991; Jenkins 1994: 441). For this reason, there is no neutral or uninvolved knowledge ('an understanding that everyone might share' [Jenkins 1994: 443]), no sharp divide between anthropologist and subject, fieldwork and the processes of everyday social life. Insider ethnography is only a case of the general situation. It is a matter of Latour's 'relativist sociology' in which I am a project actor along with others; my policies and points of view stand with theirs; as does my analysis (1996: 199). My colleagues and associates are also sociologists offering theory, explanations, trying to stabilize the project world from their varied points of view. But then, potentially, there are as many stories and authors as actors.

Gupta and Ferguson only re-state the issue when they comment, sensibly enough, that the interpretative account that is 'anthropological' always 'coexists with other forms of knowledge', and see 'the political task not as "sharing" knowledge with those who lack it, but as forging *links* between different knowledges that are possible from different locations' (1997: 39, my emphasis). But it is precisely the nature of these 'links' that is at issue, especially where the knowledge in question is that of the ethnographer's subjects. My ethnographic account does not just stand alongside or compete equally with other or preceding ones, it attempts to encompass them in the guise of subject matter.[29] My narrative adds interpretations to those of actors whose experience I share; it explicates *different* points of view, it tries to become the meta-narrative; and it is the one whose divergent evidence and inferences will be challenged as public *mis*representation, not least because the interpretations that ethnography adds come from reflection on the experience of dislocation and alterity.[30]

If the autonomous production of an 'understanding' or representation of others has lost meaning as a goal for ethnography, then perhaps it can partly be re-instated through efforts that risk placing ethnographies back within the field of relations that they describe. As Latour puts it, what the social sciences can do is to *re*-present the social to itself: 'That is, not to define the unknown structure of our actions ... [but to be] able to modify the representation the public has of itself *fast enough* so that we can be sure that the greatest number of *objections* have been made to this representation' (2000: 120, emphasis in original).[31]

This view may be suited to an anthropology that wants to sustain claims to rightness in public without either assuming epistemological privilege or retreating into 'narcissistic reflexivity' (Bourdieu 2003: 281). The process of objection – an emotion-filled relationship – reminds us that ethnographic knowledge is at root a social phenomenon, and persistently so (Hastrup 2004: 456); and that anthropological 'right-ness' – in the sense both of veracity and of *entitlement* to represent – is also social. Rightness is not a matter of the ontological status of our evidence (and this my critics grasped), and not just an individual epistemological awareness, but the outcome of social contests over boundaries and the location of knowing. And this suggests that ethnography may sometimes require institutional experiments beyond fieldwork, bringing ethnographers and their subjects together around its written outputs, experiments in objection, and the defence of ethnographic rightness such as the one I have described. Of course these exchanges cannot seek resolution. After all, following two high-profile UK government inquiries in 2004 it was clear that weapons inspector David Kelly and BBC reporter Andrew Gilligan together had had a kind of ethnographic insight or inference beyond the 'truth of events' which was simultaneously entirely right and fully deniable.[32]

To conclude: contests 'after the field' may be crucial to the recovery of anthropological claims, not least because they do not reveal any stable constellation of power in social research. Anthropologists have the power to represent; and their informants have different capacities to object. 'Objections' challenge anthropological authority, and should be welcomed for disallowing analytic closure, for sharpening our historical sense by refusing the ethnographic present, and making us clarify our generalizations (although, as I have shown, in their own structure, objections may also confirm the essentials of our interpretations – 'anything you say may be taken as evidence ...'!). Objections also remind us that however we may try to convince people that we are right, ultimately ethnographers have to concede that what we constitute as evidence is not separable from our relationship with our informants (Hastrup 2003). Conflicts arise when, as Hastrup puts it, *knowing* – 'a subtle (epistemological) relationship between subject and object', becomes *knowledge* – 'a (near-ontological) certainty' (2003), separate and in public. As ethnographic inquiry proceeds, relationships become 'evidence', which 'complicates the use of evidence as an independent measure of validity' (2003). At the same time, ethnographic representations have the potential to unravel when our informants (as did mine) attempt to unpack our 'evidence' back into relationships with them.

