

ETHNOGRAPHY AS A PRACTICE

OR THE UNIMPORTANCE OF PENGUINS.

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Mark Hobart,
Department of Anthropology and Sociology,
School of Oriental and African Studies,
University of London,
Thornhaugh Street,
Russell Square
London WC1H 0XG

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This book tells more about penguins than I am interested in knowing. [Unknown child's school essay]

We pick out a text here and there to make it serve our turn; whereas, if we take it all together, and considered what went before and what followed after, we should find it meant no such thing. [John Selden, *The scriptures. Table talk* 1689]

A problem

Ethnography, and ethnographic writing, are in a parlous state.¹ It is no longer clear how you should set about doing ethnography, writing what you experienced, or even what you should be writing about [Clifford & Marcus eds. 1986]. The image of the ethnographer as professional scientific theorist became occluded by the appreciation that what passed for ethnographic experience was a complex act of interpretation by which ethnographers textualized the welter of activities going on around them. In place of a scientific object - society - to be described, measured, analyzed and explained, there was culture to be read, appreciated, interpreted and written [Geertz 1973; 1988]. More than is often recognized, the differences between the two objects lay in part the interests of the dominant polities concerned. Europeans needed to imagine others in terms which enabled them to be administered as part of colonial régimes. Social structure was a jural notion appropriate to those whose task it was to map, control and legislate. Americans, by contrast, were concerned with developing others to share in the joys of modernization and modernity, a process which involved a quasi-religious conversion of others from a state of traditional superstition and ritual to reason and enlightenment. This required understanding the cultural values of others: what made them what they were.²

Whichever way you imagined the object of study, the difficulty remained though that the accounts were monologic. They privileged the authoritative voice of the ethnographer and silenced the polyvocal reality of social life. According to this critique, ethnographic writings should include the voices of the members of the society, so that the final ethnographic text would, in both senses, represent its various participants [Clifford 1983]. The sorts of new ethnography proposed [e.g. Crapanzano 1980; Dwyer 1982] look rather like a step backwards to extracts from the notebooks of the old scientific anthropologists with all the problems of power and knowledge swept under the (Moroccan) carpet [Fardon ed. 1990]. The idea of liberating authentic native voices to speak for themselves reinvents the originary presence and 'authorizes' the natives. As usual it is on the anthropologist's terms [Hobart 1990]. In both textual and

¹ This paper was originally written for a seminar on textuality and fieldwork entitled *Testo e terreno*, in the Dipartimento di Filosofia e Scienze Sociali, Università degli Studi di Siena. I am grateful to Professor Solinas for the original invitation and to the seminar for useful comments which I have tried to incorporate here.

² Understanding also is easily adaptable to divining what people, as consumers, want. Often, of course, this takes the form of interpreting - and so allocating - to people wishes they did not know they had. I am inclined to think that the ground gained (from the 1960s on) in British social anthropology by culture as the predominant notion is not coincidental to the belated realization of the loss of empire, nor the efflorescence of understanding as a notion with the triumph of capitalism, however disorganized.

‘dialogic’ analyses the ethnographer relates to others as subjects through an intersubjectivity, the terms of which, again, the anthropologist determines [Hobart in press [a]].

The paradox is that the more intellectually imposing the arguments and the finer the techniques for registering the actualities, the more the actions, concerns and lives of the people described tend to fade into paleness. It is not just that what is described becomes increasingly virtual. The stress is also increasingly on the knowing subject of the ethnographer, so that the people who are the objects or subjects of the account disappear, either literally or by being biographized [e.g. Abu-Lughod 1993; cf. Lindholm 1995: 809-811] and so overinterpreted and transmuted into a form quite alien to their own practices of self-description, whatever these might be. Ethnography becomes increasingly reflexive to the point that it comes close to dispensing with its object altogether.³ There is something seriously wrong here.

Quite what I think is wrong would take up at least a book. Let me deal with one strand. It is the presupposition in Euro-American academic writing, which embraces ethnography, ethnographic writing and anthropology, that representing somehow mirrors a reality, the task of experts being to establish that reality undistorted and, if possible, in its full originality. This presupposition gets in the way of wondering if reality is quite of that kind and what part the inquirer plays in determining that reality, let alone as to whether the people we work with appreciate the issue in the same way. In other words, the grip of a particular kind of timeless, situationless epistemological thinking remains the default intellectual position. The result is to create a dichotomy between epistemology and ontology, between subject and object, which runs like a fault line through much European thought.

What is so wrong with such dualism? Briefly, it is bad manners. If we are to engage seriously with others, it is an act of power to impose our categories on them before we even inquire what theirs’ are. It also arrogantly assumes the superiority of the knower to the known, in a non-mutual, non-reciprocal relationship. It recreates the world according to particular processes of mind, treated as separate from matter and bodies.⁴ By making the relationship between the Euro-American intellectual and local intellectuals asymmetrical - the latter become objects to be thought - it gets in the way of that critical appreciation and recognition of difference, which I take to be a mutual process in some sense [Collingwood 1933]. It transfers agency from people in various arrangements rethinking and reworking their lives, under conditions usually not of their choosing, onto that of the knower. The effect is to deify academics (well, it is the last chance they will get) as knowing subjects, who are invariably superior to, and detached

³ That this seems to be particularly common (but, fortunately, far from universal) among American-trained anthropologists invites more consideration than it has been given. I suspect among Africans, Asians and Latin Americans it may have something to do with a reaction against previous European objectifications of them [Said 1978]. Among those American anthropologists much given to self-centred reflexivity, I wonder whether this is not the combination of two strands among others. The first is religious practices of confession. The second relates (much as I deplore recent overuse of the term) to the United States’ period of ‘hegemony’, as part of the ‘construction’ of others in the image of self.

⁴ I think that it is easier to render a dualist system in non-dualist terms than *vice versa*, but I have yet to work this through carefully. As dualism remains the default position, even among some post-structuralists like Derrida [see Foucault 1972: 602ff.], I prefer to err towards a non-dualist analysis both because it makes a change from reinscribing a European metaphysics and because it fits better with my appreciation of the presuppositions of the people with whom I work, namely Balinese.

from, what they know. It is also hypocritical and incoherent. 'Anthropology lives by seeing and interpreting everything as culture-bound ... everything but itself' [McGrane 1989: 125]. In what follows, I wish to consider if it is possible to write about ethnography as a practice in a way which avoids both such unnecessary hierarchizing of knower and known (especially when the known are other people) and false identification by which we tame the strange, the different, by redescription, so making it merely a puzzle.

Text

Let me start with a point about which most of us might appear to agree. 'Events only *seem* to be intelligible. Actually they have no meaning without interpretation' [Sontag 1972: 655]. The first, and usual, reading (sic) is the idea that ethnography is interpretive and involves textualization as a prerequisite of interpretation. An alternative analysis might run as follows. Interpretation is a particular kind of practice which creates meaning and makes textualization appear a natural, logical, even necessary, step. The former account owes much to Clifford Geertz's [1973] reworking of Ricoeur's *The model of the text* [1971]. The latter is closer to Foucault's rethinking of Nietzsche [1990].

Textualization, on Ricoeur's and Geertz's account,

is the process through which unwritten behaviour, speech, beliefs, oral tradition or ritual, come to be marked as a corpus, a potentially meaningful ensemble separated out from an immediate discursive or performative situation. In the moment of textualization this meaningful corpus assumes a more or less stable relation to a context [Clifford 1983: 130].

The ethnographer can take these texts away for later perusal and interpretation because, unlike discourse, texts travel. In the process of separation, texts become evidence of 'a "cultural" reality', a 'generalized "author" must be invented' [e.g. the Balinese, Clifford 1983: 132]. The argument is circular. Textualization is possible because behaviour is declared to be part of a unitary and coherent corpus, and so is potentially meaningful by virtue of the analyst's constitutive activity of declaring culture to be a totality [see Fabian 1983: 156] and so - gerundively - intelligible, interpretable. The whole argument presupposes the existence of what can perhaps be best described as an abstract substance, 'meaning'. This singular entity is implicit in action, or even events. It can be revealed, rather as you use stain to differentiate organic tissue, by marking out actions as cultural and so meaningful. Or alternatively the meaning lies in the act of textualization. While the former argument tends to circularity, the latter does not necessarily, unless of course you make the slip of imagining meaning to have any existence other than by virtue of textualizing itself.

