Introduction

Rediscovery and exploration

This book rediscovers the many-sidedness of the Manchester School and its continuing impact, through a method of social biography and related intellectual history. The approach focuses on each primary subject as a member of a circle of persons powerfully significant for one another and intensely engaged with a leading member.[1] This is the circle of pioneering social anthropologists around Max Gluckman, known as the Manchester School; and in many ways, having Robert Gordon's monumental biography of Gluckman has deepened my understanding of his engagement with them (Gordon 2018). My own book reveals that members of the circle engaged in deep dialogue, enduring friendships and counterposed creativity, their apparent intent being to collaborate and yet not to trespass, so to speak, on the others' perceived domains. If specially identified with distinctive developments, such as the extended case method and its application to the study of conflict resolution,[2] they were mavericks, who claimed, among themselves, not a unity in research interests or theoretical assumptions but evolving conversations across disciplines and highly personal directions. The rediscovery of the complexity of their engagement, as well as their impact, illuminates an exploration of the frontiers between ethnography, the sociology of knowledge and the anthropology of colonial to postcolonial change and of cosmopolitanism.

My early knowledge of the Manchester School came when I was a Brandeis University undergraduate about to study in the Manchester department, and in preparation I met Elizabeth Colson at

Boston University in 1959. She found the time to give me her impressions of each of her old colleagues, so varied that I came away with what she herself might have called a good gossip. It was a fine introduction not to a harmonious team following their leader, but to some highly individual, rather argumentative players, critical of each other, with their own characteristic ways and interests, often at the frontiers of the social sciences.

My early impression of social anthropology in 1959 came in the late colonial period, now known to historians as 'The End of Empire'; and I now write in a postcolonial moment that critics consider still calls for 'decolonization'. With that in mind, I want to make it clear, from the very start, that I accept that the present challenge comes at a time when the old question of 'The School', Manchester, British or otherwise, is again being urgently asked, along with another about 'the whole development of social anthropology'. How does this twenty-first-century development fit into or depend upon the great transformation from our colonial past into our postcolonial present? What kind of knowledge are we passing on?

In response, throughout much of this book, I will consider certain salient issues of the making of anthropology in colonial times, and then in the next to last chapter, I will address questions of anthropology and the postcolonial. Some of the questions are about the past critique of power and coercion and the aftermath of struggles for liberation. Even further, there is another significant part of the knowledge needed in our troubled times. This has to illuminate the vulnerability of anthropologists ourselves and the risks we endure in researching politically sensitive issues, even apparently everyday matters. It is a strength, continuing from our past, that we remain committed to anthropology and intensive fieldwork, despite becoming prohibited immigrants or unwittingly being caught in the cross-fire of conflict, which disturbs, undoes or completely stops our research (Gray 2019). Among the lessons to be learned from the work of the Manchester School is endurance, to keep on meeting the need for rethinking in our subject, in the light of sustained and intense fieldwork, and with an informed understanding of where we have come from.

A great deal of ink has been spilt on the question of the 'schoolness' of the Manchester School, and especially its customary focus on conflict, its house style, and its sophisticated, indeed innovative research methods. It has grown to be a scholarly industry that was fostered very deliberately and with a strong grasp of popular communication by Max Gluckman himself, and it includes my own early account in *The Annual Review of Anthropology* (1984, revised 1990). In this early account I reported alternative perspectives, an outsider's and an insider's. I found it was an outsider, Mary Douglas, who in a review was the first to 'salute [the emergence of] a school of anthropology whose publications are developed through close discussion, and where each worker's work is enhanced by his focus on a common stock of problems' (Douglas 1959a: 168). By contrast, I reported, from the insider's perspective, the view of my own fieldwork supervisor, Clyde Mitchell, who told me: 'Seen from outside, the Manchester School *was* a school. But seen from the inside, it was a seething contradiction. And perhaps the only thing we had in common was that Max was our teacher, and that meant we wrote ethnography rich in actual cases' (Werbner 1990: 152–3). If the insider's perspective was one thing, and the outsider's another, so too was criticism a matter of, to use a favourite Manchester notion, situational selection. The insiders prided themselves on intense argument and direct, open criticism of each other and their works in progress, explicitly in seminars; in public and publications, disagreements sometimes appeared frank, though usually more respectful than in seminar confrontations. Of course, as I show later, the flow of frank, judgemental and telling gossip among the insiders was remarkable, as might be expected from the analysis in Manchester gossip studies (Colson 1953; Gluckman 1963a; Epstein 1969). As for the outsiders, it was Mary Douglas who took on the role of being a prosecutor or unavoidable nemesis. Douglas praised or damned in subsequent reviews monograph after monograph of the Manchester School, and for this reason, among others, I will comment later at some length on her importance for the Manchester School.

