

CONTENTS

GODFREY LIENHARDT	
Frazer's Anthropology: Science and Sensibility	1-12
GREGORY VAN ALSTYNE	
The Unvoiced Text: Allusion in Malawian Sung Poetry	13-32
SIMON SINCLAIR	
The Present Tense Again	33-48
<i>Comment</i>	
Dispersed Alliance, Terminological Change and Evidence by Georg Pfeffer	49-54
Reply to Pfeffer by Robert Parkin	54-63
Anthropology and Missionaries by Fiona Bowie	64-66
Malinowski the Photographer by Jeremy Coote	66-69
<i>Exhibition and Catalogue Review</i>	
Useful Arts: Artful Utensils (<i>Basketmakers: Meaning and Form in Native American Baskets</i>) by Stephen Hugh-Jones	71-74
<i>Book Reviews</i>	
SIGNE HOWELL and ROY WILLIS (eds.), <i>Societies at Peace: Anthropological Perspectives.</i> Reviewed by Robert Parkin	75-77
R. G. DILLON, <i>Ranking and Resistance: A Precolonial Cameroonian Polity in Regional Perspective.</i> Reviewed by David Zeitlyn	77-79

Book Reviews (continued)

RON BRUNTON, <i>The Abandoned Narcotic: Kava and Cultural Instability in Melanesia.</i> Reviewed by Richard Rudgley	79-80
HANNA HAVNEVIK, <i>Tibetan Buddhist Nuns: History, Cultural Norms and Social Reality.</i> Reviewed by David N. Gellner	80-81
JOHN H. INGHAM, <i>Mary, Michael, and Lucifer: Folk Catholicism in Central Mexico.</i> Reviewed by Anthony Shelton	82-83
STANLEY BRANDES, <i>Power and Persuasion: Fiestas and Social Control in Rural Mexico.</i> Reviewed by Anthony Shelton	83-85
CHRISTOPHER ALAN WATERMAN, <i>Jùjú: A Social History and Ethnography of an African Popular Music.</i> Reviewed by Martin Stokes	85-87
LORRAINE NENCEL and PETER PELS (eds.), <i>Constructing Knowledge: Authority and Critique in Social Science.</i> Reviewed by Chris Holdsworth	87-89
SCOTT ATRAN, <i>Cognitive Foundations of Natural History: Towards an Anthropology of Science.</i> Reviewed by N. J. Allen	89-91
Publications Received	93-98
Contributors to this Issue	inside back cover

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FRAZER'S ANTHROPOLOGY: SCIENCE AND SENSIBILITY

GODFREY LIENHARDT

LAST year saw the seventieth anniversary of the inauguration of these Frazer Lectures when Frazer was presented with a laudatory address drawn up by A. E. Housman on behalf of a large number of his friends and admirers. Frazer, who was then 67 years old, a Fellow of the Royal Society and soon to receive the Order of Merit, replied with the rather grand modesty that was one of his conspicuous characteristics:

I can only hope that, if posterity should concern itself with my writings, it will not reverse the verdict which you have passed upon them.... It is my earnest wish that the lectureship should be used solely for the disinterested pursuit of truth, and not for the dissemination and propagation of any theories or opinions of mine. (See Frazer 1927a: 365; Dawson 1932: xii)

The truth is that his lasting influence has been in the literary rather than the academic world, while social anthropologists had for the most part distanced

Revised text of the Frazer Lecture in Social Anthropology for 1991, given at the University of Cambridge on Thursday, 5 March 1992, and prepared to mark the centenary of the publication in 1890 of the first edition of *The Golden Bough*. I am grateful to the Master and Fellows of Trinity College, Cambridge, for permission to quote from a few of the letters to Frazer held in the College's Wren Library, in particular those from Sir Arthur Keith, William Steed, the Revd Montague Pollock, R. R. Marett, Sir Francis Galton and Mr J. Parrott. I am also grateful to Steven Seidenberg and Jeremy Coote for their help in preparing the text of the lecture for publication.

themselves from his theories and opinions during his own long lifetime. Some thirty years after Frazer died at the age of 87, his one-time secretary, R. A. Downie, was provoked to defend him against presumptuous younger critics, among whom he singled out the Professor of Social Anthropology at Oxford, E. E. Evans-Pritchard. 'But anthropologists are a queer tribe,' Downie wrote, 'and since 1948, when Evans-Pritchard gave his lecture on *The Divine Kingship of the Shilluk*, it has become almost traditional in Frazer Lectures to take some idea of Frazer's and pull it to pieces' (Downie 1970: 29). Downie exaggerates, though it is true that some later lecturers have followed a brief conventional acknowledgment of Frazer's undoubted eminence with papers that implicitly raise doubts about why or even whether he deserved it.

But even now, a century after *The Golden Bough* was first published, and half a century after Frazer's death, 'Frazer of *The Golden Bough*' still remains, if only by name, by far the most widely known of all British social anthropologists. Whether they like it or not, Frazer's popular reputation remains part of their own, and since he raised the question of posterity, it seems appropriate to take the occasion of these anniversaries to look back on some of the verdicts that have been passed on his work.

There was no full-scale biography of Frazer until 1987. Robert Ackerman's *J. G. Frazer: His Life and Work* draws upon most of the published and many of the hitherto unpublished sources to bring to life Frazer's character and intellectual interests (he had few, if any, which were not), in relation both to his own times and to ours. If I add here little more than a few asides from my own anthropological viewpoint, I have two excuses. The first is that my D.Phil. thesis was examined and approved by the Revd E. O. James (1888–1972), who is described by Ackerman (1987: 124) as probably 'the last of the true-believing Frazerians' and who was kind enough to suppose that at heart I shared his faith. The second is that before Ackerman thought of writing his book, I had taken an interest in some unpublished letters to Frazer, when they first began to arrive in Trinity College, Cambridge, jumbled together in a few old cardboard boxes, including a few to which Ackerman does not refer. Handling those personal letters gave an intimate impression of the fascination Frazer's work had exercised over correspondents of all kinds from all over the world; and the respect and deference, even veneration, accorded this shy, reclusive, and personally far from charismatic scholar seemed to call for some explanation. Even as first-year students of anthropology in the mid-1940s we were allowed to assume that Frazer, to put it bluntly, was a bore, if on a grand scale; and he certainly wrote at inordinate length, as you may judge from one or two passages I am bound to quote.

Frazer could scarcely have hoped for a more sympathetic biographer than Ackerman, but even he introduces his subject with the intentionally challenging statement, 'Frazer is an embarrassment' (ibid.: 1); and Ackerman knows that this has been particularly true for British social anthropologists, who have to agree that Frazer was and remains the most famous of them all, while also dissociating themselves from much that he wrote. He embarrasses us now partly for the very

reason that gave him his wider popular appeal—his apparent sympathy with the assumptions and values of his own times and social class. Though he often appears to accept those values while at the same time obliquely suggesting their limitations, he never (unlike Matthew Arnold, whom he in some ways resembles) directly confronts them. It is not necessary, therefore, to read very far in his work to find examples of the imperialism, paternalism, 'colonial mentality' and now, certainly, 'élitism' that some critics of British social anthropology still detect in his successors, like the sins of their colonialist fathers inexorably visited upon their children, innocent though they may claim to be. And, of course, those peoples whose grandparents Frazer constantly refers to as 'savages'—'your dear savages', as one of his clerical admirers called them—do not like it the better for knowing that Frazer included not only his own remote ancestors in that category, but also most of his living fellow countrymen. The masses everywhere, he wrote, constitute 'a solid layer of savagery beneath the surface of society' (cf. e.g. 1931: 129, 141). When Frazer died in 1941, Professor Fleure's obituary for the Royal Society tried to allow for some of Frazer's comments on the common people, which might be misunderstood in the more democratic atmosphere of the nation at war. As an example he quoted a Frazerian passage that ends with a reference to 'empty husks of popular superstition on which the swine of modern society are still content to feed'. We are not quite reassured, I think, when Fleure then adds, 'needless to say, "swine" is used here without any touch of unkindness' (Fleure 1941: 902). Frazer was in principle sympathetic towards the underprivileged, but he could never resist a turn of phrase or a biblical allusion.

These are questions of a change in general social sensitivities between Frazer's time and ours; but among the literary and academic public, who take Frazer's social attitudes for granted as part of a history of ideas familiar through their own parents or grandparents, the part assigned to Frazer in the history of twentieth-century ideas and sentiments—in the history of modern 'Western' sensibility, to use the word in my title—is disproportionate to the much smaller part he soon came to play in the history of social anthropology. And as his influence on so many of this century's creative writers shows, he provided general readers with more food for the imagination, and more colourful and stylized prose, than his successors usually had to offer.

Some thirty years ago, when I lectured on Frazer at the Jung Institute in Zurich, I criticized Frazer's interpretations of primitive religion more dismissively than I would now, suggesting that his ideas of primitive psychology were little more than plausible constructs of his own Victorian rationalism. The lecture was not at all well received by the students and members of the Institute, who preferred to admire him as a guide to the understanding of the human psyche, an imaginative pioneer in the exploration of Jungian archetypes. T. S. Eliot similarly found *The Golden Bough* psychologically complementary to the work of Freud, 'throwing its light on the obscurities of the soul from a different angle' (quoted in Vickery 1973: 235). I should have been better advised in Zurich to remember the line of Yeats, whose interests in Frazer were similar to those of the psychoanalysts: 'Tread

softly, for you tread on my dreams'. For Frazer in several passages sees himself as Prospero, appealing to his readers' sense of the illusions, transience and mystery of dreams. Compare, for example, the famous opening of the abridged version of *The Golden Bough* with that of Radcliffe-Brown's *The Andaman Islanders*, published in the same year. Frazer begins by promising his readers 'a voyage of discovery', from 'Turner's dream-like vision of the little woodland lake of Nemi—"Diana's Mirror", as it was called by the ancients' and so on to the secret of the King of the Sacred Wood. Radcliffe-Brown begins with a short geography lesson: 'The Andaman Islands are part of a chain of islands stretching from Cape Negrais in Burma to Achin Head in Sumatra.' Having myself started a book in the approved Oxford style of Radcliffe-Brown, I now think there may be some happy medium between the sensibility of the one and the science of the other.

Among later well-known social anthropologists, perhaps only Lévi-Strauss in *Tristes Tropiques* (itself a romantic, indeed deliberately poetic title) and Margaret Mead, in their very different ways, have invited their readers to share their personal anthropological experience, in a voyage of self-discovery. Both, like Frazer, are very self-conscious writers, and write with an eye to the reader's own self-consciousness. It is clear that Frazer's wider public often read him less for anthropological knowledge of very foreign peoples than because he encouraged them to think about themselves, and to discover themselves more interesting and exciting than they had supposed themselves to be. When he received an honorary degree at Manchester University he told the Mancunians that in their city 'the pulse of life, the pulse of Empire beats more strongly than in the peaceful, the cloistered seclusion of our ancient Universities.... Manchester ranks with Athens and Alexandria in antiquity, and with Florence in the Middle Ages' (see Frazer 1927a: 357). When Rudyard Kipling invited Frazer to accept yet another of his honorary degrees, from the University of St Andrews in 1923, he addressed him as 'one to whom our civilization owes so much of its knowledge of itself'; and in marking the recent centenary of *The Golden Bough*, some well-known modern writers and critics have on the whole confirmed Kipling's judgement, with reference to Frazer's influence on 'thinkers' and creative writers during this century, some of it of a kind he would probably have preferred not to acknowledge (see e.g. Fraser (ed.) 1990). D. H. Lawrence and James Joyce, had he taken any interest in them, would have shocked or baffled him, and he soon found Eliot's *The Waste Land* incomprehensible (see Downie 1970: 21, 60). With his fear of working-class 'Bolshevism' and his primness about sex, he would have been dismayed to find himself remembered in the company of Marx and Freud as 'one of the makers of our modern consciousness'. But there are many other representatives of our modern consciousness (Mrs Whitehouse, for example) with whom he would have felt quite at home. He denounced 'an age like ours' (this was in 1920) when marriage and family ties counted for nothing, sexual communism was commonly taken for granted, and the very distinction between the sexes was being obliterated. At such a time, he suggested, we should do well to contemplate the domestic virtues of the poet William Cowper, in order to 'repel and refute those

shallow sophisms which, addressed to the basest of human passions, would subvert the fabric of civilization and plunge us back into that savagery from which it has cost mankind so many generations of patient effort to emerge' (see Frazer 1927a: 364). Downie, by contrast, mentions that a passage in Frazer suggesting that science itself might be superseded by some quite different approach to intellectual and moral enlightenment had been quoted by 'the drug culture' of the 1960s as 'a prophecy of psychedelic insight' (Downie 1970: 47). It is ironic that Frazer, a model of middle-class morality and social conformity, should have become accepted as a herald of later intellectual and emotional emancipation.

For social anthropologists Frazer began to belong to the past at almost the same moment as these Frazer lectures were established. In 1922, simultaneously with the publication of the first abridged edition of *The Golden Bough*, came Malinowski's *Argonauts of the Western Pacific* and Radcliffe-Brown's *The Andaman Islanders*, the first outstanding products of the British field research tradition that Frazer had very actively encouraged. As every first-year student knows, these monographs began to point students towards empirical studies of Frazer's 'savages', who, when spoken to in their own homes and languages, were found to have much more of interest to say for themselves than Frazer had to say about them. The central theme (or, as he thought, theory) of *The Golden Bough*—that all mankind had evolved intellectually and psychologically from a superstitious belief in magicians, through a superstitious belief in priests and gods, to enlightened belief in scientists—had little or no relevance to the conduct of life in an Andamanese camp or a Melanesian village, and the whole, supposedly scientific, basis of Frazer's anthropology was seen as a misapplication of Darwin's theory of biological evolution to human history and psychology. This is, of course, an absurdly simplified account of what happened with the change from library research to field research, but it is enough to suggest why, among social anthropologists, Frazer's reputation declined so rapidly after he had first achieved it. For a younger generation there were, of course, other more personal reasons. As Evans-Pritchard explained:

I began to vary the tedium of the History School [at Oxford]...by taking an interest in books like Tylor's *Primitive Culture* and Frazer's *Golden Bough*.... But there was here a snag. I did not want to become, I was going to say, just an intellectual. I wanted a life of adventure too, and fieldwork seemed to be the solution to combine both. (Evans-Pritchard 1973: 18)

Frazer's immediate successors were still as committed as Frazer himself was to establishing the scientific credentials of their subject, but though their science was to be a *social* science (for Malinowski a 'science of culture', for Radcliffe-Brown a 'natural science of society') their model for this science, like Frazer's, was taken from the natural sciences. 'The great thinkers, the Newtons and Darwins of anthropology, will come after us,' Frazer had written. They are still awaited, though Radcliffe-Brown sometimes seemed to hope that he might be one of them. It must be remembered, of course, that funding for research was

more readily forthcoming in the name of the sciences than in the name of the arts, and a scientific knowledge of social processes seemed to promise the power to control them.

In answer to doubts raised about the empirical foundations of his 'structuralism' Lévi-Strauss has said recently that 'the great speculative structures are made to be broken' (see Smyth 1991), but I cannot think that he now regards all structuralist interpretations to be entirely misconceived. In a paradoxical way, for an influential anthropologist to make such a statement disarms particular criticisms of his work, and Frazer was, if unconsciously, a master of such tactics. He often reminds his readers that all theories, magical, religious, or scientific, were in the last analysis only 'theories of thought', as though it mattered little whether they were mistaken or not. I doubt if Frazer meant to erect his 'speculative structure' of the whole evolution of the human intellect, from savagery to civilization, for the scientific satisfaction of seeing his successors demolish it, but by insisting that all theories of the nature of the world and the universe are merely of notional significance, that only facts really count, he appeals to his readers' inclination to accept at their face value the choice and arrangement of the facts by which he supports the theories.

And he does so with overtones particularly calculated to appeal to his contemporaries, brought up on sermons, the Bible, and a biblical view of human ignorance in relation to divine omniscience. All our human theories were after all, he wrote, 'merely hypotheses devised to explain that ever-shifting phantasmagoria of thought which we dignify with the high-sounding names of the world and the universe' (Frazer 1900, iii: 460), all 'fated to be washed away like childrens' castles in the sand by the rising tide of knowledge' (see Frazer 1927b: 280). It would seem impertinent to ask him what the low-sounding names of the world and the universe might be, or whether 'scientific', economic or political theories, applied as they have been with disastrous consequences, were *really* no more important than sand-castles. These are typically Frazerian images, and typically he uses them not because they are good to think with, but because they were good for his readers to feel with. The anatomist and physical anthropologist Sir Arthur Keith wrote to Frazer, after he had heard Frazer's Huxley Lecture, that in him 'without doubt the "kirk" had lost a tip-top preacher as well as a scholar'. Frazer was brought up in a tradition that took sermons seriously, and John Dwyer's recent book, *Virtuous Discourse: Sensibility and Community in Late Eighteenth-Century Scotland* (1987), often reminds one of this and other characteristics of his—the cultivation of a spirit of melancholy and of rational fellow-feeling, along with admiration for Addison, Hume, Adam Smith and Malthus. He certainly derived some satisfaction from his sad reflections on the human condition, spending so much time on that 'melancholy record of human error and folly', as he called *The Golden Bough* (Frazer 1900, iii: 458).

For many modern readers, the voice of the preacher, often echoed in Frazer's writings, must sometimes seem incongruous with the voice of the scientist. In what he called 'the scientific spirit', he wrote: 'we must endeavour to investigate

the beliefs and customs of mankind with the same rigorous impartiality with which, for example, the zoologist investigates the habits of bees and ants' (see Frazer 1927b: 27). But when he develops his well-known analogy between the magician and the scientist, both attempting to control nature by the application of immutable laws, the voice of the preacher carries him away. Take, for example, the following passage where, however, a consciousness of human weakness is complemented, as often in sermons, by a message of hope:

Both of them [the scientist and the magician] open up a seemingly boundless vista of possibilities to him who knows the causes of things and can touch the secret strings that set in motion the vast and intricate mechanism of the world... They lure the weary inquirer, the footsore seeker, on through the wilderness of disappointment in the present by their endless promises of the future; they take him to the top of an exceeding high mountain and show him, beyond the dark clouds and rolling mists at his feet, a vision of the celestial city, far off, it may be, but radiant with unearthly splendour, bathed in the light of dreams. (Frazer 1900, i: 62-3)

One can almost hear the announcement of the hymn that will follow; but for readers accustomed to the language and sentiments of the scriptures, and trying to reconcile them with the scientific authority of Darwin and Herbert Spencer, this amalgamation of the scientific quest and *The Pilgrim's Progress* offered a comforting literary resolution. Frazer was awarded a Civil List pension in 1905 'in recognition of his literary merits and of his anthropological studies' in that order, and his literary and biographical essays (on Condorcet, for example, and Renan; see Frazer 1927a, 1931) are admirable for their style and content, but it was the more homiletic and prophetic style of *The Golden Bough*, in which he presented his wide range of learning, that gave him his credence among the general public and brought him a repletion of high academic and official honours.

Frazer addresses his readers directly, in a tone of gentle but entirely assured authority, as though no reasonable person could possibly disagree with him, and even those social anthropologists who had been among the first to criticize his ideas had to come to terms with an uneasy sense of his superiority. With reference to the Frazer lectures Max Gluckman, for example, said that though we no longer found his basic ideas useful, that we read him, if at all, for antiquarian interest, and often found him dull, we were still 'perched upon his shoulders, dwarfs on a giant' (Gluckman 1962: 16). Though it must seem unlikely to those who remember Gluckman that he could ever regard himself as a dwarf, either physically or mentally, it is more than conventional praise. Again, Evans-Pritchard, in spite of his criticisms of Frazer's imaginative construct of 'divine kingship' (Evans-Pritchard 1948), later paid him much the same sort of tribute as Frazer's own contemporaries, describing *The Golden Bough* as 'among the great achievements of English literature and scholarship' (Evans-Pritchard 1986: 132). As late as 1965, Edmund Leach, one of the most impatient critics of Frazer's overblown prose and literary embellishment of his sources for dramatic effect, concluded an almost entirely hostile account of Frazer's (and Malinowski's) anthropology with

the highest praise: 'It is because each of us can recognize in their pages the savage within us that we feel the excitement of insight, the unverifiable validity of a statement of genius' (Leach 1965: 36). Again, the primary interest of 'the savage' would seem to be to heighten our own self-consciousness. Leach, however, had a sharp, advocate's mind, and might equally well have argued for the prosecution, along with Wittgenstein, that 'Frazer is much more savage than most of his savages' since 'his explanations of [their] observances are much cruder than the sense of the observances themselves' (Wittgenstein 1979: 8e).

Frazer, like Wittgenstein, was a Fellow of Trinity College, Cambridge, and therefore perhaps should have known better, but Frazer received his honorary DCL from Oxford in the company of Cecil Rhodes and Lord Kitchener, and was writing for a general public that, like them, had very much cruder notions of 'savages' than those which, largely as a result of his own work, began to be entertained after the Great War. (Though to be fair, and as Brian Street has shown (1975), Frazer earlier played a part in introducing a somewhat more enlightened view of 'savages' into the works of such popular writers as Rider Haggard.)

There are now very few social anthropologists who remember from their own direct experience how and why Frazer came for a time to be so highly regarded that any criticism of his work seemed a bold act of impiety (only Andrew Lang among his contemporaries openly made fun of him). Whatever others may have thought of his ideas or personality, for them he was still an authority to be reckoned with, and an original source of their own anthropological interests and knowledge. By now he seems a remote, ancestral figure, and *The Golden Bough* has become little more than an intellectual ancient monument—'one of the most beautiful ruins in the history of thought' Lord Annan called it even some thirty years ago (Annan 1959: 11); and if we are to understand how Frazer, whose work scarcely any student of social anthropology in the last fifty years has actually read, came to be so eminent, we can do so only by excavating that ruin, in a version, perhaps, of what Michel Foucault has termed an 'archaeology of knowledge'. What fragments of 'discourse', one may ask, does one discover at the foundations of Frazer's great reputation for authority among scholars and men of letters, poets and novelists, statesmen and politicians, liberal churchmen and free-thinkers, and general readers from all walks of life?

I have chosen here only a few such fragments, of which the most complete comes from the address presented to Frazer by A. E. Housman to inaugurate these lectures some seventy years ago. Housman made some play with the idea that Frazer himself was a magician, the King of the Wood and the custodian, as well as the author, of the Golden Bough. There, Housman continued, is to be found

learning mated with literature, labour disguised in ease, and a museum of dark and uncouth superstitions invested with the charm of a truly sympathetic magic. There you have gathered together, for the admonition of a proud and oblivious race, the scattered and fading relics of its foolish childhood, whether withdrawn from our view among savage folk and in different countries, or lying unnoticed at our doors. The forgotten milestones of the road which man has travelled, the mazes and blind

alleys of his appointed progress through time, are illuminated by your art and genius, and the strangest of remote and ancient things are brought near to the minds and hearts of your contemporaries. (See Dawson 1932: xii-xiii)

It is some indication of the impression Frazer made on his readers that this eulogy takes on the cadences of Frazer's own rhetorical language. Housman spoke on the whole for the educated middle class connected with the older universities (though by no means all academics) and his praise reflects their interests—their respect for learning, their literary culture, their national pride, their relationship with colonized peoples and with their own working classes, their belief in progress, their concern with their own social origins, their taste for historical legends and classical mythology. Frazer wrote of the magician as the wisest and most intelligent of his tribe, who was, therefore, chosen as its leader. His admirers fancifully cast *him* in that role, as the repository of their myth and legend, a magician, a seer or a prophet, and Frazer was happy to accept it.

