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THE END – THE ENDS – OF ANTHROPOLOGY

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There is a significant ambiguity in the title of this collection and in the title of my own contribution: the end of anthropology. Quite obviously 'end' may mean demise – the demise of anthropology – or it may mean goal: the goal of anthropology. It may also mean a boundary or extreme edge, as in the 'the end of town', intention, result, outcome, completion, conclusion and, suggestively, responsibility, as in 'your end of the bargain'. 'End' is derived by way of the Sanskrit *ántas* from the Indo-European **ant-* whose basic meaning is 'front' or 'forehead'. In the locative form, **ant-* means 'against' with derivative meanings 'in front of', 'before', and 'end'. It is related etymologically to 'ante', 'anterior', and 'advance'. Its Indo-European etymon draws attention to both the spatial and temporal perspective from which an end is envisaged. We are always located before the end: the goal, the outcome, the completion, the demise, the death, for which – I am stretching my point here – we are not without responsibility. Anticipated, the consequences of the end have to be expressed in the future, at times in the future anterior, in whatever mood: as such, they refer back to the position of whoever announces an end and evaluates its effects – its end, the end, so-to-speak, of the end. The future, however predictable its appraisal of what will have occurred, always requires an imaginative leap, which is constrained by the conventions of the present.[1] We have in any event, therefore, to recognise the significance of the position not only from which we appraise the end of anthropology but also from which we pose its very question.[2]

I question the end of anthropology from a radically disquieting position: one that aims at breaking the complacency that comes with the institutionalisation of a discipline which by its very structure – the straddling it demands – ought to resist the deadening effects of that institutionalisation. In so doing I will no doubt tread, if only by indirection, on the work many anthropologists have produced, as I tread on my own work. I do this out of a deep concern for anthropology's future. I do not want to deny the progress

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1 My friend Stephen Foster observed on reading a draft of this essay that it is also possible to view the end – the end of anthropology – from after its demise, as 'ruins', he added pessimistically. Or after the fulfillment of its goal, we might add, were its goal not simply an unreachable telos that structures anthropology as a discipline – a telos that, though in practice continually redefined, resists final definition.

2 My discussion of the etymology of 'end' is based on the entry and appendix (Indo-European Roots) in the "American heritage dictionary of the English language" (Boston: Houghton-Mifflin 1979). I should note that Partridge (1958:182) finds **antas* akin to the Indo-European **anti* (opposite).

that ethnography has made over the last century. We have gathered an enormous amount of data. Today, there is probably no society in the world that has not been the subject of anthropological investigation. My concern is with the way anthropology conceives of itself and how this self-conception has affected its theorising, the development of which is incommensurate with the data it has collected. We have tended to borrow theoretical paradigms from other disciplines to illuminate our data, often without critical regard for how they influence our research, our conception of anthropological research, and our take on anthropology as a disciplinary practice. I believe we have not recognised how radical a critique of social and cultural understanding we can make, had we the will. We have not given sufficient attention, I will argue, to the effect of our straddling positions (Crapanzano 2004:39–65). We need to develop theories and interpretative strategies that arise from the betwixt and between from which our research proceeds – a position (if 'position' it can be called) that precludes sure footing and, as such, lays bare – or ought to lay bare – the paradoxical temporalities of social and cultural existence and the plays of power and desire that promote the punctuation of those temporalities, that punctuation's artifice. My essay will oscillate between a critique of contemporary anthropology and intimations of other possible anthropological approaches. I will focus on those anthropologies that are primarily concerned with complex societies, especially the anthropologists', and their institutions and socio-cultural arrangements. That I stress these new foci of anthropological research does not mean that that I believe we should abandon our traditional research domains and many of our research practices associated with those domains – quite to the contrary. But consideration of the future of anthropologies that preserve this interest merits a paper in its own right. Here I want simply to note that the changes in research domains and the new methods it requires will inevitably affect the research we do in more traditional domains.

As Karl-Heinz Kohl and other contributors to this collection have noted, anthropology has always worried about its end. This sense of an imminent end has been related to the fact that anthropologists have, until fairly recently, studied moribund cultures – those on the verge of total disappearance or subject to such radical change that they lose their identity and even their memory of their past. At least since Franz Boas, but, in fact, long before him, whether through the collection of artifacts, the recording of disappearing languages, the transcription of myths and folklore, or social and cultural description, anthropologists found themselves salvaging the last remnants of dying cultures. Their emphasis was on the timelessness of the traditions they studied, which were often presented in the ahistorical present tense. It seems odd that these societies and cultures should be figured timelessly, as they were subject to changes so radical that their end was imminent. One might say that the ahistorical tense – the ethnographic present – served magically to preserve what was, in fact, dying or dead. Whether the ethnographers' task was, at least in anthropology's early years, one of salvaging or preserving cultures – despite what humility they may have had – they were placed or placed themselves in a heroic position. We must remember that 'salvage' is related to 'salvation',

and 'preserve' is derived through the Latin *servare* (to keep, to preserve) from the Indo-European **ser,* which may be related to **ser-ôs* or hero. Those anthropologists had an impossible task: to save what 'their people' – the Euro-Americans – had destroyed or were destroying.

I should note, parenthetically, that there were anthropologists, like M.J. McGee, the first president of the American Anthropological Association, who wanted to preserve at least some primitive cultures as living museums and research centres. More than sixty years later, I had a brittle argument with Margaret Mead over her desire to isolate some of the Pacific Island cultures so that they would become 'reserves' for future anthropological research! We are, of course, wont to stress the grieving that accompanies the death of cultures, but we have to remember that their preservation at the end of the nineteenth and early twentieth centuries also served the prevailing belief in indomitable progress. As these primitive societies represented stages in the unilinear evolution then in fashion, they attested to America's – the white man's – extraordinary progress (Parezo and Fowler 2007, Crapanzano 2008).

More than any other human science, anthropology's self-understanding, its identity and definition, are embedded in its subject matter in an intensely personal manner. In part this is the result of fieldwork. With the exception of psychoanalysis, the practitioners of none of these sciences have as long and intimate contact with their subjects. Anthropologists see changes that are rarely happy in the societies they study; they witness the death of those they befriended, who were often custodians of their society's past; they empathise with their informants' nostalgias and regrets, their idealisations or rejections of their past, their fears of and (often unrealistic) hopes for the future, and their (nativistic) turns to the past. They feel the pain of departure – the end of what is often the most significant experience in their lives – the loss of immediate contact with friends, the fear of the future for those friends, the question of whether or not those friends will feel their loss as they feel theirs, and the translation of lived experience into memory – memories that will be so worked on that they will lose the force of immediacy and spontaneity. Death and loss have accompanied anthropology in an insistent and uncanny fashion, often resurrecting feelings that the anthropologists would prefer to ignore.