Having myself been a PhD student and later colleague of Max Gluckman, Clyde Mitchell and Victor Turner and, as well, a long-time friend of Elizabeth Colson and 'Bill' Epstein, I write, admittedly, from an insider's view, and intentionally with much affection and admiration. Of course, where possible, I have sought to reach a reasonable balance between my memories, personal documents, notes and knowledge of gossip and the correspondence and reports available in the archives, as well as the considerable body of Manchester School publications.

Not surprisingly, being at the centre of a contentious circle, Gluckman found it to be one thing to plan, but another to implement, and still another to provide a full overview of the implementation. His Seven-Year Plan for the Rhodes-Livingstone Institute (RLI), from which the Manchester School arose, provoked this criticism by the American anthropologists George Marcus and Michael Fischer, that it was one of 'ultimately unco-ordinated team projects: making systematic connections (between the studies) was left to individual readers' (1986: 91). Admittedly, it was extremely ambitious plan for comparative research, across a whole region of Africa, that was to be in accord with a typology not of 'tribes' but of rural areas with variation in these factors: 1) the presence or absence of cash crops, 2) the import or export of labour, and 3) the relative proximity to the railway network. Gluckman expected that the research would account for 'the *differential* effects of labour migration and urbanization on the family and kinship organization, the economic life, the political values, the religious and magical beliefs' (Gluckman 1945a: 9, italics mine). It is a breathtaking ambition, and the Plan explicitly took even class differentiation into account, for example, 'a class of peasant farmers emerging among Tonga, with their cash crops' (Gluckman 1945a: 9).

Colson considered the research to be framed by a method for 'the intensive study of small communities'. It was designed to test hypotheses about a limited number of factors, not to provide an account of cultures or whole societies with the same 'tribal' name (Colson 1967). Without denying that Marcus and Fischer are right in a sense – the Plan did fall short of its too ambitious promise – I would argue that in good measure it was actually carried out. Even further, in a whole series of introductions, Gluckman distilled systematically for the reader the fresh, outstanding contributions of each Manchester monograph; and he made many of the findings coherently accessible in his popular works, such as *Custom and Conflict in Africa* (1956) and *Politics, Law and Ritual in Tribal Society* (1965), and in BBC talks. As I will discuss in Chapters 1 and 2, Gluckman was a public intellectual who spoke on behalf of the continuing advance of research in modern social anthropology, and not only by his own 'School'.