As early as 1901, Wickham Steed, who was to become editor of *The Times*, wrote to Frazer after meeting him in Rome. William James, who had met him there at the same time, had found him 'a sucking babe of humility, unworldliness and molelike sightlessness to everything but *prini*' (quoted by Ackerman (1987: 175); original emphasis), but Wickham Steed saw him differently. 'My dear Frazer,' his letter begins, 'It seems rather like a profanation to drop the "Mr." to an arch-magician like you.' And R. R. Marett went further, referring, if mischievously, to the very formidable Lady Frazer as the *flaminica dialis*, the wife of the priest of Jupiter. Jane Harrison spoke for the avant-garde classicists—Gilbert Murray, and later E. R. Dodds, whose interest in what was to become his *The Greeks and the Irrational* was in part suggested by reading Frazer. The classicists were blind, Jane Harrison wrote, until Frazer came 'to light the dark wood of superstition with a gleam from *The Golden Bough*...at the mere sound of the magical words "Golden Bough" the scales fell [from our eyes]—we heard and understood' (Harrison 1925: 82-3).

And many less academic readers found inspiration in *The Golden Bough*. In his obituary Fleure went out of his way to mention a miner in South Wales for whom it was a treasured possession (Fleure 1941: 899), while Downie quotes Jane Harrison's account of meeting 'a cultured policeman, a member of the Working Men's College, who said 'I used to believe everything they told me, but, thank God, I read *The Golden Bough*, and I've been a free-thinker ever since' (Downie 1970: 64). On the other hand, a liberal clergyman, the Revd Montague Pollock, wrote to Frazer that *The Golden Bough* was 'wonderfully confirmative of one's faith in the inner validity of the Holy Scriptures...only to him who like yourself can see and hear', he told Frazer, 'is the deep truth revealed'. In relation to Frazer's anti-clerical and basically anti-religious views, Sir Francis Galton congratulated him on 'the cleverness with which you indicate without expressing conclusions at all to wound the feelings of simple orthodox persons'. For Frazer was all things to all men. Hard-headed, if liberal-minded, colonial officials welcomed his anthropology as a contribution to humane and well-informed

administration. 'Statesmanship may profitably go to school with anthropology', said Sir Frederick Whyte, the President of the Indian Assembly; and much of the British field research in the first half of this century was carried out under the auspices of such as General Smuts, for example, and Sir Hubert Murray, Gilbert's brother, the Governor of Papua. To these proconsuls, Frazer was able to argue persuasively in their own terms for the funding of anthropological research as a moral duty of imperial rule.

Very different from such men of affairs were provincial antiquarians, represented at their most charming in a letter from a Mr J. Parrot of Stockton-on-Tees, written in 1901. With the death of Queen Victoria in January of that year, the crisis of a royal succession, central to *The Golden Bough*, filled the British newspapers of the time, but for many the letter may still strike a sympathetic note:

Dear Sir,

Your books always fill me with an indescribable rest—carry me away from 'all the dreary intercourse of daily life'. Not that my life is unhappy...it is increasingly joyous since that event which your first edition of *The Golden Bough* came to commemorate—my marriage. That was the opening of the Golden Gate into the most delightful portion of my life.

You will understand me better when I say that I am not a newspaper reader, and the flotsam and jetsam of public opinion passes without effect on me, for I would sooner be concerned with finding a new fact about primitive man and fire, or the discoverer of a fire-split flint, than share all the dreary intercourse of parliamentary, municipal and social life.

Naïve, perhaps, Mr Parrot's letter may seem, but it is revealing, for it touches upon a sense of discontent with modern civilization, a longing for other places and other times, which also troubled the poets of the nineteenth and early twentieth centuries. Frazer understood this very well, and to some extent shared it. The language and apocalyptic vision of the Bible, he wrote, lifts us above 'the dull round of common life' (Frazer 1927a: 450), and he was always nostalgic for Cambridge, where 'remote from the tumult and bustle of the world with its pomps and vanities and ambitions, the student may hope...to penetrate through the little transitory questions of the hour to the realities which abide' (ibid: 441).

World-weary readers like Mr Parrot had already been well prepared for Frazer's 'voyage of discovery' by the poets they most admired: Wordsworth, for example, with his wish to exchange the worldly materialism of his own society for the spiritual vitality of 'a pagan, suckled in a creed outworn'; Matthew Arnold's Scholar Gypsy, escaping to the natural world of Bagley Wood from 'this strange disease of modern life / With its sick hurry, its divided aims / Its heads o'ertaxed, its palsied hearts...'; and Tennyson's unhappy hero in *Locksley Hall*, dreaming of the sensuous life of a tropical island:

There, methinks, would be enjoyment more than in this march of mind,
In the steamship, in the railway, in the thoughts that shake mankind.

There the passions cramped no longer shall have scope and breathing space;
I will take some savage woman, she shall rear my dusky race.

Frazer himself, of course, had no appetite for any such exotic experience. Downie (1970: 18) tells one of several similar, probably apocryphal stories of how, as a child, Frazer had fled howling with terror from the Wild Man of Borneo at a fair, and in his prime he readily found good reasons for not accompanying A. C. Haddon on an expedition to New Guinea. But for those who felt, like Mr Parrot, alienated from their own civilization, *The Golden Bough* came as a compendium, and vicarious experience, of other ways of thinking and feeling, older, more deeply rooted in human nature, and rich in symbolic content.

Hence Eliot, Pound, Lawrence and Yeats, to mention only the best known, used Frazer's anthropology to criticize the spiritual impoverishment of a civilization dominated by applied science—'an old bitch gone in the teeth, a botched civilization', Ezra Pound called it. For them, what Frazer had intended as a sustained critique of human irrationality in effect helped to restore the irrational, or at least the non-rational, to its place in artistic creativity. Frazer, ambivalent as usual, had made allowance for such a reaction, contrasting Renan's understanding of religious emotion, for example, with the arid rationalism of the German theologian Feuerbach (see Frazer 1931: 227). There was something for everybody in Frazer's anthropology. It strengthened Yeats's magical and theosophical beliefs, deepened Eliot's understanding of Christian spirituality and, conversely, directed D. H. Lawrence away from his residual Christianity towards his search for his own dark gods. For Eliot, *The Golden Bough* was a great artistic expression of religious disillusionment, 'throbbing...with the agony of spiritual life' (quoted in Vickery 1973: 236). D. H. Lawrence followed Frazer's 'savages' to New Mexico, where they freed him, he wrote, 'from the...great era of material and mechanical development...the great psyche of materialism and idealism which dominated me' (Lawrence 1936: 142). There again, Frazer had anticipated those who might regret the replacement of the old gods by what he called 'certain abstract ideas of ethers, atoms, molecules and so forth': 'Thus instead of being peopled with a noisy bustling crowd of full-blooded and picturesque deities... animated with the warm passions of humanity, the universe outside the narrow circle of our consciousness is now conceived as absolutely silent, colourless, and deserted' (see Frazer 1927b: 213). The gods that Frazer felt sadly obliged to discredit in the name of science were restored by the poets in the name of sensibility—of Christian or pagan spirituality, of intuition, of imagination, and of instinct.

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THE UNVOICED TEXT: ALLUSION IN MALAWIAN SUNG POETRY

GREGORY VAN ALSTYNE

Introduction

ALLUSIONS are employed in various African oral art genres for a variety of purposes. Scholarly treatment of allusion has been limited for the most part to consideration of the strict denotation of the word (or phrase), leaving the further, personal and local associations, derived from local knowledge and experience, largely unexamined. In addition, treatment of allusion in African verbal artistry has overlooked the contemporaneous nature of its application. It is widely recognized now that an oral art text must be 'captured' in actual performance, complete with all the attendant artistic blemishes and inconsistencies as well as the creative surprises that the vagaries of a live performance may entail. With the examination and analysis of the actual performance of a given version of an extant composition, it is revealed that the sung poem is highly sensitive to its immediate social surroundings. This fact is reflected in the allusive content of the text. Furthermore, what can be seen from the range of examples given below is that allusion is intimately tied to the content and ultimate meaning that inhere in the sung poem.

This article draws upon anthropological fieldwork conducted in Malonje village, Zomba, Malawi, between October 1988 and October 1989. During the course of this work I collected over 500 verbal art texts, none of which has ever been committed to print. That is, this corpus of sung poetry comprises an oral tradition.

Allusion may add a dimension of familiarity and intimacy to a narration or, in contrast, serve to encode meanings to be deciphered only by those privy to its particular system of communication. Not only does alluding to actual geographical features as part of the song or narrative's cosmography, as in the Sundiata epic of West Africa (see Niane 1965), bring the real into the imagined, it is dependent upon the individual members of the audience fleshing out the details in their minds' eyes. In fact, not everyone in the audience or taking part in an oral rendering of a sung poem knows the full extent of what is being alluded to in the text. For example, with reference to Gbaya storytelling in Cameroon, Noss (1972: 86-7) observes, 'it requires that they be familiar with what is alluded to and that through the allusion they supply their own dimension to the tale.... The world of the tale is normally the immediate world for whom it is told. Rarely is the tale about distant places and persons.'

Toponymous Familiarity

Noss's comment holds true for Malonje village stories.¹ For instance, the *nthano*, or tale, intoned in the Chinyanja language, about a group of girls who are engulfed by a huge fallen baobab tree in which they have taken shelter from the rain, takes place along the banks of the Mulunguzi River, one of the major watercourses that springs from the slopes of the Zomba plateau not far from Malonje village. The area is well known, and can be visualized by every villager listening to or taking part in the 'singing' of the tale.²

Paradoxically, detailed description is little used in those genres that are dependent for their imaginative actualization upon numerous allusions to familiar locations. The same can be observed for stock characters and places whose relevant associations are known intimately by the home audience but which often leave a conceptual gap for the ethnographer (cf. Scheub 1975; Seitel 1980). Likewise, the same can be said for the corpus of sung poetry I collected from Malonje.

In a medium of expression such as song, the narrative quality of the text is necessarily condensed in favour of direct allusion to extratextual phenomena that

1. Malonje village, the community in which these sung texts were taped, is multiethnic and bilingual. This is due in part to the propinquity of the village to Zomba, the former colonial seat of political power, to the area's historical prominence as a crossroads for different migrating ethnic groups, and to the village's location in a major population zone with the highest population density in Malawi. Most of the oral texts were collected in one or other of the two major regional languages, Chinyanja and Chiyao.

2. Chinyanja-speakers accurately refer to the telling of a tale as *kuimba nthano*, or 'singing (a) tale'.

are collectively known in greater or lesser degrees by those present and taking part in its performance. My chief field assistant said of Dr Banda, the Malawian head of state, that he has told his *mbumba*, or female political supporters,³ that although their songs consist of only a few different lines and appear to be simple, they mean a good number of things. In part this is because of the stylistic effect of poetic allusion.

Self-Reference and Local Allegiances

The most common allusion of those taking part in the performance of Malonje song is that of the village itself or the name of the chief (whose name, in any case, derives from that of the village). During the singing of a composition not only do the pair of song leaders make allusive reference to the village and by extension those who live in it, but also members of the group chorus may insert their own phrases in *or outside* the song text, identifying those who are singing as members of the village. In part this phenomenon derives from the performance of wedding songs when the villages aligned with the bride and groom compete to 'outing' the other. Allusive self-reference to the home village of the singers serves to identify them as a cohesive group that has come to show the members of the other village 'what they can do'.

In this way the allusion is used both in the song itself and in extratextual insertions as a means of self-identification, common exhortation, and 'friendly' provocation. This leads to a certain repetitive banality and predictability in a number of songs, but at the same time invests the sung compositions with political and regionally rivalrous overtones that add to the meaning and intent of the song texts themselves.

During the recording of this material the singers were very much aware of the fact that they were being recorded. Many held the view that the 'rest of the world' would be listening to the tapes. In this way the number of occasions on which the singers utilized this particular stylistic device is probably over-represented in my recorded material. In fact, one of the 'big' women, the chief's senior sister (MZD), whose identification with the name of the village was as great as that of the chief himself, was a frequent contributor of such allusions.

These are not random allusions intended to fill otherwise empty spaces in a song. Rather, they are there both for the reasons given above and to serve an implicit political purpose. During the course of a taping session in which the

3. Also used as the generic designation for political songs, the word *mbumba* is the traditional kinship term denoting the female matrikin placed under the guidance and protection of an *nkhoswe*, or uncle/elder brother, the male head of an extended family. His *mbumba* refer to Dr Banda as 'Nkhoswe Number One'.

Chiyao women's initiation song *Mkeka wa Kunsamala* was performed, a temporary resident of the village originally from Mangochi referred by direct allusion to her home district and traditional political leader, Traditional Authority Chief Nsamala of Machinga District:

Ambe ndamire mkeka wa Kunsamala

Give me, I must sit on (the) mat of Chief Nsamala

Everyone present at the singing session knew the meaning of her allusion, for a number of villagers have kinship ties with individuals currently living in Mangochi and have taken part in weddings and funerals there. It was repeated several times throughout the course of the first five verses of the song, at which point the chief's senior sister moved along the line of singing and clapping women to 'whisper' in the errant song leader's ear. The succeeding verses did not include the line. It was replaced by

Ambe ndamire mkeka wa Kumalonje

Give me, I must sit on (the) mat of Chief Malonje

This latter line featured in the sung verses for the remainder of the song (three additional verses). At the end of the composition the 'big' woman, who was serving as a member of the group chorus, inserted a spoken line in Chiyao as an admonitory epilogue:

Tulamire mkeka wa ambudye Kumalonje

We must sit on (the) mat of uncle Chief Malonje

It is clear that the inclusion of an allusion to the chief (and by extension his people) of an area some miles to the north of the village in which the composition was being sung, and who has no political or kinship ties to those present, was an egregious *faux pas* on the part of the song leader. She did not lead the next song.

When local villages are mentioned they are in fact usually destinations to which the persona, or character, of the song is travelling to perform a specific task; while allusions to Malawi, and the major population centres of Blantyre, Limbe, Zomba and Lilongwe are usually depicted as places to journey to for political events, simply to visit, or to escape problems in one's natal village. In the Chiyao women's initiation song *Nje-Nje-Nje*, however, Kawinga, a Mozambican locale near the border with Malawi, is characterized as a place emitting the unending sounds of war.⁴ It is a place from which to flee. Malawi itself is cited mainly in political and development songs, for obvious reasons, although it also figures in many other types of song. The names of the major cities of Malawi and of foreign

4. This composition was imported into Malonje village by Yao refugees during the independence war in Mozambique in the 1970s.

places, then, do not offer the same sort of regional competition for recognition, nor do they operate as focuses for individual and collective identity as do neighbouring villages. When localities lying further afield are alluded to in a text there is usually no special allegiance implied.

While the song leader from Mangochi transgressed through her use of improper allusion and was corrected, it should be noted that when she took control of the sung text in the capacity of leader she 'naturally' referred to her own home district and chief. She was doing only what she could be expected to do as song leader. She just picked the wrong context in which to do it. This points up the fact that not only do allusions flesh out the imagined world of the creative text with parts of the real world, but that the 'real world' has an impact upon that text that goes beyond the confines of a creative and recreational pastime. The real world is contextualized in the work of art, while at the same time the work of art is situated within and affected by the exigencies of the real world. In this case, the external world of village identity and domestic politics impinges on the content of a sung poem in the course of its spontaneous delivery.

Allusion to persons in sung poetry, however, is not limited to those to the village chief. There are many occasions during the course of a performance when the singers of the songs are themselves identified, either by their own self-reference or by those taking part in the antiphonal response. As with reference to the chief and village this may also be inserted from outside the sung text. Names of relatives, particularly the names of the performers' fathers, may be inserted in the lines of the texts. Especially in initiation ritual the individual(s) leading the song may refer to the husband or 'owner' (Chinyanja: *mwini*) of the female singer, or to the *mnamkungwi*, or ritual specialist, who controls and ritually protects the ceremony. As a consequence the world of actual people, living and dead, comes to occupy a place in the sung repertoire of the village.

Political Personages: The Wider Canvas

Direct allusion to political leaders, e.g. the President or Ngwazi, Dr Hastings Kamuzu Banda, and to leaders of traditional districts is an established technique of song leadership. In *mbumba*, or political, songs mention of the Ngwazi is especially prominent; unsurprisingly, for these songs are performed in his honour. His name, moreover, features in many other types of song. *Kwende ku Che Kamuzu* or *Kwende kwa A Kamuzu* (Chiyao: Let's go to [Mr] Kamuzu), or *Ndikupita kwa A Kamuzu* (Chinyanja: I am going to [Mr] Kamuzu) are phrases used frequently in the corpus of collected songs. Depending upon the intent of the persona of the song and the narrative context these phrases may mean going to dance and sing before him, going to Lilongwe, the administrative capital of the country, or going to see him personally. While it is in the nature of allusion often

to leave the 'correct' interpretation up to the individual listener, in such cases as these it may also serve political purposes by restricting or obscuring meanings.

Knappert (1982: 25) reports for Swahili poetry: 'this old Swahili tradition of composing political songs with hidden allusions in cryptic language which only the initiated understand, is very much alive today'. So too, in contemporary Malawi, political songs contain allusions that only those privy to their meaning(s) can unravel. In these cases, though, the allusions are meant to relay messages to the political faithful. Hence, they are less restricted in nature.

The composition *Walira Fokasi*, sung in Chinyanja, provides a prime example of allusion to an actual event through reference to the actual people involved. The persons mentioned are Fokasi Gwede, formerly Superintendent of Police, Special Branch, and Fern Sadyalunda, a junior government minister. They were involved in a plot against the government led by the then Secretary-General of the Malawi Congress Party and Minister of State in the President's Office, Albert Muwalo. Gwede and Sadyalunda were imprisoned while Muwalo was executed for treason. In the main chorus of the second verse the singers portray Gwede and Sadyalunda as contrite and sorrowful figures.⁵

L2 *Walira Fokasi*
Fokasi has cried

CH *Walira eyae-eee walira*
He has cried eyae-eee (style) he has cried

[L1][L2] *Mai walira Fokasi uyo*
Mother (!) that Fokasi has cried

CH *Walira eyae-ee walira-a*
He has cried eyae-ee he has cried-d

[L1][L2] *Fokasi wadzimanga*
Fokasi you have arrested (yourself)

CH *Wadzimanga we-ekha-a*
You have arrested y-yoursel-If

5. Every line rendered by a leader pair is considered a line of verse unless included in a main chorus, which may be composed of both leader sung lines as well as chorus lines. When leader sung lines are represented singly they are still considered the same line of verse. In this case the lines performed by the leader pair are sung simultaneously though they have been rendered in separate ways. Leader one is always harmonized by leader two in the singing of a verse line unless otherwise indicated. The chorus lines are sung by the members of the group other than the lead singers who are taking part in the performance. For a key to the symbols used in the song texts see Appendix below.

[L1][L2] *Walira Najere*
Najere [Sadyalunda's clan name] has cried

CH *Walira eyae-eee walira*
She has cried eyae-eee she has cried

[L1][L2] *Iiiii walira Najere*
Iiiii (style) Najere has cried

CH *Walira eyae-eee walira*
She has cried eyae-eee she has cried

[L1][L2] *Najere wadzimanga—*
Najere you have arrested (yourself)—

CH *Wadzimanga yekha-aa*
She has arrested herself

[S1] *Nyadani azimai*
Be proud women

Jeff Opland has observed of Xhosa oral poetry:

The imbongi, or for that matter any other Xhosa oral poet, does not tell stories in poetic form. Since the praises on which the poetry is based often commemorate events, however, izibongo do refer to actions, but they allude to them elliptically rather than narrate them explicitly in the manner of the epic. (Opland 1983: 146)

So too, in the sung poetry collected from Malonje village allusion to events, whether contemporaneous or historical, imagined or real, is elliptically expressed through pithy phrases and condensed images. In the *mbumba* song just quoted the story of political miscalculation and tragedy is left largely unexpressed. In fact, the only relationship between the text and the underlying event is the mention of two of the main actors involved. Otherwise, nearly all of the story remains subtextual.

Not only is the connection made highly elliptical in manner, but the singers refer to the dramatically *imagined* reaction of the miscreants to the events that have befallen them as a consequence of their rash actions. The incident is artfully portrayed. In other words, it need not have really happened that way. Here the elliptical relationship to the actual event is twice removed. The incident is simply referred to by means of condensed images rather than narrated in a linear fashion. Furthermore, the action pithily portrayed in the text is a creative reconstruction that need never have actually occurred. Finally, the spoken interjection at the end of the main chorus not only exhorts the singers to continue their common efforts at singing and dancing, but also serves as a moral counterpoint to the misdeeds alluded to in the song.

Histrionics and History

In the *ngoma* song rendered in Chinyanja *Namulangen*i there is reference to a historical figure:⁶

Siyo—mama—siyo—mama
Siyo (style)—women—siyo (style)—women

Tsiku anafa A Gomani—
(The) day (Chief) Gomani died—

Ndilibe mawu hoi-sa
I have no words hoi-sa (style)

Mention of the great Ngoni chief Gomani, who was shot by the British in 1896, alludes to far more than just his name. Implicit in this reference is the tale of Malawian resistance to colonial rule. Ngoni chiefs, like those of the Yao, were notorious for their bellicose stance against British imperial rule and for their reluctance to depart from 'traditional' ways and adopt 'Western' values.

According to the oral historical account believed to be true by those in Malonje village, the Ngoni chief was a major adversary of the British. Upon his capture, which he allowed to happen because he was tired, the British tried to execute him by firing squad. The bullets, however, simply could not penetrate him. The British then attempted to chop off his head with an axe, but this also proved futile. After these unsuccessful attempts to kill him, Gomani grew tired and told his captors to pick a blade of grass whereupon he would lie down and the soldiers would be able to saw his head from his body. This was duly done. Because he had proved such a difficult adversary the British buried his decapitated body and took his head to England where it was delivered to the Queen as a war trophy. This is felt to have been a fitting tribute to the Ngoni warrior.

The song's male persona states that he was speechless at this period of history, though whether the person speaking witnessed the actual event or, indeed, was alive at the time is a moot point. Clearly, it was, however, a time of great trouble. The funeral of Chief Gomani would have taken more than three days, perhaps more than a week, while everyone mourned. During this period, the cattle were not taken out of their kraals and the people stayed inside their houses. Only 'big' people could have approached the *boma*, or headquarters, of the chief without being captured and killed by Ngoni soldiers. This is the cultural and historical baggage that is attendant upon the allusion to the famous chief's death.

6. This type of song is sung by the Ngoni people. Traditionally, it was sung when the men returned from war (see Mphande 1966). The song discussed here is now sung at beer parties or upon the death of a chief. This song is not 'traditional' to Malonje village. It has been imported by its current performer.

'In the Graveyard there is no Mercy'

In the corpus of Malonje song allusion does not confine itself solely to people and places. In a society that has always placed strong emphasis upon communion with the deceased in the form of ancestor veneration, the presence and occurrence of death has been accommodated and incorporated into a specific way of living and of viewing life. Although most villagers are nominally Christian, their respect for the dead, as expressed in remembrance ceremonies and cleansing rites, has seemed to represent the core of belief around which notions of Christian ideology have been appended. Belief in witchcraft and magic, although proscribed by the Christian churches, is also prevalent in Malonje village. Deaths, not surprisingly, may be attributed to witchcraft or evil intentions.

In the Chinyanja women's initiation song *Odi-Odi Tsekula*, for instance, the song begins with a woman visiting a female age-mate who does not want to open the door to her. Both women are quite formal in their mode of address towards each other, suggesting unfamiliarity between them. The woman inside the house asks the caller why she has called upon her when she has never visited her before. Through oblique references to the dead, death and the graveyard she then alludes to the visitor's intention to use bad magic. These allusions are given prominence as the first lines of various verses sung by leader one:

Akufa sadzigwiriza ee-e-e nanga ine?
Dead people don't grip firmly ee-e-e what about me?