All of these factors intensify the anthropologists' relationship to their defining subject matter. There is, despite the anthropologists' commitment to change, an inherent traditionalism in their understanding of their discipline. Though death and loss may be less salient in the new domains anthropologists are beginning to study, they nevertheless tone that research, especially in its critical reflexivity – the critique – that is attached to it. Though many younger anthropologists are excited by the changes in anthropology's purview, they are not immune to the sense of loss of the traditional objects of research, the actual loss of the subjects of that research, and the loss thereby of the traditional, defining scope of their discipline. To all of these factors that promote a focus on the end of anthropology, we have to acknowledge the fantasies and probabilities of

world-ending that are current today. The anthropologist is no more immune to these than any other inhabitant of the contemporary world.

We live in a violent, competitive, war-besotted age that is edged by thoughts of apocalypse, at least of change so radical that it resists confident predictive articulation, and as such promotes less enthusiasm for the future than worry and despair about it. We search for security – freedom from risk – in a world that we find ever more dangerous. Georgio Agamben has argued in "Homo Sacer" that the 'camp' (death camps, refugee camps) has become our social paradigm – 'the *nomos* of the political space in which we are still living' (1995:185). He may be right, but we must not direct our attention only to those inhuman camps our biopolitics justify but also to the fact that that same biopolitics have encamped us. We wall ourselves in when we talk about globalisation, the demographic disruptions it produces, and the threat it poses to our individual and national pre-possessive identities, indeed to our survival. We tolerate crippling defense budgets (54% of the U.S. budget, that is 47% of the world's entire military spending). We find ourselves at the edge of ecological collapse. We are powerless before markets running wild – markets to whose hand-of-God dynamics we have surrendered as we might surrender to destiny, had destiny not been reduced to chance, luck and risk. We are immobilised by political systems that seem unable to grasp the seriousness of the situation in which we find ourselves or which offer us false hopes as, conned by those same hopes, they act in accordance with them. We are helpless before cosmic forces whose mythic formulations can hardly conceal the reality behind them. We focus on the immediate, we miniaturise our horizons, we reduce our goals, we materialise our aspirations, we measure our worth in greedy numbers, we take solace in the habitual and pleasure in the instant, we seem lost in a labyrinth of deflections and evasions of the consequential and, as Jane Guyer recently observed, ignoring the near future, we skip from the immediate future to a future so distant, so dreamlike, so fragile, so lonely that many people are led to an insistently literal or a selfishly allegorical reading of sacred texts they take to be prophetic (Guyer 2007:409–419).

As for the past, we seem, at least in the United States, to have lost a conception of history that lends support to our understanding of the present and future. The historian Tony Judt writes that 'we wear the last century rather lightly' (2008:16). We may memorialise it with heritage sites and historical theme parks, but we no longer give 'the present a meaning by reference to the past': now the past 'requires meaning only by reference to our many and often contrasting concerns'. Though I am not convinced that historical understanding was ever free of 'present and contrasting concerns', I have little doubt that today, fragmented as our historical understanding is, it is incapable of providing a firm and confident vantage point for appraising ends: the end of anthropology. Does the concern for the end of a discipline not resonate with our fragmented and contradictory picture of the past? Is it not conducive to an uncanny coalescence of the two primary meaning of 'end': demise and goal, death and intention? As we look back, are we destined, like Walter Benjamin's (1977:255) famous Angelus Novus, to see only

piles of debris growing toward the sky which, however, we, unlike the Angel of History, invest narcissistically with significance?

My depiction of the position from which we ask after the end of anthropology is rhetorical. In fact, I am not concerned with the end of anthropology, however 'end' is understood, but with the ends of anthropology. Indeed, with the ends of anthropologies. For years I have insisted that we pluralise anthropology.[3] By pluralisation I am referring less to anthropology's four fields, sacrosanct in the United States and largely ignored in the rest of the world, or to its ever proliferating subfields than to its diverse theoretical orientations, critical perspective, methods of research, styles of presentation and argumentation, pedagogical techniques, modes of engagement and commitment to one or other differently evaluated audiences. I am referring – more significantly – to the many ways in which anthropologies have developed in different countries and how, in their evolution, they have responded not only to local traditions and conditions, but also to the hegemony of the self-stipulated 'centres' of anthropological thought and practice in Europe and America.

Though the response of many of these 'new anthropologies', as I have heard them called, to these hegemonic centres has ranged from the apologetic to the foolishly defiant, it seems to me that we have moved beyond the era in which anthropologists of the periphery (read, in most instances, the colonies and post-colonies) are simply clones of Oxbridge, Paris, Columbia, Berkeley, Chicago and Harvard. Many of these anthropologists have gained a voice of their own and a perspective that we cannot ignore. Still there are sensitivities. I remember giving a lecture in 1988 at the International Congress for Anthropological and Ethnological Sciences in Zagreb in which my mention of the loss of influence of the hegemonic centres of anthropology on world anthropologies elicited an immediate negative response from a group of Pacific Island anthropologists who thought I was questioning their ability to participate in mainstream anthropology. That very few American anthropologists attended this meeting, I should add, was taken as a sign of American anthropology's indifference to other anthropologies.

I have to take this observation seriously. One of anthropology's virtues is the hearkening to the voice of the Other. We do not – we are not supposed to – impose our ways of seeing things on those we study. Rather, we are meant to listen to and observe them with minimal interference. And I believe most of us try, as best we can, to carry out this impossible task, even in our new settings. I will have more to say about this below. Here I want to stress that it is one thing to hearken to the voice of the Other in the field, that is, in a circumscribed situation that, despite the effect of the participation of our subjects, is largely our construction, and quite another to listen to representatives of other societies in other situations, say, among colleagues with different ways of seeing the world and different empowerments. Frequently, despite ourselves, we treat

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3 A 'pluralisation' of anthropology is also referred to by Jebens in the present collection.

them with a certain condescension, or perhaps more disturbingly simultaneously as colleagues with whom we can freely converse and as representatives of the societies from which they come, that is, as informants. This crude symptomatising stance is offensive and can lead, as I have sometimes observed at international meetings, to their near-breakdown, certainly a loss of colleagueship, which is difficult to repair. One of the most egregious examples of condescending dismissal occurred at the Zagreb meetings in which one of my well-reputed colleagues, now dead, turned to me after listening to a Japanese physical anthropologist discuss the power of *chi* (*ki*) in his discussion of Japanese martial arts, and loud enough to be overheard, called his approach hogwash or something to that effect. He never bothered to ask what the Japanese anthropologist was trying to say, what he was struggling with conceptually, and how he might be calling attention to a dimension of understanding that fell outside his own paradigm.