Gluckman put a stamp of apparent unity on it all when he introduced the collection of essays, *The Craft of Social Anthropology* (Epstein 1967), which gave a defining representation of his colleagues' methods and modes of analysis in relation to current theoretical problems:

> The contributors are anthropologists who have had the opportunity of working closely together for many years as officers of the Rhodes-Livingstone Institute, or in the Department of Social Anthropology at Manchester University, or both. They are thus able to discuss within a common framework modern fieldwork methods, not simply as a set of techniques *per se*, but rather as tools for examining a number of problems that have come to interest them. But we would like to stress that we see our own work as firmly set in the whole development of social anthropology. (Gluckman 1967: xi)

Strategies for destabilizing ethnography

Of all the ethnographic strategies that destabilize ethnography in order to reinvigorate it with fresh life and insights, the one most cultivated by Gluckman himself – for example, on the Nuer, in *Custom and Conflict* (1960) – and some of his students (Uberoi 1962; Werbner 1967, 1969, 1979a, 1981, 1990, 1992, 2015; Handelman 1990) belongs in the mainstream known as 're-analysis'. I offer an example of re-analysis in Chapter 9. Here I want to introduce re-analysis as one destabilizing strategy among others – deconstruction and redescribing – in order to enhance our knowledge of the heuristic value of each, but especially re-analysis in relation to the others and in the fresh elucidation of a classic among Manchester School studies, Victor Turner's *Chihamba, the White Spirit* (1975).

Although under the rubric 'deconstruction' I see an ethnographic strategy, I am well aware that after Derrida (1974), literary critics see and disagree under that rubric with regard to many things and approaches about which I am not competent to speak. I am taking the licence to apply 'deconstruction' to a destabilizing strategy that had its heyday following the publication of *Writing Culture* (Marcus and Clifford 1986). Deconstruction is, above all, the critical uncovering of concealment in the text of an ethnography. Hidden through rhetoric, through the appealing style of writing, through the seductive, often apparently simple and realistic narrative voice of the writer, are presuppositions, in particular about power and inequality. As Pnina Werbner reminds us, the deconstructive critiques of the Nuer in *Writing Culture* seemed to liberate the reader from politically significant complicity, unwitting entanglement in colonial domination being disguised or glossed over (P. Werbner 2018: 81). For example, James Clifford deconstructed 'an allegory of Anglo-Saxon democracy' in *The Nuer*, the people themselves being represented as 'the vanishing primitive' (Clifford 1986: 111–12). Renato Rosaldo critically uncovered the concealment of domination in a 'literary pastoral style': Evans-Pritchard described the Nuer as embodying 'democratic values, rugged individualism, fierce pride, and a warrior spirit … idealized characteristics of a certain masculine imagination … an ideal of human liberty, even in the midst of colonial domination' (Rosaldo 1986: 96). Ethnographers felt the challenges, first to interrogate political bias in their own texts as well as in anthropological classics, and in so doing to effect a critique of their own culture and its narrative arts for collusion in the domination of others. So overwhelming did this interrogation trend become that it led many to something like a failure of nerve, with doubt about whether ethnography, especially writing but also research in fieldwork, was still credible and valid or even possible.

The destabilizing strategy of redescribing, like deconstruction, aims against concealment and in favour of some liberation. Unlike deconstruction, however, redescribing turns away from power and inequality to a concern for displacement. What is it that has to be displaced? Or, perhaps exorcised? As Roy Wagner would have it:

> The future of Western society lies in its ability to create social forms that will make distinctions between classes and segments of society, so that these distinctions do not come of themselves as implicit racism, discrimination, corruption, crisis, riots, necessary 'cheating' and 'finagling' and so on. The future of anthropology lies in its ability to exorcise 'difference' and make it conscious and explicit. (Wagner 1975: 158)

I cite this passage particularly because of its signal significance in the development of redescribing. Marilyn Strathern places it on the flyleaf of *The Gender of the Gift* (1988), to indicate a starting point for her own first contribution in a major and comparative work of redescribing.

Wagner was not himself given to redescribing; it was, of course, for the future. Hence, reading his *Invention of Culture* (1975) in about 1978 (well before *Writing Culture*) 'was like a door opening' for Marilyn Strathern (Viveiros de Castro, Fausto and Strathern 2017: 44). Her remarkable body of many, richly varied studies now gives the most productive and substantial, if individual and often highly personal, examples of actually redescribing ethnography. How these bear the hallmark of redescribing is intricately revealed in the reflexive accounts and interviews, the debates and commentaries, especially on ways of rethinking and reimagining sociality, in Ashley Lebner's illuminating collection, *Redescribing Relations* (2017). I refer the reader to it as a whole, while I will regard a very small part in due course.

