CONTENTS

Obituary Notice
Ronald Godfrey Lienhardt .................................................. 99

PETER RIVIÈRE
Godfrey Lienhardt ............................................................... 101-103

EDWIN ARDENER
The ‘Personal Enemy’ in African Politics (with an introduction
by David Zeitlyn) ............................................................... 105-110

JOHN KNIGHT
The Danish Pavilion in Kyoto: Tracing the Local Career
of a World Fair Exhibit ...................................................... 111-117

Review Article
N. J. ALLEN
Debating Dumézil: Recent Studies in Comparative Mythology .. 119-131

ANDREW DUFF-COOPER
Values and Hierarchy in the Study of Balinese Forms of Life:
On the Applicability of Dumontian Perspectives and the
Ranking of Contexts ........................................................... 133-152

Comment
Parkin’s Evidence
by Georg Pfeffer ................................................................. 153-157

Second Reply to Pfeffer
by Robert Parkin ................................................................. 158-163

Malinowski and the Imponderabilia of Art and Photography
by Terence Wright .............................................................. 164-165

Book Reviews
ROGER SANIEK (ed.), Fieldnotes: The Makings of Anthropology;
and JOHN L. WENGLE, Ethnographers in the Field: The Psychology
of Research.
Reviewed by Charles Stewart .............................................. 167-169

WENDY JAMES, The Listening Ebony: Moral Knowledge, Religion,
and Power among the Uduk of Sudan.
Reviewed by Fiona Bowie .................................................... 169-171
Book Reviews (continued)

JOHANNES FABIAN, Power and Performance: Ethnographic Explorations through Proverbial Wisdom and Theatre in Shaba, Zaire.
Reviewed by Gregory van Alstyne ................................................................. 172-173

Reviewed by C. J. Fuller ................................................................. 173-175

LINDSEY HARLAN, Religion and Rajput Women: The Ethic of Protection in Contemporary Narratives.
Reviewed by Graham Dwyer ................................................................. 176-177

Reviewed by Helene Basu ................................................................. 177-180

HASTINGS DONNAN, Marriage among Muslims: Preference and Choice in Northern Pakistan.
Reviewed by Camilla Gibb ................................................................. 180-181

ROBERT BOROFSKY, Making History: Pukapukan and Anthropological Constructions of Knowledge.
Reviewed by S. A. Abram ................................................................. 181-183

GEORGES CONDOMINAS, From Lawa to Mon, From Saa' to Thai: Historical and Anthropological Aspects of Southeast Asian Social Spaces.
Reviewed by Klaus Hesse ................................................................. 183-184

GENE WELTFISH, The Lost Universe: Pawnee Life and Culture.
Reviewed by R. H. Barnes ................................................................. 184-185

JAMES R. Murie, Ceremonies of the Pawnee.
Reviewed by R. H Barnes ................................................................. 185-186

Reviewed by Marcus Banks ................................................................. 186-187

ERNEST BRANDEWIE, When Giants Walked the Earth: The Life and Times of Wilhelm Schmidt SVD.
Reviewed by Stefan Dietrich ................................................................. 187-188

Publications Received ................................................................. 189-195
Contributors to this Issue ................................................................. inside back cover

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OBITUARY NOTICE

RONALD GODFREY LIENHARDT

17 January 1921 to 9 November 1993

It is with deep sorrow that we have to announce the death of Godfrey Lienhardt on Tuesday, 9 November 1993 after a short illness. From his arrival in Oxford in 1947 until his death, Godfrey was such a part of Oxford anthropology that it is difficult to imagine what it will be like without him. Oxford and the wider anthropological community has lost one of its greatest characters.

JASO also has lost a valued friend and supporter. Godfrey always took an interest in the Journal and its fortunes, and on a number of occasions chose it as the vehicle for publishing his work. We were particularly honoured to be able to publish his essay ‘Frazer’s Anthropology: Science and Sensibility’, the revised text of his 1991 Frazer Lecture, in our last issue.

Obituaries have already appeared in the Independent (17 November), the Guardian (19 November) and the Sudan Democratic Gazette (December) and we hope to be able to publish appreciations of Godfrey’s life and work in future issues. In the meantime, we are pleased to be able to publish on the following pages the text of an address delivered at his Requiem Mass.

The Editors

A Memorial Fund is to be established in Godfrey Lienhardt’s name. It will be administered by Wolfson College, Oxford, and used to foster research. Wolfson College is also to host an event, to be held on the afternoon of Saturday 7 May 1994, to commemorate and celebrate his life. Further details about both the Memorial Fund and the event are available from the College Secretary, Wolfson College, Oxford OX2 6UD, to whom contributions to the ‘Godfrey Lienhardt Memorial Fund’ may be sent.
GODFREY LIENHARDT

Text of an address delivered at the Requiem Mass for Ronald Godfrey Lienhardt held at The Oratory, Oxford, the Catholic Church of St. Aloysius, on Tuesday, 16 November 1993.

This is an immensely sad occasion, but I do not wish to dwell on its sadness; nor would Godfrey have wanted me to. The time I am allowed is far too short to do justice to my subject, even if I were able to. A biographical sketch is out of the question; anyway, in my view, such dry bones are better confined to the obituary columns. Neither is this the time nor the occasion for an assessment of Godfrey’s significant contribution to anthropology; that is better left to the pages of learned journals. What I want to do in the few minutes available is talk about Godfrey as Godfrey. Such an approach inevitably depends to a great extent on personal reminiscences and impressions; something each of us individually has. I hope that by talking about mine, you will be able silently to recall and think about your own.

When putting together notes for this address, I found that I have absolutely no recollection of my first meeting with Godfrey. I know it must have been just over 30 years ago, but the transition from not knowing Godfrey to knowing him seems to have passed for me without memorable incident. I do remember when we discovered that our birthdays fell on the same day of the year—something we had in common with Radcliffe-Brown. We jointly celebrated the event thereafter. However, Godfrey was not necessarily that easy to get to know, for he could hold strong and not always entirely reasonable prejudices. Godfrey could be witheringly and hurtfully dismissive, and, for some, getting to know him required patience and determination. But once accepted into his circle they would find with Godfrey a deep, loyal and enduring friendship. Perhaps the best evidence for this is the extraordinary degree to which this loyalty and friendship have been affectionately
and steadfastly reciprocated—by you, and by many others all over the world who cannot be here today.

Godfrey loved being with his friends, and I doubt that he was ever happier than when surrounded by a group of them in one of the various pubs he frequented over the years. Conversation, and he was a great conversationalist, was an important art form for Godfrey. But it was not simply the conviviality of such surroundings that was important in drawing people to his side. When he was in hospital, something which occurred with distressing frequency in recent years, he could always be assured of a constant stream—flood, might be a better description—of visitors, many of whom travelled from London or further afield to see him.

What perhaps is remarkable is the wide range of Godfrey’s friends. You are not simply academic colleagues but people drawn from all sorts of backgrounds, ages and countries. Nor was his a closed circle; there was always room for more. Last June, a few days before the examinations, I took a couple of my students for a drink and a sandwich, and Godfrey happened to join us. When I had to leave, the two graduates stayed on for a long time; they were, they later told me, intrigued and fascinated by him. Godfrey also enjoyed the occasion and just before the beginning of this academic year he asked me whether I would be able to arrange for him to meet some of this year’s intake.

In many ways, you, his friends, substituted for the close family that he otherwise lacked, especially after the death of his brother Peter. He felt that loss very deeply and for a period became, by his standards, almost a recluse. I am certain that Godfrey would welcome the inclusion of Peter in our thoughts and memories today. However, as happens in even the most harmonious families, Godfrey was not above testing the relationships internal to his. You can probably think of your own examples. A particularly fine example was his plan—or threat—to retire to Madrid or Lisbon. This idea brought forth a volley of protests and objections, which, one suspects, was just the point of the ploy.

An aspect of Godfrey’s friendship was not only his generosity of spirit, but also a more material generosity. I do not wish to say much about this as it was always conducted with careful discretion and usually with total anonymity. I doubt that Godfrey ever thought much about money. His own needs were relatively simple: he ate sparsely—too sparsely perhaps; he dressed himself as often as not at Oxfam; and he cut his own hair.

There was a similar unworldliness in his approach to technology—perhaps best exemplified by his typewriter, an upright model of between-wars vintage which he never gave up using, despite the effort required to work the keys. However, in recent years, when he found it difficult to go out in the evening, he was persuaded to have a television set and fell for some most un-Godfreyesque programmes. ‘Coronation Street’ and ‘The Bill’ had become his favourites, he once confided with a wry smile.

It would be difficult for me to talk about Godfrey for long without the word ‘smile’ coming up. For me, one of the fascinating things about Godfrey was
watching his smile—or better, smiles. He had a whole repertoire of them that involved different parts of his face in various combinations. All had their own clear meanings which had to be learnt if one was to understand him. There was one which started in the corners of the eyes and ran across the bridge of the nose, which meant roughly: 'I know that what I am saying is not exactly accurate but I am not going to spoil a good story for a ha’p’orth of truth.' Another, in which the mouth played a more prominent part, indicated: 'I don’t believe a word of what you have just said but I cannot be bothered to say so.'

If the smiles were a sort of disguise, he also wore a mask of apparent indifference to many things, matters that he actually felt quite deeply about but often could not bring himself to face up to. This could be infuriating and even, at times, seems to have deceived Godfrey himself. Although I have no evidence to substantiate it, this I suspect is what happened to the part which religion played in his life—the practice, if not the belief of which he turned away from, but to which, at the end, as this requiem mass held at his request indicates, he returned.

It does not seem long ago, and indeed it is a frighteningly short time only, since many of us were gathered at Wolfson College here in Oxford to mark Godfrey's retirement. That event, a marvellous party, in itself is evidence enough of the affection in which Godfrey was held. It is a great sadness that the intervening years were marked by his increasing frailty. He himself had begun to realise that the time when he could continue to live alone was limited, and not long before his final illness was expressing some anxiety on this score. For those of you who had not seen Godfrey recently, you should know that he had become very frail. However, this was merely a physical decline; the mind remained strong and mischievous, and even until close to death there were flashes of that acerbic wit and hints of enigmatic smiles.

Godfrey is no longer with us in person, but I know that whenever a company of his friends meet, his name will be on their lips. Godfrey will be there in memory and in spirit. Knowing Godfrey has enriched my life and I am sure that it has done the same for yours. God bless him.

PETER RIVIÈRE
PHILIP BAGBY STUDENTSHIP
IN SOCIAL ANTHROPOLOGY
1994-95

Applications are invited for the Philip Bagby Studentship, which is open to graduates of any university who are suitably qualified in social anthropology. The period of tenure will not normally exceed two years, and is for a maximum of three years. The award will cover University fees (at the rate for UK and EC students) and college fees, if applicable, plus a maintenance grant the value of which will be at least £4720 (the rate for 1993-94).

Further details are obtainable from the Secretary of the Anthropology and Geography Board, c/o the Oriental Institute, Pusey Lane, Oxford OX1 2LE.

Closing date for applications is 28 March 1994.
THE 'PERSONAL ENEMY' IN AFRICAN POLITICS

EDWIN ARDENER

(with an introduction by David Zeitlyn)

Introduction

The text that follows comprises the abstract of a paper presented to the 1964 annual meeting of the British Association for the Advancement of Science (Section N) on 31 August 1964. Sadly the full paper is lost. It has not been published before and is now a document of considerable historical interest. It is published here with the kind permission of Mrs Shirley Ardener. Only minor editorial changes have been made.

'The “Personal Enemy” in African Politics’ was written early in the period of independence of the once colonial African states, before any of the subsequent coups, civil wars and other bloody events that now affect our views of Africa. Yet it is an ironic tribute to Ardener’s analysis that it makes somewhat depressing reading. For little has changed: the litany of reports on the radio and in our newspapers continues along the lines he describes here. Scandal rocks the government in Zimbabwe, suggestions of assassination and worse haunt ministers in Kenya.

Within anthropology, much has been done to explore the different African idioms and metaphors that frame discussions of politics in its local context. But despite such path-making work as that of Horton (1967), or more recently Werbner (1979) and Hallen and Sodipo (1986), which connect concepts of religion, personhood and politics, the link has not been made to a wider political domain as the term is usually understood in discussions of Europe or North America. It
is therefore to be hoped that Ardener’s paper may stimulate further discussions between anthropologists and political scientists.

It is timely to publish this paper now because the changes that Ardener signals have begun to occur. A comparison between the bibliographies of two important works by Jean-François Bayart, the most eminent political scientist to study Cameroon, reveals a remarkable change between 1979 and 1989. More anthropologists are cited in the latter work, and they are cited alongside ‘orthodox’ political scientists. I am sure that Ardener would have approved this widening of the perspective.

REFERENCES


POLITICAL commentators of the weekly journal variety find themselves out of their depth when commenting on the personalities of African politics. More words have been eaten on the subjects of Presidents Nkrumah, Sekou Touré and Tshombe (to name only three) than will bear computation. The sight of the last of these embracing his bitter enemy Antoine Gizenga was the coup de grâce for many naïve observers. In similar quarters, the trend to one-party systems has been deplored, or as unthinking praised, while vociferous opposition within the parti unique has been unnoticed or misunderstood. In one African country, civil servants expelled from their employment for alleged political activities drink at the elbows of their former ministerial masters, in the same club, in apparent amicability. In another, soldiers apologize to the victims of an unconsidered assault. The nature of opposition and conflict and the conclusions to be drawn from them are simply of different kinds from those current in the West. The ethnocentricity of political observers is very marked, a result perhaps of their living in an essentially paper world of manifestos and the press. Faced with African situations, commentary tends to fall into a tedious rehearsal of cold-war clichés on the one hand, alternating with shock and bafflement on the other—especially from self-styled ‘friends of Africa’ who find it difficult to find a suitable political
bandwagon that will not turn round in its tracks, leaving them embarrassingly facing the wrong direction. Their Eastern counterparts have already coined the term ‘tropicalism’ to account for their own difficulties.

The African field anywhere, then, provides us with examples of the need to work with an adequate theory of conflict and opposition. I come now to a simple statement of the sort of direction in which, in my opinion, comprehension lies. Taken as formal systems, traditional and modern African political activity appear completely different in kind. On the one hand, we have those systems amply documented in standard works written or edited by Evans-Pritchard, Fortes, Middleton and Tait, as well as in more recent studies. Among the latter, one by the president of this section, Professor Lucy Mair (1962), is easily available to the general reader. Such systems, as is well known, range from complex balances of forces operating through families, lineages and clans, to simple systems of hierarchical authority headed by chiefs. On the other hand, we are today presented with a great number of modern state forms—with presidential and parliamentary arrangements, political parties, elections, and the like—which appear to be the image of such systems outside Africa. There has been no formal transition between the old systems and the new. Whatever ‘social change’ is, there can surely be no more critical case of it than this! The supposition of such a sudden change was reflected in the movement of the study of African politics from social anthropologists to ‘political scientists’—whatever is precisely meant by that term—with the results that we have seen: the spectacle of shock, bafflement and the rest, and the departure of the first naïve theorists from the field.

The fault has lain in the misconception of what political systems involve: such sudden ‘social changes’ are illusory, the exchange of one card house for another. The continuity lies in the forces of ‘change’ themselves: those conflicts and oppositions that are (as we have already suggested) not transition phenomena, but the essential material of social behaviour, of which formal systems are (we may go so far as to say) merely the epiphenomena. More precisely, they are attempts to describe parts of the pattern of opposition. Perhaps the formulations describe only part of the patterns: the patterns are not thereby abolished. They remain the primary analytical units of political study.

In turning to African politics, therefore, we may ignore both the traditional systems and the modern systems as such. The bases of opposition and competition may be sought for and found in various places. I have selected only one for this paper: the concept of the personal enemy. To members of this gathering it will perhaps be unusual to think in terms of your enemies. Even individuals who are in positions of potential conflict do not necessarily become conscious of personal enmities. In modern urban societies the situation is, in Simmel’s words, ‘comparable to the bottom of a ship which is composed of many watertight compartments—if it is damaged, the water can still not penetrate all of it. Here the social principle is thus a certain separation of the colliding parties’ (Simmel 1955: 66). Such a separation did not exist in traditional African systems, and even today such a separation is only partially achieved. Indeed it is only partially
desired, for the well-known African respect for humanity is an expression of a
tendency to retain rather than to minimize direct personal contacts.

In those African societies with which I am familiar, conflict is clearly
personalized. In traditional circumstances no one is too poor or insignificant to
lack ‘enemies’. Very often these enemies are indicated by their positions in the
social structure. In one society I know, men live in suspicion and fear of the
families of their fathers’ brothers—for these ‘wicked uncles’ inherit their brother’s
lands and property should his sons all die. The uncles are believed to hate the
sons, and to wish to bring about their deaths by witchcraft. In another society men
do not build near their own half-brothers for similar reasons. In Central Africa,
says Gluckman (1956: 51):

the village headman in most tribes is the centre of a constant struggle, both in
terms of backbiting and intrigue, and of a war in the mystical world. For he is
believed to attain his position and maintain it by using witchcraft against his
rivals; and he himself constantly suspects that he is the target of the envious
witchcraft of his rivals, and of those whom he has rebuked.

Gluckman gives the telling example of the Zambian headman who kept tapping an
ulcer on his face and saying: ‘it is the government, it is the government, it is the
government’—meaning that because of his position under the government, he had
been bewitched with the ulcer. Other enmities have a historical origin, but when
they exist they may be perpetuated down the generations: when things go well in
the family the ‘enemies’ are thought to be discomfited, when the family suffers
injury or loss the ‘enemies’ are thought to be laughing or, and here we come to the
core of the matter, even to have caused the misfortune.

It should not be thought that politicians operate outside this web. On the
contrary, it is their business to utilize it and to understand it. They are also part
of it. In the former British Trusteeship of Cameroon the early political movement
was split between a KNC (Kamerun National Congress) majority and a KPP
(Kamerun People’s Party) minority. Their leaders came from the same village—a
village ridden by sectional rivalries partly originating in an old conflict from the
time of the imposition of German rule. In this village the KNC had a majority.
Outside it the KPP dominated another village that had long been in rivalry with
the first. Within the second village, however, the personal enemies of the KPP
majority supported the KNC. This pattern replicated itself in fascinating detail
throughout the area of the ethnic group concerned. The extension of the pattern
beyond the ethnic group to the country at large followed similar principles. The
rise of yet another party (the KNDP or Kamerun National Democratic Party) in
another ethnic area, and its subsequent spread, again followed this pattern. Of
course, at all stages in political party growth sectional rivalries of wider range, the
so-called ‘tribalism’ or ‘regionalism’, were mobilized—so much is now recognized
even by political commentators. These terms, however, are misleading. If they
represented realities of the first level of analysis, the ethnic map would never fail
to represent the political map. African politicians know that the problem is more fundamental.

A man begins his life with enemies; as he succeeds, they grow in number. A politician, like anyone else, carries his own opposition. The complex of political parties runs down to individuals, in opposition to others in relations of enmity and rivalry. The electorate perceives opposition in this sense. As a result there need be no theoretical end to the formation of parties: no end to the crossings of the carpet, no end to the shifting permutations of alliance and treachery. The revival or stimulation of one enmity may move the whole structure. Some of you may say that something of this sort underlies all political systems. Yes, indeed, that is one of the points of this paper. But with African systems as at present constituted we have the additional feature of the personalization of enmities. There is no doubt that at the lower party levels, and among the more unsophisticated back-benchers, the danger of witchcraft is felt to be an ever-present reality. The death of Adelabu, the minority NCNC (National Council of Nigeria and Cameroon [later, National Council of Nigerian Citizens]) leader in Western Nigeria, occurred in a motor accident, some 50 miles or more from his home town. No one held that the accident was physically engineered, yet his supporters wreaked vengeance on hundreds of enemies in Ibadan. It is of course an axiom that enemies may or may not 'exist', it is enough that they are believed to exist. It is not to be wondered that African leaders cannot, and do not, look upon opposition as a formal matter. The more free they are of such sentiments themselves, the more are they conscious of the peculiar basis of their public support. The less they are themselves so free, the less appealing is the notion of opposition. In either event, it is no occasion for surprise that the electorate would gladly be dispensed with by even the most enlightened rulers. As for a single party, they may well think that the ineradicable tendencies to opposition even within this might be enough for any system. It may be said by some of you (especially perhaps any there may be from across the Atlantic) that this is the usual pattern of trends towards dictatorship. If, by this, European examples are meant, the personalization of enmity produces something of another kind. Northern dictatorships are as insulated and impersonal as northern democracies; if perfect northern democracy does not exist in African states, nor at least does perfect northern despotism.

We come here to the final apparent paradox. If the existence of personal enemies is accepted by Africans of high and low status, so also is the need to live with some of them. Like the poor they are always present. Some may vanish from the scene, but others appear. Wickedness may even be renounced and a personal enemy become, at least temporarily, an object of trust. The coldness of the northerner, whose hates are impersonal, inhuman and (thus) consistent, is foreign. So, therefore, the friendly chats with those deprived of their employment, so the return to the fold of those denounced for the most heinous crimes, so the sudden amends to the victim.

It is to be regretted that time and prudence, since I am in Africa, do not permit further illustration of these principles here. I have talked of 'Africa'; I am
fully aware that countries in Africa differ in their political systems as in other ways; I am aware that the phenomena described are not restricted to Africa at the present time; I am aware too that our history and some of our more inbred communities illustrate similar features. This does not matter because I am making no adverse value judgements; indeed, such is the state of this subject that it is necessary to say clearly in any public gathering that there are many elements in the present African situation that are of great interest and of possible human value. The personalization of conflict is not necessarily a bad thing. Experience has shown that unconsidered violence has grown more widespread within countries in the West with the increasing depersonalization of social relations. It is a notable contribution from Africa that it should announce that even enemies are people.

Lest my conclusion should recall the statement of the clergymen in the ‘Beyond the Fringe’ sketch that, ‘we must get the violence off the streets and into the churches’, let me repeat my earlier remarks. Without an adequate theory of conflict and opposition, no comprehension of the forces at work in the new states, or in our own midst, is possible. This is a task for comparative sociologists of the most sophisticated kind, and not (if I may end on a critical note) for amateurs enmeshed in the values of formal systems, which are already inadequate to represent the realities of the countries of their birth.