When anthropologists resist this, contests may unfold in which it becomes clear that anthropological writing is not, after all, an individual effort. Ethnography's objectifications (or its style of reasoning) are themselves (no less than policy models) stabilized socially and depend upon authoritative supporters, specific institutional discourses, and (as I discovered) processes of endorsement. We should not forget that in the end anthropological knowledge is a 'social achievement' (Crick 1982: 20, in Hastrup 2004: 456), one that, as Bourdieu's notion of 'participant objectification' suggests, can be subject to the same sociological re-configuration as policy discourse, and which reveals similar contradictions between individual practice and the professional models (including Malinowskian fieldwork) that reproduce the discipline (Grimshaw & Hart 1995: 59).

Perhaps the outcome could have been other, but in this case academic discourse itself demonstrated considerable power, even against quite determined objectors. When 'studying up', we may regard those who try to censure independent research as powerful – we may imagine that we 'speak truth to power'. But the outrage at rupture can just as easily reveal the fragile hold that those who appear to be in power – in political, administrative, or policy systems – have over their legitimizing representations (or the enormous effort needed to sustain them).

It is this threat to authorized representations that makes the study of public policy and institutions so challenging, especially as insiders. On the one hand, at the extreme, defamation proceedings might rule out the possibility of such ethnography. On the other, where harm is perceived to have been done, anthropologists have to engage with that perception. Either way, anthropology does not have the option (moral or epistemological) of a devotion to science that disregards social relations that are the basis of its knowledge.[33] The right to academic knowledge has to be negotiated among other legitimate claims. And the negotiation of ethnography as a 'situated intervention' rather than a disinterested observation (Gupta & Ferguson 1997: 38) requires that its practitioners are clear on their position, perspective, and purpose.

Perhaps above all my case shows that the power of ethnography lies in the fact that not only anthropologists but also their texts are active agents in the worlds they describe, enlivening action in particular ways. Thus the social mobilization of objections to my book is as integral to project action as other events it described. And when writing becomes as much a part of our engagement with our subjects as fieldwork, we may be forced to address the Malinowskian division that has allowed an individualist free-flow rulelessness to writing in contrast to the 'rule-governed expectations of fieldwork', to borrow a useful distinction from Parkin (2000*b*: 260). Collaborative ethnography is not a solution. But maybe anthropologists should at least anticipate the continuation of fieldwork relations into writing and publication, and the ruptures that may arise. Can we really blame our informants for their misunderstanding of our intellectual goals? Can we prepare informants for ethnographic outputs? Does the ethnographic account have to be in the nature of an 'ambush' on social life? These are not issues that current ethical guidelines, framed on the basis of a narrower conception of power relations in research, are well placed to clarify.

Inviting objections will not erase the questions of power that surround research, but it may at least bring an ethnographic awareness to our writing. Writing itself can then be viewed within an anthropological frame that accounts for the relationship between ideas and social relations, and as party to transactions between moral persons, in my case within the ambiguous gift-world of an aid project.

Perhaps ethnographers of policy, professionalism, or international development – domains in *opposition* to which Malinowski defined the ethnographic field – may have a key role in re-examining the methodological and institutional foundations of social anthropology, a task that was at the centre of Malinowki's own work.

NOTES

This a slightly modified version of the lecture delivered on 2 June 2005 at the London School of Economics and Political Science. I am grateful to the LSE Department of Anthropology for the invitation to give that lecture. Thanks for helpful comments and suggestions are due to Rosalind Eyben, Richard Fardon, and Ingie Hovland, to those who listened to a preliminary version given in Edinburgh, and above all to my 'objectors' for their engagement with my ethnographic writing. I am also grateful to Glenn Bowman and to my anonymous reviewers for helpful suggestions in finalizing the article.

[1] Writing, Hobart suggests, is 'the antithesis of dialogue, which academics reattach to textualizing' (1996: 29 n. 25).

[2] As Ingie Hovland reminded me (pers. comm., 5 April 2005); see also Brettell (1993*a*).