Strathern has a gift of redescribing that appears to be paradoxical: intensely serious and, sometimes at once, deliberately and admittedly playful and tricky. It is a gift of argument and narrative. Here, I present some attention to the argument, as explicitly described for redescribing, and I offer very little attention to the substantial narrative, revealing the complexities of social life, the idioms surprisingly true to themselves, and the fine disclosure of difference in metaphysics, 'Melanesian' vs. 'Western'. I want to extract bits of her earliest account of her attempt at making the argumentative gift in her chapter on 'Anthropological Strategies' (Strathern 1988: ch. 1). Later, in my Chapter 5, I take up the substantial question of any dialogue around relational thought and social relations that might be or might have been sustained between Strathern and members of the Manchester School in their concern with the development of social network analysis.

Above all, redescribing turns to awaken consciousness of premises, to call for the unexamined to be examined, for constructs not to be taken for granted. It is no good going on pretending, for the sake of convenience in analysis, that disparate things are commensurate. If we cannot avoid the use of fictions in analysis, we have to be vigilant in learning how they may be rooted in Western metaphysics, and in letting readers be clear about the fictional problematics involved. In question, for a start, are what Strathern identifies 'as the premises on which much writing on Melanesia (though *not of course restricted* to it) has been based' (1986: 7, italics mine). The caveat in italics hints at an extended aspect of this destabilizing strategy, which Strathern takes care immediately to make explicit: 'These premises belong to a particular cultural mode of knowledge and explanation' (1986: 7). *They are ours*, as is the mode itself, but if we cannot wholly extract ourselves from them – *we are also theirs*, so to speak – we have the potential to be critical about them by making them visible, even as the metaphysical roots of our thought. Hence for Strathern redescribing is something of a wake-up call to reflect on how accounts deploy fundamental oppositions – we might say common-sense logic – in appearing to be cogent.

To engage as a Westerner in having to think and write about how Melanesians think is to have to come to terms with and find a language for fundamental disparity, the difference being in mathematics as in metaphysics. According to Strathern, for the Melanesian, no one is less than two. But the Westerner thinks and talks about one being one, as in any opposition, which is one in opposition to another. In Melanesian thought, as Strathern constructs it, the one person is no less than a composite of two opposites. Hence to engage with this dual predicament within a Western mode of knowledge and explanation, Strathern turns to two perspectives, feminist and anthropological, which, if both Western, are alternatives, sometimes overlapping, sometimes at odds with each other. Being conflicted, they are just right for a running argument that may apprehend difference without occluding it. The one ethnographer as author becomes the two sparring partners, highly appropriate for a 'Melanesianist'; if a straddler, she does not sit on the fence but takes one side against the other, in turns, even for ironic and playful effect. Strathern elucidates the critical exposure directed towards academics and members of her own society, through what emerges as a shifting 'contextualist' perspective of her own: 'I choose to show the contextualized nature of indigenous constructs by exposing the contextualized nature of analytical ones. For members of that society [her own], of course, such a laying bare of assumptions will entail a laying bare of purpose and intent' (1986: 8).[3] Notably, by contrast to deconstruction's destructive force, its unnerving of ethnographers with critical and self-doubt about ethnography, redescribing in its destabilizing of ethnography has elicited an efflorescence, known to Strathern's credit as the New Melanesian Ethnography.