References

THE DANISH PAVILION IN KYOTO:
TRACING THE LOCAL CAREER OF A WORLD FAIR EXHIBIT

JOHN KNIGHT

The international gatherings known as world fairs are often branded 'trade fairs'. The implication is that they are essentially commercial: the national pavilions are shops in which national goods can be bought and national industries invested in; only incidentally are they about cultural diplomacy. I attended the 1992 Seville Universal Exposition with just this attitude. The degree of corporate sponsorship of the pavilions, the involvement of trade ministries in running them and the fact that in some pavilions almost every 'exhibit' had a price-tag would seem to confirm such a belief. However, pavilion directors and exposition staff were at pains to deny the charge, at least in its stark form. I was regularly reminded that this was a Universal Exposition, the highest grade of fair, which serves to celebrate a particular aspect of human achievement, and which is held only occasionally. Thus 'Seville '92' commemorated Columbus's voyage five centuries earlier and was devoted to the theme of 'discovery', providing a licence for each nation to present its own unique contribution to the world. 'Trade fairs', by contrast, are the lowest grade of fair, organized with much greater regularity and accorded much less international attention. The Seville Exposition was, therefore, at least as much a matter of national prestige and international education as it was a matter of international commerce.

That the Seville Exposition was not simply a trade fair is something that I now fully accept. While it certainly was commercial, as an event its overall functioning was quite different. It was less a market for goods than a forum for identity-fixing. It was first and foremost a place of international exhibition, an arena for the display of nations. It does not follow that because goods were sold in the
pavilions, the pavilions were simply a means to sell goods. Rather, it would be more apt to see the goods as part of the national presentations and of the international competition between them. The ‘trade fair’ appellation is misleading because the rationale of the fair had much more to do with the selling of nations, their identities and their differences, than with the selling of particular products.

What convinced me that the fair was more a market of nations than of goods was a chance discovery of a rather literal instance of the sale of national identity: the purchase of the Danish pavilion by a Japanese rural municipality, Tamba Chō. In common with most other rural areas in post-war Japan, Tamba Chō has lost much of its population through migration to the cities. For many rural areas, this trend has been of such a scale, and has been sustained for so long, that depopulation has resulted, threatening the very social reproduction of some villages. That Tamba has been less affected than most is largely to do with its relative proximity to the Kansai metropolitan area, particularly to the city of Kyoto to which some of its population commute. It is, however, much more than a suburb of Kyoto, and has sought actively to develop itself as a distinctive ‘town’ in a number of ways. One of these, as we shall see, has involved the adoption of foreign motifs and even of foreign national identities. But Tamba has also drawn on a more recognizably Japanese source of identity in inviting other Japanese to make Tamba their ‘second hometown’ (*dai ni furusato*).

One notable feature of Japanese urbanization has been the tendency on the part of many migrants to remain connected to their natal village. While varying with income, sibling status and distance of outmigration, in general migrants return once a year or more to the village, visit ancestral graves with their parents, and join in village festivals. For many rural migrants the *furusato* left behind is the real home to which they hope one day to return. This involves a feeling of nostalgic longing that has been much written and sung about in post-war Japan. But in the 1990s, there are many Japanese for whom a remote little village of paddy-fields and persimmon trees, fireflies and cicadas, forms no part of a personal childhood past.

In offering itself to the population at large as a rural hometown, Tamba would appear to be directing itself to this new generation of metropolitan Japanese who, city born and bred, are no longer directly connected to the countryside through their life course. This self-marketing by rural places as villages-to-be-adopted, an increasingly common form of recruitment of tourists (as well as of customers for rural foodstuffs) in the 1980s and '90s, has become an important aspect of strategies of rural revival and development. Urbanization may have denuded such places of their population but they retain an appeal—and even appear to enhance it—albeit now as a place to be visited rather than actually lived in. Moreover, those who visit are not just first-generation migrants, but also other urban Japanese who, as *furusato members*, are in effect symbolic migrants. But Tamba Chō's
development strategy is rather more complex than this would suggest. For it does not present itself as the little village in the mountains where visitors can find the ‘heart’ (kokoro) of Japan (a much-used term in this context). This is a furusato whose appeal lies elsewhere.

World fairs are the beauty contests of nations. In Universal Expositions it is the pavilion buildings that are the main focus of aesthetic judgement. At Seville '92 there were many exotic buildings, ranging from Arabian palaces and Southeast Asian Buddhist temples to high modern glass-and-steel boxes with cascading water walls. One of the more successful buildings of the fair—on the basis of much media comment at least—was the elegant Danish pavilion. The official guide described it thus:

The Pavilion of Denmark is a reflection of the Danish landscape, characterized by its many islands. When the horizon is broken, it may well be by white sails. The Pavilion itself lies like an island, surrounded by water in a pool. It is like a sailing ship with sails 32m high... The Pavilion is an example of Danish design and craftsmanship. Inside the pavilion, Danish culture, technology and art are shown through a gigantic multi-media show.

Upon entering the pavilion, visitors were told to take a cushion, find a place to lie down, and look up at the ceiling. The main lights dimmed and three mobile sails on the ceiling were illuminated; for the next twenty minutes a film on the natural beauty and agricultural productivity of Denmark was shown. Compared to some other pavilions, packed with exhibits and featuring crowd-pulling 3-D, wraparound or giant-screen (Imax) cinema (see Knight 1992: 22-3), this ‘Danish experience’ was low-key. While its building was striking, its queues were not long and its restaurant/bar was noticeably empty. I recalled my earlier conversation with staff in the Norwegian pavilion who complained that the Spanish visitors thought Norway a province of Sweden, couldn’t tell one Scandinavian country from another and only came to get another stamp for their exposition passports. In this ‘festival of nations’ some were better known than others.

Yet I soon discovered that the Danish pavilion was the object of international attention from a specific quarter. For while speaking to pavilion staff I learnt that the Danish pavilion had been bought by the Japanese town of Tamba in Kyoto Prefecture, where it would be reassembled after the exposition. The town had even sent a young man, Hiroshi, to work in the pavilion, to learn about it and about Danish things in general. It was through a long interview with Hiroshi, along with

1. Most visitors carried with them a ‘passport’ in which to document their world journey around the site. Some even used their real passports.
some official pamphlets he provided, that I learnt the story of the Danish connection of a rural Japanese town.2

Hiroshi had just left university, where he had studied German, and had returned home to Tamba. The mayor's son, whose idea the purchase apparently was, was a friend of his and encouraged him to go to Seville for a three-month spell. He explained to me how central 'internationalization' (kokusaika) was to the future of the town, and how even before its interest in Denmark, Tamba had established an 'international exchange programme' with an Australian town whereby youngsters from one town visited the other and were hosted by local families. Tamba also had connections with a German town and had sent there one of its young people—the mayor's son—to do a full year's apprenticeship in German wine-making. He returned to establish a local wine-making factory, which now produces 'Tamba hock', Tamba hokku! Other Japanese towns and villages have been successful in their wine-making, and my strong impression was that neither the Australian nor the German ties had proved wholly satisfactory. This did not diminish the mayor's son's belief in the relationship between 'internationalization' (kokusaika) and the 'revitalization of the town' (machino kasseika). The mayor had sent him to study at a well-known American university and was apparently grooming him for the mayoral succession. The years abroad had made the mayor's son fluent in both English and German and had also inspired him with a 'vision' for the future, one which now centred on the Danish connection.

The Danish pavilion would be used in Tamba in a number of ways. First, it would be a venue for weddings. Secondly, it would be a restaurant serving authentic Danish food. Thirdly, and more generally, it would be a tourist attraction. By providing the town with a landmark, it would attract the metropolitan population of the Osaka-Kyoto region. They would admire the strangely shaped, boat-like building with its 'sails' and its architectural allusions to the island character of the Danish landscape (a point in common with Japan), and they would have the chance to try Danish cuisine.

Senior pavilion staff in Seville, for their part, saw the deal in terms of cultural diplomacy. They were relieved at and proud of the deal. Unlike most pavilions in the fair, theirs would live on and continue to exemplify Danish genius. The building would henceforth serve as a sort of outpost of Denmark in Japan and contribute to Japanese understanding of Danish culture. Among some of the younger Danish pavilion staff there was amusement at the thought that the building would be used for Japanese weddings, as well as excitement among those who would have the opportunity to visit the pavilion in Tamba.

While for the Danes the translocation of the pavilion had international significance, for Tamba it formed part of a strategy to establish a specific cultural

2. This article is based on three principal sources of information: the long interview with Hiroshi, pamphlets on Tamba Chò, and my own familiarity with similar development strategies in other parts of rural Japan, especially Wakayama where I did two-and-a-half years' fieldwork in the late 1980s. I have never visited Tamba Chò.
identity for the town in its competition for tourists. The furusato theme cannot by itself confer a distinctive identity on a town or village. Hence rural municipalities are engaged in a highly competitive struggle to develop ‘brand-names’ (burando) as a basis for marketing their goods and attracting visitors. The Danish pavilion would contribute greatly to the development of a distinctive ‘Tamba brand’.

To date, the most common means adopted to achieve this sort of diacritical identity has been for each municipality to exploit its own particular traditions. For example, Hongū Chō in Wakayama, where I have done fieldwork, while projecting itself as ‘a little furusato’ (chiisai na furusato), also proclaims to would-be visitors its 1000-year-old pilgrimage tradition, along with the mythological importance of the wider Kumano area of which it is part. The visitor is reminded that the founder of the Japanese imperial line passed through here as he fought his way to the Yamato plain where he established imperial rule. Hongū, in common with many other remoter places, also emphasizes its identity as a place of refuge for defeated warriors in the Middle Ages.

Tamba’s promotional literature also proclaims the depth of its past. It is a place of antiquity, for people have lived here ‘from the beginning of Japanese history’. Another example of the depth of its tradition is the Katsuragi autumn festival, which has been held ‘for over a thousand years without change’, and in which local men (many of them migrants who return for the occasion) carry the palanquins bearing their local deities around the town. Yet this kind of distinction is set to be replaced by one very different in nature. Instead of depth in time, Tamba increasingly prefers distance in space as the source of its self-definition vis-à-vis its neighbours.

The mayor of Tamba, prompted by his son, argued that what the town needed was a clear, attractive image in contrast to that of the ‘dark countryside’ (kurai inaka) that had driven Tamba’s youth to the cities. What Tamba needed was an image appropriate to the twenty-first century, and to this end it had decided to remake itself into a Danish town. A construction programme had been launched whereby such public buildings as schools, health centres and post offices were built or remodelled according to a ‘Danish style’. A new residential area of Danish-designed houses was also under construction. A new ‘urban resort recreation area’, to be known as the ‘Kyoto Denmark Park’, was also being planned, of which the Danish pavilion would form the centrepiece.

An ambitious programme of ‘social welfare’, aimed to raise Tamba to Danish levels, was also being enacted. First, the standard of municipal hygiene was to be raised by connecting all households to a piped sewer system. Secondly, the care of the elderly was to be improved—not by hiring more professional personnel but through such motivating measures as a one million yen reward for those who reach 100 years of age. Thirdly, civic apathy was to be countered and the civic participation of Tamba citizens boosted by the establishment of a municipal fax network, in which fax machines would be installed in all households. This new enhanced communication between the town office and its citizens was vital to the future, for only if the town acted ‘as one’ would there be progress.
What is being addressed here is the central problem in rural revival efforts in Japan today: the apathy of much of the local population in response to town office initiatives. Notwithstanding Tamba’s own claims to civic enthusiasm for its projects—its brochures mention local Australian netball teams and Danish folk dance troupes—the fax initiative suggests a local government rather desperate to achieve a greater degree of mobilization.3

Tamba is set to become a Japanese rural town with a Danish landscape at its core. But why Denmark? The official view would seem to be that Denmark serves as a model of a place that is at once agricultural and technologically advanced, whose people are active citizens, and in which care is provided for local people through advanced public welfare policies. Tamba—or at least the local government and other supporters of the scheme—is adopting Denmark, not so much because of any objective present-day similarity between the two places but because Denmark offers the municipality a model for its future. Denmark is both the key source of distinction for the new ‘Tamba brand’ and a template of self-imagination for municipal development.

There are many other examples of such international borrowings in the intense inter-local competition that is ‘village revival’ (see Knight 1993). The generalized rural decline in Japan is likely to be intensified by the prospective decline in agricultural support from central government. Tourism is seen as the way forward by many rural municipalities, but requires that they establish an effective appeal in a highly competitive market. One increasingly common strategy for establishing a distinctive brand-name has been for rural towns to take on identities from the international arena. In some cases, the idiom of nationhood is adopted in a direct and imitative manner: ‘passports’ are produced and ‘kingdoms’ and ‘republics’ proclaimed.4 More often it is a matter of borrowing particular themes and features. Thus domestic Japanese tourists find themselves beckoned by a Dutch village in Nagasaki, English farms in Hokkaido, Alpine meadows in Nagano, Swiss pensions in Wakayama, American ranches in Oita...and a Danish town in Kyoto!

3. That this sort of preoccupation on the part of Japanese municipal authorities is no recent development is suggested by Dore’s reference to village offices in the 1950s that ‘installed universal broadcast relay systems by which every house has loudspeakers—and often talking back apparatus’ (Dore 1959: 356-7).

4. These are generally done in a lighthearted way and are usually a means of highlighting other local characteristics. Thus the city of Kaya in Kagoshima, whose main product is the sweet potato, proclaimed itself the ‘Sweet Potato Kingdom’. Ideally the name should jibe with that of a real country, but sometimes this can backfire. When Susami Chō in Wakayama, a producer of domesticated boars, declared itself to be the ‘Kingdom of Inobutan’, punning the word for pig, buta, with the name of the country, Bhutan, the Bhutanese government objected—not least to the passport’s logo in the form of two pigs!
One striking feature of the Seville world fair was the way many national pavilions stressed internal diversity as a positive feature. Many nations appeared to be drawing on their constituent regions or localities in a much freer way than one imagines was formerly the case. The implication was that national identities did not have to be at the expense of local ones, and could instead draw on them. Here I have presented an example of a locality putting to its own use—albeit in the name of ‘internationalization’—a foreign national identity. Two distinct theatres of difference come together, as one makes use of the ready-made identities of the other.

The domestic appropriation of a regime of extra-domestic difference is by no means unfamiliar. The ethnic trend in Western clothing fashions is one example, and the rise of international tourism as a domain of conspicuous consumption another. Indeed, that international fields of difference may be domestically applied is an argument that has been used to account for the raison d’être of anthropology itself. Thus Boon (1982) has accused anthropologists of being in effect traders in exotic difference who exaggerate the cultures they introduce to domestic clients. Moreover, recent anthropological studies of consumption have shown that the creative domestication of the foreign is not confined to those Western countries in which anthropology arose but also extends to the non-Western world that anthropologists have traditionally studied (e.g. Friedman 1990). Yet the example presented here of a Japanese town purchasing a national identity at a world fair and putting it to a very specific use shows that the domestication process extends beyond the domain of individual consumption of foreign goods to that of local institutional deployment of foreign images. In regional Japan at least, there exists a vibrant local market for global difference.

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DEBATING DUMÉZIL:
RECENT STUDIES IN COMPARATIVE MYTHOLOGY

N. J. ALLEN

WOUTER W. BELIER, Decayed Gods: Origin and Development of Georges Dumézil's 'Idéologie Tripartie' (Studies in Greek and Roman Religion 7), Leiden etc.: Brill 1991. xv, 239 pp., Selected Bibliography, Index. Gld120.00/$68.75.


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DUMÉZIL (1898-1986) was an intellectual anomaly. He operated in the disciplinary space that lies between Indo-European comparative linguistics, comparative religion, and half-a-dozen specialized philological disciplines, but would have liked to have been recognized first and foremost as a historian. Yet there is a case for saying that in spirit his work is closer to anthropology than to any other discipline.

He was par excellence a comparativist, concentrating on the older Indo-European world, but was far from ignorant about other cultures (in particular,
Chinese, Turkish, Caucasus and Quechua). He used written sources but, like many anthropologists, was interested in them largely for what they could reveal about the unwritten traditions that shaped them. His work does indeed bear on myth, ritual and pantheons, but it goes far beyond ‘religion’, in any narrow sense of the word. It bears on social structure, law, ethnomedicine, narratives of all sorts including pseudo-history, but above all on the ideology that provides the more or less unitary framework for all these aspects of society, and for others. And he advanced strong claims. In all the main domains of the Indo-European-speaking world, he claimed, the ideology transmitted from the original period of unity remains detectable in particular contexts—at the very least we are dealing with cultural processes extending over millenniums. Unless the early Indo-Europeans turn out to have been extraordinarily unrepresentative, Dumézil’s claim ought to affect how we think in general terms about non-literate societies and the possibilities for comparing them. Provided, that is, that his claim is justified.

There are many reasons why Dumézil’s work is controversial. Some of them are good, or at least reasonable, and relate to real issues on which he may have been wrong. For instance, I have argued elsewhere that his notion of a ‘trifunctional’ Indo-European ideology was incomplete: in addition to his three main categories or clusters of ideas (pertaining respectively to the sacred, to force and to abundance), we need to recognize a fourth, pertaining to ‘otherness’ and covering both transcendence/totality (which is valued) and exclusion/nothingness (which is devalued). Such an addition opens the way to certain other revisions (Allen 1991; forthcoming a). But the present discussion concentrates on the ‘bad’ reasons why Dumézil is too little appreciated.¹

Belier’s book, formerly a Leiden doctorate, will please those—and they are many, for instance most historians of early Rome (cf. Poulet 1988)—who would like to continue comfortably ignoring the challenge posed by Dumézil. Its aim is (somewhat oddly) to ‘reconstruct’ and evaluate Dumézil’s theory for its usefulness, but not to determine its correctness (p. xii). It is little interested in Dumézil’s intellectual roots, in the other scholars who have developed his ideas, or in the primary sources he analysed, and concentrates on summarizing or excerpting from the oeuvre. A chapter on concepts and methods is followed by four others organized by region and topic and tracing the changing formulations. The conclusion is—or seems to be, for the wording is muted and elusive—that Dumézilian theory, aesthetically pleasing though it may be, is of minimal scientific

¹. The invitation to review the books listed above provided a welcome opportunity to take a broader look at a number of recent studies concerned with comparative mythology, particularly

Dumézilian, but not exclusively so.
value, that it is too vague to allow of verification or falsification, and that its application is imprecise and inconsistent.

Looking back over his oeuvre (which amounts to some 17,000 published pages), Dumézil himself distinguished three main phases (see ‘Bibliographie’ in Cahiers pour un temps 1981: 340-44). From his initial Frazerian phase little remained that he still judged useful; in 1938 the first glimpse of the trifunctional pattern ushered in a quarter century of rapid exploration; and 1966 saw the beginning of the summing up, the phase du bilan (the 1980s seem to me to constitute a fourth phase). Starting with quotations from 1924, Belier concentrates on the second phase, and although he lists the later publications, he makes little use of them. This is to work the wrong way round. A better approach to assessing the oeuvre would be to start with the mature formulations of phase 3, and work backwards only where it proves necessary for particular purposes. As Dumézil himself often observed, phase 2 saw many changes in particular interpretations, but Belier’s implicit charge—that the changes somehow vitiate the mature work—is groundless.

Dumézil did not present himself as a theory-builder (‘je ne suis pas théoricien, ni “dumézilien”’ (1987: 121)), and Belier’s attack on Dumézilian ‘theory’ is in part a rhetorical device (an unconscious one?). Dumézil’s strength lay in his sensitivity to common patterns found in different contexts, and in the prodigious erudition he drew on while exercising this sensitivity. No doubt all intellectual activity involves some sort of theory, but in Dumézil’s case it was not so abstruse as to require much ‘reconstructing’. It consisted essentially of two things: the application to extralinguistic domains of culture of the venerable family-tree model of language relationships (an endeavour already well established in the nineteenth century), and a fairly straightforward abstract formulation of the similarities of pattern that he observed. Belier neglects other Dumézilians (the term is convenient) on the grounds that they have not developed the theory, merely used it. However, the contrast between theory and application makes little sense: most of Dumézil’s own work after the early 1940s consisted of ‘applications’, i.e. of the recognition of new contexts in which the trifunctional pattern is manifested, together with elaboration of previous analyses and exploration of the relationship between new and old.

Immersed as he was in the nitty-gritty of the texts, and little interested in theorizing per se, Dumézil’s account of the notion of fonction is less full than it might have been. Belier is quite wrong, however, in thinking that the word is synonymous with activité. Having established that it covers activity (1958: 18f.), Dumézil goes on to emphasize that there is more to it. To paraphrase, the functions supplied the proto-Indo-European speakers with a heuristic and/or with classificatory principles that applied well beyond the realm of social structure. The eighteen lines of Dumézil’s text that define the individual functions are not vague, and could well have been quoted in extenso. Belier might also have meditated on the final sentence of the passage: ‘these are not a priori definitions but the convergent teaching of numerous applications of the tripartite ideology’ (ibid.).
In other words, a function is a domain within an ideology, a unit within a structure of ideas. As I have argued elsewhere (Allen forthcoming b), *function* and *ideology* in Dumézil’s writing are much like *class* and *form of primitive classification* in Durkheim and Mauss’s, and an analysis of the early Indo-Europeans in terms of the former is no stranger than an analysis of the Zuñi in terms of the latter.

Belier’s chapter on methods not only fails to come to grips with ‘function’, but also, by neglecting the later works, misses Dumézil’s ‘requirements of good sense’, the rules that he recommends to those claiming to identify trifunctional patterns. The elements of an ensemble should be ‘distinct, solidary, homogeneous and exhaustive’, as well as *obviously* pertaining to the relevant function (1979: 72). The rules are not always easy to apply, but again they are not vague.

Belier sees Dumézil as having recourse to a number of ‘auxiliary hypotheses’ to explain departures from what his theory predicts. The language is again tendentious and prejudicial, as if Dumézil (the would-be historian!) should expect to find identical manifestations of the trifunctional ideology in all branches of the Indo-European world. For instance, one of the ‘auxiliary hypotheses’ is the ‘Zoroastrian reform’, which explains why the trifunctional list of Zoroastrian ‘archangels’ (the Amesha Spentas) differs from the polytheism reconstructable for earlier periods of Indo-Iranian religion. But the problem for the comparativist is to give an account of both the similarities and the differences: how *could* this be done without postulating some change? It is no use merely referring to one of the non-comparativist Iranianists who disputes Dumézil’s position (which other specialists support).

Here and there one finds useful tabulations of information, but in general the book’s organization is unhelpful. Thus chapter 3, on the ‘Tripartite System’, starts with a section on India, covering social structure and theological patterns in the Vedic hymns, but it is not until chapter 6, ‘Tripartita Minor’, that we meet the similar pattern of gods in the *Mahābhārata*. The implied distinction between major and minor manifestations of the ideology is arbitrary and misleading. There are many details meriting criticism, but the underlying problem is one of general attitude. By casting its vague pall of doubt over the whole undertaking, and by failing even to try to discriminate between the aspects of the oeuvre that are well founded and those that are not, this backward-looking thesis has little to offer those curious about the present status and future possibilities of the field.