I shall not review the debates in detail here about the nature of 'text' or 'Text'.⁵ In the narrower sense, text refers to what Barthes called 'work', that 'is a fragment of

⁵ I have written, probably at quite unnecessary length, about meaning [Hobart 1982], texts [1985], and textuality and interpretation [in press b]. For an opposed point of view, apart from the sources already cited, see Boon [1982, 1990].

substance, occupying a part of the space of books' [1977: 156-57]. In the broader sense, text 'is a methodological field...*the Text is experienced only in an activity of production*' [1977: 157]. In the latter sense, it is of a higher logical order than the Ricoeurian text used by Geertz, which is itself a complex whole built out of sentences [1976: 1-23].

There are two obvious problems. First you cannot write an epistemological space. (Quite why spatializing, and so hypostatizing, knowing is so fraught is nicely elucidated by Lefebvre 1991.) Second, it conflates culture and work/text. Unless you inhabit a peculiarly recondite world, culture is not a text. Before the textually-inclined declare me a vulgar positivist, let me explain what I wish to say by this. It may be fruitful to treat culture *as if* it were a text. I doubt it though. This has not stopped many soi-disant postmodernists from making great reputations by celebrating the catachresis. It has become conventional in the last decade or so among those suffering PMT (postmodernist trendiness) cheerfully to talk about how texts have constituted people in ever more ambagious ways. Quite what being constituted by a text - be it a book, a methodological field or a condition of intelligibility - would actually involve is charmingly mind-boggling.

The problem with subsuming the whole strange eventful gamut of human actions and events across history under the soubriquet of 'Text' is that it homogenizes whatever has happened and sets up that comfortable, old and appealing dichotomy by which mind triumphs once again over the world. That it does so largely by definition, does not detract from the thrill of the familiar. Anyway, if everything is Text, the notion is vapid [cf. Baudrillard on Foucault's idea of power, 1987]. It becomes an abstract substance, empowered with amazing, if largely imaginary, qualities. In short, it becomes a Transcendental Agent, beyond history, with hermeneuts and deconstructionists as its immanent intelligence to tell us what It is up to.

How does this process actually work? As Clifford remarked:

By representing the Nuer, the Trobrianders, or the Balinese as whole subjects, sources of a meaningful intention, the ethnographer transforms the research situation's ambiguities and diversities of meaning into an integrated portrait. But it is important to note what has dropped out of sight. The research process is separated from the texts it generates and from the fictive world they are made to call up [1983: 132].

Meaning, text and human subjects are defined each as coherent, integrated and unitary respectively, and as related. That these turn out mysteriously to be properties of those things in the world which instantiate the ideas is tautology at its finest. And Clifford's critique has its own difficulties. Note how he assumes that culture is about meaning and, if natives do not have texts, they do not have textuality.

Clifford himself regards textualization as something which gets in the way of the native voice (his idiosyncratic rendering of Bakhtin's notion of *heteroglossia*, which, on other readings, is about speech not voice at all). Clifford judges new approaches

by their ability to give everyone involved in the ethnographic project - interpreters, informants, various groups of natives, etc. - an autonomous voice in the ethnography... Clifford's fundamental assumption is that "monological ethnography" tends to ahistorical abstract understanding of indigenous societies... The underlying assumption of Clifford's argument is that every point of view must be included

untouched, because the anthropologist's version of the other voices will "distort" them' [Kohn n.d. 5, 8-9].

Citing Collingwood [1946], Kohn points out that the quality of the analysis depends on the questions asked by the analyst and on her capacity to engage in a dialectic between the events under study as appreciated by those taking part and by the analyst using the full range of contemporary argument available to her. That Kohn, writing an undergraduate essay at the University of Chicago, could dismantle the emperor of ethnographic textuality says much about the quality of textualist thinking.

What is there other than text then? After all, according to Ricoeur, action is textlike [1971; cf. Hobart 1985]. It may be useful to distinguish textuality as a frame of reference which includes what Derrida called 'inter-' or 'pre-text' (those prior and other texts which we assume to be intelligible in speaking and writing), from textualizing, which is better reserved for the particular act of inscribing in textual form.

Textualizing also, importantly, transforms. Consider how hard it is to reconstruct 'reality' from the textualists' texts. The literary pretensions of textualists have obscured the differences in degree and kind between the object that novels notionally refer to as against ethnographies. As with scientific realism, ethnographic texts reduce complex and partly underdetermined states of affairs to less complex, and more over-determined, ones [Burke 1969: 505-507]. Like flavour-enhancers in supermarket pre-prepared food, the supplement - here coherence, meaning, adjusting to readers' preconceptions - often has precious little to do with whence it came.

Textualizing is above all, these days, an academic practice. This raises the question of whether other peoples necessarily textualize to the same degree or even in the same way. In South and South East Asia, texts are often treated less as instantiations of the mind of some superior author, but as something to be worked on by readers as agents [Errington 1979; Inden 1990]. Under these circumstances, the implicit closure of academic textualizing is reversed. Texts become a possible precondition for kinds of action which are not determined by the text itself.

Academics' reverence for texts not only enshrines closure, but also a singular form of superiority. What proportion of people in Italy, England or, say, Bali spend much of their time reading texts? My guess is that it is forms of popular culture and mass media which occupy most peoples' time and attention in many parts of the world. The idea that people might escape textual closure is so horrendous that we have two related fields, cultural and media studies, the self-appointed task of which is to textualize shopping and fashion, film and television. Just consider the title of Fiske and Hartley's analysis of television: *Reading television*. Watching television and film or shopping may well involve textuality in complex ways. It does not mean that such activities are texts. By textualizing the world academics have condemned themselves to be even more irrelevant than they already are. For all anthropologists' claims to the common touch, our practices are deeply élitist and are enshrined in our originary moment - an aristocratic Pole facing the prospect of living with savages.

Culture

What however is the object which is at once textualized and the means to textualization? It is commonly culture in some form. 'In finished anthropological

writings...what we call our data are really our own constructions of other peoples' constructions of what they and their compatriots are up to' [Geertz 1973a: 9].⁶ If thinking and speaking (which involve language) are cultural,⁷ much social life consists in people thinking and talking about thinking and talking. In fact this is where anthropologists start to come in. What we deal with are a mixture of these two, mostly the latter. Our thinking is already third order. And our note-writing in the field - our textualizing if you insist - is already fourth order. Which makes our monographs at least fifth order and 'culture' a concept so 'meta-' in its removal from any possible social action that it is best not thought about at all. Texts enter a similar spiral of essentialized levels. Arguably 'culture doesn't "include" or "contain" its basic values; it *is* its basic values' McGrane [1989: 119]. (Note how such rarifying filters out confusions, conflicts, antagonisms, which never loomed large in this vision of culture anyway.) Anthropologists have on the whole been loathe to consider the consequences of the use of culture by themselves and colonial Europeans as a dividing practice (see below) which ranked human beings [Said 1978: 45]. Much the same holds for the objectifying practice which almost everyone seems to engage in these days. It is where humans become identified metonymically either with culture as what they have made (Geertz above) or invented [Wagner 1981] as craftsmen or innovators, or else which they own, possess, borrow, acquire, commoditize and enjoy in a grand flourish of the spirit of capitalism.

Culture, like text, easily leads to a vicious circle of academicizing. So let me cut the Gordian knot. Johannes Fabian remarked, perceptively, that

contrary to its popular image, cultural anthropology has been a science, not of emergence, but of disappearance. Culture, inasmuch as it served as anthropology's guiding concept, has always been an idea *post factum*, a notion oriented towards the past (to 'custom' and 'tradition'), descriptive of a state of affairs (and often a status quo), a nostalgic idea at best (when it mixed the study of exotic societies with regret) and a reactionary ideologeme at worst (when it was used optimistically to explain away as 'variation' what in many cases was the result of discrimination and violence) [1991: 91, 193].