By comparison to both deconstruction and redescribing, re-analysis is hardly novel. That is, in good measure, because it shares very simply with much academic practice in puzzling about what is mistaken in a text, argument or received analysis. In such practice, re-analysis is pragmatic, on the creative way to ethnographic renewal. The appeal is this. Hunt for the mistake and track it down; and crucial in such detection, perhaps most fun, is the recovery, for purposes of further creativity, of both the accidental and the missing. The accidental could be a bit of fieldwork observation, information or odd data, not integral to the argument or explanation, and apparently unimportant, but given in the ethnography for the sake of a more complete record. The missing is not assumed, as in deconstruction, to have been kept out or silenced in the service of power and domination. Instead, to be learned through re-analysis is how and why the missing is missing. Sometimes, it gets recognized in comparison from one ethnography to others, and along with the accidental becomes at once destabilizing and constructive in further learning as to where a theoretical orientation constitutes blinkers and bias in ethnography. So far, to my knowledge, the engagement with theory in re-analysis has avoided coming to terms with a boldly philosophical drive, as in redescribing, rooted in 'Western metaphysics', or in the foundational ideas and oppositions of 'Euro-Americans'.[4] Instead, more within the workmanlike practice, familiar in academic scholarship, re-analysis does advance debate by pitching one theoretical tradition against another, as well as by arguing within the frame of a theoretical orientation for mistakes or shortcomings in the construction of an account.

At the heart of my re-analysis in Chapter 9, on Victor Turner's masterpiece *Chihamba, the White Spirit* (1975), I address questions of perspectival ethnography, and of the extent to which Turner's familiar ideas (on performance, liminality, semiotics and the ritual process) shed light on *Chihamba* as a ritual drama. My account speaks to a new generation of reflexive ethnographers, to close the distance between them and the radical experiments in perspectival plurality that twentieth-century ethnographies advanced. Into the foreground emerge tricks and fantasy, Bacchanalia and the preoccupation of magicians with playful sexuality for the arousal of fertility and well-being in harmony with the ancestors, male and female.

More into the background goes the 'ritual man' preoccupied with cosmic principles.

The re-analysis puts arguments to proof in a way that opens out interpretation to alternative theory. In line with Thomist philosophy, Turner gave short shrift to playful oppositions, so important for his own arguments in *The Ritual Process* (1969). He became preoccupied with isolated monism. A single principle overwhelmed his view, the essence by itself as the transcendent *Kavula*, the source of all being. In terms of *spirituality*, Turner decontextualized *Kavula* and thus misapprehended 'the pure act-of-being'. Against that, my re-analysis captures *Kavula* and the ghost, husband and wife, in their opposition, and thus conceptualizes the dynamics of their duality. In terms of *sacrifice* and an apparent ritual climax in the beheading of a victim, Turner gave us no development. My re-analysis clarifies a progression towards exorcism informed by a simple cultural axiom, namely: *the production and consumption of food and the reproduction of life are two sides of the same thing.*

In accord with that, I document in my own day-to-day account the dramatization of playful sex and reproduction in and through the means of production and consumption. The climactic moment – a rite of beheading followed by an appearance of emptiness – is sexual, good to the point of orgiastic overcoming for a satisfied ghost or her apparition, not at all like the spectral, sexless moment of emptiness at the tomb of Jesus: Turner's resurrection comparison is fundamentally mistaken.

The cultic importance of gender bias, playful sexuality and histrionic impression management raises a challenge to the relation between Turner's very rich record of vernacular texts and ritual practice and his religious and philosophical advocacy. To advance a view of 'ritual man' and his creativity in ritual drama, Turner brought together the Thomist reasoning about the pure act-of-being and Arthur Rimbaud's French surrealist theory of *voyance* – the reasoned disordering of all the senses. If much performance creates an esoteric mystery, about which an Ndembu magician boasts that it is terrifying and awesome, there is no direct observation by Turner to back his claim that it is actually so bewildering that there is the 'disordering of all of the senses' (1975: 185). In Chapter 9, I unpack the phases in the ritual process which disclose the familiar and the matter-of-fact in the ordering of the senses and the ecstatic, sensuous play on and with sexuality. Moments of Bacchanalia surge, but not *voyance*. Here the culmination of re-analysis is recognition of the continuing rich interest in an ethnography as an open text.