My worry is addressed to the insistent parochialism of the anthropologies of the centre. Here I will speak of American anthropology from – inevitably – an American critical perspective tempered by my often prolonged encounters with 'other' anthropologies primarily from Europe, Canada, Brazil, and South Africa. Despite its national and international meetings, American anthropology tends to be turned inward, principally addressing American colleagues and those few 'foreign' anthropologists who have done research in their area of specialisation; that is, if they write in English. Looking at the bibliographies in most books and articles published by American anthropologists, one is immediately struck by how few references are in languages other than English. Looking at the syllabuses of graduate (let alone undergraduate) courses and seminars, one rarely sees a reference to any but works in English. I have read ethnographic studies of Italy, Brazil, and Mexico in which there is not a single reference to a work by an Italian, Brazilian or Mexican ethnographer. This is, no doubt, a product of the United State's stubborn monolingualism, but it is also the result, I suspect, of a sense of academic superiority. It certainly reflects the prevailing attitude of superiority held by most Americans and their displays, however bankrupt, of diplomatic, military and economic power. We are, after all, at anthropology's 'cutting edge'.

I must confess that, whenever I hear the phrase 'cutting edge', I think less of a frontier of knowledge than of the aggression that lies behind a singular approach to knowledge, research and innovation. When applied to a discipline like anthropology that relies on intimate relations with informants, it is especially disquieting. What is so extraordinary about this stance is that it is never quite clear what that edge is. Does it edge on what lay before it or on what lies ahead? Is it simply dismissive of the past, the fact of its pastness meaning it is no longer of interest? It reflects not only a particular historical stance – a stance that is not necessarily shared by colleagues elsewhere, say, in Germany, where far greater attention is paid to anthropology's past. But it also reflects an idea of progress, thought dead by many but still operative, which seems at once specific in its immediacy and so open-ended that it is impossible to define its horizon.

I speak here not only of anthropology's disciplinary goals, but also of those of the individuals who engage in it.

We cannot escape parochialism, but we ought to acknowledge it and reflect critically on its implication. We have to ask, for example, to what extent our particular parochialism is a defense against the challenges posed by both our informants and other anthropologists.[4] We have to consider the blinkers – the closure – that parochialism promotes, the isolation it can produce, the epistemological terror that may result from that isolation. In her book on the religiously conservative Women's Mosque Movement in Cairo, Saba Mahmood describes the effect that working with women, whose views she found repugnant when she began her research, had on her own outlook (2005:38). She declares that one of the aims of her book is 'to parochialise those assumptions – about the constitutive relationship between action and embodiment, resistance and agency, self and authority – that inform our judgments about nonliberal movements such as the mosque movement' (Mahmood 2005:38). Though Mahmood does not take critical account of the 'parochialism' of her categories – a c t i o n , e m b o d i m e n t , r e s i s t a n c e , a g e n c y , s e l f and a u t h o r i t y – her aim is well taken. Expressed in a different language, it has been one of the principal goals of anthropology.

By personalising her reaction, however telling that personalisation may be, Mahmood side-steps what I believe is a singularly important dimension of anthropology: namely, the critical perspective, the self-reflexivity we are in a position to offer (and inevitably do offer) for better or worse the people with whom we work. We may have been over-protective of, indeed have infantilised, our informants in the past when we were dealing with simple, isolated peoples who did not share, so we supposed, our worldliness: but however justified that stance was – personally I find it demeaning – it can no longer be adopted, as we work in complex societies and in marginal ones which are informed and influenced by them. We have, as I have said, to reckon with the voices of those we study and the critique of us presented by those voices, whether at a mundane political level or at a deeply philosophical one. Yes, there has been much talk about dialogical anthropology – I have done it myself (Crapanzano 1992:Part Two, especially Chapter Eight) – but the sense of dialogue that is promoted seems to be o u r construct and rather saccharine. Dialogue always has a critical edge, however masked by *politesse*, which has to be acknowledged and even cultivated if it is to be – I hesitate to use the words – sincere, authentic and creative. I think the failure of the 'writing culture movement' to consider the critical dimension of dialogue was and still is symptomatic of an implicitly hierarchical stance in anthropological engagement – in our parochialism.[5]

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4 I should note that ethnocentrism is not the same as parochialism, for it may be a component of concern for parochialism: an ethnocentrism, for example, that is unaware of its ethnocentricity. Of course, we might well argue the same for parochialism.

5 It is also symptomatic of the stress we give to the referential rather than the indexical function of language. In a most insightful article, which has been largely ignored, Jane Bachnick (1987) demon-

Anthropology is caught between the openness to the world of those we study and the closure promoted by parochialism. How can we be at once open- and closed-minded? No doubt there are many ways. There is no end to the ingenuity with which human beings accommodate themselves to contradictions in their outlook. One way, which seems particularly relevant to anthropology and to which I have already made reference, is the framing of an endeavour. What we do in the field, what we tolerate, what we listen to and observe, how, in short, we respond to the field situation is determined by the way we frame it, how we bracket it off from our everyday experiences 'back home' or in off-moments in the field, and how responsive our informants are to the terms of engagement we bring to them. We are rarely invited to the field by the people we study. We are rather more like uninvited guests who hopefully, once welcomed, behave with consideration and perhaps even offer our hosts something of value: friendship, perhaps; money; insight; contact with an outsider and the outside, and the advantages this may bring; entertainment; a comic interlude; an escape from boredom; a critical perspective; an opportunity to be irritated and the mastery of that irritation; and – a gift that needs elaboration – counter-ethnography.

It is not only anthropologists who learn from the encounter but also the people with whom they work. It has often been noted that the best of our informants learn to adopt an ethnographic perspective on their own society. It differs from the ethnographer's if only because they do not have his or her anthropological background or distance. They may, however, suffer a painful alienation – a *Verfremdungseffekt* – that has been the source of anthropological anguish. The alienation mirrors, in many respects, the alienation that fieldwork produces in the ethnographer when he or she returns home. But what has received far less attention is what I am calling an informants' counter-ethnography: the eye they have on the anthropologist as a representative – a source of knowledge – of the anthropologist's society and culture.[6] However defensive this counter-ethnographic stance may be – after all, informants have to protect them-

strates how by considering the indexical play in dialogue, we are able not only to capture its progress – a case in point, the movements of deference and distance – but can come to appreciate the way in which interlocutors are included in each others' views. Monitoring indexical switchings (of honorifics in Japanese, in Bachnick's example) allows the ethnographers to appraise their position within the cultural universe of their interlocutors and, presumably, those interlocutors' appraisal of their position in the ethnographer's world. Focusing on the indexically constituted intersubjective dimension of dialogue, Bachnick argues, enables us to avoid problems stemming from the textualisation of dialogue understood in referential terms. Bachnick does not, however, recognise the role that a meta-indexical language, inevitably formulated referentially, plays in the understanding of dialogue by even its participants. See Crapanzano (1992:115–135).

6 Kevin Dwyer notes that the *faqir* with whom he worked had a 'certain "anthropological" perspective' (1982:230, fn 23). Stoller (1987) and Bachnick (1987) in their different ways have considered the way they were conceived of by the people they worked with, but did not appraise those informants' views of their own culture. – Cf., in this collection, Jebens's reference to indigenous ideas or constructions of 'being white' or of 'whiteness'.

selves from the challenge of t h e i r insistent, at times intrusive Other – it is not without its effect on the anthropologist, the progress of his or her research, and on the interpretations he or she makes both in the field and back home, even years after the research was completed (Crapanzano 1973).