Anthropological recourse to culture is a mortuary practice, a practice of burying the indeterminacies which keep polities going under the silt of custom, a celebration of already dated (and usually male) compromises and deals. As an overarching concept, culture is a deeply apolitical, even anti-political, idea. Anthropologists do not just study and write about - i.e. textualize - mortuary practices, anthropology itself is such a practice. It is, in Lévi-Strauss's phrase, *en clé de mort* [1966: 194]. Perhaps it is time we became a little less infatuated with the *idée fatale* of the text, or of culture.

⁶ This still leaves the question: who 'radically construes' the texts, or rather 'the constructed understanding of the constructed native's constructed point of view [Crapanzano 1986: 74]? Geertz is quite happy with the idea of fictions - in the sense of 'something made' [Geertz 1973: 15] - it is something which *has been*, not *is being* made. It is already appreciated in archaeological mode. Geertz, in *Thick description*, treats social reality as a building which has to be excavated [Hobart 1986].

⁷ What would it be to engage in any of these activities extra-culturally? The difficulty in speaking this way reveals the extent to which culture is a container, what it contains is meaning [Reddy 1979] and it must be whole. Otherwise presumably meaning would trickle out.

There is, as Fabian notes in the same article, a quite different sense of ‘culture’ as practices of thinking about and knowing about something. Moreover, whereas text (in either sense) implies a dichotomy between text, thought and interpretation on the one hand and the world on the other, knowing does not necessarily [Hobart 1993]. My concern here is with what is involved in ethnography. Few scholars have argued - yet at least - that ethnography has nothing at all to do with thinking, knowing and understanding. What I wish to put forward here is a non-dualist argument for thinking, knowing, interpreting, understanding and indeed textualizing as social practices. Both the people concerned and anthropologists engage in these practices in different ways - ones which come to overlap - as a central aspect of ethnography.

Practice

Unfortunately, for my purposes, there is no useful account of practice. Marxist accounts of *praxis* are part of a discourse of structure and ideology, singularly unsuited to my present concerns.⁸ Bourdieu’s theory of practice (an oxymoron, incidentally) is an anti-structuralist return to a form of transactionalism and again is unsuitable [Bourdieu 1977, 1990]. It owes more to an individualist and economic complementing of structure of the kind argued in anthropology by Firth and Barth than to Marxist and post-Marxist thinking. Above all it is deeply dualist: it proposes the determination of society by individual agency as against structure. Its products, *habitus*, are merely what British anthropologists used to call ‘social institutions’ (that is, ‘standardized modes of co-activity’) in a new guise, precisely the ‘custom’ which Fabian criticized. I find the work of Foucault [esp. 1972b, 1977, 1982, 1984, 1986] provides a better starting point.

What I am attempting to work out currently is an account of agency which aims to transcend the dichotomy by presuming agents not necessarily, indeed commonly not, to be individuals, but comprising complex configurations [see Hobart 1990b; Inden 1990; both following Collingwood 1992 [1942]].⁹ My account of practice presumes there to be agents who are responsible for deciding upon a course of action and who take, or are attributed with, responsibility for that action. The actions are carried out by instruments. Actions are performed upon either patients or objects, depending upon whether they are considered to be aware or effectively unaware of what is being done to them. Agents may double as instruments. So may patients, spectacularly in those forms of discipline such as the panopticon, where prisoners learn to monitor

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⁹ I understand the stress upon organizations and arrangements, from political parties, to the Church, to voluntary associations, to families, partnerships, friendships etc. to be the kind of agents about which Gramsci was writing. There is also an interesting second Italian connection, that is in the use Collingwood made of the work of Benedetto Croce.

My analysis here has been worked out over the years with Ron Inden. In fact, on this score we form a good example of a ‘complex agent’, in which other people have also played a part, notably our research students. Different people may participate in differing degrees and kinds in a complex agent. The relationship of the parts to the whole and the working of a complex agent is a convenient way of differentiating such a notion from, say, compound, composite or collective agents, the last having, of course, Durkheimian overtones.

themselves, but not as agents. Actions also have an outcome. However I also take it that evaluations of agency, action and outcome are underdetermined and always open to subsequent acts of re-presentation by subsequent agents. Following Taylor [1985], I treat consciousness as best approached as historically situated public actions, not as a mysterious, private, inner state possessed by a unitary coherent individual. Similarly I take it that knowing, thinking and even remembering are public, historical and cultural actions [see Matilal 1986 on Indian philosophical parallels, also Hobart 1990a; 1995b].

'Practice' I take to refer to certain recognized means of acting upon the world and upon humans for the purpose of producing a definite outcome. Among practices, those of particular significance deal with the making, changing or recognition of agents - complex, human, Divine - and so the unmaking of others, that is the creation of instruments and patients. Practice also involves making others, or oneself, the subject of actions. 'Actions' I take to be what agents decide upon and so to be extraordinarily varied. It seems sensible therefore to think of practices as far more encompassing than actions and, in being recognized, to be framed, whether by the analyst or by people involved. Failure to recognize the latter is not an omission but an act of negation, however careless, by an agent. By 'activities', by contrast, I have in mind congeries of actions which are less directed towards a specific goal, less formal or less explicitly recognized. Whether they are aware of it at the time or not, what anthropologists do in the field are as often activities then as they are practices.

Ethnography is not just any set of practices. It is a triply reticulated set. At once, the anthropologist is identifying, trying to work with or find out about, then study, local agents. The anthropologist is also exerting her agency - most notably after leaving the field when she 'authorizes' the people she worked with. The people and organizations she studies are usually also trying to exert their agency on her to affect the outcome. The possibility of ethnography however supposes both a difference, a tension, an antagonism [in Laclau's 1990a sense] between ethnographer and ethnographed. (The absence of the latter term, or its equivalent, from anthropological discourse speaks volumes in the silences about just how, in the end, our much-vaunted 'collaborators' and 'friends' end up.) And, far from trying to reinvent the ethnographer as hero, I am concerned with the conditions which made such determinations possible.¹⁰

Knowing, understanding, interpreting and writing, or textualizing, are not then any humdrum set of practices. They are vital ones in identifying, recognizing, imagining etc. agents and patients. In being inscribed, these accounts have a nasty habit of becoming definitive, especially when the anthropologist leaves the field or the original people concerned die (in one horrible sense, they are much the same). In short, we are close to what Foucault, partly retrospectively, elaborated as his lifelong study of 'the different modes by which, in our culture, human beings are made subjects' [1982: 208]. These were, first, 'the modes of inquiry which try to give themselves the status of sciences' such as anthropology, the objectivizing of the subject as cultural. Second, they included 'dividing practices' through which 'the subject is either divided inside himself or divided from others', young against old, males against females, the object of

¹⁰ A wonderful example in Bali is how one court, in the village of Ubud, made their vision of Bali hegemonic by 'cooperating' with, and even inviting to stay, distinguished artists like Walter Spies and scholars such as Margaret Mead and Gregory Bateson, to whose writings Clifford Geertz owes so much of his vision of Bali.

study as against the subject who does the studying. Finally there are practices by which a human being turns her- or himself into a subject [1982: 208].

Overinterpretation

Treating culture, or life itself, as a text avoids a recognition of textualizing as a cultural practice. People write, speak, read and listen; textualize events and actions in circumstances which depend on the existence of previous practices of textualizing. What I call 'the Literary Tendency', which has become the fashionable mode in anthropology, especially in the United States, is itself part of such practices. Solipsistically its practitioners hypostatize practices into abstract objects (texts) and imagine particular practices to be constitutive, essential or even universal. The sort of approach I am advocating here however treats practices as particular, historical, situated and varying in degree and kind. I assume that, far from having a determinate, extractable essence, facts are underdetermined by explanation [Quine 1953, 1960] or, put another way, that 'reality transcends the knower' [Inden 1986: 402]. On this account, any activity or practice, the agents who engage in them and the patients who are their subjects, are themselves partly a consequence of, but are not fully determined by, past practices and activities. Among practices, some rework past practices (e.g. commenting, criticizing, correcting, but also legislating or even making fun of someone);¹¹ others aim at transforming patients (e.g. graduating, curing, managing) and the agents themselves (e.g. crowning, praying, self-disciplining [cf. Foucault 1986]). Other practices are concerned with transforming reality (these include textualizing, recording, writing, measuring, televising etc. among others). Yet other practices are concerned with trying to eliminate the underdetermination of actions and events, including much academic writing and 'ritual'. I choose therefore to treat both explaining and interpreting as often practices of determination, or essentializing, in some form.