The renewed challenge, present coverage

Where, in *The Craft of Social Anthropology*, Gluckman saw for his colleagues the opportunity and ability 'to discuss within a common framework', the challenge I take up in *Anthropology after Gluckman* is to go back to being, as it were, the native having that first encounter with a social anthropologist, Elizabeth Colson, and hopefully, also, having a reasonable respect for his elders. It is to present my early impression refreshed; that is, to attend to significant difference and diversity in intellectual histories, personal dispositions and careers, while also recognizing Manchester's living legacies for 'the whole development of social anthropology'.

My coverage of contributions and their contributors is broad, though of course partial – urban and village studies, early directors of the Rhodes-Livingstone Institute (Gluckman, Colson and Mitchell) and the first Manchester PhDs (Epstein and Turner), interests in politics, law, kinship, history and social change, symbolism and ritual, methodology, urbanism, networks and relational thought. I show the linkage between their distinctive theoretical directions and their personal dispositions and special careers. On this basis, I show also some of the development of ideas and interests from generation to generation at Manchester through the work of the early School's students and some students of students.

Adam Kuper (2015) argues trenchantly for an outsider's critical view in the fourth edition of what is now perhaps the most popular textbook on twentieth-century British social anthropology's Great Men and (some) Women.[5] A brief comment on that view prepares the way for rethinking beyond the textbook and towards our own approach in *Anthropology after Gluckman*. If once a young man's amusing anti-establishment shocker, Kuper's history, now with its no less lively retrospection, has become an elder's standard textbook, in plentiful second-hand supply on Amazon. Yet as a perspective on the development of the Manchester School and, more broadly, modern social anthropology, there are remarkable shortcomings. It is hard to think of a historian of social anthropology better placed than Kuper to illuminate the important strands in the work of his Cambridge teacher, Meyer Fortes.[6] But Kuper sheds no light on Fortes's constructive response to field and process theories and turmoil in 1930s science, his wrestling with the conceptual ambiguities of his times, his use of heuristic fictions. In Chapters 2 and 5, I present a better perspective on that, and elsewhere I take up the archaeological evidence that over the *longue durée* of more than a millennium, the sacred centre of Tallensi in the Tong Hills has been a central meeting place for disparate ritual congregations (Werbner 2009). Better appreciated on this basis is Fortes's view of long-term Tallensi stability in the past and what has been seen as his alleged blind spot on history, despite his concern for the suffering and dislocation that colonial violence imposed on Tallensi.

My point is this. Kuper's history is too much in step with a growing trend to rewrite our past simply as a Golden Era of social anthropology, much influenced by *African Political Systems* (Fortes and Evans-Pritchard 1940) and segmentary lineage theory, but without an exemplary strand of connective relationality from Fortes (for examples, see Barth 2005; Eriksen and Nielsen 2013). This relational strand lies in still vital contributions distinctively by Fortes, and not by Evans-Pritchard, on unbounded relations, personal networks, overlapping fields of social ties, powerfully felt collaboration in cross-cutting ritual performance, and central place or nodal religious organizations, which I call regional cults (Werbner 1977a). It is a relational strand that is not the novelty it appears to be in recent work on arguments about the need to displace received and apparently unexamined formulations of an opposition between the Individual and Society and to rethink sociality in terms of relations and relations between relations (Lebner 2017). Recovering this strand, in its connective and relational significance, rather than as obscured by stereotypes of the 'isolated, closed society', enables me to show in Chapters 2, 5, 7 and 10 how influential this strand has been and still is for social anthropology, and in particular for the Manchester School.