The field situation, especially in foreign cultures or unfamiliar settings, lays bare dimensions of ordinary social encounters that, in their ordinariness, are usually ignored. The ethnographic encounter – at least in its initial stages, before it has become routinised – has for all parties an effect that is not dissimilar to the transformation that Heidegger attributes to conspicuousness (*Auffälligkeit*), obtrusiveness (*Aufdringlichkeit*) and obstinacy (*Aufsässigkeit*), to a break from the way the world usually presents itself (Heidegger 1993:72–76). The world, or more accurately objects in the world, can no longer be taken after such a break instrumentally, as tools (*Werkzeuge)* ready-to hand (*zu-handen*) but in a mode of disclosure, as presence-at hand (*vor-handen*).[7] Among other dimensions of interpersonal engagement, what is revealed in the ethnographic and other exceptional encounters is the terror we experience when we are forced to acknowledge the impenetrability of the mind – the thoughts – of the Other. We are no longer protected by habitual social and communicative conventions from the recognition of this impenetrability and its emotional consequences: we are not only confronted with the opacity of the Other, with that Other's penetrating gaze, but also with our own opacity, its vulnerability, and the impotence of our own gaze.

This is perhaps one of the reasons we have ignored the counter-ethnographic stance of our informants. I remember the sensation that Kevin Dwyer's "Moroccan dialogues" (1982:217–223) produced because he asked one of his informants, a *faqir*, what he thought Dwyer was doing, what he thought Dwyer thought of him, and what he thought of Dwyer. It was clear that the *faqir* was embarrassed by the questions and did his best to avoid answering them. They certainly ran counter to Moroccan etiquette, at least as I know it. When Dwyer asked him whether he had ever suspected Dwyer and his project, the *faqir* answered, 'If I reach the point of getting together with someone many times, it means that I no longer have any doubts'. Dwyer (1982:230) pushed him by quoting a Moroccan proverb (one that resonates with my focus on the impenetrability of the Other): 'One third of what is unknowable is inside men's heads'. The *faqir* answers, 'I don't have any doubts about you. My mind tells me, and my heart tells me, that between you and me there is no longer any suspicion'. He adds that he behaves in good faith but can't rely on Dwyer's (or anyone else's good faith). 'Because your good faith isn't going to benefit me, what benefits me is mine. So I have to struggle with myself to make mine good, and I don't struggle to make yours good'. It is God who will judge Dwyer's. Though one might consider the *faqir's* indifference to Dwyer's good or bad faith an expression of hostility (as at some level it probably was), it is also an affirmation of the

7 To be sure, their instrumentality is not lost but understood from a different perspective.

faqir's moral stance, his discipline. He answers Dwyer's questions, which, as he said earlier in the interview, are of no concern to him, because they serve Dwyer's purposes. They may test the *faqir's* good faith.

What is striking about Dwyer's questions and the impression they made on many of his readers is their naiveté. They assumed (at least I assume they assumed) that the *faqir* or anyone else would answer the questions in a straightforward manner. But, as the *faqir* surely knew, if one is forced to characterise oneself to someone else, that characterisation has to be judged as an expression of how one wants to be characterised (Crapanzano 1992:91–112). When Dwyer asked him what he thought Dwyer thought of him, the *faqir* answered, 'You're the one who understands that. Why am I going to enter into your head?' (1982:219) To me, at least, Dwyer's interview breaches not only the conventions of each participant's communicative etiquette, but also, no doubt, the idiosyncratic conventions the two men had worked out over their many encounters before this last interview. Dwyer calls attention to precisely what has to be ignored if an exchange is to be successful – namely the opacity of each of its parties. Of course, my stress on the terror of impenetrability reflects an epistemological tradition that is haunted by solipsism. By stressing both mind and heart in telling himself that he can trust Dwyer, the *faqir* may well be calling attention to a possibly more confident mode of knowledge of the Other – through the heart – that is less susceptible to the threat of opacity. Whatever cognitive function the heart (*qalb*) may have, if it has any, it is perhaps not so very different from the way in which the body and embodiment have come to function rhetorically in the human sciences in recent years.

Dwyer's observations, his *méconnaissance*, the simplicity of his question, and the startle it produced among anthropologists when his work was first published in 1982 reflects the 'Malinowskian' moment in anthropological research.[8] It never occurred to me (nor, I imagine, to most anthropologists) not to try to find out what my informants thought of me and my research, but I – we – did it through indirection, just as I am sure our informants do in trying to discover what we think of them. I should add that, in my research with white South Africans during apartheid, with American Christian Fundamentalists and original-intention lawyers, and even in my most recent research with the Harkis, my informants often made it quite clear what they thought of me and my research. Sometimes they were friendly, sometimes dismissive, fortunately rarely hostile. I make it a practice to discuss with the people I work with, whenever possible, how

8 I am using 'Malinowskian moment' here to call attention to the absurdity of reifying and detemporalising a practice, as George Marcus has done in his discussion of the reflexivity required by anthropology's new research domains (2006, 2008, among his other recent works). Anthropologists may have idealised Malinowski's fieldwork and modeled their own research on it, but that model has a history that anyone attuned to (self-)reflexivity has surely to recognise. Not only has fieldwork, as responsive as it is to the field situation, 'deviated' over the years, but so has the reading of Malinowski's work. Such a history has yet to be written.

they would go about doing my research. Some of these discussions have been among the most insightful from an ethnographic point of view.

At this point, I want to address the ends – the future – of anthropology. I do not want to idealise the discipline nor give it a significance it has never had and probably never will have. It is a field of study that has prided itself on its unique methodological stance – a stance that incorporates a wide range of research strategies that are often at odds with one another or, better, whose advocates are often at odds with one another. I do not want to enter into the specifics of these conflicts: they require critical historical reflection that centres on their arenas of contestation, most notably the university, its affiliated institutions, and funding organisations. I should, in fact, pluralise 'university' since there are dramatic differences in the structures, evaluations, styles, support and roles of universities around the world which are among the most significant determinants of the field. Though there have been a few anthropological studies of anthropology, like Mariza Peirano's (1995) of Brazilian and Indian anthropologies or those collected by Thomas Hauschild (1995) on German anthropology during the Nazi era, it is striking that a field that claims to be as critically self-reflective as anthropology and as sensitive to the formative power of institutions has not, to my knowledge, explored in any rigorous and historically sensitive way the relationship between the structure of the university and other relevant institutions and the manner in which anthropology frames, theorises, and conveys its subject matter. Let me be clear – I am not referring to those simplistic postulations such as 'anthropology is the handmaiden of imperialism', without demonstrating in detail what that relationship is and how it has affected anthropological practice, including, importantly, its pedagogy and its consequent theorisations (Hymes 1971). Nor am I referring to the writing culture movement, which, for the most part, was concerned with textual analysis.