Bruce Lincoln at least engages with the primary sources. Regarded by Mircea Eliade as his most brilliant student (so Wendy Doniger’s foreword tells us), Lincoln devoted his doctorate to a comparison between early Iranians, as known from the texts, and East African cattle herders, as reported by ethnographers. The thesis as published (Lincoln 1981) combined Indo-Iranian *lexical/ideological* comparativism with a more materialist approach to pastoralism as a mode of
production. The first half of Lincoln’s new book expands the former interest and assembles a number of the author’s papers from around 1980. The main aim is to reconstruct proto-Indo-European conceptions relating to death and the Other World. The next hundred pages shift the emphasis from dying to killing. We move from the *Iliad*, via an overview article on war and warriors reprinted from Eliade’s *Encyclopedia of Religion*, to other forms of violence, especially sacrifice, and to forms of ideological oppression, for example of females in early Zoroastrian ethnophysiology. Whereas the first part of the book reflects, so we are told, the loss of a beloved grandfather, the second reflects a growing enthusiasm for Marxist political attitudes, especially for the analysis of myth as an attempt to legitimize domination. The last thirty pages, comprising ‘Polemic Pieces’, essentially constitute an attack on Dumézil, of whose politics Lincoln disapproves.

Lincoln is curiously ambivalent about his own earlier pieces: while he judges that they retain some value (p. xiii), he has agreed to republish them only at Wendy Doniger’s urging (p. 125). His unhappiness arises partly because the papers reflect a family-tree view of the Indo-European language group, which he now takes to be problematic, partly because (persuaded by an Italian friend) he now thinks common myths arise not from common descent but from common social structures and historical situations; and partly again (p. 124) because the essays pay insufficient attention to competing versions of myths. Neither of the first two objections is cogent, but if the third shows awareness of a tendency to oversimplify, then it has some substance. There is indeed a pervasive tendency to tidy up the materials so as to produce a neat and unitary picture. For instance, an essay on Druids maintains that for Celts and other Indo-European peoples ‘healing is a process precisely inverse to that of sacrifice’ (p. 181). All healing? All sacrifice? Precisely?

An additional reason for unhappiness ought to have been the number of mistranslations, a fault noted in these pages by John Penney (1987) in relation to another of Lincoln’s books. Rashly, the author alludes to his own ‘philological rigour’ (p. xiii), and seems to take pride in making his own translations (p. 1 n.); but the mistakes are often elementary ones. Here is a small selection: Greek *pempô* means send or escort, not lead (pp. 23, 85); *hikonto* means arrived, not were off (p. 78); *khreôntai* (in the context) means use, not need (p. 190); *horkion tanno* means take an oath, not cut an oath! (p. 196); a sentence from Plato is curiously garbled (p. 49), and a passage from Aeschylus purportedly suggesting that Agamemnon and Menelaus are twins does not do so (p. 40); Latin *cui plurima canities* means ‘whose copious grey hair’, not ‘most of whose...’ (p. 63); Sanskrit *yâna* means vehicle (in general), not ferry-boat (p. 69). Since most of the essays are reprints, it is odd that such errors have not been noted and corrected. But perhaps not so odd after all, in a writer whose thoughts, according to Doniger’s foreword, ‘progress at roughly the speed of light’.

The opportunity might also have been taken, when reprinting a paper on warrior ‘rage’ in Homer, to inform the reader that Dumézil examined the essay (1983: 181ff.) and concluded that it was entirely mistaken. Does Lincoln have an
answer to the particular criticisms? He does take up a critique of his 1981 book by Mary Boyce (1987), who holds that ancient Iranian society was bipartite, consisting of priests and herdsman but not warriors. Lincoln convincingly defends the trifunctional view of Dumézil and Benveniste. In doing so, however, he attacks the book by Wikander (published in German in 1938) on Indo-Iranian bands of warrior youth, disapproving of its ‘ideological underpinnings and resonances’. These he exemplifies by the Swedish scholar’s description of the god Indra as ‘Führer’ of the Maruts (storm spirits). But the German word is natural in the context, and the objection is ridiculous.

The final section consists of a TLS review from 1986 of the third volume of Dumézil’s *Esquisses*, a discussion of Dumézil’s methods as applied to the vexed question of ‘The One-Eyed and the One-Handed’, and an attempt to demonstrate that he condoned Turkish massacres of Armenians. The first of these pieces proclaims a shift of scholarly attention from traditionally academic discussion of the findings and legitimacy of Dumézil’s comparativism to the ideological underpinnings of his life and work; but in so far as this has taken place, it represents a byway, not to say a dead end. Arguments about Dumézil’s friendships during the 1920s and ’30s are irrelevant to the intellectual challenge posed by trifunctional theory. Dumézil inclined to the Right, indeed to monarchism, whereas his political critics, such as the classicist Momigliano, incline to the Left; but no hard evidence exists to show that Dumézil’s political views significantly affected his comparativism. As Lincoln admits, the matter turns largely on the interpretation of silence. It is perfectly true that Dumézil did not splice his academic publications with such comments as would commend him to a politically correct American academic in 1990; but why should he have?

As we have seen, Lincoln accepts the Dumézilian view of a proto-Indo-European tripartite division of labour, but he too, like Belier, fails to grapple with the application of the notion of ‘function’ outside social structure. All one learns here (p. 61) is that Lincoln doubts the Dumézilian view of the pantheon, partly because, as has been noted by others, the deities appearing in the ‘canonical’ trifunctional sets and the deities having reconstructable proto-Indo-European names do not overlap. This is to overrate the importance of etymology in cultural comparativism, and to ignore very many careful analyses that extend far beyond the gods. Where Lincoln does approach a Dumézilian theological formulation, his choice is unfortunate. Dumézil argued for a homology between two Scandinavian deities and two legendary heroes of early Rome, so that Odin the One-Eyed:Tyr the One-Handed::Cocles:Scaevola. Accepting this basic homology, Lincoln criticizes Dumézil’s narrowness in developing it. But he would have done better to criticize the Odin–Cocles component. Even if Odin is first-functional, which I doubt, Cocles is not, and he does not clearly outrank Scaevola. For once, the similarities are most likely coincidental. Although Dumézil frequently returned to this analysis, it is among his least satisfying.

The last chapter of Lincoln’s book exhumes a paper from 1927 in which Dumézil analyses certain massacres drawn from the myths of ancient Greece, India
and the Old Testament; but the attempt to elicit from it evidence of Dumézil’s deep moral deficiency is a flop. One wonders why Lincoln so desperately seeks such evidence.

The vacuity, not to mention the malice and ignorance, of the political attack on Dumézil is definitively demonstrated in the recent book by Eribon (1992). Best known for his biography of Foucault, Eribon had enjoyed conversations with Dumézil for some years before he edited the 1987 volume of Entretiens, which, together with the posthumous collection of Dumézil’s essays published in 1992, comprises the easiest and best introduction to the œuvre to appear so far. Stung by the groundless rumours that his friend had Nazi sympathies, Eribon has now produced a study of Dumézil’s university career and of the academic subculture in which he worked. He draws on many unpublished documents and letters, and has discovered a series of newspaper articles on foreign affairs, which Dumézil wrote in the 1930s under the pseudonym Georges Marcanay. We now know for certain that Dumézil approved of Mussolini. On the other hand, he abhorred Hitler and Nazism, and not only did he have a number of friends and supporters who were Jews, but his contemporaries in the 1930s detected in his work not the least whiff of antisemitism.

Two other recent books situate themselves within the Dumézilian tradition: Emilia Masson’s Le Combat pour l’Immortalité (1991) and Françoise LeRoux and Christian-J. Guyonvarc’h’s La Société celtique (1991). Educated in Yugoslavia, Masson interweaves her Hittite material with Sjöök, using little-known sources from Belgrade libraries, as well as some material gathered by interviewing Vlachs in north-east Serbia. She is not suggesting any special Hittite–Slav relationship, merely that both are particularly archaic Indo-European cultures.

Although the Hittite language is certainly linked with Indo-European, specialists have generally held that the culture, and especially the religion, owe much more to the non-Indo-European cultures of the area—the names of the gods are of Hattic origin. Masson wants to view the religion too as essentially Indo-European, and a priori the idea is well worth exploring. Dumézil himself did little in this area. He frequently cited a famous treaty contracted around 1380 BC between Hittite and Mitannian rulers, since the gods invoked include the canonical Vedic trifunctional grouping of Mitra–Varuna, Indra and the Násatyas. He also analysed in trifunctional terms the Hittite ritual for luring deities away from enemy towns, and encouraged the pioneering study by Sergent (1983), a paper inadequately acknowledged by Masson. As for the Slavs, while Dumézil was confident that the vast folklore material would be of value to comparativism, he thought the sources too scanty to permit reconstruction of an articulated theology (1968: 624ff.; 1983: 193ff.).
Since Masson opens by claiming to be working in posthumous collaboration with Dumézil, it is disquieting that she ignores his views on his own intellectual development, as well as his advice on prudent methods of argument. Masson’s earlier chapters, dealing with winter and spring festivals, draw heavily both on Frazer and on Dumézil’s work from the 1920s, treating the latter as authoritative; but Dumézil himself thought his early work had shown ‘un laxisme consternant, tant dans la détermination des éléments comparables que dans l’appréciation des ressemblances’ (1987: 118). The three functions, so central to his mature view of the specificity of the Indo-European heritage, hardly appear here before page 225, and his view of the relation between the three functions and the three levels of the cosmos (that they are distinct phenomena occasionally brought together), is simply ignored.

In fact, Masson’s view of Indo-European ideology departs fundamentally from Dumézil’s. Take her admiration for Nodilo (1834–1912), a little-known Croatian mythologist. One is always pleased if the reputation of worthy pioneers can be resuscitated, but alas there are good reasons why the Indo-European comparativism of Max Müller’s generation is generally judged to have achieved practically nothing. There is all the difference in the world between assembling Indo-European triads, as Nodilo apparently did, and following Dumézil in recognizing three functions, which are expressed in some triads and not others, and which may perfectly well underlie quartets, quintets (such as the Vedic gods), sextets (Aramesha Spentas) etc. However, somewhat like her supporter, the Indo-Europeanist Haudry (who has been criticized with force by Dubuisson (1991)), Masson sees the three cosmic levels as more fundamental than the three functions, and as having provided the model on which the Hittites and other Indo-Europeans organized the world around them, their society, pantheon, life, even their bodies (Masson 1991: 199). ‘Collaborators’ who depart so radically and so grandiosely from Dumézil’s views need to give good reasons, and, in particular, Masson needs to explain why we should regard the three-level cosmos as specifically Indo-European.

The main straightforward trifunctional interpretation concerns the Hittite grouping Sun God, Storm God, Tutelary God (‘UTU-us, ‘U-as, ‘KAL) which Masson claims can be henceforth aligned with such canonical triads as Jupiter–Mars–Quirinus. However her trifunctional construal has a number of difficulties and is not helped by the unconvincing argument that the frequent coupling of the Sun God of Heaven with the Sun Goddess of Earth corresponds to Dumézil’s first-function dualism of Varuna and Mitra (both maieus). Dumézil applauded the salutary paper by Sergent (1979), which rejected a good number of purported trifunctional analyses of Greek material, and I doubt whether, as is claimed, he would have welcomed Masson’s book as providing ‘confirmation’ of his most important theory (Masson 1991: 231).

As in Lincoln’s book (for example when he writes on humoural theory in the middle Persian Zād Spram), one can find interesting material. I was particularly struck by the photograph of an elderly Vlach matron demonstrating how, in the world above our own, belts are worn around the neck, while in the world below
they are worn around the knees. Might one compare the mythical Irish Lughaid, who had two or three red stripes around his body, one being round his neck (Dumézil 1983: 237f.)? But the challenge for comparativism is not to cite or expound interesting material—of which there is no shortage—but to be convincing.

While Masson writes as a recent convert (of two to three years standing?), LeRoux and Guyonvarc'h, two Celtists from Rennes, ‘discovered’ Dumézil in 1948, and in turn were often referred to by him. In their recent book (1991) they discuss social structure (druids, warrior nobles, commoners) and various other manifestations of trifunctionality drawn from Irish narrative, with a quick glance at the Welsh Mabinogion. Their interpretation of the educators of Cuchulainn is persuasive, but too often the argument is arbitrary, even wild. One cannot simply affirm that the sun or the yellow broom pertain to the second function (ibid.: 103, 184), or that boar, wolf, stag are trifunctional (ibid.: 178). A crux for comparativist approaches to Ireland is provided by the Tuatha Dé Danann, a group of mythic beings deriving from the pre-Christian pantheon. Their argument that Lug is “hors-classe” par le haut (ibid.: 92) is cogent (cf. Dumézil 1968: 189 n.), but the attempt to divide the five main male figures into a first-functional ‘triad of sovereignty’ (Lug, Dagda, Ogmé) and a pair of third-functional specialists (Dian Cecht and Goibniu) is not.2

A number of the formulations are odd, for example that the third function is somehow negative and lacks intrinsic quiddity (LeRoux and Guyonvarc'h 1991: 33, 133), that the tripartite ideology is outside history (ibid.: 68), or that warfare is feminine because of the passions it arouses (ibid.: 95). The references are sometimes out of date, and the writing falls far short of the lucid organization and stylistic grace of its inspirer. For all the book’s good intentions, it may discourage more potential new readers of Dumézil than it attracts. Would-be friends may do more damage than overt opponents.

One such opponent is the Cambridge archaeologist Colin Renfrew. In his well-known book of 1987 (cf. his paper of 1989), Renfrew argues that the dispersal of the Indo-European speakers was associated with the spread of agriculture, and started not, as is usually thought, in the third to fourth millennium BC north of the Black Sea, but around the sixth millennium south of it. Our concern here, however, is only with chapter 10 of his book, in which he tries to cast doubt on ‘the whole edifice of Dumézilian scholarship’. The main argument is that, in

2. From a four-functional perspective, the obvious construal to explore would be: Lug, god of all skills, fourth function (valued); Dagda, god of druidry, first function; Ogmé, the great champion, second function; Dian Cecht, god of health (cf. the Nasatayas), third function; Goibniu, the blacksmith, fourth function (devalued). (For a sample of high-class work by a Dumézilian Celtist, see Storckz 1992.)
construing Indian and Celtic social stratification as descending from a common origin, Dumézil attributes to proto-Indo-European society a degree of stratification that, even at the later date, and *a fortiori* earlier, is archaeologically impossible. But Renfrew misreads the victim of his attack. Certainly, for Dumézil, the proto-Indo-European speakers had a conception of their social structure as founded on the distinctness and ranking of the three functions, but he was carefully and explicitly agnostic as to the concrete form or forms in which the conception was realised. One possibility among others was that the trifunctional pattern applied only to certain clans or families that specialized respectively in one of the three functions, while the mass of society was unspecialized (Dumézil 1958: 18). Renfrew cites the page but apparently stopped reading at line four. He also thinks that Dumézil needed to offer a concrete picture of the proto-society (Renfrew 1987: 245). But he was under no such compulsion. Though well aware of debates about the Urheimat, Dumézil saw them, rightly, as inaccessible to the methods he was using and essentially irrelevant to his problems (Dumézil 1958: 5).

Renfrew has other objections. Appreciating that the essence of the approach lies in seeing that A–B–C in one context resembles a–b–c in another, he doubts whether the similarities proposed are always persuasive and expresses doubts similar to those expressed by Ernest Gellner (1982) concerning Lévi-Straussian binary analyses (where the similarities would have the form: A–B resembles a–b). Of course, as we have seen, the Dumézilian literature contains a proportion of unconvincing trifunctional analyses, just as the Lévi-Straussian literature contains unconvincing binary ones. But undiscriminating doubts are valueless—the battle needs to be fought at the level of particulars and, unfortunately, non-comparativist specialists cannot always be relied on. Renfrew cites the Scandinavianist Page (1978–9), no doubt unaware that Page’s objections were answered at length (Dumézil 1985: 259–98).

Renfrew concludes that in so far as they are real, the similarities Dumézil finds are to be explained not by common origins but by a combination of coincidence, parallel development or global human proclivities (of a Lévi-Straussian nature), and that however rich they were linguistically, the proto-Indo-Europeans were probably ‘culturally rather simple’ (Renfrew 1987: 273). Anyone who can think this simply has not come to grips with the volume and, above all, the interconnectedness of the evidence. It is understandable that an archaeologist should be tempted to exaggerate the possibilities of his own discipline, but as regards ‘cultural simplicity’ it is, I think, more for archaeology to accommodate its interpretations to the facts established by comparativism, than vice versa.

Renfrew’s archaeological objection is repeated by D’yakonov in his brief ‘excursus’ on Dumézil (1990: 111–13). This senior and learned Russian philologist also maintains that Dumézil overemphasized the significance of language families, was wrong to limit himself to three functions (several others are suggested, in passing!), and sometimes made tenuous rapprochements. The third charge is true enough, and Dumézil was sometimes the first to criticize and discard his own proposals; the second shows a deep misunderstanding of Dumézil’s sense of
pattern or structure. The first raises a more useful point. A priori, it is entirely natural and reasonable to look for broader forms of comparativism that transcend the language family, and this is one of the things Dumézil was doing in his Frazerian phase during the 1920s. It would be perfectly reasonable also to work on a smaller scale, confining oneself for instance to the Indo-Iranian branch of the language family. However, as it turns out, the Indo-European family as a whole has, so far, offered a framework within which cultural comparison is particularly rewarding.

I should like to close with a brief discussion of a book that does not mention Dumézil but relates to this issue of broader-scale comparativism. Possibly the most influential paradigm for approaching myth (or ideology, cosmology, belief system...) is still Lévi-Strauss’s, and although in his foreword Sahlins optimistically refers to his former doctoral student as ‘initiating a paradigm shift’, Schrempp is essentially Lévi-Straussian. He starts from a sense of affinity between Zeno’s paradoxes concerning space or movement and certain tribal creation myths retailed by Lévi-Strauss, in which an original continuum gives way to discreteness when some primordial element is deleted. This leads him, via the Great Chain of Being, to the Maori—for his doctorate he worked in Auckland, particularly on the Maori language. In the narrative mode of Maori cosmogony, Tāwhiri, the Wind, seems to mediate between discreteness and continuity (p. 78f.). But the Maori also have genealogical cosmologies, and Schrempp associates this dualism with Kant’s antinomies, allegedly unduly neglected by Durkheim. Finally he returns to Achilles and the tortoise by looking at some foot-races in North American myths.

A bare summary of themes cannot do justice to the sheer ingenuity of the argument, but one has to ask what it achieves. Dumézil and Lévi-Strauss helped each other in their careers, and greatly respected each other’s work (cf. Eribon 1992: 329ff.); Dumézil held that there was room for both approaches and deprecated attempts to award primacy to one or the other. However, unless individuals can think up totally new approaches to myth they have to choose between those on the market, and it seems to me even harder for Lévi-Straussians than it is for Dumézilians to attain results that will endure and that can be built on by others. Polynesia must possess a common cultural heritage—indeed Schrempp refers to a ‘shared inventory of mythological themes’ (p. 71). Would it not have been more useful in the long term to explore this historically and geographically specific and coherent body of material than to work at that lofty level of abstraction where Bororo, Greek and Maori face identical cognitive problems?

3. Compare the work of Lyle (1990) which, imaginative and adventurous though it is, is unpersuasive in its attempt to build Dumézil’s functions into an ideological schema applicable to most archaic old-world cultures.
One wonders if today’s world-wide comparativism will fare any better than Frazer’s has.

Debates about Dumézil will certainly continue. The notion that alongside their grammar and lexicon the Indo-Europeans transmitted a body of ideas is so obviously reasonable that it can hardly fail to go on inspiring research. Perhaps culture changes so much faster than language (or did in this case) that all such attempts will collapse; or perhaps the (approximately) fifteen volumes of the mature Dumézil will one day be subjected to a critique so devastating that nothing of substance remains; but neither supposition looks plausible. Ideally the debate will steer a middle way between blanket endorsement of the mature views (which would be wholly contrary to Dumézil’s own example of self-criticism), and equally undiscriminating rejection: and ideally it will be equally scrupulous in its treatment of what Dumézil says and what the sources say. Revisions will certainly be needed, but the three functions, more or less in their present form, are scarcely vulnerable. On the other hand, the social organization of knowledge (with its disciplines, departments and careers) will make for difficulties, and to wear down the opposition to Dumézil may take even longer than it did to wear down the opposition to the theory of Continental Drift.

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VALUES AND HIERARCHY
IN THE STUDY OF BALINESE FORMS OF LIFE:
ON THE APPLICABILITY OF DUMONTIAN PERSPECTIVES
AND THE RANKING OF CONTEXTS

ANDREW DUFF-COOPER

Elèves, I salute you! . . .
Continue your annotations, continue your questionings.

Walt Whitman

I

Dumont's *Homo Hierarchicus* (1966; transl. e.g. 1980) is about India and the system of castes. It has, however, been employed over the past fifteen years or so to discuss values and hierarchy in many forms of life outside the subcontinent. Dumont’s theory of hierarchical opposition and the ‘encompassment of the contrary’ (henceforth, Dumont’s theory) has been adduced to explicate forms of

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life in Africa and South America, in the Rif (Jamous 1981), in Melanesia (e.g. de Coppet 1978, Iteanu 1983) and in eastern Indonesia (e.g. Barraud 1979, Platenkamp 1988), as well as in the comparison of Melanesian and other ideologies (Barraud, De Coppet, Iteanu and Jamous 1984) and of Indonesian ideologies (Barraud and Platenkamp 1990). Most recently, the following have appeared: an article about the Lio of Flores (Howell 1989); two consecutive issues of the Leiden journal, *Bijdragen tot de Taal-, Land- en Volkenkunde*, edited by Barraud and Platenkamp (Vol. CXLV, no. 4, and Vol. CXLVI, no. 1); an essay by Barraud (1990a); and a special double issue of *Etnos* (Vol. LV, nos. 3-4), edited by Howell, including essays by de Coppet (1990), Iteanu (1990), Barraud (1990b) and Howell (1990b). All these publications include more or less laudatory references to Dumont’s theory, demonstrating that, in spite of the many difficulties that have been seen with it, both middle-career and younger social anthropologists take Dumont’s theory of hierarchized idea-values to be a helpful heuristic resource (and perhaps more) in the analysis of varied forms of life from different parts of the world.