Now, if there is one practice which is distinctive of ethnography among other practices, but which is rarely recognized as such, it is what for lack of a better word I shall call overinterpreting. This is overdetermining one interpretation where alternative equally plausible interpretations are possible, or have in fact been put forward. As a practice, overinterpreting is a whole mass of practices which usually start with preinterpreting prior to any engagement with what is actually to be interpreted (i.e. before you go to the field) and concludes in defending the interpretation against criticism (i.e. often decades after everyone but you is dead).

Evidently, local people, in this instance Balinese, may well on occasion also overinterpret for all kinds of reasons. Where they differ from ethnographers is that the latter's justification for existing is that they somehow add more to what the locals are perfectly capable to saying for themselves. This something is a logical method for

¹¹ The number of activities which may go on during any one practice, such as attending a seminar, may be highly diverse. During the seminar at which I first presented this paper, I noted down the following (quite apart from the more narrowly defined academic activities usual on such occasions). They included looking at photographs, books, out of the window, at other people (no one had brought proofs to read, a British favourite), doodling; dozing, chatting; both cassette and video recording proceedings; wandering to and fro in a gallery extension, smoking; promising copies of one's work to others, avoiding certain people etc. etc.

validating probable interpretations, presumed - in a fine example of preinterpretation - to be so superior to Balinese methods that no interpreter has bothered to inquire what they are or if they even exist. Interpretation in anthropology is the trivial business of textualizing and placing our interpretations upon other peoples' practices. Instead I am concerned with analyzing how other people explained and talked about the practices in which they were engaged in terms of their own presuppositions. That is metaphysics, in Collingwood's [1940] terms.

The presuppositions behind fieldwork

What does seem distinctive, indeed constitutive, of anthropology is ethnographic fieldwork by participant-observation. But we are in deep trouble immediately. Anthropologists' investigative method depended upon a conjunction of a naturalist epistemology (facts are given, there to be collected and subsequently owned)¹² and the peculiar conditions epitomized and underwritten by colonial governments, under which the inquiring ethnographer had the right to poke her nose into other peoples' lives and write about them without let, hindrance or consideration of the consequences for those described. We are deeply scopophilic [Walkerline 1990]. (Were someone to snoop into my life as I did in my first fieldwork into Balinese villagers' lives, I would feel inclined to serve an injunction on the anthropologist to prevent her coming near my house, still less write about me without my having the right to check the published account for accuracy, defamation etc.).¹³

Participant-observation always was a rather arcane idea. It is not so much that observation belongs to naturalistic scientism. It is that the visual metaphor of knowing creates a world of relatively stable states. The image is also inadequate. When you see something in the field, you usually have to go and ask someone what it is that you have just seen. We ask questions. The problem is how to get from shifting, intensely situated polylogues and dialogues to the timeless monologues of the professionals.

¹² To tell a story against myself to make the point, like other anthropologists trained in the 1960s, I thought I was supposed to collect genealogies. That the subtle ways Balinese have of referring to relationships, might be incommensurable with the cruder, but notionally exact, ones I was taught only slowly dawned on me. It did so by a breakdown in exactitude. A matter Balinese regard as important is who agreed to the union and they give different names to, say, elopement from marriage by capture [Hobart 1991]. The trouble was the woman's family often gave a different description from the husband's, and other people in conversation would label it quite differently again. After futilely trying to sort out which was the true version, I finally realized that events do not come with neat labels attached. It was not just a matter of wanting to present your family in a good light. It became clear that people, including of course different members of the same family, appreciated what happened differently and irreducibly, and spoke of events differently according to the circumstances in which they were articulated.

¹³ A presuppositional critique of ethnographic practice might seem little *recherché* and to smack of philosophy to some of the more bluff, empiricist ethnographers. However they seem equally immune to recognizing the practical problems which indicate how far classical ethnography was the peculiar product of a particular moment in European history. Visas and permission for much research is no longer forthcoming in many countries. The conditions of funding for research students' fieldwork (upon which hump anthropologists traditionally lived for a lifetime) have drastically altered the whole enterprise. The strain is often intolerable on students who must somehow square new styles of theorizing and writing with new research conditions in a much shorter time. Yet the fantasy of the authentic ethnographic moment lives on, largely unreconsidered critically.

And as to 'participation', the less said the better. If you have seen as many anthropologists in the field as I have, or if you have ever heard people laughing over your own attempts, you need to be irredeemably narcissistic, disingenuous or plain knavish to use the expression with a straight face.

For anthropologists to sustain the fantasy of culture as whole, slow, appreciable, they have to ensure their incompetence in the vernacular. David Pocock once remarked that when you met an anthropologist who told you that after a year or two he spoke the language fluently, you had met a bloody liar. If you cannot understand people speaking to one another under any circumstance, as virtually no non-native speaker can, then you are restricted to a degree to question and answer with all its well rehearsed limitations and misdirections. When I once asked why, on successive fieldtrips, I received different answers to the same questions, my Balinese colleagues told me that I wouldn't have understood the reply earlier on. Hobart's 4th. law of ethnography reads: 'People gear down information to the ignorance of the anthropologist'. So we tend to focus on keywords and occasions that our preformed theories tell us are paradigmatic, not on the complexities of translating or figuring how to address the radically contingent nature of the conditions of possibility of social practices and their consequences. We shy away from an erotics of events, surfaces, practices, to reimagine an Ur-hermeneutic depth with a transcendental object mediated by obscure entities like 'symbols', 'rituals' and institutions [Sontag 1972]. It is like hunting by getting someone to tie down your buffalo for you first. The stuffed head on the wall, like fading copies of ethnographic monographs - tells no tales.

If you stop and think about it, fieldwork is a very peculiar practice. Quite what is it, in its pristine Malinowskian form, about a European male living among, if not part of, a group of people for two years or more, which imparts a knowledge - or understanding - achievable in no other way? The conventional answers are most unsatisfactory. It does not follow though, by recourse to a crude binary logic, that, if long-term fieldwork by participant-observation does not produce certain knowledge, it is either dispensable in favour of evocative neo-impressionism [Tyler 1986: 123; see the critique by Carneiro 1995: 12-14] or reducible to acts of textualizing, as Geertz [1988] and Boon [1982; 1990] would have it.

The problem facing ethnography, under whatever description, might appear to be that it involves two different ontological orders. On the one hand there is the disordered world of events, actions, contingencies and uncertainties, which happen, conveniently, in that remote, peripheral place, 'the field'. On the one hand, there is that coherent, structured, meaningful realm of narrative, text and professional activity, which coincidentally happens to be 'home', the centre. Quarles van Ufford has argued that it is this disjuncture between planning and its implementation in the real world which undermines so many development projects [1993]. This argument only works however from within the dichotomous ontology which gave us the hierarchy of mind over matter in the first place. The problem of both naturalism and textualism is that they both subscribe to this ontology, albeit in slightly different ways, and land in the trap of how do you represent the world in words?

Reconsidering representation as a practice provides a way of avoiding the Cartesianism which is presupposed in so much ethnographic writing. I take it that representing is always a transformational act. You represent something as something else [see Goodman 1968: 3-10] on particular occasions to particular agents or patients

for various purposes.¹⁴ Ethnographies do not reflect a reality, more or less authentically, accurately, dramatically, expressively, meaningfully or truthfully. It is practices of ethnographic writing which raise problems or rework previous arguments about what it is to be authentic, accurate and so forth. By their practices, ethnographers turn a double dislocation into a triple dislocation and consequently are engaged in a triple act of articulation.