Pakufa tidzangopita-a-a
When dying we will just be goi-i-ing

Akufa sadzigwiriza
Dead people don't grip firmly

Akufa sadzigwiriza
Dead people don't grip firmly

Kumanda kulibe nsoni eee
At (the) graveyard there is no mercy eee

Akufa sadzigwiriza
Dead people don't grip firmly

To the audience the allusion to dead people being past the worries and cares of life reflects the visitee's anxieties about the visitor's unknown intentions. The visitee expresses the sentiment that death is the condition of being past worldly cares. After all, in the graveyard death is final. Thus it is implied that opening the door to a stranger may lead to death. In the fifth line of verse nine (sung by leader two) the visitor replies to the woman's verbal prevarication:

Mai mutondiona

Mother (honour) you do see me

This counter statement alludes to the fact that the visitor is carrying nothing visible with which to harm the visitee. The implication is that, after all, the woman can see for herself. Yet, those in the audience know that the person intending to do harm could poison the other during her visit. Alternatively, the visit could simply be a reconnoitring mission to see the layout of the house. A magical line could be drawn at the entrance to the house or a root buried in the ground between the house and the *chimbudzi* (Chinyanja: [roofed] pit latrine) to poison the unsuspecting walker.

In another women's initiation song, performed in Chinyanja, the dead are likened to trees. *Amai Ndaima Pano* alludes to the frequency of funerals and the finality of death:

Amai ndaima pano, maliro ndi mtengo, sindinaone maliro a kale

Mother I have stood here, (a) dead person is (a) tree, I have never seen (a) funeral of (the) past

—*ndaima pano, maliro ndi mtengo, sindinaone imfa ya kale*

—I have stood here, (a) dead person is (a) tree, I have never seen death of (the) past

Maliro ndi mtengo

(A) dead person is (a) tree

This song, then, alludes to death. The female persona laments a death that has just occurred. She implies that funerals were scarce in the past. Her contention, 'A dead person is a tree', echoed by the chorus, refers metaphorically to the completeness of death, alluding to how a tree cut down falls to the forest floor:

[L1] *Ukamva gubudu maliro ndi mtengo amai ndaima pano*

When you hear *gubudu* (a) dead person is (a) tree, mother I have stood here

[L2] *Ati ukamva gubudu maliro ndi mtengo amai ndaima pano*

Ati (style) when you hear *gubudu* (a) dead person is (a) tree, mother I have stood here

CH *Maliro ndi mtengo*

(A) dead person is (a) tree

The finality of death is voiced in the idiophonic sound of a fallen tree, *gubudu*. The singer believes that death and funerals were more respected in the past. Death is now more frequent. Yet this presumption is based upon the observation that

people have become inured to the commonplace occurrence of death and burial. In the past they were not so inured as they are now. Then, a person could live to his or her teenage years before attending a funeral. As can be seen, the implications of these allusions are left largely unexplained. Yet they attain a concise summation of what they mean to express. The song alludes to a universal condition of man in the vocabulary and idiom of sung poetry.

Sexual Prohibition and AIDS

The female persona of the Chiyao wedding song *Mkaitanda Ine* enjoins her husband not to make sexual advances towards her in case he should contract a dreadful disease from such an unmindful action. This is expressed primarily through allusion. In the third line of the first verse the leaders set the backdrop for the cautionary statement:

[L1] [L2] *Mwanache mbeleche liso mbole-mbole*
Child I gave birth yesterday take it easy

The chorus then provides the allusion:

CH *Mkaitanda ine-eee—*
(You) do not provoke other thi-i-ings—

Kum'ona mwanache 'kwaŵa—
Seeing it child crawling—

Nikuitanda ine-eee
And then you provoke other thi-i-ings

Ngachidyog'opa chinyera kog'oya-aa
Without fearing dangerous 'venereal' dis-ease

Thus, the woman who has just given birth to a child is admonishing her husband to desist in his untoward actions. Not only is the sex act implied through euphemistic allusion, 'other things', but so is the traditional injunction against couples engaging in sexual intercourse before the new-born child is six months old. It is believed that should this injunction not be adhered to the man will become seriously ill and that if a traditional healer, or *sing'anga*, is not consulted right away the man may die. Through this premature act the man is believed to soak up the woman's 'unclean liquid', which then enters his bloodstream. When the impure liquid 'decays' it poisons him. It is the 'remains' left inside the woman after the birth of the baby that is considered 'dangerous'. It is clear that the

sexually contracted disease is considered deadly. In the last line of the song (the last line of the final main chorus) the singers replace the term *chinyera*, the traditional word for an impure ritual state of being, with *edzi*, or AIDS, the modern loanword for an equally deadly and mysterious disease contracted in the same physical manner:

CH *Ngadyidyog'opatu edzi kog'oya-aa*
 Without fearing this dangerous A-AIDS

Such a song as this, performed at a wedding by the 'big' women, serves as a mnemonic admonition to the newly married couple to observe the correct moral and ritual modes of behaviour towards each other. The message of wise counsel is couched in poetic language. It is not stated directly as in bland didactic formulas, but expressed by way of allusive reference. The allusive nature derives not only from the song's direct allusion to specific, but related, kinds of diseases. Rather, the opprobrious nature of such untimely actions are acted out verbally in front of the listeners. The allusive quality of the song attains its full force through the voice of the female protagonist. It is she who states her misgivings about the man's actions and who alludes to the consequences of such behaviour.

Generic Cross-Reference

Reference may also be made in sung poetry to other genres of verbal artistry. Allusion may display a self-referential aspect. As has been demonstrated, geographical place-names, historical personages and events, local chiefs, family members and moral injunctions may be alluded to in sung poetry. Poetic language, in the guise of another generic form, may also establish the allusive backdrop to a sung poem. In fact, without it the song may have no meaning at all.

For example, the women's wedding song *Tsamba Likagwa*, performed in Chinyanja, contains numerous repetitions of the phrase that makes up its title. If the phrase were absent, only personal names and terms of address would be left. The two-word phrase not only embodies the main semantic import of the song, but also alludes to a proverbial expression. To the audience the two words do not stand alone, but form the first part of the proverb, *Tsamba likagwa sabwerera mtengo*, that is, 'When (a) leaf falls it never returns to (the) tree'.⁷

7. There is another extant Chinyanja proverb that begins with the same two words: *Tsamba likagwa manyazi agwira mtengo*. This may be translated as, 'When (a) leaf falls, shame holds (the) tree', and can be taken to mean that whatever actions a child may perform will also affect his/her parents and family; see Salaun 1969: 108 where, however, neither English translation nor exegesis is provided.

Most listeners, then, would know to which proverb it alludes and finish, in their own minds, the incomplete phrase. As this song is sung by young women at weddings it can be assumed that the proverb and the ability to 'fill in the blank', as it were, would be within the ken of most of those present. The allusion to the proverbial expression through the use of an incomplete portion of it, however, is only part of the semantic adjustment that a listener must make. The listener must know the meaning of the allusion and subsequently apply it to the contextual backdrop of the song text.

The allusion, then, not only refers to the form of the proverb, but also to its content. Just as when a leaf falls from a tree, it never returns to it; so too, when a person possesses something, he or she must nurture it, otherwise it will decay or be lost forever. As with most proverbs, the meaning is general enough to have almost universal validity even when applied to specific social situations. In this case, however, the listener has the song text to which to refer for the specific application.

The singer directly addresses the man and woman in turn and advises them that they must regard each other in this light. (The anthropologist is also referred to by name in the text, with reference to his relationship with his own wife.) Thus, in the context of a wedding song the meaning of the proverb alluded to in the text becomes an advisory note to the newly-weds to safeguard the sanctity of their marriage. The allusion is achieved through the partial provision of an extant proverb, the complete form and meaning of which must then be applied to the overarching and interacting contexts of the song's text and the event at which it is being performed.

The Anthropologist as Character Actor

Less rarefied personages may also serve the poetic purposes of allusion. In a number of songs the visiting anthropologist features large as a contemporaneous allusion to present events. New songs can be and are composed to suit new occasions. Nevertheless, in some (and probably most) cases old songs are given new form and relevance by the inclusion of new and fresh allusions (cf. Gunner 1982). In one sense they are 'new' compositions, in that the people or events alluded to have never featured in a song in such a way before. It is through this poetic technique that current events can be included in an already extant repertoire. In fact, the established tradition of sung poetry proves highly sensitive to occasion and context. It is in the nature of Malonje sung poetry to incorporate the surrounding and immediate world of social and political life into the realm of creative expression. In a composition derived from a women's initiation song, *Che Giregi Zikomo*, sung in Chiyao at a beer feast, for example, the anthropologist is thanked directly for a service he had rendered the song leader when she had been

ill. I give verses 7 and 8 here. Due to the improvisational nature of the sung text the second leader has a difficult time harmonizing with the main leader:

- [L1] *Tinjijuga lipe-e—*
I will beg (a) long gra-ass (blade)—
- [L2] *Tinjijuga lipe*
I will beg (a) long grass (blade)
- [L1] *Ku Ingalande mkudya mmwedyi zikomo tinjijuga lipe—tinjijuga lipe—tinjijuga lipe-e*
To England you are going you thank you I will beg (a) long grass—I will beg (a) long grass—I will beg (a) long gra-ass
- [L2] —*ga lipe—ga lipe—ga lipe—ga lipe*
—beg (a) long grass—beg (a) long grass—beg (a) long grass
—beg (a) long grass
- CH *Lipe amao-o*
(a) long grass (blade) mother-r (!)
- [L1] *Tinjijuga lipe-e*
I will beg (a) long gra-ass (blade)
- [L2] —*ga lipe*
—beg (a) long grass
- [L1] *Che Giregi zikomo nambo tinjijuga lipe—tinjijuga lipe—mumbere mtera mmwedyi—tinjijuga lipe-e*
Mr Greg thank you but I will beg (a) long grass—I will beg (a) long grass—you have given me medicine you—I will beg (a) long gra-ass
- [L2] —*ga lipe—ga lipe—ga lipe—ga lipe—nasalire—ga lipe-e*
—beg (a) long grass—beg (a) long grass—beg (a) long grass—beg (a) long grass—I must tell you—beg (a) long gra-ass
- CH *Lipe amao-o*
(a) long grass (blade) mother-r

The anthropologist is referred to by his title and Christian name, Mr Greg. He is thanked by the persona of the song who also happens in this case to be the performer herself, for giving her medicine. In fact, the degree of referencing is less elliptical in this instance than is usual in sung texts. The situation alluded to is thereby rendered fairly distinct. Far less direct, though, is the metaphorical

allusion to the service rendered, an allusion that pervades the text in the phrase 'the blade of long grass'.

The grass blade represents the 'insignificant' favour that she requested of the anthropologist, i.e. the provision of medicine. Thus, the favour is alluded to with two very different styles of reference. One is highly metaphorical, the grass blade, and could only be deciphered by those privy to the urgent occasion. The other is an almost prosaic rendering of what had occurred, the straightforward reference to medicine being provided. Furthermore, the text is as much an allusion to the present circumstances of offering thanks as it is to the original favour. Through allusion, then, the anthropologist and the blade of long grass are joined together in the composition as agent and action.

The anthropologist is also referred to in other texts in which he has not escaped some degree of irreverence. In their article on the inclusion of anthropologists in the traditional narratives of the Kuna of San Blas, Howe and Sherzer (1986: 889) state, 'thus what these everyday little narratives do is fix and stabilise the identity of a certain problematic kind of foreigner as marginal natives [*sic*]'. Furthermore, laughter and trickery are seen as ways of 'preserving equality by defeating dignity and social distance' (ibid.: 888). To some extent this is also true for the placement of myself in the sung compositions of Malonje village. In the Chinyanja wedding song *Chinangwa Changa*, for example, the unpredictable anthropologist is accused of making off with the singer's cassava:

[L1][L2] *Chinangwa changa n'nasiya pa moto*
My cassava I left on the fire

CH *Ee-eee*
Ee-eee (style)

[L1][L2] *Ndikamabwera ndipeza palibe*
When I am coming back I find nothing there

CH *Yaya-aa*
Yaya-aa [nickname for a female age-mate]

[L1][L2] *A Giregi ee-e-ee*
Mr Greg ee-e-ee

CH *Ee-eeee*
Ee-eeee

[L1][L2] *A Giregi ee-e-ee*
Mr Greg ee-e-ee

CH *Yaya-aa*
Yaya-aa

The culprit normally cited in the song is an age-mate named Yaya. As can be seen, the chorus never wavers from its assertion that Yaya is the one who always makes off with the roasting cassava from the ashes of the fire. This is the usual rendition of the text. The leaders of the song, however, accuse the anthropologist of the recurring deed. This accusation, furthermore, is accomplished in the presence of the accused, so to speak. I was crouched in front of the singers recording the text.

The effect of this allusion is not to recall some event that occurred in the 'timeless' past but to situate the anthropologist within the current enactment of the song: 'when the members of a society deal with anthropologists through humour, they do more than put individuals in their place. They also create that place, by situating an elusive and liminal social category' (Howe and Sherzer 1986: 891). What is being alluded to at that moment, moreover, is the occasion happening there and then. A 'special' category of person is taking part in the present performance of a sung composition.⁸ That is the object of the allusion. Other associations may be adduced to provide further explanation.

The women are also 'playing' with me, as it were. As females who occupy the same generational level as myself these singers can be classified as potential marriage partners. Teasing, in this way, is characteristic of such a relationship. The allusion, however, is ultimately extratextual. The anthropologist is portrayed as a guilty protagonist in the event being acted out at the textual level, while the allusion created, intentionally through his referential inclusion in the text, is to his presence and participation in the performance of that text, that is outside of it.

The singers are, in effect, provocatively 'pointing' him out in the words of the song in order to underline his presence outside of the text. This action then accords, and at the same time recognizes, his singular status as special visitor. It is not that he occupies a 'liminal and elusive category' but, rather, that he is seen to participate in a number of intersecting categories that allow him to be utilized in a creative and entertaining manner.

This interpretation of the situation is given further buttressing by the inclusion of the anthropologist in the Chinyanja wedding song *Kumanda Kulibe Chisoni*, sung at a beer feast. The text of the song is also an allusion to an action and its implications, which are not spelled out in a narrative manner. The concise and oblique rendering of a statement in order to refer to an action that has occurred but is not textualized is a common stylistic trait of sung poetry. The consequence of such an occurrence may be stated in the text but the event may be and usually is left out of the discussion:

8. I prefer 'special' to 'liminal' as a qualifier because the people in Malonje village have lived and worked with Europeans for almost a century. Although Westerners are not deemed to be part of Malawian society, they certainly fit a category coterminous and coexisting with it. I was not part of a totally new and strange phenomenon, but a member of a known and, to be sure, stereotyped social category.

[L1][L2] *A Giregi mulibe chisoni*
Mr Greg you have no mercy

[L1][L2] *Mwan'tengera*
You have taken away my

CH *Chibade-eee*
Darlin-ng

Amama ndilire bwanji maliro?—
Mother, how do I cry (for) (a) dead body?—

Amama chisheri changa chapita dzulo
Mother, my dear one went yesterday

Amama ndilire bwanji maliro?—
Mother, how do I cry (for) (a) dead body?—

Amama chisheri changa chapita dzulo
Mother, my dear one went yesterday

The direct allusion to the anthropologist places him as a central actor in an unfolding drama. He has taken away the woman's loved one. What is left unstated is that the first person addressed (Mr Greg) is leaving with her husband who is in his paid employment. The woman plainly blames the situation on the employer. While away working in another location there is always the nagging possibility that the man will meet another woman who will receive the greater portion of his wages. If the husband has been the bread-winner of the family this can be serious—the thatch-roofed and mud-walled house can dilapidate in three years. Also, there may not be enough garden to support a single family depending upon its produce alone for food. Thus, there is the tragic likelihood that the first wife and family will starve. If the employer had provided her, as he has her husband, with a passport and transport she would be praising him instead. Clearly he has not. This is the main (unstated) import of her claim that he has no mercy.

There is, moreover, no mistaking in which camp the 'European' anthropologist has been placed. His direct inclusion in the text aligns him with White employers and the institutionalized system of migrant labour. Before independence (when passports were introduced) many men travelled to Northern or Southern Rhodesia (now Zambia and Zimbabwe respectively) with their employers to work as gardeners or cooks, or in other domestic jobs. In the song text the anthropologist has been conflated with the colonialist system. In part this is because he is conceptualized as being of a class apart from that of those in the village. After all he is European and enjoys what appears to be an enviable life-style. Certainly, he is envisaged to be as affluent as those other Europeans residing in the Zomba area

who have either been employed by foreign-funded government aid programmes or by Chancellor College, the largest constituent college of the national university, and who have in recent times been the largest employers of Malawians as domestic staff, including a number of people from Malonje village.

Yet once again, the inclusion of the anthropologist in the text must be seen as a form of humorous and artful expression. The singers of the song are teasing me because I am there, involved in the current performance. In contrast to the direct allusion in *Chinangwa Changa*, however, I am not characterized as taking part in a minor peccadillo, the jejune nature of which helps to enhance the humour. Rather, my metonymic association with an unthinking and impersonal system of labour exploitation has a more sobering effect.

Allusion, then, works in a number of interlocking ways in the song text cited above. The text explicitly states that I am taking away the woman's husband. Furthermore, it accuses me of having no mercy. What is left unsaid, however, is the substance of the allusion. The man has opted to remain in employment in an overarching capitalist system in which jobs are at a premium. He has chosen this option in spite of having to temporarily desert his family. The female persona of the song can only lament her personal misfortune. She and her children must reap the consequences of the man's action. The 'dead body' is the husband who may never return. The mention of my name cuts across this dramatic *mise-en-scène* of individual fortune set amidst an enduring politico-economic regime.

In one sense the direct allusion to the anthropologist serves to lighten the effect of the tragic import of the text. Seen in the light of the previously cited song the anthropologist can be viewed as a trickster figure who has light-hearted and humorous qualities, but who, in the dramatic portrayal of the present song, can take on the characteristics of an oppressor (cf. Basso 1979). It is humorous that the anthropologist has been allotted this role; but at the same time the contextual ambience is ambivalent.

Added to these levels of ambiguity is the fact of the anthropologist's presence and interaction during the performance. The composition was performed during the rumbustious and uncontrolled merry-making of a beer feast. Not only is the contemporaneous inclusion of the anthropologist in the song text an allusion to his involvement in its enactment, but it is also a reflection of the festival atmosphere that pervaded its public performance.

Conclusion

These examples underline the fact that the use of allusion in traditional Malawian song, whether to current objects and events or to those of the past, is a complex matter the full denotation and connotation of which must be 'teased out' from the ethnographic context in which the content of the song is placed. For each allusion has its own range of locally restricted associations that bring their own semantic

loads to bear upon the final meaning of the sung text. Each allusion, moreover, interacts with other allusions in the same text, and these must be combined to reach the final statement, as it were, of the composition.

In the past, treatment of allusion has been limited to consideration of the strict denotation of the word (or phrase), leaving the further personal, topical, and local associations derived from local knowledge and experience, largely unexamined. What can be seen from the range of examples given above is that allusion is intimately tied to the content and ultimate meaning of the sung poem.

It is in fact impossible to separate the concept of allusion from that of content. However, the manner in which content is illuminated by means of allusive technique is often through oblique reference and unstated inference. Inasmuch as the nature of language in sung poetry is characterized by compressed and allusive statement, it only serves to enhance the 'unvoiced' aspect of content. Even if the allusive device is directly stated, it still carries with it a load of cultural associations that most passive bearers of the oral tradition will call into play as a means of deciphering the ultimate message of its referent.

Thus allusion may be carried out by direct or indirect reference, but it will always leave some semantic residue unstated. It is this residue that, along with the surface content, will comprise the ultimate meaning of the sung text for the listener. This highlights the versatility of allusion for displaying current concerns in traditional texts as well as voicing moral codes of received knowledge in a new setting. Allusion is one of the most important devices used in the composition of sung poetry in Malonje village.

APPENDIX: KEY TO THE SYMBOLS USED IN THE SONG TEXTS

[L1]	lead singer (harmonized)
L1	lead singer (not harmonized)
S1	singer (other than leader)
CH	chorus (group)
!	exclamation
honour	honorific
style	stylistic sound
'—	ellipsis/elision
x—	run-on (singer continues to following line without pause)

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THE PRESENT TENSE AGAIN

SIMON SINCLAIR

THE use of the present tense in anthropology continues to receive critical attention. Taking issue with the work of both Fabian (1983) and Davis (1992), I should like to propose a way of understanding the use of the present tense in anthropology as part of the conventional dialogue found in all science.

Fabian's Thesis

Fabian treats contemporary anthropology very much on its own, isolated from other sciences, and sees it as at a point of significant historical development. Working backwards from this position, he provides a historical account of anthropology (1983: 2-35), describing its origins in some evolutionary theories prevalent in Europe in previous centuries. These theories are said to have derived from the Judaeo-Christian tradition a tendency to regard 'far away' as 'long ago'. The effect of Fabian's historical approach is to create a chronological funnel, wider at the earlier end and narrower towards the present, ending (as far as he can see) blindly in the future. I think that Fabian's isolation of anthropology from the rest

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of contemporary social and natural science not only provides a misleading historical origin for anthropology but also has serious consequences for his argument.

Fabian states that the absence of an adequate consideration of time in structural-functionalism and structuralism is part of the continuing (and probably unalterable) inability of anthropologists to acknowledge that the people whom they study are (or were) living on the earth at the same time as them (what he calls 'coevalness'). In anthropology there is 'denial of coevalness. By that I mean a persistent and systematic tendency to place the referent(s) of anthropologists in a time other than the present of the producer of anthropological discourse' (ibid.: 31). So, 'American anthropology and French structuralism, each having developed ways to circumvent or preempt coevalness, are potential and actual contributors to ideologies apt to sustain the new, vast, anonymous, but terribly effective regimen of absentee colonialism' (ibid.: 69). To this he notes 'the colonial involvement of British anthropology has been well documented, which is one reason why it will be little discussed in these essays' (ibid.: 174-5). This means that British functionalism is not analysed in detail, a rather surprising omission in view of the many criticisms of functionalism's failure to deal with the passage of time. It is not, however, Fabian's contentious history of anthropology that I wish to consider here but his views on the present tense.

Nowadays, the denial of coevalness by 'allochronism' (ibid.: 32) means using the present tense to talk about things that clearly happened in the past, so removing the referents from 'the dialogic situation': 'the present tense is a signal identifying a discourse as an observer's language' (ibid.: 86). The distancing and diminishing effect of the present tense is made worse by the habit of anthropologists, in their ethnographic accounts, of putting the people studied into the third person ('they') while implicitly putting the reader ('the dialogic Other' of the scientific community) into the second person ('you'), further distancing their informants by making them objects of study: 'pronouns and verb forms in the third person mark an Other outside the dialogue' (ibid.: 85).

Fabian then (ibid.: 105-41) describes the emphasis in science, including anthropology, on sight. He stresses the predominantly visual nature of scientific data and the importance attached to observations, and he approves pleas for more value to be attached to hearing and speaking. I mention this part of Fabian's thesis because I think that his own stress on the visual aspects of science may have diverted his attention from the pre-eminence of the heard and the spoken in scientific activity. Further, though these visual metaphors are partly due to the convention that scientific evidence should be communally observable, there is another aspect to their use that he has overlooked because of the errors of his main thesis about the present tense.