Dumont’s influence has been less marked and direct on studies about Balinese forms of life. Boon (e.g. 1977: 92, 120, 148-9; 1990: 137, 161-8 *passim*) refers to Dumont’s work, but generally either to illustrate a point that he himself wants to make that has no direct bearing on Dumont’s theory, or to take Dumont’s part against such of his critics as Appadurai (e.g. 1988). Clifford Geertz acknowledges (1980: 238 n., 125-31, 239) that his views about Indian Brahmins and kings, as well as his developing line of thought, derive from Dumont’s ‘seminal work’ in *Homo Hierarchicus* and elsewhere. Such other writers about Balinese forms of life as Charras, Gerdin, Hinzler, Angela Hobart, Mark Hobart, Hooykaas, Ramseyer and Schaaereman do not invoke Dumont’s theory to explicate their materials. I have repudiated (1987: 196) an earlier reference to it (1985a: 245) as being unnecessary; while Howe (1989: 53 n.3) ‘on the whole’ accepts the criticism of *Homo Hierarchicus* that it overemphasizes one set of values (hierarchy) and ‘the fixed and static nature of the “traditional” caste system’.

The contrast between the employment of Dumont’s theory demonstrated above and its almost total disregard by writers about the Balinese is arresting. Since at least 1914, Bali has repeatedly been asserted or argued to bear marked resemblances to, or to be comparable with, forms of life both of the Pacific (e.g. van Kol 1914: 343; Chegaray 1955: 196-8; Boon 1977: 111, 238 n.6) and of eastern Indonesia (e.g. Esser 1938: map 9; Schaaereman 1986: 142; Boon 1990: chs. 5, 6; Duff-Cooper 1990a: ch. 2); while its system of the four estates (*catur bangsa*), under some aspects, is rather akin to representations of the Indian caste system. Perhaps the contrast is, at least partly, explicable by writers about the Balinese not

1. Guermonprez (1987: 211 n. 14) is therefore *inexact* in ascribing to Boon the distinction of being the first in Balinese studies to draw the parallel. But perhaps Guermonprez has a restricted understanding of ‘Balinese studies’, since he credits (ibid., ix) their real inception to the Geertzes.
having had the close institutional contact that forms part of the professional biographies of very many of those who espouse Dumont's theory in their work. By 'institutional' is meant the research team Équipe de Recherche de l'Anthropologie Sociale: Morphologie, Échanges (Erasmus), of the CNRS in Paris, of which Dumont was the founding director and de Coppet is the present director, and to which Dumont seems still to be very close.

One writer about an aspect of Balinese life, though, has had contact with Erasmus, and he, Guermanprez, indeed espouses Dumont's theory, in a conclusion to his book about the Balinese Pandé that, one understands (Guermonprez 1987: 198), was the only part of his thesis (1984) to be rewritten before it went to the printers. In section II below I outline parts of Guermanprez's new conclusion, in which 'the 'discovery' in Bali of Hierarchy in Louis Dumont's sense' (see Guermanprez 1987: xiii) is explicated. In section III I consider how far Guermanprez follows Dumont's theory, and whether and in what regards it was novel (see ibid.: 200) and useful for him to do so. In section IV I address the question, raised by the matters addressed in sections II and III: 'Are (Balinese) contexts ranked?'

II

Guermonprez's first conclusion to his study about the Pandé employed the notion of 'social space' borrowed from Condominas. This notion, though, suggests a multi-dimensional space without levels and underestimates the social fact that Balinese society is also an ordered, hierarchicized whole, in line with Dumont's assertion (1966: 14) that hierarchy is the fundamental social principle (Guermonprez 1987: 199, xiii).

In Guermanprez's view, the Maussian total social fact that is significant above all others in Balinese society is the temple. Furthermore, it is the only obvious order of the society that does not evince such immediately observable structural features as the prestations and counterprestations and the exchange of women by men that are manifest in many eastern Indonesian societies (ibid.: 200). However, to appreciate the significance of temples it is necessary to recognize that religious values impose themselves in all Balinese relationships and that they define a level and an order that are not reducible to one component, among others, of the social system (ibid.: 199).

Guermonprez, then, introduces the notion of hierarchy as the order that (necessarily) results from the putting into play of value (Dumont 1983a: 263). And he explains that the relationship to gods and ancestors (a totalizing expression, like gods/ancestors, justified by the way in which shrines, everywhere in Bali, always refer to a founding ancestral core and to what can be called the gods of the universe, including such elements of the geography of the island as mountains and
lakes (Guernonpréz 1987: 202)) is absolutely central to an understanding of Bali. It is the pivot around which are ordered the values of the society and the levels that follow from them. With the idea of gods/ancestors is associated a value. The putting into play of this produces an order, hierarchy and levels (ibid.: 199-200). Thus there is a level of (Guernonpréz calls it) the autochthonous, to which kinship and the village belong. This level is hierarchically integrated into the ultimate whole of the universe by its encompassment by the society of castes. At the former level, the idea-value, gods/ancestors, is put into play with an emphasis on ancestors; at the latter, the idea-value is also put into play, but the basic idea-value is separated into two poles: one, an ancestral pole, Majapahit; the other, a more explicitly divine pole, Siwa (ibid.: 203). The castes (bangsa), understood as the encompassing unities of concrete kin groups, derive from Majapahit. This is the master symbol upon which is based a totalizing theory of Balinese society as a unified social body of which the four castes are the hierarchically articulated limbs. But there also exist groups known as soroh, the members of which are all taken by their classification as such to be symmetrically related. Soroh (and the people who comprise them qua their constituent members) simply differ one from another, as mangoes, say, are all alike and just differ from papaya, which are also all alike (ibid.: 54). Soroh stand at the summit of the order of kinship. Hierarchy, though, imposes itself on soroh through titles. From soroh to bangsa—from a whole in itself, a Leibnizian monad, to elements only conceivable in their relationships to an encompassing whole—is the move from the level of the autochthonous to that of the castes. There is, moreover, a tension between soroh and bangsa that has a cosmic dimension to which the polemic (discussed earlier in Guernonpréz’s book) over holy water (tirtha) draws attention (ibid.: 203-5).

Holy water (ibid.: 207) gives direct or indirect access to the universal whole. Pedanda, Brahmana priests, who in their daily meditation called Suryaséwana make holy water and who, of course, are at the head of the society of castes, evidence the ultimate encompassing order of which the god Siwa (pedanda may also be called siwa) is the pre-eminent figure, the second master symbol subsuming all the levels of the gods/ancestors idea-value (ibid.: 205). So origin-points (kawitan), which can be symbolized as house compound and other group temples, culminate in Majapahit, while Siwa, the apical deity, is the integral figure of all the gods/ancestors. At this point, Guernonpréz introduces the lotus seat (padmasana), the place of the god Siwa as Surya, the sun god, usually located in the north-east corner of temples—the strategic angle where the shrines of the ancestors, which have ‘the head to the mountain’, and those of the gods, which have ‘the head to the rising sun’, converge. This seat objectifies the universal whole that designates the level of the society of castes (ibid.: 204).

The holy water that pedanda make is superior to that had from the gods/ancestors in temples. Pedanda are the purest and most direct descendants of Majapahit. They have access to the highest level of the values of the society of castes. Similarly, the holy water they make (or rather, that Siwa makes through them) has a universal value; that made by the gods/ancestors in temples does not.
But these two kinds of holy water are not incompatible; they are superposable, just as the ritual of the Brahminical priest can be superposed on that which the pemangku, the priest of a temple who is usually Sudra, orchestrates (ibid.: 205-7).

At the level of the society of castes, Siwa integrates the whole, gods/ancestors. From here come the world and holy water of the highest quality, a truly nourishing liquid. Below this level is found the deified ancestral core of Majapahit, constituted by the Javanese mpu, semi-divine beings who engendered the diversity of ‘the people of Majapahit’ (wong Majapahit) hierarchicized in castes and title groups, and from where derives in particular the division between the Triwangsa (the three finest ‘castes’: Brahmana, Ksatrya, Vésia) and Sudra. This level exposes and orders the encompassment of humankind in the superior whole of the society of castes. Similarly, the idea-value Siwa exposes and orders the encompassment of the society of castes in the ultimate whole of the universe. Both Siwa and Majapahit are origin-points, but at different levels. The idea-value gods/ancestors traverses Balinese society from the domestic temples legitimating houses to the construction of the highest sky where Siwa is found; and it allows more and more inclusive levels to be distinguished. Three principal levels may be distinguished thus in Balinese ideology: the level of the autochthonous, that of the society of castes, and the level of the universe or cosmos (ibid.: 208).

III

Anyone familiar with Dumont’s theory will grant that Guermonprez appears to be very faithful to it. Thus he stresses that Balinese society is a unity (1987: 199, cf. viii, ix, x, 66 n.22), a whole, just as Dumont emphasizes that ‘sociologically, India is one’ (e.g. 1957: 9, original emphasis; 1983b: 106). Of course, the wholeness is a precondition for the application of Dumont’s theory (Dumont 1978: 105). The whole (globalité) is ordered and hierarchical (Guermonprez 1987: 199). This order results from ‘the putting into play of value’ (Guermonprez 1987: 199, quoting Dumont 1983a: 263).

Hierarchy, in Dumont’s sense, consists of ‘two contradictory aspects of different level: distinction within an identity [and] encompassment of the contrary’ (Dumont 1983a: 263; see also 1980: 239). The part, that is, is identical to the whole, and the part is not only the whole which is not necessarily a part: A is evaluated as superior to B, and A stands for and includes B. An example from Guermonprez’s conclusion is as follows. The gods (ancestors) in a compound temple are identical, at the autochthonous level, with the apical god Siwa, but they are not only Siwa, and Siwa is not necessarily the gods in a compound temple. Siwa is valued as superior to the gods in a compound temple, and Siwa includes and stands for them. Siwa is positioned at the level of the total society and the cosmos. Between this level and that of the autochthonous there is the level of the
society of castes. Majapahit stands in a relation to this and to the autochthonous level that is analogous to the relation of Siwa to them. The relations, though, hold at different levels. These levels are created by the putting into play of the idea-value gods/ancestors.

Towards the end of his conclusion, Guermonprez gives various reasons why the ‘discovery’ in Bali of hierarchy in Dumont’s sense is important. Among these are that it permits the identification of idea-values and that these, Guermonprez suggests (1987: 210, 211), will release Bali from being a sociological curiosity, and will also introduce Bali to a comparative dialogue both with India and the Pacific and with societies in eastern Indonesia, from which it has hitherto been excluded. Things seem not to have turned out as Guermonprez hoped, however. While, for instance, both Barraud (1990b: 216) and de Coppet (1990: 149) refer to studies that reveal a more or less clear or explicit appreciation of the heuristic value of Dumont’s theory, neither Guermonprez’s study in general nor his conclusion (nor any of his other publications) are mentioned by them, nor are they cited in any of the post-1987 articles mentioned in section I above. When Bali is mentioned, a piece by Howe is referred to.

This ignoring of the only interpretation of aspects of Balinese ideology in terms of Dumont’s theory might seem odd, were it not that in three main respects (at least), Guermonprez renders his conclusion in a way that makes it unlikely to appeal to such writers as de Coppet and Barraud (and Platenkamp). First, Guermonprez employs an etic idiom when writing about his materials, by employing, without qualification, such categories as ‘kinship’, ‘caste’, ‘history’ and ‘religion’. The stance that this employment evinces is unlikely in the extreme to be acceptable to members of Erasmne, or their associates, who are extremely careful in their use of such pairs of labels as ‘sacred/profane’ and ‘soul/body’ (see e.g. Iteanu 1990, Barraud 1990b). Indeed, one has written: ‘every society possesses a different conception of what is real and consequently develops a different and probably unique categorisation of its experience; how then can we understand society by imposing on it the Western conceptions of reality and Western society’s system of values?’ (de Coppet 1981: 198); to which is footnoted: ‘which categorises observed phenomena as “psychological”, “political”, “economic”’ (ibid.: 202 n.15).

Secondly, in spite of Guermonprez’s employment of the idiom of Dumont’s theory, and especially its references to levels, the division of Balinese ideology into three principal levels is not accompanied by the description and analysis of any reversals or inversions. These operations, which are largely equivalents (Needham 1983: 96), are inseparable from the notion of levels—Dumont has it (1982: 224) that the ranking of idea-values ‘includes reversal as one of its properties’—and they are evidences of changes of levels (Dumont 1978: 106; cf. de Coppet 1981: 198-9, 202 n.16). Any analysis that employs Dumont’s theory with its references to levels but does not point to and then analyse reversals (inversions) as indicating changes of levels must, it would seem, be unfaithful to
a main plank of the theory. Perhaps Barraud (and Platenkamp) and other members of Erasme have taken the same view. 2

Finally, in spite of the idiom that Guerononprez adopts, the division of Balinese life into three is in fact rather conventional. It was adopted by Korn in Het Adatrecht van Bali (1932), still ‘a standard work for every researcher concerned with (the) Balinese’ (Schaarreman 1986: 1). In Korn’s view (1932: 76-9, 227-8), Bali consisted of an ‘Old Balinese’ region and an ‘apanage’ region, the latter being divided into two zones. Later, Howe (1989), in a remodelling of Korn’s thesis, divides the island into three contiguous zones: the mountains, an intermediary zone and the southern plains zone.

This latter attempt to classify forms of Balinese social organization is, of course, subsequent to Guerononprez’s and at points it relies on the latter, though not on his rendering of his materials into the terms of Dumont’s theory. But both are conventional in this sense: from at least the 1880s until the recent past (see e.g. Jacobs 1883: 52; Hilda Geertz and Clifford Geertz 1975: 198; Clifford Geertz 1980: 45-7), writers have divided Bali into a Bali Aga (‘Original’ Balinese) and a ‘Hindu–Bali’ zone. Korn (1932: 2-3) had already criticized this overly simple division; and, if it was not inevitable, it is not very surprising that when he (and later Guerononprez and Howe) should have reconsidered it, because it was unsatisfactory, the division should have been expanded to a tripartition, the next more numerically complex mode of classification by division. Korn splits one of his two basic components into two. Howe interposes a medial type, that combines attributes of the original two, between them. Each is a familiar way of expanding a bi- into a tri-partition (Hocart 1970: 289; Needham 1979: 9). Guerononprez does not adopt either of these strategies: he adds a level, that of the cosmos, to his two more basic principal levels. This approach is dictated by Dumont’s theory, as explicated by, for instance, de Coppet: ‘the whole, to which all social actions are geared, is society and universe combined’; ‘the cosmos... cannot [should not?] be separated from the society on which it relies for its existence’ (1990: 148). Or, as Barraud (1990b: 216) suggests, a non-modern society is a ‘socio-cosmic whole’. 3

The strategy might also be thought consonant with the view that many writers about Balinese ideology have held to, namely, that it is a whole every aspect of which is pervaded by ‘religious values’.

2. In fairness it should be said that Guerononprez (1987: 198, cf. 210-11) starts by saying that his ‘new’ conclusion is an introduction to further studies. It may be, also, that the refusal of Pandé to accept holy water from pedanda while accepting the supremacy of the god Siwa and using the Brahmanical ritual to make it for themselves, thus annulling an essential hierarchical relationship, is a reversal (ibid.: 206), perhaps Needham’s type 12, ‘abstention from normal practice’ (1983: 100, 116). But it seems not to evidence a change of level, or at least Guerononprez does not say it does.

3. The distinction between ‘modern’ and ‘non-modern’ is from Dumont (e.g. 1982: passim). On difficulties with this aspect of Dumont’s theory, see e.g. Needham 1987: 104-6.
I do not want to disparage these schemes because they employ tripartition. But although triality is a very common mode of classification by division in Balinese ideology, it is far from being the only or indeed the main one. Bipartition is also common and important, and so, more or less, are the divisions of an entity into from four to eleven constituent aspects. These aspects may be related one to another in various ways. They may be related symmetrically and/or asymmetrically, and by one of the modes of reflexivity and of transitivity. These may be dynamically expressed as periodic modes, and they may be reversed (inverted). Past analyses (see references in note 4, especially 1987a) show that these principles in different combinations order aspects of the contexts of Balinese ideology. The same combinations are evinced in different contexts, and different combinations are evinced by the same contexts.

Mention of contexts brings us to a further, and for the present purpose the last, major divergence between Guermonprez and Dumont’s theory. Although Dumont (1978: 108 n.23) alludes to a ‘variety of contexts and the conceptual differences that go with them’, he later (1982: 225) came down heavily against contexts, because ‘they are foreseen, inscribed or implied in the ideology [under study] itself’, his preference being for ‘different “levels” hierarchicized together with the corresponding entities’. None the less, Guermonprez refers to ‘registres ou...contextes’, i.e. ‘levels or...contexts’ (1987: 208). This is not permissible within Dumont’s theory. But Guermonprez’s elision of levels and contexts does make his reference (ibid.: 208) to levels that are ‘more and more inclusive’ intelligible. Levels, in Dumont’s theory, are strata-like, standing one above another like the strata of a geological formation (see Needham 1987: 185, 144), or they are positions within a hierarchy. Neither strata nor such positions can have extension, and thus levels cannot be ‘inclusive’ to any degree. But contexts, of course, do so and may be. Guermonprez’s equation of levels and contexts also lends support to the view that ‘“level” stands for nothing in particular [in Tcherkézoff 1983]. Whenever the word is used, in relation to ethnographic data, it can be replaced by “context”’ (ibid.: 182).

Howell mentions (1990a: 137-8) that the members of Erasme maintain that ‘complementary oppositions’—a phrase that has ‘a more or less adventitious use as a term of expository rhetoric’, but not one that is ‘suitable for employment in comparative analysis’ (Needham 1987: 101; see also Duff-Cooper 1991a)—‘can always be replaced by hierarchical oppositions whereby one of the pair stands for the whole and thereby encompasses its opposite’. This, though, is an empirical and not a theoretical matter. Recourse to Balinese ideology and the opposites

4. For definitions of these relations and for discussion of their places in Balinese ideology see respectively e.g. Duff-Cooper 1985b and 1991a; 1988a; 1990a: ch. 4; 1986a; 1986b.

5. I have elsewhere (1990d) suggested that these combinations of principles are probably the most satisfactory criteria by which to order forms of Balinese life, and I have done some of the preparatory work (1991c). The method is not new; in its essentials it is that proposed by Lowie (1917).
sun/moon and high/low, for instance, in which neither 'sun' nor 'high' can stand for the wholes 'sun/moon' and 'high/low' and therefore does not encompass its opposite, shows that what the members of Erasme maintain, as Howell describes it, is wrong. However, Barraud and Piatenkamp (1990) have analysed comparatively the rituals of three of the societies written about in the issues of *Bijdragen tot de Taal, Land- en Volkenkunde* they edited, by authors who do not refer to Dumont’s theory, and say (ibid.: 106) that although their analyses 'do not always coincide with those by the authors themselves', who [it might be thought consequential] collected the data they analyse, they 'do not question their [the original authors'] interpretation'. My attitude to Gueronprez's second conclusion is similar: I do not question his interpretation, which of course was written from a particular point of view, that of Dumont's theory. Allowing that it can be given an exact meaning, a point of view cannot be wrong. I do maintain, though, that it is unnecessary to invoke Dumont's theory to understand Balinese ideology, especially when Gueronprez's apparent attitude to holistic analysis is to disregard in his analysis almost everything that constitutes the whole as a way of life for Balinese people.

Had he paid more analytical attention to these other aspects, which include sex, rice-growing, and eating and defecation, among many more, he would perhaps have discovered that there is not one representation, physical or ideational, that is conventional among the Balinese that looks anything like encompassment (as depicted in Dumont's second figure (1980: 242) of a rectangle in the centre of which there is a second rectangle): whenever pairs of opposites are represented, they are juxtaposed (like the half-male, right side, and half-female, left side, icon Ardhanārīśvara (and see also e.g. Hooykaas 1973: 232-3, 236)) as depicted in Dumont's first figure of a rectangle cut vertically into two.6

Furthermore, while Gueronprez suggests, in line with many writers about the Balinese, that 'religious values' pervade their lives, his view of these values is very impoverished. *Everything* that constitutes Balinese ideology comes from the gods, and *everything*, and not just what a particular sociological theory suggests, is to be

6. The first figure is supposed to represent complementary opposition, though the equality of the halves into which the rectangle is divided subverts it: most writers about such opposites maintain that they are unequally valued (e.g. Barnes 1985: 14). *Pipis bolong*, old Chinese coins with a hole at the centre that are used in Balinese offerings look somewhat like a Venn diagram, which is ideally suited to illustrate encompassment (Needham 1987: 134, fig. 2). But these are Chinese coins, and China is precisely an example of a society where 'encompassment of the contrary' is not the kind of opposition evinced (Dumont 1978: 108). This seeming divergence can be explained in at least two ways: representations like these coins do not reflect the kind of opposition the culture they derive from employs, or perhaps the interpretation of the form of the coins here is faulty. Given Leenhardt's authoritative insistence (1975: xv) on the help that the plastic arts of a people can give in understanding their form(s) of life, I should say that the coins do not represent encompassment, which does not appear in China. Similarly, they do not so in Balinese ideology which, to labour the point perhaps, seems not to discriminate this kind of opposition.
interpreted by reference to those values. Different writers have done this in different and more or less compellingly cogent ways. My attitude has been, and remains, that because everything derives from, is pervaded by, and is contained in Ida Sang Hyang Vidhi, the high or highest Balinese god, everything is to be interpreted by reference to Vidhi in various guises. Because of this derivation, and because everything is therefore pervaded by Vidhi, Balinese ideology, I have often maintained, is a sacred whole of which every aspect is ritual; or, it is a whole that is, so to say, taken for granted by Balinese in the middle world (madyapada), the realm of material human beings, and completely ordinary (cf. Jeremy and Robinson 1989: 185). Either way, it can be seen as consisting in various contexts. This is expectable: the strategy or style of ‘contextualizing’ (nganutang, literally ‘fitting’) is not ‘an analyst’s importation’, but one adopted by Balinese (cf. Hobart 1986b: 151).

Dumont complains (e.g. 1978: 108, cf. 102) that a certain style of symbolic analysis confuses or elides contexts. Needham has shown (1987: 112-19) that Dumont’s complaint, as he expresses it, makes no sense. But perhaps there is a point worth considering, to which we have not been brought (cf. Duff-Cooper 1991a: 58 n.3) but have been further nudge by Guermonprez and his elision of ‘level’ and ‘context’: ‘Are contexts in Balinese ideology ranked?’ Considering this question will once again emphasize a pons asinorum in the study of symbolic classification, namely, ‘the importance of keeping the context...perpetually in view’ (Needham 1973: xxvii). It will also advance understanding of the analysis of (Balinese) forms of life.

IV

The OED gives various meanings for ‘context’. The most relevant is: ‘the parts which immediately precede or follow (something) and determine its meaning’. More generally, and usefully, I take ‘context’ to be that ‘which environs the subject of our interest and helps by its relevance to explain it’ (Scharffstein 1989: 1).