Let me clarify what I am saying. I am deeply unhappy with the persistent tendency in the human sciences to dichotomize explanations of reified notions of society in terms of either structural determinism or free human agency. (Appeals to 'culture' instead merely displaces the problem onto an obscure ontological entity, meaning [Quine 1970; Hobart 1982].) Textualizing is a most effective means of dissipating the degree of radical undecidability. Far from the social comprising a coherent structure, it is more sensitively appreciated as rent by processes of dislocation (the subject existing in virtue of such dislocations) and countervailing practices of articulation. Articulation here has a double sense: it is both making intelligible and connecting that which is, or has been, separated [Laclau 1990a; cf. Hall 1986]. It is not so much that the epistemological and structural aspects of the term overlap, whereby privileging one over the other impoverishes and trivializes the notion. It is rather that the order which structure notionally instantiates is not separable from inquiring into, talking about, or assuming that order, just as, recursively, such practices presume an object. Articulation presupposes there to be something - under whatever description - which needs articulating.

A double dislocation then predates, and is not just the precondition for the ethnographers' arrival, but for the ethnographers' existence as such: these are the dislocations of each ethnographer's own society and those of the society which she studies. In turning up in the field, the ethnographer, whose profession, existence and presence in the field is predicated on one set of dislocations, is faced not only with another set. That, after all, was the precondition and *raison d'être* of ethnography. She is also faced with a dislocation between the two dislocations brought about by the practice of ethnography itself, which sets up all sorts of incoherences, challenges to order, surprises, crises. These are far from being of a purely insulated intellectual nature. They affect what the ethnographer does (and even is) subsequently. How temporary and superficial this is depends in part on how successfully the ethnographer manages to deny them. What Laclau refers to as antagonism [1990a: 17ff.], for ethnography I take to be evinced in the intellectual, political, material, gastric and other shocks that ethnographic flesh is heir to.¹⁵

¹⁴ For instance, when I originally presented this paper, I was asked how identity fitted in with my argument. I hope it is clear that I do not adhere analytically to the idea of a unitary subject, far less any identity of persons, objects or events, which may be extrapolated as an essential perduring set of properties from historically specific acts of representing as.

¹⁵ If you study your own society, the dislocations appear to be rather less acute. However Europeans and Americans rarely study the social class they come from in their own society. Other groups in your own society may be as shockingly different as some 'remote' society. Non-Euro-Americans are usually constrained anyway to represent their own societies in terms of an alien set of discursive practices. Anyway, as in psychoanalysis, those practices produce their own dislocations of the familiar. Dislocation is not some 'objective' defect in structure perceived by a neutral observer, but emerges in the course of inquiring.

It is the self-appointed task of ethnographic writing to articulate these three non-isomorphic dislocations and their accompanying antagonisms. Ethnographic writing then is a peculiarly complex practice, because it involves notionally three kinds of articulatory practices. To simplify matters, ethnographers usually privilege one over the others in their writings, and so drastically oversimplify the issue of multiple, non-coordinate kinds of articulation. A remarkable amount of ethnography is about using others to understand ourselves better. The hallmark of such writing is often a singularly vague and incoherent grammatical subject, the 'we', 'the west' which such accounts serve to suture. I wonder though quite how often they are used seriously to address, rather than re-insulate, the unrecognized contingencies which permeate the ethnographer's home society. My impression is that there are few indigenous ethnographies which primarily articulate antagonisms in the society in question, partly because the whole enterprise of ethnography is conditioned by, defined in terms of, and addressed to, the dislocations of a quite different society. The reflexive turn in anthropology attempts to address the third dislocation.¹⁶ Especially in some of the woollier recent American versions however, articulation is reduced to epistemological navel-gazing and so loses any point [e.g. Clifford & Marcus eds. 1986; Marcus & Fisher 1986]. Epistemologizing ethnography condemns it to become an ever more obscure and narcissistic pursuit, because the conception of knowledge and the subject is largely, if inevitably, defined in terms of, and addressed to, the dislocations of the knowing subject's own society. The problems remain, indissolubly ontological and epistemological at once, of how others address the conflicts, contradictions and dead-ends of their worlds, and how ethnographers relate to, and relate, this double dislocation.

What is difficult, important and exciting about ethnography as a serious practice then is precisely that it is one of the few ways in which we can engage with these dislocations directly. The imponderables which necessarily surround fieldwork - where you happened to land up, who befriended you, when you happened to be there, what chanced to happen and what not - are not mere accidents, but part of the contingency which marks dislocation. There is then a double contingency. Some anthropologists fear that to recognize this would make a mockery of any remaining natural scientific pretensions. So they attempt to paper over the cracks by appealing to an abstract substance - the collective, or meaning, or some such - of which any event is simply an instance.¹⁷

Naturalizing then requires the coherent, given object; whereas semanticizing presumes some kind of coherence-seeking, self-constructing subject. An alternative approach which appreciated the extent of historical situatedness, contingency and antagonism both in the society under study and in the circumstances of study is not

¹⁶ By contrast, the reflexive movement in philosophy, whether in its modernist Habermasian, postmodern Derridean, or pseudo-pragmatist Rortian turns, remains incredibly myopic, caught up as it is in the dislocations of some Gargantuan, if largely mythical, 'west', which philosophers set about resuturing (Habermas), desuturing (Derrida) or simply critically admiring (Rorty). Put this way, for all its faults, by comparison anthropology looks surprisingly adventurous and relevant to the approaching third millennium.

¹⁷ This is circular, of course. All Balinese (or whoever) share common features, so if two or three (or however many) Balinese say the same thing, we have a social fact. How do we identify a collectivity, 'Balinese', in the first place? It is because they share the same features and agree to the same social facts!

without its difficulties. We have lived so long in the remorseless shade of structure (together with its props: cause, meaning, function, context) and with genres of writing which reflect or create such shadows, that thinking seriously about alternatives has barely begun. One way to start is to rethink ethnography as a practice.

Fieldwork situations

The circumstances of fieldwork bear directly on some of the practices of ethnography. My age, seniority, wealth, contacts, previous research and ability in language all affect how I worked and was allowed to work. So too has the transformation of the settlement where I work from a poor agricultural community into a relatively affluent, centre of handicraft production through which thirty or more tourist buses thunder each day. Agency was an issue in my fieldwork from the start.¹⁸ When I began postgraduate research in Bali in 1970-72, the problem was how to choose a village. Everyone I spoke to dismissed everyone else's suggestions and, worse, hinted at dark secrets from the time of the mass executions of the Communist Party members in 1965-66. Choosing the least awful-sounding patrons, I was placed inevitably with a branch of their family in the local court in a suitable remote [Ardener 1987], and therefore wonderfully authentic, village. I was allocated a bodyguard, which seemed unnecessary at the time, but which turned out to be life-saving. Before I had spent a night in the village I was a political issue, both because the rival faction to the court's wanted money from my presence and because I had stepped unwittingly into a hotbed - even by Balinese standards - of murder and post-1965 intrigue. I had, especially initially, to work through relations which had been assigned me. I felt less an agent than like a ping-pong ball. The village was then remote. As far as I have been able to ascertain, I seem to have been the third European to set foot there. But I doubtless exaggerate.

During my second fieldtrip in 1979-80, I rented a compound in the same village ward and held open house. My research bill included £ 3,000 for alcohol, mostly fruit liqueurs, which were a great attraction. The style of fieldwork suited my study of indigenous styles of argument and philosophical ideas, as people would turn up and argue with one another. I belatedly realized that Balinese do much of their thinking in informal and changing groups; and that discussing, arguing, having meetings are among the most common practices in many places. So I have worked with single 'informants' only as an exception since then.

My third study in 1988-89 was on the media and development. My closest companion had insisted that I stay with him. No alternative was acceptable to him. By then public life in the village had contracted sharply. People worked day and halfway through the night making carvings; and watched television while doing so. No one would come to an open house; and my previous key site - the village food and coffee stalls - had mostly closed for lack of business. I worked mainly with various groups of

¹⁸ In a fuller account I would begin with how I came to decide to study Bali in the first place. Briefly, after graduating I taught anthropology at the University of Singapore with the aim of finding a suitable society to research. I had to teach the work of Clifford Geertz on Java and Bali. Having had undergraduate training on South and South East Asian anthropology, Geertz's representation of Bali as totally un-Indic looked odd. I decided to go and have a look.

people who were thrown together by circumstance and choice in an attempt to interpolate myself into existing groupings. Of course, my presence changed what happened, although it would have been easy to fool myself that it did not.