Of course blinkers blind the anthropologist to the effect on his or her discipline of the university, the political and, yes, politics. The latter was, in my experience, true of hiring practices in France at least during Mitterrand's presidency. It is my impression that, after a conservative government has been in power for several years, American anthropology takes a positivist – a scientistic – turn. My observation is casual and requires proof. Whether right or wrong, it does call attention to the need to investigate the relationship among anthropological practices, prevailing political currents and mediating institutions like funding agencies. Our discipline is probably not unique in its failure to subject itself to the same scrutiny as it does to its ostensible subjects of investigation.

Like many other academic disciplines, anthropologists are rather more concerned with the responses of their colleagues to their research than to the way that research circulates and is made use of outside the discipline, the university and the scholarly community at large.[9] We have paid scant attention to how anthropology's findings are

[9] I am indebted to David Harvey for calling my attention to this lack of concern and what it says about anthropology.

used in marketing, advertising, journalism, travel guides, tourism, religious services, law, diplomacy, the arts and their promotion, theater, propaganda, policing, and government policy, though we have expressed concern about their use by the military, the CIA, the FBI and other intelligence gathering agencies and anti-terrorist (and perhaps even terrorist) organisations. I remember an agent of the CIA who tried unsuccessfully to hire me complain that of all academics anthropologists were the most difficult to recruit for 'government service'. Whatever we think of the CIA, his observation merits consideration. Why are we so inwardly turned, so indifferent to the use made of our work, except when it proves detrimental to the people we study or is made by organisations like the CIA whose activities we disapprove of and which run counter to our moral responsibility to our informants?

Critical reflexivity seems particularly important, since anthropologists have begun studying not only marginalised groups in complex societies, but also institutions and networks in globalised and globalising societies which had never been – indeed had never even been imagined as being – in anthropology's purview. The subject of these studies ranges from derivatives and other 'new' financial instruments to human rights and the legal institutions that support them; from insurance companies to hospitals; from gated communities to refugee camps; from traffic in human bodies and body parts to NGOs in war zones; from theatre groups to missile defense systems. In other words, anthropology can no longer be limited to the tribe or village, economic anthropology to the 'stone age', or legal anthropology to tribal councils. Perhaps it ought never to have been. Today it is near-impossible to find societies in which such 'traditional' approaches can be applied with any legitimacy. I am certainly not the first to observe this. Nor am I the only one who suffers a certain nostalgia for their possibility, but such nostalgia should not be exempt from critical regard, for it may well hinder the development of new research strategies demanded by our new domains of research. (I will return to this below.)

George Marcus (1995, 2006, 2008), ever ready to hail a new wave of anthropology – 'second-wave reflexivity' he calls this one – argues that anthropology's new field sites require new methodologies founded on a reflexivity that becomes 'the key means or operation of determining new forms and norms in the evolution of the multi-sited ethnography'.[10] Marcus's description of the new sites, however generalised, is well taken, though he gives, in my view, too much weight to experts and science studies. He suggests that there are three operations of reflexivity that define his new kind of ethnography: (1) in the materialisation of the object and space of study; (2) in defining and managing collaborative relationships within fieldwork; and (3) in the politics of reception of the study. The first of these demands monitoring (a) the role of (initial) personal contacts in constituting the field, and (b) the evolution of the sites of field research in

[10] On 'multi-sited ethnography', see also the contributions by Godelier, Jebens and Kohl in the present collection.

which informants play a more active role in that evolution. The second focuses on the need to keep track of the researcher's relationship with experts who are now taken as collaborators. Finally the third appraises the role of a new readership that extends beyond the anthropologist to the research subjects themselves.

There is no doubt that the new field sites require reflexivity, but, I must confess, the reflexivities that Marcus describes have played an important role in more traditional research. Anthropologists have always had to consider the role of those who introduced them to the field site in the constitution of that site. Psychoanalytic anthropologists, working in 'traditional' settings, have, for example, insisted on the importance of monitoring one's entry point in the field (Hunt 1989:29, Kracke 1987). Anthropologists have always had to monitor the progression of their research and the determinants of that progression. They have always collaborated with their informants, some of whom take on the position of the expert. I am certainly not unique in having discussed my work with my informants and asked them for procedural advice. The writings of anthropologists, like those of all writers, have always been influenced by images of their readership, including, however fantasmatic, even their illiterate informants. Clearly, however, the anthropologist's writings will be read in a different way by his or her collaborators: namely as a contribution to their purportedly joint endeavour.

The relations between anthropologists and their 'collaborators' present problems that traditional fieldworkers did not usually have to face, but these relate not only to the complexity, fluidity, and multiplicity of research sites, but also to the authority, confidence, class, and privilege of those collaborators. More important, as Marcus (2008) recognises, is the co-planning of research projects and jointly seeking funding. What has to be considered, however, is the function of the anthropologist in such collaborations. Are they actively contributing to the stated goals of the research? Or are they proffering a reflexivity (or the illusion of a reflexivity) that may or may not contribute directly or indirectly to those goals? Serendipity, rather more than systemic programming, is at play here.[11] Or, most cynically, do they serve simply as decoration for funding? What is required here are rigorous studies, rather than off-the-cuff pronouncements of the interaction of the anthropologist and his or her collaborators. To me, the most significant contribution Marcus makes is his stress on the 'within' (or between) from which the field is constituted. His greatest weakness is his failure to consider the defensive role that disciplines and institutions play in the 'evolution' of research. That anthropologists have not always proclaimed their reflexivity does not mean that they have ignored it. It is rather their mode of critique that demands scrutiny.

Critical reflexivity is of singular importance – perhaps not so singular – in a discipline like anthropology which straddles different cultural and social traditions, producing thereby an instability and fragility that seem to demand correction. It is not

11 On the importance of serendipidity, see also the contribution by Spyer in the present collection.

altogether clear to me why instability and fragility should demand correction. They have their virtues, just as straddling does, despite the groundlessness or, perhaps more accurately, the illusion of groundlessness it produces. It is, in fact, this straddling that lies at the heart of anthropology, and it merits far greater epistemological reflection than it has received. We have been rather too content to decry the pain, the confusions, at least, that the conceptual gymnastics of such a position requires than accept the challenge it poses.

Remember, for example, the hue and cry – the mud-slinging – that surrounded the now-fading declarations of postmodernity, and especially postmodernist approaches to social and cultural reality (if indeed 'reality' were to have a referent). There is no doubt that postmodernism was a conceptual fad just as globalisation has become one. The fact that it has had its foolhardy enthusiasts, who delighted in Nietzsche's play without ever recognising his seriousness and his deep moral concern, or in declarations that all the world's a text or a mess of simulacra does not mean that 'postmodernism' does not challenge some of anthropology's time-worn conceptual apparatuses.