[3] The more celebrated cases of negative reactions to anthropologists' work from those who have been the objects of study, include outrage among the Ik when they were informed of the content of Turnbull's derogatory *The mountain people* (1972; Heine 1985); the community and press reaction to Vidich and Bensman's ethnography of a village in upstate New York (*Small town in mass society*, 1958); the public controversy in Mexico following the Spanish translation of Lewis's *Children of Sánchez* (1961); or the upset from the people of Ballybran (Ireland) described in Scheper-Hughes's *Saints, scholars and schizophrenics* (1982). Brettell's collection *When they read what we write* (1993*a*) provides an overview of these cases and brings together a range of more recent experiences in ethnographic writing. In what Brettell describes as a 'revolution in readership' (1993*b*: 3), informants confronting public representations of their lives and words accuse anthropologists of betrayal and broken confidences, public shame, and damage to reputations or self-images, of violations of confidentiality or of the etiquette of gossip. They take exception to too little or too much anonymity, to the fragmentation of lives in text, and they are disturbed by the idiom and

terminology of social science, in which they do not recognize themselves. Fuelled more by accounts in the press and rumours of 'the book' than by actually reading it, they respond by rejecting the anthropologist as an outsider/stranger who can never understand (Brettell 1993*a*).

[4] Relational in the sense both that knowledge is collaborative, dialogical, gained by way of relations; and that (in consequence) the relationships between researcher and object of inquiry become a property of the object itself (Hastrup 2004: 457).

[5] There is a parallel here with situations in which ethnographic subjects have strongly reified cultural self-representations – e.g. cultural or ethnic nationalist groups – where anthropological re-interpertation may even carry mortal risk (Whitaker 1996: 5).

[6] Imagine, for example, a visit by the official DFID monitoring 'mission' when interpretative possibilities are constrained through the organization of space and time in visited villages so as to resemble the policy text read by outsiders; the co-existence of completed PRA (Participatory Rural Appraisal) maps on the ground, the smart treated landscape, new woodlots, and colourful groups of women provide the simultaneous presence of the village plan and its execution – contingency and time are suppressed; individuals and events subordinated to policy ideas.

[7] A distinction borrowed from Baudrillard (1988: 209) via Hobart, who explains the work of development organizations as 'asserting determinate representations' (1995: 6, 10).

[8] And this of course is no less true of British domestic policy. As his former chief speech-writer notes, 'Blair promises to deliver on things he has little power over: exam results, crime levels, cancer mortality rates' (Hyman 2005: 380).

[9] Expressing an individual rather than an institutional view.

[10] Although (I warned), such assertions might become self-fulfilling by giving the damaging impression that the project was a weak organization able to survive and attract funds only on the basis of fragile representations.

[11] As recalled by Rosalind Eyben (pers. comm., June 2004).

[12] As I note in the book itself, 'In spite of formal demands for objectivity and independence, experts are charged with producing, and themselves intentionally construct, the evaluation story as a "shared commodity" (Phillips & Edwards 2000: 57)' (Mosse 2005*a*: 158).

[13] A point made by Rosalind Eyben (pers. comm., June 2004).

[14] A proper project history would begin with policy intention and design and explain how this was implemented. My account reversed this, starting with events and relationships before turning to rationalizing representations.

[15] One can also say that while narratives of success emphasizing expert ideas are theory-rich, those of failure are by contrast 'event-rich' (Mosse 2006).

[16] The ambiguous concept of 'embarrassment' (to DFID or its partners) sets the criteria for refusing use of data from consultancy in DFID contracts.

[17] Presumptions about the possibility of consensual narratives, or the unity of native voice (Lassiter 2005), suggest that critical anthropology could learn from the critiques of participation in development (e.g. Cook & Kothari 2001).

[18] Making social maps from gaps and spaces, 'listening for the unsaid' rather than the guarded statement (Dresch 2000: 123).

[19] This is to say that ethnography itself (of a certain kind) is among other mechanisms – Strathern discusses proprietorship and certain kinship arrangements – that cut the self-enlarging social networks that actor network theorists describe; and that it can do so not just conceptually, but also socially.

[20] This is not to say that writing need always have this effect or that it cannot in some instances work as a bridge (Ingie Hovland, pers. comm., 5 April 2005); indeed some of my relationships from 'the field' were strengthened through this writing.

[21] Comments that were 'incorrect' in the book were previously acceptable in my consultancy reports, which remained internal and had a restricted readership. It is significant also that I (or my colleagues) could publish on crops or 'tribal' communities without offence, since these realms were distant from the social world of my critics and subject to a 'technical discourse'.

[22] It is for this reason that dialogical or 'reciprocal' ethnography, or the inclusion of (negative) native responses (Lassiter 2005), ultimately fails to address questions of power in ethnography.