These errors are as follows. The present tense in anthropology, rather than separating anthropologists and the people they study, in fact brings them together—the use of past tenses is much more likely to cause such separation. The use of the third person, rather than excluding other people from the dialogue, in fact allows them entry to it. Removal of other people from the dialogue is,

therefore, not effected by either of the methods Fabian describes, but by either or both of two other methods: by using past tenses or by denying them personal qualities (principally either by denying them subjectivity or by interfering with their independence as participants in the dialogue).

As a preliminary to elaborating these assertions, I now summarize the way Davis (1992) has categorized the uses of the present tense in anthropology, as I find some instructive omissions in his account.

Davis's Account

Davis's account of the way that present and past tenses are used in anthropology is practical and realistic. He lists the ways he has found the present tense being used, using empirical examples from anthropological literature to illustrate, though not to derive, these categories. He then comments on some lack of clarity that may arise from the use of the present tense and suggests that past tenses might with benefit be used more often.

His discussion of the uses of the present tense is prefaced by the statement, 'in English we have at our disposal a repertoire of eight uses of the present tense', and then, apparently as straightforward amplification of this assertion, 'people write eight kinds of thing exclusively in the present tense' (Davis 1992: 206). He lists eight kinds of thing, grouped into three categories. The first three are participatory, as in synopses, liturgies and stage directions; the next two are observational, as in describing pictures, photographs and maps (with anthropological analogies); the last two are scientific, as in statements that are true by definition or that have been demonstrated to be true by induction and experience.

The three kinds of thing described in the participatory category are exemplified by extracts from operatic, religious and dramatic writings. In these formal, non-scientific enterprises, actions have a relatively fixed procedure and words spoken or sung are pre-ordained. Despite being put in the participatory category, such performed and spoken activities can only be partially or alternately participated in by the readers of these accounts (as audience, congregation or actors). Where liturgies and stage directions (two examples in this category) are followed, speakers and actors do not say what they as free agents think or do at the time but rather what they think or do as actors following a script. Synopses, the third example in this category, are provided by one knowledgeable observer for other observers, whose participation is dependent on their observation of actors, who are themselves constrained by a script. Synopses, in fact, quite apart from their directive educational force, represent a further shift away from unrestrained participation.

In the observational group, the things that are described in the present (pictures, photographs and maps) are all artefacts, explicitly permanent and continuous and available to anyone else present. Although for Davis case-histories

and significant incidents in ethnographic research are analogous in some way to photographs, it is clear that the things in this category that are discussed in the present tense are separate in both space and matter, though not in time, from the person discussing them. Given that the uses of the observational present that he describes seem therefore to relate to quite specific objects or analogous situations, it is confusing when he later states that 'we conventionally generalise in an "observational" present' (1992: 211). It would in fact seem more consistent (and hence logical) to put generalizations in the third, scientific group, along with laws and tautologies. But I do not think that calling this last category scientific is accurate. I shall propose that science depends on argument, and there is obviously little question of either laws or tautologies being the subject of disagreement and hence argument.

Unless I have very much mistaken what Davis means by his categories, I think there are five other important ways in which the present tense is used in anthropological writing and which, indeed, (with one exception) he has used in his article. Even if I have misunderstood him, I think it is worth drawing attention to these ways in which the present tense is used. My examples are taken from anthropological literature.

Other Uses of the Present Tense in Anthropology

Type 1. The present tense is used for reporting publicly observable data.

- 1a. These [spondylus shells] are freely, though by no means easily, accessible in the coral outcrops of the Sanaroa Lagoon. It is from this shell that the small circular perforated discs (*kaloma*) are made, out of which the necklaces of the Kula are composed, and which also serve for ornamenting almost all the articles of value or of artistic finish which are used within the Kula district. (Malinowski 1922: 367)
- 1b. The Andamanese belong to that branch of the human species known to anthropologists as the Negrito race. They are short of stature with black skins and frizzy hair. The Nicobarese, on the other hand, resemble the races of Indo-China and Malaya, and have brown skins and lank hair, and are of medium stature. (Radcliffe-Brown 1922: 2)
- 1c. So Bushmen survive in the most rigorous places; they survive in the dense, mosquito-ridden papyrus swamps of the Okovango River, steaming like a jungle and dangerous with snakes and fever, where the River Bushmen live, the only Bushmen with plenty of water; they also survive on the vast, rolling steppes of central Bechuanaland, the territory of the Gikwe Bushmen, who for the nine dry months of the year have no water at all and do without it. (Thomas 1959: 26).

- 1d. The Gypsies or Travellers are dependent on a wider economy within which they circulate supplying goods, services and occasional labour. Unlike migrant workers moving from a single locality to another for 'settled' and wage-labour jobs, Gypsies operate largely independently of wage-labour. (Okely 1983: 49)
- 1e. Anthropologists do not write exclusively in the present tenses. (Davis 1992: 209)

These excerpts show the use of the present tense for describing objective factual data, available to anyone else present. There is obviously a range of factual objectivity in these extracts—the statements in 1a are not controvertible in the way those in 1d might be; especially with the passage of time, the situation may change, as implied in the statements in 1c. But, at the time of observation (and, by convention, also of writing) these certainly were facts, permanent and objective, as permanent and objective as any facts described in similar statements in natural science.¹ There may, of course, be changes not only in the factual circumstances described, but also in the manner of description, or the categories used—the racial categories in 1b were important at the time of writing.

Type 2. The present tense is used for making observations into public spoken statements.

- 2a. 'Poverty', I was repeatedly told, 'resides in the anus of the Brahman'. (Parry 1985: 621)
- 2b. *Adalo*, like the living, crave pork. (Keesing 1982: 128)
- 2c. Sharing rights for pregnant women are particularly emphasised by the Hadza: they have the right to ask anyone for food at any time and are believed to be at risk if they are refused. (Woodburn 1982: 442)
- 2d. There are those, like André, who really believe that the pygmies are inferior and are meant to be treated like slaves, but for the most part the villagers are much more sensible and realistic. (Turnbull 1961: 161)

The first of these excerpts is an example of reporting speech directly. The others show how speech is indirectly reported: they are examples of writing in the present tense what people have said in the present tense about what they believed or thought, at the time they spoke, which is in the past. Such beliefs and thoughts

1. See for example Egan *et al.* 1992: 522: 'The human immunodeficiency virus-1 (HIV) is neurotrophic and enters the nervous system soon after initial infection. One long-term consequence of this brain penetration is the development of progressive impairment of cognitive and motor function due to a direct effect of the virus on neurons.'

expressed in speech are not themselves publicly observable (unlike the facts that Type 1 statements are concerned with) although the speech is itself publicly observable, and may be about publicly observable things or events and may have publicly observable consequences.

Here the present tense is the tense in which the original statements were made. The author has heard the statement, translated it (and transformed it in other ways) and written it in the same tense as that in which it was spoken. The speakers may be unnamed (as in 2a), only implicitly named (as in 2b), generically named (as in 2c) or individually named (as in 2d). This lack of consistent attribution may lead to confusion with other types of statement made in the present tense, particularly those of Types 1 and 5, where originators of statements are usually not explicitly named.

Because the preparatory work for their studies did not involve much listening to other people talking, neither Davis nor Fabian uses the present tense in this way; for the same reason, nor do I.

Type 3. The present tense is used for reporting writing.

- 3a. As Lienhardt says, the action of *thuic* is relatively trivial, yet 'the principle involved...is similar to that which obtains in symbolic action in situations which, by their very nature, preclude the possibility of technical or practical action as a complete alternative (*ibid.*)'. (Ortner 1978: 6)
- 3b. Whatever else it may also be, sacrifice can be regarded, as Socrates says in the *Euthyphro*, as being in a sense a 'commercial technique', a way of doing business between gods and men. (Evans-Pritchard 1956: 224)
- 3c. In his discussion of Mauss's *fait total social*, Lévi-Strauss (1950, pp. xxiv-xxx) gives a special place to social anthropology within the social sciences, by virtue of the otherness and strangeness of the societies studied. (Lewis 1980: 218)
- 3d. Maxwell Owusu, in an essay 'Ethnography in Africa' (1978), argues, on the basis of writings considered exemplary, that almost all the 'classical' ethnographers failed to meet one basic condition: command of the language of the peoples they studied. (Fabian 1983: 32)
- 3e. Rosaldo, for instance, argues that even though Evans-Pritchard's account of how he did his fieldwork reads a bit bleakly, that is a characteristically British style—'tongue-in-cheek understatement', perhaps even a deliberate attempt to exaggerate the overwhelmingness of the odds against producing such a fine book (Rosaldo, 1986: 89). (Davis 1992: 209)

In 3a, as in 2a, the writer is quoting directly, but this time from another writer and not a speaker, despite her use of the word 'says'. Before quoting Lienhardt,

she introduces him in the present tense, as I introduce her in the present tense. In 3c, 3d and 3e the writers are reporting what other writers have written in the present tense, in a way identical to the indirect reporting of speech described in Type 2 above. The use of the present tense is combined with an acknowledgement of the past nature of the event of writing by the inclusion of dates (see 3c, 3d and 3e). Like Ortner, the authors of 3d and 3e are referring not to other speakers but to other writers, even though they are reported as speaking and arguing. Conversely, in 3b 'as Socrates says' could be expanded to 'as Plato says (or writes) Socrates says', because what Socrates said was recorded in writing by Plato in the *Dialogues*. In my terminology, Evans-Pritchard has conflated Types 2 and 3, preferring Type 2.

Type 4. The present tense is used for stating the writer's personal position (views, beliefs, feelings), as distinct from the writer's theories.

- 4a. I am personally rather tolerant of disorder. (Douglas 1966: 2)
- 4b. My two fears are distortion and inaccuracy, or rather the kind of inaccuracy produced by too dogmatic a generality and too positivistic a localized focus. (Said 1991: 8)
- 4c. I am divulging here practices and theories that the Baruya have striven fiercely to keep secret, prudently, obstinately concealing them from the whites, whose contempt and aggressiveness they fear more than anything else. (Godelier 1986: 51)
- 4d. In the end, I cannot accept what I appear to be granting now: that anthropology could ever legitimately or even just factually circumvent or preempt the challenges of coevalness. (Fabian 1983: 38)
- 4e. I should say that I think I understand Evans-Pritchard's reasons for using the present tense so generally in the Nuer [*sic*]. (Davis 1992: 212)

There is obviously no equivalent of 2a and 3a here, because there is no need for writers to put an account of their own position at the time of writing in reported direct speech in the present tense. It is confusing and unnecessary to write, for example, 'I believe that "I believe that..."', so the excerpts here demonstrate the simple use of the first person in the present tense. The purpose of this use of the present tense is to be self-explanatory, to help the reader understand the writer and the writer's position, which may make the statements in the writer's theories (Type 5 statements) more understandable too. This aim is not always realized.

Type 5. Lastly, the present tense is used for making assertions of varying generality or, in other words, stating theories. These assertions are not statements of objective fact (Type 1), nor of what others have said (Type 2) or written (Type 3), nor of the writer's own personal position (Type 4). Based on all these other sorts of statement in the present tense, they are rather new conclusions, which can then themselves be argued about.

- 5a. The close relationship between parent and child, which has such a decisive influence upon so many in our civilization that submission to the parent or defiance of the parent may become the dominating pattern of a lifetime, is not found in Samoa. (Mead 1943: 168)
- 5b. However, the obstinate fidelity to a past conceived as a timeless model, rather than a stage in the historical process, betrays no moral or intellectual deficiency whatsoever. It expresses a consciously or unconsciously adopted attitude, the systematic nature of which is attested all over the world by that endlessly repeated justification of every technique, rule, and custom in the single argument: the ancestors taught it to us. (Lévi-Strauss 1972: 236)
- 5c. Though post-modern ethnography privileges discourse, it does not locate itself exclusively within the problematics of a single tradition of discourse, and seeks, in particular, to avoid grounding itself in the theoretical and commonsense categories of the hegemonic Western tradition. (Stephen A. Tyler 1986: 129)
- 5d. Enlightenment thought marks a break with an essentially medieval, Christian (or Judeo-Christian) vision of Time. (Fabian 1983: 26)
- 5e. The case for writing abstraction in the present tense is also a strong one. (Davis 1992: 215)

The present tense is used here in a way that is similarly assertive but less law-like than that which Davis describes in his scientific category. These statements are assertions, of a general or specific nature, which have been or will be justified; they are the writer's theories, not the writer's views (which are Type 4). Unlike views, theories are not simply an expression of the writer's position and can therefore (unlike views) be detached from the writer for the purposes of free discussion and argument.

There is obviously a danger that statements of Type 5 may be confused with other sorts of statements made in the present tense, particularly with Type 1 objective statements. Something of this potential confusion may be seen by considering the excerpts above, taken out of context as they are. For example, 5a might be confused with a Type 1 statement, and 5b with a Type 4 statement. The fact that a writer's views (Type 4) and theories (Type 5) both become Type 3 when reported may be a further source of confusion. For clarity in later discussion, I shall call the method of reporting someone else's views Type 4 in

Type 3, and someone else's theories simply Type 3. Given this potential confusion, every writer's job is to be clear about the status, origins and authority of everything that he or she writes. Readers may then disagree with either the form or the content of the assertion or its justification.

The Basic Dialogue of Types 3, 4 and 5

Fabian's contention that the use of the third person marks 'an Other' outside the dialogue implies that dialogue can only take place between two people, which is wrong both etymologically and in practice. Although originally dialogue meant a spoken discussion between two or more people, one person alone can conduct an internal mental dialogue. Discussion and argument, either external or internal, is the basis of science.

I have described above the use of the present tense in three related ways (Type 3, to report other writers; Type 4, to provide an account of the writer's own position; and Type 5, to make assertions that can be argued with) and now suggest that these three uses provide the basic form of the written dialogue. The writer (as Fabian describes) is in the first person ('I'), the reader is implicitly in the second person ('you') and other writers are in the third person singular or plural ('he', 'she', 'they'). The present tense in such dialogue then has precisely the opposite effect to that which Fabian says it does: placing everyone who might have something to contribute to the dialogue in the present tense unites them (that is, the writer, other writers and the reader) in time and place (that is, in the reader's present and the reader's person). By the use of the present tense, the reader is enabled to take part in this conventionally created mental dialogue in the reader's real subjective time. While the immediate exchange is between the writer and reader, placing other writers in the third person does not bar them from the dialogue but enables them to be heard.

Removal from the Dialogue by Objectification

In natural science (which Fabian and Davis hardly mention), dialogue may be conducted about Type 1 statements in the present, dealing with the publicly observable permanence of nature, or (as in history) about such statements in past tenses, which might deal, for example, with accounts of either the present writer's or other people's actions and observations. But whether in past or present, there is no question of inarticulate nature joining in the dialogue. In human sciences, the people studied may or may not be involved in the dialogue, depending both on

the nature of the question and the writer discussing it. The question of the measured height of a group of people, for example, is determined by using only publicly observable methods; to answer it, there is no need to listen to the group's views on this or any other matter.² But where the actions of a group of people are concerned, the writer may either continue to act simply as an observer (and write Type 1 statements) or may listen to statements about what they are doing and report them (as Type 2). The writer may then treat their statements in two ways, either as objective data (again, Type 1), or as personal views or theories (Type 4 in Type 3, or Type 3). In the first case, with other people's statements treated as objective data, argument is about what they say (with them excluded from the argument); in the second two cases, argument may still be about them but they now have a voice in the argument.

Placing informants in the third person and what they say in the present tense does not, then, automatically include them in the dialogue. It is whether or not they are treated as having views or theories that determines inclusion and on what terms. Full inclusion is only granted if informants are considered to have theories, which can be argued with, as well as views, beliefs or feelings, which cannot.

A different way (not too whimsical, I hope) of considering these different roles in the dialogue of those present in the third person, provided they are allowed a voice, is based on the roles of the various participants in an English trial. Here the jury (readers) are addressed, as 'you', by a barrister (the writer), whose Type 5 conclusions they judge. The barrister uses the third person to refer to other counsel and their arguments (in Type 3 statements) and to witnesses and their subjective evidence (in Type 4 in Type 3 statements). But if people are not granted a voice in such a trial (i.e. if their Type 2 statements are treated as Type 1 data), the effect is quite different. In this case, the informants (that is, other people) cannot give evidence or argue about it, or judge the argument; they themselves and their statements now actually constitute the evidence exhibited, objective and inanimate.

2. For an example of some human scientists' account of their actions, see Takei *et al.* 1992: 506: 'We sought Mental Health Enquiry data on all first-admission patients discharged from psychiatric hospitals in England and Wales between 1976 and 1986 who received an ICD-8 or ICD-9 diagnosis of either affective or schizophrenic psychosis.' Such accounts are, interestingly, often put in the passive voice: 'An intravenous cannula was inserted into a forearm vein and sealed with a rubber bung. The cannula was kept patent by flushing it with Heprinse (0.5 ml, 50 units heparin) after samples of blood were taken and the first 2 ml extracted at each time point was discarded...Prolactin levels were measured by fluoroimmunoassay (LKB method) as described by Lovgren *et al.* (1985)' (Lucey *et al.* 1992: 518).

For an example of such an account given of other human scientists' actions and observations, see Tyler *et al.* 1992: 481: 'In 1983, close linkage was found between Huntington's disease (HD) and a DNA marker (G8, locus D4S10) in two large kindreds, one from USA, the other from Venezuela (Gusella *et al.*, 1983). The linkage was confirmed by studies in the UK (Harper, 1986) and numerous other countries; data pooled from a total of 70 families showed no evidence for more than one locus for HD (Conneally *et al.*, 1989).'

Removal from the Dialogue by Denying Autonomy

Such denial of personal subjectivity by objectification is, therefore, one method of removing other people from the dialogue. Another method of removal by denying personal qualities is by interfering with the autonomy of the notional persons involved in the dialogue. For example, the reader (previously in the second person, 'you') can be removed from the dialogue with the writer ('I') by the latter's use of the first person plural ('we', 'us', 'our'), which forcibly associates the otherwise distinct reader with the writer. This conscription makes it more difficult for the reader to dissociate from, in order to argue with, the writer. Fabian is quite fond of this device, using it, for example, four times on one page (1983: 88). He also favours the use of the impersonal 'one' (e.g. *ibid.*: 78), an anonymous third person singular that is neither 'he' nor 'she', and so has a similar effect in preventing dialogue. Explicit identification of the reader as 'you' may also interfere with the reader's own participation in the dialogue; to the extent that the reader could be anyone, ascription of a particularity (an opinion or a question, for example) denies the reader's anonymous generality. This device is often used to set up arguments that the writer then easily disposes of (the 'Aunt Sally' or 'straw man' ploy).

To the extent that any academic argument in the present tense is with identifiable individuals rather than with groups of people, Fabian is right about the distancing effect of the use of the third person, but only in the plural. The ethnographic use of collective nouns, which may have an objectifying aspect, also limits argument, only permitting it on the assumption of the homogeneity of the group. Fabian frequently uses 'anthropology' as such an objective collective noun, using it five times in this way on one page (*ibid.*: 143).

These uses of 'we', 'one', 'you', 'they' and 'it' are all examples of rhetoric, which uses language itself, rather than argument, to convince and persuade.

The Errors in Fabian's Thesis

I contend, then, that Fabian's thesis about the present tense and the third person in anthropology is wrong. These linguistic forms, rather than distancing other people, are the conventional way of bringing together people who have, or have had, things to say and write on the subject under discussion. Fabian's stress on the unbalanced preponderance of visual accounts in anthropology and his wish to promote the heard and spoken is consequently misplaced—the implicit stress in anthropological texts is already on the heard and the spoken of conventional scientific dialogue, into which is incorporated what has been written and seen (and touched and smelt), as well as what has been heard. Indeed, in anthropology of all sciences the privilege given to verbal communication, both spoken and written,

can much more easily be criticized than further encouraged. The use of visual metaphors in texts is no doubt partly demonstrative, as Fabian describes, but is also partly related to the fact that anthropological accounts, like other scientific accounts, are written and therefore read and that reading is dependent upon visual activity; the written dialogue is read and then transformed by the reader into the crucial internal dialogue.

The important point arising from this discussion is that these conventional conversational uses of the present tense (which include, confusedly, somewhere among them 'the ethnographic present') are not just found in anthropology but in all Western scientific enquiries, past and present (a point that I suppose Fabian misses because of his singular historical approach). I shall call this tradition 'the convention of academic scientific enquiry' for the following reasons: 'academic' describes its origin in Plato's Academy; 'scientific' means that it makes knowledge; and 'enquiry' describes the general process of approaching, rather than claiming, truth.

This man-made convention of scientific dialogue is one way round a practical problem that Fabian almost acknowledges right at the end of his book, when he states that the denial of coevalness ultimately 'rests on the negation of the temporal materiality of communication through language' (1983: 164), a real temporal materiality that means that even when we—I use the first person plural because I do not think there can be any argument—talk to one another in the so-called here and now, we cannot enter into precisely simultaneous exchanges of talking and listening. We talk in response to what someone else has said, which, however recently, is definitely in the past; but, for the purposes of communication, we assume that they still hold in their mind what they have just expressed in speech.

An Error in my Contention So Far

So far I have talked about the present tense as the only tense in which conventional academic scientific dialogue is conducted. As, however, a brief consideration of this sentence will reveal, some modification of this assertion is needed. Although the present tense enables listeners and readers to join the dialogue by aligning their own subjective present with the present tense of the dialogue, this subjective present has past and future, as well as present, aspects. Linguistically, these may be called 'primary' tenses in English, as they are in Latin and Greek. In the first two sentences of this paragraph I have used three primary tenses, the perfect tense ('so far I have talked about') and the future tense ('as a brief consideration of this sentence will reveal'), as well as the present tense ('is conducted', 'is needed'). For the remainder of this essay, I shall use the division of tenses into 'primary' and 'historic', where historic refers to all past tenses (e.g.

imperfect, 'I was talking'; simple past, 'I talked'; and pluperfect, 'I had talked') except the primary past tenses, the most common of which is the perfect.³

The Use of Historic Tenses in Anthropology

Fabian's approval of practical and realistic methods of enquiry involves insisting on the accurate recording of linear time, on affirming coevalness and denying allochronicity. Given the real practical problem of temporal materiality, any such insistence in speech could only prevent spoken communication between people, and in writing would prevent scientific discussion.⁴ His positivist position on the recording of time is not even found in discussions about the nature of time itself, where the form of these discussions is as conventional as ever. Coveney and Highfield (1990), for example, describe ways in which time may go backwards, but their discussion and conclusions are both still in the primary tenses.

Using historic tenses is an important way of removing other people from the dialogue, distancing the reader from some people whom the use of the present brings close. This is a differential distancing, in that the reader may still be close (or indeed much closer) to the writer (and to the writer's views in particular) but tends to become less so to others. The tendency to distance other people in this way may only be overcome by the sort of effort that Collingwood has notably advocated, the attempt at the subjective recreation in the present of someone else's past state of mind (Collingwood 1989: 282-302).

Of the anthropological works that are written predominantly in the historic tenses, Fabian mentions Lévi-Strauss's *Tristes Tropiques* (1973) and Turnbull's *The Forest People* (1961); Barley's *The Innocent Anthropologist* (1983) is also in this category. Compared to the average ethnography, these autobiographical accounts may be more chronologically and factually accurate, but they may also be uncheckably inaccurate. They are more emotionally evocative (more elegiac, lyrical and funny, respectively), a consequence of these three writer's views being given greater prominence than their theories, which can only be derived from

3. I plan to discuss elsewhere other aspects of this dialogue and its dependence on a surprisingly large number of assumptions that are either unverifiable or wrong, a dependence which justifies the description of scientific dialogue as ritual. This discussion will include the contention that the uses of the present tense described above, with other primary tenses, constitutes an important and probably necessary aspect of both cognitive modernism and technological advance.