Despite initial resistance, deconstruction (which, strictly speaking, should not be confused with postmodernism) has not been without its effect on contemporary anthropology, if only by passing through the 'defiles' of postcolonial studies. It is no longer possible to assume without question the totalizations that lay behind the great master narratives that concerned themselves with psyche, history and society, or to ignore the fact that all power is institutionally lodged. Aside from its incorporation of notions like hybridity, the subaltern, heteronomy, and the simulacrum, contemporary anthropological theory and ethnographic description are far more sensitive to the fissures, fragments, disjunctures, transgressions, paradoxes, aporiae, the in-between, the liminality and the multi-perspectivalism of socio-cultural life.

However tempered by disciplinary conservativism and the allure of simpler conceptions of society, these changes have ethical as well as epistemological and observational import. Think, for example, of Homi Bhabha's reconfiguration of 'cultural difference' (1990:312–315). He notes that, although the conceptualisation and consequent policy of multiculturalism serves the interests of the dominant, insofar as it acknowledges socio-cultural difference it opens up a space of resistance for the marginalised. He refuses to understand cultural differences in terms of their eventual assimilation into the dominant culture. 'The question of cultural difference', he writes, 'faces us with a disposition of knowledge or a distribution of practices that exist beside each other'. It does not surmount 'the space of incommensurable meanings and judgments that are produced within the process of transcultural negotiation'. Put rather more simply than Bhabha, if I understand him correctly, the marginalised hold their position – their cultural assumptions – as do the dominant in negotiations and accommodations not in the 'space' of the simple contestation that arises with essentialist stereotypes of each other, but in an 'in-between' of identificatory interdependence that operates in both conscious and unconscious ways.

Bhabha's language is obscure. His argument, moving indiscriminately from one conceptual level to another, is inconsistent if not contradictory. He often fails to distinguish the descriptive from the prescriptive. But his critique of assimilationalist goals and essentialist characterisations of the Other, as well as his acknowledgement of the interdependence of each party's identity in contestatory situations, does demand a rethinking of the binary thought – the coloniser versus the colonised, the host versus the guest, the owner versus the worker, the powerful versus the powerless – that has characterised so much of our social thought. We have, of course, to ask why the ever-shifting interstitial has become so attractive, especially to the formerly colonised; why they are so attracted to the 'perpetual critique' of Derridian thought; and why they move so promiscuously from one mode of conceptualisation to another. In part this is a result of the paradoxical situation in which the postcolonial intellectuals find themselves. They are caught between often dramatically different audiences and audience phantasms, each of which makes different and often conflicting demands on them. They are also weighed down by theoretical paradigms that, in speaking in different ways to different people, put their own identity in question and thereby the possibility of a stable vantage point. Gayatri Spivak (1985) writes of the silence, the voicelessness, of the subaltern and in so doing speaks for them. But how can she? With what right? In what language? She has to deconstruct – 'destabilise' is perhaps a better word – as she writes. She is caught in the midst, as are Bhabha and countless other intellectuals who attempt to speak, to represent, those whose language they themselves do not know in a language that is not even their own but that of the former coloniser – one that is philologically weighted by domination.[12] They, too, have lost their voice as they voice and ventriloquise vociferously. This is more than an epistemological conundrum: it is a seemingly irresolvable moral dilemma – certainly less acute than that of those of whom they speak, but who cannot, so they say, speak for themselves.

Bhabha may write of the negotiations that occur in the space of the juxtaposed, but he offers no concrete picture of how such negotiations would proceed. He fails to give full recognition to the possible, indeed the likely role of power – brute power – in overriding 'incommensurable meanings and judgments' in 'the process of transcultural negotiation'. He has, of course, been criticised, for his failure to produce hard evidence for his argument. Who is to say that the marginalised don't want to assimilate? That is an empirical question, one that anthropologists could and have, in fact, answered. But, as I have already noted, Bhabha, like other postmodernist and postcolonial intellectuals, conflates the descriptive and prescriptive, a conflation that offers them an illusory but rhetorically potent means of escape (see Crapanzano 1991). To condemn them on

12 By 'language' I am not referring simply different languages as 'language' is popularly understood, but also to languages which share the same features but are connotatively weighted in different ways: by class, gender, age, wealth, poverty, hegemonic position, authority, lack of authority, power, powerlessness, experience, and history.

these grounds is far less interesting than to ask why they conflate the two. Are they offering a new mode of articulating the social? Though I am sceptical, I do believe that this conflation arises out of the interstitial situation in which they find themselves – one in which it is impossible to separate objective description from moral and political engagement.

There is an obvious parallel between the postcolonial intellectual's situation and that of the anthropologist. Both operate in the in-between. In the case of anthropologists, their interstitial position is voluntary – an artifice of their research – from which, despite all the alienation they feel upon their return home, they are able to depart, thus returning to the epistemological if not the ethical comfort of home. Though still concerned with the problem of how to mediate their culture of orientation with that of their research subjects, they are now able to bracket it off in a way in which, I suspect, the postcolonial intellectual, despite his or her privilege, cannot. They are, I believe, far less comfortable with the authorised positions the anthropologist is afforded: the theoretical, analytical and hermeneutical frames that his or her discipline validates in one manner or another, like science, for example. Still the parallel between the two highlights the difficulties associated with straddling.

It is important to note that straddling does not require equilibrium: the cultures the anthropologist and the postmodern intellectuals straddle never have the same significance. The culture of orientation, however contorted, hybrid and ill-defined it may be, always has greater weight than the culture under study, though the latter, in so far as it challenges the presuppositions of the former, may be more forceful in effect at the time of engagement. We come to the encounter with what Hans-Georg Gadamer (1985:235ff.) calls prejudice and fore-understanding (*Vorverständnis*), which we have to bracket off or open to question as best we can so that they do not predetermine our take on what transpires during the encounter. Engagement and interpretation are temporal processes that are arrested from time to time through reflection, summation, evaluation, judgment, and decisive action. At such times straddling gives way to taking a position, ephemeral though it may be. The culture under study, as well as one's own, is objectified, detemporalized and exoticized. The in-between gives way to polarisation.

Is it possible to found a body of knowledge of theoretical or practical import from within the interstitial? Is it possible to develop a meaningful ethics from within the in-between? Or are we forced to disengage ourselves from that position and accept the reductions and distortions that come with that move? I cannot answer these questions, which are, in any event, rhetorical. But I would like to suggest that one way in which an anthropology of the future can respond to some of them is by stressing the temporal dimensions – note the plural – of social life, including anthropological fieldwork. I am certainly not the first to note the extent to which space and its metaphorisations ground (!) social and cultural description. It has constituted the way ethnographers construct the field! It delimits context, even historical context through placement, which may serve to arrest time. As we begin to study complex societies, institutions whose particu-

lar locus is of little impress, given their spread around the world, and networks whose positionality is precipitated by intervention – usage, interpretation, static or breakdown – which may itself be spatially without location, how are we to carry out our fieldwork? Multi-sited ethnographies may be an answer to a few of these new research domains (!), but they are still sited.