Mark Hobart has written most extensively about ‘context’ in Balinese ideology, and about the relationships of contexts to meaning and to power (1986b); and he has discussed problems involved with making a theory of context in the light of

7. Guermonprez rejects this possibility with some superior and in places inexact remarks about Vidhi (1987: vi-vii) and about those who take seriously what is said about Vidhi by, for example, Brahmana pedanda and villagers, young and old, ‘educated’ (scolaris) or not. For references to studies that do take seriously what is said, and written, about Vidhi by Balinese people, see Duff-Cooper 1991d. See also Hooykaas (1973: 19): Vidhi is ‘the highest principle’ and ‘a generally used word’. One widely respected writer about aspects of Balinese life equates Siwa (cf. sec. II above) with Vidhi (Ramseyer 1977: 131).
Dumont's theory (Hobart 1985). In the course of the former study, he makes a cogent argument, first, for denying that contexts can be identified as 'marriage', for instance, or as pertaining to the 'village' (désa). Thus he argues that it is arbitrary to conclude that 'one feature of an institution is essential and the others ancillary', especially when 'which feature is to the fore' for Balinese depends upon 'interpretative style, context, and personal perspective' (1986b: 132, 133, 135). Hobart's arguments are even more persuasive when they are read in conjunction with Needham's demonstrations that 'marriage', 'incest' and 'kinship', among others, are 'odd-job' words, 'very handy in all sorts of descriptive sentences, but worse than misleading in comparison and of no real use at all in analysis' (1974: ch. 1, esp. 44); and with Hocart's assessment (1952: 25) that the result of forcing the customs of alien forms of life into the familiar categories of religion, the state, the family, medicine, and such like, has been 'disastrous'.

Secondly, Hobart makes the persuasive case, after an examination of a situation that arose where he was doing fieldwork, that 'hierarchies were referred to.... All [involved] seemed to operate on the assumption that a correct hierarchy existed.... But...hierarchy did not exist as a fixed system of reference; various elements in it were variably invoked to interpret the situation' (1986b: 138). Let us concentrate here on the report that all involved seemed to operate on the assumption that a correct hierarchy existed. Adopting this line (in accord with Traube's remark (1986: 93) that Mambai do not hold to a uniform dogma about conception and birth, but that 'what individuals share are certain classificatory principles, from which they construct variant representations') will allow us to make headway with answering the question posed at the end of section III.

In an essay entitled 'Language Strata', Waismann (1968: 119, 120) says that we can look at things, pragmatically speaking, 'from within' by 'formalizing' the enquiry. The approach is akin to that of Needham (e.g. 1978: 57) in his analysis, to begin with, of myths: 'we are not after types, but properties. If we can isolate significant features, then we shall try to see what they are properties of.' It is also consonant with the approach adopted in my analysis of Balinese rice-growing, and less explicitly in other studies, where the definition of 'aspect' adopted is, "the way in which the planets, from their relative positions, look upon each other," not, it should be emphasized, "their joint look upon the earth" (Duff-Cooper 1989: 127). That is, it is probably preferable not to try to isolate this, that, and other contexts and to establish their relative evaluation, which would always be open to dispute by Balinese and non-Balinese alike. I suggest, rather, that we return to the findings of analyses of social facts that constitute 'aspects', as just defined, of Balinese ideology. These social facts were not defined in advance as pertaining to this or that context. They were adduced as was found appropriate to establish the associative settings (Scharfstein's 'environs') of the matters being analysed. These were defined, in a blurred way, by employing odd-job words.

The principles mentioned above (sec. III) that order Balinese ideology all derive from Vidhī—'Divinity as order, what orders' (Hobart 1986c: 11; cf. Swellengrebel 1960: 53)—constructible in the guise of Sunya, the Void, as perfect
bilateral symmetry. Here, the two constituent aspects of Vidhi are identical one with another, and with the whole that they constitute (Duff-Cooper 1990c). Vidhi as Sunya also evinces perfectly reflexive and transitive relations. Other combinations have been arrived at through other analyses. These combinations have all been ranked (Duff-Cooper 1991c) by various criteria, among which are, summarily, (1) what more closely approximates Vidhi is evaluated as superior to what does so less closely; (2) a negated principle (e.g. irreflexivity, intransitivity) is evaluated as inferior to the principle of which it is a negation; (3) these negated principles are evaluated as superior to the third mode of each: the former implicate necessity, the latter only contingency; 8 and (4) the modes of transitivity are prior, for aesthetic reasons, to the modes of reflexivity. 9 The resulting ranking is shown in Table 1, which leaves out, because they are not directly germane to the present exercise, the concatenating relations, analogy and homology, and the operations—dynamic modes and reversal/inversion—that may be performed on the social facts in which the combinations were discerned (the latter operations, incidentally, not indicating changes of level or anything of that kind but contexts where misfortunes and/or deaths (see e.g. Weck 1937: 42-3) predominate).

<table>
<thead>
<tr>
<th>modes of partition:</th>
<th>division of an entity into from two to eleven aspects</th>
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<tr>
<td>relations in combination:</td>
<td>perfectly symmetrical, totally reflexive, transitive</td>
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<td></td>
<td>symmetrical, reflexive, transitive</td>
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<td></td>
<td>symmetrical, reflexive, none</td>
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<td></td>
<td>symmetrical, non-reflexive, in- or non-transitive</td>
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<td>asymmetrical, irreflexive, intransitive</td>
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<td>asymmetrical, reflexive, intransitive</td>
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8. Vidhi is also Sang Hyang Tuduh; ‘tuduh’ meaning ‘what fate brings, lot, destiny’ (Swellengrebel 1960: 52).

9. In an earlier study (Duff-Cooper 1990b: 293), the modes of reflexivity are judged prior to the modes of transitivity, on the ground that ‘a mode of reflexivity is built into “simple” duality while the modes of transitivity necessarily implicate three entities’. This divergence shows that even when an analysis employs such notions as these principles, which have a surety that substantive matters of social fact generally do not possess, context can have a marked impact on the decisions the analyst makes and the findings (if any) to which s/he comes.
These combinations of principles, and, it may be presumed, others still to be discerned, give Balinese ideology its order. By 'order' is meant that aspects of Balinese ideology correspond more or less closely to Vidhi. They also render the present and earlier representations of it in order, in the sense that they attain their envisaged ends. These are: first, an enhanced appreciation of Balinese ideology as a totality; second, a way of comparing aspects of Balinese life with aspects of such other forms of life as the Japanese (e.g. Yoshida and Duff-Cooper 1989; Duff-Cooper 1992); and third, to pave the way for an ordering of Balinese forms of life anywhere (and, indeed, at any time). I have suggested, also, that 'the appearance and reappearance of these principles determine the texture (as it were) of each aspect of Balinese ideology' (Duff-Cooper 1991: 201). A question with which to conclude the present section should now be considered: 'What is meant by “texture”?'

It is tempting, in trying to answer this question, to try to construct a typology by reference, say, to a combination, or to combinations, of principles and the types of situation, e.g. harmonious or acrimonious, in which they are discernible. This approach, though, would at once subvert the formality of the method adopted and would push us back towards definition, with all its attendant disputability, and the questions of power that such disputes implicate. It is therefore preferable to look at things as follows. An analyst, for his or her own purposes, more or less imprecisely defines an aspect of, say, Balinese ideology and its 'environs' to consider. As Frake puts it (1969: 132), when the appropriate changes have been made, 'there is no one way to separate the conceptual structure of a people into a finite number of discrete, clearly delimited domains'. Rather, and for preference taking our directions from Balinese people who know what they are talking about, we move about the form of life by a variety of paths (as it were), that are all more or less directly interconnected. In doing so, the analyst who employs the formal notions shown in Table 1, for instance, shines light on the interior of the whole from different angles. Doing so enables him or her to establish the various ways in which order—by which is here meant correspondence in transformation with Vidhi—is procured. What can then be maintained is that the order procured has different aspects, which are themselves finer or coarser approximations to, or which have finer or coarser correspondences with, Vidhi. These aspects can all be brought together and expressed synchronically and diachronically by a threedimensional model (though not simultaneously unless more than one model is made) (see Duff-Cooper 1990b).

None of this precludes other approaches, of course. Indeed, they are to be encouraged because, as Mark Hobart puts it (1985: 50), knowledge is built up from

10. 'Context' and 'texture' are, of course, etymological cognates.

11. The verb 'to argue' is derived from Indo-European *arg-, to be white or bright, so that 'the outcome of a convincing argument is a proposition that throws light on whatever is at issue' (Needham 1983: 19).
a plurality of perspectives; though not any perspective—some are incoherent (e.g. Howe 1989), others are unnecessarily complicating and/or impoverished (e.g. the conclusion to Guermonprez 1987), and others again may bear so distant a relationship to social facts that it is hard to make out what they are supposed to be about (e.g. Boon 1990) (see respectively Duff-Cooper 1990d; sec. III, above; 1991f).

So it can no longer be accepted that the appearance and reappearance of the principles in combinations ‘determine’ the texture of aspects of Balinese life. Rather, they are themselves an aspect of those textures, now understood as the formative elements or constituents of contexts. These, and what for instance Rössier (1990: 293) refers to as ‘specific cultural themes as well as specific patterns in religion, world view, ethos, and norms and values’, which in some cases may be transformations, or the direct employment, of themes and the rest, that have a more or less wide incidence, contribute to us being able to say, ‘this is Balinese, and fine’, for instance, as Balinese people themselves do.12

V

Dumont’s work about India, from which has come what I have been referring to here as Dumont’s theory, is a contribution to the debate about what Appadurai (1986a: 357) refers to as the ‘gatekeeping concepts’ that define the quintessential and dominant questions of interest, such as, in India, hierarchy (ibid.: 360).

Dumont, of course, and according to Howell the Erasme team in general, privileges theory. Such privileging may in general be earmarked by hierarchy and essentiality (Ryan 1982: 130). It is certainly so in Dumont’s work. Not only is it based on the hierarchical relationship between parts and whole, and between one part and its opposite; but his work also distinguishes ‘levels’ of professional social anthropological tendencies, which are implicitly ranked in a hierarchy, conventionally speaking. It is to the highest level of these tendencies—the true nature of anthropology—that he and his congeners in Erasme, we may assume, hold (Dumont 1978: 84-5). Furthermore, it has been suggested that Dumont is personally committed to hierarchy and totality, and that he is ‘clearly against egalitarianism and individuality’ (Needham 1987: 144). Moreover, Dumont

12. Cf. Boon 1990: 210 n.2: “the Balinese” hypostatize themselves (and others’). On many of these formative elements or constituents, see e.g. Duff-Cooper 1988b, 1990c. These analyses, incidentally, show that Hobart’s view that context ‘is not an object but sets of relations’ is too partial. But the relations among the relations and the more substantive things, pragmatically speaking, that combine to form contexts are indeed ‘complex’ (Hobart 1985: 48). On the complexity of the concatenating relations, analogy and homology, in the Balinese case, see Duff-Cooper 1991a.
explicitly states (1980: 219) that he believes that hierarchy is ‘essentially...a relation that can succinctly be called the “encompassing of the contrary”’ (emphasis added).

Appadurai points out that in the study of South Asian society, the tropological hegemony of hierarchy is being loosened in two main ways, namely by ‘explicit critiques’ and by a ‘proliferation of anthropologies’ (1986b: 757)—what Dumont would perhaps refer to as ‘pseudoanthropologies’ or ‘antianthropologies’ (see Dumont 1978: 84-5), subverting the position of ‘the true...anthropology’. The flurry of papers from Erasme, and the special issues of Bijdragen tot de Taal, Land- en Volkenkunde and Ethnos, and the rest, can be seen in part at least as an attempt both to reduce the impact of those proliferating anthropologies on the hegemony of ‘hierarchy’ in South Asia and to extend Erasme’s gatekeeping role to other parts of the world.

Guermonprez’s view of his work on the Balinese Pandé in his second conclusion is an explicit attempt to bring Bali (and of course his work and, it follows, himself) into the comparative arenas of India and eastern Indonesia and the Pacific. This attempt seems so far to have been unsuccessful. But more interesting questions are suggested by Appadurai. While, he grants, most routine ethnographies profit from those ‘summarizing metonyms’ like ‘hierarchy’ that provide a point of orientation for the non-specialist reader, does such an attempt as that of Guermonprez and the reanalyses conducted by Barraud and Platenkamp (1990), for instance, reveal a relatively arbitrary imposition of the whims of anthropological fashion on particular places? Do these impositions ‘cost us more in terms of the richness of our understanding of places than they benefit us in rhetorical or comparative convenience’? (Appadurai 1988: 45; 1986a: 358).

Readers must of course judge and answer these questions for themselves; but I think that my answers to these questions will come as no surprise. In making Bali look like India, as seen through Dumont’s eyes, and like the parts of Melanesia and eastern Indonesia, and elsewhere, that the members of Erasme and their collaborators have studied and/or reanalysed, I do indeed judge that the employment of Dumont’s theory costs more in terms of the possible richness that understanding of Balinese, and other, forms of life can attain, and gives us very little, if any, theoretical and/or comparative benefit in recompense. This paucity, in the main, derives from the theoretical and/or comparative propositions arrived at (e.g. Barraud, de Coppet, Iteau and Jamous 1984: 514-18; Barraud and Plakenkamp 1990: 117-21) being premised upon and couched in the idiom of Dumont’s theory.

Nevertheless, Guermonprez’s employment of Dumont’s theory brought us, again, to consider ‘context’ in anthropological analysis and to the question, ‘Are contexts ranked in Balinese ideology?’ Put this way, the question seemed to ask for an answer through, at first, the construction of a typology. But this, like any typology, would in principle be disputable, and I do not wish to get embroiled in the matters of power which, Mark Hobart has shown (1986a: 15), ‘the definition of context in Bali turns on’. But I do want to try to understand forms of Balinese
life in a way that does not privilege theory and that helps advance along the lines of approach that have been adopted before. The approach through the combination of those principles listed in Table 1 at once evades the problems with definitions and power that a typology of contexts would inevitably bring upon us, combines theory and social facts in such a way that the social facts are privileged, and through the approach 'from within' allows us to see at once both that the question 'Are Balinese contexts ranked?' is ill-framed and just what the relationship of the principles, in combinations, is to the aspects of a Balinese life in which they have been discerned (or by which they are evinced), namely an aspect of the formative elements or constituents of indigenously defined contexts of the form of life.

13. Boon is clearly incorrect when he asserts, in the sentence ending his note cited in note 12 above, that 'typology cannot be escaped'.

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COMMENT

PARKIN'S EVIDENCE

These further and final remarks on Robert Parkin's JASO article (Vol. XXIII, no. 3, pp. 253-62) and on his reply (Vol. XXIV, no. 1, pp. 54-63) to my comment (Vol. XXIV, no. 1, pp. 49-54) will only reinforce my previous points about his form of evidence and add two further short points. More examples of Parkin's methods can be found in his original article and in his recent book (Parkin 1992). Having been accused by Parkin of unclear wording (‘Reply’, p. 55n.), I shall try to avoid all ambiguities.

1. The passage in Parkin’s original article that reads, ‘comparing like with unlike, for example northern address terminologies with southern reference terminologies in Dumont’s attempt (1966) to prove that the former were as classificatory as the latter’ (p. 253) is a misrepresentation. Implicitly and explicitly—'the vocabulary...has no “structure” in the strict sense'; 'we shall...call our system descriptive’—the very opposite was elaborated by Dumont (1966: 96), who anyway withdrew this contribution—'I came to acknowledge that I had made a radical mistake' (Dumont 1975: 198)—seventeen years before Parkin chose to criticize it.

2. Parkin’s thesis depends essentially upon ethnographic data on the Juang marriage system. He writes of ‘the rule of delay of three generations’ (p. 256). This ‘rule’ is Parkin’s invention. Several ethnographers, besides myself in my own primary ethnographic research, have discovered such a rule among other tribes but not among the Juang. Parkin’s constant mixing of ethnographic bits and pieces relating to several quite different tribes does encourage confusion,¹ but in the present case he has explicitly named McDougal (1963, 1964) as his source, even though he carefully avoided a more detailed reference in the original article. None of the references in Parkin’s reply (p. 55) indicates a ‘rule of delay of three generations’, which is supposed to constitute Parkin’s ‘Juang-type system’ in his crucial ‘third stage’ of kinship evolution in South Asia. What exactly did McDougal’s dissertation report?

The fifth chapter, on ‘The Marriage System’, has an initial section headed ‘Marriage Rules and Preferences’, which Parkin has chosen not to consult for his reply. It informs us of two—and only two—such rules: ‘the first rule is that a man must marry a female belonging to a group which his own classifies as bondhu’, i.e. as hereditary affines; ‘the second rule divides the bondhu category into halves, one prescribed and the other prohibited. A man must marry a female belonging to his generation-set—his own generation or an even-numbered one’ (McDougal 1963:

¹. As far as I can see, none of Parkin’s publications contains a formal analysis of a single terminological system as a whole that would be applicable to a particular middle Indian tribe.
154 Comment

155). As examples, the ethnographer explicitly names 'five...kin-types from which a spouse may be taken', and among these are 'na (e.g. FMBSD) and bokosini (e.g. FFZSD)' (ibid.: 157), i.e. members of the affinal group already allied with ego's in the second ascending, the grandparental generation. I am not sure if marriage 'rules of delay' could be stated in any clearer wording, and I fail to understand why Parkin continues to defend his contrafactual account. In the present context it is unimportant whether Juang marriage rules are 'therefore reflected in the terminology', as Parkin (p. 256) pointed out, even though in his reply he could not conceive 'how any sort of reflectionism' could be attributed to him (p. 57).

The Juang do not just exist in written sources, the people themselves can provide evidence of their rules. They can be reached from any European airport within forty-eight hours. The passage and a two-month stay in a Juang village costs less than required by the most modest standard of living in Western Europe. Altogether I have spent eighteen months in the middle Indian hills visiting most of the tribal areas in excursions (1972, 1978, 1980/81, 1982, 1983, 1987, 1988, 1990) lasting between two weeks and six months. Most of this time has been spent in the Phulbani and Koraput Districts of Orissa. On my next middle Indian excursion this coming winter I could visit the Juang in the company of established anthropologists from Bihar, West Bengal or Orissa in order to re-examine—for the third time—the Juang marriage rules as described by McDougall. Parkin must have had good reasons for avoiding travelling to South Asia so far, but he could be very helpful by suggesting the conditions under which he would accept ethnographic data from the primary accounts of other authors.

3. On many occasions Parkin has reified the linguistic category 'Munda' into a social group named 'Munda' (subdivided into such similar subgroups as 'the North Munda' and 'the Koraput Munda'). In 1985 he defined 'the Munda' as a group of tribes living principally in southern Bihar and Orissa, and speaking languages which are unrelated to Dravidian or Indo-European' (Parkin 1985: 705). The accompanying figure (ibid.: 706) introduces 'the Munda language family (after Zide 1969: 412)'. Parkin also complains that 'virtually no comparative work has been done on them as a group outside the realm of linguistics' (ibid.: 705).

The latter statement is correct. Apart from Parkin no anthropologist—not to speak of the tribals themselves—has ever been aware of Parkin's 'group'. Since the pioneering work of Sarat Chandra Roy on The Mundas and their Country (1912), anthropological usage has confined the sociological term 'Munda' to a group of this name, nowadays registered as a 'scheduled tribe' on the lists of several Indian states. Historians and administrators have discussed the well-known actions of this tribe in the nineteenth century, just as innumerable Indian anthropological reports have described the group itself. But middle Indian ethnography is poorly represented in Europe and the USA, and 'Parkin's group' has thus been able to survive in the most respectable journals.

Similar phantom groups have attracted previous scholars. In South Asia 'the Aryans' are the most popular such group. For decades Leach (1990) could not demolish the concept of 'Aryan invasions' some millennia ago. But the people
Parkin classifies as ‘the Munda’ can be personally consulted. The Gadaba of Koraput (who speak a Munda language) would not normally know of the existence of such tribes as the Munda, Santal, Kharia or Juang (who also speak Munda languages) as no form of communication is maintained. But most Gadaba identify themselves as part of the Desya or indigenous people of Koraput, who share totemic and status categories across the tribal boundaries between Jodia, Rona, Kondh and Ollari people, i.e. speakers of Indo-European or Dravidian languages. In the north, the Juang (with a Munda language) are seen as the ‘junior siblings’ of the Bhuiya, who speak an Indo-European language. Similar dual classification across the boundaries of the language family is found elsewhere.

The issue may be illustrated by an analogy. Anyone could define all the speakers of Germanic languages (including Swedish, Flemish and English) as ‘the Germans’ and write books and articles on their ‘social organization’ that would deal with such similar ‘subgroups’ as ‘the Scandinavian Germans’ and ‘the West Germans’ in Belgium, the Netherlands and Britain. Describing such a social universe would imply that, apart from the linguistic commonalities, these ‘Germans’ shared a unique social criterion when compared to, for example, the Finns, Walloons or the Welsh. But I am not so sure that the anthropological public or the people themselves would readily accept such a classification.

4. In both his original article in JASO (p. 259) and in another article in another journal (1990: 73) Parkin has introduced primary ethnographic data without any reference to the ethnographers themselves. In the course of a general, evolutionist argument he has assigned these data to a tribe he called ‘the Malto’ (and nothing else) without further explanations. No other author of any type of literature has ever called this tribe ‘the Malto’.

In his reply Parkin justifies these omissions as if his articles were not meant for a general readership: ‘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’ (p. 56). Following this hint, I purchased a copy of Parkin’s book and found there general references to the ethnographies of Sarkar (1933-4), Vidyarthi (1963), Bainbridge (1907-10) and Verma (1959) (Parkin 1992: 276), but no detailed indication of who was responsible for the specific data. In particular, nobody was identified as the author of the ‘terminology poised between the last stages of prescription and individualizing north Indian’ that ‘the Malto’ were supposed to frequent as representatives of ‘the fourth stage’ in the evolutionary scheme for South Asia.

In 1987 Parkin kindly gave me a copy of a manuscript entitled ‘The Sons of Man: An Account of the Munda Tribes of Central India’. He seems to refer to this work now, but misses my point. I had always known that ‘the Malto’ did not exist, whereas other readers of his articles could hardly evaluate his evidence without being provided with a reference to the primary research and the correct name (Maler or Sauria Pahariya) of the tribe that was supposed to represent Parkin’s ‘fourth stage’. A lack of differentiation between primary, secondary and tertiary research is Parkin’s general problem.
presented in Parkin’s *JASO* article (p. 258). Having thus been forced to examine these sources in detail, the only account I have found that appears to fit is Sarkar’s (1933-4: 257-9), though when compared to Parkin’s table (p. 259), there are minor differences of spelling. The other sources differ or are irrelevant.