4. Scientific dialogue is usually conducted in the indicative mood, which implies reality (like the 'is' in this sentence), though other moods may be used. One example of the use of the subjunctive mood is my 'could' and 'would' in the sentence to which this note is appended, which imply a hypothetical or conditional state of affairs, though still in a primary tense; another is 'should' in 4e.

argument with other people. In other words, as regards such autobiographical narratives in historic tenses (but only as regards them), Fabian is right that another person in the third person singular is distanced from the dialogue, because in these books the dialogue does only have two people in it, the writer and the reader. With such autobiographical texts in the historic tenses, the reader is more closely aware of what happened as experienced (or, at any rate, described) by the narrator. This awareness produces an informative and emotional effect on the reader (similar to the effect of Type 4 statements of the writer's position—views, beliefs and feelings—on the reader), both effects depending, as in fiction, on the writer's approach to what is being described. Any such emotional effects, whether generated by reading in the armchair—as in these cases—or by being in the field, do not at the moment have generally accepted applications because of the lack of general acceptance in anthropology of a subjective psychology of emotion.

Where only historic tenses are used in non-autobiographical narrative, there is no dialogue between the reader and the writer at all—the latter has disappeared, leaving only objective statements, similar to Type 1, but in historic tenses about the past. This approach may be combined with the academic dialogue in the primary tenses, as is found in academic history and in natural science. An example of this combination of historic and primary in anthropology is to be found in another paper by Davis (1991: 12): 'Uduk construed duration as a series of alternations; Kédang added to that a sense of direction, and hence of cycles of renewal. They contrast with Yemeni tribesmen who construed time as a necessary sequence of events, and who used a generative model to explain the setting of their lives.'

In both sentences, Type 2 information has been put in a historic tense, similar to Type 1 and treated objectively. Informants have therefore been excluded from the scientific dialogue in the primary tenses, both by objectification and by the use of historic tenses, and, in consequence, they can only be observed and talked about. It is no doubt historically correct that the peoples described were, in the past, seen and heard by observing and listening anthropologists who, also in the past, recorded what they saw and were told but, as historically correct facts, these are not live contentions that are easily discussed or are apparently worth discussing. This lack of argument is brought out in two different ways by the statement in the present tense in the second sentence ('they contrast...'). First, the absence of the writer from this statement leaves it unclear to the reader whether it is of Type 1 or Type 5, whether it is an objective fact or the writer's assertion. Secondly, contrasting (and comparing) is not generalizing (Leach 1961: 6)—generalizations are Type 5 assertions, the synthetic products of observation and verbal argument, provoking further observation and further argument.

The unusual effect of the passage is increased by the absence of the definite article for the collective nouns ('Uduk' and 'Kédang', rather than 'the Uduk' and 'the Kédang'). To the use of historic tenses is added collective but apparently indeterminate objectivity, making an even greater barrier to the peoples discussed in the dialogue being admitted to it.

Conclusion

I have argued that Fabian's thesis about the use of the present tense and the third person in anthropology is wrong. I have suggested that these and other conventions of academic dialogue allow anthropologists to give to the people that they study the same status that they give to each other, in a way that is currently precluded by using historic tenses. It is a different matter whether anthropologists do in fact grant equal status to the people they study, in academic dialogue as well as in the field. In the past, some anthropologists may have been prevented from being individually introspective or sociologically reflexive by incorrect notions about the people they studied being in important ways different from themselves, though even evolutionary errors do not deny dialogue. Anthropologists should not now easily dismiss the relatively egalitarian assumptions of their tradition of enquiry by treating the people they study as qualitatively Other because chronologically Over.

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COMMENT

DISPERSED ALLIANCE, TERMINOLOGICAL CHANGE AND EVIDENCE

The general problem of Parkin's recent article, 'Dispersed Alliance and Terminological Change in South Asia' (*JASO*, Vol. XXIII, no. 3, pp. 253-62), has been raised by its author in an earlier and related publication (Parkin 1990). There he discredited others for their 'uniform reliance on slender and suspect evidence' (*ibid.*: 70). These attacks against 'such figures as Barnett, Carter and above all Dumont' (p. 253) are now continued, so the question of 'evidence' should be discussed. Coincidences between Parkin's article and my own comparative ethnographic work in middle India (1982, 1983) will not, however, be elaborated in the following examples of 'evidence'.

1. Anyone who actually takes the trouble to read Dumont's contribution of 1966 will have to notice how Parkin misrepresents essential statements. Dumont has never attempted 'to prove' that northern address terminologies 'were as classificatory as' the southern reference terminologies (p. 253). He has, moreover, repeatedly (1975: 198; 1983: 23, 160) renounced his publication of 1966. Parkin's article simply ignores this radical shift.

2. The most relevant ethnographic data in Parkin's article relate to 'the rule of delay of three generations' (p. 256) among the middle Indian Juang. Thus the 'Juang-type system' is born, representing the crucial 'third stage' (p. 258) of the overall scheme. In fact, these data are just a good example of invented ethnography. Neither McDougal (1963) nor any other ethnographer of the Juang has ever reported such a 'rule'.

3. Who are 'the Malto' (p. 258)? In almost all his previous publications Parkin reified categories designed by linguistic experts (e.g. 'the Munda' or 'the Koraput Munda') as if they were social groups whose social norms and categories differed from those of speakers of other languages within the village, tribe or region where these linguistic categories could be applied. In a kind of escalation, in this latest article (and its predecessor) Parkin has now reified a language—Malto—into a social group when, it seems, the tribe called Sauriya Pahariya by officials and Maler by its members is meant. By this technique it will be impossible to detect other 'evidence' on 'the Malto', since the fairly large number of ethnographers of the Sauriya Pahariya use the correct name. Thus the uninformed reader is likely to think that the data on the group rechristened 'the Malto'—including the Table, or caricature of a kinship terminology (p. 259)—were the result of the author's primary ethnographic research. No evidence of the real sources is supplied. Since (after Parkin 1990), this is the second attempt to bestow reality upon a hitherto unknown tribe called 'the Malto', we can presume that evidence is created by repetition.

I will not bother further about South Asian ethnography here but concentrate my other comments on 'typology and transformation', i.e. the theme of the Moscow conference at which the paper on which Parkin's essay is based was originally given. My own work on 'dispersal' or *Zerstreuung* (1985: 178) of invariant ties of intermarriage in South Asia will not be introduced, even though it deals with the by now familiar Juang and Jat, north, south and central Indian as well as Omaha marriage rules, and the equation, in contrast to the differentiation, of MB and WF. It was written in a foreign language, too difficult to be checked. Only those colleagues I had invited as guests to the Berlin Institut für Ethnologie had been forced to accept copies of the paper.

Carrying coals to Newcastle may be a better idea. More than two decades ago, a prominent representative of Oxford anthropology gave the following verdict:

What I wish to propose therefore is quite seriously that this kind of typology, i.e. one in which the types are defined by isolated features of named societies arbitrarily selected as paradigm cases, should be entirely abandoned. It is methodologically faulty, it misdirects research, and it has served no useful purpose. (Needham 1971: 16-17)

The substance of Parkin's article is an example of such a type of typology, so little else need be said. Furthermore, it advocates a particular path of evolution among the terminological types. The *lineal* will be ultimately transformed into the *cognatic* type that is identical to the pattern of our own vocabulary of kinship. The shift is supposed to take time. FB is said to 'switch...from the terminological companionship of father to that of MB', but not in one step (p. 254). The hop, skip and jump of his performance are described as intermediary evolutionary stages.

The nineteenth-century evolutionists had also seen our own pattern of kinship vocabulary in the ultimate stage of their scheme. In comparison with Parkin's article, I prefer their work, because they do not isolate kinship as the only domain. In contrast, the 'diachronic approach' of Parkin's article elaborates historical transformations of marriage rules and terminological patterns unrelated to either the technological and economic sector or the political and administrative systems, not to speak of such sociocultural values as status. Had the scheme referred to these features, the diachronic sequence attained in the Table opposite would have been the outcome. The rather unusual sequence of developments on the side of the political economy, ordered according to the historical transformations in the field of kinship, would require some further explanation of the 'diachronic approach' advocated by Parkin.

My own evolutionary speculations of about a decade ago (1982: 97) refer to the tribal pattern as initial. The structure of terminology and alliance may be described as the repetition of intermarriage in every second or fourth generation. Such a highly sophisticated scheme would go together with the simple political economy of swidden cultivators as witnessed today among the Juang and others. In the course of diachronic change, the growing sophistication of the political

TABLE 1: Kinship Terminology and Alliance versus Values and Political Economy¹

	<i>Kinship Terminologies and Alliance</i>	<i>Values and Political Economy</i>
Stage 2	'the Dravidian system we are all familiar with, from Morgan onwards' (p. 258)	caste order plus sophisticated government and economy based on plough cultivation
Stage 3	'the Juang-type system, which retains some prescriptive features' (p. 258)	tribal order in acephalous society and simple economy based on swidden cultivation
Stage 4	'the Malto...poised between the last vestiges of prescription and individualizing north Indian' (p. 258)	same as Stage 3
Stage 5	'the Jat system, terminologically north Indian, with the four- <i>got</i> rule ensuring dispersal' (p. 258-9)	same as Stage 2
Stage 6	'standard north Indian terminologically with no particular tendency to renew alliances' (p. 259)	same as Stage 2

economy would coincide with alternative schemes of simplification of terminology and alliance: *either* affinal prescription would be simplified and then imply *immediate* repetition of intermarriage, as is well known in south India today; *or* the distance between alliances of different generations would lead to an individualizing scheme, as is well known in north India today, implying the end of affinal prescription and alliance by terminological and normative prohibitions of repetition and reversal of marriage ties. *Both* the simplifications could be traced back formally to the sophisticated tribal pattern.

1. I deliberately leave aside Stage 1 or 'tetradic society' (Allen 1986) since this scheme belongs to an analytical level altogether different from empirical cases. Like this *pure* case, the Juang and Njul-Njul schemes alternate kinship categories in adjacent generations. In Allen's parsimonious model, the two-line scheme is retained, while in the Juang and Njul-Njul case alternate generation equations imply four terminological lines.

What do we compare when we compare 'kinship terminologies'? Parkin's article offers a straightforward reflectionist account ignoring all differences among societal types. A number of well-known Oxford and Paris analysts have, however, convinced myself and others of the value of a sceptical approach towards any given domain of 'kinship'. 'Kinship' differs, as societies differ. Some of these differences include the following principles:

1. A term we call 'kinship term' may relate ego to another individual and denote alter's kin type. Designating classes of kin types, the vocabulary may reflect the manner of ordering such individual relationships, as is the case in most European languages. Parkin generalizes the reflectionist approach, but many other authors will differentiate. Although common sense might assume that all 'kinship terminologies' depend upon egocentrically articulated references or addresses, common sense may not be the best of guides.²

2. In other societies, such as the north Indian one, ego may apply a term to a class of alters that is not exclusively designated by genealogical space. My informants in coastal Orissa, for example, referred to a person as *phupha* (FZH) by the logic of oppositions and *not* because of (non-existent) genealogical links. His daughter would be ego's 'sister' and ego's daughter's *phuphi* (FZ) etc. The simple reflectionist dogma does not apply but, at the same time, no other *overall* principle of order can be detected.

3. Prescriptive and classificatory equations (see Allen 1989: 176) may result in a *single unified system* of terms that is *not only* ordered by the egocentric perspective (see Dumont 1983: 5). An example of such a holistic system is 'the Dravidian system we are all familiar with from Morgan onwards', i.e. a system of two affinally interrelated terminological lines. Parkin ignores the order of the whole and the particular implications of holistic orders. He is only concerned with partial relationships and their transformations.

4. A fourth concept of a 'vocabulary of kinship' may offer the same type of system as the third one, but—in addition—relate the order of terms to orders of ceremonies, settlements, dances, groups etc., i.e. a general pattern of classification beyond the domain of 'kinship'.³ The two-line order of the Garo in north-east India (Burling 1963: 348f) may serve as an example,⁴ but anthropologists unfamiliar with South Asia may be better acquainted with the four-line system of the Njul-Njul (see Elkin 1956 [1938]: 70).

2. An as yet unpublished talk by Serge Tchekézoff on terminological 'type' in Samoa (given at the same conference as the paper on which Parkin's article is based) has cautioned me against such generalizations.

3. This is, of course, a reference to Dumont's 'global formula of exchange' (Dumont 1971).

4. Essential is the holistic—and not the local—point of view. The terminological system of the Garo does *not* completely fit the marriage rules. I doubt if *any* empirical system permits such a complete fit.

The Njul-Njul alternate generation equations (see Allen 1989: 177) differ from those of the Juang, but the latter's overall terminological order also implies two alternating terminological lines of affines for the elaborated line of kin. Another term gives a hint of a fourth line that is logically implied by the system. Quite different from the Njul-Njul order, the Juang system contains within each of the three central generational levels expressions of relative seniority for both male and female affines and consanguines, but these additions can be accommodated without contradictions within the overall four-line structure (Pfeffer 1982: 63).

Quite irrespective of these details, on the evidence of his *JASO* article Parkin would evaluate the Njul-Njul order—or any 'Aranda-type system'—in the manner applied to the Juang case. The Njul-Njul scheme would be 'obviously intermediate' and 'the WB of one generation becomes, of course, the MB of the next, but his son does not succeed to his position: MBS is never WB, and MB never WF, hence the regular terminological separation of direct affines and cross kin...the Juang [and Njul-Njul] system is clearly transitional, a symmetric prescriptive system in process of decay, preserving original features in the +1 and -1 cognatic terminology, but losing these in ego's level and with separate affinal terms in all three medial levels' (pp. 256-7).

I disagree with such an interpretation. Njul-Njul and Juang apply a holistic terminological scheme that is truly structural and does not separate a domain of 'kinship' from other domains of social organization. Parkin fails to take notice of the social whole. He equates systems of symmetric prescription with two-line systems. The sophistications of four-line systems are, therefore, viewed as 'decay', or at best as 'transition'.

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REPLY TO PFEFFER

My gratitude to Professor Pfeffer for his comments is eclipsed only by my astonishment at the confusion of thought that underlies practically all of them. I will start with the first of his objections, namely my supposed misinterpretation of Dumont. There is no question that Dumont introduces data from address usage at several points in his treatment of North Indian kinship terminology (1966: 96-103), which he initially sees as descriptive in type. For instance, he concludes his discussion of the term *bhai*, 'brother', which has a wide meaning extending far beyond the close genealogical specification implied by the English translation, by saying: 'If I am right, this is equivalent to leaving the descriptive scheme and using its categories, or some of them, *as if they were classificatory*. This tendency might well be indicative of Dravidian "contagion"; *it is clearly more developed in address than in reference*' (1966: 101; my emphases). And when discussing the treatment of ZH, FZH and MB, which he regards unambiguously as affines in the southern terminology, Dumont says that in the north too, the fact that they are addressed as *pahuna*, 'guest' (ibid.), means that they 'are actually treated as affines, but the terminology [i.e. the reference terminology] does not register the fact' (ibid.: 102).

I had acknowledged Dumont's own misgivings about the feasibility of this comparison on two previous occasions (1990: 69-70; 1992: 12), but chose in the present case to focus, not on his later change of heart, but on the doubts he expressed right at the outset: 'the postulate of the unity of India is seriously

challenged in the field of kinship' (1957: 18). This did not prevent him from going ahead with the attempt none the less—as, of course, he was perfectly entitled to do. Indeed, my own position was and remains one of applauding the aim but of finding fault with the approach. Thus I do not think I was being unduly unfair to Dumont in choosing this as the best and most famous example of the sort of difficulty I was talking about. At all events, I have not ignored the matter in my work generally, but merely found a different, and earlier, statement of Dumont's to cite.

As regards his second point, Pfeffer is evidently not quite so familiar with McDougal's writings as he thinks. McDougal's major work (1963) has plenty of references to the fact that marriage ties between spouse-exchange groups are discontinuous down the generations, in the sense that there are norms against the immediate repetition of alliances between the same alliance groups (*ibid.*: 158ff., 168, 174-5, 428, 429). This means that one cannot repeat the marriage of one's father, as with cross-cousin marriage; and other, statistical data recorded by McDougal (*ibid.*: 160) suggest that the marriage of one's FF is also avoided. This actually entails a three-generation rule of delay, since the two-generation rule it logically implies would come up against the strict opposition of alternate generations that is so crucial a feature of Juang life and thought: FFF is linked with one's father and therefore belongs to a genealogical level defined as adjacent to ego's, into which marriage is formally prohibited and whose own marriages ego does not repeat.

Such rules are by no means uncommon in middle India. They may sometimes be negative rather than positive, i.e. they impose a delay before realliances become possible, rather than actually enjoining repetition after the stipulated number of generations has passed. This does not stop them from being considered, or described as, rules. The Munda may even represent an instance of a positive rule, if a fairly explicit statement by Yamada (1970: 385) is anything to go by. Elsewhere in the area, the possible existence of such rules is supported by evidence of spouse-selection being directed by kin term (Munda, Korwa: *goi*; Santal: *sangat*; Juang: *salirae*) and, more circumstantially, of a rule against marrying anyone with whom *no* previous relationship can be traced (the Hill Kharia). The data assembled in my book (Parkin 1992: especially ch. 8 and appendix II) substantiates these matters fully, giving all the necessary references.

In fact, it would seem that Pfeffer is not terribly familiar with his own writings either. Two passages of his (1982: 97, para. 3; 1983a: 101, para. 3) can certainly be construed as accepting the existence of a three-generation rule among the Juang and other groups.¹ Other passages (especially 1982: 56-60) are still more specific in mentioning delays of three generations before alliances can be renewed in a number of different groups. Many of his statements refer explicitly

1. Though in neither instance is the wording entirely clear, as is often the case in Pfeffer's work. It is this, not the language in which it is written, that is the real reason for it sometimes being 'too difficult to be checked' (see above, p. 50).

to this practice as a 'rule', and two (ibid.: 57, on the Dangria Kond; ibid.: 60, on the Bhuiya) are based on his own field enquiries.

Pfeffer's third comment, on the reification of linguistic names as ethnic groups, has no basis in my own writings. Indeed, in the preface to my major work (1992: ix), I expressly deny any intention to argue for any sort of linguistic determinism. I accept at many points (ibid.: ix, 4, 217, 225-6, 234-9) that Munda speakers, like Austroasiatic speakers generally, share much with immediate neighbours who speak different languages and that they cannot really be distinguished from them on other grounds. The chosen topic of that work was the comparative ethnography of a group of middle Indian peoples who all speak a Munda language, and as such the word 'Munda' was bound to appear frequently in it. This simply means that the term 'Koraput Munda', for instance, refers to 'the peoples who speak languages of the Koraput Munda branch' or (which comes to the same thing) 'the Munda-speaking groups who live in Koraput district': it certainly does not represent any sort of reification.

As for the term 'Malto', my employment of it in the article in question is admittedly looser than it might have been. None the less, as a general principle its choice can be readily justified in the present context, where it is mainly kin terms and terminologies that are being discussed. Given the embodiment of terminologies in language, 'Malto', being the term for the language of the Maler, seems more natural here than the ethnonym itself, just as one would normally speak of the Latin, not the Roman, kinship terminology (not to mention the Sanskrit, Hindi etc. terminologies). As for the complaint concerning absence of sources, these are given in full in Appendix II of my book (1992: 234-6; and in the notes, ibid.: 276), a manuscript copy of which Pfeffer has long possessed. It is hard to see why Pfeffer should decide that my table is no more than a 'caricature' of a kinship terminology: it shows just what it purports to show, no more and no less, namely the transitional nature of the pattern in the parental (i.e. +1) level of two terminologies, the part that is most significant to the argument currently being pursued, being in each case that part where the last traces of prescription are to be found.

It is when Pfeffer goes on to complain about my treatment of kinship terminology in general, and to offer his own disquisition on the topic, that we begin to meet with real caricature. At one point (p. 52), he calls my study a 'reflectionist account ignoring all differences among societal types', his train of thought apparently being that because, in his view, there is no separable domain of kinship, one has to anchor one's study of it firmly to other sociological factors. This is an opposite use of the word 'reflectionist' to the one with which I am familiar. In my experience, it is usually applied precisely to those explanations for kinship terminologies that see them as epiphenomena of other social facts, such as (depending on the writer) descent or marriage systems, behavioural stereotypes, or status. McKinley, for instance, argues:

[the reflectionist tendency] assumes that the *only* way to demonstrate a relation between kinship terminology and features of social organisation is by locating the

points at which the terminology appears to be a direct reflection of some aspect of the social or behavioural system. In other words, a certain amount of one-to-one mapping or congruence is expected between terminology and social structure. Usually the terminology is treated as the dependent side of this relationship. (1971: 239-30; original emphasis)²

This careful definition might be called the restricted view. Under certain circumstances, the term 'reflectionist' might equally be applied to correlations with societal type of the sort that Pfeffer appears to be advocating. The debate is an old one, going right back to Morgan and one of his earliest critics, Kroeber. The latter, in a famous paper of 1909, was the first to advocate the treatment of kinship vocabularies in their own right, as autonomous systems of classification. This, in general, has always been my own tendency too. Indeed, I cannot conceive how any sort of reflectionism can be attributed to myself, either in this article, or in anything else I have ever written. My own position is and always has been that it is perfectly legitimate to restrict one's investigations to one particular aspect of what we call kinship; that if that aspect happens to be terminology (as is largely the case in my article), then to begin with it can be treated simply as a matter of classification; and that it is no more than prudent to concentrate initially on establishing the nature of the system before looking to see what correlations of a sociological nature *might* emerge from the data. I am certainly not hostile to attempts to establish correlations as such: I simply believe that the onus of proof lies on the author of them, and that they are much less easy to bring to fruition than is often realized. The major obvious exception is the possibility of correlating prescriptive terminologies with those systems of affinal alliance loosely but not entirely satisfactorily referred to as 'cross-cousin marriage'. While I accept the strictures of those who claim that such coherence, though logical, is strictly ethnographically contingent, there are certainly cases where the fit does obtain—including, in my opinion, the Juang and allied groups (as I define their systems). Yet not even this degree of recognition entails giving an existential priority to either kinship terminology or affinal alliance system, as a reflectionist approach would require.

In fact, Pfeffer's own attempts to set up a range of associations between kinship terminology and societal type stops just short of being reflectionist, since he does not go so far as to accord existential priorities to either. None the less, they bring him much nearer to such a position than anything in my work brings me. The correlations he suggests are considerably more ambitious than my own much more modest attempts to match—and only where ethnographically appropriate—affinal alliance system to terminology. I infer from the last paragraph on p. 50 that the intention of all this is to challenge the validity of the diachronic stages I had advocated for types of terminology by showing that the order does not correspond with his own evolutionary scheme of societal types. Whatever the

2. See also Barnes 1976: 385. Pfeffer seems to have understood the standard definition better on an earlier occasion (see his 1985: 701).

validity of this scheme as such, what it shows is simply the difficulties involved in establishing regular correlations between the two, difficulties that are severe enough to make most authorities content to assume that there are none. My diachronic suggestions, on the other hand, are based on transformations internal to the terminological data themselves that are both logical and, I believe, ethnographically justified. This is yet another reason for wanting to concentrate on terminology as a separate domain, a decision supported by the circumstance that a terminology is embedded in linguistic much more than in societal factors. Indeed, there is no evidence that I know of showing that kinship terminology differs comprehensively according to societal type. A simple example will suffice here. Pfeffer associates the 'Dravidian', i.e. South Indian terminological system, with 'caste order plus sophisticated government and economy based on plough cultivation' (p. 51). The doctoral thesis of one of his own students (Werth 1992) has shown that the wandering Vagri of south India have a terminology that is basically standard South Indian in type (though lexically Indo-European, not Dravidian). Yet they live by hunting, scavenging, begging, minor trade and such ritual services as healing, not at all by plough cultivation, and they can hardly be said to have 'sophisticated government'—whatever that is supposed to mean.