In a recent paper, the Argentinean-Brazilian anthropologist Rita Segato has argued for a new conceptualisation of territoriality, one which is defined by networks (rather than specific locations), biopolitics and specularisation (2008:204). She is writing of new patterns in contemporary religions that escape our attention because of the focus on secularisation and religious mobility. She suggests that it is the body that bears participating identity in a network, and that 'competing networks suffocate and stress their unity vis-à-vis other networks by the management of bodies as emblems of belonging' (Segato 2008:210). This new territoriality treats space the way it treats bodies. It is possible to speak here of bodies in their behavioural space, since the territory becomes the outcome of the presence of the plastic human web, imprinting its traces as it expands or consolidates its existence under a new territorial paradigm. Segato's argument is complex, and I am not doing justice to it here, especially with respect to her take on the new ways governments are forced to adjust to these transnational networks. I simply want to illustrate one attempt to offer a new paradigm that breaks with traditional notions of space and territory.

My own approach is rather different. I would suggest an anthropology of the occasion. By 'occasion', I mean a constellation of occurrences that are not yet articulated as an event, which occurs somewhere, in virtual reality even, but whose effect as it spreads may render its site of origin insignificant or simply an icon of its effective spread. By insisting on its location, we may well blind ourselves to that spread – to its radiant effect.[13] The icon – which paradoxically serves, in a counter-movement, to re-affirm the original event – has its own history distinct from the spread, but not without effect on that spread. The (inevitable) translation of an occasion into an event produces its conformity to prevailing takes on the world or, usually within conventional limits, transgressions of that take. In either case the translation is subject to political manipulation. So powerful, so habitual, so culturally and linguistically determined is 'eventing' that my separating the event from the occasion has to be considered an artifice for drawing attention to its socio-political implication and manipulation.

I cannot develop the notion of an anthropology of occasion here. But I do want to discuss in a far too general, too idealised fashion the temporal movement internal to one such occasion, in fact, a coalescence of occasions, namely, fieldwork. It seems to me that that the pictorial quality of most ethnographic description leads us to ignore or dismiss

13 Compare Derrida (1967). He is, of course, writing about structures and not networks, but the centring effect on the structure, its re-articulation, is pertinent.

the effect of the complex temporalities of straddling – of living in, or living as though one were in, the in-between – of anthropological research and the conclusions we draw from it. Fieldwork cannot be reduced to a single practice or held to a single perspective. There is dramatic development in its pursuit, and this development is primarily a result of the exchanges that occur in an arena that is spatially demarcated as the 'field'.

I use 'arena' here, not to stress the contestatory nature of fieldwork – though, as we shall see, it can be of singular importance – but to focus on the changes that occur both within its formal features and to the modes of engagement of all parties to it. These developments affect the mindset of the participants, but, given the opacity of the mind of the Other, we have no unmediated access to their effect, and, given our own immersion in the exchanges, only limited and presumably distorted access to our own. This is not to say that these presumed changes in mindset do not have an effect on the progress of our encounters in the field: on the contrary, they provide a somewhat anxious horizon to our understanding. We presume that what our informants do and say is always accompanied by what I have called shadow dialogues (Crapanzano 1992:213–215) – silent, internal, usually quasi-articulate evaluative conversations they have with themselves and with us. There is then at least a double movement in the encounters: one which is perceptible to the participants and one which is not, though any of the participants may 'intuit' it by reading the perceptible movement as symptoms of the silent thought of the Other.

Since it cannot be fully identified with the shadow dialogues because it is focused on and in the manifest dialogues, reflextion requires a move – if I might use Kantian aesthetic vocabulary – from interest to disinterest. The anthropologist's engagement in the field demands interest, for otherwise he or she would not be able to engage. It is purposeful (*zweckmäßig*), in fact multi-purposeful, for the anthropologist has to take an active interest in whatever is being pursued and its research import. Reflection, like aesthetic contemplation, requires a disinterested stance at least toward the immediate transactions but, unlike aesthetic contemplation, governance by the research purpose. Disinterest does not mean indifference or distancing, nor does it imply that that its object is uninteresting, as Kant (1990:41, fn) noted in a footnote in his discussion of the beautiful. Obviously what is interesting is determined by both the quality of the object, its allure and the purposeful orientation of (secondary) reflection. Theodor Adorno argues that disinterest, if it is not to become indifference, 'must be shadowed by the wildest interest'.[14]

[14] Adorno (1997:11). Unlike Kant, Adorno stresses the fact that works of art necessarily evolve in a dialectic of interests and disinterests. He argues that for Kant the aesthetic becomes 'a castrated hedonism, desire without desire' (1997:11). This is not the place to pursue the role of desire in the formulation of the 'ethnographic', but it certainly merits a critical investigation that was entirely missing from Geertz's and Clifford's pathetically facile notions of ethnography as fiction, as fiction-making. We might well consider ethnography by analogy with Adorno's observation that '[t]here is no art that does not contain in itself as an element, negated, what it repulses': t h e r e i s n o e t h n o g r a p h y t h a t d o e s n o t c o n t a i n i n i t s e l f a s a n e l e m e n t , n e g a t e d , w h a t i t r e p u l s e s .

It seems quite obvious that interest and disinterest are not simply isolated attitudes, as Kant assumed, but are embedded in the complex social and cultural surround and subject to the constitutive plays of power and desire in that surround – plays of power and desire whose import can only be grasped through consideration of their extension over time and how they punctuate that temporal stretch. The temporal flow of field research and its aftermath (not to mention its preparation) is punctuated by the oscillation of interest and disinterest, purpose and purposeless purposefulness, unreflective engagement and reflective disengagement, and, most importantly, the witting and unwitting accommodations to the empowered and empowering demands of each of the symbolically vested interlocutors. Disinterest, purposeful purposelessness, reflection and accommodation serve to arrest time and in so doing enable the static pictures we draw, interpret and explain. There is no way to avoid these arrests, but when, why, and how they are carried out and how they are ideologically supported merits continual monitoring. This attention has to take account of the ethical, political and epistemological consequences of the arrests, and indeed the letting-flow.

Any anthropology of the future will have to engage with ethical questions that extend far beyond the ethics of fieldwork. I do not wish to deny the importance of the ethical dimension of field research, but I think we have to ask why we have so often been content with delimiting our ethical concerns to so tiny a domain.[15] Is it an evasion? Discussions of moral relativism in cultural relativistic terms are also evasive insofar as, in their generality, they avoid concrete situations. Today these evasions are no longer possible, if only because our informants will no longer let us make them. Yes, we have to hearken to their voice, but – and this is important – we have to probe our own moral values before we either accept or reject their position. By position I mean manifest ones, like wearing the veil, stoning homosexuals, ignoring the lives of thousands devastated by natural disaster because the lives of the masses are thought less significant than the maintenance of power or perhaps even a game of golf, respecting national boundaries and sovereignty despite what one believes to be heinous practices, or invading countries to foster one's own values, like those of democracy American-style or of one religious fundamentalism or another. I am also referring to the underlying epistemological assumptions of moral outlooks, like the separation or non-separation of description from prescription. We cannot simply look to an ethics of practice, essentially a descriptive one, which does not take account of serious cross-cultural differences in practice.