Sarkar was a meticulous reporter. His account introduced six different terminologies collected in six different villages among tribals he correctly introduced as ‘the Malers’. The six terminological schemes offered the vocabulary for 54 kin types within each of them. Sarkar also wrote on language—‘the Malers speak Malto’ (1933-4: 151)—but Parkin omits this reference. Parkin also refers to important variations in meaning among five of the eight Malto terms supplied, and attributes them to ‘some dialects’ (p. 259). But neither Sarkar nor any of the other sources inform us about ‘some dialects’. Thus, instead of a reference to Sarkar’s elaborate ethnographic work, Parkin represents the ‘fourth stage’ in the evolution of South Asian kinship by eight terms, of which five have a different meaning in ‘some dialects’ unknown to any of the ethnographers.

It would be helpful if Parkin could correct or confirm my researches on his ethnographic sources. His ambitious hypothesis requires careful study of the data base. Having accused several renowned authors of a ‘uniform reliance on slender and suspect evidence’ (Parkin 1990: 70), he could explain why it was so difficult to detect the sources of his data on ‘the Malto’, and why his selection of eight terms out of Sarkar’s rich and informative ethnographic account should be the only basis of the hypothesized ‘fourth stage’ of the ‘evolutionary paradigm...for south Asia’. I raise these questions because the ‘system’, i.e. the eight terms published by Parkin, is supposed to be ‘especially significant for the overall hypothesis’ (p. 258).

5. A positive aspect of Parkin’s reply is the reunification of his ‘fifth stage’, i.e. ‘the Jat system’ (1992: 258) with his ‘sixth and final stage’, i.e. ‘standard north Indian’ (p. 259). He recognizes now that the ‘four-got rule’, as described by Tiemann (1970), has ‘implications for the dispersal of alliances in north India generally (i.e. not just among the Jat)’ (p. 62).

When I introduced Tiemann’s ethnography in the course of my evolutionary speculations on the dispersal of alliances (Pfeffer 1983: 115; 1985: 179), I could not have anticipated Parkin’s subsequent elaboration of a separate evolutionary stage for the north Indian subcaste named Jat, in contrast to a stage for the other people of north India. I selected Tiemann’s account of the Jat (1970) as a primary source for the ‘four-got rule’, because his work was easily available to German (and other) readers. Had I known the consequences, I would have chosen Blunt (1931: 60-62) or any of the many older, ethnographically more general sources, as I had done before (Pfeffer 1970: 83).

6. Parkin’s hypothesis of terminological evolution in South Asia proposes that prescriptive terminologies give way to cognatic ones. In several steps FB is supposed to ‘switch...from the terminological companionship of father to that of MB’ (p. 254). It would be helpful if Parkin could provide a single South Asian kinship vocabulary containing a ‘terminological companionship’ of FB and MB.
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SECOND REPLY TO PFEFFER

Nothing in Pfeffer's latest set of remarks leads me to withdraw or change what I have said previously, though obviously some further clarification is called for. This is partly to deal with the extra points he has made, and partly because his attempts 'to avoid all ambiguities' now enable us to see a little more clearly the reasoning behind some of his objections and the slender basis of them all.

Dumont. It is astonishing that Pfeffer goes on talking about misrepresentation in the face of the passages from Dumont's text I had cited. His own new quotation makes no difference: it has long been clear to me that Dumont was not arguing for the identity of north and south India but simply searching for what similarities there were between the two areas; a legitimate exercise, though one bristling with difficulties. The tension this involved is revealed on the very first page of Dumont's article: 'I shall not be able to define a North Indian kinship system, but I shall try to express the similarities between North and South as a common, pan-Indian pattern' (1966: 90; my emphasis). There are many similar passages in the same paper. In the conclusion (ibid.: 113), one key point is partly italicized: 'it [the common pattern] consists essentially, if not perhaps exclusively, in the valuation, and in the consequent elaboration and ordering or patterning, of affinal relationships'. Far from being withdrawn in the paper of 1975, this phrase is quoted as something which 'is now widely acknowledged' (Dumont 1975: 197). Although Sylvia Vatuk had, in Dumont's own words, 'made light of my differences between north and south' (ibid.: 198), what Dumont identifies as his 'radical mistake' (ibid.) is not, as Pfeffer would have it, his 1966 comparison of these two regions but a detail of his analysis, dating originally from 1962, of the Gorakhpur terminology. One might infer from this that the former too was faulty, but this is certainly not made explicit. In fact, it is only much later (1983: 23) that Dumont finally appears to feel that this invalidates the comparison he had attempted. But only in part, it seems, since even this withdrawal is restricted to the terminologies, i.e. to just one aspect of the original comparison.

Pfeffer cannot have it both ways: either I have misrepresented Dumont by attributing to him what he did not say, or there really was something amiss; otherwise, what was the point of Dumont's withdrawal, however partial, which Pfeffer is at such pains to stress? I should add that I have no hostility towards Dumont himself, nor towards his work on India; on the contrary, I think he has done more for the anthropology of the area than any other scholar. But this is one respect in which I cannot follow him, nor it seems would he himself any longer entirely desire one to.

Juang. Although I did not give the Juang marriage rules as reported by McDougal in my first reply, they appear in my original JASO article (p. 256), where I properly record saliray (inter alia eGEyZ) as the main category from which a wife should be taken. Although it is not the only such category, McDougal makes clear its priority in terms of affinal alliance, both through explicit statements and statistically (1963: 157, 162; 1964: 331). Pfeffer, on the other
hand, prefers to emphasize different terms, *na* and *bokosini*, especially their second-cousin specifications (p. 154, above), glossing over the fact (despite his reference to the +2 level) that they are also PM and CD respectively (McDougual 1963: 162). Focusing on them supports Pfeffer’s own hypothesis that the Juang have a four-line, Aranda-type system (criticized briefly in my earlier reply, pp. 60-61). However, as McDougual also makes clear (ibid.; 1964: 331), the statistical significance of such specifications as potential marriage partners is actually very low. This far from exhausts the problems with Pfeffer’s hypothesis, a detailed discussion of which is now available (Parkin 1993), but the passage above is particularly tendentious.

It should be obvious that my use of the word ‘reflected’, which also appears in the sentence following that cited by Pfeffer, is not accorded any explanatory force but is used normatively in both cases. I have never associated myself with the view that terminologies are dependent for their form on other factors of a social-structural kind.

As for Pfeffer’s main objection, the three-generation rule is, as he admits, described in the literature on a number of groups, namely the Munda, Santal, Korku, Maler, Mal Pahariya, Hill Bhuiya and Hill Kharia (discussed with references in Parkin 1992: ch. 8, appendix II), to which we can now add the Sora (Vitebsky 1993: 38, 43, 48, 185). The failure of ethnographers working on other groups to report such rules certainly has to be treated with caution, but I eventually came to realize that many unclarities in their work could only be explained by taking the rules into account. This also applies to the Juang, though to a lesser extent, thanks to McDougual’s very full and detailed ethnography.

What seems to happen in most of these groups is that alliances should not be repeated before the passage of three generations, which we might describe as a negative rule. Only in one case, the Munda, is there any suggestion that alliances should be repeated after three generations have elapsed, i.e. that they have a positive rule (see Yamada 1970: 385). The connection of the latter with positive marriage rules in the usual sense of the term is unclear, but I should be inclined to consider systems with a positive rule, such as that indicated by Yamada, as closer to prescription than those with a purely negative one. In some other cases—and the Juang is one of them—evidence that alliances are directed by kin term may indicate the existence of a positive rule of delay, but we cannot be certain. However, we can be considerably more confident about the existence of negative rules, among the Juang and elsewhere. I certainly agree with Pfeffer that McDougual does not use the word ‘rule’ himself (despite what Pfeffer appears to think, I have never said that McDougual does). However, I do not feel this affects the issue, given the weight of other evidence he produces. Here yet again, therefore, is a brief review of that evidence as I see it, which for the sake of variety I present in terms slightly different from those I have used before.

McDougual himself makes it clear that, at least when seen from the point of view of lower-order segments, generational delays in the immediate repetition of alliances are in operation here (e.g. 1963: 158, 168-9). Terminological conflicts
arise if alliances repeat those of the previous generation (ibid.: 159). McDougal is not specific about the number of generations. In a table in my book (1992: 176, table 2), I actually left open the possibility that it was one, which would suggest repetition of the marriages of the +2, i.e. grandparental generation. However, I expressed a preference for three in the text (ibid.: 171), a preference I continue to hold for the following reasons.

First, there are clear statements by McDougal himself (1963: 159 n.1) that the delay of a single generation is in no sense a norm, and indeed that it is actually usually more:

> there is no special propensity for marriage choices to be related to [i.e. to repeat] those of close agnates belonging to the second ascending generation.... The marriage choices of persons in any generation are inclined to be related to one another, but unrelated to those formed by close agnates of all previous generations.

Secondly, only 10% of marriages in McDougal’s sample repeated those of the FF (ibid.: 160). For these two reasons alone, therefore, the indications are that the normal delay is longer than one generation. Thirdly, however, a two-generation delay, i.e. the repetition of the marriages of the FFF, would entail a six-section system with marriages between adjacent generations (the so-called ‘Ambyrn’ system; cf. Dumont 1983: 206-8). Whatever its feasibility in a general sense, this would offend against at least one norm of the Juang system, which is that marriages between adjacent generations should not take place; besides, the Juang do not have six nor any other number of sections. This makes four generations or any other even number equally unlikely. Fourthly, a greater number than three would require greater local knowledge about previous alliances than is likely to be possible. Fifthly, the comparative ethnography of the area suggests three as the upper limit. For all these reasons, three is indicated as the strongest possibility in the Juang case.

Pfeffer’s reluctance to accept this may be connected with the fact that his own hypothesis of a four-line, ‘Aranda’ system requires a one-generation rule of delay. A delay of any greater number of generations, whether norm or rule, would have the disadvantage of further undermining a hypothesis that is already quite unacceptable on terminological grounds (Parkin 1993: 326-9). Yet it is precisely a delay of this magnitude that McDougal’s evidence indicates. Neither his failure to write down the word ‘rule’ nor the fact that all rules, including Juang ones, are broken on occasion can be considered material objections here.

It should also be made clear that, when it comes to the evolutionary hypotheses I have been putting forward, it is the existence of a delay in renewing alliances, not the actual number of generations involved or the existence of a positive rule stipulating precisely when they should be renewed, that is significant in distinguishing systems like the Juang from prescriptive South Indian on the one hand and non-prescriptive North Indian on the other. In the last resort, my evolutionary hypothesis does not even depend on the Juang—all the necessary
evidence is available from other groups. Even if Pfeffer does return from his forthcoming field trip with the information that the Juang have no positive rule of this sort, nothing material would change in this respect. In fact, I should be inclined to welcome such information, since it would provide further if minor support for my longstanding contention that the affinal alliance systems of many of these groups are transitional between prescriptive and non-prescriptive, and therefore that they provide a historical link between south and north India.

Conversely, Pfeffer’s own hypothesis that the Juang have a four-line, ‘Aranda’ system would benefit from a positive rule of some sort, since this would provide at least a marginal indication that this was a fully prescriptive system, as the hypothesis requires. Whether this would affect my misgivings concerning that hypothesis is another matter, given that the terminology as recorded by McDougal has no diagnostic features of a four-line system (Parkin 1993: 326-9). Alternatively, Pfeffer might find that there is no rule or norm of delay, even in the negative sense. This could mean one of two things: either that there is no direction at all to the formation of alliances; or that one is expected to renew alliances with the same alliance group in the immediately following generation, which if followed consistently would produce a system based on first cross-cousin marriage. Either outcome would make everyone look silly, since neither bears any relation to existing Juang ethnography, whether McDougal’s, Pfeffer’s or anyone else’s (and by extension, of course, nor to my attempts at interpreting it).

Munda. The term ‘Munda’ is of Sanskritic origin and therefore not original in any sense to Austroasiatic speakers, although it has come to be used by one tribe as an alternative to their own term ‘Horo’ (i.e. Roy’s group; cf. Pfeffer above, p. 154; also Parkin 1990: 17, 23). Having been applied first by administrators and then by early ethnographers, it was taken over by linguists as a designation for the whole branch of Austroasiatic to which the language of this tribe belongs. These are quite radical shifts in meaning, especially the second, though it is generally accepted even by anthropologists, including, it seems, Pfeffer himself. My use of the term simply entails a further extension, which I carefully described and justified in my main comparative work, together with other modifications made in the interests of avoiding confusion between the various possible uses of the term as they already existed (Parkin ibid.).

Whatever objections might be raised against such well-meaning modifications, given my numerous explicit disclaimers (cf. my previous reply, p. 56), the suggestion that I have been guilty of reification or linguistic determinism as a result is absurd. On the contrary, differences as well as similarities between Munda groups are recognized where appropriate in all my work, however condensed its presentation. Pfeffer’s objection depends on eliding the various meanings of the word ‘group’, though I should have thought a phrase like ‘a group of tribes’ could not possibly be confused with the idea of a socially bounded whole. What Pfeffer dismisses as ‘Parkin’s group’ (p. 156, above) is clearly the former, as the sentence of mine he quotes in his first paragraph makes clear. In the latter sense, I intend it simply as shorthand for ‘ethnic group’, a formulation
I actually chose—I did not make this clear at the time—because it had the advantage of avoiding another potential misunderstanding, namely the sometimes derogatory connotations, elsewhere if not in India, of the term ‘tribe’.

There are, of course, cogent reasons for concentrating on a particular language family when comparing kinship terminologies in a historical sense, given that these are aspects of language and that linguistic relationships have a historical aspect. This is one of the things I am interested in, and it explains the way I chose to present this material in the relevant chapter of my book (1992: ch. 7; cf. Pfeffer’s objection above, p. 153 n.1). To pursue the matter of my forms of presentation further, while I sometimes produce composite descriptions to save space, I do not indulge in ‘mixing ethnographic bits and pieces’ (p. 153 above). On the contrary, in analysis, where it really matters, I am careful to treat each group separately to begin with, only then seeing what correlations (and, of course, differences) might emerge. Chapters 4, 8 and 10 of my book (1992), dealing with respectively descent, affinal alliance and reincarnation, are exemplary in this regard.

_Malto_. I am the author of the phrase ‘terminology poised between the last stages of prescription and individualizing North Indian’, as should have been clear from the original context. The reason for concentrating on just the +1 level of the Malto terminology has already been given in my earlier reply (p. 56). As to the only other real query of Pfeffer’s here, the fact that Sarkar gives terminologies from six different villages, with considerable variation in form, is, I think, a not unreasonable basis for assuming that there may be some dialectal variation, to which unwritten languages are particularly prone. However, it is not something I am going to insist on, especially since it would actually strengthen the argument from redundancy if purely dialectal variations could be eliminated as an alternative explanation.

For the rest, Pfeffer’s basic concern seems to be to heap praise on Sarkar for his abilities as an ethnographer—which I do not remember ever having doubted—and to complain that he personally has been put to an unnecessary degree of inconvenience in tracking down my original sources. I fail to see why it should have been quite so difficult: the two earlier articles of mine that discuss this matter (Parkin 1990: 74; 1992: 276 n.2) contain all the information necessary for my sources to be traced. At all events, Pfeffer obviously managed to locate them in the end. Yet to what purpose? No material challenge to my hypothesis has resulted from all this trouble.

_Jat_. The significance of Tiemann’s article on the Jat (1970) is that he shows how the four-got rule, found there and elsewhere in north India, actually works. It was never part of my purpose in referring to it to suggest either that the rule was restricted to the Jat (hence the use of the phrase ‘the Jat and other groups of north India’ in my original article (p. 258)), or that the statistical tendency for lineages to renew alliances with one another in the longer term was a feature of north India generally, a conclusion which would certainly be premature.

_Companionship_. It is clear from the original context that the phrase about changes from prescriptive to cognatic (the typology is Needham’s) is intended to
be a general point on the evolution of terminological patterns. \(FB = MB\) equations are, of course, cognatic, and I was not suggesting that they were to be associated with South Asia. I pointed out at the end of the previous paragraph that north Indian terminologies are non-cognatic, in which respect, if not in others, they resemble their South Indian counterparts. Perhaps Pfeffer would none the less be prepared to accept a South Asian example of ‘terminological companionship’ between equivalent female specifications, \(FeZ\) and \(MeZ\). There is at least one example in the literature, for which we must return to the Malo terminology. My immediate source is actually Pfeffer himself (1982: 90), though he has taken it from a homegrown ethnography. And the author of that turns out to be none other than that ‘meticulous reporter’ Sarkar.

ROBERT PARKIN

REFERENCES


MALINOWSKI AND THE IMPOUNDERABILIA OF ART AND PHOTOGRAPHY

I was very interested to read Jeremy Coote’s comments on Malinowski’s photography (JASO, Vol. XXIV, no. 1, pp. 66-9). I agree that any full account of Malinowski’s preoccupation with photography in his fieldwork should include an examination of the influence of Stanislaw Ignacy Witkiewicz. In fact, in a recent, as yet unpublished paper (Wright 1992) I have proposed that early contact with the Witkiewicz family could well account for Malinowski’s photographic vision and powers of observation. Furthermore, his acquisition of these visual skills was to play a major part in the development of his fieldwork method of participant observation. Malinowski did not just take a camera to the field, he evolved his own photographic working methods during his fieldwork. I have described in these pages his photographic experiments with Billy Hancock (JASO, Vol. XXI, no. 1, pp. 41-58) and, as Coote suggests, photographic experimentation was not new to Malinowski, for he had participated in Witkiewicz’s early experiments with natural light photography. Indeed, a good example is a photograph of Malinowski from around 1912 which displays a strong ‘Rembrandt lighting’ effect (reproduced in Micińska 1990: 96).

I believe that the success of Malinowski’s fieldwork photography was due to both his visual awareness and his emphasis on observation and detail. At the same time, a combination of these characteristics can be seen to have been evolving into his methodology of participant observation. For Malinowski, the minute detailed observations recorded by the camera (which perhaps went unnoticed at the time) had special significance. These ‘imponderabilia’ had an important role in his post-fieldwork analysis where, he wrote, ‘the control of my field notes by means of photographs has led me to reformulate my statements on innumerable points’ (1935: 461). Similarly, participant observation paid particular regard to such incidental elements: the ‘imponderable yet all important facts of everyday life are part of the real substance of the social fabric’ (Malinowski 1922: 19). In Leach’s words (1957: 120), ‘Malinowski trained his fieldworkers to observe the apparently unimportant minute detail... It is in the quality of the observation rather than in the interpretation that the merit of “Malinowskianism” lies’. I suggest that Malinowski’s development of participant observation was informed by and indebted to his first-hand knowledge of the characteristics of photography.

In summary, we can perceive a gradual transition in Malinowski’s fieldwork methods, determined by a variety of social and cultural factors, as well as personal influences, which brought about a new kind of visual enquiry. From the evidence of the Trobriand photographs, we can propose that Malinowski was not only the father of ethnographic fieldwork, but that his photographic innovations deserve wider recognition in the history of photography.
REFERENCES


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BOOK REVIEWS


It is said that when two anthropologists who have worked on the same society meet they make every effort to avoid speaking to each other in the language of their field research area. The reason seems to be that linguistic competence offers an uncomfortable ‘objective’ index of how well one knows the given society. There lurks a paranoid fear that each grammatical error, every lapse or misuse of vocabulary, will be taken as an incriminating sign of limited understanding, of inadequate fieldwork, in short, of a botched job.

Similar things could be said about fieldnotes. How many of us would happily make ours readily available, filled as they are with evidence of embarrassing culture shock, puerile understandings, lack of rigour, all recorded in disgracefully unpolished, boring prose? Fieldnotes, every bit as much as linguistic ability, are an ‘objective’ indicator of competence, with the one advantage that they can be stored under lock and key at home, or even ‘lost’. Little wonder then that fieldnotes have for so long been one of the more mysterious areas of anthropological practice. Students are rarely shown them, much less instructed on how to keep them, but are instead expected simply to ‘get on with it’ when their turn comes to go to the field.

Roger Sanjek’s Fieldnotes, with contributions from more than a dozen American anthropologists, is designed to remedy this situation, and it is fittingly dedicated to ‘the next generation of ethnographers’. This accurately recognizes who this volume’s most interested readership will be. Post-fieldwork anthropologists will have derived most of the volume’s insights from experience, but students and others preparing to enter the field will find in it plenty of reassuring observations. For example, Jean Jackson’s survey of seventy anthropologists (mostly from the East Coast of the USA) exposes the considerable variation in what people consider fieldnotes to be. Some included only descriptive notes taken in the field, while others extended the term to cover recordings, transcriptions, photographs and virtually anything else of informative value deriving from the fieldwork experience. This then raises a contrast with memories. If one recalls something months after returning and writes an additional page into a field notebook while seated at a desk in Oxford say, does this qualify as a fieldnote? Are headnotes fieldnotes? One respondent to the survey simply replied, ‘I am a fieldnote.’
Fieldnotes can profitably be viewed according to theories of orality and literacy. They are written down and thus fixed, although they constitute an unusual sort of text since author and audience are generally one and the same person. One consequence is that fieldnotes are only fully intelligible to the individuals who write them. Lutkehaus’s account of her use of Camilla Wedgwood’s fieldnotes, in preparation for her visit to Manam Island thirty-five years after Wedgwood’s, clearly shows up the difficulties, though she also had some unexpected advantages as she was slotted into the local society as Wedgwood’s categorical granddaughter.

While fieldnotes are fixed, headnotes can change as a function of memory, fieldsite revisits and intellectual maturation. One of the problems, then, is that order may be achieved in the form of a definitive analysis of a society only to be contradicted by the ‘facts’ recorded in one’s fieldnotes. As Lederman observes, fieldnotes are dangerous because they threaten to contradict one’s confident ethnography.

There was some disagreement among contributors as to how ‘raw’ the data of fieldnotes actually are. Plath views them as very raw indeed and of a completely different order from the ‘write-up’ (i.e. the resultant ethnography). Clifford, by contrast, maintains that the very ideas of ‘raw data’ and ‘fieldnotes’ are just constructions, albeit central to anthropological practice since Malinowski. They create the all-important spatial dichotomy between here and there, data and theory, upon which modern ethnography is predicated. He contends that everything anthropologists write in the field is in fact already structured—partially ‘cooked’—on account of the training and theoretical questions with which anthropologists enter the field. Fieldnotes thus already possess many of the traits of finished ethnography.

Ottenberg describes how his supervisor, Melville Herskovits, commanded him to send back his typed fieldnotes every few months and how he resented this intrusion and often felt anxious about the resultant criticisms (which he later learned were written by Herskovits’s wife). Throughout graduate school he was reduced to feeling that fieldnotes were part of his intellectual childhood, an anthropological infancy with the Herskovitses as parents, one in which he was made to feel dependent emotionally and psychologically for approval.