Since 1989 I have been back every year to run a project to record Balinese cultural programmes on television. I have employed two Balinese to record and transcribe the text for the last five years. They have become relatively affluent as a result, in a community where some people are rapidly getting rich. Not much is said about the fact that anthropologists pay people as assistants, informants or 'friends'.¹⁹ If we are to understand how people articulate their worlds to us, it is important not to dissociate issues of epistemology from power.

Before and after

While fieldwork may be a situated activity, it does not follow though that ethnography is not highly structured. On the contrary, ethnography is only one of a long series of overlapping practices, most of which happen far away from the field. Focusing on fieldwork as the dramatic moment of authenticity sidelines the fact that what you will see, and the practices you engage in, have been largely determined long before you arrived there; and will be redetermined after you leave.

Ethnographic practices begin, I would argue, during undergraduate training. Crucial to these is the excruciatingly boring nature of most university teaching, which is designed to turn acolytes into subjects whose primary objective is self-discipline [in Foucault's 1977 sense], not inquiry, as an end not just a means.²⁰ At this stage, the nature of the field, the purpose and goals of research, the expected format for presenting results, the role of texts, the place of field research and publication in academic anthropology and the university as a polity have been effectively prescribed. Examinations form the *rite de passage*, proof positive of your submission.

Research training ensures that you have read, learned, marked and inwardly digested previous ethnography so thoroughly that the risk of your encountering anything new when you get to the field has been almost totally eliminated. This will, of course, not appear to be the case because research by definition is about discovering something new. However what counts as new - like what makes up news on television - is massively pre-structured and predetermined [Fiske 1989]. Likewise you are taken through methodology courses which, by definition, predetermine how you will assess, record, measure and write about precisely what ought to be unknown, situational, contextual, to-be-thought-over and under no circumstances to be taken for granted or 'routinized'. We also learn language as if it were a tool (to open a mental can?), not itself a contested and underdetermined set of practices [Volosinov 1973]. But the more that grant-giving bodies demand rigorous training, the more we train thought out of future researchers. The paradigm for research is previous examples of successful

¹⁹ On my first trip, I did not even have a choice. I was assigned a driver/bodyguard and two young men as cooks and cleaners. Providing as many people as possible with a livelihood was an obligation still in 1980. By my next trip, labour was at a premium.

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research. A moment's thought shows this to be retrogressive. If research is about rethinking and breaking away from past practices, why train people to emulate them, except to learn from mistakes and go beyond? If I drove a car around London facing backwards the whole time, I would rapidly crash if I were not arrested by the police first. Formulaic military regulation is a better image of research than is critical thinking. It is not for nothing that Foucault wrote about disciplinary practices.

A further crucial set of practices begin on your return from the field. This is the business of 'authorizing' the people you worked with, so becoming their agent [Asad 1986; Hobart 1990]. If you were well trained, your notes are already so textualized that the transition from provisional text to final text is relatively painless. The problem of articulating the conditions of your interpolation in other peoples' lives effectively bypassed. Students who forgot their disciplinary background for a moment and actually got on with the people they studied often have terrible trouble textualizing what happened. (There is a crude inverse correlation between the originality of ethnographic fieldwork and professional advancement.) From the moment of return, the researcher is caught up in a series of ever more rigorous disciplinary practices - from interpreting the results, presenting drafts of thesis chapters, to giving seminar papers, to attending Anthropological Association meetings, to the monograph (which as the name suggests forecloses saying you got it wrong), to gossip, to 'networking', to be allowed to practise teaching on new initiates - designed to turn her into a fully-fledged, totally-textualized, reality-free professional. Thereafter designing course outlines, teaching, examining, politicking and publishing because it is required are all practices of reinscription. The best way to cope with the superfluity of fieldwork, its sheer exuberance, complexity, uncertainty, danger, misery, fun - all the things academics are in business to textualize and deny - is not to ignore all this, but to sanctify it by declaring ethnography the foundation, the originary moment, the epistemological precondition or some other waffle. By whatever means, the complex of practices which is ethnography must be carefully circumscribed and turned into a simulacrum [see Baudrillard 1983].

Subject positions and activities

Finally I can turn to ethnographic practice in the narrowest sense. However, let me not start with a round-up of the usual suspects, about which too much imaginary has been written in accounts of fieldwork, but with our relationship with the local people into whose lives we have interpolated ourselves. We cannot do fieldwork, as Evans-Pritchard discovered among the Nuer, unless they cooperate at least minimally. So the first kind of practice in which the ethnographer is engaged, the precondition to all others, is disciplining the natives. This takes many forms. Minimally, we have to train them to speak, think and act towards us in ways with which they are unlikely to have been familiar. Without this, there is not the minimal degree of commensurability we require to engage with them. As Wittgenstein remarked, 'If a lion could talk, we could not understand him' [1958: 223].

Our relationships with our main informants go much further. In order to engage in critical discussion with them, we have to persuade them to think about – and perhaps engage in – their own societies, their actions, themselves and their relationships in ways

quite different from those to which they are used. The process requires them to adopt unfamiliar 'subject positions' [in Foucault's terms, 1972]. I have been through this myself, when an ESRC research team was interviewing members of my department about our experiences of teaching research students. I found myself representing such teaching in coherent terms which bore little relation to my experience of the situated practice of supervising. The circumstances of the inquiry made me articulate relationships, roles, practices, difficulties, both intellectually and, later, in practice. I found myself adopting a subject position I had never experienced before. If Laclau's idea of the subject being constituted in dislocations at the edge of structures [1990a: 61] had seemed elegant but somewhat ideal beforehand, it gained a curious immediacy through the encounter.

To elaborate briefly, I found myself at once exposed and answerable not just for my own actions but for accepted administrative procedures, which appeared strangely shorn of the situated understandings that make them possible. I was required to step back and objectify lived relationships into detached prose. Being over-coherent was effectively obligatory if I were to speak about what we never spoke about that way. What surprised me far more was the sudden sense of the chasms which yawned under everyday activities, the shockingly contingent nature of what it was to supervise students and my own relationship - or lack of it - to the supervisor in question. At the end, the researcher thanked me for my openness - my colleagues had apparently sutured their work better than I. I was aware while being interviewed that, if the researchers were to know what was happening, it was incumbent upon me to tell the truth in full as I saw it, not to hide behind administrative platitudes. Many Balinese I have worked with seem to me to have struggled to maintain a kind of self-objectifying honesty when working with me. On several occasions those I know the best have told me how straining it was, not because they did not wish to tell the truth. As I now think back on what they said, it may have been because it was an unfamiliar kind of truth and the telling of it a dislocating experience.

Being on the receiving end is a salutary experience. As ethnographers, we are far more familiar with the disruptions we encounter when entering the field. There is an entire genre, anthropological autobiography, to celebrate the experience. At its best, it may say something about the dislocation around which ethnography centres, but seems to address less the other dislocations which are its precondition. (One drawback of going once or twice a year to Bali is that I slither gracelessly, but painlessly, from one massive apparatus for the displacement of radical contingency to another and back.) The ethnographic process feels very different depending on whether you are the agent (for the moment, the ethnographer) or its subjects. If we appreciated the complexity of the circumstances we induce in those we talk to and work with, fieldwork would, I hope, be a vastly different activity. Margaret Mead once said that, to study somewhere like Bali, you should be parachuted in with no prior knowledge. My point is that we carry that knowledge as practices, including practices of non-reciprocal understanding, with us anyway.