Pfeffer also appears to want to say (p. 52) that some terminologies are not only, or even not at all, egocentric, a view apparently based on the mistake of thinking that terminologies are egocentric because, or when, they chart genealogies (assuming this is what is meant by 'individual relationships'; cf. also the words 'genealogical space' in the following paragraph). Here, Pfeffer has got himself well and truly tangled up in an elementary confusion that has bedevilled anthropology for far too long. All terminologies are egocentric, simply because they are systems of classification centring around, i.e. seen from the perspective of, an imaginary person called ego. This has nothing to do with genealogy as such. Kin terms may delineate actual genealogical positions, and even particular analytical kin types, but they need not and frequently do not. What they always do, however, is to map out the indigenous categories ego uses for the people he recognizes as relatives.

These principles apply just as much to the English term *uncle* as to the Tamil term *maman*: an English ego may have several uncles, each representing a separate genealogical position and a separate kin type. It is only when this English ego wants to define the relationship of one particular uncle more exactly for some purpose that he or she resorts to genealogy. In my view, those alliance theorists who risk the wrath of the extensionist school in distinguishing non-Western terminologies from Western ones because the former use category rather than genealogy actually understate their case. The distinction is a false one, though the real reason for this is the opposite to the one usually given. All kinship terminologies deal in categories, and all peoples have ways of describing relationships more exactly than reference to category is always able to do. One example of the latter, though it is not the only one, is genealogy. The distinction

is thus not one between types of society but between sorts of knowledge, i.e. between the contexts in and for which relationships are identified.

Pfeffer none the less appears to think (p. 52), on the authority of Dumont, that a separate class of terminology is defined by the possession of a holistic perspective. Again, I would say that *all* terminologies are holistic, in the sense that they form a discrete semantic domain. What Dumont is actually doing in the place cited (1983: 5) is warning us against the mistake of taking the genealogically minimal definition of a term as its chief meaning and the classificatory referents of the term as mere extensions. The example he gives concerns the relationship between two classes of +1 males who are minimally defined as father and mother's brother in the typical Dravidian terminology. Its purpose is certainly to show that this is a relationship between two classes of affines (i.e. affines to one another) that in itself is structured not by their own individual relations to ego, but by their involvement in repeated affinal alliances with each other. However, none of this affects the circumstance that a kinship terminology is egocentric. Ego is also implicated in this arrangement, it clearly being Dumont's intention to argue that ego's relationship to the mother's brother is just as affinal as that between the father and mother's brother. Indeed, the entire analysis would lose much of its force were this not the case, given that ego's father and mother's brother will be affines to each other in any society, ours as much as among Tamils (this is true by definition, i.e. mother's brother will always be ego's father's wife's brother). Thus, with regard to the question of whether or not terminologies are egocentric, it matters not at all whether ego's relationship with the mother's brother is traced indigenously through the mother, exploiting links that are entirely consanguineal, or through the father, as mother's brother's affine. In either case, father and mother's brother are defined in themselves—like their classificatory equivalents, where appropriate, and even though all are subsumed under a common term—precisely by their relation to ego. They are respectively *ego's* father and mother's brother, or, in Tamil, *ego's appa* and *maman*, *not* each other's. Dumont himself says, in deliberate and direct contradiction of his own question as to whether his account entitles one 'to speak of a structure *sensu stricto*': 'but here lies the characteristic of a kinship terminology as compared with other kinship groupings, *that it is a constellation revolving around the Ego*' (1983: 12; my emphasis).³

This in no way contradicts the idea of a kinship terminology as a whole, i.e. a discrete semantic domain. But the notion should not be confused with other classifications that are sometimes found alongside the kinship terminology. Although there are frequently terms for whole classes of relations, such as affines, cognates, and patri- or matrikin, these are not part of the kinship terminology as such, which, in Dumont's famous phrase, 'has not as its function to register *groups*; it is on the contrary the basic fact of its nature that it universally ignores them' (1962: 92; original emphasis). Rather, they form a parallel classification

3. Here too, Pfeffer had more success earlier in interpreting Dumont correctly (see his 1985: 702).

supplementing, but not normally contradicting, the terminology. Labels for phratries, moieties and sections are an obvious example. However, their presence is strictly contingent: it was the respective presence and absence of sections that led Dumont to distinguish the Karia and Dravidian systems from each other as global and local forms of exchange (1983: ch. 5). In practice, it is mostly these, rather than kin terms, that one works with when attempting to establish correlations with 'orders of ceremonies, settlements, dances, groups etc.' (p. 52).

The Garo example given by Pfeffer proves, when examined, to have precious little to say about such things, Burling being mainly concerned on the pages cited with kinship terms and how they are used. Perhaps, as Pfeffer suggests, data on the Njul-Njul of Australia would prove more fruitful, though as I have never even mentioned them in print he is hardly entitled to say (p. 53) how I would treat their system. What is more remarkable about this part of Pfeffer's critique is his attribution of a four-line 'Aranda-type system' (p. 53)⁴ to the Juang of central India, the main example of my article. I have already challenged this claim briefly in print (1992: 187). The point deserves developing at length, but suffice it to say here that the Juang terminology as recorded by McDougal (1964: 329) bears no relation to the hypothesis. One can find four terms in each of the three medial levels readily enough, but the specifications given for them do not correspond to a true four-line scheme: in particular, they are not joined by lineal descent to corresponding positions in the other levels. A number of terms are for affines only, another deleterious circumstance. Nor is any category of second cross cousin stipulated as alliance partner, as in most such systems. As already mentioned in my article, marriage partners among the Juang and similar groups are rather to be seen as GEG categories to each other. The vacuousness of Pfeffer's hypothesis is shown by the passage in which he says that the Juang's 'overall terminological order also *implies* two alternating terminological lines of affines for the elaborated line of kin. Another term gives a *hint* of a fourth line that is *logically implied* by the system' (p. 53; my emphases). Such language does not improve one's confidence in the sort of reasoning being employed, since all it demonstrates is a fondness for deep but hypothetical mental structures as a way of compensating for what is really a complete lack of evidence.

Here, then, is a fundamental disagreement over how an important and (thanks to McDougal's ethnography) well-described affinal alliance system is to be interpreted. Because he sees it as four-line prescriptive, and is therefore able to link it with a textbook scheme of kin classification, Pfeffer treats the Juang system as static. I, conversely, see it as closely related but by no means identical to the two-line symmetric prescriptive scheme of south India, and therefore as transitional between this and non-prescriptive North Indian. Pfeffer accepts the possibility of change between terminological types, but as regards India, he places the supposedly four-line system of the Juang not in any transitional position, but at the

4. I draw attention to this phrase because it offends against Needham's strictures as quoted so approvingly by Pfeffer three pages earlier.

evolutionary starting-point. Quite apart from the spuriousness of the four-line hypothesis, this would reverse the order conventionally assumed to be the most plausible, namely that it is two-line symmetric prescriptive systems, especially if they have extensive alternate generation equations, that have evolutionary priority over other forms (see Allen 1986 for the most developed account). Given all this, it is Pfeffer's notion of the South Indian pattern as a 'simplification' of the supposedly four-line systems of middle India (p. 51) that needs justifying.

Such disagreements, and the confusion and lack of clarity I frequently detect in Pfeffer's writings, account for my previous reluctance to put them to use in my own work, save to some extent ethnographically. This is important, because it provides an answer to Pfeffer's remaining point, to the effect that I have disregarded some prior claim he has on this material (pp. 49, 50).⁵ I am happy to concede that Pfeffer has been actively studying middle Indian kinship for almost as long as I have. My own involvement dates back to the autumn of 1977, when I started intensive library research on the Munda. Pfeffer's earliest writings on tribal kinship would seem to derive mostly from two brief field trips to Orissa in 1980-81 and 1982, heavily supplemented by literature research that presumably preceded them in part.⁶ It is also true that Pfeffer beat me into print on this topic by approximately a year: as far as I have been able to determine, his own first relevant publications are dated 1982, while mine are dated 1983.

There is, of course, a degree of convergence between our writings, but this is limited to generalities, including the existence of dispersed alliance in India. As regards the occurrence of this phenomenon among Munda speakers, my own views on the matter have certainly developed over the years, but I can point to some brief remarks in an article published in 1985 which, though they have come to seem unsatisfactory from many points of view, do show clear parallels with Pfeffer's paper (Pfeffer 1985) published the same year.⁷ In my paper (1985: 716-17), I suggest that very few Munda-speaking groups have affinal alliance systems based on 'the direct exchange of women between alliance groups, repeated generation after generation', and that 'the north Indian preference for avoiding close kin in marriage seems to prevail'. And my very first published paper, on

5. One would not normally bicker over such matters. When this complaint originally arose, however, it was made so seriously that it led to my being expelled from a wing of an institute whose hospitality I had been enjoying and in which, indeed, I had even been allowed to teach on occasion. I hope the reader will therefore forgive me for taking this opportunity to clear my name of the charges of ethical misconduct then laid against me.

6. See Pfeffer 1983b: 649n. An earlier field trip to Orissa in 1971-2, undertaken as part of the Orissa Research Project into the Jagannath cult, led to Pfeffer's habilitation thesis (1976) and two other publications (see his 1978: 422 n.5 and *passim*; and 1982: 1 and Part I) on the Brahmins of Puri. It would appear that other members of the project had responsibility for the tribal dimension on that occasion, especially Hermann Kulke and Anncharlott Eschmann.

7. A revised account of my own paper can be found in my book (1992: ch. 7).

Lévi-Strauss's treatment in *Elementary Structures* of Austroasiatic-speaking groups, shows me grappling with the intricacies of these systems, not as yet entirely clear as to their nature, but at least recognizing the absence of cross-cousin marriage in many of them, with in its place rules of delay before alliances may be repeated (1983: 80-83). Both circumstances automatically entail the dispersal of alliances for any one affinal alliance group. It does begin to look as if Pfeffer and I have been working in parallel to some extent, at least as regards such general points.

Yet neither of us, I think, can really claim priority even here, nor as regards fleeting comparisons with so-called 'Crow-Omaha' systems. It was McDougal who originally worked out the Juang system, in his excellent thesis, thirty years old this year (1963). Parry introduced Crow-Omaha kinship into the question of affinal alliance in India back in 1979, clearly alluding to the system in Kangra as one with 'Crow-Omaha-type rules of exogamy' (1979: 312; the local rules are described at 221ff.) and linking such systems with 'the development of a pattern of repeated alliances between descent groups' (*ibid.*: 313); in other words, the people of Kangra can be said to have dispersed alliance mitigated by the clear possibility of renewal in the long term. And it is Tiemann, I would say, who deserves most credit for having worked out, now well over twenty years ago (1970), not only the nature of the four-*got* rule, but its implications for the dispersal of alliances in north India generally (i.e. not just among the Jat). Nor should Trautmann's extensive and able treatment of Indian affinal alliance and terminology be forgotten (1981).

I wonder if Professor Pfeffer has received as few complaints from these authorities as I have. If, by whatever chance, he has received any at all, it is certainly not my intention to add to them here. But at least these few remarks may be enough to indicate that, in putting forward a claim to precedence concerning these matters, he is simply making a mountain out of a molehill that was never entirely his to begin with.

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ANTHROPOLOGY AND MISSIONARIES

Editor's note: The following comment on the recent Special Issue of *JASO* on 'Anthropology and Missionaries' (Vol. XXIII, no. 2) was solicited by the editors. Dr Bowie's D.Phil. thesis (Bowie 1985) is one of the few in Oxford in recent years to deal centrally with missions. She is also one of the editors of a forthcoming collection of essays on women and missions (Bowie, Kirkwood and Ardener (eds.) 1993).

Studies dealing with the interaction between anthropologists and missions, as well as anthropological studies of mission, are still so scarce that any addition to the field is to be warmly welcomed. The guest editor of the recent *JASO* Special Issue, W. S. F. Pickering, is therefore to be thanked for putting together these articles on 'Anthropology and Missionaries', with his introduction. It is clear from the papers selected for publication (and it is possible that the addition of those that had unfortunately to be excluded would have strengthened this impression) that anthropological perspectives and cultures vary as much as do those of missionaries and their host societies. While, for example, Joan Burke is able to present an insider's view of the Roman Catholic Church and the Sisters of Notre Dame in Zaïre, R. H. Barnes looks at inculturation in Indonesia from the outside.

The recent discussion in mission circles of 'inculturation' or the cultural accommodation of Christianity to a non-Christian, usually 'tribal' setting has obviously spurred anthropologists to look at missions anew, and to engage with some of the theoretical and practical questions asked by missionaries themselves. One might be forgiven, however, given the papers presented here, for thinking that the debate is confined to the Roman Catholic Church. The terminology varies, but similar questions are being addressed by Protestant missiologists, as Pickering indicates in his introduction (see also, for example, Scherer and Bevans 1992; Winter and Hawthorne 1992).

The question of the extent to which the missionary can, or wants to, use anthropological information about a host society, and conversely, the value to anthropologists of ethnographic data collected by the missionary are central concerns. Stefan Dietrich's article on Pater Schmidt is a useful exercise in clarifying some of the differences between an anthropological and a missiological perspective, particularly as the writings of such anthropological missionaries as Aylward Shorter tend to merge the two. Dietrich's reminder that the description, study and analysis of religions began in theology, and that the social scientist is therefore using a terminology that begs a different set of assumptions (such as the distinction between 'religion' and 'magic') is worth reiterating. I also would concur with Dietrich's conclusion that 'anthropology cannot answer theological questions, but perhaps it can be used by theologians to help in framing well the questions—and, in consequence, the answers'. Dietrich's article underlined for me the continuity between pre- and post-Vatican II Roman Catholic emphasis on the 'essentials' and 'inessentials' of faith. Missionaries continue to manipulate or 'exploit' cultural knowledge in the service of Christianity, with indigenous

Christians as frequent participants in this process. Indeed, this raises various ethical questions for the anthropologist whose cultural expertise might be tapped, questions that are dealt with in the present collection only by Joan Burke, who was invited by her subjects to undertake her study in order to enable them to become increasingly conscious agents of their own history.

The value of localized studies is demonstrated in the essays by Macdonald and Howe. Archival material, official mission documents, the memories of those involved, as well as more conventional fieldwork, are all needed in order to evaluate the relationship between missionaries and locals in specific geographical and historical situations. These tantalizingly brief glimpses might, it is hoped, inspire other anthropologists to re-evaluate their own fieldwork data, or to undertake new studies of the interaction between missions and local cultures. While, therefore, the *JASO* collection is to be warmly welcomed it also reminds us of how much more could be done.

Kanogo (1993) and Basu (1993) have provided two of the very few contributions to the literature from the perspective of recipients of missionary activity (although they write primarily as historians rather than as anthropologists). Moreover, societies supposedly Christianized for many centuries are under-represented in current literature, despite the fact that they often pose a continuing challenge to the missionary policy of the church (see, for instance, Skar 1993). For the Roman Catholic Church in Wales the Principality is still mission territory, with many parish priests, most of whom are Irish, being members of the missionary order of the Oblates of Mary Immaculate (OMI). These priests are more resistant to the use of the vernacular (i.e. Welsh in Welsh-speaking areas) than are OMI missionaries in Cameroon, or in many other more 'exotic' mission contexts. And Gaelic-speaking priests do not seem to be markedly more sympathetic to Welsh-speakers and their aspiration to worship in their native tongue than priests from English-speaking backgrounds. There is also a vast amount of potentially very valuable comparative work that could be done on Hindu, Muslim, Buddhist and Bahai missionary activity. Given that the anthropological study of missions is in its infancy, however, the *JASO* collection constitutes an important contribution. I sincerely hope, though, that the papers that could not be included in the *JASO* Special Issue will be published elsewhere and become similarly accessible.

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MALINOWSKI THE PHOTOGRAPHER

In discussing some of Malinowski's Trobriand field photographs, Terence Wright (*JASO*, Vol. XXI, no. 1, pp. 41-58) calls attention to his 'concern with imagery' (p. 50) and his 'familiarity with the characteristics of visual expression', as well as his practice of making drawings (p. 51). In her subsequent comment, Elizabeth Edwards (*JASO*, Vol. XXIII, no. 1, pp. 89-91) draws attention to the lack of integration between Malinowski's photography and his fieldwork, something on which, she points out, Malinowski himself remarked (p. 91). With reference to these two aspects of Malinowski's field photography I wish to remind *JASO* readers of his friendship with Stanisław Ignacy Witkiewicz, to which long-standing readers will remember having their attention drawn by Krystyna Cech in 1981. Known to Malinowski as Staś and more generally, after his own styling, as Witkacy, Witkiewicz was a renowned dramatist, novelist, painter and, we must add, photographer, whom Malinowski had known since childhood.

In her essay, Cech (1981) argued that knowing about Malinowski's early life, his friendships, his social circle and the intellectual and cultural life in which he was involved, is essential if one wants to understand 'Malinowski the man' (especially as revealed in his diaries). I suggest that knowing in particular about Malinowski's friendship with Witkiewicz may well be essential if one wants to understand 'Malinowski the photographer'. Having no Polish I cannot explore to their full extent the sources of further information about this relationship that are available (see, for example, the references in Cech 1981; Gerould 1981; Jakimowicz 1987; and Ellen *et al.* (eds.) 1988). What I can do is report the bits and pieces of information that I have been able to gather and that strike me as significant.

First, photography was more than a casual interest of Witkiewicz's. He had been taking photographs—of trains, landscapes and people—since he was fourteen years old, since before he and Malinowski became friends. The Witkiewicz

scholar Daniel Gerould claims (1981: 3 n.2) that Witkiewicz was 'an excellent photographer' and reports him taking 'many photographic portraits of himself, his father, and his friends and associates' between 1905 and 1914.¹ It is clear that photography was an essential aspect of Witkiewicz's artistic practice. I can do no better here than quote, from the English translation of Irena Jakimowicz's account of Witkiewicz's life and art, a passage that will provide food for thought for anyone interested in understanding Malinowski's field photographs:

When speaking about artistic experiences of young Stanisław Ignacy Witkiewicz one cannot fail to mention photography; its impact was both significant and many-sided. In Witkiewicz's hands the camera played a two-fold function: an instrument of knowledge used for documentary purposes; and also of a medium opening up new creative possibilities. The photography allowed him to compile systematically arranged collections: whether of landscapes, locomotives, or of models who sat for his drawings and paintings, often photographed in the same costumes. Even in the early period the young artist started to take pictures of scenes deliberately arranged by himself. At the same time, however, the camera lens was a tool which revealed unexpected ways of handling objects: it not only 'perceived' landscapes according to the prevailing art nouveau style, but also anticipated matters yet unknown. Sometimes, when regarding a given face, it mercilessly exposed and enlarged the texture of the skin with all its flaws; at other times, it blurred the contours of the face, covering it with an idealizing mist. Some photographs acquire unusual qualities thanks to unconventional framing, in others, the face is repeatedly photographed in different modes, as if the artist wanted to make it more and more unreal. (Jakimowicz 1987: 10-11)

Was Malinowski interested in photography before he embarked on his anthropological career? Given Witkiewicz's intense interest, it is difficult to imagine that the two of them did not discuss photography at length, as they discussed everything else, perhaps even taking photographs together as Malinowski was later to do in the Trobriands with Billy Hancock. I can only speculate, but a trawl of the available primary sources—letters, diaries (including the unpublished parts of Malinowski's own) etc.—might tell us something.

The potential importance of this relationship between anthropologist and artist for understanding Malinowski's later field photographs is strengthened by a fact that has remained largely unremarked by historians of anthropology. As Cech relates (1981: 182), Witkiewicz accompanied Malinowski on his visit to Australia in 1914 to attend, as Marett's secretary, the meetings of the British Association for the Advancement of Science. Witkiewicz's trip was arranged to get him away from Europe, where his fiancée had recently committed suicide. According to

1. Krystyna Cech, who kindly read a draft of this comment, tells me that Malinowski was often the subject of Witkiewicz's photographic experiments. Examples of such photographs are to be found, she tells me, in a book of Witkiewicz's photographs published in 1986 (see Witkiewicz 1986); see also plates 9, 10 and 11 in Ellen *et al.* (eds.) 1988.

other authorities, however, the plan was not just for Witkiewicz to accompany Malinowski to and around Australia, but for him also to go with Malinowski to New Guinea as draughtsman and photographer to the 'expedition' (see Degler 1974: 36; Gerould 1981: 13; Wayne 1988: xvii; Kubica 1988: 99; Jerschina 1988: 141). While it is not clear from the brief accounts these authorities give how serious this plan was, it does seem that for a while at least Malinowski was expecting to have someone else make drawings and take photographs in the field.

As it happens, Witkiewicz did not accompany Malinowski on his first field trip to Mailu. On the outbreak of war, Witkiewicz returned to Europe to fight with the Tsar's forces. By doing so he lost himself a greater part in the history of anthropology (our loss, not his, no doubt), but perhaps ensured Malinowski a greater place than he might have achieved if he had not carried out his first fieldwork 'alone'. It is remarkable, though, how the very fact that Witkiewicz was with Malinowski in Australia and was planning to go with him to New Guinea has been omitted from accounts of Malinowski's early researches. Even the published edition of Malinowski's diary explains who Staś is, but without mentioning his presence in Australia (see Malinowski 1967: n. 3), while Michael Young's (1988) authoritative account of Malinowski's first field researches does not mention Witkiewicz at all.

To return to the photographs. Given even the little extra information we now have, would it be too much to see Malinowski's 'visual sophistication', on which Wright comments, as a product, in part at least, of his friendship with Witkiewicz? And might the lack of integration between Malinowski's photography and his fieldwork, on which Edwards comments, be at least partly explicable as a result of his having to take on a task that he thought his friend Witkiewicz was going to undertake? Furthermore, if Malinowski did, as Wright elsewhere (1992: 21) suggests, 'evolve an exploratory use of the camera in the field' might not this owe something to the photographic experiments of his friend Witkiewicz?

Any influence that Witkiewicz had on Malinowski's field photography would presumably have been at its greatest at the time of Malinowski's first field trip to Mailu in 1914-15, while the remarks of both Wright and Edwards are directed towards Malinowski's later Trobriand photographs of 1918 or thereabouts. But there is no reason to suppose that Witkiewicz's influence would have waned completely by the time of Malinowski's Trobriand researches. Moreover, any attempt to understand Malinowski's Trobriand photographs would surely benefit from comparing them with his earlier, Mailu, photographs, while any attempt to understand the latter would have to take into account Malinowski's experience of photography before he first went to the field. Any complete account of Malinowski's field photography, therefore, will have to take into account his friendship with Stanisław Ignacy Witkiewicz.

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EXHIBITION AND CATALOGUE REVIEW

USEFUL ARTS: ARTFUL UTENSILS

STEPHEN HUGH-JONES

Basketmakers: Meaning and Form in Native American Baskets, Oxford: Pitt Rivers Museum, 16 June 1992–22 May 1993.

LINDA MOWAT, HOWARD MORPHY and PENNY DRANSART (eds.), *Basketmakers: Meaning and Form in Native American Baskets* (Pitt Rivers Museum, University of Oxford, Monograph 5), Oxford: Pitt Rivers Museum 1992. 175 pp., Bibliography, Index, Maps, Figures, Plates. £16.25 including postage and packing (from the Pitt Rivers Museum, South Parks Road, Oxford OX1 3PP).