This theme of fieldwork as rite of passage is the primary topic of Wengle’s Ethnographers in the Field. For this book he interviewed in depth a handful of American anthropologists to elicit their experiences during fieldwork. To this he adds one chapter analysing Malinowski through information provided in his diary and one chapter considering the pseudonymous German–Canadian anthropologist Manda Cesara on the basis of her published personal account of fieldwork in Reflections of a Woman Anthropologist. Wengle’s unexceptional contention is that fieldwork is a rite of passage personally, as well as professionally. Fieldworkers’ experiences of loneliness and isolation are in all cases ‘identity-dystonic’, meaning, as far as I can see, that they are plunged into alien situations where they cannot simply be themselves. He is particularly interested in his informants’ fantasies, as
well as in the practices in which they engaged in order to preserve their sense of self (including, in one case, the use of a vibrator powered by a transformer).

One American woman researching in a northern European fishing village took the trouble of bathing every morning in a plastic tub in front of the fire: ‘atypical by local standards. But still, I did, every day, because it’s American to do that.’ This may seem ludicrously extreme, but Wengle coherently argues that such familiar practices as dressing for holidays or for certain meals, storing certain foods, reading novels, and eagerly meeting up with compatriots even though they are not of the type with whom one would normally keep company, are all examples of identity maintenance in the face of fieldwork.

His enquiries into his informants’ dream and fantasy worlds provide unusual materials, even if the general theme of these has to do, unsurprisingly, with family and loved ones ‘back home’. The question arises if all of the time and effort directed at identity defence affect the collection of data and the eventual writing of ethnography. Here the author shies away from any rigorous attempt to draw conclusions or correlations. In the case of Malinowski he briefly hazards the opinion (in a footnote) that Sex and Repression in Savage Society ‘cries out to be read as (auto)psychobiography’.

Like Fieldnotes, Ethnographers in the Field will appeal most to students contemplating, or better still, doing fieldwork. It will help them prepare for what fieldwork holds in store emotionally. I found the psychological terminology hard to swallow, but the book does have the virtue of being brief and, in the middle chapters, devoted in the main to original narratives. This brevity distinguishes it from Fieldnotes which, swollen by five separate contributions from its editor, is about 150 pages too long.

CHARLES STEWART


This book, the promised account of Uduk religion, ritual and healing, signposted in Wendy James’s earlier account of the Uduk in ‘Kwanim Pa: The Making of the Uduk People (Oxford, 1979), will surely take its place alongside Nuer Religion and Divinity and Experience as a classic of African religious ethnography. It is characterized by meticulous ethnographic detail, a sensitivity to historical context and a deep respect for the subjects of the study. James is scrupulous in recording and acknowledging the circumstances and limitations of her contacts with the Uduk, seeking to avoid generalizing from conversations with particular individuals and the temptation to present her own interpretations as universal truths. The
effect of many different, contextualized accounts is to present a picture of Uduk society, behaviour and belief as a dynamic force. There is a recognizable structure that acts upon, but is also constantly remodelled by, individuals within that society.

An ‘Introductory Essay’ outlines some key features of Uduk ethnography and gives a brief historical and geographical sketch of their territory. The methods of analysis and presentation stem in part from the nature of the material. Uduk ethnography simply will not yield a structural-functionalist account of a people with a coherent, discrete cosmos. There is no ‘seamless whole’ that can be taken as a working model of ‘culture’ (p. 3). Competing sources of power provide the framework for an analysis of Uduk moral and religious systems. This necessitates looking, as the Uduk themselves do, at the peoples and events outside their immediate language group and territorial area. Peoples who do not quite so obviously occupy an area of competing and shifting political and intellectual claims nevertheless form part of a wider system, and James’s account provides an analytical model that does justice to this wider context and to the role of individual agents: ‘not only do the small language communities receive ideas from powerful neighbouring or encompassing civilizations, but the people themselves appropriate and modify, and they respond intellectually, emotionally, and politically to some extent within the terms of reference offered them’ (p. 3).

The Uduk present an assorted jumble of Islamic, Christian, Nilotic and more local ritual practices and conceptual systems. These various religious groupings do not, however, give rise, either historically or in the present, to firm or absolute divisions within Uduk society. They can rather be viewed as a palimpsest on which an underlying and more unified ‘moral knowledge’ can be discerned. The term ‘moral knowledge’ refers to implicit certainties about human experience, from which the Uduk’s response to imported religion stems (p. 4). It should not be thought of as ‘a rigid and enduring system of encoded prescriptions for behaviour. It indicates rather the store of reference points from which a people, as individuals or as a collectivity, judge their own predicament, their own condition, themselves as persons’ (pp. 145-6).

The book is structured in three main sections. Part one, ‘The Archive of a Hunting People’, describes the Uduk understanding of themselves as a forest people, the source of much of their language, imagery and conceptual world. In a postscript to this part, James looks at recent philosophical debates about the role of the person and at the distinction between morality and religion in the light of the Uduk material. Part two, ‘The Claims of High Theology’, details the interaction between Nilotic prophets, members of the Protestant fundamentalist Sudan Interior Mission (SIM) and the Uduk. In the postscript to this section, Uduk encounters with and reactions to Islam are discussed briefly.

A fascinating and sensitive account of the problems faced by members of SIM in translating the Bible into Uduk highlights the distinctive role of their particular brand of Christian theology. Considerable emphasis was placed on ‘work with paper’, i.e. literacy and education for reading the Bible, and on verbal statements of belief, confession and repentance. It would probably be hard to find a people
in the Sudan for whom this approach could have been less familiar, for Uduk cults, many of which cross linguistic boundaries, are 'celebrated through symbolic action, movement, and music, rather than defining themselves in the explicit instructions and formulae of the word' (p. 219). The language consciously chosen by the Bible translators, who worked with Uduk converts on the vernacular terms to be used, is strongly dualistic, stressing notions of purity and impurity, good and evil, world and spirit, alien to Uduk thought. The missionary notion of God emphasized God's separation from humankind, and SIM theology stressed the separation of believers from non-believers, both in this life and in the world to come. Uduk traditional belief, in contrast, makes no distinction between the source of life and the life force in individuals and in the world at large. Rather than dividing the dead into those who dwell in eternal bliss and those in eternal darkness, Uduk see the next world (unlike the present one) as 'an earthly place close to home where all would be joined together again' (p. 229).

The expulsion from the Sudan of Western missionaries in 1964 led, after a period of stagnation, to a series of Christian revivals, prompted in some extent by a belief that Christ's second coming was imminent, but also by an accommodation to Uduk indigenous ideas and customs under the guidance of an Uduk pastor. James also records that the story of a dead and resurrected Christ holds considerable power for the Uduk, and finds echoes in the life of Leina, a prophet from the neighbouring Meban, who is also a focus of ritual action.

The third section of the book, 'The Ebony Speaks', describes the spread of the 'ebony order' among the Uduk and its place in divination and healing. The success of the movement is linked to its appeal to 'the wild', which resonates with older Uduk understandings of the natural world and human order. Aloysius Pieris has correlated the success of a 'metacosmic' religion, which moves away from its point of origin, with the degree to which it is able to accommodate the older 'cosmic' systems it meets (see An Asian Theology of Liberation, Edinburgh, 1988). Although the ebony order would not fit into Pieris's 'metacosmic' (world religion) category, his thesis is equally valid in this case of religious acculturation. The ebony order has obviously resonated strongly with Uduk 'cosmic' consciousness, enabling a new synthesis to become established.

An epilogue points to the role of women as guardians and repositories of the older 'moral knowledge'. They are marginal to the various religions and cults, which seek primarily to recruit young men, and through their kinship networks link representatives of the different religions. It is through women, therefore, that the largely implicit Uduk moral community exists and is held together, perpetuating their existence as a small people surrounded by more powerful neighbours.

FIONA BOWIE
Johannes Fabian's new book is both an attempt to portray ethnography in a reflexive and non-interpretative manner and a treatise on the theoretical and methodological implications of such an endeavour. The work deals with a contemporary Zairean theatre group, Le Troupe Théâtrale Mufwankolo, based in Lubumbashi. In following this popular drama group through the creative vagaries of conceptualizing and performing a work of dramatic art, by means of various meetings and rehearsals, culminating in a filming and televised broadcast of the play in a local village, the author hopes to demonstrate as closely as possible the ethnographical antithesis to 'traditional' value-laden anthropology.

After preliminary ruminations upon the proper course that the 'New Ethnography' should take (more on that below), Professor Fabian begins his chronicle with a consideration of a proverb-like expression, le pouvoir se mange entier, that he had encountered during an earlier stay in the same geographical vicinity whilst engaged upon another fieldwork project. Not only is this sententious phrase rendered solely in the former colonial language, but it is found to have no direct translation in the various local indigenous languages and a shifting and amorphous meaning for those asked to explain its import and bearing upon the peculiar nature of 'power'. The phrase, then, becomes the subject of the theatrical group's next recorded performance.

In certain ways the subject of 'power' and 'power relations' infuse the pages of this book, albeit as interpretatively supplied by the reader. Hierarchy and negotiation are demonstrated in the way in which the play is gradually pieced together and shaped under the active participation of the group's senior and junior members. The 'content' of the filmed dramatization takes place in a 'traditional' village under 'traditional' headmanship. In fact, the performance is 'about' chiefly authority and responsibility.

Also, there is always the metonymic reminder that the nationally broadcast version of the play will be interpreted by the viewing audience in such a way that its subject-matter will be scrutinized against the prevailing system of power in the country. In true Solomonic fashion the actors have to exercise utmost discretion under trying circumstances in choosing dramatic material to present before a politically astute audience, as well as in determining the eventual outcome of its plot. This subject of power, although prominently displayed in the title of the book, is in my view only a subtheme of it.

The book is really about how to approach the subject of cultural knowledge and the epistemological foundations of such an approach. In Fabian's opinion, a diffuse view of the 'core of cultural tradition' is permitted through the mediation of knowledge by actual performances, an acting out, as it were, rather than a
discursive commentary on social life. Fabian also takes pains to avoid the theoretical pitfall of interpreting these ‘displays’ of ‘cultural tradition’, providing instead step-by-step verbal transcriptions of the events leading up to the final televised version of the play. The series of meetings and rehearsals that culminate in the last enactment are felt, correctly, to be every bit as legitimate and important as cultural performances and/or demonstrations of cultural praxis, as the final statement.

If the reader is inclined to look for an application of performance theory (if I may take the liberty of labelling it in so grandiose a fashion), or a discussion of the aspects of verbal poetics inherent in such modes of creative and artistic behaviour, he or she will not find it. Rather, the author would feel (I think due to an extreme reading of such studies) that these considerations are but empirically based and, as a consequence of harbouring theoretical biases, politically naive.

To some extent this is true, but the honourable quest for hermeneutic ‘purity’ must be set against what I feel is an inadequate attempt to marry an overt discussion of the role and method of ethnographic enquiry and presentation with an actual ethnographic context and record. Perhaps this is, in fact, the challenge for the ‘New Ethnography’ and its proponents (whose arguments are in most cases completely justifiable). In this the author is, therefore, constrained to present an anaemic ‘noninterpretation’ of a potentially exciting subject for study and analytic scrutiny.

GREGORY VAN ALSTYNE


Everywhere in South Asia religion is a complicated matter, but nowhere more so than in the Kathmandu Valley of Nepal, where Buddhism and Hinduism are thoroughly intertwined. For many Nepalis, as well as their observers, it has often been impossible to decide whether they are really Buddhists, Hindus or both, though there has been a definite tendency to label a large proportion of the people as Hindu and to identify their religion as just another variant of Hinduism. One of David Gellner’s principal aims in this monograph is to clarify the situation and to show that ‘Newar Buddhism’—‘the traditional Mahāyāna and Vajrayāna Buddhism practised by the Newars’ (p. 5), who make up the ethnic group most closely identified with the Kathmandu Valley—stands somewhere between Hinduism and Theravada Buddhism as practised in Sri Lanka and Southeast Asia, but also differs from both of them.
Gellner carried out fieldwork in Lalitpur, the most Buddhist city in the Kathmandu Valley, and concentrated on religion, especially on the rituals that are the heart of highly ritualistic Newar Buddhism. Gellner’s material on ritual is particularly impressive, both in its detail and in his grasp of the overall logic, which owes much to his enviable ability to make good use of indigenous texts. Except for some jarringly convoluted sentences (which could have been corrected by better copy-editing), the book is clearly written and organized, even if the sections on ritual may prove to be heavy going for non-specialists. Whether Gellner is always accurate in his reading of Buddhist material I cannot properly judge, but he is rarely mistaken about Hinduism, and he is particularly right to stress, in his subtle discussion of the relationship between the two religions in Nepal, that whereas Hinduism can easily accommodate Buddhism as a lower form of itself, the reverse is not true.

The leading subjects of Gellner’s study are the Vajrācāryas and Śākyas, the Newar Buddhists, who are pre-eminently monks, though they are also married householders; Vajrācāryas also act as priests. Contrary to much received wisdom, the distinction between monks and householders does not define that between Buddhists and Hindus. Within the Newar caste system, as explained in chapter 2, Vajrācāryas and Śākyas together constitute a high-ranking Buddhist caste positioned alongside the Hindu Brāhmans and Śrēsthas; the other, lower castes include Maharjans and the rest of the ordinary Newars, who are not unequivocally identified as Buddhist or Hindu. Chapter 3 discusses the relationship between Buddhism and Hinduism, and chapter 4 examines the basic notions of Newar Buddhism, including the distinction between the Three Ways: the Disciples’ Way (Śrāvakayāna), the Great Way (Mahāyāna) and the Diamond Way (Vajrayāna)—the last (and highest) being an esoteric path within the Great Way. Chapter 5, which looks at the basic rituals of Newar Buddhism, contains an exemplarily clear analysis of the fundamental rituals, themselves made up of a patterned series of elementary ritual actions, out of which more complex rituals are constructed. This analysis, suitably adapted, could usefully serve as a model for comparable analysis of Hinduism’s basic rituals as well.

The bulk of Gellner’s monograph comprises a detailed description and analysis of the monastic ideal in the Disciples’ Way (chapter 6), the Buddhist householder and the guthis (socio-religious associations) in the Great Way (chapters 7 and 8), and the priesthood and the Tantric deities’ cult in the Diamond Way (chapters 9 and 10). A level-headed discussion of the place of Tantra in Newar Buddhism, and its contemporary decline, follows in chapter 11; and there is a short concluding chapter 12.

Gellner’s claim that Newar Buddhism is characterized by variant—higher and lower, esoteric and exoteric—interpretations of the same ritual is well-supported by his evidence. It deserves close attention from other scholars of South Asian religion, who have sometimes underplayed the diversity of indigenous interpretations that exists even when there is no overt stress on esotericism, which there is in the Newar case owing to its Tantric component. I am less happy, however, with
Gellner’s predilection for typological classification, which sits uneasily alongside his emphasis on interpretative variation. Indeed, Gellner appears to have perpetrated in a new way the error so pervasive in earlier literature on Hinduism and Buddhism, for he has converted a fluid indigenous classification of Ways into an analytical typology that is imposed upon the data, much like others have imposed hard and fast distinctions between the ‘great’ and ‘little traditions’ (or cognate concepts). Gellner claims that his categorical division between Ways is ‘emic’—unlike, say, Spiro’s categories in his study of Burmese Buddhism (pp. 4-5)—and that the hierarchy of the Three Ways provides an integrated framework for Newar Buddhism. I find this unconvincing, however, for Gellner’s categories, and the framework fashioned from them, have a fixity that they lack in indigenous discourse. Thus, for instance, we are told that the distinction between the Three Ways is reflected in the classification of deities (p. 113), but this grossly simplifies earlier and more complex analysis of the pantheon (pp. 73-83), and later we learn that many deities actually cross the divide between the Great and Diamond Ways, partly because they are subject to different levels of interpretation (pp. 253-7). Hence Gellner’s own evidence shows that deities cannot be systematically and hierarchically assigned to the Three Ways without serious distortion. Or, to cite another example, although Tantric initiation and worship plainly belong to the Diamond Way, Gellner never explains how worship organized by guthis belongs to the Great Way, except in the broad sense that the latter encourages worship of all gods (p. 114), and the allocation of guthis to the Great Way seems to be determined by the typological scheme, rather than by the facts themselves. Gellner also makes considerable use of a Weberian analytical distinction between soteriology and social or instrumental worldly religion; although this is often helpful, I do not see how he can conclude that the analytical distinction ‘may indeed be universally valid’ (p. 337), except in the sense that ideal types, if defined loosely enough, can always be made to do some work.

There is, therefore, a tension in this monograph between the emphasis on interpretative variation and the dependence on typological classification, a tension that seems to prevent Gellner from seeing that although interpretation is consistently made with reference to the Three Ways, these referents are themselves kept fluid through the very act of interpretation. Nevertheless, for the quality of its ethnography and its ambitious attempt to make sense of an extremely complex religious field, Gellner’s book deserves to be recognized as a major contribution to scholarship.

C. J. FULLER

Before 1818, the year in which the heads of Rajasthan’s princely states signed treaties with the British, a Rajput warrior guardian was a soldier and/or ruler who defended his realm and conquered new territory. Conquest and even death on the battlefield were his principal goals. The role of the warrior, however, conflicted with the woman’s domestic role, since, as a pativrata guardian (i.e. one who has taken a vow of devotion to her husband), a woman was responsible for his preservation and protection. The incongruity between a man’s caste duty to protect his community and a woman’s gender duty to protect her husband is reflected in the religious narratives of Rajput women. Drawing on her recent fieldwork among Rajputs, Lindsey Harlan examines the inherent tension in the relationship by exploring three types of women’s narratives, related respectively to kuldeviś (clan/family goddesses), satimatās (women who have immolated themselves on their husbands’ funeral pyres) and heroines. In addition to her analysis of these stories, she also discusses Rajput devotional practices in order to assess current indigenous constructions of caste and gender.

In her introduction and chapter 1, Harlan is concerned primarily with the manner in which Rajputs articulate their shared sense of identity. She states that they are keenly aware of being distinct, as being persons with royal blood. The worship of kuldeviś and the veneration of family satimatās both reinforce and express this sense of distinctiveness. Although a number of Indianists have analysed the Rajput kul as a kinship unit, in-depth study of kuldevi worship has been largely neglected. Similarly, there has been little scholarly treatment of contemporary satimatā veneration. By investigating these devotional practices, which are not merely emblematic of kinship, but also symbolize Rajput collective identity, Harlan provides an important contribution to the ethnographic literature on north India and to the understanding of Rajput religious behaviour today.

Chapters 2 and 3 look at kuldevi myths. The author comments that, although the kuldevi has only one task (to provide protection), she operates in two separate arenas: on the battlefield and in the home. In her martial form, the goddess is a consumer of husbands whom she sacrifices as warrior-protégés for the sake of the clan’s preservation. This contrasts with her marital form. In the image of a pativrata, she is a dutiful wife, a husband-protector, and guards the family against sickness, poverty and infertility. Although these representations are contradictory, Harlan persuasively argues that they are also viewed as symmetrical and complementary. Thus, positive conceptions emerge regarding the goddess’s protective functions. Moreover, Harlan not only illustrates how the problem of incongruity is resolved spatially by the myths (women usually stress the domestic role of the kuldevi), she also demonstrates that the goddess remains a powerful source of inspiration to women, a model of the pativrata ideal.
Chapters 4 and 5 explore the way in which the tension between caste and gender is represented in satīmatā stories. It is suggested that these narratives differ essentially from the above variety only in one respect: they articulate the ethic of protection temporally rather than spatially. (On the other hand, Harlan states that, because women often contextualize kuldevī and satīmatā stories by referring to stories of Rajput heroines, the latter operate simultaneously in both spatial and temporal dimensions. Tales of heroines are discussed in chapters 6 and 7.) Regarding satīmatā stories, Harlan comments that the satī scenario creates a temporal solution to the problem of dissonance by condensing time. While the warrior’s death on the battlefield suggests failure on the part of the wife to provide domestic protection, this is negated by means of a further sacrifice, that of the wife on behalf of the husband. Thus, ‘the dying woman who has outlived her husband becomes a sahagūmi, one who “goes” at the same time. She is understood, retroactively, to have protected her husband throughout his life and death, both of which she has shared’ (p. 224). Because satīmatā stories explicate the ethic of protection, Harlan shows that they continue, albeit obliquely, to inform and to sanction women’s domestic behaviour. For although self-immolation is prohibited today, in these stories (as in the narratives of kuldevīs and heroines) ‘women find paradigms that help them construct personal interpretations of pativrata duty’ (p. 51).

The book does have one problem in that it fails to analyse the relationship between knowledge and power. Although all Rajput women are familiar with stories of kuldevīs, satīmatōs and heroines, as well as with the rules that govern their religious customs, only noble and aristocratic women possess in-depth knowledge of these traditions. It seems that this is partly why high-caste Rajput families continue to exert influence in the community. Moreover, because choṭā bhāī (ordinary) Rajput women do not possess specialist knowledge and tend to have only a superficial understanding of these traditions, their families appear to be excluded from the local prestige system. This may also account for the fact that they remain socially inferior. It is unfortunate that Harlan does not address the issue of power. Nevertheless, Religion and Rajput Women is an important book and will be welcomed both by anthropologists and South Asianists.

Graham Dwyer


Gananath Obeyesekere is well known for his extensive researches into the ethnography of Sri Lanka and South Asian symbolic systems. The present volume
comprises the Lewis Henry Morgan Lectures given at the University of Rochester in 1982. Here Obeyesekere weaves his earlier findings into a theory of the production of culture. Starting from an ontological approach to culture, he unfolds his argument along two critical lines. One pertains to basic anthropological assumptions that exclude the individual (or the ‘spider at work’, the subject creating networks of cultural meaning) from the vision of the anthropologist (p. 285), while the other implies a critical review of the metapsychology of Sigmund Freud. As the title *The Work of Culture* suggests, Obeyesekere builds on Freud’s rules of the dreamwork in his own hermeneutical attempt to extract the rules of interpretation of cultural forms.

According to Obeyesekere, the ‘dethroning’ of consciousness through Freud’s discovery of unconscious processes deconstructs the radical hiatus between psyche and culture in the conventional anthropological distinction between ‘private’ and ‘public’ forms of life. (A detailed discussion of this point, especially concerning the position taken by Leach, had already been included in his earlier essay *Medusa’s Hair* (Chicago, 1981).) Concerning the notion of the unconscious, however, Obeyesekere examines Freud’s two models—the first and second topographies—in relation to their applicability to the study of non-European societies. Thus he rejects the second topography—the model of id, ego and super-ego—as being too constrained by contemporary Judaeo-Protestant values. Instead, he considers the earlier tripartite model of conscious, pre-conscious and unconscious to be a relevant analytical tool for the study of other cultures. He also argues that societies in South Asia have themselves dethroned consciousness in favour of realities dominated by unconscious thought (trance, possession etc.), and that Buddhism and Hinduism also contain knowledge of unconscious motivation, without having formulated theories about it (pp. 52, 253).