In a further shortcoming, Kuper turns the stereotype of exclusion and closure on to the Manchester School itself. True, members of the Manchester department, like members of other British anthropology departments, now as in the past, did publish some edited collections consisting mainly of essays by fellow members

(Gluckman and Devons 1964; Epstein 1967a), but other collections were more inclusive (Colson and Gluckman 1951; Gluckman 1962b; Turner 1971). What the stereotype obscures, contrary to any fresh understanding of the Manchester School, is the actual engagement by its members in collaboration at frontiers in anthropology and across disciplines. Much of *Anthropology after Gluckman* documents this in chapter after chapter: to be an insider of the circle was to be also a critical and influential contributor as a frontiersman or frontierswoman distinctively and in one's own right. It is to that interaction on the inside and at the frontiers that we owe the distinction and the distinctiveness of the Manchester School.

Gluckman fostered a toxic loyalist milieu around himself, imagines Kuper, partly in macho, no-holds-barred seminars. In Kuper's first edition, and only somewhat revised in the fourth, the story is that 'deviants and turn-coats were tolerated with great ferocity, but no criticism was tolerated from outsiders' (Kuper 1973: 160). Others have told the Mancunian story differently (see Gordon 2018: 368–70). A. L. Epstein recalled the difference between the LSE seminars when he was a student, under Firth, who would 'spring on people' so that 'I was shaken rigid', and, under Gluckman, the Manchester seminars: 'If you didn't feel you had anything to say, you could be quiet throughout the seminar' (Yelvington 1997: 293). Kuper's textbook history hides what Frederick Barth, like others knowing the seminars well, perceived first-hand: 'Gluckman had an unusual ability to wrestle directly with the ethnographic data of others as presented in their papers, and he used it with great skill during seminar discussions' (Barth 2005: 38). About the RLI seminars, Clyde Mitchell recalled that Gluckman would ask what appeared to be irrelevant questions, and then 'slowly a pattern would begin to emerge and soon we were all agog with excitement as he showed us how it would all fit together in a meaningful way' (Gordon 2018: 337). In Chapter 2 and later discussions, I offer further understanding of argument and academic life, in its more creative, if sometimes fierce, intensity under Gluckman's leadership at Manchester.[7]

Notes

1 In that circle, John Barnes was a prominent member of a primary triad, along with Elizabeth Colson and J. Clyde Mitchell. Of the members of

the Manchester School I discuss, Barnes was the only one with whom I had no personal contact, and I was never his student or friend. For this reason, and my intention to build my account on first-hand knowledge and personal relations – as it were, my inclusion in the same 'thick' or effective network – I mention some of his important contributions but I do not devote a chapter primarily to his life and intellectual history

2 See Douglas 1959a, 1959b; Kapferer 1987, 2006; Evens and Handelman 2006; Mills 2008; Kuper 2015; Englund 2018; Gordon 2018: 367–8.

3 See Strathern 1996 for a more recent clarification of this strategy in her view of how 'Euro-American concepts of hybrid and network' might be extended with social imagination: 'That includes seeing how they are put to work in their indigenous context as well as how they might work in an exogenous one. It also includes attention to the way they become operationalized as manipulable or usable artefacts in people's pursuit of interests and their construction of relationships' (1996: 521).

4 In redescribing, 'Euro-Americans' is not a term for an actual population but a shorthand personification for convenience in exposition: 'I personify a discourse for expositional convenience' (Strathern 1996: 531 n.2).

5 Regrettably, Kuper pushes his stimulating intellectual argument into a personal attack and a paranoid imaginary of Manchester seminars and departmental life. Contrary to reality, Gluckman appears to be a mad boss, rather than an inspiring, charismatic and, of course, argumentative, even awkward one, with great vitality.

6 For a valuable, in-depth re-evaluation of Meyer Fortes's thought and works, with a fine, amicable touch of ancestor worship, see Kuper 2016.

7 For a perceptive account of 'cohesion and solidarity' within Gluckman's 'team' at the RLI, see Gordon 2018: 334–6.