Who is it that anthropologists talk to anyway? Marvin Harris [1969] tellingly pointed to the importance of 'well-informed informants', people who are often marginal to their own society and who, like Cassius, 'think too much: such men are dangerous' [Shakespeare, *Julius Caesar* I, ii]. Perhaps the most famous example is Victor Turner's relationship with the thinker and diviner, Muchono (*The Fly*), a man

deeply distrusted, it seems, by the Ndembu. I would suggest that the people anthropologists relate to, and can work with, are local intellectuals. We may get on well with other people, but *we cannot talk to them* about what matters to us. There is an important work to be written about the relationship between academics and local intellectuals, be these traditional, organic, episodic or whatever.

What subject positions then did Balinese in the village where I worked ascribe to me, and I to them? And what were the attendant practices? I shall deal mainly with the first trip. At that time Balinese villagers had three kinds of ways of dealing with outsiders, depending upon whether they were traders (to be dealt with according to accepted language and etiquette of the market), bureaucrats (to be deferred to and, if possible, persuaded to go away or at least placated into directing their attention elsewhere) or other visiting Balinese (to be treated initially according to caste and supplemented by any other available knowledge). I fell roughly into the second category. The means of getting rid of me was to put it about that I was a spy sent to investigate the murders in 1965. In my attempt to escape this categorization, it became clear I had powerful contacts. This made me dangerous. What do you say to such a person? It is best to avoid them altogether - not a good basis on which to do fieldwork. Formally my landrover was available for important public needs, including taking people to hospital. Years later I discovered the villagers considered this as my main value, one they were prepared to match with conditional toleration of my presence. What I did not realize until still later, when I started to rethink ethnography in terms of practice, is that the rumour-mongers had a far subtler appreciation of what I was doing than I, the trained expert, had.

Alongside this, to the far smaller number of people with whom I regularly had dealings, I gradually became - I like to think - part-human. To these people, one key subject position I occupied, and which lasted over two fieldtrips, was of overgrown child to be educated into Balinese ways of doing things, behaving and speaking. They became responsible to the village for my not causing serious abuse, disruption or pollution, no easy task in Bali at that time. Simultaneously, in accepting money in some form, these people were also my employees or clients, relationships which they understood because both were common. The problem was - did I? Again, they had to discipline me into understanding what was required of me, then ensure I continued to do it. What stands out from my first fieldwork is how much time Balinese spent disciplining me.

On my second visit, I realize in retrospect that I tried to introduce a new kind of relationship and so new practices. I think, again after the event, that I was allowed to do so because people thought I had learned enough self-discipline to be semi-safe out on my own, as it were. In having an open house, I was to a degree emulating *ngajakang*, when a wealthy villager invites others to work for him and reciprocates by showering them with food, drink, cigarettes (later cigars and even pipe tobacco) and gifts. It is a relationship marked by much obligatory jolliness. It does not last. It had at least the virtue of including rupture, which is important. Anthropologists go away: the people you work with know you are going to go away. They are stuck there. Going back every year has made matters very different. There is now a degree of continuity and with it I have started to engage in idiosyncratic relationships which go beyond easily recognizable practices with a few Balinese with whom I work regularly in different capacities.

Thinking back on it, during the first fieldtrip I saw the villagers in sharply contradictory roles. First and foremost, they were a resource, a source *par excellence* of information. My purpose in being in Bali was to extract that information.²¹ If I could get inside their heads and empty out the contents, I would have revealed Bali and would be a professional success. My subject position was investigator, which required me to be interrogator, at times inquisitor - or so I thought. So my practice was geared to extracting information, which I took to be an internal state.

Like other ethnographers, I mustered a ragbag repertoire of specialized activities to achieve this. They included interviewing, attending meetings, listening, observing, discussing, above all misunderstanding; measuring, counting, enumerating, weighing, listing, recording, above all writing notes, sketches, lists, often compulsively, usually unthinkingly; attending, eliciting, emoting, confiding (for the purposes of being confided in), being taught, above all judging. I often talked when I should have listened; and, especially during the early stages, thought I should observe when I would not have understood what I saw anyway. I almost invariably judged prematurely and regretted it later.

All this pales beside the central and crucial practice: misunderstanding. For a start it is far from clear what it is to understand someone. And is what we understand a person or their actions? Do we, through the mystic can-opener of intersubjectivity, get direct psychic access to other minds? And how does this work when we barely command the language? Or do we have to infer intentionality from its traces in what people said and did? And, if we resolved these problems, we would still have to work out on what grounds we may assume others to understand one another in terms we can understand without begging the question. Anthropology is nothing if not a massive monument to human misunderstanding.

All this required discipline: attending endless rites, going through the etiquette required before 'getting to work', asking well-formed questions in Balinese, looking pleased when they told me what I wanted to hear and troubled when they did not, writing things down when they expected me to. I learned to do what I thought they expected and to be what I thought they expected.

There were other contradictory images. I imagined Balinese at moments as very stupid (they did not answer my questions as I wanted them to - I still have difficulty with this one); as clever but unwilling to confide; as people from a different planet.²² Part of this though is retrospective from subsequent fieldwork. At the time, I remember seeing the villagers as dangerously unknown and exotic to be handled with as much care as they themselves handled the spitting cobras, which were everywhere. My key practice then can be summarized as placatory-extractive. I cajoled and, as my bodyguard was a professional hit-man, sometimes bullied them for information, all the while stockpiling what I fondly imagined to be the facts. In short, I wheedled. After about a year I began to realize the futility of this, but it was not until my second trip that I managed more or less completely to give on the idea of ethnography as extraction.

²¹ This raises questions about what is the object, or subject, of anthropological inquiry. It is an issue anthropologists have dodged for the most part because the answers are banal, incoherent or frightening.

²² The longer I am there, the more often this thought passes my mind. But then it does about my colleagues and sometimes myself.

Over some issues, different groups of villagers and I developed shared concerns. I was a source of amusement, mostly due to my incompetence, ineptitude (for example, one day I got 23 blisters on my hands from agricultural hoeing) and for mistakes.²³ The other day I attended the 50th anniversary of Independence in the village and it was clear that after twenty-five years most of the senior villagers are still not quite sure what to say to me. So much for integration. I was, by agreement, a source of help and of cash to individuals and public causes. They were however clearly aware of the responsibility for my welfare which was thrust on them by my presence. They have never collectively, but occasionally individually, asked to make use of my official connections in Bali. By contrast, the more senior government officials in Bali seemed to find my presence when I was spotted lent a certain cachet to their visits. My first fieldwork can perhaps be summed up as me extracting, them tolerating me and trying to limit the damage I might cause. To my shame, it was only after rethinking fieldwork as a practice for this paper that I realized how important it was for the villagers at such a sensitive time to minimize the injury I could cause them.

Ethnographic practices

Ethnography, I suggest, involves discipline. Ethnographers discipline the people they work with, these in turn have as a matter of urgency to discipline the ethnographer. Balinese have had to self-discipline themselves greatly in dealing with me; and I have learned to discipline myself with them. This makes ethnography a singular, but not unique, congeries of practices. What I had not appreciated at the time was the degree of mutual disciplining and self-disciplining.

The discipline was not however to those ends with which we are familiar from *Discipline and punish*. Those bear more closely on examination (including, for instance, the examination of your work as a research student).²⁴ The villagers were concerned by contrast, I think, with discipline to quite other ends. I needed to be contained, first from making a nuisance of myself; later, and more seriously, from inquiries which threatened their accommodations with their own dislocations. (From one point of view, travel literature, ethnography, New Age movements and the vogue for Asian religions are all ways we have of accommodating ours, whoever 'ours' is here.) My disciplining Balinese was to make them tractable to a different set of practices: investigation. Having engaged in it for many years, I think Foucault may have been justified in describing it 'as an authoritarian search for a truth observed or attested...[a] sovereign power arrogating to itself the right to establish the truth by a number of regulated techniques' (1977: 225). To read the work of Firth [1971] or Nadel [1951], or more narrowly that infamous series, *Notes and queries on*

²³ The most mortifying was when I had been treating a woman for tropical ulcers who was married to one of two identical twins in the village. When I bumped into the other twin and asked how his wife was, he replied 'Not bad, considering she's dead'. For a moment, to everyone's great amusement, I thought I had killed her.