Apart from the usual objections to psychoanalysis as a ‘Western science’—a fact which, according to Obeyesekere, holds good for all anthropological theories (p. 219)—he considers the adoption of the pathological model of psychoanalysis by cultural anthropologists (Kardiner, Whiting, Spiro, etc.) to be a major drawback in its application. (A detailed discussion of this subject in the original lectures was excluded from the present book but will appear in the Spiro Festschrift being edited by Jordan and Schwartz.) For Obeyesekere, cultural forms are not merely epiphenomena of defence and projection but the results of processes of symbolic transformation.

Another problem arises from Freud’s notion of religion as illusion, in which Obeyesekere does not find any concern for the creative potential being part of both the dreamwork and the work of culture (pp. 18ff.). Borrowing the concepts of ‘progression’ and ‘regression’ from the French philosopher Paul Ricoeur, he identifies a dialectic in the formation of symbols pertaining to processes of distancing from or moving closer to archaic or psychogenetic materials. In this context he introduces the notion of ‘symbolic remove’, operative in the dreamwork as well as in the work of culture, as one of his key concepts (pp. 51ff.); closely
related are the analytically distinct terms of 'personal' and 'cultural' symbols (cf. Medusa's Hair).

In the first of the four lectures into which the book is divided, Obeyesekere unfolds his argument in relation to the meaning of ritual on the level of individual action on the one hand and of collective representations on the other. Beginning with a confrontation between two types of ecstacies that he described at length in Medusa's Hair, he presents the reader with a distinction between symptom and symbol on the basis of his concept of symbolic remove to archaic motivation: while the symptom is over-determined by motivation, the symbol is over-determined by meaning. He shows how the actions of certain ascetics are initially triggered by idiosyncratic conflicts that lead, in the course of ritual action, to the solution of these conflicts by creating a new cultural reality. In collective representations, on the other hand, one is confronted with another variety of performances not bound to individual motivations and life histories. Here, Obeyesekere presents two types of ritual performed in Sri Lanka: the serious drama Killing and Resurrection, in which the goddess Pattini tears off her left breast and throws fire on the city of Madurai in order to revenge her dead husband; and a new and vulgar drama called Bambura, in which allusions to genitals, copulation and castration abound (cf. his The Cult of the Goddess Pattini (Chicago, 1984)).

The serious drama, or dromonon as he calls it, has not been performed since the late 1950s and has instead been replaced by the vulgar or cathartic ritual. The themes of the dromonon, embodying cultural, religious and philosophical values, do not seem to be related on a manifest level to those of the cathartic ritual. But, Obeyesekere argues, there are several sets of symbols that cluster around latent themes widespread in the culture concerning fears of impotence and castration. Thus he identifies an unconscious equation of breast and penis expressed in the lex talionis reaction of Pattini to the death of her husband. While according to Obeyesekere both rituals contain similar motives (in one domain, at least), they move in different directions: 'if a dromonon is engaged in a progressive movement away from the sources of infantile conflict and anxiety, the cathartic ritual moves in a regressive direction' (p. 28).

In the second and third lectures, Obeyesekere examines South Asian mythologies under the same aspect of hidden deep motivational themes and their cultural treatment. He revives the discussion buried long ago relating to Oedipal conflicts and postulates a complex existing in multiple forms exhibiting family resemblances — a term he borrows from Wittgenstein in order, simultaneously, to criticize his highly relativistic position. Even Wittgenstein could not, says Obeyesekere, avoid admitting a common ground for all humanity, however 'muddy' the bottom may be. Still, the concept of the Oedipal complex is, like other concepts of the social sciences, a fictive one, according to the author. Comparing the symbolism of the paradigmatic Hindu Oedipal myth of Ganesha with Buddhist Sri Lankan mythology of the kings, he finds a striking difference between the models of filicide and parricide they contain. Both are related respectively to differences in Hindu and Buddhist family models; authority
structures and—in the latter case—historical dynamics producing patterns of interdependence between kings and monks.

The final lecture assesses the relevance of Freud’s metapsychology for anthropology. While stripping it of certain drawbacks, Obeyesekere regards it as a crossroads where three intersubjectivities meet: first, the relation between the anthropologist and his or her informants; secondly, the dependence of the ethnographic production on this intersubjective relationship; and thirdly, the production of anthropological theories in the context of academic debate. Included in this section is a discussion of the validity of ‘psychoethnography’ through case histories and myth associations.

Obeyesekere’s argument is far more complex than can be shown here. In a way, his perspective implies a critique of his own society as well as of certain aspects of anthropological tradition. Regarding the latter, he finds particularly lacking the dimension relating to human suffering, impermanence and death, or the experience he calls the ‘dark night of the soul’ (p. 11). His work is an attempt to integrate this dimension of the basic human experience with current debates in anthropology. In his own concluding remarks he expresses clearly the difficulty of reconciling these domains: ‘Freudianism per se is no solution. The trouble with the Freudian analysis of culture is precisely its inability to see man within the context of a cultural tradition and encompassed by the institutions of a society in which he is placed. The whole thrust of this work has been to partially redress the balance by introducing the idea of the work of culture, that discontinuous movement from the ideational representatives of deep motivations to their transformations into culturally constituted symbolic forms’ (p. 289).

HELENE BASU


Hastings Donnan’s new book effectively serves as a case-study of the application of Islam and as a reminder that alternative theological agendas and cultural adaptations exist in specific cultural and historical locales. In the context of the village of Choaya in the north-western Punjab is found the complex interchange between a Muslim socio-religious order governed by the Hanafi school of Islamic jurisprudence and the expression of Dhund culture and social organization. Donnan neglects to identify some particular cultural elements as fundamentally Islamic, so some previous knowledge of Islam is required. However, if we keep in mind the synthesis and negotiation necessary to the expression of Islam in any given context we may disregard this identification and consider it peripheral. Thus the irony in the rather grandiose title Marriage among Muslims becomes apparent,
and increasingly so when we consider Donnan's thesis that the Dhund consider the most salient difference between themselves and others to be their marriage system.

Revelation about this system is suspended as Donnan contextualizes his study within a composite sketch of village life including religious, linguistic, historic and economic aspects of Dhund culture. In chapter 3 he traces the complementary life histories of male and female based on the dichotomy of female as liability and male as asset. Female liability is identified in both social and economic terms. With the centrality of protecting a woman's virginity in order to ensure marriage-ability and maintain family honour, a woman is considered a social liability to her family, and in turn an economic liability because the seclusion necessary for her social protection curtails her wage-earning potential. What Donnan has established by the end of this chapter is that marriage preferences and choices for males and females are necessarily different, particularly since they tend to be specified by the families of the individuals concerned.

In chapter 4, Donnan proceeds to identify other criteria that the Dhund consider with regard to marriage preference and choice. Although he admits such a scheme to be problematic, Donnan attempts to organize factors relating to marriage choice into a hierarchy of determination. He subsequently undermines this attempt through documenting two family histories that illustrate how these preferences, based on aspects of religion, culture, kinship and geography, can have varying relevance in terms of actual marriage choice.

What Donnan establishes through this exhaustive survey is the inadequacy of focusing on rules or preferences to understand marriage choice, and how the same confluence of factors considered favourable in one context are wholly insufficient in another. Here he is able to justify his rejection of the rule-bound analysis of marriage and marriage preferences, which he sees as having concealed the complexity that underlies marriage choice. What is alternatively more suggestive, states Donnan, is to look at why particular preferences are advantageous in each situation, and to look beyond their immediate transactional value to the broader social, economic and political concerns that such negotiations and alliances fulfil.

CAMILLA GIBB


The inhabitants of the Polynesian atoll of Pukapuka deserve our sympathy. They have been hosts to many and various anthropologists and foreign observers during this century, to the point where their knowledge of their own history has been brought into question. The problem that inspired this ethnography concerns a
particular form of traditional social organization, known as the *akatawa*, which the island's Council of Important People decided to revive in 1976. Unfortunately, data collected by various Westerners visiting the island over a period of several decades suggest that the *akatawa* probably never existed before 1976 and certainly never occurred at the time claimed by some knowledgeable Pukapukans' (pp. 1-2). This discrepancy challenges the validity of both accounts of the past in a way that threatens their use in the determination of the future. Whereas without a historical precedent the Pukapukans might have found changing their socio-political organization in this way problematic, their use of traditional knowledge authenticated the new system. On the other hand, the suggestion that generations of academics might have missed such a significant feature of atoll life could seriously undermine their claim to the accuracy and thoroughness that form the basis of academic integrity.

Apart from this direct confrontation, though, much more profound questions arise concerning the nature of knowledge and its appropriation and usage. Borofsky asks what it is about the nature of historical knowledge that can lead to such different versions of a history. He approaches the problem by questioning 'how different people construct different versions of the atoll's past' (p. 2). Essentially, this means showing how Pukapukan knowledge acquisition differs from that of Western academic anthropologists. Thus Borofsky wishes the reader to perceive 'how Pukapukans, in the process of learning and validating their traditions, continually change them. And we see how anthropologists, in the process of writing about these traditions for Western audiences, overstructure them, how they emphasise uniformity at the expense of diversity, stasis at the expense of change' (p. 2).

As Borofsky does well to point out, many social scientists have already emphasized diversity in the organization of cultural knowledge, nor is he the first to explain that contextual considerations shape the way we present knowledge to others. However, in writing what is essentially a comparative ethnography of Pukapukan knowledge acquisition, Borofsky brings these issues to life, exploring the ways in which knowledge is shaped by the process of learning. This gives us a different slant on what is now a firmly established body of literature about the authentification and revival of traditions. The contrast Borofsky draws between Pukapukan and anthropological ways of knowing and learning highlights academic arguments over the non-negotiability of textual historical accounts, an idea at the heart of the post-modern movement. Borofsky's is not a dogmatic work and does not deal in power politics. It shows, in the best possible anthropological sense, how there are at least two sides to every story and many equally valid versions of every event. In a Pukapukan sense, knowledge acquisition does not often take the form of formalized teaching and learning, but is likely to be a more subtle blend of indirect copying by observing other people's practices and picking up information during discussions about the way things are done. Memories may be confirmed or adjusted in conversation, so that a consensus account of history can develop that can include various people's recollections and interpretations. This
contrasts strikingly with the formalized learning of anthropologists, which stresses individual written, and therefore much less negotiable, versions of times and events.

*Making History* provides a lively account of how, in preserving past traditions by resurrecting them, Pukapukans thereby altered them. In doing so, it places the previous ethnographic accounts of Pukapukan social organization firmly in a particular time and place. It also adds to a growing body of literature that questions the uses of knowledge and the role of anthropologists and historians in non-literate societies.

S. A. ABRAM


This short book, edited by Gehan Wijeyewardene, brings together two articles by Georges Condominas, a French specialist in Southeast Asian anthropology and history, originally published in French in 1974 and 1976. The first article, ‘Notes on Lawa History Concerning a Place Named Lua’ (*Lāwa*) in Karen Country’, is mainly concerned to point out the importance of a hitherto little-known site for the further understanding of state formation in the history of the northern Thai and proposes further study of the site and its area. The second deals with the evolution of Thai political systems in historical perspective and proposes a typology for them, consisting of local chiefdoms with an emerging class formation, political systems that are now conveniently labelled early states, and the Thai and Khmer empires. What Condominas calls the ‘social space’ under consideration is northern Thailand, Laos and Vietnam and the influence exerted over the area by Mon kingdoms and the Khmer empire. The time dimension is less clear: on the one hand, Condominas refers to documents and social events from the thirteenth to the sixteenth centuries, and on the other to ethnographic evidence from the twentieth century as well as to local mythology. He states his indebtedness to Braudel and his concept of the *longue durée* without explaining it further in respect of the area under consideration.

The epigraphic, historical, linguistic, archaeological and ethnographic material Condominas draws on in this second paper is rich. He describes effectively the internal composition of the various levels of socio-political integration and state formation, as well as the internal structures of these formations, the relations of rulers and ruled, and particularly the social structures, internal divisions and social conditions of the latter. However, the weak point in the essay is his rather
ambitious theoretical framework, which draws on a wide range of theories and combines them in order to come to terms with the different social formations at the margins of the established and centralized states of the Mon and the Khmer. This blending of a number of different anthropological theories can be justified because of the differences in state formations, but the evidence given for the postulated shift from feudalism to the Asiatic mode of production is unconvincing. Condominas's notion of the latter is anyway spurious and misleading, and not only because of its identification with the concept of 'hydraulic society'. His idea of feudalism, though less vague than Leach's, is also far from convincing. Here, Condominas refers to Bloch, Duby, LeGoff and others for his evidence, but the resemblances seem weak or too generalized. His special point of reference with regard to the relations between rulers and ruled in feudalism is the position of the peasantry within the socio-economic formation of emerging or formed states and systems of social bondage, from which he argues for a strong resemblance between the mass of the peasantry and European 'free peasants' in the Middle Ages and in later periods of feudal society. However, this concept is misleading in two ways. First, the idea of the 'free peasant' concerns a rather special relationship in European feudal history, where vilainage was far more common. Secondly, Condominas seems to be underestimating such traditional social ties as bonds of kinship, village life, religion and social obedience that are particular to the areas under investigation and that—on a comparative level—are characteristic of Asian societies in general. His references to feudalism and to the Asiatic mode of production are thus neither particularly convincing nor useful.

Despite the weaknesses of its theoretical framework, this account of socio-political formations and of relations between the different peoples in the areas under discussion is certainly interesting, at least to one who is a student of state formation but not a Thai specialist.

KLAUS HESSE


This book is a republication of a study that first appeared in 1965 and has been reprinted several times since. It is based primarily on fieldwork in Oklahoma carried out in the 1930s and more recently, but also draws on Alice Fletcher's 'The Hako: A Pawnee Ceremony' (1904) and a manuscript 'Ceremonies of the Pawnee' by James R. Murie, which was first published only in 1981 (see review of 1989 edition below). It complements, in certain ways, Alexander Lesser's Pawnee Ghost Dance Hand Game (1933).
The lost universe of the title is that of the Pawnee prior to their removal to Oklahoma in the 1870s. The concluding chapter is titled 'The Universe Regained: Ideas and Forecast'. The universe regained is that of Post-Industrial Man, and the chapter contains some rather eccentric comments on automation and the need for standardized apartments modelled on a modernized version of the Pawnee earth lodge. The main text is a mixture of ethnographic description and narration of experiences, both actual and typical, of Pawnee life based on informants' memories. In the course of 65, often very brief, chapters, the book covers such topics as planting and harvesting, human sacrifice, the buffalo hunt, camping routines, skirmishes with the Sioux, domestic life, ceremonialism, sorcery, and the manufacture of various tools.

There is indeed a great deal of original information here, and some readers have obviously enjoyed its narrative approach, but its aim of relating this narrative to problems of New York urban existence in the early 1960s has produced results that inevitably have dated. The mixture of descriptive ethnography, historical reconstruction, and semi-factual narrative places obstacles in the way of readers looking only for ethnographic and historical fact or scholarly interpretation, while the ethnographic passages may be felt to be an interruption by readers who only want to enjoy the story. Readers who can combine both interests will undoubtedly be best satisfied.

R. H. BARNES


James Murie, who lived from 1862 to 1921, was the son of a full-blooded Pawnee mother and a Scot who was captain of Pawnee scouts. He received four years of schooling at the Hampton Institute in Virginia. Later he assisted Alice Fletcher, George A. Dorsey and Clark Wissler. He collaborated with Dorsey in publications of Pawnee and Arikara mythology and acted as an independent researcher. Parks writes that 'most of what is known today about nineteenth-century Pawnee ethnography is attributable to Murie either directly or indirectly, since in addition to his own writings he worked with virtually every anthropologist who visited the tribe in Pawnee, Oklahoma' (p. vii). Although he served as an informant, he also provided access to older Pawnee, for whom he interpreted. His influence extended to Gene Weltfish (see review above) and Alexander Lesser, who worked with the Pawnee after his death but who used his manuscripts in their own work.

Although only one publication appeared under his name alone during his lifetime, he left behind a large unpublished record, including the manuscript of
Ceremonies of the Pawnee’, which he wrote after 1912 in collaboration with Clark Wissler for the Bureau of American Ethnology, completing it just before his death. For reasons which are not known, plans to publish it then came to nothing. In 1929, John R. Swanton arranged for Gene Weltfish to check Marie’s linguistic transcriptions with Pawnee informants, which she did in 1930 and 1931. She revised the manuscript for publication, phonetically retranscribing the Pawnee, but again for unknown reasons the project was abandoned. Parks has retranscribed Weltfish’s phonetic versions into phonemic ones and revised the translations. In addition to editing the manuscript, he has added an overview of Pawnee social organization and religion, indexes, an expanded bibliography and a biography of Murie. The book is divided into two parts. The first describes the ceremonial bundles, songs and procedures of the Skiri band. A substantial account of the ceremony of human sacrifice to the morning star is included. The second section provides similar information for the South bands.

Ceremonies of the Pawnee was first published in 1981 in two parts as one of the Smithsonian Contributions to Anthropology. Despite this late publication date, the monograph retains the solid scholarly feel of the earlier classic reports and bulletins of the Bureau of American Ethnology, with which in conception it is contemporary.

R. H. BARNES


This work consists of a short introduction, 41 maps, a bibliography and a ‘culture index’ that lists the ethnonyms of 3500 ‘cultural groups’ worldwide. One can use the atlas in two ways: one can turn to a map and find an indication of the ‘cultural groups’ that live in that part of the world, or armed with the name of a ‘cultural group one can look it up in the index and find out where it lives. After both processes, one can then look up a relevant bibliographic entry and discover more about the group in question.

An enterprise such as this is inevitably fraught with difficulties—several of which Price mentions in his introduction—and the book is a tribute to Price’s diligence and tenaciousness. Unfortunately, it is little else. The maps are drawn in outline only and the numbers that identify the ‘culture groups’ are scattered across a near featureless landscape (coastlines and major—though unnamed—rivers are shown). Price acknowledges that the maps ‘are intended to assist in the navigation of libraries rather than the navigation of South American rivers or African coastlines’ (p. 11), but without even national boundaries or latitude/
longitude indicators even navigating a conventional atlas would be difficult. The index of groups contains only one name for each, and while this is often the one by which they are commonly known in the literature it is of no help in difficult cases. (Yolngu/Murngin was one I tried but neither are listed.) Finally, the bibliography (some 1237 references) while large, is also eccentric. The latest reference I could find was dated 1986, which is admirable, but there are many, many more from the 1930s and 1940s than are acceptable. Moreover, several of the entries are not the ones one would expect; surely there must be a better work on ‘Gujaratis’ than Majumdar’s Races and Cultures of India (1961). According to the list of references, the most important works on the Maasai are a 1905 publication by Alfred Hollis and a 1930 article by L. S. B. Leakey.

It would be pointless for me to discuss either the enormous difficulties involved in trying to discern which are the world’s ‘cultures’ that are to be mapped (Price does this to some extent in his introduction, but his solution—that he follows the authors of the works cited in the bibliography in defining the group in question—is a dissimulation), or to point out the numerous resulting errors, inconsistencies and biases. Two examples should suffice. First, some of the bibliographic reference numbers given after a culture group’s name are wrong, so that, for example, the sole reference given for that well-known group in the ethnographic record, the inhabitants of the Isle of Man, turns out to be a 1964 article on Icelandic kinship terminology. Secondly, the bizarre assortment of language groups, tribes, ethnic groups and nationalities reflects neither the available literature nor the local situations, so that, for example, while the Gujaratis, Mizos and Marathis are each accorded a place among the cultures of the Indian subcontinent, no other group defined by state boundary is (Majumdar is again the cited authority on both the Mizos and the Marathis).

The Atlas is presumably aimed at libraries and, going by title alone, many librarians may well be tempted to buy it. There are, however, better ways of spending £35.

MARCUS BANKS


Wilhelm Schmidt (1868–1954) was one of the outstanding figures in German anthropology. This biography is, therefore, most welcome, especially since most articles on Schmidt, as well as the only other full biography, by Fritz Bornemann, are in German.
The book is divided into four parts, three of which provide a chronological narrative of Schmidt’s life. Apart from his research and related activities (for example, his involvement in the Anthropos Institute, various journals and the Lateran ethnographic museum), we learn, for example, of his role as the field chaplain of the last Austrian emperor (1916–18), about his pastoral work, about his close relationship with Pius XI, and about his political writings. Schmidt’s antisemitism, placed within the historical context of nineteenth- and early twentieth-century Vienna, is extensively discussed, and Brandewie concludes that it was, apart from feeding on ‘common prejudices’, mainly ‘political’ rather than ‘racial’ (p. 242). The book ends with the story of the amazing intrigues directed against Schmidt during the last months of his life, intrigues orchestrated by his former pupils who by that time had taken over the Anthropos Institute and journal. Why this ‘sordid affair’ (p. 325) happened, Brandewie does not attempt to explain. The narrative is interrupted by a systematic consideration of Schmidt’s The Origin of the Idea of God, especially of Schmidt’s definition of culture, his view of anthropology as a historical discipline, his theory of culture circles, and his theory of primitive revelation and monotheism. Brandewie underlines the role of apologetics as a motivating force behind Schmidt’s research and the importance of Thomism for his thinking.

It is a pity that Brandewie contents himself with only one aspect of Schmidt’s view of religion. As important as primitive revelation and monotheism are, through his culture circles Schmidt ‘proved’ that they are only the starting-point of a larger developmental scheme of ‘degeneration’ of religion (the ‘second revelation’ being the chance to put things right). With this theory Schmidt refuted ideas of progressive evolution and criticized secularism and materialism as well as conditions and politics based on these mistaken Weltanschauungen. From this perspective it would be possible to relate Schmidt’s academic work to his views on the issues of the times (his ‘non-anthropological’ and ‘popular’ writings) and his pastoral activities.

Brandewie succeeds best at giving us ‘a “feel” for Schmidt as a person’ (p. 5), for example his single-mindedness and perseverance in pursuing ideas and plans, a characteristic that made him such a successful organizer and gave unity to his work throughout his life, though it at times exasperated his colleagues and religious superiors. While we learn a lot about Schmidt’s work, it seems that Brandewie, perhaps inevitably in a biography, chose to focus on certain issues while leaving others unexplored. One thinks of Schmidt’s relation to wider intellectual and theological traditions (deism, the concept of natural religion, the history of the comparative study of religion etc.), the foundations of his analytical terminology and epistemology, and the reception of his theories in theology. In short, there is still much to learn about Schmidt.

STEFAN DIETRICH
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