[23] The latter revolved around the 'serious concerns' that the book 'will harm professional reputations', especially of Indian mangers, uncomfortable with academic prose and ill equipped to defend themselves, who had not been given adequate chance to reply, and who, while not named, could be identified.

[24] Correspondence from the university to 'the objectors', March 2004.

[25] Rosalind Eyben (pers comm., June 2004).

[26] In this controversy (and in the book itself), when defending an interpretation that I claimed to be true, I revealed a representationalist view of truth (that mind or language allows for the representation of reality, and that some representations are more accurate than others). I insisted upon evidence independent of relationships and resisted assertions that truth was a matter of agreement and consensus. An anonymous reviewer cogently summed up the irony 'in the stand-off between Mosse and his objectors: their positivist ontology was protected by a relational epistemology; his relationalist ontology is defended by recourse to what is ultimately (at second order) a positivist theory of truth.' (*JRAI* Reviewer comments, September 2005).

[27] Ethnographic comprehension is necessarily deductive (Descola 2005: 67).

[28] Anthropological understanding, Descola notes, comes from confronting acts/utterances with our own responses to the same circumstances, and from identification with the motivations that may lie behind the actions of others, rather than with 'the culturally codified responses that these motives generate' (2005: 70). Similarly, Bourdieu comments on the importance of anthropologists using their own native experience in order to understand and analyse other people's (2003: 287).

[29] I am grateful to Richard Fardon for clarifying this point (pers. comm., 15 October 2004).

[30] In the case of insider ethnography, social exit rather than entry is the critical shift for defamiliarization. If we agree with Bowman (1998) that ethnographers 'abroad' cannot communicate another culture to their readers, but only the dislocation of their experience of alterity, then ethnographers 'at home' convey – to their readers and to their colleagues or co-residents – the equivalent 'conceptual distortion' of exit from familiar shared assumptions. Considered through the reader's own experiences, the fractures of exit, like the fractures of fieldwork, can be embraced as insightful or rejected as disturbing (or 'wrong').

[31] Others influenced by Wittgenstein's later philosophy point in a similar direction. Whitaker suggests treating ethnography as a set of 'pedagogic experiments', descriptive 'tries', or exercises in publicly displayed learning, judged not by how well they construct their object, but by how well they bring the parties involved into 'some kind of lucid contact' (1996: 8), and Hobart suggests reframing the problematic anthropological goal of 'understanding' in terms of mutual 'recognition' (1996: 31). Whether 'objection' or the more bloodless 'lucid contact' (or mutual recognition) is the better metaphor will depend on the degree of contestation and what is at stake.

[32] The reference being, of course, to the controversy over the representation of the threat of 'weapons of mass destruction' that justified the decision to go to war in Iraq in 2003, and eventually led to Kelly's suicide; and to the Hutton Inquiry (2004) into the circumstances surrounding the death of Dr Kelly, and the Butler Inquiry (2004), which reviewed intelligence on weapons of mass destruction.

[33] Thanks to Ingie Hovland for raising this point and some of the questions in the next paragraph in response to an earlier draft.

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Une anthropologie antisociale ? Objectivité, objection et ethnographie des politiques publiques et des communautés professionnelles

Résumé

La méthode ethnographique de Malinowski nous a laissé en héritage la séparation du « terrain » et du « bureau ». Le savoir des anthropologues est indissociable de leur relation avec ce qu'ils étudient (l'épistémologie est relationnelle), mais l'écriture ethnographique coupe les liens du travail de terrain, disperse le réseau et dresse des frontières : elle est nécessairement antisociale. Lorsque les anthropologues, dans leur étude de ce que les gens croient, disent et font (et les incohérences entre les trois), s'intéressent aux institutions interconnectées qui composent le monde moderne, à la politique et aux communautés professionnelles dont ils peuvent également être membres, il leur devient plus difficile d'aller et venir entre les mondes sociaux. Argumentant en faveur de l'importance d'une approche ethnographique des pratiques des institutions de pouvoir, l'auteur utilise des recherches récentes sur l'aide internationale et le développement pour montrer comment les informateurs influents s'opposent aux comptes-rendus ethnographiques, résistent à l'établissement des frontières anthropologiques et tentent de « détricoter » le savoir académique pour le réinsérer dans les relations.

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