²⁴ That mindless discipline, designed to turn agents into the instruments of their own self-subjection, required an altogether different régime of power, the market, and managerialism as the means of interpreting and imposing its writ. Thatcherism and Reaganism have been the means to rounding up feral and unreliable agents like academics by ever more rococo disciplinary means, such as endless efficiency and productivity reports.

anthropology, culminating with Ellen [1984], you might be led to think that someone had thought through techniques to address the difficult questions of forensic inquiry, textual criticism, modes of evaluating hearsay, justified inference from contradictory sources, forms of non-fallacious reasoning which an ethnographer, like a magistrate, requires as a matter of daily routine. You would be disappointed. Some ethnographers are of course superb inquirers, as is clear from their work. The 'regulated' techniques are consigned though to courses called 'fieldwork methods', relegated in my institution to be taught by casual hired labour. On the other hand, what a course on methodology that confronted ethnography's triple dislocations and articulations, and then regulated them all, would look like is the stuff nightmares are made of.

There are other consequences of treating ethnography as a set of activities which is part of a larger complex, designed to achieve more or less clearly stated goals. It makes people easily dispensable as a means to an end. What concerns me here is the kinds of practice implicit in those activities. During my first fieldwork, the practices which I imagined I was engaged in were locative (finding the best informants, i.e. those who confirmed my preconceptions) and exploitative (the parallels with mining and with Dutch colonial economic interests is not coincidental). During my second fieldtrip, I was much more concerned with textualizing.²⁵ I took it that there was implicit knowledge which Balinese presupposed in order to get through the day, make choices and think retrospectively about those choices. I imagined, I think, that knowledge as textual, in the sense of it being inscribed, if not physically, at least in peoples' memories. So I wrote endless notes, made hundreds of hours of cassette recordings (the arrival of the cassette recorder changed ethnographic practice) and promptly textualized the recordings by paying Balinese to transcribe them. So I turned conversations into texts in the narrow sense. Since then I hope I have complicated my practices somewhat to diversify from textualizing, but I am still too close to them to judge what it is I am doing now.

In more general terms, during my first fieldwork I naturalized Bali and Balinese. There was a sort of object, society, comprising social institutions, structure, dyads, networks, symbols, beliefs etc. which had a sort of objective existence. Balinese talking and doing things were evidence of these objective structures instantiated. I am less interested in whether the conception of society preceded the practices of studying or *vice versa* than I am in how I objectified Balinese practices, while at the same time subjectified what they took to be objective, like treating techniques for dealing with the non-manifest as 'ritual'.

I look back on that time with growing horror. Unfortunately, while my switch to inquiring into Balinese philosophical ideas was an improvement in that I no longer sought objective structures, in textualizing - and carefully contextualizing - those ideas I desituated them. (Constructivism is of course the reverse process of subjectivizing everything which leads, by yet another reduction, to biography as the essence, the true voice, of the native, free of colonial hegemony, subalternism and ethnographic writing itself.) I returned to Bali explicitly rejecting the idea of abstract coherent patterns of

²⁵ Some of the staple activities of textualizing seem to be: reading, re-reading, reflecting, savouring, empathizing vicariously. There follows the more familiar stages of writing, although I think this familiar ground needs critical re-examination. In sum, textualizing seems to me a rather solitary, even onanistic, activity. It is in many ways the antithesis of dialogue, which academics reattach to textualizing, but which seems to me potentially to run the opposite way.

knowledge independent of situated practice. However, my method of inquiry detached Balinese intellectual practices more than necessary from the situations in which they took place.²⁶ Now I am not alone here. Balinese objectify themselves in different ways on different occasions through various practices. Where I went wrong is in failing fully to link my account to the articulations in which Balinese were engaged in so doing.

For the world at large though it is the objectifying practices of anthropologists and others which may have the most serious consequences. Balinese now learn what they are really like at university by reading Geertz's *Person, time, and conduct in Bali*, an object lesson in textual objectification by reducing persons to names, time to calendars and action to etiquette. There are subtler forms of objectifying practice. Two recent ones are singularly efficient in that they involve, crucially, self-discipline. One is development planning. Balinese are urged by endless government advertising, civil responsibility campaigns etc. to turn themselves into self-regulating recipients of development. The other is television. Television-viewing practices are means by which Balinese turn themselves if not yet always into citizens of Indonesia, at least into consumers, participants in new worlds and adopt new subject positions in the course of so doing. In the same breath, as it were, they are caught up in their own subjectification as cuddly creatures to be filmed, photographed, financed, flown all over the world, flirted with and fucked by.

What to do now?

A conclusion would be out of place - a Requiem perhaps less so. The possibility of anthropology has rested upon several implicit, interlocked relations of understanding. These are the ethnographer's understanding of the actions of the people she works with; their understanding of one another; their understanding of the ethnographer; and anthropologists' understanding of one another. Ethnography is largely confined to the first of these possibilities, where understanding becomes a form of power far more frightening than mere knowledge. It claims direct access to others' minds, an access which is not reciprocal. Ethnographic writing is the business of enabling distant others, who have no relationship with the people in question, to understand them vicariously, by proxy, requiring no more effort than simply reading the written page.²⁷ In their practice most anthropologists pay so little attention to the conditions under which others are supposed to understand one another, that even this seeming-innocent possibility is negated by lack of interest.

The whole approach is fraught with problems. The practice of understanding here is not simple or unitary. Indeed I very much doubt that there is a discriminable class of actions (far less mental states which, following Needham [1981], I try to avoid imputing) which corresponds in English to the act, or state, of 'understanding', let alone in French, German, Italian or Balinese. I am tempted to reverse the grand thrust

²⁶ There is, of course, no authentic moment or experience which acts as a yardstick or guarantee. It does not follow by crude dichotomous logic that we have nothing therefore but texts, the position only the truly dim-witted embrace without qualm.

²⁷ In this, oddly, it resembles some feminists' complaints about female pornography [but see the reply in Gibson & Gibson 1993]. Is part of the link the voyeurism they share?

of *Hermeneutik* and return understanding to an odd-job idea or perhaps, with Wittgenstein, argue we should

try not to think of understanding as a 'mental process' at all.-- For *that* is the expression which confuses you. But ask yourself: in what sort of case, in what kind of circumstances, do we say "Now I know how to go on" [1958: 61e, #154].

On that account, understanding is a way of going about something when you appreciate what it involves. It bears an interesting relationship to articulating which became left out of the tetradic formulation of understanding above. The effect was to impute something essentially similar to the way, under different circumstances, people address, articulate or understand (in Wittgenstein's sense) the contingencies and dislocations with which they engage, and in so doing share something in common. There is nothing more in common in acts of understanding than there is between different ways of 'going on'. It does not follow that ethnography is reduced to a bloodless enterprise. In place of understanding, perhaps we should think in terms of recognition. To me 'recognition' suggests the appreciation of difference, be these in circumstances, in goals, interests or inclinations. What is important is that recognition is mutual and momentary, and between agents or patients, be they complex or simple. Understanding in ethnography is usually far from mutual. It also easily becomes timeless: once I have understood x, there is the assumption that I shall always do so. Recognition cannot be one-sided, nor can its continuity be presumed. It lends itself less easily than does understanding to verbs of power and possession.²⁸

Perhaps then, recognizing others in the fullness of their difference and being recognized in return is part of the point of ethnography and part of its justification. To take the man I have known for twenty five years and with whom I now stay when in Bali, to say I understand him or that he understands me, seems a singularly vacuous statement. What would it imply? To say that I know such-and-such about him and he knows such-and-such about me hardly sets the pulse racing. To say that we recognize each other as persons, as non-unitary and complex beings, who are each grappling with very different life circumstances (dislocations, if you will), and that our asymmetrical relationship bears on how we are each separately engaged in articulating our lives within the social arrangements of which we are part, at times through this strange relationship, makes good sense to me at least. Perhaps this is what ethnography is partly about and not the ponderous juggernaut of naturalization and its fey successor, textualization. Otherwise ethnography becomes just telling more about penguins than anyone could possibly be interested in knowing.

²⁸ This raises the question of Hegel's problematic relationship of master and slave. Recognition is central. The relationship hinges on the master's need for recognition of his mastery, wherein lies its complexity.

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