There are no easy answers to these problems. I certainly don't have any. I do want to note that some of them arise from – or are at least foregrounded by – the way we

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And we might relate this to anthropology's moralistic stance towards its subject matter. Think of my discussion of Mahmood in this respect.

15 I am not considering here the ethico-political position our professional societies take in their pronouncements and lobbying, if only because they require far more attention than I can give them here. Central to them is human dignity and the rights that follow from it.

frame our research. We have been wedded to a field methodology that has given preference to supposedly objective observation, that is, to an observational mode that demands minimal interference from the observers. There are virtues in this approach, but it should not be fetishised. While there is a time for this observational perspective (provided that it is treated with a certain scepticism), there is also a time for a more critical, more argumentative approach. Agonising over the moral dilemmas posed by one's reaction, say, to the Woman's Mosque Movement when that movement is described in a way in which critical engagement – in conversation and debate – is either avoided or eliminated in its presentation is, in my view, a morally dubious reaction to the objectification of the movement. I am not denying the ethical problems, but rather the way in which the anthropologist, in adhering to the objectivistic methods condoned by the discipline, has failed to provide a sound basis for such agonising. It seems to me that we owe the mosque women and ourselves the opportunity to engage in a respectful debate with us about our respective beliefs and practices. (As researchers, we have of course the luxury of not coming to a decisive conclusion.) Apologetics are always addressed to an opponent. Moralising agony aside, apologetics itself, as it is displayed in such engagements, is certainly a social fact. It can best be elicited through critical encounters.

I certainly do not want to deny the delicacy of carrying out ethnographic debate. Timing is of the essence. It took me months of research with American Christian Fundamentalists before I felt comfortable enough to engage in a critical conversation. I was interviewing an elderly professor of New Testament theology who had just completed an enormous commentary on Revelations. He was a gentle, understanding man, warm but not particularly charismatic, who had to cancel our first appointment nearly a year earlier because of an emergency heart operation. I could not help thinking that his confrontation with death had given him a wider perspective than most of his colleagues. I told him that one thing that troubled me about evangelical Christianity was its focus on Christ's Second Coming: it seemed to ignore His First Coming and His message of love. The professor was startled by my observation. He remained silent for what seemed to me ages. The room darkened for me; he suddenly seemed frail and very old – vulnerable. I regretted my question and was sure that I had hurt him deeply. Finally he spoke. 'I've never thought of that. You may be right. I'll have to think about it'. The room brightened; the professor lost his frailty, his vulnerability, and became a man of wisdom, spiritual wisdom. Not only was I relieved by his answer, but I felt open to him, as I believe he felt open to me. From that point on I was able to engage in critical discussions with some of my informants. They were willing enough and, in fact, seemed relieved by my (our) change in style. These discussions were perhaps the most insightful I had.

I believe an anthropology of the future, particularly one that focuses on the anthropologist's own culture, risks losing this edge, which is fundamental, in my view, to the anthropological endeavour. It might be asked how essential this straddling will be to an anthropology of the future as the world homogenises, as anthropologists devote more and more attention to their own cultures. I believe it is essential. The anthropo-

logical stance rests on real or artifactual alterity and distance. It gives anthropology its particular angle on both the society under study and the anthropologist's. It serves as a corrective to unquestioned cultural assumptions and provides a 'basis' for social and cultural critique. It impedes the replication of a society's self-understanding, as is the case with so much sociology, by distressing that understanding, often, though not uniquely, by revealing its negative undersong. Anthropology has an important iconoclastic dimension. [16]

In the past it has been the exoticism – the often profound differences between the anthropologists' own culture and that of the people they were studying – that gave them an edge, or at least the illusion of an edge, on those people, their culture and by extension the anthropologists' own culture, their people. For those anthropologists who are not completely charmed by their own interpretive strategies and socially condoned explanations, that edge produces, ideally, a conceptual anguish that demands a critical rethinking of the categories and values prevailing in their society of orientation. It calls attention to the way in which the metalanguage of social and cultural description and critique refract and are refracted in their social and cultural understanding.

If Wittgenstein and the deconstructionists have taught us anything, it is that a metalanguage wholly independent of its target language is impossible. But – and to me this is perhaps anthropologists' most important role – we can t r o u b l e that language and its metalinguistic presumption. In so doing we not only call attention to the limits of our social and cultural assumptions, but may even open up other possibilities (though I must admit a certain scepticism in this regard).

Now, before I am called to account, I should note that I am not claiming that the edge produced by our engagement with an exotic culture – I use this inflammable word here and above purposely, to inflame – is not itself subject to the force of our hegemonic understanding and to our complex and often contradictory projective capacities. But it is safe to say that those exotic cultures resist (in the phenomenological sense) that understanding and those projections in a way in which our own culture and society cannot. Herein lies a serious danger: how do we evaluate the edge we have on our own society, the distance, the difference, the alterity we assume? Are they simply refractions of our own culture that give us the illusion of a critically independent edge?

From my first field research, I have been impressed by the social role of the trickster, as well as the metaphorical role that the trickster may have for suppressed dimensions of ethnographic research and interpretation. Over the years I have met many tricksters and have come to admire their savvy. They know, at least the best of them, that they themselves can be conned by their own tricks. They recognise, in effect, their artifice and the power of that artifice to deny its own artifice. They are caught not be-

16 On cultural difference, critique and the 'in-betweenness' of the anthropologist, see also the contributions by Godelier, Jebens, and Kohl in the present collection.

tween artifice and reality, but between artifice and artifice's denial of itself. They are in a position that is not unlike that of the anthropologist, who is caught, so it can be argued, between two artifices, that of her or his own culture and that of the culture under study. They have no firm footing. But unlike the trickster who is liberated by his or her savvy and takes delight, at times painful delight, in the plays it afford, anthropologists are often tortured by the complex straddling in which they find themselves. They straddle not just two or more cultures but two or more artifactual realities – call them social constructions if you prefer – that proclaim their reality as their contingent juxtaposition (brought about by the anthropologists' presence) disclaims that reality. (Their situation may even be more complex if the people they are studying, like certain Sufi mystics, or my hypostasised trickster, delight in artifice.) We may seek firm footing in what we assume to be reality – that is, in naive empiricism or positivism or a realism that we assume gives direct access to reality without our acknowledging that realism is only a style. But if the anthropology of the future is not to end in a deadening academicism that, however quickened by nostalgia, sentimentality and an elegiac sense of belatedness, is destined to repeat again and again its 'tried and true wisdom' – the uncritical litany of class, gender, race and ethnicity, for example – it must, I believe, reckon with its artifice and the ethical, as well as the political and epistemological consequences of that reckoning.

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