THE new temporary exhibition at the Pitt Rivers Museum and the accompanying catalogue together make a sustained and largely successful effort to rescue baskets from the twilight of evolutionary and taxonomic ethnology, to recognize their aesthetic significance, and to give some recognition to the lives and values of the people who made, and still continue to make, them.

In the artistic stakes, pots definitely have the edge over baskets—find me a basket in a decent art gallery, name me a Bernard Leach or a Josiah Wedgwood in the world of baskets! Like sluggish tortoises, overtaken by ceramics, textiles, paintings and sculptures in the race towards high culture, baskets seem to have balked at the very first hurdle, the division between arts and crafts. Dry carapaces whose regular forms and geometric patterns bear silent witness to past life, they were picked up and placed in ethnological museums ruled by the strict taxonomic regimes of type, technique and provenance, or played second fiddle to tribal masks in galleries of 'primitive art'. As head of the anthropology department at Cambridge, Jack Goody used to talk of 'basket weavers' as a shorthand for museum curators lacking in anthropological expertise—for him baskets seemed to epitomize everything that he felt was wrong about museums and material culture.

Heirs of a pre-ceramic technology, light, strong, functional and flexible but also ephemeral and easily replaced, baskets have not fared well on the road to modernity either. The carefully woven fabric of each basket incorporates many hidden hours spent on learning skills and on fetching and preparing materials—the catalogue makes clear the impact of environmental degradation on the availability of materials for basketry and the sometimes extraordinary lengths that contemporary Native Americans go to obtain them. Made by poorly remunerated peasants or Indians whose time is not money, baskets were rapidly displaced by plastic and metal alternatives, both cheaper to make and tokens of a new identity. The fate of many a basket is summed up by a Yavapai Indian from Arizona in a quotation displayed in one of the exhibition cases: 'Her father sold it 'cos Rena going to school, and after that maybe she marry a whiteman, don't live like Indian, don't want basket.' The 'best' of these unwanted baskets found their way to museums and galleries, the rest were forgotten.

Squeezed into a relatively small area, the main exhibition is divided by region, but other cases cover prehistoric basketmakers, the different techniques and uses of basketry, the way increasing commercialization has transformed basketry form and function, and the lives and products of contemporary Native American basketmakers. Uncluttered by excessive commentary, the cases allow the pieces to stand for themselves as objects of beauty, displays of technique, and as ingenious solutions to practical problems in a workaday world. Some excellent and informative photographs taken by historical and contemporary collectors provide information about materials, settings and contexts and serve to restore the objects to the hands and faces of their makers.

The catalogue is lavishly illustrated, its ten colour plates highlighting the role of colour in pattern and design and illustrating both the subtle tones of natural materials and vegetable dyes and the possibilities opened up by the use of chemical substitutes. The catalogue's divisions follow those of the exhibition but its text provides several further dimensions that add greatly to the appreciation of the objects displayed. After an introductory essay on the museum's basketry collections and collectors and a clear summary of different basketry techniques, regional specialists 'from Alaska to Tierra del Fuego' provide succinct accounts of local variations in construction technique, form, function and basketry design in archaeological, historical and traditional contexts. In their different ways, however, the essays go well beyond the concerns of technology and tradition to provide accounts of the cultures and histories of both basketmakers and basket collectors and to examine the changing status of native basketry as part of the colonial encounter. What comes across especially interestingly, both in some of these accounts (notably that by Ann McMullen) and also at a material level in the exhibits themselves, is the way in which Native American basketry has been transformed through time.

Though basketry has always been an item of exchange and trade, in all the cases considered here, baskets were once largely produced for internal consumption within 'tribal' groups made up of people who lead lives that depended, to a greater

or lesser extent, on their use and who held a distinctive set of ideas and values in common as part of their own identity. In Amazonia this still largely applies, but elsewhere basketmakers have responded through time to a number of selective pressures that have altered radically both what they produce and the way their craft and its products are evaluated. These pressures involve not only the introduction of new techniques, materials and uses, and the displacement of baskets by new, alternative technologies, but also the increasing targeting of baskets to an external market made up of both users and collectors whose demands for 'authentic Indian basketry' have transformed quite radically the objects themselves. The wonderfully kitsch Passamaquoddy ash-splint strawberry basket made by Clara Keezer is but an extreme example of selective pressures leading towards miniaturization and decorative elaboration. The juxtaposition of the contemporary and the traditional and the exploration of the links between them are welcome and important features of an exhibition that places objects firmly in the context of the lives of the peoples who make them.

For many Native American groups, basketry is one of their most important and significant vehicles of aesthetic expression, a point that is clear from the displays and that is explored in Howard Morphy's stimulating concluding essay (a revised version of which appeared in *JASO* (Vol. XXIII, no. 1, pp. 1-16)). Rather than being set aside as 'art', baskets are firmly anchored in practice and practicalities—baskets are as beautiful as they are well made, serve useful ends and are integrated into culturally meaningful activities. The cases display an astonishing array of different basketry forms, each relating to a specific function—not just picking, carrying, harvesting, winnowing, sieving, straining, storing, but also carrying water and serving food and drink. There are also mats and cages, fire-fans, traps and toys, and even baskets for dancing and getting married with.

As hats, bindings and belts and as the support for feather head-dresses, basketry also figures as part of bodily aesthetics. This connection between bodies and baskets is not entirely fortuitous. Many Native Americans began their lives in the basketry cradles that Colombian peasants refer to as '*un moisés*' ('a Moses'): a free-standing Yurok cradle complete with movable sunshade is a *chef d'oeuvre* of the exhibition—and baskets have served as coffins too. For the Trio and other Indians of lowland South America, human bodies are animated baskets and thus share the same repertoire of painted designs. As one exhibition case reveals, an informed reading can translate the intricate designs on Hopi ceremonial basketry plaques into the powers and attributes of Kachina spirits that are represented in the costumes of dancers. The Californian Pomo decorated their baskets with beads and coloured feathers in the manner of human bodies; given away as gifts and burned at funerals, these valuables inspired many copies that were sold in the expanding market for Indian arts.

Drawing on David Guss's exploration (Guss 1992) of the cultural significance of basketry for the Venezuelan Yekuana, Morphy argues that an appreciation of alien aesthetic traditions may allow us to see objects through eyes that can appreciate the meanings and values that lie behind form and function. The

problems here are those of scale. Guss deals with a culture at one moment of time while the exhibition deals with a continent and a long pre- and post-colonial history; Guss's weaving includes houses, the cases contain more collectable basketry. The exhibition faces up to these difficulties honestly: as the catalogue makes clear, museum collections are usually products of the whims, fancies, prejudices and logistical constraints of assorted collectors, and not usually representative samples of the objects produced by any given culture. But such difficulties severely constrain our capacity to appreciate other aesthetics at a glance. To go further requires extensive research—the catalogue contains an excellent bibliography.

REFERENCE

- GUSS, DAVID M. 1989. *To Weave and Sing: Art, Symbol, and Narrative in the South American Rainforest*, Berkeley: University of California Press.

BOOK REVIEWS

SIGNE HOWELL and ROY WILLIS (eds.), *Societies at Peace: Anthropological Perspectives*, London and New York: Routledge 1989. ix, 243 pp., Indexes. £11.95.

The main purpose of this collection is to overturn the usual unthinking assumption that human beings are naturally aggressive by focusing on a group of 'peaceful' societies and arguing instead for sociality as the key human condition. In addition to the editors' introduction there are eight ethnographic papers, plus two on the development of sociality (in human beings and in human society respectively), and two on the theoretical problems of defining peace as a category and of identifying peaceful societies.

The editors and contributors are understandably opposed to the work of most biologists and psychologists, whose only resource in arguing for the innateness of human aggression is observation, not merely of human but of animal behaviour, analogies being drawn from the latter to the former. One problem with the analogizing approach has always been that animal societies are coterminous with the whole species, whereas human ones subdivide it—hence the inevitability of a degree of cultural variation, in attitudes to aggression as in everything else. Another is that only observed behaviour is recoverable for animal societies, yet it is wholly inadequate in understanding human ones. The editors quite rightly insist, therefore, that the focus should be on 'humans as meaning-makers, rather than humans as biological primates' (Howell, p. vii), in order to get beyond the idea of aggression as simple motor response. This leads to a stress on personhood, on 'what it means to be a human being' (ibid.), something which the new anthropology has made particularly its own.

Various authors occasionally recognize (Howell and Willis, p. 5; Howell, p. 58; Carrithers, Heelas, *passim*) that to regard peace as inherent in human nature may be as dogmatic as the traditional focus in some quarters on aggression. But in general, the dangers of over-correction are ignored in the rush to get across the point that 'sociality, not aggression, is the key human trait' (the title of Carrithers' very programmatic paper). Actually, this does not say very much that is new, given that Carrithers defines sociality simply as the individual's 'capacity for complex social behaviour' (p. 197): such has long been recognized, and not only in anthropology, as the basic human attribute, and is essentially what anthropology is all about. In practice, there is a tendency throughout the book to inflate this definition to mean social co-operation and the avoidance of conflict and of aggressive acts, even feelings. And here we hit a snag, for the continual implication that aggression and co-operation are somehow mutually exclusive can hardly be sustained.

For a start, it neglects the many examples of feud, or of conflict between affinal alliance partners, within what are clearly single ethnicities (cf. Leach's observation, in *Political Systems of Highland Burma* (p. 153), that 'to the Kachin

way of thinking, co-operation and hostility are not very different' or the Mae Enga dictum recorded by Meggitt, in *The Lineage System of the Mae Enga of New Guinea* (p. 101), that 'we marry the people we fight'). In other words, sustained conflict requires, and achieves, the co-operation of opponents reacting in like manner rather than turning the other cheek. Secondly, while it may be true, as the editors say (p. 4), that some aspects of, especially modern, warfare need not entail aggressive feelings, all warfare demands a degree of co-operation between participants. Modern armies, for all their stress on cold efficiency in killing an often unseen, unheard enemy, are like mini-societies and are among the most highly ritualized institutions of modern life, using ritual to enforce both discipline and purpose. Tactics involving the closer combat associated with more traditional forms of warfare also habitually require at least the encouragement and excitation of aggressive feelings towards the enemy, even though these are commonly bound up with the cultivation and expression of stereotypical grievances, be they political, mystical or material in content. Finally, even if war *were* waged totally dispassionately, what of the passions commonly involved in insurrection and riot—the *intifada*, say, or the activities of Class War? The basic problem is that knowledge of exactly what inner states another person is feeling is notoriously difficult to uncover, especially in collective activities subject to cultural expectations, and this applies to aggression and its absence as much as to anything else. Aggression may be a socially constructed act, not a biological or psychological necessity; the act of defining it may be subject to Western ethnocentrism; and it may be absent from the values of a good many societies. But it is just as surely present in others, and may at times, or permanently, be their dominant structuring value.

By no means everything in the collection calls forth the urge to criticize. Robarchek reminds us that avoidance of conflict need not entail absolute passivity, but may engender passive resistance instead. From Gibson and Overing we learn that eating may be regarded as a violent act, especially if, as is true of the Piaroa, eating is necessarily cannibalistic (here, game is of one nature with humanity). With Howell and Overing we remember that not all societies have institutionalized sanctions against wrongful behaviour, as traditional Durkheimian orthodoxy teaches—a corrective against any image of society as a bunch of fractious individuals straining at the leash of social constraint. Both Chewong and Piaroa actively value peaceful co-operation, and the latter, at least, regard it as a goal to be striven for constantly, not a given whose presence can always be relied upon. Less happy is the editors' more generalized correlation between peaceability and equality (p. 24; here, gender equality in particular). Away from such societies as the Chewong and Fipa the matter is not so clear—the various 'peaces' of Rome, the British Empire and the Soviet Union, for example, have been peaces established by the one-sided force of a paramount ethnicity, and not characterized by equality of treatment for all, even at the ethnic level, let alone between genders.

Certainly, as regards human peace and aggression, innateness would not seem to enter the matter at all. While animals are simply predatory, acting with purpose but without meaning in the anthropological sense, only humans seem to be capable

of both lethal intra-species violence and the outright valuation of peace above the satisfaction of other conceivable interests. Both qualities, as well as all points along the continuum that links them, require the conscious and collective construction of meaning, which will differ from one society to another. The present collection emphasizes this process of construction as regards peace, but despite the cautions expressed by Campbell and Heelas at the end, few of its contributors seem to find the categories of peace and co-operation themselves at all problematic, nor the assumption of their necessary association. This marks the essential difference between this collection and David Riches' earlier and complementary volume, *The Anthropology of Violence* (Oxford, 1986), which achieved a greater degree of balance in seeing not merely violence but images of what constitutes violence as culturally constructed: arguing away the innateness of aggression does not deprive it of conventionality. To take it so much for granted that all humanity shares a single view of what peace actually is, or that it values co-operation in exactly the same ways, merely displays a naïvety sharply at odds with the sophistication most of these contributors have demonstrated elsewhere.

ROBERT PARKIN

R. G. DILLON, *Ranking and Resistance: A Precolonial Cameroonian Polity in Regional Perspective*, Stanford: Stanford University Press 1990. 310 pp. Index, Illustrations, Maps. No price given.

This book presents the corpus of data of Dillon's 1973 thesis and uses it to discuss the conditions for the maintenance of an acephalous polity. The Meta' are an acephalous group to the west of the kingdoms of the Bamenda Plateau. They are part of the regional system of the Cameroon Grassfields whose dynamics has been analysed by Dillon's fellow student at Pennsylvania, J.-P. Warnier (*Échanges, développement et hiérarchies dans le Bamenda pré-colonial (Cameroun)*, (Wiesbaden, 1985)). Chapter 3 comprises a good introduction to the Grassfields and to Warnier's regional analysis. One reservation, however, is that the concentration on the southern end of the Grassfields has led to the elision of the problematic Tikar influence of the north-western chiefdoms. This is unsurprising since the Meta' are found in the south of the grassfields, but to mention the Chamba but not the Tikar creates an imbalance in the discussion. Hence the summary cannot be wholeheartedly recommended as an introduction to the subject.

Dillon presents a reconstruction of the Meta' polity at the end of the nineteenth century before German influence was felt. Having described the economic system and the importance of patrilineal descent, Dillon gives an account of the ideal model of village-level leadership. This model was elicited through interviews with old men during fieldwork. He continues to describe some actual cases mentioned

by the interviewees, since these show how the ideal model worked in practice. Individuals may strive for power and seek to expand their influence. The distributed set of rights and powers meant that expansion had to be at the expense of others in a similar position in the lineage structure. At base, quarrels could be resolved by a recourse to force, but any fighting quickly triggered the most powerful conflict resolution processes, which rested on supernatural backing.

To call Meta' acephalous is not to say that they lacked chiefs. Dillon explains how village chiefs were powerful and respected but retained their power and respect only as long as they did not 'rule' or 'bear down' upon their subjects. They were foremost among a group of notables and could neither address the people directly nor act in their own right. Any attempts to do so would reduce the powers of other notables and hence be resisted by them. Since the notables spoke for and acted on behalf of the chief, he was rendered powerless to expand by the very powers vested in him. Dillon argues for a model of acephalous societies that combines Robin Horton's historicism with the results of the analysis of conflict resolution. Conflict may lead to fission unless balanced by such centripetal forces as the control of economics in the chiefdoms.

The Meta' were both producers of palm oil and important traders in oil, transferring it from the regions to their west to the kingdoms of the Bamenda Plateau. Trade, and its maintenance, was the major economic constraint on political structure and process in the late nineteenth century. First of all, the main commodity trade was a staple not a rare luxury good (such as iron, bronze or slaves), so that much of the population participated rather than a few specialist traders. Secondly, the need to keep trade routes open led to the strict control of slavery. It was a last resort to remove trouble-makers, or witches, and could only be undertaken after consultation with the senior kin of the person concerned. Attempts to circumvent this led to violence, the kin group of the enslaved person would defend them. Moreover, incursions from neighbouring groups led to defensive coalitions, which form the basis for the sense of unity of all Meta'. The only other arena for the unified polity was the ritual treatment of homicide. Power was held by the heads of patrilineal groups, which were usually co-resident. The dissemination of various ritual and political offices among the different lineage heads was the main force counterbalancing the centralizing tendency of a chief eager for power.

A major omission is that there is no discussion of the role of women. This probably reflects current interests at the period of fieldwork (1970). Granted that women's political action has been documented both to the north and to the south of Meta', one wonders what means Meta' women had of countering insult and other forms of oppression.

One of the puzzles left unresolved is that posed by Meta' witchcraft. The section that discusses it (pp. 185-91) describes Meta' witchcraft as occurring only between kin motivated by spite or jealousy, or by those who felt slighted, for example, by not receiving their due share of bridewealth. If divination diagnosed a case of witchcraft, then oaths could be taken and as a final resort the accused

person could be either executed or sold as a slave. This account sits uneasily with an earlier passage describing the rites to close a village to sorcery, which appears to enter from outside the polity. The relationship of sorcery, *sa*, to witchcraft, *izik*, is not discussed.

DAVID ZEITLYN

RON BRUNTON, *The Abandoned Narcotic: Kava and Cultural Instability in Melanesia* (Cambridge Studies in Social Anthropology 69; gen. ed. Jack Goody), Cambridge: Cambridge University Press 1989. viii, 178 pp., References, Glossary, Index, Plates, Diagram, Maps, Tables. £25.00/\$35.00.

Brunton begins his analysis with a reconsideration of Rivers's original diffusionist explanation for the scattered geographical distribution of contemporary kava use. Rivers had suggested that the arrival of a different cultural group (the betel people) with their easier-to-prepare intoxicant was the primary cause of the decline and eventual abandonment of kava in various Oceanic societies.

Kava is found in various parts of Melanesia (including New Guinea, New Britain, New Ireland, the Solomon Islands, Fiji and Vanuatu), throughout Polynesia (except for Easter Island, New Zealand, the Chatham Islands and Rapa), and scarcely at all in Micronesia. Brunton considers three possible explanations for the fact that kava-drinking regions are interrupted by groups of non-consumers: (1) that there were never direct links between regions and that the psychoactive nature of kava was discovered independently in each area; (2) that there were direct links in trade or a migration occurred—in either case the plant and/or knowledge of its preparation were transmitted; and (3) that such links were indirect and that intermediary populations abandoned the use of the plant prior to European contact. On the basis of botanical, linguistic, ethnological and (rather scanty) archaeological evidence the author concludes that the third explanation is almost certainly correct. Having established this he proceeds to refute Rivers's kava-people and betel-people theory partly on the grounds that the supposed exclusivity of the practices is contradicted by twentieth-century ethnographic reports. Arbitrary characteristics of kava drinking that recur in diverse regions are used to show how the complex diffused, for such factors cannot be adequately explained away by geographical and biophysiological restraints. Many of these characteristics (for example, preparation in the particular form of a drink, the belief that kava-related material culture should be kept from contact with the ground, and origin myths concerning the plant) link it with religion and ritual activity.

The significance of this is elaborated with data from Brunton's fieldwork in Vanuatu (New Hebrides), mainly on the island of Tanna, during the 1970s. With the effects of contact with Christianity and the subsequent secularization and

profanation of kava drinking came problems of excessive use on the one hand and a religious reaction on the other. The latter, at least in certain parts of Tanna, resulted in revelations ordering its suppression in social and religious life. How far these findings from Vanuatu hold true for Melanesia in general is unclear. To be fair, the author does not overstate the possibilities of such comparison. The well-known themes of Melanesian cultural obsolescence and susceptibility to acculturation are seen by Brunton as significant in the region even prior to pacification and missionization. This is put forward as the key factor in the abandonment of kava in various areas of Melanesia. Kava usage in Polynesia is seen as more stable because the legitimization of authority is not a significant problem there. Clearly, this is a more satisfying overall explanation than Rivers's, although Brunton does see betel-chewing as a factor in kava's decline.

The book augments Brunton's 1989 *Man* article 'Cultural Instability in Egalitarian Societies' with its in-depth treatment of the Melanesian manifestations of this trait. The accompanying tables of kava cognates with additional commentaries make for a rather abstruse part of the book, yet its rigour—including a database of 460 Papuan and Oceanic Austronesian languages—compensates for this disruption in what is otherwise a flowing text. The illustrations are disappointing in that neither the plant itself (*Piper methysticum*) nor any of the attendant material culture are featured. The two plates showing Tannese ritual are of poor quality. The lack of emphasis on material culture is also reflected in the text itself, though this is a common omission in modern anthropological writing. Despite these reservations the book is valuable, both in tackling the Melanesian problem of cultural instability from a novel perspective and in providing a study of intoxicant use for those of us interested in the wider comparative aspects.

RICHARD RUDGLEY

HANNA HAVNEVIK, *Tibetan Buddhist Nuns: History, Cultural Norms and Social Reality*, Oslo: Norwegian University Press and The Institute for Comparative Research in Human Culture 1990. 218 pp., Plates, Glossary, Bibliography, Appendix. £25.00.

This smartly produced book, evidently the author's doctoral thesis, would probably never have been published in this form in the USA, but would rather have circulated in microfiche or microfiche photocopy. This would have been a pity. It is written in a simple and refreshingly unpretentious style. In spite of a few repetitious passages and its uncritical summaries of what Sanskritists and Tibetologists have said, on the basis of texts, about the position of women in Buddhism, it is the first work on an interesting and very worthwhile subject. It should interest specialists on Tibet, on Buddhism and in women's studies.

Havnevik's research, based in one of two Tibetan Buddhist nunneries established in exile in India, was carried out in English over a relatively short period. No doubt an anthropologist working in the vernacular would have produced data of greater depth and subtlety. None the less, Havnevik does provide some new and interesting information, thereby demonstrating that it is just as important to ask the right questions as it is to ask them in the local language. Since she names informants and quotes their words at length, others will be able to use and build on her work. Two small examples may be mentioned here. Evidently, lay Tibetans believe nuns particularly appropriate as performers of the worship of the goddess Tara; in fact the Tilokpur nuns derive most of their income from such Tara rituals sponsored by the laity. Secondly, nuns who are rumoured to be having an affair with their lama are treated with great respect by Tantric practitioners as embodying the highest female wisdom. This is in accord with Tantric ideology, but one would never learn this from better-known authors, whether Tibetologists or anthropologists, who tend to write as if the values of celibacy and chastity are unequivocally upheld and never challenged within Tibetan Buddhism.

Havnevik is much more interested in the question of the nuns' 'secondary' status as women and nuns. Much space is devoted to the question of how they can overcome this, and to the question of whether only women who are in some sense failures in lay life become nuns. There is no doubt where the author's sympathies lie on these questions. Occasionally, one may convict her of anthropological naïvety. For example, she comments on the disapproval Tibetans generally feel for certain nuns of Western origin who wear the monk's yellow vest: 'It seems to be a classic cultural impasse created by lack of communication on both sides' (p. 204). The well-meaning assumption that more communication would resolve the issue could easily be turned on its head. However, Havnevik very plausibly identifies a vicious circle in which Tibetan nuns are caught. Because they are not respected as much as monks they receive few invitations to rituals; thus they receive few donations; thus they can rarely afford to invite high lamas to give the teachings that would command respect; and so they are not respected, because they are relatively unlearned; and so on. She also notes, however, that even if nuns do acquire some of the formal qualifications of the monks (that is, through study and spiritual retreats) they will probably still have secondary status, this being a reflection of women's lower status in Tibetan society as a whole.

DAVID N. GELLNER

JOHN H. INGHAM, *Mary, Michael, and Lucifer: Folk Catholicism in Central Mexico* (Latin American Monographs No. 69), Austin: University of Texas Press 1989 [1986]. x, 216 pp., Bibliography, Index, Tables, Maps, Figures. \$25.00.

Ethnographic work on Nahuatl-speaking populations has accelerated extraordinarily in the past twenty-five years. When reviewing the state of scholarship on Mexico's indigenous population in 1967 Ralph Beals noted: 'for great areas occupied by the largest language group in Mexico, the Nahuatl speakers, we have absolutely nothing beyond a few casual observations, if even that'. Knowledge was restricted to the area around Mexico City, and even then one could only count Oscar Lewis's studies of Tepoztlán, William Madsen's on Milpa Alta and José de Jesus Montoya Briones's monograph on Atla.

John Ingham's *Mary, Michael and Lucifer* marks a major development in modern Nahuatl studies. More than any previous monograph on the area, Ingham's new work combines historical and structural insights concerning the problems of syncretism and acculturation in an elegant analysis that benefits from more than twenty years of fieldwork experience. His long-term commitment to Tlayacapan has given rise to rare and authoritative insights into the metaphysical constitution of the body, into ideas about conception and sickness, and into the relationship between these and indigenous views of the morally constituted community.

Ingham distinguishes between structural and semantic levels of enquiry. While tracing changes in the meaning the community gives to the world, as a result of nearly 500 years of post-contact history, he nevertheless notes the persistent social classifications inherited from pre-Hispanic times that continue to structure ideology and social organization. In his interpretation, sixteenth-century Catholicism not only encouraged the recognition of similarities between the saints and pre-Hispanic deities as a matter of expediency, but also assimilated a considerable part of the older structure on which belief and social organization were based. Indigenous conceptions of space, time and the relations between ethical categories have remained remarkably persistent.

After the Conquest, the Spanish reorganization of the community substituted barrios in place of the old *calpulli*, transforming their leaders into *mayordomos* who retained responsibility for religious celebrations and administrative matters. Ingham's older collaborators recall the town being divided into twenty-eight barrios, each with its own chapel and local saint that had been substituted for a pre-Hispanic deity. Although the town is left with only thirteen barrios and seven functioning chapels, on some ritual and social occasions the town still acts as four principal barrios, thus recalling the pre-Hispanic pattern where each *calpulli* was organized within four larger wards.

Spanish pragmatism also permitted Tlayacapan to retain a modified form of the pre-Hispanic *Tonalpohualli*, or calendar, which instead of structuring the ceremonies of various Aztec deities now organized those of the Catholic saints who were attributed similar powers and jurisdictions. Barrios were left the

responsibility to organize, in the main, agricultural and life-stage festivals, while the large Christian feasts, fairs and carnivals were organized at the village level.

Tlayacapan family relations and spiritual kinship may bear less pre-Hispanic influence than is found in other domains of the local culture. The family is structured on an ambiguous metaphor of the Holy Family. When a woman is nursing and sexually continent, she is compared to the Virgin Mary, while the man when dutiful and self-denying, parallels the role of St Joseph. Nevertheless, male and female sexual roles and men's behaviour in masculine groups lead to other aspects of life being compared to the relations between Adam and Eve. Ritual kinship is interpreted as a way of domesticating adverse behaviour that strays from the ideal roles represented by the Holy Family. Godparents are assigned at baptism, when a child is spiritually regenerated (after his conception through sinful sexual intercourse) and incorporated into the body of the church. The ritual of baptism and blessing purifies the child and introduces it into the spiritual community whose members organize and participate in Christian ceremonies and fiestas that closely associate them with the Holy Family and the saints. Although Ingham does not argue that metaphorical linkages between human and divine families may themselves be based on pre-Hispanic antecedents, evidence from the Huichol and Mayo suggests that this may be a more general characteristic of Uto-Aztecan classifications. As in other parts of Latin America, people who are not part of the moral community are identified with the Devil and seen as opposed to it. The different manifestations of demonic power, La Llorona, the Culebra de Aqua and Lucifer, are seen as derived from the evil pre-Hispanic deities, Cihuacoatl, Quetzalcoatl and Tezcatlipoca, suggesting that Christian ethics have wholly substituted more ambiguous pre-Hispanic categories, which probably did not distinguish absolute categories of good and evil. While Tlayacapan conceptual classifications illustrate a high degree of structural coherence, Ingham emphasizes the role of pragmatism in the choosing and manipulation of categories in everyday life, thereby acknowledging the historical dimension of the community's existence.

ANTHONY SHELTON

STANLEY BRANDES, *Power and Persuasion: Fiestas and Social Control in Rural Mexico*, Philadelphia: University of Pennsylvania Press 1988. 112 pp., Maps, Tables, Illustrations, Glossary, Bibliography, Index. £26.55/£14.20.

Tzintzuntzan, a mixed Indian/Mestizo municipality on the shores of Lake Patzcuaro, Mexico, is one of the country's few communities to have received intensive long-term anthropological attention, in this case extending over forty years. Brandes's account of the dynamics and pragmatics governing the community's religious celebrations draws upon earlier studies by George Foster

and others, to provide a historical description of the organization of fiestas and an interpretation of their changing importance for the community's identity and cohesion. As Tzintzuntzan has become increasingly incorporated into the Mexican state, communal celebrations have declined while those that survived have become more elaborate. Surviving communally based ceremonies were refinanced to spread the burden from a few individual sponsors to larger segments of the community, thereby shifting the potential for competitiveness from individuals and families to wider, though still internal, community divisions. However, family celebrations centred around baptisms, weddings and funerals have also become more lavish, providing a new arena for competitively displaying both wealth and personal qualities.

Brandes challenges common assumptions about the timeless and intransmutable character of religious celebrations. In Tzintzuntzan, the church played an important role in reforming and reorganizing the fiesta system. During the 1930s and 1940s, the number of official positions for religious sponsors (*cargos*) was much reduced and the means of financing celebrations reformed. Some episodes that the church or civil authorities deemed unsuitable were discontinued while others were elaborated. Particularly noteworthy is the community's adoption of a very elaborate 'Night of the Dead' celebration, encouraged and marketed by state and church agencies to bring more tourism to the town. The decidedly low-key celebrations described by Foster before the 1970s contrast markedly with later events, which have assumed many of the attributes of the more elaborate expressions found elsewhere in Mexico. Furthermore, although 90 per cent of the population of Tzintzuntzan is Mestizo, the community has come to identify itself strongly with a ceremony that has strong indigenous roots and has contested the authority of the local church to simplify it. Even the relative importance of one celebration to another can change. For example, the celebration of the miraculous image of El Señor del Rescate assumed greater importance than that of the feast day of the town's patron saint, after the former was credited with saving the town from a smallpox epidemic in the late nineteenth century.

Not only does Brandes describe the fiesta system as an open institution subject to different internal and external exigencies, but, he argues, the competition and mediation of these diverse influences provides a rich medium for examining the power relations between different groups. Brandes offers a functional explanation for the fiesta system, arguing that it provides a means of reiterating the ideal parameters of socially sanctioned behaviour that are contrasted with their antithesis: a world of disorder, evil, ill-health and, ultimately, death. The fear in which villagers hold evil constrains their everyday behaviour.

Studies of such communities as Tzintzuntzan, where the working of social forces has been well documented over a protracted period, provide examples of how the effects of the indigenous encounter with the modern world might be better appreciated and of how the mechanisms underlying incorporation, rejection or partial assimilation might be explained and more general models of change devised. They may also eventually be able to help us explain changes in the

consciousness of identity, away from village or insular affiliation towards acceptance of a wider national identity. Brandes's work is an important contribution to the history and ethnography of Tzintzuntzan in particular, while providing a timely corrective to some widespread and false assumptions about Mexican fiestas in general.

ANTHONY SHELTON

CHRISTOPHER ALAN WATERMAN, *Jùjú: A Social History and Ethnography of an African Popular Music*, Chicago and London: University of Chicago Press 1990. xii, 242 pp., Glossary, Bibliography, Index, Maps, Plates Figures. £35.95/£17.50/\$51.75/\$19.50.

Waterman's study of Jùjú, based on fieldwork conducted between 1979 and 1982, is a big book for its 242 pages. Each paragraph is saturated with description and argument, his summaries of anthropological and ethnomusicological theory are studies in precision, and he possesses a descriptive and analytical style, often drily humorous, that cuts to the quick.

The text consists of two quite distinct halves. The first is a history of Jùjú that situates documentary sources and oral accounts in the wider history of West Africa's political and economic transformation. Lagos itself was a complex urban environment before colonization. Its distinct class and ethnic make-up in the early twentieth century structured the emergence of a proliferation of popular genres: the Brass Bands, Highlife, European popular and classical genres favoured by the 'Black Europeans', Brazilian popular musics adapted by returnee Yoruba slaves, and Christian church and north Nigerian Muslim styles. Rural-urban migration began to transform Lagos in the 1920s and '30s, and guitar-based 'Palmwine' music, which emerged in new migrant recreational contexts in the city, began to bring together diffuse stylistic currents, named Jùjú, in 1932. During and immediately after the Second World War, musical entertainment in the 'hotels' (bars-cum-brothels) of Lagos, and the emergence of new Nigerian élites provided patronage and performance contexts for Jùjú musicians. Waterman describes how the genre was subsequently transformed by the technologization of instruments (allowing for the expansion of the bands to incorporate electric guitars, talking drums and vocal choruses), and its involvement with foreign recording companies and the Nigerian media.

Ethnomusicologists have often talked in a simplistic fashion about the socio-economic context of music. Waterman points out that this not only reifies music as an irreducible object or essence, but it also ignores the fact that music is itself a context in which other things can happen. The second half of the book, an ethnography of musicians in Ibadan, examines this assertion. Jùjú is an

essential component of *àrìyá* rituals, neo-traditional events that celebrate births, deaths, weddings, namings and business launches. The ability of musicians to sustain the spatio-temporal 'textures' of the *àrìyá* is critical to the success or failure of the event, in which statuses and identities are transacted and negotiated through competitive 'spraying' (covering musicians and hosts with bank notes) and dancing. The aesthetic vocabularies and performance techniques that make Jùjú time 'roll' are complex, but Waterman's own experience as an evidently accomplished Jùjú bassist is a fascinating key to his discussion of Jùjú aesthetics and techniques.

Musicians in Ibadan today are entrepreneurs, obsessed with technological modernization, the creation of new genres, and the crossing of religious and ethnic boundaries. Their role in successful *àrìyá* celebrations is vital to the social mobility of Nigerian entrepreneurial élites. Since the establishment of electoral politics in Nigeria, these élites have made intensive use of Jùjú musicians in their creation of clientage networks. Consequently, successful bands are highly dependent upon them for patronage—kitting out a nine-piece Jùjú band is an expensive business. It is not surprising that Waterman concludes that Jùjú performance encodes and perpetuates the values of this élite. Jùjú bands provide a model of a hierarchically organized whole, consisting at each level of interlocking instrumental textures united by a leader/soloist. This, Waterman argues, provides an apt metaphor of the unified heterogeneity of an idealized Nigeria: a hierarchical society whose inequalities are mitigated by the generosity of the rich and possibilities of social advancement for the poor. This is endorsed through the rags-to-riches mythologies of the 'commanders', 'kings' and 'admirals' of Jùjú, which obfuscate processes of class stratification in Nigeria's oil-boom economy. Waterman's final statement is, however, one of an ambiguity that lies at the heart of any performance, for musicians are indeed engaged in the hegemonic reproduction of dominant values, but at the same time their performance 'preserves as well as conceals alternative readings'. Music can transform as well as reproduce.

The question is how. Waterman is less convincing here. Partly this is due to the fact that his study is essentially an ethnography of Jùjú musicians. Seeing Jùjú from this angle has the evident advantage of Waterman's insider experience, and any ethnography is bound to select certain angles and perspectives at the expense of others. This approach, however, is inclined to under-represent the way in which the music is experienced by its fans, most of whom are the urban dispossessed, and (since band members are all men) the ways in which gender must affect the experience of Jùjú. 'Listening' is itself a performed event in which alternative readings are always possible. A phenomenology of fanship, for example, might have provided a view of how different popular genres (such as Muslim Nigerian Fuji) are seen in relation to one another, and the hopes and fantasies embodied by particular musicians. An account of how women experience Jùjú might have provided a means of assessing the socially transforming potential of a music that seems firmly embedded in male sexual ideology.

Waterman's description of the way in which popular musicians shape notions of power and identity fills many gaps in the anthropological and ethnomusicological literature. The idea of a detailed ethnography of a popular genre itself breaks new ground. It will also be of value outside anthropology and ethnomusicology, in that variety of disciplines that share an interest in, and a stake in defining, 'the popular'.

MARTIN STOKES

LORRAINE NENCEL and PETER PELS (eds.), *Constructing Knowledge: Authority and Critique in Social Science* (Inquiries in Social Construction; ser. eds. Kenneth J. Gergen and John Shotter), London etc.: Sage 1991. xvi, 202 pp., References, Index. £12.95.

Constructing Knowledge is a collection of papers delivered at the conference on critical anthropology sponsored by and held at the Department of Cultural Anthropology, University of Amsterdam, in December 1988. Generally, the papers address the present situation of uncertainty in anthropology, which some have labelled the 'crisis in anthropology'. They deal with such issues as anthropological authority, the identity of the anthropologist in the field, feminist critique, grand theory, cultural relativism and ethnocentrism. In other words, they address the problems created by anthropologists' endeavours to acquire systematic knowledge of 'other' people.

One of the better papers is the editors' introduction. Instead of the standard synopsis of each paper they have opted to contextualize them by providing a brief history of critical anthropology that could easily be extended into a book. Beginning with Malinowski's critique of science their narrative progresses through anthropologists' use of Thomas Kuhn's idea of paradigm shift to attack anthropology's scientific presumptions, to the founding of the critical anthropology movement with the publication of Dell Hymes's *Reinventing Anthropology* (1974). They continue with a comparison of Marxist and feminist critiques and end their history with the critique of ethnography and the breakdown of anthropological authority in the 1980s. Their thesis is that postmodernist thought, with its tendency to undermine efforts at legitimation of the scientific project (i.e. anthropology's crisis) is the product of a history of critical reflection. In other words, it is critical anthropology itself that has caused the crisis in anthropology.

In an interesting essay entitled 'Anthropological Doubt', Tom Lemaire deals with the contradiction at the heart of anthropology: it is a science rooted in a particular culture that claims to be universal in scope. He argues that fieldwork exposes one's habits, ideas, concepts and values to other societies, and that in order to understand these alien categories one must entertain doubts about the

validity of one's own categories. Self-criticism allows one to perceive or contemplate the existence of an alternative. In other words, for anthropology to be possible it must necessarily be self-critical. In a rambling essay drawing on his experience with Latin American and African peasants Gerrit Huizer asks 'whether anthropology in crisis can learn from the way "men and women in crisis" [i.e. peasants] deal with crisis' (p. 41). Peasants deal with it by understanding the causes of their situation and by a continuous spiritual revitalization. Unfortunately, it is the latter that Huizer stresses. Robert Pool presents the text of a fieldwork interview as the basis for some critical remarks about ethnographic representation. The author 'participates' in the interview as an observer, the interview itself being conducted by an interpreter. Pool points out that the use of interpreters is common, especially during the early months of fieldwork when the researcher lacks the necessary fluency, but that little mention of their contribution is made in the resulting ethnography. He discusses some of the problems of this method and suggests that the interpreter should, at the very least, be recognized as making a creative contribution in constructing the ethnography. After a couple of pages of babble Stephen Tyler settles down to make an obtuse comparison of the abstract 'essentials' of modernism and postmodernism. His dense poetical style, however, is a turn-off. The next essay, by Jonathan Friedman, is little better. Also written in a dense style, it takes anthropology's concern with the 'other' as a search for Western identity, an identity which, he asserts, has been disintegrated by postmodernism. The next two chapters deal with feminist anthropology's encounter with postmodernism. Annelies Moors compares the construction of difference in feminist studies in anthropology with that typified by Said's *Orientalism*. Particular emphasis is placed on the characterization of Middle Eastern (Oriental) women in the anthropological literature. Annemiek Richters fails to deliver on her promise to answer the question whether postmodernism is 'yet another masculine invention engineered to exclude women' (p. 125) and instead provides a feminist critique of modern mental health diagnosis.

Perhaps the best essay in the book is that by Olivia Harris. In 'Time and Difference in Anthropological Writing' Harris begins by pointing out that although it is the individual who conducts fieldwork and encounters the world of the 'other', what is at issue is how the society of the 'other' and the anthropologist's own society are juxtaposed, compared and brought into some sort of relationship. The fieldworker acts as a mediator. Time is a powerful metaphor of otherness that such anthropologists as Evans-Pritchard, Geertz, Whorf, Lévi-Strauss and Leach, among others, have used to construct difference and establish the relationship between their society and 'ours'. Harris is not concerned with the 'other's' conception of time, however, so much as she is with 'ours'. She discovers that not only do anthropological discussions of time depend on (usually) unexamined comparison with 'our' time but that among anthropologists the concepts of 'our' time differ. This raises the important question, 'What do we mean by "our" or "we"?' All anthropologists invoke a broader collective self and Harris goes on to examine the implications of this invoked 'we'.

Joke Schrijvers actively assisted a group of poor women in Sri Lanka escape their poverty by organizing a small collective farm. In the penultimate essay in the book she discusses the change in these women as they learn to stand up for themselves and the difficulties in dealing simultaneously with people in different positions of power, i.e. the Dutch government agency employing her and the women she was helping. The final article is a personal account by Johannes Fabian of some of the problems—such as revealing secrets, identifying informants, taking sides, exposing information to the powers that be, and so on—he struggled with in his initial fieldwork among the Jamaa of Zaïre. He has few new insights to offer. I do agree, however, with his final statement, which seems to epitomize the entire collection of essays: ‘we need critique (exposure of imperialist lies, of the workings of capitalism, of the misguided ideas of scientism, and all the rest) to *help ourselves*. The catch is, of course, that ourselves ought to be them as well as us’ (p. 201).

CHRIS HOLDSWORTH

SCOTT ATRAN, *Cognitive Foundations of Natural History: Towards an Anthropology of Science*, Cambridge etc.: Cambridge University Press/Paris: Maison des Sciences de l’Homme 1990. xii, 320 pp., Index, References. £35.00/\$49.50.

Many anthropologists, especially perhaps from Oxford, locate classification at the core of their discipline. Thus, in their introduction to *ASA 27, History and Ethnicity* (London, 1989), Chapman, McDonald and Tonkin write of subsuming the classification of people within ‘classification in general—an area of expertise that anthropology has made its own’; and the word recurs many times in their brief text. Without disputing the suggestiveness and value of the term itself, one might wonder just how wide-ranging and well-articulated is that expertise. Atran’s book is about one of the most obvious sorts of classification, that of living kinds (plants and animals), but the issues it raises could not be called hackneyed.

Just as a kinship analyst can reasonably (I think) choose to concentrate on formal analysis of the classification of relatives while ignoring the practical significance of the categories, so Atran bypasses classifications based on the use or value of living kinds for humans (i.e. such taxa as fruit, weeds, pets, beasts of burden), as well as issues of symbolism. His concern is rather with the nature of folk biological nomenclature or ethnotaxonomies (sparrows and birds, oaks and trees), and with the relationship between these taxonomies and scientific ones. He is not an ethnographer, but an explorer, rather in the tradition of Dan Sperber, of the borderlands between anthropology, psychology and the history and philosophy of science.

Atran's central claim concerns the plurality of cognitive subsystems. Back in 1975 he had contrasted Piaget, who believed that humans approached all domains of knowledge and experience with the same undifferentiated faculty of general intelligence, with Chomsky, who held that humans can draw on a number of distinct and specific cognitive capacities, for instance the one that enables a young child to master a language in spite of scrappy exposure to it. Atran favours Chomsky, and envisages 'many more or less autonomous psychological sub-systems' going into the making of culture (p. 50). In particular, the domain of living kinds, he argues, is cognized differently from the domain of artefacts, and it is partly for its blurring of the difference that he attacks the prototype theory of Eleanor Rosch and followers. The classification of living kinds conforms to a cut and dried hierarchy and contrasts with the 'notoriously open-textured' cross-cutting classification of artefacts. Are pianos musical instruments or pieces of furniture? Is a wheel-chair or a bean-bag or a car-seat really a chair? We do not ask parallel questions of a tiger, which is unambiguously an animal and which we conceive of as possessing an underlying essence or nature. A chair is a different matter, and here we do more readily think in terms of prototype plus penumbra of comparable objects, bearing a more or less close family resemblance to the focal type.

What general features are typical of folk biological classifications? The genus is often the smallest grouping recognizable without expert study, and in any case, within a given environment, many genera will be represented only by a single species. Thus the basic-level taxon is here called the generic-specieme (e.g. horse), though this is not to deny the sporadic occurrence of finer differentiations. Between the unique beginner (animal, plant) and the generic-specieme, the main level of grouping is the life-form (mammal, bird, tree, bush...) (on which see my review in *JASO* (Vol. XV, no. 2 (1984)) of Cecil Brown's *Language and Living Things*). At a lower level there may well also be 'family fragments', groupings recognized by informants but often unlabelled. These may cross-cut life-forms, and are unsystematic in the sense that they do not form an exhaustive partitioning of a local flora or fauna.

A good two-thirds of the book deals with the history of systematics from Aristotle to Darwin, and some readers may prefer to skip from the end of part I direct to the conclusions. But the history is instructive. Aristotle starts not far from the scale and suppositions of unlettered folk the world over, dealing with fewer than 600 species from his own environment and only 30 exotics. Evidently a major feature of the story is the growth in the number of species included in scientific classifications after the voyages of discovery and the spread of printing. Tournefort in 1694 was already dealing with between 6,000 and 10,000 species of plants, but by reducing them to 600 genera he was harking back to the order of complexity faced by Aristotle. Moreover, in elaborating their hierarchies the naturalists were able to develop the covert 'family fragments' of the folk classifications, as well as draw on such metaphysical ideas as the great chain of being and evolution.

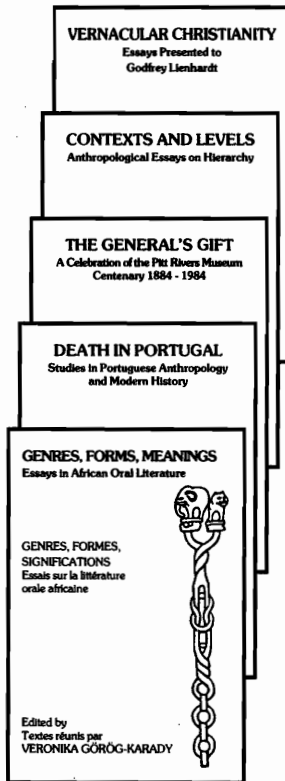
One purpose of the book is to champion the 'common sense' that produces folk taxonomies. Even in the scientific age common sense retains a role, not only for laymen but also for scientists, whose specialities presuppose that they understand the 'naïve' notions from which the speciality developed. In any case, is it naïve to hold that the generic-specieme *dog* is more real to us than the family fragment *canine*? Science is a curious social institution, to which participant observation of behaviour in laboratories is only one among the various possible anthropological approaches. On the basis of his cognitive approach, Atran aligns it, albeit almost in passing, not so much with other subcultures as with other such second-order elaborations of common-sense classification as totemism.

With its considerable range (45 pages of end-notes, 550 references) and its numerous critical comments on other scholars, this is scarcely an easy read. But it is a rich and provocative book that gives some insight into what a lot we should have to know really to be experts in classification.

N. J. ALLEN

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